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'Homogeneous Turbulence' and Its Relation to Realizable Flows

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Abstract

This paper examines in some detail how the decay of homogeneous turbulence in the absence of shear is affected by the constraints of finite boundaries. It demonstrates how the ratio of the wavenumbers of the spectral peak to the lowest resolved wavenumber (or tunnel size to integral scale) can influence directly the time dependence of the energy, the integral scale, and even the rate of decay of the turbulence. If this ratio is not large enough, the length scales grow too slowly and the energy decays too fast. Criteria are proposed for assessing the validity of the data; but few experiments or simulations satisfy them.

Introduction

The theory of homogeneous isotropic turbulence begins with G. I. Taylor in the 1930's [8], who was among the first to realize that simple closure approximations were not leading to a solution of the turbulence problem. Most have learned about such turbulence from the monograph *Homogeneous Turbulence* by G. K. Batchelor [1] whose career we honor at this meeting. Because this book was written more than 50 years ago, our understanding has changed in the past half century, and it was evolving even at the time the book was being written. Even so this book stands as a landmark achievement, and remains one of the most eloquent and elegant expressions of the theory of homogeneous turbulence.

Yet it is a truth that once an idea is in print, it remains in print forever, even if the idea is wrong. It is read by succeeding generations as truth, even if the ideas expressed therein have long been proven wrong. This is because the first time reader is usually not aware of the subsequent advances or new questions, nor for that matter are many experienced researchers who were not involved in them. Therefore the bulk of the research community remains more or less frozen in time — the time the book was written — until a newer book comes along. The wait can significantly stymie progress, since it can lead to the view that all the problems in a given field have been resolved and no further research is necessary, when in fact much may remain unresolved. Because of the well-deserved stature of Batchelor's *Homogeneous Turbulence* and the lack of major breakthroughs, this has been the case with the subject of homogeneous turbulence. Newer books have come along, some massive volumes which meticulously detail our progress, others which oversimplify — all lacking the appeal and authority of Batchelor's.

This paper is an attempt to review the progress of our understanding about some of the ideas in homogeneous turbulence since Batchelor's book was written. It is our view that to do so (and to do so at this meeting) is very much in the spirit with which the original book was written — as snapshot our understanding in 1951 for the purpose of stimulating further work. If Batchelor had intended otherwise, he would have published it as a text instead of a monograph. We can only honor him by continuing to push forward the agenda he laid out on a subject so clearly dear to him.

Some historical perspective

Homogeneous turbulence is the simplest type of turbulence, especially if the further assumption of isotropy is made. In the theoretical problem there is no mean flow, no production of energy, no spatial gradients of any averaged quantity. The turbulence simply decays, so it is described exactly by :

$$\frac{d}{dt}\frac{<q^2>}{2} = -\varepsilon \tag{1}$$

where ε is the rate of dissipation of turbulence energy per unit mass defined by $\varepsilon = 2\nu < s_{ij}s_{ij} >$. (The instantaneous turbulence kinetic energy per unit mass is $q^2 \equiv u_i u_i$ and s_{ij} is the fluctuating strain rate.)

Normally it is **assumed** that the energy decays as a power law in time; i.e., $< q^2 > \propto t^n$ where for decay we must have $n < 0$. Now there are only two theories that actually derive a power law from more or less first principles. The first was due to von Karman and Howarth [7] in their famous 1938 paper in which they *derive* from a similarity hypothesis that $n = -1$. There were many attempts to obtain this result in the laboratory by measuring downstream of a grid. Initially it was believed $n = -1$ was pretty good (see [1]). But as tunnels got better (longer and lower background intensity) and hot-wires got better (smaller and lower noise), it became clear that the Townsend/Batchelor measurements yielding $n = -1$ were really in error. Corrsin was the main dissenter, and the careful experiments at Hopkins by he and his students throughout the 1950's consistently got numbers for $n < -1$. But he could never really convince the Cambridge researchers to admit their data might be in error, so he finally shipped all his grids to Cambridge so they could check them for themselves. They never did use them (to the best of my knowledge), but Townsend did use Corrsin's results in the second edition of his book, so I guess he must have decided Corrsin was more or less right — and the Comte-Bellot/Corrsin [3], [4] papers did appear in the *Journal of Fluid Mechanics* of which Batchelor was founder and editor.

So the end result of all this is that few now believe you will ever see $n = -1$. In fact, it seems that almost everyone more or less *assumes* that the Comte-Bellot/Corrsin experiments were the last word on this subject and that it is established forever that $n = -1.25$. Now in fact, CBC got this result for only two rather low Reynolds numbers which differed only by a factor of two, and then ONLY for the square bar grids! All of the other grids gave very different results, and the tables they include of the measurements of others vary from near $n = -1$ to $n = -1.5$. And curiously, the DNS people have been trying for years to reproduce the -1.25 number, and only recently are succeeding — maybe [2], [10]. What they usually get instead is a wide range of values from close to $n = -1$ to $n = -2.5$. The number appears to go down as the grid Reynolds number increases. And there are also indications that the result depends on how the flow begins (e.g., the size of the energy scales relative to the size of the box), and even more importantly, which part of the decay

one decides to fit a power law to. Some of this will be discussed in the next section.

Why bother? Isn't homogeneous turbulence irrelevant?

There are many people, not only in industry but in the research community as well, who state quite openly that no one should waste any time on homogeneous turbulence. They really believe it is irrelevant to the many important engineering and scientific problems in turbulence. Well, it is easy to show they simply don't understand what is at stake here.

Let's consider the simple $k-\varepsilon$ turbulence model (which accounts for about 95 % or more of the industrial use at present). The model equations reduce to:

$$\frac{dk}{dt} = -\varepsilon \tag{2}$$

$$\frac{d\varepsilon}{dt} = -C\frac{\varepsilon^2}{k} \tag{3}$$

where it is hoped that C (usually called C_{ε_2}) is a universal constant — or at least a universal parameter which can be determined from a simple experiment. In other words, if this model is to be of any use at all, it must be possible to determine C independent of the flow being calculated. The obvious choice of experiment is homogeneous decaying turbulence, exactly the flow under discussion here.

Now let's assume (as all do) that the energy decays as a power of t, say $k = At^n$ with constant n. This together with equation 2 implies that $\varepsilon = -nAt^{n-1}$ and $d\varepsilon/dt = -n(n-1)At^{n-2}$. So equation 3 implies:

$$\frac{d\varepsilon}{dt} = -n(n-1)At^{n-2} = -C\frac{[-nAt^{n-1}]^2}{At^n} \tag{4}$$

It follows immediately that our simple "constant", C is given by:

$$C = \frac{n-1}{n} \tag{5}$$

It is easy to see why turbulence modellers desperately want n to be a constant and universal, since for the range of values cited above, $1.4 \le C \le 2$. So they have a strong motivation to pick the experiment they want, and to assume all the others wrong. And given the range of uncertainty, it is not hard to understand why most engineers using the model simply pick the value that works best for them, losing in the process the confidence that they can really "predict" anything at all. (A lot of turbulence "prediction" is like predicting yesterday's weather — you change the model until you get the right answer. It works almost every time — but only for yesterday.)

OK, you say, we simply make C a function of the local Reynolds number. Nice idea, but in fact it appears that the primary determinant of n is who did the experiment or simulation, how they started the flow, and even how big the experimental or computational box is relative to the size of the energy containing eddies. These are definitely not factors one wants to have to try to parameterize into an engineering turbulence model! It is not likely *all* of the careful researchers could be so incompetent to produce such a range, were there not some underlying physical reason. So clearly if we are to move beyond our current confusion, we must first understand why we get the various values of n in the first place. The reasons suggested below are twofold: first, that we may not have done the experiments right; and second, that nature may actually produce different values even when the experiments (and simulations) are performed properly.

Can we ever obtain a truly homogeneous turbulence?

This is a very important question, but it really is not often asked? (By contrast, there has been a lot of interest over the years about whether it is possible to generate isotropic turbulence.) The answer to both questions is unequivocally NO! The definition of a homogeneous flow demands that the statistics be independent of origin. Obviously no finite experiment or computer simulation can achieve this. So the questions we really ought to be asking are:

- Can we make useful approximations to homogenous turbulence?
- Under what conditions are our approximate attempts valid?

The problem we face is easily seen by examining the three important spectral integrals determining the energy, the dissipation and the integral scale. If, following Batchelor [1], we denote the energy spectrum averaged over spherical shells of radius k as $E(k)$, these are:

Energy

$$\frac{3}{2}u^2 = \int_0^\infty E(k)dk \tag{6}$$

Integral Scale

$$L = \frac{\pi}{2u^2}\int_0^\infty \frac{E(k)}{k}dk \tag{7}$$

Dissipation

$$\varepsilon = 2\nu\int_0^\infty k^2 E(k)dk \tag{8}$$

Clearly we can not expect to model scales larger than the box size in an experiment or lower than the lowest wavenumber in a periodic simulation. Therefore the wavenumbers below some cutoff low wavenumber, say k_L, are not present. And, of course, we cannot integrate to infinity either, because we are limited by probe response and numerical resolution to some finite wavenumber, say k_H. It should be obvious from the equations above that even these three simple measures, u^2, L, and ε will be affected very differently. A simulation or experiment may adequately resolve the energy, but be seriously in error for the integral scale or dissipation. Worse, the energy which should have been in the missing scales has not simply disappeared, it has been redistributed and may have an adverse effect on the entire spectrum. *It is important to note that this should not be taken to imply the experiment or simulation is wrong, only that it is not a proper approximation for homogeneous decaying turbulence.*

The hard part of the problem is to figure out which scales (if any) are correctly represented and which are not; and which of the statistical quantities are properly representative of homogeneous turbulence, which are not. Clearly there is no reason to expect that all statistical quantities are properly represented just because a few are, especially since some may depend on the small scales, others on the large. Nor is there any reason to expect our approximate attempts to be valid for all times. There may be transients at the beginning, and confinement effects at the end, since the scales grow in time during decay. So our goal has to be to figure out what approximations are valid and when.

These same questions were asked by the early experimenters, and the rather crude answers were decades in coming. For example, the early experiments of G.I. Taylor [8] used a very short tunnel with a very crude grid. It really wasn't until the

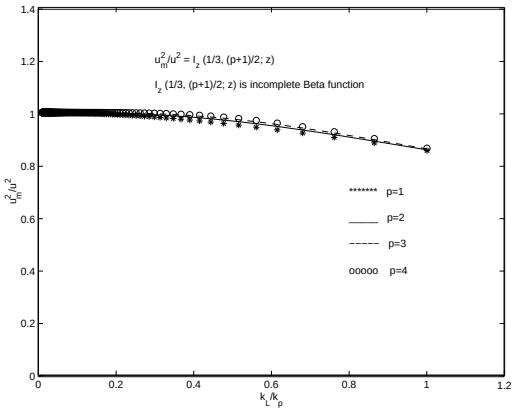

Figure 1: Ratio of energy above k_L to total energy versus k_L/k_p.

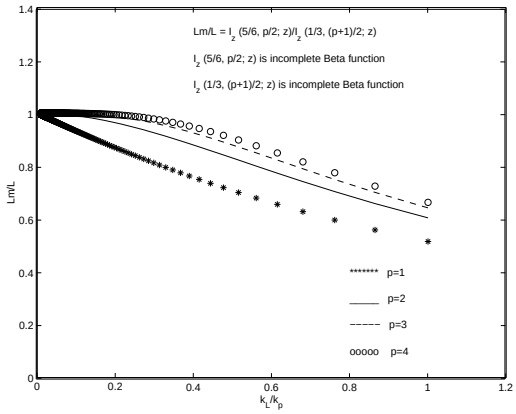

Figure 2: Ratio of portion of integral scale above k_L to total integral scale versus k_L/k_p.

careful experiments of Corrsin and co-workers that it was confirmed that the energy didn't really decay properly unless you had many meshes (typically 20 or more) across the flow. And also it was necessary to start measuring farther away from the grid than about 20 to 40 mesh lengths for the flow to behave properly. These were really empirical findings obtained with great effort. With modern CFD we can ask much more difficult questions. But the answers will be much the same. Crowd the turbulence into too small a space (physically or computationally) and the decay rate will be faster than the corresponding homogeneous turbulence. Statistics which depend on the smallest scales will be more accurate than those that depend on the largest, unless the former are also influenced adversely by resolution problems.

Before we proceed further along this line, let's deal with one of the common fallacies that one often hears: namely that we really shouldn't bother with the theory of homogeneous turbulence, but we should treat the experiments (or data) as sacred. Of course, assuming the measurements are correctly taken and the simulations properly done, they are indeed accurate representations of the flow generated. But unfortunately, as such, they are really pretty useless to the would-be turbulence modellers, since who wants a turbulence model that has built into it the idiosyncracies of a windtunnel or a CFD code. Measurements are only useful if we can sort out the physics from the experimental or computational boundary conditions. If there were a corresponding theoretical foundation for periodic turbulence or box turbulence, we could use that. But so far, it is only homogenous turbulence which provides us all the relationships and consistency checks that Batchelor [1] so nicely put forth. And it is only the assumption of homogeneity that makes all those other difficult terms (like convection and turbulence diffusion) disappear so we can isolate terms we wish to model. So approximate this ideal state, we must. In the following sections we shall review recent progress in understanding the limitations of our approximations.

How serious is the problem of the large scales?

This problem has recently been considered in some detail by Wang and George [9] who used a model spectrum to evaluate how much the missing largest scales contribute to the kinetic energy and the integral scale. Figures 1 and 2 summarize their findings as ratios of the partial integrals *above* the cutoff wavenumber k_L, to the *total* integrals for the energy and integral scale (equations 6 and 7). The cutoff wavenumber has been normalized by the peak in the energy spectrum, k_p, but can equally be expressed using the integral scale itself as shown below. Clearly as the peak wavenumber moves closer to the cutoff wavenumber, the values that can be estimated from the data are reduced, and in the case of the integral scale, significantly. The parameter p is the power law dependence assumed for the spectrum at the origin where $E \propto k^p$. Not surprisingly, the integral scale shows a strong dependence on this very low wavenumber behavior.

The model spectrum used is a generalization of the spectrum originally proposed by von Karman/Howarth [7], and in non-dimensional form using u^2 and L is given by:

$$E(k,t) = u^2 L \frac{C_p (kL)^p}{[1+(k/k_e)^2]^{p/2+5/6}} \tag{9}$$

This normalization collapses the one-dimensional spectra in grid turbulence well into the equilibrium range. This particular form is the infinite Reynolds number limit of the composite spectrum used by Gamard and George [5], the one-dimensional form of which provides an excellent fit at all wavenumbers to turbulence spectra behind a grid from $50 < R_\lambda < 500$. The coefficient, C_p, and the cut-off wavenumber, k_e, must be chosen so that the spectral integrals produce the proper values for the energy and integral scale (as shown below).

The value of p must be specified, but will be chosen in the subsequent sections to correspond to the George [6] similarity relation:

$$p = -2n - 1 \tag{10}$$

where n is the decay exponent. One advantage of this choice is that C_p is then an invariant of the decay. A disadvantage is that it may predispose the answer, a subject addressed in some detail below. The quantities k_e and C_p can be related to u^2 and integral scale, L by substituting equation 9 into equations 6 and 7. The results are:

$$k_e L = \frac{3\pi}{4} \frac{B(\frac{5}{6}, \frac{p}{2})}{B(\frac{1}{3}, \frac{p+1}{2})} \tag{11}$$

and

$$C_p = \left[\frac{4}{\pi B(\frac{5}{6}, \frac{p}{2})}\right]^{1+p} \left[\frac{B(\frac{1}{3}, \frac{p+1}{2})}{3}\right]^p \tag{12}$$

where B is the familiar beta function. Thus both $k_e L$ and C_p are constants dependent only on the value chosen for p.

Another wavenumber of interest is the wavenumber at which the spectrum $E(k,t)$ has its maximum, say k_p. It is straightforward to show (by setting $dE/dk = 0$) that k_p is given by:

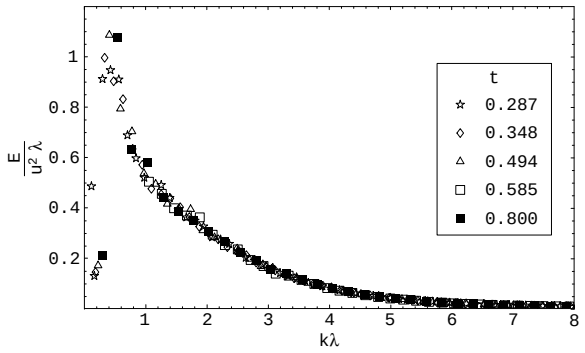

Figure 3: $E(k)/u^2\lambda$ versus $k\lambda$ for the de Bruyn Kops/Riley [2] DNS data.

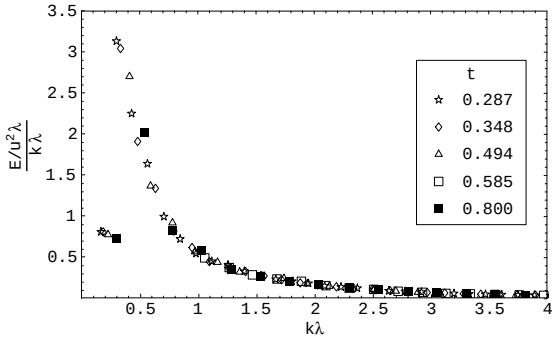

Figure 4: $[E(k)/u^2\lambda]/k\lambda$ versus $k\lambda$ for the de Bruyn Kops/Riley [2] DNS data.

$$k_pL = \frac{3\pi}{4}\left[\frac{3p}{5}\right]^{1/2}\frac{B(\frac{5}{6},\frac{p}{2})}{B(\frac{1}{3},\frac{p+1}{2})} \tag{13}$$

Correcting the integrated spectra for the missing wavenumbers

The advantage of the spectral model of equation 9 is that the ratios plotted in Figures 1 and 2 could be obtained analytically. The result for the energy ratio that has been plotted in Figure 1 is:

$$\frac{u_m^2}{u^2} = I_z(\frac{1}{3},\frac{p+1}{2};z) \tag{14}$$

where I_z is the incomplete Beta function. The variable z is defined as:

$$z \equiv \frac{1}{1+(3p/5)(k_L/k_p)^2} \tag{15}$$

where k_L is the lowest (or cutoff) wavenumber of the simulation or spectral estimator, and k_p is the peak in the energy spectrum.

Similar considerations showed that the ratio of the computed to true integral scale is given by:

$$\frac{L_m}{L} = \frac{u^2}{u_m^2}I_z(\frac{5}{6},\frac{p}{2};z) \tag{16}$$

Another advantage of these analytical results is that they can be used to estimate what the proper values of u^2 and L might have been had the missing low wavenumbers been available. This is not simple because the true integral scale, L, appears in both equations (since it determines k_e or k_p), and on both sides of equation 16. This can be done, however, by iteration: simply supply the measured values along with the cut-off wavenumber, k_L, and iterate the choices of u^2 and L until equations 14 and 16 are satisfied. The problem is how to choose p without biasing the answer. The results of such a procedure will be shown below, along with some considerable discussion of the validity of the entire procedure. Obviously such a procedure makes sense only if the part of the spectrum that is available from the data itself reasonably models a homogeneous turbulence.

What do real DNS data look like?

Figure 3 is taken from [9], and shows the recent 512^3 simulation of de Bruyn Kops and Riley [2]. The data have been plotted as $E(k)/u^2\lambda$ versus $k\lambda$ as suggested by [6], and have been taken only from the approximately constant power law region identified below. Note especially the scarcity of wavenumbers below the peak for the longest times. And even for the early times only a few wavenumbers are available. Clearly the energy is underestimated if the integral cannot be performed over wavenumbers significantly below the peak wavenumber. Since the spectral peak is moving to lower wavenumbers as the turbulence decays, the situation gets worse with time. So even if isotropic turbulence decayed with a simple power law with constant exponent, the uncorrected attempts to simulate it would appear to decay with a time-dependent and increasingly negative exponent. Note that the problem addressed here is *only* assuming that the energy spectrum itself has not been affected by its inability to send energy to these missing low wavenumbers.

Figure 4 shows the same data plotted using E/k instead of E, also normalized using u^2 and λ. Since the integrand of equation 7 depends on E/k, the effects of the missing low wavenumbers occur much earlier for the integral scale than for the energy. Thus values of k_L/k_p which have negligible effect on the energy, cause a major underestimate of the integral scale.

Because of the scarcity of spectral data below the peak wavenumber in all the simulations (having mostly to do with the manner in which the averages are made), very little of the published DNS spectral data can be used for the energy without worrying about the contribution below k_L, and virtually none is acceptable for the integral scale. Since the lowest wavenumber of the simulations is known, however, it is possible to make an approximate correction to the values of u_m^2 and L_m determined directly from the data if p is known. (Note that it is very important to only use data determined by integrating above the lowest wavenumber, since any attempt to fill in the amount from zero to k_L will result in an over-correction.) As is clear from the Figures 1 and 2, the amount of correction is quite sensitive to the value of p. The effects of such a correction will be demonstrated in the next section once it has been established that a power law decay is an appropriate description.

Does real turbulence decay follow a power law decay?

A sensitive test for whether turbulence in fact decays as a power law with constant n is provided by the Taylor microscale defined by:

$$\lambda^2 = 15\nu u^2/\varepsilon \tag{17}$$

where ν is the kinematic viscosity and ε is the rate of dissipation given by:

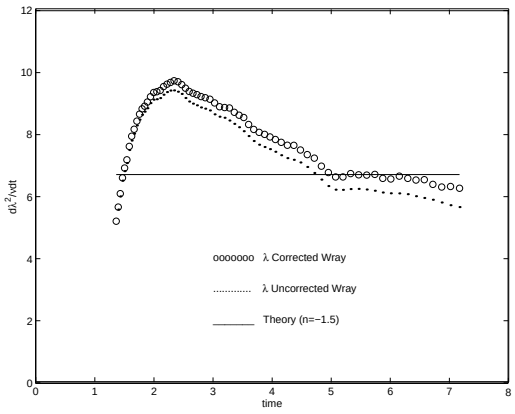

Figure 5: Corrected and uncorrected $d\lambda^2/\nu dt$ versus t for Wray DNS data [10].

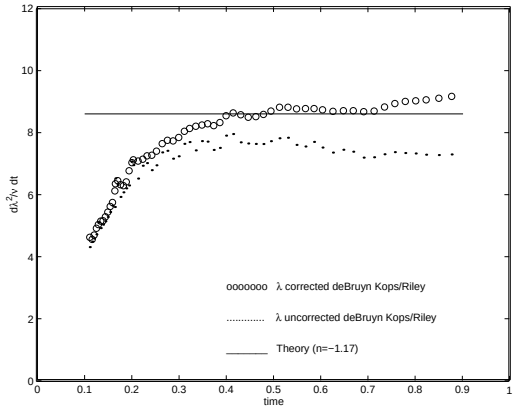

Figure 6: Corrected and uncorrected $d\lambda^2/\nu dt$ versus t for de Bruyn Kops/Riley DNS data [2].

$$\varepsilon = -\frac{d}{dt}\left[\frac{3}{2}u^2\right] \tag{18}$$

It follows immediately that if the turbulence decays as a power law decay (with constant n), then the Taylor microscale squared grows linearly in time [1]; i.e.,

$$\lambda^2 = 2A\nu(t - t_o) \tag{19}$$

where A is a constant and t_o is a possible virtual origin. The value of $2A$ is specified by the George [6] theory to be:

$$A = -\frac{5}{n} \tag{20}$$

In practice a power law curve can almost always be made to fit some portion of every experiment or simulation since, including the virtual origin, there are three parameters. This arbitrariness can be entirely avoided by taking the derivative of equation 19 to obtain:

$$\begin{aligned} \frac{1}{\nu}\frac{d\lambda^2}{dt} &= 2A \\ &= -\frac{10}{n} \end{aligned} \tag{21}$$

Thus this derivative produces a simple constant value *if the turbulence decays as a simple power of time*. If the George theory is correct, the value of this constant is determined exactly by the decay rate parameter, n. Moreover, the virtual origin can be chosen by fitting λ^2 since this must grow linearly.

Figures 5 and 6 display $d\lambda^2/\nu dt$ versus time for two attempts to simulate decaying isotropic turbulence using DNS. The first is a 512^3 simulation due to Wray [10]; the second also a 512^3 simulation due to de Bruyn Kops and Riley [2]. The time, t, in both plots is in the scaled variables described by the respective authors. Each plot shows two curves, one obtained by directly integrating the spectral data, the other by correcting for the missing low wavenumbers as discussed in the next section.

Figure 5 would suggests that there is at most a very short region that might be described by a power law with constant exponent ($4.5 < t < 6$). The beginning behavior ($t < 4$) is undoubtedly the starting transient (perhaps due to the limited high wavenumber resolution as demonstrated below). The slow decline for large

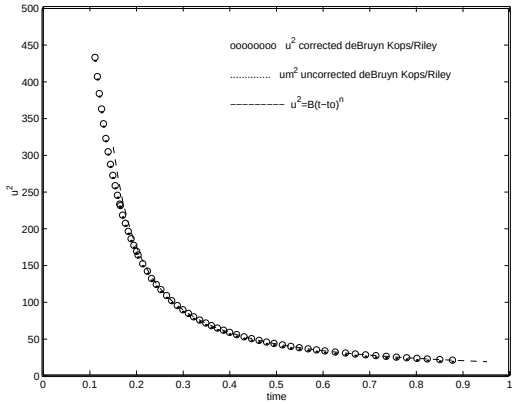

Figure 7: Corrected and uncorrected u^2 versus t for de Bruyn Kops/Riley [2] data.

times is problematical, but probably due to the fact the energy that would have gone to the low wavenumbers has been redistributed. The horizontal line is the value computed from equation 21 by Wang and George [9] using $n = -1.5$ which corresponds to $2A = 6.67$.

Figure 6 by contrast shows an extensive region for which $d\lambda^2/dt \approx constant$. The slow roll-off for very large times of the original data from the integrated spectrum is slightly overcorrected, most likely a consequence of the breakdown of the homogeneous approximation at this long time. The horizontal line corresponds to the value $n = -1.17$ used by Wang and George [9] which yields $2A = 8.54$ from equation 21.

Thus, for at least part of the time, these simulations are clearly consistent with the a power law decay (with constant n), and also with equation 21. Since the latter depends explicitly on the assumption of homogeneous turbulence, it seems reasonable to conclude that these flows are acceptable approximations for homogeneous turbulence — at least once correction is made for the missing large scales. Equally, it makes no sense to use data outside these regimes to validate (or invalidate) theories of homogeneous turbulence, at least those theories which depend on the assumption that $n = constant$.

Figures 7 and 8 show plots of the corrected and uncorrected de Bruyn Kops/Riley data for the energy and Taylor microscale, along with the theoretical relations. The relative rms error between corrected data and theory over the region from $0.35 < t <$

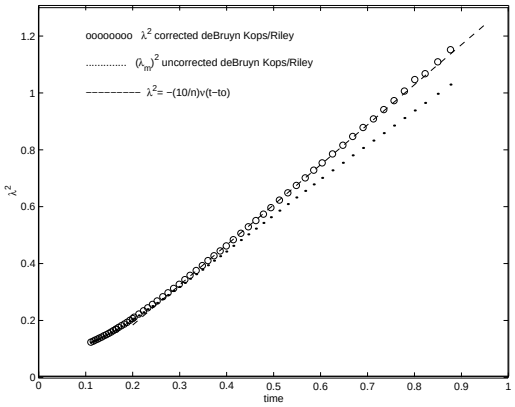

Figure 8: Corrected and uncorrected λ^2 versus t for de Bruyn Kops/Riley [2] data

0.7 (for which the power is constant) is less that 0.2 %. What is important to note about these plots is that the only adjustable parameter is the virtual origin, $t_o = 0.072$ (which is same for all quantities), since the power was determined by the plots of $d\lambda^2/\nu dt$ and equation 21. This considerably constrains the possible fits. Without this constraint it is still possible to fit these same data with very different power laws to almost the same accuracy, even for the region where $d\lambda^2/dt \approx constant$. But these other powers will not yield the value of $10/n$ for the constant, thus failing to provide internal consistency.

How about the integral scale?

As expected, the biggest differences between corrected and uncorrected data by far are for the integral scale. Figure 9 shows the variation of integral scale with time. The uncorrected data follow a power law growth with exponent below 0.4, but the corrected data are within 0.24 % of the same square root dependence of the Taylor microscale. This is even more evident in the plot of L/λ shown in Figure 10 for the corrected and uncorrected data. This plot is of considerable interest (at least to these authors) since the George theory [6] predicts this ratio to be constant, but dependent on the initial conditions. For the corrected data, this ratio is constant at 3.40 to within 0.16 % for the region for which $n \approx constant$, and to within 0.4 % for the entire time of the calculations! Clearly for the de Bruyn Kops/Riley [2] data this ratio is remarkably constant, at least when corrected for the missing lowest wavenumbers.

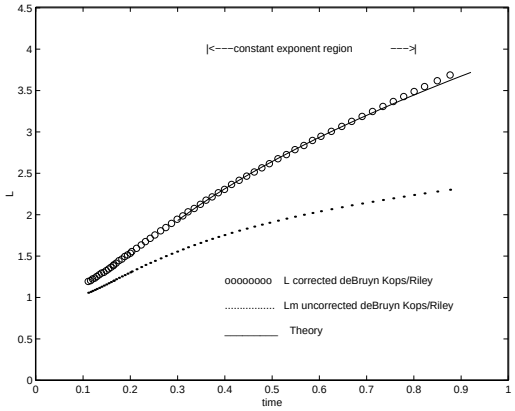

Figure 9: Corrected and uncorrected L versus t for de Bruyn Kops/Riley [2] data.

What happens when the box is truly too small?

How can two simulations, both of which appear to have been carefully carried out, have produced such different results. It is, of course, understandable that both take different routes to achieve the expected power law state, since their initial conditions were very different. But it is not immediately obvious why one simulation remains in that state, while the other does not.

Also, correction of experimental data for any reason is at best a risky business, since it is very easy to have assumed the answer. Certainly the model spectrum of equation 9 itself does not bias the data toward the similarity theory of [6]. Even so, p had to be chosen using the similarity relation (equation 10), since it could not be determined directly in view of the missing lowest wavenumbers. Thus even though extensive consistency checks were performed iteratively, the results must be regarded as tentative until subjected to definitive tests.

Wollblad et al. [11] report a first such effort, but using three rather small simulations, 32^3 and 64^3. All three used identical starting spectra proportional to $k^2 exp(-Ak^2)$. The peak wavenumber was placed in exactly the same place, but the ratio of peak wavenumber to lowest wavenumber was exactly half for the 32^3 simulation compared to the 64^3; i.e., $k_p/k_L = 3$ and 6 at the beginning of the simulation. For one of the 64^3 simulations, the energy transfer was deliberately blocked to all wavenumbers greater than half the highest wavenumber; the other two were resolved to wavenumbers well above the Kolmogorov wavenumbers, and these are of primary interest here. A nominal value of R_λ for all three simulations was 35.

Figures 11 and 12 show the energy spectra for the well-resolved 64^3 and 32^3 respectively, both normalized using u^2 and λ. No corrections were made to either set of data. Figures 13 and 14 show the same data plotted as E/k, also normalized by the same u^2 and λ. The effects of the low wavenumber cutoff are dramatic. The spectral collapse is affected at all wavenumbers, but especially at the lowest wavenumbers. Energy which should have gone to the largest scales has obviously ended up somewhere else. Clearly the 32^3 spectrum is not representative of homogeneous turbulence. The 64^3 spectrum, on the other hand behaves very similarly to the much larger simulation of [2] described above. The causes are quite evident from Figure 15, which shows the evolution of k_p/k_L for the three simulations.

The effects of these spectral differences on the integral scale, L and the ratio of L/λ are demonstrated in Figures 16 and 17. The integral scale for the 32^3 does not grow as fast as the 64^3 sim-

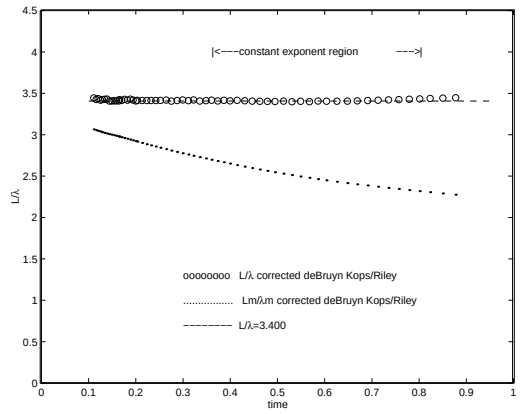

Figure 10: Corrected and uncorrected L/λ versus t for de Bruyn Kops/Riley [2] data.

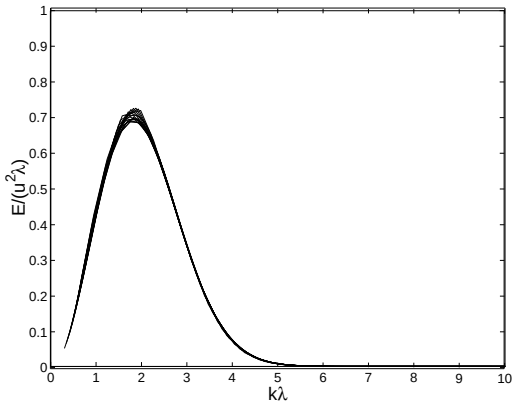

Figure 11: $E(k)/u^2\lambda$ for 64^3 DNS data of Wollblad et al. [11].

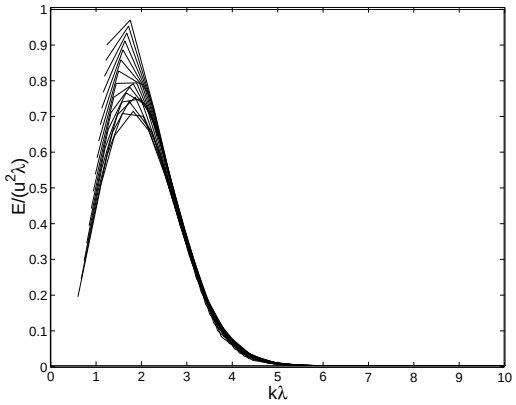

Figure 12: $E(k)/u^2\lambda$ for 32^3 DNS data of Wollblad et al. [11].

ulations, and in fact the two resemble closely the differences between the corrected and uncorrected de Bruyn Kops/Riley data shown above. The ratio of L/λ is nearly constant for the 64^3, dropping very slowly as the ratio of spectral peak to lowest wavenumber decreases. By contrast, the ratio of L/λ drops noticeably for the 32^3 simulation, much like the uncorrected data of de Bruyn Kops/Riley shown above.

Figure 18 shows $d\lambda^2/\nu dt$ for all three simulations. For the well-resolved 64^3 simulation, the value is very nearly constant at $-10/n$, corresponding to a constant power exponent of $n = -1.5$. This value of n is exactly what would have been expected from equation 10 if the spectrum maintained its initial low wavenumber behavior of $p = 2$ throughout decay. By contrast, the 32^3 simulation drops rapidly and shows no constant power exponent region, consistent with the arguments earlier that the redistributed energy has increased the decay rate. Finally, the curve labelled 'truncated' shows what happens if the energy flux to highest wavenumbers is also blocked, and in fact resembles somewhat the early time behavior of Figure 5. In both cases, the high wavenumber resolution improves with time and the untruncated results are eventually obtained, but only near the very end of the calculation.

Summary and Conclusions

The goal of this paper was to review the state of homogeneous decaying turbulence. It has been strongly suggested that we may not have been doing things entirely correctly in our attempts to realize such flows. In particular it has been argued that the large scales are much more important than previously

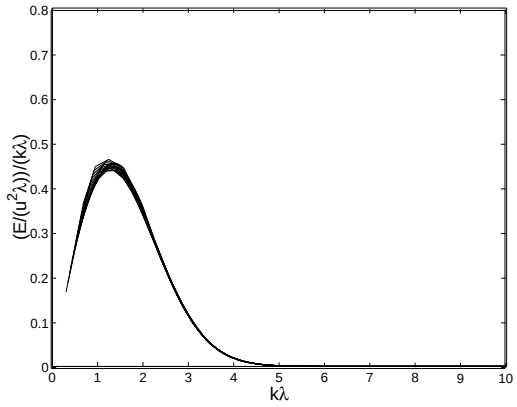

Figure 13: $E(k)/k$ normalized by $u^2\lambda$ versus $k\lambda$ for 64^3 DNS data of Wollblad et al. [11].

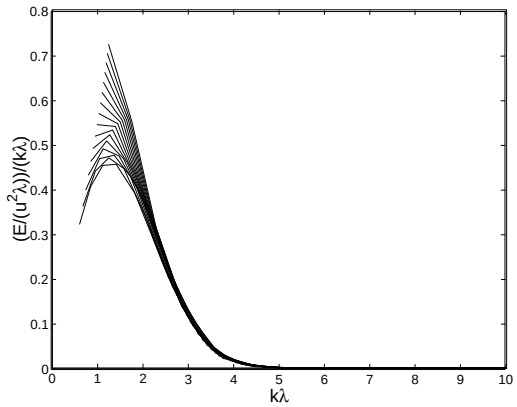

Figure 14: $E(k)/k$ normalized by $u^2\lambda$ versus $k\lambda$ for 32^3 DNS data of Wollblad et al. [11].

believed. This is especially true for the integral scale determination, but also for the overall energetics of the flow as well. If these conclusions are correct and the treatment of the data sound, the theory of homogeneous turbulence is in a much better state than might have previously been believed. This includes not only the formal relations which appear in Batchelor [1], but the recent similarity theory of George [6] as well.

By contrast, there seems to be much left to be done for the experimentalists and DNS'ers. But nothing will be contributed by simply producing data and doing curve fits. The field has moved beyond this. Unless very large experimental facilities and very large computers suddenly come along, progress will only be made by careful interplay between theory and experiment in the manner demonstrated herein. The differences between current theories vary by very small powers of time (e.g., $t^{0.1}$ typically for the integral scale). This is on the order of all the other factors that affect the determination of turbulence quantities, like resolution, box-size and background noise.

Most advancements in science raise as many questions as they answer. Such is the case here. How, for example, can turbulence remember its initial conditions? Or as important technologically, how can we account for it in our engineering models? Must we move to LES, or can a single point structural model (like that being advocated by W.C. Reynolds) account for these effects? Thus, the state of the field is this. Never have we had more questions to answer, nor more hope of finding the answers.

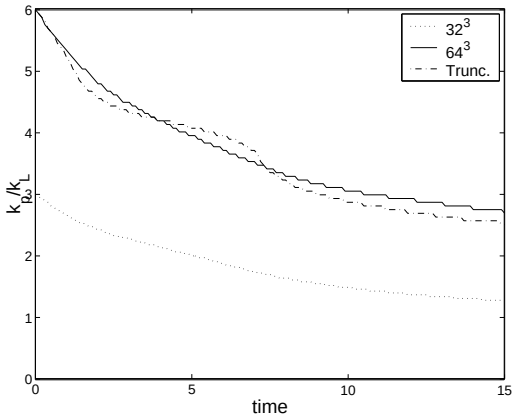

Figure 15: k_p/k_L versus t for DNS data of Wollblad et al. [11].

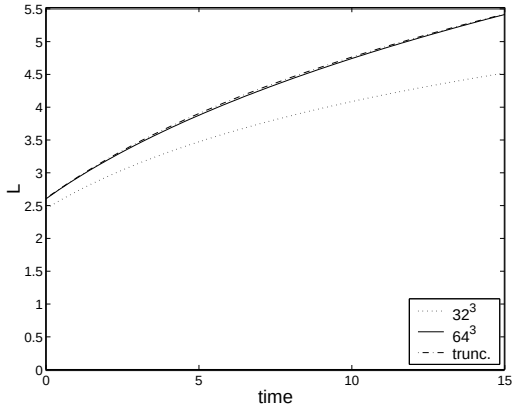

Figure 16: L versus t for DNS data of Wollblad et al. [11].

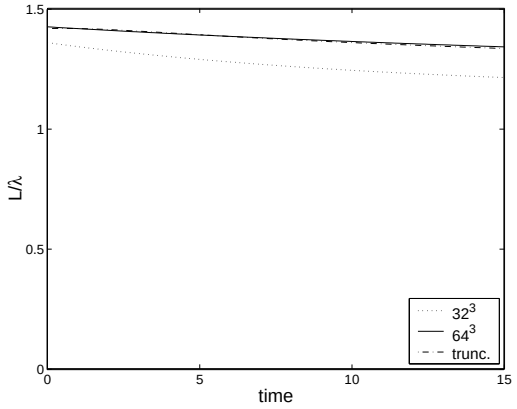

Figure 17: L/λ versus t for DNS data of Wollblad et al. [11].

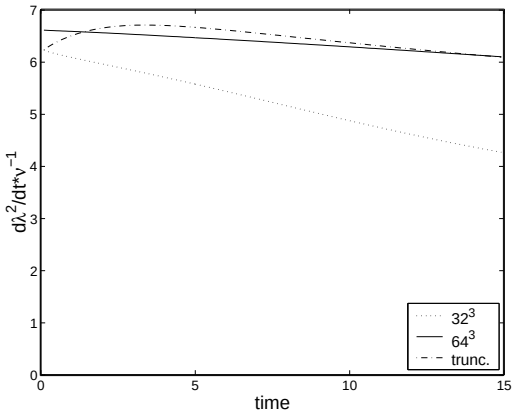

Figure 18: $d\lambda^2/\nu dt$ versus t for DNS data of Wollblad et al. [11].

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