Photograph of John Lumley taken in 1985
at the wedding of Alexander Smits

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John Leask Lumley: Whither Turbulence?

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Abstract

John Lumley's contributions to the theory, modeling, and experiments on turbulent flows played a seminal role in the advancement of our understanding of this subject in the second half of the twentieth century. We discuss John's career and his personal style, including his love and deep knowledge of vintage wine and vintage cars. His intellectual contributions range from abstract theory to applied engineering. Here we discuss some of his major advances, focusing on second-order modeling, proper orthogonal decomposition, path-breaking experiments, research on geophysical turbulence, and important contributions to the understanding of drag reduction. John Lumley was also an influential teacher whose books and films have molded generations of students. These and other aspects of his professional career are described.

1. HIS LIFE AND STYLE

1.1. Biography

John Lumley was born on November 4, 1930, in Detroit, Michigan, and died of a brain tumor on May 30, 2015, in Ithaca, New York. He was the only child of Charles Swain Lumley and Jane Leask Lumley. His parents were immigrants, his father from England and his mother from Scotland. John's father was an architectural engineer and instilled in him a deep appreciation of good design and architecture. According to his daughter Jennifer (personal communication), when his mother

> first came to this country as a teenager, she secured a job in a New York City department store as a human calculator. My dad told me that at the time, it was customary to call someone in this position over when a customer had made her selections. My grandmother would then calculate the totals in her head. . . . I think Dad was proud of his mother's mathematical ability and the care she took in instilling those abilities in him as he grew up—tempting him with trips to the movies that were dependent on his ability to recite his times tables out loud in the car on the way.

His mother was also the likely source of his extensive repertoire of British aphorisms that he sprinkled in his conversations.

The family moved several times during the Depression, finally returning to Detroit. These many relocations were no doubt difficult for a small child. John was given the task of helping with the systematic adjustments in their new living spaces. This included changing out all the faucets to replace hot and cold spigots with temperature-mixing ones, among other similar jobs. John's relationship with his parents was complicated. He respected them and admired their achievements and talents, but they lacked compassion and patience, and could be demanding.

John enrolled in Harvard University in 1948 and received an A.B. in engineering sciences and applied physics in 1952. His interest in statistical physics was piqued by a course taught by the great mathematician Stanislaw Ulam, who was visiting Harvard. He chose to attend Johns Hopkins University for graduate work, primarily because of the attractiveness of their recruiting brochures (or so he said). After receiving an M.S.E. in mechanical engineering in 1954, he switched to the aeronautical engineering program to work with Stanley Corrsin on turbulence, earning his Ph.D. in aeronautics in 1957. After two years as a postdoctoral fellow with Corrsin, he joined the Pennsylvania State University initially as a research professor at the Garfield Water Tunnel of the Applied Research Laboratory, and then as a professor in aeronautics. By age 44, he was appointed Evan Pugh Professor of Aerospace Engineering, the youngest person to hold this title. In 1977, he accepted an offer from Cornell University as the Willis H. Carrier Professor of Mechanical and Aerospace Engineering. He thrived at Cornell, and built the Cornell turbulence group.

John's first research activities upon joining Corrsin's group were in the laboratory. Apparently, that did not go as well as he had hoped, and he moved to a theoretical project. That did go well, and he had found his personal scientific niche as a theoretician. That said, he was always well versed in experiment and wrote papers on instrumentation and experimental methods. Many of the 34 or so Ph.D. candidates who he supervised at Penn State and later at Cornell wrote theses on experimental topics.

While at Harvard, John met Jane French, a student at Radcliffe College. They married while John was a graduate student and their three children, Katherine, Jennifer, and Christopher, were born in Baltimore.

1.2. Scope of His Research

It is difficult to think of a facet of turbulence, whether it is formal mathematical theory, fundamental physics, or engineering and environmental applications, to which John did not make seminal contributions. Although others may have probed as deeply on a particular topic, we can think of no other who has covered the whole gamut, from Hölder continuity to hot-wire circuitry. In each sphere, John's reach was broad. On the applied side, he wrote on drag reduction, buoyant plumes, gravity waves, turbulence interaction, turbulence in the presence of stable stratification, and the effects of electromagnetic fields on turbulence, among other things. He even wrote a paper on flow through a teat canal in a dairy cow (Lissik et al. 1984). John's fundamental contributions span mathematics, stochastic processes, spectral dynamics, and the dynamics and modeling of all the generic flows. He pioneered the proper orthogonal decomposition (POD) approach in turbulence that unambiguously extracts coherent structures from turbulent flows (which, though random, contain structures that occur repeatedly) and orders them according to their energy content. This provides a mathematically optimal description that can be used to construct low-dimensional models of the flows. In this review, we can only touch on a few aspects of his work.

He wrote six books: *Structure of Atmospheric Turbulence* (Lumley & Panofsky 1964), *Stochastic Tools in Turbulence* (Lumley 1970a), *A First Course in Turbulence* (Tennekes & Lumley 1972), *Turbulence, Coherent Structures, Dynamical Systems and Symmetry* with P. Holmes and G. Berkooz (Holmes et al. 1996) [and a second edition with added author C.W. Rowley (Holmes et al. 2012)], *Engines: An Introduction* (Lumley 1999), and *Still Life with Cars: An Automotive Memoir* (Lumley 2005). He edited many more. He also wrote 229 scientific papers and produced and performed in two films in the well-known National Science Foundation series on fluid dynamics.

In addition to his books and papers, he was extraordinarily active in the scientific community in numerous ways, including memberships and chairmanships of many national and international committees and editorial duties for several journals, including over 30 years with the *Annual Review of Fluid Mechanics*, 19 years of which he was Co-Editor or Editor [see the appreciation of John by Parviz Moin and Stephen Davis in the introduction to volume 48 of this journal (Moin & Davis 2016)]. His impact on all aspects of the field was impressive and lasting.

1.3. Behind the Iron Curtain

During the Cold War, Soviet scientists had developed turbulence theory and experiment further than their counterparts in the West. John brought their advances to the attention of Western researchers first by editing English translations of the important two-volume treatise *Statistical Fluid Mechanics: Mechanics of Turbulence* (Monin & Yaglom 1971, 1975). These had to be smuggled out of the Soviet Union. He also edited the translations of the books *Variability of the Oceans* (Monin et al. 1977) and *Turbulence and Atmospheric Dynamics* (Obukov 2001), a collection of A.M. Obukov's early works put together by his colleagues. In his preface to *Turbulence and Atmospheric Dynamics*, John writes in his characteristically elegant style: "He [Obukov] was one of the most creative of Kolmogorov's students and many of the papers in this volume are very pretty mathematically . . . Obukov did not have the reputation as a very clear writer but the science and mathematics I find remarkably fresh and double distilled" (Obukov 2001, pp. xi–xii). In addition, for many years he edited the cover-to-cover English translations of *Izvestiya: Atmospheric and Oceanic Physics*, a transaction series of the Soviet Academy of Sciences.

John made several trips behind the Iron Curtain and was much admired there. His work first caught their attention with his book *The Structure of Atmospheric Turbulence* with Hans Panofsky (Lumley & Panofsky 1964).

1.4. Honors

Among the most prominent of the many honors John received were his elections to the National Academy of Engineering and the American Academy of Arts and Sciences; he was a Fellow of the American Physical Society and a Fellow of the American Academy of Mechanics; he was awarded the Timoshenko Medal of the American Society of Mechanical Engineers, the Fluid and Plasmadynamics Award of the American Institute of Aeronautics and Astronautics (AIAA), the Fluid Dynamics Prize of the American Physical Society, and the Dryden Lecture in Research Award of the AIAA. Symposia were organized in honor of John's sixtieth birthday in 1990 at NASA Langley Research Center (**Figure 1**), with selected presented articles published in *Studies in Turbulence* (Gatski et al. 1992), and in honor of his seventieth birthday in 2001 at Cornell, with selected articles collected in *Physics of Fluids* (volume 14, issue 7).

He also received honorary doctorates from the University of Poitiers and the École Centrale de Lyon. He was especially proud of these.

Figure 1

Group photograph from the 1990 NASA symposium in honor of John Lumley's sixtieth birthday. The authors flank Lumley (*front center*), S.L. to his right and Z.W. to his left.

1.5. Cars

John developed a love for automobiles as a small child that stayed with him for his lifetime. He attended a preparatory school in Detroit together with children of auto company executives. In addition to a fine academic curriculum, the school offered shop courses, including ones particular to the automobile industry, which he appreciated and in which he excelled. Throughout his life, his avocation was the repair of family cars, mostly his family's small fleet of Volkswagen Beetles, and the restoration of classic cars. The six classic cars he restored—a Jaguar, two Armstrong Siddeleys, a Bentley, a Lagonda, and a Franklin—ranged from about 50 to 80 years old. He was a self-taught craftsman, rebuilding cars that arrived at Lumley's Good Enough Garage in poor condition and, on one occasion, in boxes. Sometimes John was willing to include his cars in friends' special events. He used an Armstrong Siddeley to drive one of our sons (S.L.'s) and his bride to their wedding. He arrived to pick them up unexpectedly wearing a chauffeur's cap, which he removed afterwards as he joined the party.

He did all aspects of the restorations himself, including all mechanical work, body work, painting, the fabrication of the interior, and even the cutting and sewing of the leather upholstery and the reconstruction of the interior wood veneer (for examples of some of this work, see **Figure 2**). His work was of high quality, and some of his cars won prizes at major auto shows. Part of the restoration process is captured in his memoir written after retirement, *Still Life with Cars: An Automotive Memoir* (Lumley 2005).

Figure 2

Examples of the classic cars John Lumley restored. (*Top left*) One of two Armstrong Siddeleys. (*Top center*) John and the Bentley. (*Top right*) The Lagonda. (*Bottom left*) The completed Bentley, with John examining the underside. (*Bottom center*) The restored Bentley rear seat. (*Bottom right*) The Lagonda engine compartment.

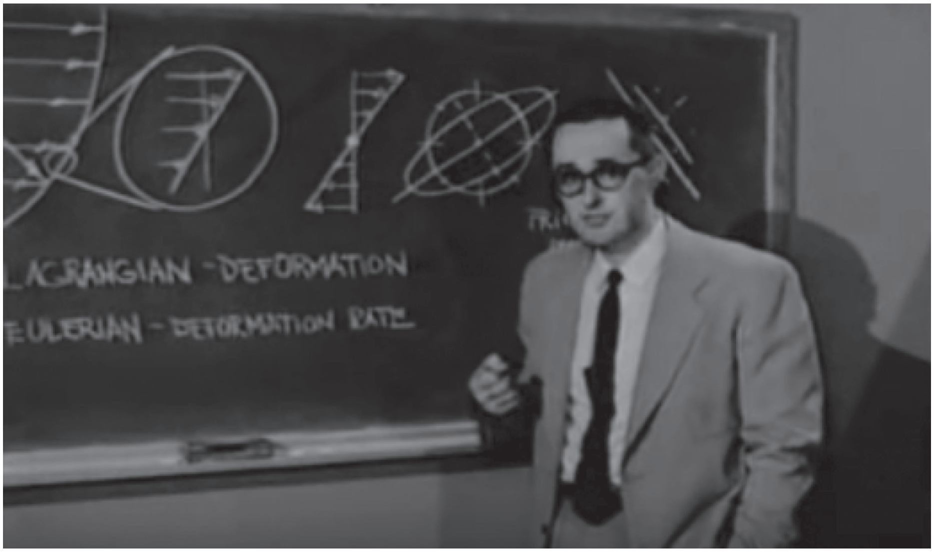

Figure 3

A frame from John Lumley's film *Deformation of Continuous Media* (Lumley 1963a).

Problems frequently arose with the reconstruction process; parts were often not available and he had to make them from scratch himself. Things did not always go smoothly, as the following story John told Dietmar Rempfer illustrates (personal communication):

> I don't remember the make and model, but it was a convertible, and John had to reproduce significant parts of the body and the roof from scratch. He had found a source for the canvas, but it came undyed, so he had to dye the canvas to the required deep black color himself, which he did in their washing machine upstairs. Unfortunately, the washer overflowed, and the Lumleys had that deep black dyeing solution running down through the ceiling into their kitchen. Jane was not happy, of course. When John told me the story he dryly remarked that this was almost "the end of a long and beautiful relationship." Of course, it wasn't, and when I asked Jane about this later it seems that while she was upset at the time at the damage, she had taken it as one of the things that are to be expected as consequences of John's work with his cars.

John had expert knowledge of the history of the automobile and enjoyed talking about it. He was especially interested in the engineering solutions to various subsystems that the designers adopted, some of which he admired, and some not.

1.6. Food and Wine

John and Jane were gourmets, which no doubt was the reason John preferred France as the destination for his sabbatical leaves. Jane taught in the School of Hotel Administration at Cornell and was a restaurant critic for Distinguished Restaurants of North America. Both John and Jane loved to cook and hosted many delightful dinner parties at their home. His appreciation of food and wine led to his service at the various Johns Hopkins dinners, held annually at the American Physical Society Division of Fluid Dynamics. At these dinners, John usually chose the restaurant and made the wine choices. He had no qualms about ordering the wines he preferred, (almost) without regard to price. This left any postdocs or graduate students sitting at his table with serious sticker shock.

1.7. Style

John's curiosity and memory were remarkable, as was the facility for language evident in his writings. Together with his love of reading and sense of humor, these characteristics made conversation with him entertaining and rewarding. Despite this, he was not at ease with those he did not know well and could seem reticent in their company. At other times, he could be testy. Although he had strong opinions about research and rapidly arrived at theories for controversial questions, he was always willing (although not always happy) to abandon a pet theory if experiment proved it untenable.

Although he was not a natural classroom teacher, his books and films provide a lasting testament to his role as an educator. His graduate students, and the many others whose careers John promoted, write of their deep appreciation of his influence. He taught the research method by example: few spoken words and many written words communicated by handwritten notes.

Like many academics, John wrote, taught, gave seminars, made educational movies, and edited journals and books. In each case his style was different.

He did not relish teaching, and throughout his career he did not vary the courses he taught (as many professors do): At Cornell his staples were the beginning graduate course on turbulence, teaching from a *A First Course on Turbulence* (Tennekes & Lumley 1972), and another required graduate course, usually Incompressible Fluid Mechanics (which he sometimes referred to "Incomprehensible Fluid Mechanics"). Latter in his career, he was asked by one of us (S.L.) to substitute for a senior level course on automotive engineering. He agreed, although somewhat reluctantly, and this assignment led to his book *Engines: An Introduction* (Lumley 1999). At Penn State he gave occasional courses on turbulence modeling and on the mathematics that comes into play in turbulence. His book *Stochastic Tools in Turbulence* (Lumley 1970a) contains (in a very terse style) virtually everything a student needs to know to approach problems in turbulence. Although his lectures were rather dry, they were full of insights, and we know many graduate students who sat in on the turbulence course for a second time in order to absorb the nuances. He often did lengthy calculations on the blackboard without notes.

His seminars and conference talks were dense and incomprehensible to all but a few specialists. One felt that he was deliberately trying to obfuscate and that he had a fear of sounding too trivial.

His books are another matter. There is conciseness, clarity, and wit in the writing. Sitting at his typewriter or computer was his happiest work state. Soon after email was invented, he once confided that he was very happy with it because he did not have to talk with people. Spontaneity was not one of his strong points.

In his writing there are good uses of analogies and metaphors. John understood and emphasized the importance of making back-of-the-envelope estimates. Estimates in turbulence cannot be exact, and John spent much time explaining this to students and faculty who often want to work to four or five decimal places. In *The Structure of Atmospheric Turbulence* he explains the meaning of the symbol $\gg$:

> Often in turbulence theory it is convenient to imagine something [very] "large" compared to something else, symbolized by $A \gg B \ldots$. Unfortunately, concepts of negligibility are purely subjective. In ordinary usage a "large" number is one at least as big as a number lying between 3 and 100, that is, somewhere above 10^k, where k may range from 1/2 to 2 or higher. For many purposes 1% is regarded as good accuracy. This corresponds to regarding only numbers larger than 100 as large. A dining table whose legs differ in length by 1% is unusable; one must presume that cabinetmakers regard only numbers larger than 300 as large. (Lumley & Panofsky 1964, p. 220)

We have referred a number of times to *A First Course in Turbulence* (Tennekes & Lumley 1972). Henk Tennekes (personal communication) told one of us (Z.W.) that he wrote chapters 1, 2, 3, and 5, and John wrote the other four. Although the work is well edited to provide a unified read, John's chapters are more difficult and more terse in their exposition than those of Henk. But they are masterpieces of exposition and originality. An example is provided in the section on the energy cascade, where he describes a simple eddy as a wave packet, anticipating later work on wavelet analysis. *The Structure of Atmospheric Turbulence*, written with Hans Panofsky (Lumley & Panofsky 1964), was instantly recognized because it brought rigor to a subject that had been largely empirical.

In the 1960s, the National Committee for Fluid Mechanics Films made a number of movies on fluid mechanics under a grant from The National Science Foundation. The cast of presenters included many of the luminaries of the day: G.I. Taylor, M.J. Lighthill, J.A. Shercliff, S.J Kline, Asher H. Shapiro, Donald Coles, R.W. Stewart, Arthur E. Bryson, Stan Corrsin, Erik L. Mollo-Christensen, and J.E. Ffowcs Williams, among others. John did the first two presentations, *Deformation of Continuous Media* (Lumley 1963a) and *Eulerian and Lagrangian Description in Fluid Mechanics* (Lumley 1963b). These films are available on MIT's TechTV and on YouTube (see links in the Literature Cited). They are models of clarity and attention to detail in the making of the props and experiments. Although in his early thirties (and probably the youngest presenter), many of the characteristics of the older man both of us knew are already there: precision in articulation coupled with a slight impatience at having to state what seemed so obvious. These films probably still provide the best pedagogy in the subject.

As we mention in Sections 1.1 and 1.2, John did extensive editing, and translating from Russian. His editing style tended to show a lightness of touch, but he was quick to pick up on translation as well as conceptual errors. More than once he would come by our offices exasperated at what he saw being published.

2. EXAMPLES OF HIS WORK

In this section, we discuss a few of the many areas to which John has made major contributions.

2.1. Turbulence Modeling

In his article with Akiva Yaglom, John wrote,

> We believe that even after 100 years, turbulence studies are still in their infancy. We are naturalists, observing butterflies in the wild. We are still discovering how turbulence behaves, in many respects. We do have a crude, practical, working understanding of many turbulence phenomena but certainly nothing approaching a comprehensive theory, and nothing that will provide predictions of an accuracy demanded by designers. (Lumley & Yaglom 2001, p. 241)

John saw the prediction of turbulent flows as difficult and largely intractable. Of course, much of his career was devoted to making predictions, but he never lost sight of the immensity of the problem. Turbulence modeling attempts to describe and predict turbulence behavior by seeking approximate equations and relationships that are simpler in form than the Navier–Stokes equations, thereby foregoing some of the physics. It spans from simple scaling arguments to complex closures.

The simplest models are scaling arguments, and John was a master of them. In this sense he was close to the Russian school of Kolmogorov and Landau. Like them, he had immense mathematical abilities but was always attracted by physical insight and intuition as a first step. Perhaps the best example is his book with Henk Tennekes, *A First Course in Turbulence* (Tennekes & Lumley 1972). This book provides a detailed analysis of the various generic engineering flows (wakes, jets, mixing layers, and boundary layers) and remains the best example of scaling arguments applied to the modeling of these flows. The book does not contain much mathematics and for this reason, students tend to find it difficult. (Many years ago a senior professor at Cornell stated to Z.W. that the book is not very useful because it does not contain differential equations that can be solved!)

Lumley used scaling arguments à la Kolmogorov to predict the form of the turbulence energy spectrum under various conditions (Lumley 1967a), including the effects of uniform strain, where he showed that the covariance spectrum follows a $k^{-7/3}$ law (where k is the wave number), thus decreasing more rapidly than the energy spectrum ($k^{-5/3}$). This is an example of a return to isotropy and has excellent experimental verification (Saddoughi & Veeravalli 1994). In the same article, he addresses the effects of viscosity, buoyancy (see also Lumley 1964), magnetic fields, and elasticity. However, these Kolmogorov-type scaling arguments do not hold up well when dealing with the fine-scale structure of turbulence (Shraiman & Siggia 2000).

Up until the 1970s John's career was an eclectic mixture of basic theory, experiment, and instrumentation. There was some work on particular turbulent flows (e.g., mixing layers, wakes) but there was no consistent framework or approach for the analysis and prediction of these flows. In *A First Course in Turbulence* (Tennekes & Lumley 1972), he uses eddy viscosity arguments extensively to provide models (e.g., the mean velocity profile of a wake), but little could be done to model higher-order statistics such as the profiles of the turbulent stresses. Indeed, at that time there was no known systematic way of doing this. Attempts to model were largely ad hoc.

The advection term of the incompressible Navier–Stokes equations has a quadratic nonlinearity. To find the mean velocity, one averages the equations of motion. The resulting first-order equation contains a second-order quadratic term, the Reynolds stress, which is unknown. This is the turbulence closure problem. First-order modeling approaches (i.e., equations for first-order quantities, such as the mean velocity profile) replace the second-order quantities with models that depend only on the averaged velocities. The result is like a constitutive relation: the eddy viscosity mimicking the molecular viscosity, relating the stress to the strain rate in laminar flows (although the molecular viscosity is a material property, whereas the eddy viscosity also depends on the fluid motion). This approach can be effective, and "in the hands of a clever engineer can often produce satisfactory results, although it is known to be wrong in principle" (Lumley 1979, p. 1). (John used the word "clever" sparingly to indicate high praise.) The problem of using an eddy viscosity to relate the stress to the rate of strain tensor is that the temporal and spatial scales of the eddies are generally of the same order as those of the mean flow. However, the ratio of the molecular timescale to a characteristic scale of a (laminar) flow is very small such that the molecular motion can rapidly adjust to changes imposed by the mean flow. For flows that are evolving slowly and for which there is only one characteristic scale, the approach can work well, but for a rapidly straining flow, as in a contraction, the approach fails completely (e.g., Pope 2000).

In second-order modeling, equations for the first- and second-order quantities are derived using Reynolds decomposition and the third-order terms are modeled. The equations are then solved computationally by time stepping. The equation for the rate of change of the turbulence Reynolds stress $\langle u_i u_j \rangle$ is

$$\frac{\mathrm{D}}{\mathrm{D}t}\langle u_i u_j \rangle = -\frac{\partial T_{kij}}{\partial x_k} + P_{ij} + R_{ij} - \frac{2}{3}\epsilon\delta_{ij}, \qquad 1.$$

where $\partial T_{kij}/\partial x_k$ is the transport of Reynolds stress, P_{ij} is the turbulence production tensor, R_{ij} is the pressure–rate-of-strain tensor, and $\frac{2}{3}\epsilon\delta_{ij}$ is the dissipation tensor (Pope 2000). If the balance is dominated by the production, dissipation, and pressure–rate-of-strain (i.e., if the nonlocal processes are small in comparison to the local processes), then it is reasonable that eddy viscosity arguments will hold. In second-order modeling, models for ϵ, T_{kij}, and R_{ij} are required for closure. A veritable industry has arisen in modeling these terms.

The first systematic attempts to do second-order closure appear to be due to Donaldson (cf. Kline et al. 1969, pp. 114–18), Daly & Harlow (1970), and Hanjalić & Launder (1972). Lumley's first article directly on this subject appears to be in 1975 (Lumley & Khajeh-Nouri 1975). In his publication list of 229 articles, approximately 50 are devoted to second-order modeling. His peak interest was in the late 1970s and the early 1980s, and those who knew him were infected by his enthusiasm for second-order modeling. It was the combination of mathematical rigor and physical intuition that attracted him to the subject. At last, so it seemed, he could combine these two characteristics to provide accurate models of real turbulent flows. John learned the techniques he used in his modeling efforts while a graduate student. Johns Hopkins was a hotbed of rational mechanics espoused and taught by Clifford Truesdell and Jerald Erickson. Unlike them, John solved problems. Second-order modeling has burgeoned with hundreds of articles appearing each year. Here we illustrate John's contributions by looking at two important flows not amenable to simpler, first-order closure that he focused on early: (*a*) the return to isotropy of homogeneous turbulence and (*b*) buoyancy-driven flows in the atmospheric boundary layer.

When a turbulent flow passes through a contraction (here we consider an axisymmetric contraction with axial strain in the streamwise direction), it is strained and becomes strongly anisotropic. After the contraction, where the strain is released, the turbulence slowly relaxes back towards isotropy. It is the modeling of this return toward isotropy that was the concern of Lumley & Newman (1977). The redistribution of energy and hence the modeling of the pressure-rate-of-strain tensor are of prime importance here. Interestingly, Rotta and Davidov produced a (linear) model for this term (Rotta 1951a,b; Davidov 1961). In his important review, John states, "It is not an exaggeration to say that there is little in use at the present time that was not suggested by these authors" (Lumley 1978, p. 125). Apparently John was unaware of this important closure until relatively late: It does not appear in *A First Course in Turbulence* (Tennekes & Lumley 1972).

In their return to isotropy article, Lumley & Newman (1977) describe the evolution of turbulence towards isotropy in homogeneous, anisotropic flows with no mean velocity gradient. The article focuses on the behavior of the anisotropy tensor, $b_{ij} = \langle u_i u_j \rangle / q^2 - 1/3\ \delta_{ij}$ (which vanishes identically if the turbulence is isotropic), in terms of its invariants (e.g., Lumley 1970b): quantities that are obtained (from even-ranked tensors) by summing or contracting all the indices in pairs so that no free indices remain. These quantities are the same in all coordinate systems. For this problem, the rate of change of the Reynolds stress may be written as

$$\begin{aligned}\frac{\partial \langle u_i u_j \rangle}{\partial t} &= -\left(\langle p_{,i} u_j + p_{,j} u_i \rangle\right)/\rho - 2\nu \langle u_{i,k} u_{j,k} \rangle + \frac{2}{3}\epsilon \delta_{ij} \\ &= -\epsilon\left(\phi_{ij} + \frac{2}{3}\delta_{ij}\right), \end{aligned} \qquad 2.$$

where ϵ is the rate of dissipation, whereas ϕ_{ij} contains all evidences of anisotropy. The tensor ϕ_{ij} can be written (with the assumption that ϕ_{ij} is determined by $\langle u_i u_j \rangle$ and ϵ) as

$$\phi_{ij} = \beta b_{ij} + \gamma\left(b_{ij}^2 + \frac{2}{3} II \delta_{ij}\right), \qquad 3.$$

where β and γ are unknown scalar (invariant) functions of the invariants I, II, and III. The first, second, and third are $I = 0, II = b_{ij} b_{ij}$, and $III = b_{ij} b_{jk} b_{ki}$ (Lumley & Newman 1977).

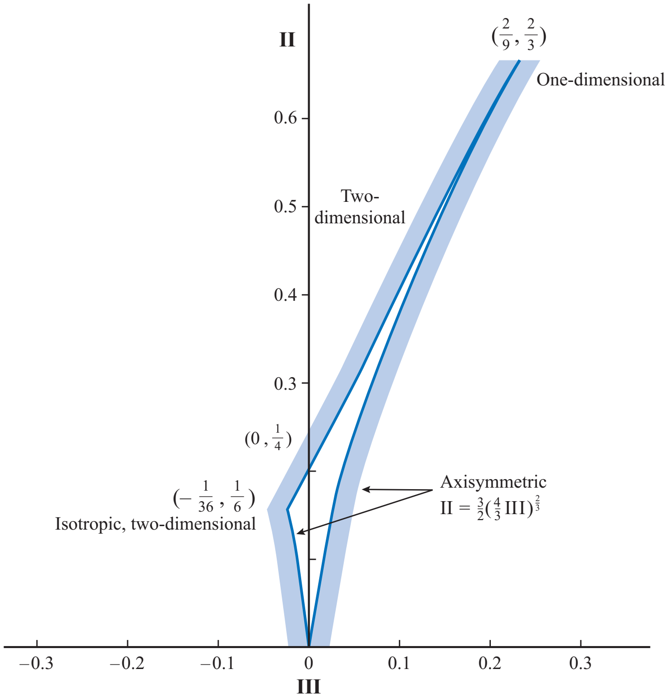

Figure 4

The Lumley triangle, adapted with permission from Lumley & Newman (1977). There are various other forms (Lumley 1978, Pope 2000). The vertex at (0, 0) is isotropic turbulence.

Lumley chose two independent invariants, *II* and *III*, which can be determined from the Reynolds stresses at any point or for any time in the flow. Thus it follows that the anisotropy tensor may be described on a plane. **Figure 4** shows this plane of the two invariants. All realizable Reynolds stresses that can occur in any turbulent flow must be contained within this triangle, now known as the Lumley triangle. The vertices and lines correspond to particular turbulent states, as shown on the diagram. Points outside the triangle have negative or complex eigenvalues and are therefore nonrealizable. The image of the triangle was chosen for a plaque awarded to John at the conference held on the occasion of his sixtieth birthday (**Figure 5**). The concept of realizability was first introduced by Schumann (1977) but systematically exploited by Lumley (1978). A difficulty in turbulence modeling is that calculations can yield negative component energies. Imposing realizability conditions guards against this and many other violations of the physics of turbulence, such as limiting the absolute value of a correlation coefficient to unity or less (Lumley 1978). Using realizability to model the return to isotropy and dissipation terms in the second-order equations, Lumley & Newman (1977) were able to satisfactorily model experimental data on the relaxation of turbulence toward the isotropic state. The pressure term appearing in the transport equations was separated into two parts: a rapid part, corresponding to the pressure appearing in rapid distortion theory, and a return to isotropy part, containing the effect of the nonlinear mixing of the turbulence by itself (tending to make the turbulence more isotropic). The trajectory of the return towards isotropy can be plotted on the Lumley triangle (e.g., Pope 2000, Choi & Lumley 2001). The Lumley & Newman (1977) model shows excellent agreement with experimental data, but adjustments have to be made in the modeling in light of more recent data, with different geometries (Choi & Lumley 2001).

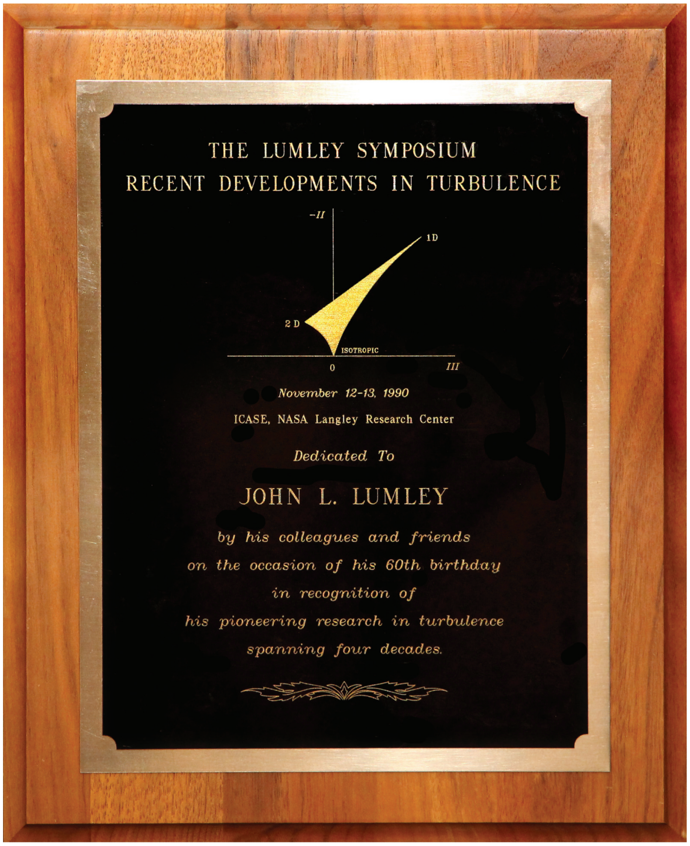

Figure 5

Plaque awarded to John Lumley at the NASA conference celebrating his sixtieth birthday.

It is worth pointing out that return to isotropy is extremely slow and depends on the type of flow. The Lumley & Newman (1977) model was for isotropic turbulence that had been strained. Here a relaxation back to the isotropic state may be expected. More complex turbulent flows like wakes and mixing layers generate large coherent structures. For these flows, Julian Hunt (private communication) points out A.A. Townsend's observation that "eddies are like cartwheels": As they slow down, they remain anisotropic. John was well aware of the problems of large-scale structures and addresses this issue in his important article "Toward a Turbulent Constitutive Relation" (Lumley 1970b), which is in many ways a precursor to his later work on modeling. Readers are also referred to John's conference "Whither Turbulence? Turbulence at the Crossroads" (Lumley 1990b).

In the atmospheric boundary layer, the flow is buoyancy driven by the upward surface heat flux (e.g., Wyngaard 2010). The divergence of this flux drives the turbulence. At the top of the layer, the turbulence erodes the inversion base, mixing the stably stratified fluid into the layer and causing it to thicken. The vertical transport of turbulent energy removes turbulence energy near the surface and transports it up towards the inversion base. Thus there is a net loss near the surface and a gain aloft. Lumley et al. (1978) show that gradient transport models (where the flux is proportional to its gradient) are wildly in error in predicting the vertical transport, and thus in predicting the rise of the inversion base with time.

Without wind shear, the buoyancy transport term, which is a third-order term, plays a dominant role in the heat flux analog to the Reynolds stress Equation 1. Zeman & Lumley (1976) were the first to model the transport term of the heat flux. In the then-existing models, the influence of the transport was neglected but was included in the second-order equations by simple gradient transport models. Lumley et al. (1978) use the eddy-damped quasi-Gaussian approximation to model the buoyant transport. In the equations for the third-order quantities (e.g., the transport of kinetic energy or the temperature variance), the fourth-order products (which appear explicitly in these third-order equations) are replaced by their quasi-Gaussian form. [Independently, André et al. (1976a,b) developed another effective method for dealing with buoyant transport; see Lumley et al. 1978.] Pressure correlations in the transport term are replaced by third-order terms divided by a timescale. Model equations are generated for the destruction of temperature variance and other terms. Time stepping the resulting modeled equations showed remarkable correspondence with the observations of the various turbulence quantities.

But there are difficulties with such modeling: If there are two independently evolving scales in the mechanical field, or if there are two or more independent scalar fields (Pope 1983), such as occurs with plumes from multiple sources each with their own length scales, then the scheme breaks down unless a full spectral model is employed, adding great complexity and cost. John was very aware of these limitations (Lumley 1983).

These examples of the relaxation of a turbulent flow after it has been strained, and of the convective boundary layer, illustrate how John was attracted to tackling difficult problems that were intractable unless higher-order terms were modeled in the governing turbulence equations—in the above examples, the return to isotropy and third-order transport terms. To do this, he set in place rigorous constraints and used a systematic approach to closure. The reader is referred to Pope (2000) for a thorough review of second-order modeling and its subsequent developments.

Yet John was under no illusion that the modeling procedures can be predictive in the sense that some of the grand theories of physics can be. At his conference "Whither Turbulence? Turbulence at the Crossroads" (Lumley 1990b), John replies to R. Narasimha's criticisms of turbulence modeling in the following way:

> Professor Narasimha states that the turbulence models have not predicted anything. That, of course, is true. However I believe it is foolhardy to expect them to. These models are simply an embodiment of experience .... A model cannot, except by accident, contain more than is put into it .... You should never expect a model to predict something you did not foresee. Use it to get a better numerical value for something you can already estimate on the back of an envelope. (Lumley 1990a, pp. 55–56)

2.2. Experiments

John thought of himself primarily as a theoretician. On the occasion of receiving the American Physical Society Fluid Dynamics Prize, he wrote, "theory is what gives meaning to observation. Understanding is the process of constructing simple models that explain the observations, and permit predictions. What the theoretician does is a vital part of the loop" (Lumley 1992, p. 210). In the same address, John talks of the atmosphere towards theoreticians in the United States and (one would expect with tongue in cheek) of how experimentalists and practical engineers regard theoreticians with alarm:

> The United States is a curiously unsympathetic environment for a theoretician .... We have a sociocultural/historical myth ... of egalitarianism, practicality, inventiveness. An American, in this

> myth, is a man who rolls up his sleeves and pitches in, solving the problem at hand in a clever, simple, practical way (often involving bailing wire and a wad of chewing gum), usually saying over his shoulder that he does not hold with book learning . . . In this environment, the theoretician is viewed with alarm, and felt to be irrelevant . . . . It does not help that any theoretician worth his salt can come up with several contradictory theories a day. He had a beautiful theory to explain yesterday's data, but this morning it seems that those data are wrong; this afternoon he has a new theory to explain the new data. Who can trust a man like that? (Lumley 1992, p. 210)

He ends his address: "Tomorrow it [a theory] may be wrong. Even so, it deserves to be regarded as one of the better things of which man is capable" (Lumley 1992, p. 210).

Despite his strong bent for theory, John followed the experimental literature very closely. Throughout his career he did some of the key experiments in the field, and he designed new instrumentation. His experiments were motivated by theoretical questions and in this regard he has much in common with G.I. Taylor.

His first important experiment was on particle tracking (Snyder & Lumley 1971). Most measurements of turbulence are done in a fixed Eulerian framework, but to understand dispersion, fluid particles need to be tracked. Lumley & Snyder built a vertical wind tunnel and designed an elaborate camera system that photographed the particles as they moved through the turbulent flow. It was a difficult experiment and the first to determine the particle auto correlations and to study the effects of different particle sizes. It was not until the advent of high-speed detectors and cameras in the late 1990s, preceded by the direct numerical simulations of Yeung & Pope (1989), that the field advanced further. The subject has now burgeoned and has provided a completely new perspective on turbulence (Toschi & Bodenschatz 2009). Lumley's experiment was ahead of his time.

With Z.W. he did experiments on passive scalars in grid turbulence (Warhaft & Lumley 1978). These were motivated by questions posed in second-order modeling, and the results presented even greater questions and problems because they showed the multiscale nature of the scalar decay, a difficult issue to deal with in second-order modeling.

Two other important experiments were inspired by modeling. The first was a detailed study of the evolution of a helium jet. At the time, there were no reliable data on density fluctuations (at low Mach number) in simple flows. With Panchapakasen, John constructed a jet facility (Panchapakesan & Lumley 1993b) and used a Way-Libby probe (Way & Libby 1971), consisting of two interfering hot wires to discriminate between the velocity and density fluctuations (in this case a helium jet in quiescent air). Measurements at a point were done by moving a shuttle vertically through the flow. The kinetic energy budget was determined, and a full model for the triple moments was compared with the data. It remains a benchmark experiment. In order to prepare for this experiment, equally detailed measurements of an air jet were done (Panchapakesan & Lumley 1993a).

A second experiment examined the return towards isotropy using three different types of distortion to produce homogeneous, anisotropic turbulence (Choi & Lumley 2001). The trajectory of the return to isotropy (plotted on the Lumley triangle) was shown to be nonlinear (in contrast to the Rotta linear model). As in the Pachapakasen experiment, extensive modeling of the results was presented.

Earlier at Penn State, John worked on a variety of projects for the Applied Research Laboratory. One important effort was a study of the viscous sublayer in a glycerin tunnel. John conceived the study and supervised its design and construction. The high viscosity of glycerin allows for a thick viscous sublayer, thereby facilitating detailed measurements. With Bakewell, John was one of the first, along with Kline et al. (1967), to show that the viscous sublayer could not be considered passive but plays an active role in the generation and preservation of the shear flow (Bakewell & Lumley 1967). Herzog (1986) and Lumley did extensive work using POD to quantify the large-scale structure of this flow in the wall region.

Apart from experiments, John developed new instrumentation. This included the design of thermistors and hot-wire anemometers (Lumley 1962, Wyngaard & Lumley 1967, Sheih et al. 1970, Lumley et al. 1971), Laser Doppler velocimetry (George & Lumley 1971, 1973; Buchave et al. 1979), as well other instrumentation (Wyngaard & Lumley 1967).

2.3. Geophysical Fluid Mechanics

John's first publication related to geophysical flows was in 1964, and it addressed the issue of how stable stratification affected the low-wave-number portion of the velocity spectrum, a subject of considerable interest then and now (Lumley 1964; see also Lumley 1967b). In the same year he published his book *The Structure of Atmospheric Turbulence* (Lumley & Panofsky 1964). Geophysical flows had a number of attractions for John: First, they were a source of very high Reynolds number, and John developed instrumentation and conducted aircraft measurements to measure turbulence in the atmosphere (Payne & Lumley 1966, Lumley et al. 1971). Second, they provided motivation for theory and modeling. We have discussed his modeling of the buoyant boundary layer (with Otto Zeman), but there are many other areas that attracted his attention. These included wave–turbulence interactions in the upper ocean (Donelan et al. 1982, Kitaigorodskii & Lumley 1982), salt fingering (Zeman & Lumley 1982a,b), Langmuir circulations (Leibovich & Lumley 1982), plume growth (Lumley 1971) and transport (Lumley 1978). In much of this work he collaborated with experts in the field.

2.4. Proper Orthogonal Decomposition and Coherent Structures

John contributed three short articles to a 1965 meeting on atmospheric turbulence and its effects on radio wave propagation (Yaglom & Tatarsky 1967). Of John's three articles, the most important is that introducing the POD as a tool for turbulence research (Lumley 1967b). POD is known by several names depending on custom in the various fields in which it is applied (e.g., Karhunen–Loéve decomposition, principal components analysis). It is an important statistical tool used in a wide range of applications. To our knowledge, John's derivation of this decomposition was unique, and allows the theory—at least in principal—to be used in continuum theories.

In a general review of turbulence, Liepmann (1952) appears to be the first to write about the concept of coherent structures (those patterns that appear to recur repeatedly though irregularly in a variety of turbulent flows): "In recent years the importance of the existence of a secondary, large scale structure in turbulent shear flow has become apparent.... Intermittency and thus the existence of elements of a very large scale seem to be typical for turbulent flows with free boundaries" (p. 413). He cites wartime reports on experiments on the heated jet (Corrsin 1943) and on rotating Couette flow (Pai 1943) that demonstrated intermittency. These were followed in a short time by experiments by Townsend (1949), who described similar phenomena in the wake of a circular cylinder, and by Townsend (1951) on the flat plate boundary layer.

It is difficult to describe exactly what if anything these experimentally observed structures have in common, and the methods of detection are subject to individual interpretation. John introduced POD (Lumley 1967b) as an unambiguous method of identifying coherent structures in turbulence, which will be outlined below. Furthermore, in that paper John outlined an ambitious program of using the decomposition to study the detailed dynamics of turbulence in wall layers, ideas that bore fruit more than two decades later (Aubry et al. 1988). This will be described after a brief discussion of POD.

In John's development of POD for turbulent flows, he suggested that, given experimental (or computational) velocity data $\mathbf{u}(\mathbf{x}, t)$, one should search for a deterministic vector field $\boldsymbol{\phi}(\mathbf{x}, t)$ that is as parallel as possible, on average, to $\mathbf{u}(\mathbf{x}, t)$. This can be done by considering the inner product

$$\alpha = \frac{\int \mathbf{u}(\mathbf{x}, t)\cdot\boldsymbol{\phi}^*(\mathbf{x})\,\mathrm{d}\mathbf{x}}{\left(\int \boldsymbol{\phi}(\mathbf{x})\cdot\boldsymbol{\phi}^*(\mathbf{x})\,\mathrm{d}\mathbf{x}\right)^{\frac{1}{2}}}, \qquad 4.$$

where * indicates the complex conjugate (if there is a homogeneous direction, then it is convenient to use a Fourier decomposition for $\mathbf{u}$ in that direction, hence the allowance for complex $\mathbf{u}$), and the integrals are over the flow domain. Commonly, the deterministic field is assumed to depend only on $\mathbf{x}$, as shown here. The denominator removes dependence on the magnitude of $\boldsymbol{\phi}$ because only its direction is of importance. The best choice for $\boldsymbol{\phi}$ is then the one that maximizes the averaged value of $|\alpha|^2$. This is a straightforward problem in variational calculus and leads to the following equation for $\boldsymbol{\phi}$:

$$\int \langle u_i(\mathbf{x}, t)u_j(\mathbf{x}', t)\rangle\, \phi_j(\mathbf{x}')\,\mathrm{d}\mathbf{x} = \int R_{ij}(\mathbf{x}, \mathbf{x}')\phi_j(\mathbf{x}')\,\mathrm{d}\mathbf{x}' = \overline{|\alpha|^2}\,\phi_i(\mathbf{x}), \qquad 5.$$

where the angled brackets indicate the appropriate average. [The numerical effort in computing the eigenfunctions can be reduced by the method of snapshots first described by Sirovich (1987a,b,c), a procedure that has been widely adopted.] The best choice for the vector field that is on average closest in direction to $\mathbf{u}(\mathbf{x}, t)$ is the solution to this eigenvalue problem. The eigenfunctions are a complete, orthogonal set and can be normalized. This set provides a basis for an expansion of the turbulent velocity in a series:

$$\mathbf{u}(\mathbf{x}, t) = \sum_{n=1}^{\infty} a_n(t)\boldsymbol{\phi}_n(\mathbf{x}), \qquad 6.$$

where each coefficient $a_n(t)$ describes a stochastic process and, given $\mathbf{u}(\mathbf{x}, t)$ and the $\boldsymbol{\phi}_n(\mathbf{x})$, can be computed as

$$a_n(t) = \int \mathbf{u}(\mathbf{x}, t)\cdot\boldsymbol{\phi}_n^*(\mathbf{x})\,\mathrm{d}\mathbf{x}. \qquad 7.$$

The energy/mass in the first N terms in Equation 6 is

$$E_N = \frac{1}{2}\sum_{n=1}^{N} a_n^2. \qquad 8.$$

This expression results from any expansion of $\mathbf{u}(\mathbf{x}, t)$ in an orthogonal basis. If the basis set is $\boldsymbol{\phi}_n(\mathbf{x})$, then because of Equations 4 and 7, a_n^2 is a maximum for each n. As a consequence, for a given N, more energy is captured by Equation 6 than any other orthogonal basis. In this sense, the expansion Equation 6 is optimal.

The inhomogeneous case described here is the simplest to explain. If there are homogeneous directions, they are described by Fourier expansions. In Lumley (1967b), John presented an extension for such cases.

John introduced POD for identifying coherent structures and, with Aubry, Holmes, and Stone (Aubry et al. 1988), suggested the value of the POD eigenfunctions as a basis for approximations of the Navier–Stokes equations as a means to develop an understanding of their dynamics. The first major step in this program was carried out in Aubry et al. (1988) and was followed by a series of other papers. In these papers, flows approximated by truncated Galerkin approximations are introduced into the Navier–Stokes equations. This leads to low-order dynamical systems approximations for the expansion coefficients $a_n(t)$, from which a representation of the dynamics can be extracted (Nadine Aubry, a graduate student at the time, suggested the form of the Galerkin expansion, a key step in the application of the wall layer). If the analysis is confined to the near-wall region, the Reynolds number is not large. Consequently, it is sensible to assume that the dynamics might be adequately described by a small number of degrees of freedom, so there is a prospect that a low-dimensional model of the flow can be successful, although it is clear that this would not be so if the entire flow is considered and many more modes would be needed. [The method and some results of this article are summarized in Berkooz et al. (1993). Holmes et al. (1996), and a later edition (Holmes et al. 2012), provide a more relaxed exposition of the theory, as well as proofs of points involved in the basic theory. Readers are referred to Aubry et al. (1991) for an enlargement of the theory of POD applied to spatio-temporal fields.]

To devise a model treating only the near wall region, one must consider how the flow interacts with the outer flow. This is one of the issues that must be confronted—indeed, the modeling effort of Aubry et al. (1988) is fraught with difficulties and is nothing short of heroic. One of the important issues is the connection of the near-wall region with the outer flow. This was done by a pressure boundary condition linking the wall region and the outer flow. Because no experimental data were available to supply the pressure at the boundary, Aubry et al. (1988) used the numerical data obtained earlier by Moin (1984) (see the quote from Moin at the end of Section 2.6.)

Coherent structures in wall layers (boundary layers and channel flows) have been described by a number of authors. The following description was first given by Kline et al. (1967). The process involves the formation of vortices nearly parallel to the basic flow but at a small upwards angle to the wall. These vortices produce regions of flow upwards from the wall termed ejections, which carry low-speed fluid away from the wall. This results in low-speed streaks in the upwelling regions. These streaks create inflection points in the flow, followed by instabilities causing a burst of Reynolds stress, as shown by Corino & Brodkey (1969), and then a slow downdraft sweeping high-speed fluid towards the wall. This sequence of ejections due to roll formation, bursting, and then sweeps repeats itself at irregular times and locations on the wall.

Low-order models involving five or more ordinary differential equations for the coefficients $a_n(t)$ in Equation 6 are described in Aubry et al. (1988) and Sanghi & Aubry (1993). These systems produce results with irregularly occurring sequences of features like the low-speed streaks, ejections, bursts, and sweeps observed in turbulent wall layers, together with intermittency. Although these studies captured the salient features and dynamical behavior of this series of events, the timescales between bursts [which are influenced by the magnitude of the perturbations to the system such as the pressure from the outer region and nonzero streamwise modes (Sanghi & Aubry 1993)] were still too long. Podvin et al. (1997) noted that the earlier estimates by Aubry et al. (1988) (which contained a scaling oversight) and by Sanghi & Aubry (1993) (who recovered the correct scaling) did not account for the advection of structures past the stationary sensor. Accounting for this brings the inter-event durations further in line with experiments. One can conclude that the models have had some success in shedding light on the dynamics of the wall layer, perhaps as much as John might have hoped.

In terms of scientific reach, John's development of POD may turn out to be his most influential work. Since he introduced it, POD has inspired nearly 30,000 journal articles, virtually all of which follow John's approach of using POD to produce equations determining the dynamics. The very brief outline of a method to explore the dynamics proposed in Lumley (1967b) turned out to be too difficult—perhaps even impossible—to implement. Most of these papers follow Aubry et al. (1988) in using POD eigenfunctions in Galerkin approximations to yield low-order dynamical models, not the method used in Lumley (1967b). These papers cover not only most subfields of fluid mechanics, but also a host of other applications in many areas where one might expect similar issues to arise, including mechanics, geophysics, biology, computational finance, electrical circuit analysis, data assimilation, numerical analysis, medicine, and many others. Several of these applications include automotive engines, which John would be particularly interested in.

We have mentioned the surprisingly wide array of problems to which POD has been applied. John would of course be most satisfied with its use in turbulent flows. Examples of canonical turbulent flows that have been treated in the same way as the pioneering wall layer studies briefly described here include the turbulent jet (Glauser et al. 1992), the transitional boundary layer (Rempfer & Fasel 1994), the forced transitional mixing layer (Rajaee et al. 1994), and the wake of a circular cylinder (Siegel et al. 2008).

2.5. Drag Reduction

In the late 1960s and early 1970s, John did seminal work on drag reduction in turbulent flows (Lumley 1969, 1973). This work stemmed in part from his affiliation with the Garfield Thomas Water Tunnel at Penn State, then the largest circulating water tunnel in the world. It was built to further torpedo research, and John became an expert in the design of quiet water tunnels (Lumley & McMahon 1967) and the aspects of undersea warfare in which turbulence plays a role.

Drag reduction using polymer additives was discovered by Toms (1948). Its applications are broad, ranging from reducing drag in pipelines, fire hoses, and storm water sewers to ship hulls. The drag can be cut in half by the addition of extremely small concentrations of high molecular weight flexible polymers. The subject is difficult, and it is still not satisfactorily understood because it couples our imperfect knowledge of turbulence in the boundary layer with the behavior of polymer chains in that strongly perturbed flow regime. The distinguished polymer physicist Pierre de Gennes (1990) stated (in his work that proposes a competing theory), “The main (tentative) interpretation of this effect is due to Lumley” (p. 35).

John summarizes the physics as he understood it (with unusually complex syntax reminiscent of Henry James) as follows:

> The postulated mechanism, which is at once simple, but rather subtle: at sufficiently high wall shear stress, the fluctuating strain rate causes the molecules to expand, the extent of the expansion depending on the concentration; the increased effective viscosity damps the small eddies, but does not affect the viscosity deep in the viscous sublayer, where the molecules are not expanded; due to the decreased intensity of the small eddies, the reduced Reynolds stress at the buffer delays the reduction of the mean profile slope, thereby thickening the sublayer; the large eddies expand with the sublayer; the expanded large eddies produce, from the mean velocity profile, an increased streamwise fluctuating velocity, primarily in the buffer layer; in the maximum drag reduction regime, primarily large eddies remain. (Lumley 1973, p. 288)

He closes that review with a perspicacious comment: “The study of these drag reducing flows has shed considerable light on the dynamical structure of the Newtonian turbulent boundary layer. *This is probably a general truth: that one can gain considerable insight into the behavior of a physical process by adding a new stimulus*” (Lumley 1973, p. 288) (emphasis added).

De Gennes postulated an alternative viewpoint (Tabor & de Gennes 1986, de Gennes 1990): The elastic energy stored by the partially stretched polymers is of prime importance, and the increase in effective viscosity is small and inconsequential (White & Mungal 2008). As yet, there does not seem to be consensus on the mechanism, in part due to the lack of high-quality, high–Reynolds number data. But John’s work remains the model on which latter work builds. For recent advances, and a more detailed assessment of John’s contributions, readers are referred to Ptasinski et al. (2003), White & Mungal (2008), and Robert et al. (2010).

2.6. Reviews

John wrote approximately 20 reviews of various types and forms. These include five in the *Annual Review of Fluid Mechanics* (Lumley 1969, 2001; Buchave et al. 1979; Berkooz et al. 1993; Lumley & Blossey 1998). Other reviews include encyclopedia entries, presentations on receiving awards, and introductions to books and conference proceedings, as well as more formal, solicited reviews. In his reviews we see a more relaxed writer, full of insights and prepared to make controversial statements. There is a persistent theme concerning our poor understanding of turbulence and the importance of theory. On talking about the necessity of modeling for engineering design, he states, "In our present state of understanding, these simple models will be based, in part on good physics, in part, on bad physics, and in part on shameless phenomenology. This is basically engineering" (Lumley 1992, p. 203). And on the need for good theory, "The computationalists themselves do not bring understanding, they are simply very detailed exploratory numerical experiments. Understanding only comes from a good, creative theoretician, who can use the data to support or disprove an idea regarding turbulence dynamics" (Lumley & Yaglom 2001, p. 247). Despite this opinion about computationalists, John recognized the transformative consequences of computation. Parviz Moin (private communication) writes,

> In 1980 a new chapter in turbulence research opened using data from LES and DNS. John was among the first to recognize the potential of simulation databases in turbulence research, and thus began a wonderful relationship between us. Investigation of coherent structures was one focus of this new tool (the other was turbulence modeling). In particular, the simulation data were ideal to test the convergence and utility of POD in three dimensions. In 1983, when I was at NASA Ames, I used the LES data to investigate POD in channel flow (1984 AIAA paper presented at the 22nd Aerospace Sciences Meeting). Later, the Aubry et al. paper used the pressure signal from the LES data (and the near-wall POD eigenfunctions) to close the dynamical systems equations near the wall.

3. CONCLUDING REMARKS

John held strong opinions and sometimes this got in the way of his judgment. But mostly they were a driving force, often providing the motivation to develop theory or to do experiments. Here is John at his most combative, written during a period when there were strong rifts in the turbulence community, taken from his poster for his conference at Cornell, "Whither Turbulence? Turbulence at the Crossroads":

> Turbulence is rent by factionalism. Traditional approaches in the field are under attack, and one hears intemperate statements against long time averaging, Reynolds decomposition, and so forth. Some of these are reminiscent of the Einstein-Heisenberg controversy over quantum mechanics, and smack of a mistrust of any statistical approach. Coherent structure people sound like *The Emperor's New Clothes* when they say that *all* turbulent flows consist primarily of coherent structures, in the face of visual evidence to the contrary. Dynamical systems theory people are sure that turbulence is chaos. Simulators have convinced many that we will be able to compute *anything* within a decade. Modeling is thus attacked as unnecessary, or irrelevant because it starts with Reynolds stress averaging or ignores coherent structures. The card-carrying physicists dismiss everything that has been done on turbulence from Osborne Reynolds until the last decade. Cellular Automata were hailed on their appearance as the answer to a maiden's prayer, so far as turbulence was concerned. It is no wonder that funding agencies are confused. (quoted in Cantwell 1990, p. 97)

This statement expressed his feelings at the time that modeling approaches were unwisely being abandoned by funding agencies, and that the funding agencies were not competent to direct the fluid mechanics community's (and most likely many other scientific communities') research direction. To make sure that they got the point, he invited government funding managers to the meeting. Most immediate in his mind was the abandonment of turbulence modeling by the agencies, a serious mistake in his view. He also believed many of the new approaches of the hour that were the darlings of the agencies would ultimately be abandoned. At the time of the "Whither Turbulence?" convocation, and until the end of his research career, John was heavily involved in two of these new approaches that were (and remain) the rage: coherent structures and dynamical systems theory. His opinions about the value of and need for statistical approaches and modeling had not changed, however, and he felt a sense of responsibility to the subject to try to change what he felt was leading the field astray.

There is little question that John Lumley led his subject. His research was frequently groundbreaking. In organizing meetings like "Whither Turbulence?", acting in his many editorial capacities, and chairing important committees, he led the community. Because of John's breadth, in terms of both topics and techniques, and because he excelled in whatever problems he addressed, we believe his work will stand out as the most significant of the second half of the twentieth century. Some may have probed deeper, but none were as broad. We greatly miss this eclectic engineer-scientist of strong convictions.

DISCLOSURE STATEMENT

The authors are not aware of any biases that might be perceived as affecting the objectivity of this review.

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