Chapter 2
A 50-Year Retrospective and the Future

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2.1 Big Data

Experimental turbulence research has always been about "Big Data"—and usually never enough of it. Part of the reason has been because of the need to use statistical measures. Data records measured in thousands of time integral scales are necessary to make even the simplest estimators converge, sometimes even tens and hundreds of thousands of integral scales in length for probability density functions and correlations at large lags. As a general rule the time (or length of record required) for a given statistical error is proportional to the rms fluctuations of the statistical quantity being estimated divided by the square root of the number of effectively independent realizations of it. Note that the variance of *the quantity being measured* is not the same as the variance of the underlying process. For example, if a second moment is to be measured its variance is $\langle [u^2 - \langle u^2 \rangle]^2 \rangle$, which for a Gaussian process is 3 $[\langle u^2 \rangle]^2$, and can be much larger for non-Gaussian processes which are common in turbulence. The pre-multiplying factor for simple powers of the variance increases rapidly with the order of the moment, so demands on data length can increase very rapidly (v. [15, 29, 30], or appendices of my turbulence notes available at www.turbulence-online.com). The same is true for attempts to measure events of decreasing probability (like the tails of a pdf), since the lower the probability of it being observed, the more "statistically independent" data that must be acquired to measure it. Fractional statistical error, or variability, is the rms fluctuation of the quantity being measured divided by its average or expected value, or the variability of the quantity desired itself. So the higher the variability of the process, the more independent samples are required to estimate it. Quantities with zero mean will always have infinite variabilities, but finite errors. Many a student has thrown away

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excellent statistical estimates as “too noisy” because they failed to realize that the quantity they were estimating was zero. Modern DNS is now encountering these same difficulties that have plagued experimentalists for decades: as Reynolds numbers get higher, simulations must be much longer to examine the rare events which have become of increasing interest.

So whether experiment or numerical simulation, how to store the data for even a short while is a major issue. Long term storage is even more problematic, so often it is easier (at least for DNS) to contemplate doing the simulation again instead of storing the data. Or alternatively, only storing certain “starting points.” Or even more efficient, storing the snapshot POD coefficients at these starting points. Given the difficulty to get an experiment right in the first place and the cost of maintaining the facilities, this is not an option for most experimentalists. But neither is simply storing the data a viable option either, since the march of technology will soon make it unreadable, or simply make it useless to anyone but those who actually recorded it. This is of course a colossal waste, and any efforts (like this meeting) to address ways to preserve data for posterity are laudable.

Sadly I have little to offer as a solution to this problem. In spite of my best efforts, the rush of universities to clear disks and tapes, the urgency of students to get on with their careers, and the simple press of academic survival has meant that some really wonderful data has simply disappeared, or cannot be recovered for use even if preserved. Even preserving a simple email address or website after retirement or change of location seems to be too much of a stretch for modern university administrations. Only it seems to me if there is a national repository and a contractual commitment to use it might there be the possibility of preserving it.

So while I do not have any panacea for the problem of how to keep “big data” once we have acquired it, I do have considerable experience in generating it. The experiments of me and my students almost always stretched the bounds of what was possible at the time we did them. Since that is likely to be true for all turbulence experiments in the future (whether laboratory or computer-generated) the amount of data collected will undoubtedly increase. In the absence of a general solution to the storage problem, it therefore becomes more and more crucial to make sure we use it properly when we have it. And that is the point of this paper.

2.2 What Should We Be Looking For?

There is no point it seems to me to be carrying out large simulations and experiments if we don’t have some idea what we are looking for in the first place. The last thing turbulence needs is more random numbers, more disks of poorly generated data, and more poorly thought-out applications of existing or yet-to-be-developed technology. Or for that matter, more pictures of random intertwined vortices. What exactly are we trying to find? How will we know when we’ve found it?

2.2.1 We Have No "Exact" Solutions

The single biggest problem of turbulence research is that we really have no exact solutions. Similarity solutions for averaged quantities (especially multipoint) should have been particularly useful (e.g., [33, 35, 40]). These have, however, been quite controversial. In large part I believe because most of these early similarity solutions were done incorrectly, usually by making wrong assumptions at the first step or overconstraining the solutions. I have written a great deal about this starting in the late 1980s [10, 12, 14]. Of course these "wrong solutions" did not agree with most experiments. And the few that appeared to were discredited as better experiments were performed. So a mythology was born and took root which argued that such solutions never applied to turbulence. And alternative ideas took over—like that turbulence could never be described by a single length scale, which meant that pretty much any simulation or experiment could be justified if it collapsed in a local scaling over a range of scales. So our field has become cluttered with experiments which claim to be one thing—like jets or decaying turbulence or wakes—but really are flows largely determined by initial and spatially confined boundary conditions.

In the absence of believable analytical solutions, or at least some way to decompose turbulence, there is no way to sort out boundary and initial condition effects from actual dynamical processes. We do have such tools for homogenous flows, even though we seldom use them as such. And (as we shall see below), we have them for several other classes of turbulent flows as well. But first let's look at a field which closely resembles our own, so we can see what we are missing.

2.2.2 Examples from Wave Research of What We Are Missing in Turbulence

My very first research experience was using a small internal wave tank in the lab of Owen Phillips at the Johns Hopkins University. In many ways this set a pattern for much of my career which was to follow. Phillips was at heart an applied mathematician with extraordinary abilities and physical insight. He was also a mesmerizing lecturer, and it was the simple elegance of the sophomore mechanics course he taught which led me to work in his lab in the first place.[1] Phillips' experimental work, mostly on wind waves and non-linear interactions, was pretty much left to his Ph.D. students and undergrads like me.

The original goal of my study was to examine how internal waves at a saltwater/freshwater interface broke and propagated when subjected to a current—which we generated by towing a contraction through the tank. Sinusoidal waves indeed

[1] A fraternity brother and physics student, Ben Wegbreit, who was already working for Phillips, made the suggestion to me that I trade my job in the library sorting books for one in the lab, and for that I have been forever grateful.

steepened, broke, then reformed and propagated as symmetrical disturbances. What we were seeing were multiple solutions to the governing equations which very much resembled the Schrödinger equation. For a budding young physicist the excitement was very real. Parallel experiments at Cambridge in the UK pretty much scooped our focus on the second and higher modes, so nothing on our work was published. But thanks to an elegant analysis by Phillips, and hundreds of meters of 8 mm movie film of aluminum-flake particle paths, we did manage to provide a rather nice analysis and experimental confirmation of how the waves were steepened by the current [32].

It was this synthesis of phenomena we could clearly see, elegant analysis of well-posed equations, and massive amounts of data (for that era at least) which really captured me. I fell in love with waves, and it very much influenced my professional (and non-professional) life thereafter. It was the very need of such theory for turbulence which attracted me to it. And in an interesting way to my linking up with John Lumley.

What is nice about surface water waves in particular is that you can easily see and generate them. Throw a stone into a pond, pull a boat through the water, or just blow on a surface, and all sorts of interesting mathematics take real form before your eyes. They often organize themselves into recognizable forms, and they can be analyzed by equations which are for the most part tractable. The reason is that wave non-linearities are relatively weak—at least compared to turbulence. For surface waves, these non-linearities enter at fourth and fifth order in expansions about the wave slope (amplitude divided by length), so waves can travel great distances without significant modification.

This weak non-linearity implies that any coherent features of surface waves are maintained or destroyed almost entirely by the phase speeds and bandwidth of the Fourier components that are present. The more monochromatic the wave, the farther it can propagate, even if different wavelengths are traveling at different phase velocities. In fact the "visibility" (or lifetime) of a group of deep water waves is entirely determined by the bandwidth of the disturbances comprising it. The reason is that for deep water waves the speed is proportional to the square root of the wavelength. (A deep water wave is one whose length is much less than the water depth.) This is why it is impossible to track a deep water wave on the open ocean for very long by watching it—it just disappears (or comes "unglued") as the different components comprising it propagate through. Shallow water waves whose length is greater than the depth by contrast can be followed indefinitely since all phases propagate at the same speed which is proportional to the square root of the depth of the water. This is why we can follow the surf in at the beach, and why the energy piles up as the depth is reduced and the wave slows down.

Now all of the above is well known, and has been for a long time. Coherent features, clearly visible, and their dynamics completely explained (at least until they start to break) as the action, interaction, and superposition of eigensolutions of the governing equations. The structures themselves are what we see. But it is the behavior of the underlying eigensolutions in the governing equations which enables us to understand the dynamics.

Now what does this all have to do with turbulence? And big data?

2.2.3 How Can We Decompose Turbulence?

Everyone who has ever looked at turbulence sees structures. Artists painted them centuries ago. Children and adults alike are fascinated by them. We see them in moving rivers because of the disturbances they generate. Clouds track them in our skies, even the simple processes of stirring our food and drinks present them to us. But what are they? The last five decades have seen a massive effort to find and quantify turbulence structures. And while these have cured us of any illusions about whether an average flow really exists, they have contributed little to our understanding of turbulence. We still don't know how to write equations for them, much less predict them. We suspect they are important but really can't prove it. We are sure they are a necessary part of turbulence and many believe they must be accounted for if turbulence is to be controlled. But we have made almost no progress in proving the first nor moving forward with the latter.

Many have argued that these structures are vortical, and that we simply need to concentrate on vortex dynamics to understand and predict them. Indeed DNS and PIV coupled with clever vortex recognition algorithms show tantalizing strings of vorticity—usually quite concentrated in strings or sheets with lots of empty space around them. Whether these are related to the large scale coherent features we see is debatable, but the role of vorticity is not! Even so, recognizing these concentrations of vorticity has not been particularly helpful. Thanks to the Biot–Savart law, each vortex feels the velocity of all the other vortices in the field. So the problem is quite complex, even without the complications of viscous effects (like "cut and connect," vorticity diffusion, etc.). Like the water waves above if we had just taken pictures of them, simply studying pictures of what we see in turbulence and labeling it vortical has not led us to a methodology for predicting anything about them.

Our modern quest for coherent structures really started with the Schlieren pictures of a mixing layer [3] and the low Reynolds number near-wall dye boundary layer studies of Kline and co-workers [26]. But even before that Townsend and his co-workers (v. [35]) had noticed that correlation functions seemed to go to zero at large separations and time lags slower than they might have guessed. And they postulated that these were a result of "large eddies," which provided spatial coherence over large distances, but relatively little energy. No dynamic role for these was suggested, so they were quite different than the role postulated for coherent structures in the early 1970s by many (e.g., [21, 22]).

It was in trying to understand and contribute to the original Townsend idea that John Lumley [28] made what I believe was his most important contribution to turbulence. Published in an obscure Russian proceedings in 1967, I first became aware of it scarcely a year later. I was taking my first turbulence course, and was chasing down a paper on the buoyant subrange by Phillips who had presented it at the same meeting. And there in the same volume was Lumley's paper, seemingly the answer to the questions I had been posing above to myself about the differences between waves and turbulence. Little did I know that in a short while we would be working together, first with Lumley as my mentor and thesis advisor, then as my colleague. In order not to interrupt the narrative of this paper, I have placed in Appendix 1 my own mostly personal history of Lumley's idea and my involvement with it. It might be of special interest to anyone struggling to be heard in the hostile world that sometimes turbulence becomes for new ideas.

2.3 Lumley's Great Idea

What Lumley had proposed was an objective way to decompose a turbulent flow—or for that matter, any random process. His stated goal was quite modest—to find an objective way to identify Townsend's "big eddy." What he found was a way to decompose almost any flow into an infinite set of eigenfunctions. What he did NOT find was a unique way to identify any one of them as a large eddy or coherent structure.

It is not clear when Lumley himself recognized this, but most likely it was when Mark Glauser (then my Ph.D. student) and I showed him the results from our round jet mixing layer experiments in the early 1980s (see Figs. 2.1 and 2.2). What was clear to us from our very first results was that our "Lumley decomposition" had led us to a set of eigenfunctions which turned on and off, different eigenfunctions describing whatever structure was there at different points in its life cycle. Unfortunately (for us) a journal editor pretty effectively prevented us from publishing our results archivally. It is with some satisfaction that our meeting papers have exceeded 100 citations anyway (e.g., [16]). And probably not unrelated that Holmes et al. [20] included an entire chapter on our early results.

So what exactly did Lumley propose? This: imagine a random vector field of space and time. If you like pictures, think of time-resolved holographic PIV images

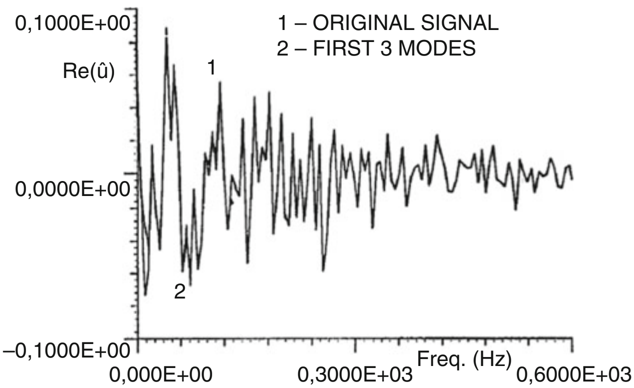

Fig. 2.1 Reconstruction of Fourier transform of instantaneous streamwise fluctuating velocity in center of axisymmetric jet mixing layer at $x/D = 3$ with only three POD modes. POD performed across mixing layer using seven probes. From Leib et al. [17, 27]

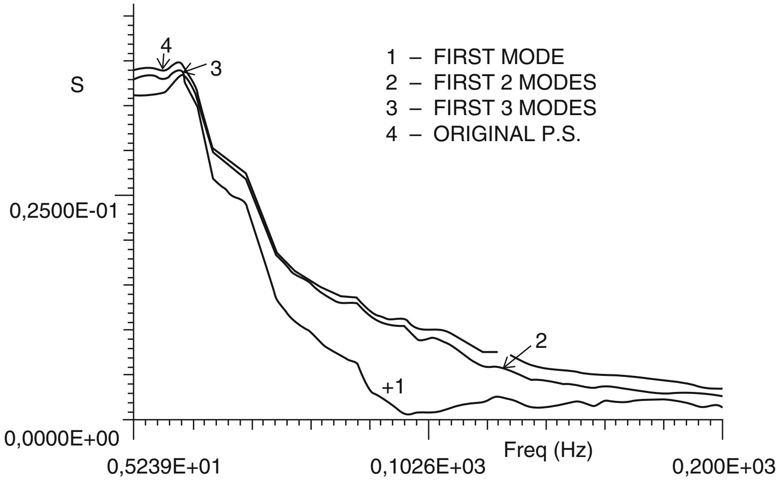

Fig. 2.2 Reconstruction of streamwise fluctuating velocity spectrum in center of axisymmetric jet mixing layer at $x/D = 3$ with only three POD modes. POD performed across mixing layer using seven probes. From Leib et al. [17, 27]

of an entire field in which the vectors are changing with time at every point and different in every realization. Now imagine another vector field that is not random, but is doing its best to track the random one. How could one chose the deterministic field so that it optimally (in some statistical sense) follows the random one?

Now here it is the brilliant part of what Lumley did. And by doing so managed to formulate the problem mathematically so that the solutions could be related to *existing* mathematics—and the Navier–Stokes equations. Lumley proposed to maximize the mean square inner product *in two senses*. First, the usual scalar product of two vectors; but then second, the inner product of two fields in the sense of projections in Riemann space. This was in my mind the great moment (and Lumley's great talent)—taking an idea, and turning it into mathematics. Not even new mathematics, but very old established mathematics *and* statistics. This was new—very new! That he initially misinterpreted what he did (as did most others) is irrelevant. A great idea was born, and a whole new way of thinking about turbulence had begun. Thanks to my brief background in waves, I recognized this immediately, even at this very early point in my career and while still a student. It was therefore a great shock for me to discover subsequently how controversial Lumley's approach had become—even ridiculed (see Appendix 1).

So how does this great idea work? Here it is. If $u_i(\mathbf{x}, t)$ represents the random field of space and time, and $\phi_i(\mathbf{x}, t)$ the deterministic one which is to optimally track it in a mean square sense, then we need to maximize the square of their "double inner product," $\langle|\alpha|^2\rangle$; i.e.,

$$\langle|u_i(\mathbf{x}, t)\ \phi_i(\mathbf{x}, t)|^2\rangle = \langle|\alpha|^2\rangle \tag{2.1}$$

where

$$u_i(\mathbf{x}, t)\ \phi_i(\mathbf{x}, t) \equiv \int \dots \int_{\text{all space, time}} u_i(\mathbf{x}, t)\ \phi_i(\mathbf{x}, t) d\mathbf{x} dt \tag{2.2}$$

defines the inner product in *both* senses. This was the brilliant part! For want of a better terminology I will call this the "*Lumley integral*" or the "*Lumley projection*."

From here on the rest is easy, and could have been performed by even the most mediocre mathematician. From all the hostility the results generated in some quarters (then and even now), and the misuse of them since, it must be presumed that many turbulence researchers qualified (at least then) as less than mediocre mathematicians.

Squaring the integrals, averaging and maximizing the variation of it yields what I have chosen to call the *Lumley Integral Equation*:

$$\int \dots \int_{\text{all space, time}} R_{i,j}(\mathbf{x}, \mathbf{x}', t, t')\ \phi_j(\mathbf{x}', t') d\mathbf{x}' dt' \ = \ \lambda\ \phi_i(\mathbf{x}, t) \tag{2.3}$$

where the kernel, $R_{i,j}(\mathbf{x}, \mathbf{x}', t, t') = \langle u_i(\mathbf{x}, t) u_j(\mathbf{x}', t') \rangle$ is the two-point two-time correlation (or two-point two-time Reynolds stress tensor). Equation (2.3) is sometimes erroneously called the *POD integral*. It most certainly is NOT! There are circumstances under which it reduces to a proper orthogonal decomposition. As we shall be reminded below, more often it does not!

What is important is that the integral converges and does so independent of the boundaries used in practice to truncate its estimation (e.g., field of view, size of tunnel, computational domain, etc.). In fact, it is not at all obvious whether the integral even exists in many turbulent flows; for example, flows which are statistically homogenous and/or stationary. Both of these types of flows are of necessity of infinite extent, and hence have infinite energy. Not surprisingly, these flows behave very differently than flows which are of "finite total energy," meaning that either they are bounded in space or die off rapidly enough in all directions (and time).

Only when the finite total energy property is satisfied in ALL directions AND time does the integral equation yield what can properly be referred to as POD solutions. Failure to recognize this has been the source of much confusion over the years since Lumley first derived it. I have discussed these various conditions in detail in several papers [11, 13], and in several places in my turbulence notes (available on www.turbulence-online.com). And of course pretty much all of the information is there quite cryptically in Lumley's original paper and even more obscurely in his *Stochastic Tools in Turbulence* [29]. In the succeeding sections, I shall review what Lumley told us, and then some of what we have learned since.

2.4 It is All About the Total Energy in the Flow

If the integral of $R_{i,i}(\mathbf{x},\mathbf{x},t,t) = \langle u_i(\mathbf{x},t)u_i(\mathbf{x},t)\rangle$ itself converges when integrated over all directions and time, then the flow is said to be of *finite total energy*; i.e.,

$$\int_{\text{all time}} \int\int\int_{\text{all space}} \langle u_i(\mathbf{x},t)u_i(\mathbf{x},t)\rangle d\mathbf{x}dt < \infty. \tag{2.4}$$

Note that this "finiteness" does not imply that the field itself need be finite, only that the total energy in it is finite.

"Finiteness" is, of course, automatically satisfied if the field is of finite extent or time, so the results of it can be applied to all experiments and computer simulations. But this "finite energy by truncation" can be quite artificial and can yield solutions which have nothing to do with the flow and everything to do with the boundary conditions imposed upon it. Obviously it makes a difference whether we are truncating just the integral by our "field of view" or actually creating the flow in a finite space. Note that a similar phenomenon occurs with spectral analysis when the window is too small relative to the flow integral scale—the resulting spectrum looks more like the Fourier transform of the window than the flow. In optics and signal analysis usually we can evaluate the effect of windows since we know the eigenfunctions. In turbulence we do not—at least without Lumley. All of our experiments and DNS are always of finite total energy, even when we are trying to model flows which are not. So we have much to be concerned about. Since these "eigenfunctions" in principle vary from flow to flow, no general conclusions are possible about the validity of our results without first finding them. Hence what I believe should be the primary goal of our "big data"—making it possible to find the appropriate basis functions for the flow of interest.

2.4.1 Flows of Finite Total Energy

If the energy integral converges, then the solutions are indeed what are referred to as the "classical proper orthogonal decomposition solutions" (or "classical POD"). When most people refer to the POD (or the Lumley decomposition), these are the kinds of solutions they think they are referring to. There can be denumerably infinite eigensolutions, they are orthogonal, and proper (meaning their eigenvalues can be arranged so the lowest order one has the most energy, the next the second most energy, etc.). The sum of the eigenvalues is equal to the total energy in the field; i.e.,

$$\int \cdots \int_{\text{all space time}} R_{i,i}(\mathbf{x},\mathbf{x},t,t)d\mathbf{x}dt = \Sigma_{n=1}^{\infty}\lambda_n \tag{2.5}$$

Finally the two-point two-time Reynolds stress tensor, the original kernel, can be reconstructed like this:

$$\langle u_i(\mathbf{x}, t)\, u_j(\mathbf{x}', t')\rangle = \Sigma_{n=1}^{\infty}\, \lambda_n\, \phi_i^{(n)}(\mathbf{x}, t)\, \phi_j^{(n)}(\mathbf{x}', t') \tag{2.6}$$

Most importantly, any random realization of the field can be represented using appropriate "random" coefficients determined by projecting the entire field onto the eigenfunctions; i.e.,

$$u_i(\mathbf{x}, t) = \Sigma_{n=1}^{\infty}\, a_n\, \phi_i^{(n)}(\mathbf{x}, t) \tag{2.7}$$

where the random coefficients, a_n, are determined by:

$$a_n = \int\int\int\int_{\text{all space}} \phi_i^{(n)}(\mathbf{x}, t)\, u_i(\mathbf{x}, t) d\mathbf{x} dt \tag{2.8}$$

The eigenvalues are given by:

$$\lambda_n = \langle a_n a_m \rangle\, \delta_{mn} \tag{2.9}$$

since the coefficients are uncorrelated.

Thus each realization of the entire instantaneous field can be reproduced from its POD decomposition. But to determine the coefficients requires the entire field at once. This is where the really big data problem enters. The actual determination of the eigenfunctions needs only statistics, which in turn requires only simultaneous measurements at two space-time points. So pairs of points can be considered one pair at a time. But to reconstruct the instantaneous field we need the entire space-time field at once. It should be noted that while this POD representation can in fact describe any existing field, there may also be other types of solutions (non-orthogonal) needed to get to any field from a transient condition. This is no different than the transient versus steady-state solutions of every other differential equation.

It is a common misconception that there are only empirical solutions to Eq. (2.3). A simple analytical example proves otherwise. Assume the two-point correlation for a one-dimensional scalar field to be given by $R(x, x') = R(0, 0)\ \exp[-(x^2 + x'^2)/2L^2]$. Substitution into the Lumley integral yields

$$\int_{-\infty}^{\infty} R(0, 0)\, e^{-[x^2+x'^2]/2L^2}\, \phi(x')\, dx' = \lambda\, \phi(x) \tag{2.10}$$

But the kernel can be separated to yield:

$$R(0, 0)\, e^{-x^2/2L^2} \left\{ \int_{-\infty}^{\infty} e^{-x'^2/2L^2}\, \phi(x')\, dx' \right\} = \lambda\, \phi(x) \tag{2.11}$$

The integral in brackets is just a number proportional to L. So clearly $\phi(x) \propto L\, e^{-x^2/2L^2}$ where the factor of proportionality must be chosen to make the integral of $\phi^*(x)\ \phi(x)$ unity. Clearly in this case there is only a single eigenfunction, and the corresponding eigenvalue contains 100 % of the energy. It should not be at all surprising that a flow might have eigenfunctions that resemble the correlation function, since it might be simply a random superposition of them sprinkled in space.

Unfortunately very few flows are of finite total energy, and in fact none of the "ideal" ones we realize in the laboratory (since most are statistically stationary and many are assumed to be "homogeneous" in one or more directions). A number of investigators, however, not realizing this, have produced data (usually using the snapshot POD) which showed the "POD-modes." And many of them noticed the domain dependence of their data. The reason for this is obvious (at least in hindsight): the finite domain they could see from their PIV (or DNS) was an arbitrary truncation of the energy to make it finite, so indeed it was the domain that determined the eigenfunctions, not the flow. This was *not* what Lumley had in mind! But it is *exactly* the problem we have in all of turbulence—and one of the main points of this paper. In the absence of an exact solution, or at least basis functions, how can we know the degree to which our computational or experimental domains are determining or affecting our solutions?

An example of a flow that truly fits the POD solution might be a mass of turbulence without boundaries which is decaying. But even such a mass (like the sun) is probably spinning. So this imposes a preferred axis with consequent azimuthal homogeneity and periodicities—which like the periodic box turbulence moves it into a different mode of solution—at least in the homogenous periodic direction. And this makes the solutions and coefficients (POD though they may be) dependent on the azimuthal mode number. And since they will be complex, phase is also important. This immediately almost guarantees that any actual "coherent features" or structures will be transient, as they will be comprised of different modes at different times. In other words, they will "evolve"; meaning sometimes you will see them, then other times you won't. Most maddenly they will seemingly vanish before your eyes!

2.4.2 Homogenous Fields of Infinite Energy

In spite of their highly idealized nature, statistically stationary and homogeneous flows are those that have received the most attention theoretically. In particular, homogeneous turbulence which is either forced or decaying, and homogeneous shear flow turbulence. In the former the energy is usually assumed (or derived from similarity considerations) to be decaying either exponentially or as a power law. And in the later to be increasing exponentially in time. Since they are all assumed homogeneous they are of infinite energy in the spatial directions quite independent of the time dimension. Few (if any) have considered such flows in the context of the Lumley integral above. But without understanding the implications of the Lumley integral on these solutions, flows with partial homogeneities cannot be understood.

And it is that lack of understanding which I believe has held our field back for the past half-century. We have looked for things that were not there; and when we have seen things, we often did not understand them.

Most of the flows we create in the laboratory are presumed to be excellent approximations to stationary random processes. And for decades we have attempted to study them by using various grids in wind and water tunnels. While the flows we investigate are of course of finite total energy (since we have truncated them by our experimental boundaries or record lengths), it is important to recognize that our goal is to measure flows that can be interpreted theoretically. Hence, the short-comings of our attempts to create them notwithstanding, the "real" flows are of infinite extent in all directions and time. Otherwise the statistics cannot be independent of origin, a necessary condition for both homogeneity and stationarity. And as a consequence, most "real flows" of interest are of infinite energy, no matter which direction or time is used to examine it.

Since homogeneity and stationarity imply that the integrals are infinite and the flows of infinite energy, then the solutions will be entirely determined by the boundaries we arbitrarily impose on them— *unless there is some property of the kernel itself which makes the integral converge*. Solutions which depend only on how the field is truncated are pretty useless. CFD people are very conscious of these problems, since they are always trying to make the computational domain as small as possible. In my experience experimentalists are generally not nearly as concerned as they should be, and for sure not as concerned as the integral equation above demands they must be. Not unexpectedly, a number of experimenters through the years have noticed this field dependence of their "raw" application of the POD solution of Lumley's integral (or its snapshot version). Few though recognized the reasons.

2.4.3 A One-Dimensional Field of Infinite Energy

That the Lumley integral can converge even with infinite total energy can best be illustrated by a simple one-dimensional example. Consider a stationary random process with the two-time correlation function, $R(t,t') = B(\tau)$ where $\tau = t' - t$. If the limits of integration are infinite, then the Lumley integral equation reduces to:

$$\int_{-\infty}^{\infty} R(t,t')\phi(t')dt' = \int_{-\infty}^{\infty} B(\tau)\phi(t+\tau)d\tau = \lambda\,\phi(t) \tag{2.12}$$

But this can be rearranged as follows (since the integral is only over τ):

$$\int_{-\infty}^{\infty} B(\tau)\left\{\frac{\phi(t+\tau)}{\phi(t)}\right\} d\tau = \lambda \tag{2.13}$$

There is no time-dependence left on the right-hand side, so clearly the solution must have the property that makes the bracketed term t-independent! Only an exponential function has this property, and only if the argument is pure imaginary does it not either blow up or die out.

Clearly this implies that the eigenfunctions for a stationary random process are harmonic functions (sines and cosines or complex exponentials). The "eigenvalues" are just their spectral energy content. And as before we determine the coefficients of the eigenfunctions by projecting the field upon them. But these are just Fourier transforms in the continuous radial frequency variable ω; i.e.,

$$\hat{u}(\omega) = \frac{1}{2\pi} \int_{-\infty}^{\infty} e^{-i\omega t} u(t) dt \tag{2.14}$$

where the 2π is placed before the integral so that the spectrum defined below integrates to the energy.

So statistical stationarity implies Fourier transforms in time. And of course *in the generalized sense* for the instantaneous random fields that are being represented. Statistical stationarity also implies the Fourier coefficients at different frequencies are uncorrelated, so:

$$\langle \hat{u}^*(\omega)\hat{u}(\omega')\rangle = F(\omega)\delta(\omega' - \omega) \tag{2.15}$$

where $F(\omega)$ is the "spectrum" defined as just the Fourier transform (in the ordinary sense) of the two-time correlation, $B(\tau)$; i.e.,

$$F(\omega) = \frac{1}{2\pi} \int_{-\infty}^{\infty} e^{-i\omega\tau} B(\tau) d\tau. \tag{2.16}$$

2.4.4 A Homogeneous and Stationary Random Field

So what does this imply if the field is homogeneous and stationary? Then the optimal representation is also harmonic functions and the coefficients are Fourier transforms *in the sense of generalized functions*, but of *four* dimensions; i.e.,

$$\hat{u}_i(\mathbf{k}, \omega) = \left[\frac{1}{2\pi}\right]^4 \int_{-\infty}^{\infty} e^{-i[\mathbf{k}\cdot\mathbf{x}+\omega\ t]}\ u_i(\mathbf{x}, t)\ d\mathbf{x}dt. \tag{2.17}$$

where the $\hat{u}_i(\mathbf{k}, \omega)$ are the Fourier coefficients, which are functions of both spatial wavenumber, $\mathbf{k}$, and radial frequency, ω, both defined over infinite domains. These Fourier "coefficients" tell how much of each Fourier "eigenfunction" is present in the field.

The field can of course be reconstructed using them and the eigenfunctions as:

$$u_i(\mathbf{x}, t) = \int_{-\infty}^{\infty} e^{+i[\mathbf{k}\cdot\mathbf{x}+\omega\ t]}\ \hat{u}_i(\mathbf{k}, \omega)\ d\mathbf{k}d\omega. \tag{2.18}$$

Homogeneity and stationarity both dictate that the Fourier coefficients in non-overlapping bands are uncorrelated; i.e.,

$$\langle \hat{u}_i^*(\mathbf{k}, \omega)\hat{u}_j(\mathbf{k}', \omega')\rangle \ = \ F_{i,j}(\mathbf{k}, \omega)\ \delta(\mathbf{k}' - \mathbf{k}, \omega' - \omega) \tag{2.19}$$

where $F_{i,j}(\mathbf{k}, \omega)$ is the four-dimensional cross-spectral tensor which itself is the four-dimensional Fourier transform (in the ordinary sense) of the two-point two-time cross-correlation (or Reynolds stress tensor) given by:

$$B_{i,j}(\mathbf{r}, \tau) = \langle\ u_i(\mathbf{x}, t)\ u_j(\mathbf{x} + \mathbf{r},\ t + \tau)\rangle \tag{2.20}$$

$F_{i,j}(\mathbf{k}, \omega)$ and $B_{i,j}(\mathbf{r}, \tau)$ are well known to be a four-dimensional Fourier transform pair; i.e.,

$$F_{i,j}(\mathbf{k}, \omega) = \frac{1}{(2\pi)^4} \int\int\int\int_{-\infty}^{\infty} e^{-i[\mathbf{k}\cdot\mathbf{x}+\omega\ \tau]} B_{i,j}(\mathbf{r}, \tau) d\mathbf{r} d\tau \tag{2.21}$$

and

$$B_{i,j}(\mathbf{r}, \tau) = \int\int\int\int_{-\infty}^{\infty} e^{+i[\mathbf{k}\cdot\mathbf{x}+\omega\ \tau]}\ F_{i,j}(\mathbf{k}, \omega) d\mathbf{k} d\omega \tag{2.22}$$

where $\mathbf{r} = \mathbf{x}' - \mathbf{x}$ and $\tau = t' - t$ are the spatial separation and time lag, respectively. This is, of course, just the classical Wiener–Khintchine theorem stating that in homogeneous and stationary random fields the Fourier coefficients at different wavenumbers and frequencies are uncorrelated.

So these are exactly like the Fourier decompositions of the random water waves (or acoustic or optical waves) described in Sect. 2.2.2 above. And it gives a rationale for books on homogeneous turbulence which seem to have almost arbitrarily chosen to represent homogeneous fields by continuous Fourier modes.

Now there are a several things we can surmise immediately if a homogeneous turbulent field of uniform density were to be decomposed into these optimal Fourier modes. First, consider what happens after transforming in four dimensions the $u_j\ \partial u_i/\partial x_j$ term in the instantaneous velocity fluctuation equation. Since the Fourier transform of a product is simply the convolution of its Fourier transforms, then the four-dimensional transform is

$$\begin{aligned} &\frac{1}{(2\pi)^4} \int_{\mathbf{x}}\int_t e^{-i[\mathbf{k}\cdot\mathbf{x}+\omega t]} \left[u_j(\mathbf{x}, t)\ \frac{\partial u_i(\mathbf{x}, t)}{\partial x_j} \right]\ d\mathbf{x}dt \\ &= \int_{\mathbf{k}'}\int_{\omega'} \left[-i\mathbf{k}_j'\hat{u}_j(\mathbf{k}', \omega')\hat{u}_i(\mathbf{k} - \mathbf{k}', \omega - \omega') \right]\ d\mathbf{k}' d\omega \end{aligned} \tag{2.23}$$

where all integrals are over the infinite domain. Thus *all* the non-linear interactions are among triads of the four-dimensional wavenumber–frequency combinations $(\mathbf{k}, \omega)$, $(\mathbf{k}', \omega')$; and $(\mathbf{k}' - \mathbf{k}, \omega' - \omega)$.

That there are interactions among triads of wavenumbers of Fourier decomposed homogeneous fields has been well known since the 1940s at least. But that frequency also interacts as triads of frequencies does not seem to have been previously noticed. *What this also means is that unless there is some mechanism to make the phases of these superimposed eigenfunctions lock together, then a stationary random field can have no definitive lasting (or coherent) structure.* The reason is that the phases will cancel each other out as the eigenfunctions of different wavenumbers and frequencies move and evolve. Or said another way, analysis of a field with time-dependent Fourier coefficients (as is common), say $\hat{v}_i(\mathbf{k}, t)$, will be very different from a complete four-dimensional analysis using $\hat{u}_i(\mathbf{k}, \omega)$ since any complex phase frequency information in the latter will be smeared out in time in the former. This could have important implications for our studies using forced turbulence since it is both homogeneous and stationary (and in fact the only flow we know that is). I know of no attempts to look at four-dimensional triadic interactions, but they could be very interesting as noted below.

Turbulence in this regard is very different than a homogeneous field of surface water waves, for example, where the dispersion relation between wavenumber and frequency means different wavenumbers propagate at different speeds. To the best of my knowledge no such general relations for turbulence exist. Maybe structures can form and persist anyway. But without including frequency in the analysis along with wavenumbers we will never really be able to tell. For example, one question which might be asked is: when we see these vortical structures in DNS of forced turbulence, how do they evolve and how long do they persist? And what Fourier wavenumber–frequency combinations make them up? This we know from wave theory: if these vortices are spatially compact, they must have a very wide spectrum of contributing wavenumbers. But if they persist for long times (say relative to the Kolmogorov or Taylor microtimes), then they must have very narrow bandwidth in the frequency domain. Persistence time will be inversely proportional to frequency bandwidth, just as spatial localization corresponds to a broad spectral content in wavenumber space. The wider the bandwidth, the less localized the disturbance is in either space or time. So the whole idea of associating small scales with high wavenumbers and frequencies is intrinsically wrong. If one is localized in space, then a broad band of wavenumbers must be in play. If it persists for a long time, however, the spectral band in frequency must be quite narrow. (Note that in a real flow, "frequency" becomes a bit garbled by advection, so frequency is probably best thought of in a moving frame. Or by removing $\mathbf{k} \cdot \mathbf{U}$ from it where $\mathbf{U}$ is the mean velocity; i.e., $\omega - \mathbf{k} \cdot \mathbf{U}$.)

2.4.5 Homogeneous Periodic Flows

Homogeneous periodic flows are a special case as was noted by Lumley [28]. (See also [11, 13], the latter of which shows how the above relate to them.) The eigenfunctions can similarly be shown to be Fourier modes, but this time at frequencies which are integral multiples of the fundamental (or inverse of the period). This is of course just classical Fourier series.

Note that it is commonly assumed that when we model homogenous flows as periodic, then "real" turbulence must also be periodic. This is false. There is a fundamental difference between a truly periodic flow, and one which we have only imagined as such. The difference is whether the flow has been "windowed" or not. Periodic flows are not "windowed." But flows artificially treated as periodic are. The differences can be quite important. For example, it is quite common to analyze finite records of time series of statistically stationary flows by assuming them to be periodic. This is incorrect, and inverse transformation leads to estimates of the correlations that are wrong unless the window is included.

2.4.6 Mixed Flows

Flows that we commonly encounter can be a mixture of all the possibilities above: inhomogeneous and of finite energy in one-direction, homogeneous in one (or more) directions, perhaps periodic in one and stationary in time. This is in fact pretty close to flow in a turbulent pipe, and fairly typical of many axisymmetric flows we generate in the laboratory. Axisymmetric jets and wakes have received particular attention (e.g., [4, 8, 9, 16, 23–25, 34, 36, 41]). Even high Reynolds number turbulent boundary layers have been investigated using Lumley's approach [37].

The easiest way to address these flows is to carry out the Fourier decompositions first, usually on the instantaneous velocities; then carry out the Lumley integral in the inhomogeneous and non-periodic directions using the cross-spectral tensors computed from them. Finally determine the random POD coefficients by projecting the resulting eigenfunctions onto realizations of the random Fourier coefficients. For example, imagine the flow to be homogeneous in x, periodic in an azimuthal coordinate θ, stationary in time t, and inhomogeneous in a transverse coordinate y. The corresponding Lumley projection is

$$\int_{\text{finite energy direction}} F_{\alpha,\beta}(y, y'; k, \omega, m)\psi_\beta(y'; k, \omega, m)dy' = \lambda(k, \omega, m)\ \psi_\alpha(y; k, \omega, m) \tag{2.24}$$

where the subscripts α, β indicate the appropriate components in the various directions (streamwise, azimuthal, and transverse). Only the triple transform of the instantaneous velocity can be constructed by summing up its POD modes, all of which are functions of not only y, but also the wavenumber, k, the frequency, ω, and the periodic mode number, m; i.e.,

$$\hat{u}_\alpha(y;k,\omega,m) = \sum_{n=1}^{\infty} a_n(k,\omega,m)\ \psi_\alpha^{(n)}(y;k,\omega,m) \tag{2.25}$$

The velocity itself (or some portion) of it can only be obtained by inverse transformation over k, ω and inverting the Fourier series for each harmonic component m. Since these Fourier coefficients are complex, this can yield results that look nothing like the individual eigenfunctions. And reconstructions and inferences from partial decompositions may look totally different.

It should be easy to see from this example why simplifying the decomposition to make the random coefficients a function of time only most likely will not succeed, since the various Fourier contributions from k, ω, m to the complex random coefficient $a_n(k,\omega,m)$ can change very much which POD modes contribute to the sum as various Fourier modes turn off or on, or propagate through. In general, homogeneities, stationarity, and periodicities tend to "destroy" the very coherent features that they have built up. This is very much like the disappearing deep water wave groups described earlier. It is probably more correct to think of coherent features in turbulence as "*groups*" rather than structures, "groups" whose lifetime and visibility depend on the frequency bandwidth of the Fourier components comprising them. Or they may act more like solitons and actually propagate as fixed form solutions of the equations. In the absence of suitable eigenfunctions to study turbulence we simply can never know.

Unfortunately it has taken us (me in particular) decades to realize the source of the problem with "partial dimension" decompositions—which in fact describes all our experiments to date (and those of others as well). While those applications of Lumley's methodology using partial-dimension decompositions have given us considerable insight into many flows, they have not produced for us the eigenfunctions we sought. The reason is that no partial decomposition can avoid the phase-scrambling that comes from ignoring even one dimension. This is even more of a problem with the snapshot decomposition as noted in Appendix 2. The good news is that we have now learned enough to know how to carry out a complete four-dimensional Lumley decomposition, especially using the multipoint similarity methodologies described in the next section. And experimental and computational capabilities are now large enough to be able to support it. So my challenge to the generations behind me: do it!

2.5 Extensions Beyond Lumley

There is little in the preceding section which was not in Lumley's original paper, or at least could not be inferred from it. But part of my life-long quest has been to extend the same kind of analysis to flows for which there was no obvious solution to the Lumley integral, nor any reason to believe such solutions might exist. All of these extensions involve similarity solutions to the actual governing equations, equations which were only tangential to Lumley's decompositions. But for the extensions described briefly below, it is similarity solutions to the actual two-point two-time equations which provide the key to solving the integral, and indeed to whether or not it is solvable at all. Amazingly, it turns out that in these cases at least, the integral not only has solutions, they are analytical. So the real challenge for the future for these flows becomes not to find empirical eigenfunctions from knowing the flow, but to use the analytical ones to calculate it. Two examples are described in the following sections, the second for the first time.

2.5.1 Partially Inhomogeneous Flows of Infinite Extent and Infinite Energy

Since this example has already been partially published elsewhere, I shall keep this discussion brief. But I would call attention to the paper on the fully developed jet by Velte et al. [39] in this volume which discusses some recent extensions of this work and summarizes it nicely.

Inhomogeneous flows of infinite extent were not a case considered by Lumley, but it was always a major interest for me and my students because of our interest in turbulent jets exhausting into a quiescent environment. And it should be a major concern for anyone worried about most turbulent shear flows. In the ideal case the jet in the streamwise direction continues forever. If the ideal flow can be assumed to statistically stationary (as opposed to a starting jet), then momentum conservation ensures that total energy associated with that direction is infinite. So the Lumley integral can converge ONLY if some property of the kernel ensures that it does. But what is that property?

The answer is: two-point similarity of the averaged equations [6, 7]. What we found was that two-point similarity of the axisymmetric far jet allowed us to transform the streamwise coordinate logarithmically. As a result the kernel of Eq. (2.3) could be rewritten as $R(x, x') = U_s(x)U_s(x')B(\xi' - \xi)$ where $\xi = \ln x/D$ where D can be any convenient dimension (like jet exit diameter). The result of this is that the transformed flow is now homogeneous in the transformed streamwise direction. And as noted above, a direct consequence is that the eigenfunctions are now Fourier transform modes in the logarithmic coordinate, ξ. *In other words, it was NOT necessary to solve the Lumley Integral Equations empirically, the solutions were analytical!* Note that these results are not approximations, they represent an exact solution of the instantaneous equations.

That real jets behaved this way were confirmed by the extensive experiments of Bettina Frohnapfel (reported in her master's thesis) and in Ewing et al. [7]. Subsequently we were able to confirm these results in the studies at the Danish Technical University by Maja Wänström [41, 42] using both cross-plane and streamwise plane

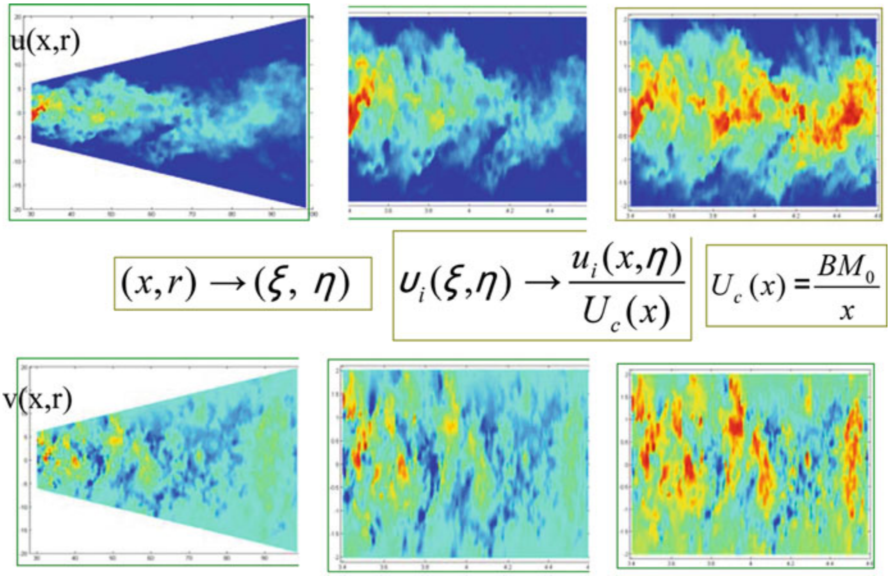

Fig. 2.3 Composite images of streamwise and radial velocities showing homogenization of field by mapping from physical to similarity coordinates. From Wänström et al. [42]

PIV. (As noted above, Clara Velte and Azur Hodzic are continuing these studies at the Danish Technical University.) But more importantly we were able to extend them to the *instantaneous field*. The thesis of Wänström reports these studies in detail. But Fig. 2.3 summarizes the important points. The actual jet measurements from 30 to 100 diameters were transformed using only the local downstream half-width and centerline velocity into a flow which was homogenous in the streamwise direction. And by stretching the streamwise coordinate logarithmically, it was possible to analytically determine the eigenfunctions to be Fourier modes in the logarithmically stretched coordinate. The great streamwise extent of the flow was necessary to avoid windowing when Fourier transforming in the stretched streamwise variable.

Unfortunately we were not able to measure all directions simultaneously. So we ran into exactly the problem that quite surreptitiously complicates most attempts to use various decompositions (or to determine turbulence structures). Even though we had three (of four) dimensions, the missing fourth dimension meant the eigenfunctions were contaminated by the complex phases of the missing dimension. So even though the results were quite robust and repeatable, each partial decomposition gave different profiles for the actual radial dependence of the eigenfunctions. In the light of our discussions above, this is exactly what we should have expected. Efforts are now underway by Velte and Azur and co-workers to use the similarity results and log stretching to perform a DNS of a jet of sufficient length ($x/D > 100$) to allow Fourier analysis in the streamwise direction and decompose all four dimensions simultaneously. Someday, hopefully soon, we will see what the full four-dimensional eigenfunctions really look like.

2.5.2 *Homogeneous, but Non-stationary Turbulence*

This is a particularly interesting class of flows since it includes several of the turbulent flows which have received the most attention: decaying homogeneous turbulence and homogeneous shear flow turbulence. Some years ago a master student of mine at Chalmers (Adam Wachtor, now at Los Alamos) and I considered applying the POD to a DNS simulation of decaying turbulence to see what the eigenfunctions in time would look like. It seemed like the perfect application, since it really was in a box and even of finite total energy in time (since it decayed more rapidly than t^{-1}). Unfortunately he had only one realization to work with. This is because almost all DNS of decaying turbulence compute only a single realization, but then "average" over space (or spherical shells of radius k) to get the time-dependent statistics. Since he could not average we (really he) simply applied the POD to the entire time record and to our great puzzlement (and subsequent amusement) found the entire flow described by just one eigenfunction—which looked exactly like the data. We laughed when we realized that the optimal projection on just one data set is of course just the data set itself. I never told John Lumley about this, but I suspect we were not the first nor the last to do this. In fact a few months later a Danish Ph.D. student showed me exactly the same result applied to the wake of a wind generator. Obviously statistics are an important part of the whole Lumley projection idea. Clearly application of Lumley's methodology can advance only if we have enough realizations to perform reasonable statistics. Unfortunately, except for "forced turbulence," this is seldom the case for DNS. And while experiments often have well-converged statistics, they often have a very limited spatial field, thus either arbitrarily truncating (or windowing) the flow, or limited resolution and not resolving the important scales.

But let's think ahead and presume that someday we will have enough statistical information at sufficient spatial resolution. Here is what I think the lucky person who does this will find—probably from a DNS with many integral scales in the field of view.

First since the three space dimensions are presumed to be homogeneous, then the spatial eigenfunctions will be Fourier modes. Moreover, since the scales grow in time, the energy moves to lower wavenumbers with time. If we further assume the George similarity theory for decaying turbulence to be correct [12, 14], then we know there are single length scale similarity solutions for which the Taylor microscale is the best choice of scaling parameter, and the integral scale is proportional to it. Since for long times at least our length scales grow relative to the computational box (or move to lower wavenumbers in a fixed wavenumber domain), our resolution will improve at the smallest scales in time, but the large scales will try to grow out of the box. The result will be more energy at not just the lowest wavenumbers, but also at the highest wavenumbers because of the triadic interactions (between two high and one low wavenumber). The net result will be to "artificially" increase the dissipation—at least relative to the flow we were trying to create. So to avoid this, what we really need is a simulation which rescales dynamically so the box grows in time with the turbulence length scales. This actually seems quite straightforward, but surprisingly does not seem to have been done. So let's imagine that we have done it. How do we apply Lumley's projection to this field?

If $F_{i,j}(\mathbf{k}, t)$ is the two-time three-dimensional cross-spectral Reynolds stress tensor from the result of decomposing in space first, Lumley's projection integral leaves us with the following integral equation for the time-dependence:

$$\int_{t_o}^{\infty} F_{i,j}(\mathbf{k}, t, t')\ \phi_j(\mathbf{k}, t')dt' = \lambda(\mathbf{k})\ \phi_i(\mathbf{k}, t) \tag{2.26}$$

where the wavenumber part of the three-dimensional cross-spectral tensor is given by the three-dimensional spatial transform of the two-point two-time correlation tensor; i.e.,

$$F_{i,j}(\mathbf{k}, t, t') \ = \ \frac{1}{(2\pi)^3} \int\int\int_{\text{all space}} e^{-i\mathbf{k}\cdot\mathbf{r}}\ \langle u_i(\mathbf{x}, t) u_j(\mathbf{x}+\mathbf{r}, t')\rangle\ d\mathbf{r} \tag{2.27}$$

where $\mathbf{r} = \mathbf{x}' - \mathbf{x}$ is the separation vector in the homogeneous directions. Clearly we need the two-point two-time cross-correlation, $\langle u_i(\mathbf{x}, t)\ u_j(\mathbf{x}', t')\rangle$, or its three-dimensional spatial transform. In spite of all the experiments and DNS of decaying turbulence I am not aware that this has ever been computed.

But there are a few facts which can make our job easier. First, since the flow is homogeneous at every time even though its scales grow in time, we can define a growing coordinate system using $\boldsymbol{\eta} = \mathbf{x}/\delta(t)$ where $\mathbf{x}$ is our space coordinate at time, t, and $\delta(t)$ is our length scale, and $\boldsymbol{\eta}' = \mathbf{y}/\delta(t')$ where $\mathbf{y}$ is our coordinate system at time t' and $\delta(t')$ is the length scale at t'. (Note that $\delta(t)$ can be either the Taylor microscale or the physical integral scale, since they are proportional. But the former is usually more accurately determined.) By examining the two-point two-time Reynolds stress equations (which I have been working on with Clay Byers and Marcus Hultmark of Princeton University) it is possible to show that they admit to an exact similarity solution for all times and spatial points of the following type:

$$\langle u_i(\mathbf{x}, t)\ u_j(\mathbf{x}', t')\rangle = R_s(t, t')\, f_{i,j}(\boldsymbol{\eta}, \boldsymbol{\eta}', t, t') \tag{2.28}$$

By substituting into the two-point two-time Reynolds stress equations, the function $R_s(t, t')$ can further be shown by equilibrium similarity arguments to decompose into two functions, one of t only, the other of t' only, and they are the same functions; i.e., $R_s(t, t') = U_s(t)\ U_s(t')$. It is easy to show that this reduces to the two-point, single time result of [12] if $U_s(t)^2$ is identified as the turbulence kinetic energy per unit mass.

Second, it turns out that the turbulence is *also homogeneous in the scaled space coordinates*. Now we already know that homogeneity in any coordinate (scaled or unscaled) implies that the Fourier modes are the solutions to the Lumley integral in these directions. So our two-point two-time Reynolds stress tensor in scaled space will behave very much as it does in regular space with one important difference—in scaled space wavenumbers, say $\boldsymbol{\kappa}$, the scaled spectra will be fixed. There will be no evolution as no energy moves to lower wavenumbers. The energy will, of course, be moving to lower wavenumbers in physical space wavenumbers, but not in their scaled space counterparts. The computational advantages of using this coordinate system for any simulation should be obvious, and completely eliminate some of the concerns expressed earlier about resolution and box-size by making sure that the scales of turbulence do not change relative to them. This should help immensely in sorting out how things really vary in time.

Third, it also turns out that time should be measured (and discretized) logarithmically. This should have been obvious to us from the single point similarity equations, but for some reason was not. It leaps out at us inescapably from the two-point two-time similarity Reynolds stress equations in similarity variables. So it means we really should be looking at everything from the perspective of things evolving in *logarithmic time*, say $\tau = \ln t$ and $\tau' = \ln t'$.

But it gets even better yet. It is straightforward to show that in fact in these new logarithmically stretched time-coordinates *the scaled statistics are statistically stationary*. To anyone who has been reading and understanding the essential points of this paper, the implications for the Lumley integral for this flow are obvious—and striking! The solutions in logarithmic time are also Fourier modes—Fourier modes of dimensionless frequency and logarithmically stretched time. In other words the solutions are known analytically!

2.5.3 But What If Turbulence Decays as t^{-1}, or More Slowly?

One of the oldest points of discussion in turbulence is about whether homogeneous decaying turbulence should decay as t^{-1}. Many theories suggest it should. All experiments show it decays more rapidly. The reason for the latter is now obvious in light of the previous section: there is no way to do an experiment or DNS which does not have a finite total energy in the field. Even if allowing for the finiteness in the spatial directions, there is no way to feed an infinite amount of energy in. So the energy must decay more rapidly than t^{-1}. So our "theoretical" flow is never obtainable.

In spite of our inability to generate it, there is something even more interesting that we can learn from the theoretical possibility of an infinite energy flow, a BIG BANG TURBULENCE if you like. Let's suppose that the integral of Eq. (2.26) is indeed infinite. Can the Lumley projection still apply? Amazingly the answer is yes—*if the two-time kernel can be shown to be statistically stationary in an appropriately stretched coordinate system*. But this is exactly what we just discovered in the preceding section.

What this means in practical terms is that the solutions in a truly infinite time, infinite energy domain (the flow we think we are analyzing) are fundamentally different solutions than the ones we can realize in the lab or the computer. Thus any attempt to verify infinite domain theory by experiment is doomed to failure—at least unless the differences are recognized and accounted for. Some aspects will be testable, others will not. The point is that the eigenfunctions describing them are fundamentally different.

Before leaving this point, let me note the analogy to standard Fourier analysis. We have long known that windowing affects our ability to do Fourier analysis. The same is true in our fluid experiments but with one difference. We are not really windowing an infinite solution, the flow we have generated inside our finite domains is truly different. It is only at best an approximation to those we analyze theoretically with assumptions which make analysis possible. The great challenge is to understand how the two relate. When is the experimental data telling us the theory is wrong? And when is the theory telling us the experiment is not a valid test? Lumley's Projection seems to have provided a way.

2.6 Conclusions: Similarity and the Lumley Projection Integral

The importance of symmetries for POD has long been recognized (c.f. Holmes et al. [20]). But the interrelation of similarity theory and the Lumley integral for inhomogeneous flows of infinite extent are only just beginning to be explored. The relation of two-point two-time similarity and the Lumley integral has been presented here for the first time. And it raises several interesting questions.

First, it is clear that in flows which are not of *finite total energy*, unless there are symmetries or some type of similarity (of the type exploited above), then there is no possibility of representing (or decomposing) the flow in the manner suggested by Lumley. It is only the additional information that the statistical properties bring to bear that makes the integral solvable.

Second, is it possible that Lumley's optimal projection integral is really more than that? Is it possible that the optimization criterion Lumley has applied, also implies that similarity solutions exist for these flows. Clearly the inverse is true. Two-point two-time similarity in at least the few cases we have considered yields analytical solutions to the Lumley integral equation. In both cases (decaying turbulence and the axisymmetric jet), the optimal bases are Fourier modes in a logarithmically stretched coordinate system. But are solutions of the Lumley type a necessary feature of all flows? And in turn, do they imply similarity in some? If so, this would be a very powerful result indeed, and substantially enhance our understanding of turbulence.

Acknowledgements I want to express my gratitude to the organizers of this conference and those who participated in it. A number of participants, especially Clay Byers and Azur Hodzic, provided helpful comments about the manuscript which were very much appreciated.

One has to be incredibly lucky to have been able to enjoy working in turbulence for five decades. And even more lucky to be honored by his fellow travelers and former students. I am humbled, and truly grateful to all those who have been a part of my life's journey to this point.

Appendix 1: A Brief History of Lumley's Projection and My Involvement with It

History of science is always difficult, since the published literature often does not correspond to the way things happened, and especially since order of publication is often unrelated to the actual chronology. The "first results" are sometimes not published at all, or published much later. As a result, newcomers frequently are misled about who did what and when. This is especially true when an area of research is suppressed, as was the case with Lumley's early work on this subject as well as my own work with my students. Since I joined this area of research just about the time Lumley was losing his enthusiasm for it, and we played some small role in both keeping it alive and re-inspiring him, the following account might be of some value to those struggling in their own isolated corners of turbulence.

With just a few exceptions, Lumley's ideas beginning in the early 1960s were not received with great enthusiasm by the turbulence community. Nils Busch, a colleague and friend of Lumley's in Meteorology at Penn State returned to his native Denmark to set up the Meteorology group at the Danish National Lab (RISOE) and carried Lumley's ideas with him, most notably the Ph.D. thesis of Eric Lundtang Petersen (of Wind Atlas fame) who applied it to turbulent gusts. And Rex Reed at U. Missouri at Rolla (in Switzerland at the time) also saw the advantages, and for years tried to carry out experiments to obtain enough information to apply them. The group at Poitiers under the leadership of Jean-Paul Bonnet embraced it somewhat later, and collaborated extensively with Glauser starting in the 1980s (e.g., [5, 38]). So this particular respect for Lumley's work was very much a part of the reason that the U. Poitiers gave him an honorary doctorate. But most others in the world, and especially the USA, ignored this part of Lumley's work. In part, this was because they didn't understand it. But that alone cannot explain why a few were so openly very hostile to his idea.

Most of this hostility I believe was based on misinformation, in part a consequence of the early applications of Lumley and his students themselves. In particular, Bakewell [2] carried out experiments in the viscous sublayer of the glycerine tunnel at Penn State built especially to apply the decomposition to near-wall turbulence, and Payne [31] applied it to existing measurements of a turbulent plane wake using Grant's measurements [19]. Both had the tremendous disadvantage of working with very little data. Bakewell's near-wall measurements were taken with a single hot-film probe along a single line perpendicular to the wall, with only a single component of velocity and only out to $y^+ = 40$. By using a series of "tricks" to fill in some of the missing component and cross-stream data, they inferred that the near-wall structure might be counter-rotating vortices, and created the schematic seen in many publications. Payne had even less data to work with, but managed to produce a pair of counter-rotating structures which spanned the wake. Unfortunately both of these efforts, instead of stimulating more work, just increased the criticism and the cynicism.

I don't have any accounts of the Moscow meeting where Lumley presented his now famous paper, other than the paper which got my attention as a first year grad student. But I do know a bit about what happened later—mostly from Lumley himself. Lumley's problems with the fluids community's lack of acceptance, or even interest, began with his presentation at the 1967 APS meeting in Hawaii. He felt humiliated by the comments after his presentation, especially by the public criticism from Otto Laporte, and often cited this bad experience as one of his reasons for avoiding such meetings as much as possible—at least in his earlier years. Ironically Laporte did not criticize the ideas themselves, but instead castigated (Lumley's words) him for using dots ("·") instead of symbols for the independent variables $(\mathbf{x}, t)$, even though these were quite commonly used by mathematicians.

But the bigger problem with acceptance came from the emerging coherent structures community, and from Lumley's own misunderstanding of what he had done. The coherent structure people led by the Cal Tech and Stanford groups were observing very active events, but Lumley was still thinking like Townsend's big and mostly passive eddies. The attacks at meetings were fierce, so that by the time I arrived on the scene in the late 1960s Lumley had already lost interest (at least in the fight). His unwillingness to respond to unsolicited attacks often left me in the 1970s as his lone defender at APS and coherent structure meetings, even when he was present. He often thanked me for my efforts to defend his ideas in public, but clearly was disheartened by the need to do so.

By contrast with Townsend's "passive large eddies," and probably thanks to my wave background, I had never thought of Lumley's decomposition as passive. From the very first I saw the dynamic possibilities, and went to Penn State to work with Lumley—in part because of my enthusiasm for working on the decomposition with him. (The other reason was to avoid being sent to Vietnam. Thanks to being hired at Penn State on a Navy contract I was able to avoid a war I did not approve of. And still finish my dissertation at Hopkins, but under Lumley's supervision, thanks to Stan Corrsin's intervention on my behalf.)

Once at Penn State, and somewhat to my disappointment, I instead ended up taking over the non-Newtonian drag-reduction experiments which were underway. And this in turn led to my early work with polymer drag reduction and the LDA. But immediately upon finishing my Ph.D. I set about to reactivate the glycerine tunnel to look for dynamic near-wall events. My very first proposal was to the GHR program of ONR and it was funded in 1972 with Lumley as co-PI. The idea was to include *time* in the measurements as well as multiple velocity components in multiple planes so we could see how things changed in time, not just space. Unfortunately I left for Buffalo before the facility modifications were finished, and the work was ultimately taken over by Siegfried Herzog. Shortly thereafter Lumley moved to Cornell, leaving "my" experiment and Herzog behind to finish it. (Siggy eventually wrote his dissertation at Cornell about 10 years later.) As per normal the experiments had proven more difficult than we had hoped, mostly because of probe and data storage issues. Herzog did complete them, but by this time the DNS efforts of Moin and Kim had caught up, in part because of our interaction with them. Our combined efforts proved important in laying the groundwork for the dynamic systems work of Aubrey et al. [1] later.

At Buffalo starting in 1974 I set out to do the same type of experiments that I had started at Penn State, but in the axisymmetric jet mixing layer. With a modest grant from NSF and some support from AFOSR in a collaborative program with Roger Arndt at Minnesota and Hassan Nagib at IIT, my students and I began the series of experiments which continue until today. Given the acceptance and widespread use of POD-based techniques today, it is hard in hindsight to imagine the hostility we faced at every step of the way. While I had gotten used to the negative proposal reviews, I really was quite surprised at how ready opponents were to make their disdain obvious in public. It was never clear, at least early on, whether the opposition was to Lumley's idea, or to Lumley himself with me as surrogate for their attacks.

The turning point for me came at the 1976 APS meeting in Eugene, OR where in an invited talk, Hans Liepman of Cal Tech went out of his way to trash Lumley's ideas. His specific comment after a brief tirade: "I've never seen any structures just sitting there." There was really no opportunity to question him in the plenary session, but seeing him with a small group at the coffee break which followed I tried to engage him about his comment (quite gently—since I was only 31 at the time). I suggested he was being unfair and clearly lacked understanding of what Lumley had actually done. And I tried to explain briefly why the time-dependence was really all there. Hans was not in a mood to listen and pretty much exploded in my face. By this time a rather larger group of 20 or so had gathered around us, probably smelling my blood. At the most intense moment I felt an arm pushing me aside and Bill Reynolds of Stanford stepped between us. He tapped Hans on the stomach with this program and pointed over his shoulder with his thumb at me and said with a big smile: "The kids's right, Hans!". Liepman said not a word, turned on his heel and left.[2] Needless to say, Bill Reynolds was my hero after that. And while I got my reputation as "controversial," not many took cheap shots in public after that at either me or Lumley or his decomposition.

The real breakthrough in our collective thinking about Lumley's decomposition actually came with the work of Mark Glauser and Stewart Lieb, both Ph.D. students of mine in the early 1980s. My colleague Andres Soom at Buffalo had several MS students design for us a special computer controlled rig to make the measurement

[2]Seven years later I went to Liepman's 70th birthday celebration at Cal Tech, a marvelous event celebrating his life and career. We had not spoken since the meeting in Oregon. When he expressed his surprise at seeing me there, I explained that many of the things I thought I had learned from Corrsin and Lumley actually started with him. And I wanted to personally thank him. He seemed quite appreciative. Indeed he was the bridge between turbulence and the classical physics of Europe. But I never understood his problem with Lumley, nor to the best of my knowledge did Lumley.

program possible—a real novelty in the late 1970s. It was constructed by Scott Woodward, who for many years afterward was an important part of my life and lab. Mark copied an idea from Hassan Nagib of making rakes of probes instead of individual ones. So we had finally both space and time information simultaneously. Figures 2.1 and 2.2 show the results of this decomposition. It was clear to us from the moment we saw the first reconstructed velocity traces that the Lumley integral had produced almost exactly the most dynamic events. And it had done so with only three eigenfunctions.

We showed these very plots to John Lumley during a break in a meeting at Cornell in the summer of 1983 and presented them about the same time [17, 27]. Looking at the plots together was truly the moment I think that John first realized the meaning and potential for what he had done. And it is also this moment I think that his interest in POD was reborn, but recast this time in the context of dynamic systems. Our paths also diverged—no longer was his decomposition about coherent structures or large eddies. He, with the collaboration of Phil Holmes and students Aubrey and Podvin and Bergooz, went for how to model these dynamic events. A whole new field of dynamic systems approach to turbulence was born—about which much has been written (c.f. [1, 18, 20]).

My group, by contrast, continued our quest to find the eigenfunctions and learn how the flow itself put them together. And that quest continues until this day. Not as much as been written, but enough to merit some discussion of its theoretical underpinnings. That is what this article was about—understanding what Lumley's integral truly implies about (and demands of) the underlying flows, whether experimental, computational, or theoretical.

Appendix 2: The Problem with the "Snapshot POD"

The so-called Snatpshot POD was introduced by Sirovich in the early 1980s and has been used extensively since for a variety of purposes. It has been extremely popular for the dynamics system attempts to understand and control turbulence, especially since the pioneering study of Aubrey et al. [1] and the book by Holmes et al. [20]. It is easily implemented if one has many 'snapshots' of data like those commonly produced by PIV. But it is a mistake to confuse it with what has come to be known as the 'classical POD', and it has only a superficial connection to the Lumley decomposition discussed above.

The primary problem with it for real turbulent flows can be demonstrated quite easily. Understanding why there is a problem is a bit more subtle. To simplify things, consider a field of only the spatial variable, x, and time, t. The snapshot POD basically replaces the instantaneous velocity with the following expansion:

$$u(x,t) = \sum_{n=1}^{N} a_n(t)\, \phi^n(x) \tag{2.29}$$

Like the classical POD, the eigenfunctions, $\phi^n(x)$, are still orthogonal and the random coefficients, $a_n(t)$, are uncorrelated at different times. The two-point two-time correlation can therefore readily be computed as:

$$\langle u(x,t)u(x',t')\rangle = \sum_{n=1}^{N} \langle a_n^*(t)a_n(t')\rangle \, \phi^{n*}(x)\phi^n(x') \tag{2.30}$$

The connection to the classical POD (first noted by Sirovich in the early 1980s) comes about by replacing the classical POD integral with its finite difference approximation over space. In most applications this number is quite low so the matrices involved are quite manageable. But when using all the data available from DNS or PIV, these spatial arrays can be very large—typically 10^6 or more. It might be argued, why not just take a smaller number of points—say a subset of those available. Unfortunately this causes serious aliasing, exactly like what happens if a time series is sampled too slowly. Now this can be overcome by spatially filtering the instantaneous data, but this can also be quite computationally intensive.

So enter the snapshot POD. By assuming the flow to be stationary, time averaging can be recognized to also be a summation, not over space but over snapshots. And by comparison the number of snapshots can be quite manageable, thousands instead of millions. Now comes Sirovich's clever trick: interchange the order of summation so the time 'average' is outside the double summation and solve that eigenvalue problem instead. Presto, the classical POD and snapshot appear to have produced exactly the same result! So where is the problem?

Implicit in the derivation of the snapshot POD is the assumption of statistical stationarity, hence $\langle a_n(t)a_n(t')\rangle = F^n(t'-t)$ only. So letting $\tau = t'-t$, we can rewrite Eq. (2.30) as:

$$\langle u(x,t)u(x',t')\rangle = \sum_{n=1}^{N} F^n(\tau)\, R^n(x,x') \tag{2.31}$$

where $R^n(x,x') = \phi^{n*}(x)\phi^n(x')$, There is nothing in principle wrong with this except for the fact that I know of no turbulence which behaves this way. It would be a very rare flow indeed were the turbulence scales uncoupled from the temporal evolution of the flow.

This problem with the snapshot was first pointed out by me and Mark Glauser in 1986 in an APS/DFD presentation, but ignored and even disputed (see note by Aubrey et al. [1]). The failure of near-wall models and other dynamic system models to advance beyond relatively simple or separated flows, I believe can still be largely attributed to this underlying deficiency.

The underlying rationale for the snapshot POD lies in the clever interchanging of order of summation and the numerical approximations to both the Lumley integral and the finite sum arithmetic used to estimate an averaged value. The basic problem lies in the fact that Lumley's optimization applied to a field which is inhomogeneous in space but stationary in time implies that the time-modes are Fourier modes in frequency. And this means all of the spatial eigenfunctions are functions of both space and frequency, not separate functions of space and time. This was one of the most important points of the body of this paper. Similarly, if the field has some directions which are homogeneous and/or periodic, then the eigenfunctions in these directions are Fourier modes, and so in the other directions they depend on frequency and mode number as well as space. Moreover, stationarity implies complex coefficients so that the same eigenfunctions can be used with different phases between them. The snapshot POD cannot reflect this since it is missing information, and simply mixes them.

Now my negativity about the snapshot POD should not be interpreted to mean that I think it cannot be useful. It can be very useful—just not in the sense of Lumley's projection. Sometimes the periodic spatial pieces can be sorted as noted in the work of my students and co-workers and those of Glauser, Tinney, and co-workers. But in general, the time–frequency problem does not appear to be tractable, and this complicates attempts to use it for understanding dynamics. On the other hand, the snapshot POD can be a VERY useful way to sort and reduce data sets. One example previously mentioned was suggested to me by J. Freund who stored snapshot POD coefficients to provide a starting field for large scale CFD computations of a compressible mixing layer well into the run. Another example is from the work of Wänström et al. [41, 42] who used the snapshot POD results to filter and reconstruct the cross-correlation *before* applying the classical POD to the snapshot results.

References

1. N. Aubrey, P. Holmes, J. Lumley, E. Stone, The dynamics of coherent structures in the wall region of a turbulent boundary layer. J. Fluid Mech. **192**, 115–173 (1988)
2. H. Bakewell, Viscous sublayer and adjacent wall region in a turbulent pipe flow. Ph.D. thesis, Department of Aerospace Engineering, Pennsylvania State University, 1966
3. G.L. Brown, A. Roshko, On density effects and large structure in turbulent mixing layers. J. Fluid Mech. **64**, 775–816 (1974)
4. J.H. Citriniti, W.K. George, Reconstruction of the global velocity field in the axisymmetric mixing layer utilizing the proper orthogonal decomposition. J. Fluid Mech. **418**, 137–166 (2000)
5. J. Delville, L. Ukeiley, L. Cordier, J.P. Bonnet, M. Glauser, Examination of the large scale structure in a turbulent mixing layer part 1 : proper orthogonal decomposition. J. Fluid Mech. **390**, 91–122 (1999)
6. D. Ewing, On multi-point similarity solutions in turbulent free-shear flows. Ph.D. thesis, Department of Mechanical and Aerospace Engineering, State University of New York at Buffalo, 1995
7. D. Ewing, B. Frohnapfel, W.K. George, J.M. Pedersen, J. Westerweel, Two-point similarity in the round jet. J. Fluid Mech. **577**, 309–330 (2007)
8. S. Gamard, W.K. George, D. Jung, S. Woodward, Application of a "slice" proper orthogonal decomposition to the far field of an axisymmetric turbulent jet. Phys. Fluids **14**(7), 2515–2522 (2002)

9. S. Gamard, D. Jung, W.K. George, Downstream evolution of the most energetic modes in a turbulent axisymmetric jet at high Reynolds number. Part 2. The far-field region. J. Fluid Mech. **514**, 205–230 (2004)
10. W. George, The self-preservation of turbulent flows and its relation to initial conditions and coherent structures, in *Advances in Turbulence*, ed. by W. George, R. Arndt (Hemisphere (now Francis and Bacon), NY, 1989), pp. 1–41
11. W.K. George, Insight into the dynamics of coherent structures from a proper orthogonal decomposition, in *The Structure of Near Wall Turbulence, Proceedings of the 1988 Symposium on Near Wall Turbulence*, Dubrovnik, ed. by S.K. Robinson et al. (Taylor and Francis, New York, 1990), pp. 168–180
12. W.K. George, The decay of homogeneous isotropic turbulence. Phys. Fluids A **4**(7), 1492–1509 (1992)
13. W.K. George, Some thoughts on similarity, the POD, and finite boundaries, in *Fundamental Problematic Issues in Turbulence*, ed. by W.K.A. Gyr, A. Tsinober. Trends in Mathematics (Birkhäuser, Basel, 1999), pp. 117–128
14. W.K. George, Asymptotic effect of initial and upstream conditions on turbulence. ASME J. Fluids Eng. **134**(6), 1–27 (2012)
15. W.K. George, P.D. Beuther, J.L. Lumley, Processing of random signals, in *Proceedings of the Marseille-Baltimore Dynamic Flow Conference on Dynamic Measurements in Unsteady Flow* (Dantec, Skovlunde, 1978)
16. M.N. Glauser, W.K. George, An orthogonal decomposition of the axisymmetric jet mixing layer utilizing cross-wire measurements, in *Proceedings of the Sixth Symposium on Turbulent Shear Flow*, Toulouse (1987), pp. 10.1.1–10.1.6
17. M.N. Glauser, S.J. Leib, W.K. George, An application of Lumley's orthogonal decomposition to the axisymmetric jet mixing layer, in *Bulletin of American Physical Society, Division of Fluid Dynamics Annual Meeting*, Houston, TX (1983)
18. M.N. Glauser, X. Zheng, C. Doering, The dynamics of organized structures in the axisymmetric jet mixing layer, in *Turbulence and Coherent Structures. From Turbulence 89: Organized Structures and Turbulence in Fluid Mechanics*, Grenoble, Sept. 1989, vol. 2, ed. by M. Lesieur, O. Metais (Kluwer Academic Publisher, Dordrecht, 1991), pp. 253–265
19. H.L. Grant, The large eddies of turbulent motion. J. Fluid Mech. **4**, 149–190 (1958)
20. P. Holmes, J.L. Lumley, G. Berkooz, *Turbulence, Coherent Structures, Dynamical Systems and Symmetry* (Cambridge University Press, Cambridge, 1996)
21. A.K.M.F. Hussain, Coherent structures-reality and myth. Phys. Fluids **26**(10), 2816–2850 (1983)
22. A.K.M.F. Hussain, Coherent structures and turbulence. J. Fluid Mech. **173**, 303 (1986)
23. P.B.V. Johansson, W. George, The far downstream evolution of the high-Reynolds number axisymmetric wake behind a disk: Part 2. Slice proper orthogonal decomposition. J. Fluid Mech. **555**, 387–408 (2006)
24. D. Jung, S. Gamard, W.K. George, S.H. Woodward, Downstream evolution of the most energetic POD modes in the mixing layer of a high Reynolds number axisymmetric jet, in *Turbulent Mixing and Combustion. Proceedings of the IUTAM Symposium*, Kingston, June 3–6, 2001, ed. by A. Pollard, S. Candel (Kluwer Academic Publisher, Dordrecht, 2002), pp. 23–32.
25. D. Jung, S. Gamard, W.K. George, Downstream evolution of the most energetic modes in a turbulent axisymmetric jet at high Reynolds number. Part 1. The near field region. J. Fluid Mech. **514**, 173–204 (2004)
26. S.J. Kline, W.C. Reynolds , F.A. Schraub, P.W. Rundstatler, The structure of turbulent boundary layers. J. Fluid Mech. **30**, 741–773 (1967)
27. S. Leib, M. Glauser, W. George, An application of Lumley's orthogonal decomposition to the axisymmetric turbulent jet mixing layer, in *Proceedings of the 9th Rolla Symposium on Turbulence in Fluids* (University of Missouri-Rolla, Rolla, MI, 1984)
28. J.L. Lumley, The structure of inhomogeneous turbulent flows, in *Atmospheric Turbulence and Radio Wave Propagation*, Nauka, Moscow, ed. by A.M. Yaglom, V.I. Tatarsky (1967)

29. J.L. Lumley, *Stochastic Tools in Turbulence* (Academic, New York, NY, 1970)
30. J.L. Lumley, H. Panofsky, *The Structure of Atmospheric Turbulence* (Interscience, Hoboken, NJ, 1964)
31. F. Payne, Large eddy structure of a turbulent wake. Ph.D. thesis, Department of Aerospace Engineering, Pennsylvania State University, 1966
32. O.M. Phillips, W.K. George, R.P. Mied, A note on the interaction between internal gravity waves and currents. J. Deep Sea Res. **15**, 267–273 (1968)
33. H. Tennekes, J.L. Lumley, *A First Course in Turbulence* (MIT Press, Cambridge, MA, 1972)
34. C. Tinney, M.N. Glauser, L.S. Ukeiley, Low dimensional characteristics of a transonic jet. Part 1. Proper orthogonal decomposition. J. Fluid Mech. **615**, 107–141 (2008)
35. A.A. Townsend, *The Structure of Turbulent Shear Flow* (Cambridge University Press, Cambridge, 1956)
36. M. Tutkun, P.B.V. Johansson, W.K. George, Three-component vectorial proper orthogonal decomposition of axisymmetric wake behind a disk. AIAA J. **46**, 1118–1134 (2008)
37. M. Tutkun, W.K. George, M. Stanislas, J. Delville, J.M. Foucaut, S. Coudert, Two-point correlations and POD analysis of the wallturb experiment using the hot-wire rake database, in *Progress in Wall Turbulence: Understanding and Modeling*. ERCOFTAC Series, vol. 14, ed. by J.J.M. Stanislas, I. Marusic (Springer, New York, 2009), pp. 95–102
38. L. Ukeiley, L. Cordier, R. Manceau, J. Delville, M. Glauser, J. Bonnet, Examination of large-scale structures in a turbulent plane mixing layer. Part 2. Dynamical systems model. J. Fluid Mech. **441**, 67–108 (2001)
39. C.M. Velte, A. Hodzic, K. Meyer, POD mode robustness for the turbulent jet sampled with PIV, in *Whither Turbulence and Big Data 2015*, ed. by A. Pollard et al. (Springer, Berlin, 2016)
40. T. von Karman, L. Howarth, On the statistical theory of isotropic turbulence. Proc. R. Soc. Lond. A **164**, 192 (1938)
41. M. Wänström, Spatial decomposition of a fully-developed turbulent round jet sampled with particle image velocimetry. Ph.D. thesis, Department of Applied Mechanics, Chalmers Tech. U, Gothenburg, 2009
42. M. Wänström, W.K. George, K.E. Meyer, Streamwise and radial decomposition of a turbulent axisymmetric jet, in *Progress in Turbulence and Wind Energy IV, 2010 ITI Conference*, Bertinoro, IT (Springer, Berlin, 2012), pp. 147–150

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