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A Century of Turbulence ⋆

JOHN L. LUMLEY
Sibley School of Mechanical & Aerospace Engineering, Cornell University, Ithaca, NY 14853, U.S.A.

AKIVA M. YAGLOM
Massachusetts Institute of Technology, Cambridge, MA 02139, U.S.A.

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Abstract. A brief, superficial survey of some very personal nominations for high points of the last hundred years in turbulence. Some conclusions can be dimly seen. This field does not appear to have a pyramidal structure, like the best of physics. We have very few great hypotheses. Most of our experiments are exploratory experiments. What does this mean?

We believe it means 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.

Key words: history, turbulence.

1. Introduction

Peter Bradshaw (private communication) has suggested that this title is likely to make trouble, since it may be misinterpreted in databases as referring to politics.

Let us make clear at the outset that we have not personally experienced the entire one hundred years of turbulence. JLL has only been involved in this subject for slightly less than half that time, since he went off to graduate school and took Corrsin's course in the fall of 1952. AMY has been involved in the subject for slightly more than half that time; Kolmogorov proposed his thesis topic in 1943.

As we began to prepare this paper, we soon realized that it was possible to offend a very large fraction of our colleagues, since we could not restrict the paper to the work of dead people. We have tried to make a very subjective selection of

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seminal works. There are also contributory works, works that elaborate, extend, support, and explain seminal ideas. Most work in any field falls in this category. Sometimes, the seminal paper ultimately proves to have been a mistake. It may nevertheless have been extraordinarily influential. It was probably the contributory works that established the incorrectness of the seminal paper. The contributory works are therefore equally important. Nevertheless, here we are going to confine ourselves (with rare exceptions) to the seminal works. That means that we will probably be leaving out many of your favorite works, for example, the boilerplate experiments in which canonical flows were exhaustively explored, and which have served as grist for a hundred theories.

1.1. Statistics

To begin with, we wanted to accumulate some statistics about the field. We decided that the number of significant papers published per year might be useful. The easiest way to do this is to count papers in five-year intervals in the bibliography of Monin and Yaglom's massive work *Statistical Fluid Mechanics* [144] from 1900 to 1965, where the bibliography stops. Although AMY is revising this work, and will eventually bring the bibliography up to date, this is not yet available.

On the basis of this data, necessarily highly selective (representing only those papers that Monin and Yaglom thought to be significant), we can say that the number $N(t)$ of turbulence papers published per unit time (measured in years) is given by

$$N(t) = 0.434e^{t/11.8}, \tag{1}$$

where t is the number of years since 1900. That is, the number of papers published between t_1 and t_2 is the integral of $N(t)$ over that interval. The fluctuations begin in the neighborhood of $\pm 20\%$ near 1900, and fall steadily to $\pm 2.5\%$ near 1965.

We should consider the possibility that the exponential curve exists for other reasons. The further in the past an event took place, the more perspective we have, and the more critical we can be. Hence, we probably regard as noteworthy a smaller and smaller number of papers, the greater has been the time lag between their writing and the present. Even if the output of papers were constant, this would create an exponential growth. Recent papers are more difficult to evaluate, and there would be a tendency to include a larger fraction as possibly noteworthy. So, too much reliance should not be placed on Equation (1). The exponential growth is probably a little slower than it suggests.

The question is, where is the beginning? If the beginning is estimated from a linear extrapolation to zero based on the current slope, the beginning always appears to be 11.8 years in the past. This probably explains the natural human tendency to pay attention only to recent papers.

A slightly better estimate of the beginning could be obtained as that point after which 99% of all existing papers were published. This always appears to be 54 years in the past. So, from this point of view, turbulence currently appears to have started about the end of the second world war, corresponding very roughly with our professional careers.

Equation (1) suggests that there were only about five papers from the beginning of time until 1900, or 4.5 from 1874 to 1900. In fact, [144] list thirteen, one each by Hagen, Darcy and Helmholtz (in 1839, 1858 and 1869), two by Boussinesq (in 1877 and 1897), three by Reynolds (1874, 1883 and 1894) and five by Rayleigh (1880, 1887, 1892, 1894 and 1895). This level of fluctuation is considerably above (by a factor of six) the fluctuation level extrapolated from that observed between 1900 and 1965. It does suggest that the last quarter of the 19th century was an exceptional time, and that these men, Rayleigh in particular, were exceptional people. In fact, it is well known that this period was extraordinary [73]. If we take the period from 1864 to 1894, and limit ourselves to the physical sciences, we have Hamilton, Maxwell, Curie, Faraday, Kelvin, Haber, Gibbs, Millikan, Galton, Mendelyev, Rutherford, Westinghouse, Bosch, Marconi, Bell, Rayleigh, Wimshurst, and Hertz (in chronological order), and these are just the ones whose names are instantly recognizable. (Wimshurst may not be instantly recognizable – he invented the Wimshurst generator, a machine that accumulates static charge.) All these people did something noteworthy in this period, and some (e.g., Kelvin) more than one thing.

On a non-statistical basis, it is probably fair to date the beginning of the field from 1874. Leonardo da Vinci, of course, also wrote some interesting things about turbulent flows, and we are neglecting him. In addition, Monin and Yaglom [144] refer in their introduction to several unspecified papers in the first half of the 19th century which remark on the existence of two distinct states of flow.

Please note that we do not yet know what the field has been doing since 1965. It seems doubtful that it has continued to grow exponentially. Based on Equation (1), the doubling time for the number of papers is 8.2 years. When G. I. Taylor wrote Diffusion by Continuous Movements in 1921, there were 2.6 papers per year. When JLL started graduate school, there were 36 papers per year, which could still be easily read by one person. If we can believe Equation (1), there are currently 2000. We doubt this – we believe that the number has ceased to climb exponentially, and the curve is leveling off a bit. Still, there are probably more papers in a year than one person can comfortably absorb.

In fact, of course, we know something about the mechanisms at work here. We know that in 1965 we were still in the initial phase of growth, since the increase in publications was still exponential. Presumably, the working out of a single major idea (say, the statistical approach to turbulence) would produce first an exponential increase in publications, then a gradual saturation, a leveling off, and finally an exponential decay. We have not seen statistics on this canonical life-cycle. However, most fields benefit from the infusion of new ideas at various times, which would upset this simple statistical picture. In turbulence, the idea of coherent structures might be regarded as a new idea that reinvigorated the field. There is no reason other than pessimism to suppose that new ideas will dry up any time soon.

Public support of research in engineering and the hard sciences did not begin until just after the Second World War, which is to say, about half-way through our century of turbulence. We tend to think of current research as being inextricably tied to this public support, and find it difficult to imagine maintaining the present research establishment without this support. Yet, examination of the production of papers during the period 1900–1965 (by which time public support had been in place for 20 years) shows absolutely no effect. The exponential growth in number of papers continues as though nothing had happened. Considering the current parlous state of public support for turbulence, we might find this encouraging – perhaps we will be able to keep on quite nicely without support. On the other hand, perhaps the growth would have leveled off without the start of public support. That suggests that we may be in trouble due to the drying up of support.

In 1945, the US first used the atomic bomb in warfare. The research establishment of the USSR was galvanized. Money poured in from the central government in 1946–1947. The prestige and priveleges of scientists grew enormously. Many people were attracted to science who do not seem to have had much of a vocation. There is no trace of all this in the production of papers.

Kolmogorov became active in turbulence about the time of the Second World War, and had a number of distinguished students. One might have expected that this would create a visible bump in the curve of paper production, but it does not appear to have had any effect. Perhaps the appearance of a Kolmogorov, however extraordinary, is one of those random events from which a field benefits from time to time, in this case perhaps compensating for the effects of the Second World War.

In addition, in 1956, the Soviet Union placed Sputnik in orbit, and the US research establishment was galvanized. Money poured into US universities, which expanded their engineering and hard science faculties substantially. JLL was just about to receive his Ph.D. His thesis advisor (Corrsin) said (roughly) that if we all could hang in there, we would all be department heads. In fact, looking at the record (which cuts off nearly ten years later) there is no indication that anything happened.

We can only speculate as to the reasons. We suspect that the people who are interested in turbulence are individualists, little influenced by external factors. They have always been rare in the population, and the existence of more support does not necessarily cause more turbulence workers to appear.

The curve of paper output dips slightly for the two world wars, but the dips are no larger than the general statistical variability, and have no lasting effect on the curve. If the reader did not know there had been a war at that time, s/he would not conclude from the data that anything extraordinary had happened.

AMY believes that there was a much more significant dip in the Soviet Union, since the graduate student population essentially dried up in the mathematics and physics departments of Moscow State University, and in the physics and mathematics institutes of the USSR Academy of Sciences.

The appearance of promising new ideas and methods that seem significant to us (Kolmogorov's 1941 theory, coherent structures, DNS, chaos, fractals, strange attractors) cannot be identified from the data.

2. A Few Threads

From the very beginning, there have been two major threads in turbulence research. The first concerned the calculation of the practical effects of turbulence, primarily the momentum, heat and mass transfer, associated with the design of devices and their interaction with their environment. The other concerned the physics of the turbulence phenomenon. Both these threads were present in the initial work of Boussinesq and of Reynolds. They are still with us. The practical thread really was two threads – one is technological, and the other geophysical. The geophysical branch might well be called atmospheric and oceanic engineering, since it is motivated by a desire to calculate the effects of turbulence in order to predict the behavior of atmospheric and oceanic phenomena, in which turbulence is only one of many players. A fundamental desire to understand turbulence is not always a basic motivation.

The practical thread is often referred to as the semi-empirical thread. When JLL was a graduate student, Corrsin refused to discuss this material. That attitude can probably be traced back to Liepmann, Corrsin's thesis advisor. In fact, it is the interest of industry in this thread which drives the funding of most turbulence research. If it were not for the hope that we could one day carry out calculations that would be of some use in design, siting, and so forth, essentially no money would be available for any of our research. Much of the work that has been done on this thread is, indeed, semi-empirical, associated in the early period with the names of Boussinesq, Taylor, Prandtl and von Karman, among many others. This is the thread in which we find gradient transport and eddy viscosities. This thread leads to what we now call turbulence modeling (which had its beginning with P.-Y. Chou in 1940 [28]), and much of which is certainly semi-empirical or worse. However, it is not entirely accurate to call the entire thread semi-empirical, since considerable work of a fundamental character has been done in support of this effort. An example is the work of Monin and Obukhov on the parameterization of the atmospheric surface mixed layer (see [144]) which we will discuss below.

Another long thread concerned the establishment of the mathematical basis for the treatment of stochastic fields. There is, of course, much work on this that has no explicit connection to turbulence. The part that is explicitly related to turbulence probably began with Keller and Friedmann's paper in 1925 [89].

We may mention as an aside boundary layer theory, which began in 1904 with Prandtl's famous paper [173]. This subject, and the discipline to which it led, that of matched asymptotic expansions, is not strictly specifically related to turbulence, although it has been applied with notable success to several turbulence problems. However, formal applications of the modern theory to turbulence problems are relatively rare. Nevertheless, we feel that the general approach has strongly influenced how we think about the various thin shear layers, boundary layers in particular, but also wakes, jets and mixing layers. Among other things, the concept of a turbulent boundary layer on an arbitrary body as being driven by the pressure field generated by the inviscid external flow, which is determined by the body shape plus the displacement thickness, is taken over wholesale from laminar boundary layer theory, and has been extraordinarily useful.

We may also mention the advent of large-scale computing of turbulent flows, which began in 1972 with the work of Orszag and Patterson [164], and which has surely grown exponentially since then (we have not done a statistical analysis). Although the Reynolds numbers attainable are still quite limited, direct numerical simulation of turbulence has taken the place of experiment for some simple flows, since it permits access to quantities that are difficult to measure, such as pressure fluctuations. In this connection we should include the DNS of the development of flow disturbances (as a part of nonlinear instability studies) and especially the studies of subcritical instability, where usually the Reynolds number is not too large.

2.0.1. *Semi-Empirical Approaches*

We have already mentioned the early work on the semi-empirical approach, associated with the names of Prandtl [174, 176], von Kármán [241, 242] and G.I. Taylor [218, 219]. High-speed digital computers, of course, did not yet exist, so that the flows that could be computed using these simple models were few. The beginnings of modern turbulence models, with the work of Chou [28], Kolmogorov [103] (who proposed a two-equation model) and a little later, Rotta [186] were still impeded by the general unavailability of high-speed computation. Finally, with Daly and Harlow [39] and Donaldson [43], followed closely by Launder, Reece and Rodi [120] we come to the arrival of large-scale computing on the scene, and the beginning of the modern period of turbulence modeling.

2.0.2. *Simulation*

We should mention here the direct numerical simulation of turbulence, and its close relative, large eddy simulation. DNS began in 1972 with Orszag and Patterson [164]. Orszag and his students made enormous contributions to the techniques necessary for this approach, progressively increasing accuracy and reducing computing time. Large Eddy Simulation, in which only the large scales are resolved, the smaller scales being parameterized, appears to have been invented in 1962 by Smagorinsky [197] and utilized for our sort of flows by Deardorff in 1970 [40], who used Smagorinsky's sub-grid scale model. The approach was extensively developed at Stanford University in collaboration with NASA Ames Research Laboratory (the group that became the Center for Turbulence Research), beginning about 1973 [181] (although others certainly contributed, e.g. Schumann [192, 193]). This same group has heavily exploited both DNS and LES ever since, so that these techniques are strongly associated with their name, although of course these techniques are now used everywhere. Both methods have steadily improved in Reynolds number, and there now exists an enormous data base of archival flows at CTR. Unfortunately, the existence of this data base has not resulted in a corresponding increase in our understanding of turbulence. The computations 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.

2.0.3. *Similarity Laws*

Some of the early results obtained by the semi-empirical approach were related also to the attempt by Prandtl and von Kármán to formulate some elementary similarity laws for the viscous region and the outer layers of developed turbulent parallel wall-bounded flows. These laws have simple forms and may be justified by elementary dimensional analysis; thus they seem relatively fundamental. In 1937 Izakson showed that the simultaneous validity of both these laws, which occurs in the intermediate layers of pipe, channel and boundary layer flows, implies the validity there of the universal logarithmic law for the mean velocity $U(y)$ and the difference $U_0 - U(y)$ (where y is the coordinate normal to the wall, and U_0 is the maximum mean velocity at the pipe or channel centerline or in the free stream above the boundary layer. The logarithmic law for the velocity profile $U(y)$ of the form

$$U(y) = u^*[A \ln(yu^*/\nu) + B], \tag{2}$$

where u^* is the friction velocity, ν the kinematic viscosity, $A = 1/\kappa$ and B are universal constants (and κ is the von Kármán constant), was derived in the early 1930s by von Kármán and Prandtl based on semiempirical arguments, and was well known in 1937. A similar logarithmic law for $U_0 - U(y)$ was then new and shortly later (in 1938) Millikan showed that by summing the two logarithmic laws it is easy to obtain the corresponding 'logarithmic skin-friction laws' which were also derived earlier by Prandtl and von Kármán from semi-empirical arguments (see, e.g., [144]).

The logarithmic velocity-profile and skin-friction laws led to results which agreed quite satisfactorily with the majority of the data; hence these laws were widely used in engineering practice. However values of the coefficients A (or $\kappa = 1/A$) and B of Equation (2) from the available data proved to be rather scattered (this scatter usually did not affect too much the practical applications of log laws). For a long time the most popular estimates of these coefficients were $\kappa = 0.41$ (or 0.40) and $B \approx 5.2$, but many other values of κ and B were also met in the literature. The limits of the range of y-values belonging to the 'logarithmic layer' of log-law validity were also subject to considerable scatter; most often it was suggested that the lower limit of this range was near $y = 50(\nu/u^*)$ (values in the range from 30 to 100 were also sometimes used), and the upper limit is near $y = 0.15L$ (where L is the pipe radius, channel half-thickness, or boundary-layer thickness, and the coefficient 0.15 was also often replaced by another number of the same order). The present state of this matter will be considered later. The Izakson-Millikan approach represents in fact the first application to turbulence of the method of matched asymptotic expansions which later was found to be useful for a number of other physical problems. It was also found that the von Kármán-Prandtl logarithmic law Equation (2) for the mean velocity in the intermediate region of turbulent flows over smooth walls is only one example from a large family of similarity laws describing the profiles of other flow parameters in the same intermediate layer. For example, the logarithmic law of the form of Equation (2) is valid also for the velocity profile $U(y)$ in the intermediate part of wall flows over rough walls covered by homogeneous roughness; in this case the coefficient A does not change but B now depends on the geometric characteristics of the roughness (the derivation of the rough-wall skin-friction law by the above method was also considered by Millikan). Similar to Equation (2), a logarithmic law describes the mean temperature profile $T(y)$ in flows over a heated wall; here the friction velocity u^* must be replaced by the so-called 'friction (or heat-flux) temperature' T^* determined by the heat and momentum fluxes at the wall, the coefficient A is replaced by the new universal constant A_T, and B – by the universal function $B_T(P)$ of the Prandtl number $P = \nu/\xi$ where ξ is the thermal diffusivity. (The log law for $T(y)$ was first proposed in 1944 by Landau in the first edition of the book [115] by Landau and Lifshitz, and the derivation of the heat transfer law for wall-bounded turbulent flows by the Izakson-Millikan method (above) was considered in [86].) The similarity laws for values of the statistical moments of turbulent velocity fluctuations adjacent to the wall and in the outer layers of turbulent wall flows imply that these moments must take constant values in the intermediate ('logarithmic') layer; similarity laws of somewhat more complicated form are valid for correlation functions, spectra, probability densities of turbulent fluctuations and so on (for some examples see, e.g., Townsend's book [226], Monin and Yaglom [144] and the survey by Yaglom [254]). A similarity approach of the same type was applied also by Monin and Obukhov in 1954 to the parametrization of the atmospheric surface mixed layer in terms of the Obukhov length (introduced by Obukhov in the 1940s in a paper based on the semi-empirical method); see, e.g., [144]. These developments gave the flavor of fundamental science to some earlier semi-empirical arguments. However, at present some scientists are inclined to suppose that the simple similarity laws devised by Prandtl and von Kármán, and motivated by seemingly self-evident dimensional arguments, in fact have only limited accuracy. The data on which this opinion is based, and the possible reasons for deviations from logarithmic form of the velocity profiles will be considered below.

2.0.4. *Stability Theory*

We should probably mention stability theory. Another dictum which JLL learned at Corrsin's knee, which probably came from Liepmann, was that stability theory had nothing to do with turbulence. Certainly, the connection of linear, small-disturbance stability theory with fully developed turbulence is remote, and that is the kind of stability theory that dominated the scene half a century ago. (Of course, in industry they still (more-or-less successfully) estimate the location of transition using linear stability theory and empirical growth factors.) There is no question that the loss of stability of the laminar flow, and the amplification of the disturbances present, leads the flow to a new attractor, a new (turbulent) state. Examining this process in various ways seemed to hold greater promise than a direct attack on fully developed turbulence, and so much early work dealt with stability, some of it by the energy-method, and hence of greater relevance. The gradual development of large-disturbance, non-linear stability theory has lead to findings of greater and greater relevance to turbulence; this has faded seamlessly into dynamical systems theory and low-dimensional models, which have certainly been shown to have great relevance for an understanding of turbulent flows [79]. Some recent applications in stability theory of rather abstract achievements of dynamical systems theory will be briefly discussed in Section 2.3.

2.0.5. *Partition*

If we examine the 28 papers between 1900 and 1925, and sort them by topic, we find ten papers on some aspect of stability theory; six on momentum, heat and mass transfer; three each on statistical approaches and geophysical flows (all atmospheric); two on transition in pipe flow; three on fully developed turbulence; and one on twinkling of stars. We see here the practical thread (ten papers) and the physical thread (eight papers) plus stability (ten papers), which should really be included in the physical thread, since they represent an attempt to understand how turbulence gets that way. Hence, practical:10, physical:18. These categories are somewhat arbitrary, like Procrustes' bed; for example, we are identifying as 'fully developed turbulence' papers which consider energy budgets or the influence of turbulent fluctuations on the mean flow in a pipe.

Between 1925 and 1950, 269 papers were published. Of these, by far the greatest number, 123 or 46%, can be classified as fundamental – that is, motivated neither by a practical interest in predicting drag, heat or mass transfer, nor by geophysical interests, but by a desire to understand the nature of turbulence, either by measurements or by constructing a theoretical framework; 59, or 22% are geophysical in motivation; 31, or 12%, relate to prediction of drag, heat or mass transfer; 26, or 10%, are concerned with stability; and 30, or 11%, develop stochastic tools with which to attack turbulence.

2.1. THE MATHEMATICAL BASIS

We must discuss the establishment of the mathematical basis for the treatment of turbulence problems. In general the interrelations between pure mathematics and its applications are quite complex. Of course, the appearance of new mathematical notions and concepts is often stimulated by practical needs. However, in some cases the appearance of a new mathematical theory was due to abstract ideas unrelated to practical problems or was stimulated by practical needs but quickly spread outside the limits of the specific applications which gave rise to it. Then the authors often begin to look for more sophisticated problems to which their theory may be applied. This was just so in the case of irregularly fluctuating functions of one or several variables.

2.1.1. *Kolmogorov 1941*

The modern mathematical theory of stochastic processes of one variable was originated by Wiener [246] who had been stimulated by the works of G.I. Taylor on turbulence, but who considered only one scalar function of time, and applied his results to Brownian motion. Then Kolmogorov [99] developed the rigorous axiomatic foundations of probability theory which included the strict definition of the notion of a stochastic function of any number of variables. Kolmogorov also began with application of his results to Brownian motion but he was looking simultaneously for possible applications of his new theory of random fields. This search attracted his attention to turbulence. He gave full credit to the early attempt by Keller and Friedmann [89] to develop a probabilistic theory of turbulent fields but was mainly interested in investigating the physical mechanism of small-scale turbulent fluctuations. His two short notes [101, 102] on the statistical theory of turbulence included a rigorous statistical description of the fields of turbulent variables as random fields in the sense presented in [99], as well as the definition of a new important class of random fields which had been unknown. Primarily, however, they were devoted to a remarkable attempt to explain the mechanism of small-scale turbulence. To achieve this Kolmogorov stated two general statistical hypotheses which describe the universal equilibrium regime of small-scale components in any turbulent flow at high enough Reynolds number (the existence of such a universal regime was predicted by Kolmogorov). Then he considered the application of these hypotheses to the determination of the shapes of some specific characteristics of turbulent fluctuations. Kolmogorov's theory of 1941 (which is now expounded and discussed in many monographs and surveys) fundamentally changed the state of turbulent investigations and is basic to all subsequent developments of turbulent studies during the second half of the 20th century. Publications devoted to turbulence which were stimulated by Kolmogorov's theory at first involved mainly the Moscow and Cambridge research groups (in Cambridge Batchelor discovered Kolmogorov's papers shortly after their appearance and at once understood their importance) but then enveloped all developed countries.

2.1.2. *Kolmogorov 1962*

Kolmogorov's statistical hypotheses were based on non-rigorous but seemingly convincing heuristic physical arguments. Since the first attempts to compare the predictions of Kolmogorov's theory with the results of specially posed experiments led to confirmation of this theory, the theory at first caused no doubts. However, measurements of small-scale turbulent velocity fluctuations made in the laboratory by Batchelor and Townsend in 1949 [10], and, even more, the measurements in the atmospheric surface layer by the Moscow group in the late 1950s revealed data clearly contradicting Kolmogorov's predictions. This showed that Kolmogorov's similarity hypotheses of 1941, which at first seemed quite convincing, in fact were only approximately true for the velocity field. In 1962 Obukhov [155] sketched some arguments which explained the contradictions by the influence of the spatial variation of the rate of energy dissipation $\epsilon(x, t)$. He produced a crude quantitative estimate of this influence, while Kolmogorov [104], expanding on Obukhov's argument, formulated a generalized version of his two hypotheses of 1941, and supplemented them by a third hypothesis, the three forming the new theory of small-scale turbulence. Kolmogorov's theory of 1962 was consistent with new experimental data for the velocity field and implied a number of additional consequences which could be experimentally verified.

The new Kolmogorov theory at once attracted attention and produced new activity in small-scale turbulence studies. Many results may be found in books by Monin and Yaglom [144], Hunt et al. [85], Frisch [64] and Bortav et al. [16], and in research papers which continue to appear regularly. The modified theory required the introduction of another new class of random fields, and it proved to be linked with a new extension of the arsenal of mathematical weapons used in turbulence.

2.1.3. *Homogeneous Turbulence*

Early attempts were made on a fairly rigorous basis to analyze homogeneous turbulence. In 1956 Batchelor published his well-known book [8], which really dealt with isotropic, homogeneous turbulence. The analysis of homogeneous shear (homogeneous turbulence in the presence of a uniform mean shear) is less familiar; there were two contributions, one from Burgers and Mitchner in 1953 [22] and the other from Craya in 1958 [38].

2.1.4. *Fractals etc.*

The new extension (end of Section 2.1.2) related to the class of functions and point sets considered. Functions which are continuous but nowhere differentiable were first described by Weierstrass in the 1870s. These functions met with a mixed reception from both mathematicians and physicists. Apparently the first use of these functions in a physical theory was due to Wiener [246] who proved that the Einstein–Smoluchowski model of Brownian motion implies that the trajectory of a Brownian particle is nowhere differentiable with probability one (because inertia had been neglected). Moreover, even more exotic curves (and point sets) were constructed at the end of the 19th and beginning of the 20th centuries by Peano, Serpinski, Hausdorff and others; in their works planar curves filled some two-dimensional areas and the planar and spatial sets have fractional dimensions. Some of these curves were selfsimilar, and later one special selfsimilar curve having a fractional dimension was applied by L.F. Richardson as a model of a natural object – a convoluted geographical boundary [132]. Then Mandelbrot showed that selfsimilar curves and more general planar and spatial selfsimilar sets having fractional dimensions (he called such object fractals) give natural models for many spatial patterns of applied origin. Although turbulence was not at first one of the patterns considered, he later found that some features of small-scale turbulence could be modeled by fractals, in particular the dissipation field; see, e.g., [131, 133] and references therein, [205] and [64]. Here again the appearance of a new mathematical technique expanded the sphere of turbulence studies. Fractals have become fashionable, and may be over-used; the final clarification of their real scope in small-scale turbulence must be left to the future.

2.1.5. *Characteristic Functionals*

One more very general mathematical approach to the theory of turbulence is connected with the use of the notion of the characteristic functional (generalizing the notion of the characteristic function used very widely in probability theory since the middle of the 19th century). The characteristic functional determines uniquely the probability distribution of a given random function of one or several variables; it was first introduced by Kolmogorov [100] who indicated its exact form for Gaussian random functions. Later Hopf [81] introduced the characteristic functional of the random velocity field of a turbulent flow and used the system of Navier–Stokes equations to derive the linear functional differential equation for the characteristic functional. At that time no methods of solution of functional differential equations were known and there were no existence or uniqueness theorems for possible solutions; therefore Hopf's paper at first did not attract much attention. More recently, several such theorems were proved, some exact solutions and methods of computational solution were found, and in general considerable progress has been made – see e.g. the books [240] (inspired by discussions with Kolmogorov), [27, 42]. It seems possible that in the 21th century the characteristic functional approach will become a useful part of the statistical theory of turbulence.

2.1.6. *DIA, EDQNM, etc.*

We should not forget to mention here the Direct Interaction Approximation of Kraichnan (DIA) [106], and all its offspring [135]. These shed a great deal of light on the mechanics of the interactions between various wavenumbers. At first it was not clear whether this approach was a model, or an approximation; ultimately it was seen to be a model. It has not proven to be a very useful computational tool, primarily because it has been limited to homogeneous situations, and has proven to be computationally more expensive than a direct numerical simulation.

There are several other approaches of this general type, beloved of physicists, because they appear to capture in different ways, and to different extents (all to a greater extent than the crude engineering approximations of turbulence modeling) the detailed mechanics of turbulence. For example, we may mention the Eddy Damped Quasi-Normal Markovian approximation, or EDQNM [122, 163, 235]. This is much simpler than the DIA, and begins with the quasi-normal approximation for the fourth moments, which was early found to produce negative energies. This was corrected by including a relaxation term (the eddy damping).

We should also mention the Test Field model of Kraichnan [107], and the Renormalization Group approach [122] (associated with the names of Greene, Orszag, Yakhot and McComb). The latter is an organized technique for constructing approximations, which has had some success in predicting the coefficients and forms used in turbulence modeling.

A great deal of effort has gone into the development of these approaches. Whereas turbulence modeling makes assumptions closing the averaged equations at first or second order, these approaches typically make assumptions closing the equations at higher order, or closing the spectral equations, or applying approximation techniques from other branches of physics (e.g.,diagramatic expansions, renormalization group expansions). The hope is, that in this way more dynamical detail will be captured, and the approach will perhaps be capable of reproducing more complex statistics. Because the assumptions appear to be more subtle, less crude, the approaches are more respectable. Generalizing, however, the assumptions are made at a much greater remove from experience; only by extensively exercising the approaches can they be seen to be satisfactory (or not). The complexity of these approaches usually prohibits their use in geometrically complex situations, and the amount of exercise they get is small and limited compared to engineering turbulence modeling.

2.2. LARGE EDDIES/COHERENT STRUCTURES

The last 25 years have coincided with an enormous explosion of interest in the more-or-less organized part of turbulent flows. To be sure, it has been recognized for some 55 years that turbulent flows have both organized and apparently disorganized parts. Liu [127] has documented the first appearance of this idea around the outbreak of the second world war. The idea was probably first articulated by Liepmann [125], and was thoroughly exploited by Townsend [226], but all within the context of the traditional statistical approaches. However, in [19, 20], Brown and Roshko presented visual evidence that the mixing layer, in particular, was dominated by coherent structures, and this captured the imagination of workers in the field, who were ready for a new approach. Within two years, the number of citations in this area had gone up by a factor of four, and within two decades had risen by a factor of ten. A lot of immoderate things have been said about coherent structures, and the statistical approaches that were previously popular; a discussion of this, and a current position on why coherent structures are present in turbulent flows, and why they are present in different strengths in different flows, and how they can be calculated, and when they need to be calculated, and what to do with them once you have them, are all discussed at length in [79]. Briefly, the coherent structures appear to be the result of an instability of (what must be an imaginary) flow with turbulence but without coherent structures, and they can be calculated approximately by using stability arguments of various sorts. It is not always necessary to take them into account explicitly, depending on the particular purpose of the calculation; on the other hand, it can be, for some purposes, very profitable to model the flow as coherent structures plus a parameterized turbulent background, and so construct a low-dimensional model of the flow. Such models can be used whenever an inexpensive surrogate of the flow is needed, and have been very helpful in shedding light on the basic physical mechanisms.

At the same time that Townsend [226] indicated that large-scale structures play an important role in turbulent shear flows, he also considered generalized wall and outer region similarity laws (he was apparently the first to present wall similarity laws for moments of velocity fluctuations and to formulate the general Reynolds number similarity principle). This coincidence is interesting, since in fact the coherent structures violate the ordinary wall and outer layer similarity laws, as Townsend later showed (see [169, 227, 228, 255, 256]). In [169, 227, 228] and [255] it was shown that the influence of coherent structures leads to variability (namely, to dependence on y/L, where L is the boundary-layer thickness) of the root-mean-square values of two horizontal velocity fluctuations u and v in the 'logarithmic region' of the turbulent boundary layer (where these rms-values must take constant values according to the standard similarity laws for wall-bounded turbulent flows). In [256] similar arguments, based on the inclusion of the large-scale- structure contributions, were used to explain the strong deviations from Monin-Obukhov similarity in the convective atmospheric surface layer observed in [168] (according to results of [256], wind properties close to the ground depend on the thickness of the atmospheric boundary layer, which is of the order of a few kilometers). The arguments in these papers have something in common with those of Kolmogorov [104], according to which the influence of organized structures (namely, of structures describing the spatial patterns of the dissipation field) also led to the dependence of the small-scale turbulence on the Reynolds number and the external length scale of turbulence. The large-scale structures considered in the references cited above do not affect the mean velocity profile and thus do not violate the logarithmic mean-velocity law. However, coherent structures of many different types exist in turbulent wall flows and there is no reason to exclude the possibility of the influence of some such structures on the mean-velocity profile. In Section 2.0.3 it was said that the available data on mean-velocity profiles of wall turbulent flows as a rule agree satisfactorily with the logarithmic law Equation (2) but the measured values of coefficients of this law and of the limits of the layer of its validity prove to be rather scattered. Recently two new series of velocity measurements in turbulent pipe and boundary-layer flows at very high Reynolds numbers were published in [258, 162] and [24]. Both groups of experimenters claimed high accuracy for their measurements and stated that their data agree well with the logarithmic law but the values found of coefficients κ and B and of the limits of the logarithmic layer did not agree with each other or with the values which had always been considered to be most precise. According to pipe measurements by Zagarola and Smits [258], $\kappa = 0.436$, $B = 6.15$ and the logarithmic law is valid for $600(\nu/u*) < y < 0.07R$ (where R is the pipe radius). Boundary-layer measurements described in [24, 162] led to values $\kappa = 0.38$, $B = 4.1$ for the coefficients of the logarithmic velocity-profile law which is valid, according to these measurements, for $200(\nu/u*) < y < 0.15d$ (where d is the boundary-layer thickness). If all the measurements described in [258, 162] and [24] are accurate, then the comparison of the results of two groups of authors makes one doubt the universal validity of the logarithmic law Equation (2) with fixed coefficients in a uniquely defined intermediate layer of any wall turbulent flow with high enough value of the Reynolds number. Hence, these data suggest that perhaps the logarithmic velocity-profile equation represents only a useful approximation to the available data. Note in this respect that recently two groups of authors (headed on the one hand by Barenblatt and Chorin [4, 5], and on the other by George [65, 252]) tried to justify theoretically the replacement of the logarithmic velocity profile by a power law (according to George's group the velocity profile in turbulent pipe flow has an even more complicated form). In the case of laws considered in [4, 5], the question of the degree of their agreement with the data of [162, 258] and [24] produced a long vigorous discussion (where the authors of the cited experimental studies objected to attempts by Barenblatt et al. to prove the exactness of such agreement). In [65, 252] it was also stated that the new laws presented there agree well with the available experimental data but the results of the latter two papers have, so far, not been subjected to wide discussion. In any case, the final solution of this controversy would clearly require clarification of the physical reasons which might produce deviations from the logarithmic law and quantitative analysis of the influence of these reasons. Such a solution lies in the future.

2.3. Chaos/Dynamical Systems Theory

During the last 30 years or so, it has been found that certain finite-dimensional dynamical systems were capable of chaotic behavior, that is, possessed a strange attractor. Chaotic behavior is complex, aperiodic and appears to be random, but is, in fact, deterministic. Mechanical systems that behave chaotically display an extreme sensitivity to initial conditions, with solutions initially very close together separating exponentially. This has been called in the popular literature the 'butterfly effect', and has appeared even in such movies as Michael Crichton's *Jurassic Park*;

there is surely no literate person over the age of thirteen who has not heard of chaos and the butterfly effect. It is not difficult to show (using simple models) that this sensitivity to initial conditions is so exquisite that, after a relatively short time, the divergence of the solutions depends on details of the initial conditions that are practically unknowable.

Existing numerical studies of chaotic behavior and of the corresponding strange attractors appear to relate only to the simplest dynamical systems with a few degrees of freedom; see, e.g., well known examples described in [76, 128]. However, for the extension of these ideas to continuous fields, such as the velocity field of a fluid filling a domain, some serious difficulties must be got over. We know only that turbulent motion of a fluid at a fixed space point may be represented by a chaotic function of time, though the motions of fixed Lagrangian fluid particles are deterministic and display the butterfly effect. According to certain definitions, all this may be described by saying that here there exists a strange attractor (see below), although without precise definition of this notion, and the proof of the existence here of just such an attractor, that becomes not much more than a matter of semantics.

The early investigations of instability and transition were mostly based on classical nonlinear dynamical systems theory, originated by Poincaré at the end of the 19th century. Therefore, transition to turbulence was modeled by a sequence of simple Hopf bifurcations (or, what is the same, Poincaré–Andronov–Hopf bifurcations; see e.g. [134]). Every such bifurcation increases by one the dimension of the phase space of the dynamical system, making its behavior more and more complicated and disordered. This scenario of transition was proposed by Landau [114], illustrated by some mathematical examples by Hopf [80] and for many years was considered as the unique plausible scenario; in particular, it was the only scenario considered in the early editions of Landau and Lifshitz [115] and in Monin and Yaglom [144].

However, the rapid development of dynamical systems theory in the second half of the last century, well illustrated by the paper by Smale [198], showed that in fact the Landau scenario often is far from typical or cannot be realized (see, e.g., the last edition of [115]). Smale showed that for many dynamical systems a more probable scenario involves the appearance in phase space of a ‘strange attractor’, a set of very complicated topological structure, to which the system trajectories are gradually attracted. This implies chaotic behavior of the system, and the exponential divergence of neighboring trajectories. Ruelle and Takens [187] were the first to assume that the Navier–Stokes equations have a strange attractor, appearing after a few elementary Hopf bifurcations, and that the resulting chaotic behavior described transition.

Ruelle and Takens did not know in 1971 about the paper by Lorenz [128], who modeled the convective flow of a viscous and diffusive fluid by a simple system of three ordinary differential equations. Numerical integration showed that this simple system displayed behavior corresponding exactly with what would later be described as a strange attractor. Later it was shown (see [76]) that a strange attractor can appear also in some very simple two-dimensional dynamical systems.

After Lorenz's remarkable confirmation of the applicability of the strange attractor theory to a model system of hydrodynamic origin there appeared very many studies of this and other possible scenarios of transition of finite-dimensional dynamical systems to chaotic behavior (such as, e.g., the Feigenbaum scenario of a period-doubling cascade and the Pomeau–Manneville scenario of dissipative transition to chaos; see, e.g., [6, 11]). These scenarios were then combined with mathematical studies of the large-time behavior of solutions of evolution equations and used to explain the features of the transition process in various fluid flows; see, e.g., [6, 11, 12, 75, 115, 116, 210] (the last edition), [3, 32, 50, 112, 239] which represent only a fraction of the works devoted to this subject. The theoretical studies of different transition scenarios include also many attempts to estimate the (often fractal) dimensions of the attracting sets in phase space of fluid flows. Up to now such attempts as a rule have been considerably more successful in the idealized case of two-dimensional turbulence than in real three-dimensional turbulence (see, e.g., [3, 30]). This may be connected with the fact that the strange-attractor scenario of transition to turbulence and the other proposed model scenarios were related to finite-dimensional dynamical systems;they are clearly not universal and their appropriateness for three-dimensional fluid flows is still not clear (see, e.g., [18, 52, 72, 142, 151]). In applications of the finite-dimensional transition-to-chaos models to three-dimensional turbulence usually the simple finite-dimensional approximations are used instead of the full Navier–Stokes equations; however it is unclear to what degree the behavior of such approximate models agrees with the behavior of real fluid flows. Thus, the role and importance of the strange-attractor model and other simplified models of transition to chaos for the theory of turbulence certainly needs further investigation.

So far, the finite-dimensional approximations have been successfully applied only to turbulence near transition or near a wall, so that a relatively small number of degrees of freedom will have been excited, the turbulence will be relatively simple, and its description by a mechanical system of relatively few degrees of freedom is fully justified. As the system moves farther from transition, or from a wall, more and more degrees of freedom are excited, until the structure of the attractor becomes so complex that the system must be treated statistically. The ideas from dynamical systems theory have been successfully used to analyze the low-dimensional models that have been constructed of turbulence near a wall or near transition. See also [79].

3. Density Fluctuations

We are interested in density fluctuations primarily because of our interest in aerodynamic noise, compressible flow, the mixing of density inhomogeneities and fluctuations in index of refraction. We are giving this topic (for the most part) separate treatment, because it usually requires different approaches. Mixing of density inhomogeneities is almost entirely geophysical (atmospheric and oceanic), and that will be covered in Section 4.3. We will mention here several laboratory flows involving the mixing of scalars.

Since 99% of the papers on density fluctuations were published since 1946, we must ask what was published before that date. We have found four papers before 1946 which dealt with density fluctuations: one by Richardson in 1920 [183], two by Hesselberg in 1924 and 1926 [77, 78], and one by Dedebant and Wehrle in 1938 [41].

In [183], Richardson essentially derives the gradient Richardson number by physical reasoning.

The other papers all deal with density weighted averaging. There were, in addition, works by Van Mieghem in 1948 and 1949 [232–234], by Blackadar in 1950 [14] and finally by Favre in 1958 [54–57], on this subject. Hesselberg wanted to maintain the gas law using averaged quantities. If a customary average is used for pressure and density, and a density weighted average is used for temperature, the perfect gas law is preserved.

As we know, the density-weighted average is now used extensively in the analysis and modeling of flows with large density fluctuations, and is associated exclusively with Favre's name, even though he was the sixth person to favor this approach over a 34-year period. This is some sort of an object lesson in the progress of science, if we only knew what it meant. Certainly, Favre pushed this approach much more energetically and persistently than the others had, and in a more comprehensive, organized and fundamental way. However, it probably also has something to do with the evolution of the field, which had in 1958 a serious need for such a tool, which was no more than a curiosity in 1924.

3.1. Aerodynamic noise

In the 1950s civil aviation was expanding rapidly. Airlines were changing to jets, runways were being extended, and new airports were being built. The noise of aircraft taking off and landing was a serious public problem. Into the breach stepped Lighthill [124] and Proudman [178], whose papers essentially defined the new field of aerodynamic noise. Within a decade there were more than one hundred papers on the subject, and a great deal of controversy. At this point Laufer, Ffowcs Williams and Childress, in a definitive AGARDograph [117] explained Lighthill, and brought order to the field.

At the present time, the production of aerodynamic noise by subsonic flows is regarded as no longer a problem. It is well understood, and existing theories are satisfactory for computation. The treatment of supersonic jets, however, is entirely empirical. For example, (Dennis Bushnell, private communication) water droplets are routinely injected in supersonic jets to suppress noise. No theory or computational technique exists to predict this effect. The best treatment of aerodynamic noise from supersonic jets can be found in [213]. There are three major sources of noise in these jets: the so-called screech tones, broad-band shock-associated noise, and turbulent mixing noise. We are able to compute the first two, but have no way of computing the turbulent mixing noise.

In general in this paper, we are dealing with the past, not the present. In this case, however, we would like to mention a paper from 2000 [15]. This paper is noteworthy because it is a Large Eddy Simulation of a high subsonic ($M = 0.9$) jet at reasonably high Reynolds number ($\mathrm{Re} = 65{,}000$), using a Smagorinski sub grid-scale model. The point here is not the sub-grid scale model, which is primitive. This is a direct calculation of the acoustic and the aerodynamic field, which is difficult for a number of reasons, not the least being the great disparity in levels and scales of the two fields. Non-dispersive, non-dissipative numerical techniques were used, with non-reflective boundary conditions (both a radiation condition plus a beach or sponge at the downstream boundary). DNS of the same flow, which had to deal with the same difficulties, had appeared a few months previously by Freund [61, 62].

3.2. Supersonic flow

During the 1950s and early 1960s this was a very popular area. However by the late 1960s most questions that appeared critical at the time had been answered. In consultation with the major laboratories, the agencies decided to withdraw their funding, and the majority of the supersonic tunnels were decomissioned. At the present time, there are only two serious supersonic turbulence tunnels still active, so far as we are aware: the tunnel at IRPHE in Marseille (formetly the IMST of Favre) in which Gaviglio and his students did their work, and the tunnel at Princeton, in which Bogdanoff and his students did their work.

There is now a resurgence of interest in this field, due to the interest in the High Speed Civil Transport (the second generation supersonic transport) and the U.S. National Aerospace Plane (the so-called Orient Express, the hypersonic transport). Neither one of these will probably be built, for complex political reasons, but it appears likely that something in this speed range will eventually be built, and there are certainly still a number of unanswered questions about supersonic turbulent boundary layers. All this serves to explain why the investigation of supersonic turbulent flows proceeded slowly during the 1960s, 1970s and 1980s, only becoming more active during the 1990s. For this reason, we will mention more recent works than we have done in other sections.

The noteworthy early papers in supersonic turbulence begin with that of Kovasznay in 1953 [105]. There he showed that compressible turbulence could be decomposed into three modes: the vorticity, sound and entropy modes. This was also shown independently and earlier (1948) by Yaglom in [253], but the paper was little known in the West. If the fields are weak, the three modes obey independent linear differential equations. If the fields are strong, the equations are coupled. Kovasznay measured the three modes in a turbulent boundary layer at $M = 1.7$.

He found that the fluctuation field was like that of a low-speed boundary layer, except for the intense temperature spottiness resulting from the large heat transfer. The sound mode was quite audible, but energetically negligible.

In 1962 we have Morkovin's paper [148], in which he proposed what is now known as Morkovin's hypothesis and the strong Reynolds analogy: that the structure of turbulence at high Mach numbers is unaffected, because the *fluctuating* Mach number is still low; only the mean properties are affected. This, of course, explains Kovasznay's observations.

We should also mention the Rapid Distortion Approximation, which was used by Dussauge [47, 48] to describe the passage of turbulence through a shock.

We will also mention here a few notable experimental investigations of supersonic flows; these could as well be cited under the experimental section. Experiments in supersonic flows have their own special problems, notably the large forces experienced by the probes, vibration and shocks formed on the probes. So far as shear layers are concerned, we may mention the work of Brown and Roshko [20] and of Clemens and Mungal [29]. They looked at the coherent structures and entrainment in the supersonic mixing layer.

The structure of the turbulent boundary layer at high Mach numbers has only been extensively investigated fairly recently. We may mention the 1988 investigation of a Mach 2.3 boundary layer using the laser Doppler anomometer by Elena and Lacharme [51], as well as several investigations of organized/coherent structures by Horstman and Owen (1972) [82], Spina and Smits (1987) [203], and Donovan, Spina and Smits (1994) [44]. For the first time flow visualization techniques have been used in a high Mach number flow, giving density cross-sections by ultra violet Rayleigh scattering in Smith, Smits and Miles (1989) [199].

Computational fluid dynamics has, of course, begun to make a contribution to the understanding of supersonic flows. Lee, Lele and Moin (1993) carried out direct numerical simulation of isotropic turbulence interacting with a weak shock wave [121]. Very recently, Guarini et al. [74] did a direct numerical simulation of a supersonic turbulent boundary layer at $M = 2.5$.

3.3. Scalar mixing

As we noted above, most of the material on scalar mixing will be covered in Section 4.3 on geophysical turbulence.

Scalar fields differ markedly from velocity fields in turbulent flows, in that their strong local anisotropy violates the Kolmogorov-41 similarity hypothesis. This aspect will be covered in Section 4.2.6.

We will mention here only a few works on scalars in laboratory flows, recent, since such works are relatively rare. In particular, Dowling and Dimotakis (1990) [45] investigated similarity in the concentration field of gas phase turbulent jets. Miller (1991) in his thesis [138–140] looked at Reynolds number dependence and interface characteristics of mixing in high Schmidt number turbulent jets, and Panchapakesan and Lumley (1993) [167] made extensive measurements in an axisymmetric jet of helium into air.

3.4. INDEX OF REFRACTION FLUCTUATIONS

This area was begun in 1941 by Obukhov [156, 159–161], probably motivated by atmospheric short wave propagation. At the time (before his Ph.D. defense) he was working in the Institute of Theoretical Geophysics of the Academy of Sciences of the USSR, which included a small laboratory on atmospheric wave propagation headed by Khaikin. In this laboratory was then working V.A. Krasil'nikov, who became one of Obukhov's friends. It is possible that Krasil'nikov's interest, and the close friendship, was the real motivation for Obukhov's interest in wave propagation. Tatarski, who was a student of Krasil'nikov at Moscow State University, picked up the thread in 1953 ([214–216]; see also [217], extensively revised and supplemented), later joining the Institute of Atmospheric Physics headed by Obukhov. Obukhov's 1953 paper [159] was motivated by Tatarski's theoretical work and the experiments of Kallistratova, a student of Obukhov. Batchelor in 1955 [7, 9] appears to have independently contributed to this area. It is perhaps worth explaining to young readers that until Sputnik in 1957 the west did not pay much attention to the Soviet literature. Batchelor did identify Kolmogorov's 1941 papers [101, 102], and the paper of Obukhov in 1949 on the temperature spectrum [158], but he evidently missed the index of refraction paper, since his is quite different.

The 1941 Obukhov paper [156] assumes that the velocity field is homogeneous and isotropic, and computes the scattering due to the velocity fluctuations from a region with a size of the order of 100 wavelengths, large compared to an integral scale. He obtains a perturbation solution to first order in u/c (where u is the r.m.s. turbulent fluctuating velocity, and c is the speed of sound), and computes the mean energy scattered at a given angle from the incident dierection.

The 1953 Obukhov paper [159] considers only index of refraction fluctuations, and ignores the velocity fluctuations. He describes this as applicable to the scattering of either sound or light, but of course it is a better approximation for light. His motivation here was probably stellar scintillation, radar scattering, and scattering in short wave communication links. He was commenting on some work by Krasil'nikov [108–110]. Krasil'nikov assumed a geometrical optics approximation, and obtained a result in which the variance of the logarithm of the scattered amplitude is proportional to the 3/2 power of the thickness of the turbulent layer. This did not agree with experiment. Obukhov obtained a general solution, and explored the applicability of Krasil'nikov's approximations. He found that the variance of the logarithm of the amplitude is proportional to the 1/2 power of the layer thickness when the layer is thin, and the 3/2 power when the layer is thick.

In 1955 the School of Electrical Engineering at Cornell invited Batchelor to spend the summer, and deliver a series of lectures on the scattering of radio waves by index of refraction fluctuations in the atmosphere. The substance of these lectures became [7, 9]. In [7], it is clear that Batchelor knew of the work of Pekeris (1947) [170] and of that of Villars and Weisskopf (1954) [236], as well as that of Obukhov (1949) on the temperature spectrum [158]. Batchelor considered single scattering from small regions – the spacial structure of the field did not enter. In this sense, it is considerably less general than the 1953 work of Obukhov. Batchelor does correct the Villars and Weisskopf estimates of ℓ and u^2 (the turbulence scales), which he shows correspond to a value of the dissipation four orders too high. At least in part, the errors in Villars and Weisskopf came about because they were not familiar with [158] or with [36].

3.5. COMBUSTION

In combustion, the density changes by a factor of roughly seven. This renders useless the approximations that are habitual in other fluid mechanical situations, most of which (e.g., the Boussinesq approximation) depend on the assumption that the density fluctuations are small relative to the mean density. These large density differences interact with pressure gradients and real or apparent body forces, notably centrifugal. This may be particularly important in gas turbines. In this situation, the density fluctuations are probably no more than perhaps a factor of 1.5 (since combustion has finished before the gases enter the turbine), but 1.5 is more than large enough to cause interesting dynamical effects. Favre averaging is universally used, often with turbulent models appropriate for constant density flows.

This field is relatively young and rapidly evolving, so that there are no 50-year old classical papers to which we may refer. We may mention here only Stårner and Bilger (1980) [207], who investigated a flame with a mean axial pressure gradient – they found that the mean pressure gradient term (which is multiplied by the velocity-density correlation) was of the same order as the maximum shear production. Takagi et al. (1985) [212] examined a confined swirling jet, and found that the corresponding term was a major contributor to the Reynolds stress balance, strongly suppressing the turbulence. For a general review, see [13].

3.6. LOW DENSITY JETS

We mention this subject here because it has aroused a great deal of interest in the community that studies density fluctuations, and not because it has a long history.

When a low density jet exits into higher density fluid, the convective instability changes to an absolute instability as the jet density decreases [146]. The appearance of a finite region of absolute instability leads to self-sustained oscillations [147, 206]. A supercritical Hopf bifurcation appears as the density is lowered [179]. And finally, these self-sustained oscillations are global modes [83]. These oscillations lead to the formation of intense side-jets [145], which increase the spreading rate of the parent jet. The radial ejection associated with these side jets appears not to be directly related to the formation of the primary vortex rings, but rather to be due to coherent streamwise vortex pairs in the braid region. If this is so, this is probably the same instability mechanism at work in the surface layer of the turbulent boundary layer, and in the formation of Langmuir cells.

4. Experiments

The study of turbulence has been supported by enormous numbers of valuable experiments. Some of these broke new ground, exploring areas that had not previously been examined, while others investigated in depth areas that were already known. We will, for the most part, confine ourselves to the first category.

The experimental part of turbulence may be said to have begun with the observations of Osborne Reynolds in 1883 [180]. These were qualitative in nature, but were the first observations of instability and transition in pipe flow.

We may also mention the very careful, detailed experiments of Nikuradse [152–154] in 1930–1933, on turbulent pipe flow in smooth and rough pipes. Although only mean velocity profiles and drag were measured, the results served for decades as a base for our understanding of this technologically important flow.

G.I. Taylor reported in 1938 [220] on turbulent correlations and spectra that had been measured for the first time by Simmons.

The intermittency of the dissipation attracted a great deal of attention beginning in 1959 (and continuing to the present), when Kolmogorov and Obukhov presented their revised hypotheses at a meeting at Favre's Institut de Mécanique Statistique de la Turbulence, in Marseille. These hypotheses included the effect of the intermittency, which slightly changed the slopes of many functions from the values predicted by simple similarity theory. However, the first measurements of this intermittency of the dissipation had been made ten years earlier by Batchelor and Townsend [10].

4.1. Experimental techniques

4.1.1. *Measurement Techniques*

This is perhaps a good place to mention the evolution of turbulence experimental techniques over the years. Visualization was probably the first technique. We should mention Leonardo da Vinci [237], who observed in turbulent flows the difference between the mean flow and the turbulent fluctuations, and specifically commented on it. Visualization was used by Reynolds [180] to identify the onset of turbulent flow in a pipe. We are not ignoring Pitot [171] who in 1732 constructed a device with two glass tubes (one bent and one straight) with which he measured the speed of the Seine; although he comments on the presence of velocity fluctuations, and associates these with shallow water and/or rough bottoms, he measured only the mean velocity.*

The hot-wire anomometer was developed around the turn of the century (see the excellent bibliography of Thermal Anemometry by Peter Freymuth [63]). It appears to have been used for air velocity measurements in the first decade of the new century. By 1924 it had become part of the arsenal of the fluid mechanics laboratory of Burgers, and was used by him and van der Hegge Zijnen [23, 58], to measure mean velocity profiles.

Following this, the hot-wire was used by Huguenard and co-workers in 1926 [84] to make atmospheric measurements at two heights. In 1929, Dryden and Kuethe [46] used the hot-wire to make measurements in the boundary layer and in cylinder wakes. In 1934, E. Richardson [182] used the hot wire to make measurements behind a spinning cylinder in a uniform flow, behind a wing at an angle of attack, in a jet, and in an organ pipe. Finally, Gödecke [70] in 1935 used the hot wire to make remarkably accurate measurements of the lateral velocity structure function in the atmosphere.

The Laser Doppler Velocimeter arrived sometime in the early 1960s, and the particle image velocimeter perhaps ten years later. These are only the most commonly used – techniques have proliferated, and there are now many others. Visualization, for example, is still with us, but now it involves illumination by laser sheets generated in various ways. Hot wires led to hot films, for use primarily in liquids. People involved in combustion use many sophisticated excitation/interrogation techniques, some involving the use of digital image capturing devices. We will restrict ourselves to an historical perspective.

4.1.2. *Signal Processing*

Signal processing has likewise undergone a considerable evolution since the early period. Initially, the experimentalist captured his data in real time using amplifiers, spectral analyzers, oscilloscopes, and averaging by eye. The oscilloscope was the instrument of choice for measuring probability densities. Mean values of fluctuating variables were obtained by staring at a meter for a few minutes, estimating the mean position of the wildly fluctuating needle, and writing down the number on a clipboard. JLL was employed in this manner as a graduate student. It is frightening to consider that the variance of even a Gaussian variable has a standard deviation of 140% [129], with a quite long tail, and that many of the numbers on which we have come to depend were obtained in this way.

Eventually (roughly, the mid-fifties) operational amplifiers arrived, and data were processed in real time at the experimental site using squaring circuits, averaging circuits, and so forth, cobbled together on a plug-board from non-linear components like diodes and thermistors.

* Pitot's paper predates the metric system (as well as the revolution), and all measurements are given in feet and inches.

Tape recorders also became available a little later. This was a boon for field experimentalists, permitting measurements to be captured and taken home for processing. Unfortunately, for some two decades the quality of the magnetic material available for tape fabrication in the Soviet Union was considerably inferior to that available in the West, so that the West had an unforseen advantage, extending also to digital computers, which advanced much more rapidly in the West. At the time JLL said that it was the scarcity of good tape recorders and computers which forced the Soviets to think harder and be more clever.

Finally, analog/digital conversion and digital signal processing in real time arrived (say, in the early seventies). This gives the impression of great accuracy, but does not necessarily reduce errors.

4.2. The Modern Period

4.2.1. *Cambridge*

We come now to the beginning of the modern period in experiments. It is convenient to sort the experiments from this period by institution. The laboratory of Townsend at Cambridge was responsible for much of the early work on isotropic turbulence [225]. Corrsin carried on a correspondence with Townsend for several years regarding the isotropy of grid turbulence. Corrsin's measurements indicated that turbulence in Baltimore was not quite isotropic, while Townsend continued to refer to his turbulence as isotropic. At one point, Corrsin wrapped up one of his grids and sent it to Townsend. There was no reply. Ultimately, the difference was traced to X-wire calibration and the hot-wire length correction. In fact, as Corrsin had claimed, the grid turbulence was not quite isotropic (see below).

In 1956 Townsend published a work [226] that would indeed have a profound influence on the development of the turbulence field, and which has stood the test of time. We have discussed elsewhere the idea of coherent structures. This idea was introduced much earlier, but Townsend was the first to explore it in depth, with the support of his own extensive measurements. He showed that many turbulent flows contain coherent structures of various relative strengths.

4.2.2. *U.S. National Bureau of Standards*

At this time the U.S. National Bureau of Standards had a group doing experiments in turbulent flow, with which the names of Dryden, Schubauer, Skramstad and Klebanoff are associated. In a series of absolutely definitive papers [96, 189–191] they explored instability and transition in the boundary layer, defining the phases of this type of transition that we still use today.

4.2.3. *CalTech*

Under the guidance of von Kármán and Liepmann, the California Institute of Technology produced several definitive early experimental works. In particular, Corrsin, Laufer, Coles and Roshko were all Liepmann's students. In 1942, for his Engineering thesis, Corrsin made measurements of the turbulence downstream of a classical grid, exploring the slight lack of isotropy, and the influence of the hot-wire length correction and calibration techniques, presaging the later controversy with Townsend on these points [35]. Corrsin, in 1944, also made measurements in the flow downstream of a square-mesh grid, determining that the flow became unstable (the jets coalescing randomly) when the solidity exceeded 0.42 [34]. In 1951 and 1954 Laufer made definitive measurements in the channel [118] and the pipe [119], which served as touchstones for decades (and, in fact, still do). Brown and Roshko in 1971 made measurements and took photographs [19] in the mixing layer from an undisturbed source, demonstrating graphically that this flow is dominated by coherent structures. Their paper started the avalanche of coherent structure papers, even though the concept had been in the literature for more than three decades, and an entire book had been devoted to the idea in 1956 [226].

4.2.4. *Johns Hopkins*

At Johns Hopkins, an extensive series of measurements was made behind a heated grid [94, 95] and using a contraction to improve the isotropy of the grid turbulence [31]. In addition, some of the earliest measurements were made in supersonic flow, by Morkovin and his student Phinney [149].

4.2.5. *High Reynolds Numbers*

In the realm of very high Reynolds numbers, we must mention the early work of Grant, Stewart and Moilliet [71], who measured the velocity spectrum in a tidal channel, providing many more decades of $-5/3$ behavior than had ever been seen at the time, as well as a considerable part of the dissipation range of the spectrum, which was completely unexplored at that time. Later many data at high Reynolds numbers were obtained in atmospheric measurements; such measurements have specific difficulties, and we will not consider them here. Measurements at very high Reynolds numbers are of great interest to theoreticians, but they are very difficult to make, because they require apparatus which is either very large, or very fast or filled with a fluid of very high density. There are such facilities, but they are not usually available to modest scientists. JLL's old boss of many years ago, Wislicenus, always wanted to make measurements in the penstock of Grand Coulee Dam, which would certainly provide splendid values of the Reynolds number. Unfortunately, this was never realized.

We may mention here a few modern experiments which have achieved very high Reynolds numbers. Let us begin with turbulence studies based on the use of very large wind tunnels designed for the needs of the aviation industry, where it is often necessary to accommodate entire aircraft in the tunnel. In 1994 Saddoughi and Veeravalli [188] made measurements in the boundary layer of the 80×120 ft wind tunnel at NASA Ames. They showed that the high wavenumber boundary layer turbulence at these Reynolds numbers is indeed isotropic to second order (Shen and Warhaft have recently shown that the small scales are still anisotropic at higher order even at high Reynolds numbers – see [194] and the related remarks in Section 4.2.7.).

Russian measurements (see, e.g., [172]) were carried out in the big wind tunnel at TsAGI (Central Aero- and Hydromechanical Institute near Moscow - actually in the small town of Zhukovsky which grew up around TsAGI, named for the founder and first director of the Institute, of Kutta-Zhukovsky fame). Similar French experiments were performed in the ONERA wind tunnel in Modane (see [68]).

In 1996, Tabeling and his colleagues [211] made hot-wire measurements in low-temperature Helium, achieving values of R_λ up to 5040. They measured probability density functions, skewness and flatness factors.

Finally, Lex Smits at Princeton has been responsible for designing and building the Superpipe, which circulates air at three atmospheres, and achieves its high Reynolds numbers in that way. This is a descendant of the Cooperative Wind Tunnel, which was built in California just after the Second World War, by a consortium of aircraft companies. It too was pressurized (to four atmospheres) to raise the Reynolds number. Relatively few fundamental measurements were made in this tunnel; in 1961 it was dismantled, and at the last minute the CalTech people were allowed to run two fundamental experiments, in which they investigated the high Reynolds number wake of a circular cylinder, and a high-Reynolds number grid turbulence [92, 93]. Such facilities are expensive to build and operate, and keeping them fed is always difficult. Data from the Superpipe are beginning to appear [258]. This was discussed in Section 2.2.

4.2.6. *Scalar Anisotropy*

The subject of anisotropy of scalars at high wavenumbers has grown gradually over the last thirty years, and is still in active development. It is of great interest to theoreticians because anisotropy at high wavenumbers is not consistent with the idea of local isotropy which Kolmogorov suggested in 1941 [101]. The ideas put forward by Kolmogorov in 1941 have been of enormous value, and have proven to be for the most part very successful. So far as the velocity is concerned, it is only as measurements have become more precise and extensive that inconsistencies have been found. Scalars are another story – scalar anisotropy is a significant and easy-to-observe effect. For example, odd order temperature structure functions take non-zero values, although for locally isotropic scalar fields they must vanish (this circumstance was also considered in [244]).

The anisotropy of scalars at high wavenumbers has to do with the ramp-cliff structures that are formed in these flows. The first measurements showing this anisotropy were reported by Stewart in 1969 [208], but no connection was made with the ramp-cliff structures. These were first observed in the atmospheric surface layer by R.J. Taylor in 1958 [221]. Possibly the best known paper on the subject, that first explained the local mechanism that produces the ramp-cliff structures, is by Gibson et al. in 1977 [66]. In 1986 Antonia et al. contributed to the topology [2]. Sreenivasan summarized the position in 1991 [204]. Finally, Warhaft has made extensive measurements at higher Reynolds numbers than have previously been obtainable in the wind tunnel,⋆ using an active grid [244]. See also the survey by Shraiman and Siggia [196].

4.2.7. *Intermittency*

There are several kinds of intermittency. In one, the boundary that separates turbulent fluid from non-turbulent fluid is quite sharp, and a fixed probe passes into and out of turbulent fluid. This type of intermittency was first explored by Corrsin and Kistler [37].

In what is sometimes called internal intermittency, the dissipative structure of the turbulence is not uniformly distributed. Kolmogorov called attention to this in his paper of 1962 [104]. In 1972, Corrsin published measurements by Corrsin and Kuo [111]. In 1984, Antonia, while on leave in France, measured higher order velocity structure functions in collaboration with the Grenoble group [1]. Later such measurements, and the determination of such functions, from numerical simulation data were performed many times to determine the scaling exponents in the relationship $D_n(r) \propto r^{\zeta_n}$, which describes the behavior of the velocity structure functions $D_n(r)$ of order n in the inertial range of distances r. The deviation of the exponents ζ_n from the values $n/3$ which correspond to Kolmogorov 1941 is a convenient characterization of the intermittency. Finally, Shen and Warhaft [194] have just published extensive measurements taken in unusually high Reynolds number wind-tunnel turbulence generated with an active grid, showing that moments above the second have no tendency toward isotropy with increasing Reynolds number; quite the contrary. The measurements were made in a homogeneous shear flow, and the anisotropy was found in both the dissipation and the inertial ranges.

4.3. GEOPHYSICAL TURBULENCE EXPERIMENTS

4.3.1. *Atmospheric Turbulence*

Atmospheric turbulence is of great interest not only for meteorologists and aerospace engineers, but also for fluid mechanicians since the atmosphere provides us with an inexhaustible source of developed turbulence at very high Reynolds numbers. Early important studies of this turbulence were described in the classical papers by G.I. Taylor (e.g. [218]) and L. Prandtl (e.g., [175]) while some early hot-wire measurements of wind-velocity fluctuations were mentioned in Section 4.1.1

⋆ Warhaft and Shen [194] have obtained values near 1100, while Kistler and Vrebalovich [92, 93] obtained only 750 according to [69]; Gibson, working in Corrsin's round jet, obtained values of 780 [69]. It is straightforward to estimate that to obtain 1100 in the round jet would require a doubling of Gibson's jet Reyolds number.

above. The first monograph on atmospheric turbulence [123] was published in Germany in 1939 – just before the beginning of World War II. Then the work in this field was almost fully stopped, but did not disappear completely – during the war years Obukhov completed an important theoretical investigation which was published only after the war in 1946 (see [25, 157]). This investigation served as a foundation for the general similarity theory of the atmospheric turbulence surface-layer developed by Monin and Obukhov, which became known in the West from the books by Priestley [177] and Lumley and Panofsky [129] (see also the book by Monin and Yaglom [144]). This theory stimulated the wide range of experimental studies of atmospheric turbulence which developed after the war simultaneously in the US, Canada, USSR and Australia. Up until 1970 the measurement results obtained in different countries usually agreed upon the general shapes of the curves (as a function of wavenumber, for example; e.g. the five-thirds laws were confirmed everywhere), but the numerical values of statistical quantities like turbulent fluxes and fluctuation variances often differed quite significantly. Therefore in 1970, the various groups agreed to meet at the field station near Tsimlyansk, in the southern steppe of the former Soviet Union. Everyone brought his instrumentation and data processing equipment, and they all measured approximately the same thing at approximately the same time. The major instruments were the US sonic anemometer, the Kaijo Denki sonic anemometer, the Australian Fluxatron, and the Soviet sonic anemometer, hand made in the workshop of the USSR Institute of Atmospheric Physics. In this way, they sorted out the discrepancies in calibration, response, processing and so forth. The USSR Institute of Atmospheric Physics was the host, and was represented by Tsvang, Koprov, Zubkovskii and Yaglom (though not an experimentalist, Yaglom was head of the Atmospheric Turbulence Laboratory at the Institute*). Australia was represented by Dyer of CSIRO, the USA by Hicks from the University of Washington and Canada by Miyake and McDonald, from the University of Vancouver (Stewart from Vancouver participated in the preparation of the paper describing this work, but was not present at the experiment. However, without Stewart's visit to Tsimlyansk for a few days in 1963 – the first such visit for which permission could be obtained – the 1970 experiment probably would not have been possible). Businger of the University of Washington appears to have been present, although he did not take part in the preparation of the paper.

Tsimlyansk has been a regular summer field station of the Institute of Atmospheric Physics for more than 50 years up to the present time [229]. The data from that first summer have served as a touchstone in atmospheric turbulence [230].

Before the collapse of the Soviet Union, there were only two successful attempts of the Soviet Group to participate in experiments abroad. Before 1970, Koprov and

* This contradiction was clear to Obukhov too; the laboratory had a longer name: 'Laboratory of Atmospheric Turbulence and Statistical Methods' and it included a small group of Yaglom's former students applying the theory of random processes and fields to meteorological problems. Later the experimental group was made a special subdivision of the laboratory under Tsvang's leadership (an administrative change that both welcomed).

Tsvang participated in a Canadian experiment arranged by Bob Stewart. A much more ambitious experiment took place in 1976 in Australia – the International Turbulence Comparison Experiment (see e.g., [49, 60]), involving groups from the USSR, US, Australia and Canada. The USSR was represented by Tsvang, Koprov, Volkov, Zubkovskii and others. For approval of these visits it was important that soviet participants be politically squeaky clean, a characteristic not well-correlated with their other characteristics.

Slightly later (1972–1974), extensive measurements were made in Kansas by Kaimal and Wyngaard of the US Air Force Cambridge Research Laboratory. These data have also served as a benchmark for decades. Julian Hunt now claims (private communication) that these data are seriously filtered at low wavenumber. There are evidently data of Högstrom at Upsala which were suppressed for decades because they didn't agree with the Kansas data at low wavenumber, which suggest the presence of elongated coherent structures. The Högström data are also not fully reliable, because they were obtained under conditions of much worse terrain inhomogeneity than in Tsimlyansk or Kansas. The Högström data also contradict Monin-Obukhov similarity theory, but this does not mean that these data are necessarily incorrect.

4.3.2. *Oceanic Turbulence*

We have already mentioned in Section 4.2.5 the measurements of Grant, Stewart and Moilliet in a tidal channel.

In the ocean, extensive basic turbulence measurements were made by Carl Gibson of the University of California at San Diego (La Jolla), and by Monin and Ozmidov of the Soviet Union's Institute of Oceanology, joined from time to time by Kolmogorov. These measurements were made on the FLIP (which Gibson believes stands for Floating Instrument Platform), and from the *Dimitri Mendeleev*. FLIP was a largely cylindrical vessel of great aspect ratio, with a bridge on one end. In transit to a measurement site, it was horizontal and floated on the surface. At the site, tanks were flooded, and the vessel upended to float vertically, mostly submerged, with the bridge out of water. It thus provided a stable platform for measurement, since it essentially did not respond to wave motion.

The *Dimitri Mendeleev* was one of perhaps three major oceanographic vessels belonging to the Institute of Oceanology of the Soviet Union. Fully outfitted as an oceangoing laboratory, it could visit any ocean on earth. Turbulence measurements were made primarily from towed arrays of hot film probes [143]. Gibson joined the Russian group on board the *Dimitri Mendeleev* for the 11th Cruise, and joint measurements were made [67]. It is tragic that at the present time, due to the economic situation in Russia, the *Dimitri Mendeleev* and other oceanographic vessels belonging to the Institute of Oceanology have been pressed into commercial service. We understand that the *Academician Keldysh* and its specialized underwater vehicle were involved in the filming of *Titanic*.

4.3.3. *Laboratory Measurements*

In the laboratory Phillips and his students carried out several clever experiments [87, 88] shedding light on the way that a turbulent layer propagates into adjacent stably stratified non-turbulent fluid. This happens in the ocean at the bottom of the surface mixed layer, and in the atmosphere at the top of the surface mixed layer.

4.4. THE WALL REGION

The wall region of the turbulent boundary layer came under intense scrutiny during the 1960s and early 1970s. The Stanford Group were responsible for visualization that brought to light the coherent structures in this region, and identified the bursting process [90, 97, 98]. Willmarth and his students were responsible for extensive hot-wire measurements, in which he separated the contributions to the Reynolds stress into quadrants, quantifying the bursting process [247, 248]. Wallace, Eckelman and Brodkey made hot film measurements at nearly the same time, in which they also did a quadrant anaylysis of the Reynolds stress [243].

Measurements of pressure fluctuations have been rare: away from the wall, no satisfactory probe has been developed (though not for lack of trying). At the wall, measurement is at least possible, but the magnitude of the fluctuations in air is small, and the measurements difficult. It is easier in water because the pressure levels are higher, but the instrumentation becomes more difficult. Willmarth and a series of three students made measurements in air during the sixties [249–251], as did Bull [21] and Corcos [33].

Quite recently DNS of channel flow [113] has begun to shed some light on the behavior of the pressure near the wall.

4.5. LAGRANGIAN AND PARTICLE VELOCITY AUTOCORRELATIONS

There are very few of these difficult measurements. Snyder and Lumley measured velocity autocorrelations of various particles (of different inertia and terminal velocities), releasing individual particles and photographing them as they moved down the tunnel [202]. Shlien and Corrsin measured Lagrangian velocity autocorrelations by measuring the dispersion of heat behind a heated wire [195].

Some recent results of direct measurements by particle tracking techniques and by DNS, of correlation functions of particle velocity and acceleration, and of characteristics of relative particle dispersion, were presented in particular in [165, 257, 238]. The first two are experimental, while the third uses DNS.

4.6. DISPERSION

We could not finish this survey without mentioning one of the earliest measurements of dispersion in the atmosphere, by L.F. Richardson in 1920 [184]. Richardson appears to have carefully observed the dispersion of nearly everything that comes close to floating in the atmosphere; JLL's favorite is dandelion clocks, or parachutes, as Richardson calls them. These also show vorticity very nicely.

Of course, we do not intend to neglect Richardson's famous and important paper of 1926 on relative dispersion [185].

Corrsin and his students made some of the earliest measurements (of which we are aware) of dispersion in the laboratory, from the wakes of heated wires singly and in pairs, in 1953 with Uberoi [231] and in 1956 with Kistler [91].

4.7. Complex flows

This is a term that was invented by Bradshaw, to describe turbulent flows that differ in important ways from the simple nearly-parallel canonical flows. In particular, he considered a slight divergence, a small curvature, a gentle slewing of the mean flow direction with height; what he called added rates of strain. He found that these 'small' effects result in disproportionately large changes in the flows [17]. Bradshaw and his co-workers explored these flows in several papers, e.g., [200, 201].

4.8. Clever techniques

Several interesting experimental techniques, quite out of the ordinary, have been devised to deal with difficult experimental problems, and have resulted in unique experiments. We will mention just a few.

4.8.1. *The Flying Hotwire*

This was devised to deal with experimental situations in which a stationary probe would be subjected to flow reversal. In this technique, the probe is moved at a velocity sufficient to avoid the reversal. The first version was developed by Coles and his student Cantwell at CalTech in 1973–1974, and followed a circular path [26]. This concept was extended to a linear horizontal path, and extensively exploited by Perry and his students Watmuff, Chong and Kelso at Melbourne in the early 1980s [245]. At Imperial College, Whitelaw and his student Thompson a year later used a wire following a boot-shaped path, dipping into and out of a separated flow on the upper surface of an airfoil [224]. Finally, the path was made vertical by Panchepakesan and Lumley between 1985–1990 [166].

4.8.2. *Interface without Shear*

In 1968 Mobbs did a very clever experiment in which he created a laminar two-dimensional stream in the middle of a turbulent field, both having the same uniform mean velocity [141]. In this way, he could investigate the propagation of turbulence into non-turbulent fluid, without the influence of shear. He did this by making a gap in a grid, but edging the gap with a wider strip, so that the average pressure drop of the gap plus the strips on either side was the same as that of the grid. This is somewhat analogous to the wake of a self-propelled vehicle, and in the same way any velocity shear decays quite rapidly.

4.8.3. *Active Grids*

Corrsin is probably the father of this idea. A grid of solidity greater than about 0.4 does not produce a stable flow. This limits the turbulence level that can be produced by a grid, since the turbulence level is proportional to the drag coefficient, and that is proportional to the solidity. Probably in the 1960s Corrsin suggested that a higher turbulence level could be obtained by an active grid. He suggested at first a grid with small propellors at the intersections. So far as we are aware, he never constructed such a grid. However, Mathieu, at Lyon, had a student construct such a grid for a *Travail de Fin d'Etude*, and make measurements behind it [136, 137]. The increase in turbulence level was not large enough to be worth the effort.

Later (1974), Corrsin and Gad-el-Hak experimented with a grid with jets at the intersections. Again, the increase in turbulence level was not great enough to repay the difficulty of fabrication.

The final stage of this came in 1991, when Makita [130] devised a grid with flaps that moved randomly. Each grid bar is rotated by a stepper motor according to a random protocol, and each bar carries in each mesh space a triangular flap that occupies one quarter of the grid mesh. This results in a very substantial increase in the turbulence level, and in the Reynolds number of the resulting field. This has been extensively exploited by Warhaft [150].

4.8.4. *Stratified Flow Tunnel*

It is very difficult to pump stratified fluid without destroying the stratification. The stratification is routinely destroyed and re-established in air, but in salt water (for example) it is a more serious problem. Van Atta experimented with several clever schemes before he finally hit on one that satisfied him [209].

4.8.5. *Shear-Free Boundary Layer*

Thomas and Hancock in 1977 carried out a very clever experiment in which grid turbulence flowed through a duct, one wall of which was moving at the free-stream speed [223]. In this way, a boundary layer was formed without shear, so that the influence of the no-penetration condition could be examined more-or-less in isolation.

4.9. OVERVIEW

This has been a brief, superficial survey of some very personal nominations for high points of the last 100 years in turbulence. Even from this rather disorganized approach, however, some conclusions can be dimly seen. This field does not appear to have a pyramidal structure, like the best of physics. We have very few great hypotheses, which are then tested extensively, leading to modifications, and so forth. We can really only point to the von Kármán–Prandtl–Millikan similarity laws and to Kolmogorov's hypotheses of 1941 and 1961, and perhaps the hypothesis of Townsend in 1956 regarding a double structure in turbulent flows. The first two were based on seemingly evident dimensional arguments, which later were found to not be exact because of the neglected influence of coherent structures. Most of our experiments are exploratory experiments. What does this mean?

We believe it means 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.

Turbulence desperately needs theoreticians, but their preparation is a bit daunting: to be effective, they must be familiar with 100 years of experiments, not to mention all the engineering approaches that have been tried. Each of the latter represents a kind of experiment, that can tell us something about turbulence. For example, gradient transport ideas (which have been around since the beginning) are understood to be wrong in principle, yet they are used daily with moderate success by industry. Understanding how this can be (it is thoroughly explained by Tennekes and Lumley [222]) sheds light on turbulence.

4.10. Conclusion

We may consider the likely age of the field. In a recent article [59] the work of Gott was described, in which he suggested calculating the lifetime of human institutions on a Copernican basis; that is, by assuming that the present is not an exceptional time (neither the beginning nor the end). This approach is a rather blunt instrument, which says, for example, that with probability 1.0 the field of turbulence has a future duration somewhere between zero and infinity. However, it is possible to extract some useful information, if we do not ask for too much. Thus, if our field has lasted 100 years, and we assume that we are most likely in the middle half of its life time, we can say with probability 0.5 that it will last at least another 33 years, and at most another 300 years.

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