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G.K. BATCHELOR AND THE HOMOGENIZATION OF TURBULENCE

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Key Words homogeneous turbulence, passive scalar problem, Kolmogorov theory, turbulent diffusion

■ **Abstract** This essay is based on the G.K. Batchelor Memorial Lecture that I delivered in May 2000 at the Institute for Theoretical Physics (ITP), Santa Barbara, where two parallel programs on Turbulence and Astrophysical Turbulence were in progress. It focuses on George Batchelor's major contributions to the theory of turbulence, particularly during the postwar years when the emphasis was on the statistical theory of homogeneous turbulence. In all, his contributions span the period 1946–1992 and are for the most part concerned with the Kolmogorov theory of the small scales of motion, the decay of homogeneous turbulence, turbulent diffusion of a passive scalar field, magnetohydrodynamic turbulence, rapid distortion theory, two-dimensional turbulence, and buoyancy-driven turbulence.

1. INTRODUCTION

George Batchelor (1920–2000) (see Figure 1) was undoubtedly one of the great figures of fluid dynamics of the twentieth century. His contributions to two major areas of the subject, turbulence and low-Reynolds-number microhydrodynamics, were of seminal quality and have had a lasting impact. At the same time, he exerted great influence in his multiple roles as founding Editor of the *Journal of Fluid Mechanics*, co-Founder and first Chairman of Euromech, and Head of the Department of Applied Mathematics and Theoretical Physics (DAMTP) in Cambridge from its foundation in 1959 until his retirement in 1983.

I focus in this article exclusively on his contributions to the theory of turbulence, in which he was intensively involved over the period 1945 to 1960. His research monograph, The Theory of Homogeneous Turbulence, published in 1953, appeared at a time when he was still optimistic that a complete solution to the problem of turbulence might be found. During the 1950s, he attracted an outstanding group of research students, many from his native Australia, to work with him in Cambridge on turbulence. By 1960, it had become apparent to him that insurmountable mathematical difficulties in dealing adequately with the closure problem lay ahead. As

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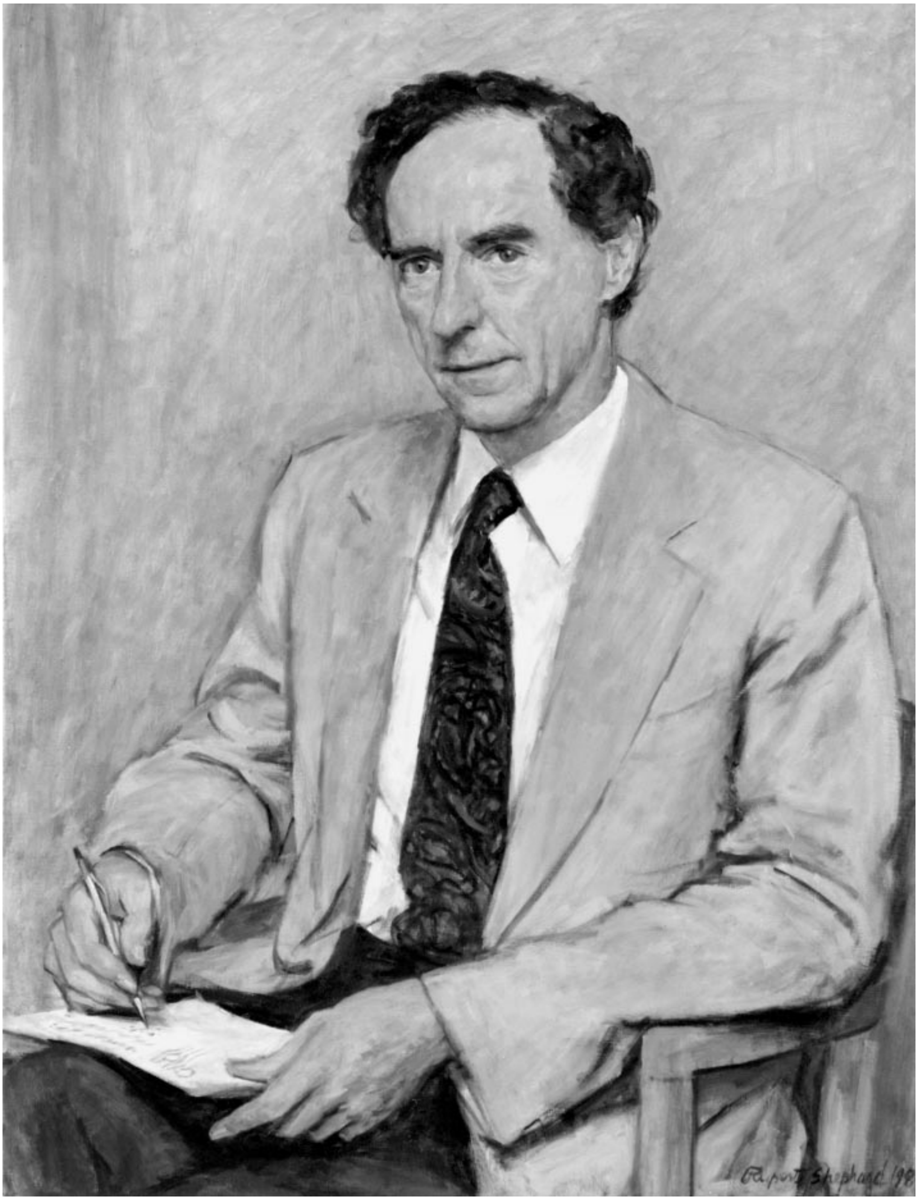

Figure 1 Portrait of George Batchelor by Rupert Shephard (1984).

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he was to say later (Batchelor 1992), "by 1960 . . . I was running short of ideas; the difficulty of making any firm deductions about turbulence was beginning to be frustrating, and I could not see any real break-through in the current publications." Over the next few years, Batchelor focused increasingly on the writing of his famous textbook, *An Introduction to Fluid Dynamics* (Batchelor 1967), and in the process was drawn toward low-Reynolds-number fluid mechanics and suspension mechanics, the subject that was to give him a new lease on research life in the decades that followed. After 1960, he wrote few papers on turbulence, but among these few are some gems (Batchelor 1969, 1980; Batchelor et al. 1992) that show the hand of a great master of the subject.

I got to know George Batchelor in 1958, when he took me on as a new research student. Batchelor had just completed his work (Batchelor 1959, Batchelor et al. 1959) on the passive scalar problem, i.e., the problem of determining the statistical properties of the distribution of a scalar field that is convected and diffused within a field of turbulence of known statistical properties. There was at that time intense interest in the rapidly developing field of magnetohydrodynamics, partly fueled by the publication in 1957 of Cowling's *Magnetohydrodynamics*. Batchelor had written a famously controversial paper, On the Spontaneous Magnetic Field in a Conducting Liquid in Turbulent Motion (Batchelor 1950a; see also Batchelor 1952b), and it was natural that I should be drawn to what is now described as the passive vector problem, i.e., determination of the statistical evolution of a weak magnetic field, again under the dual influence of convection and diffusion by a known field of turbulence. Batchelor gave me enormous encouragement and support, for which I shall always be grateful, during my early years of research in this area.

My view of Batchelor's contributions to turbulence is obviously colored by my personal interaction with him, and the following selection of what I regard as his outstanding contributions to the subject has a personal flavor. But I am influenced also by aspects of his work dating from the period 1945–1960 that still generate hot debate in the turbulence community today. Among these, for example, is the problem of intermittency, which was first identified by Batchelor & Townsend (1949) and which perhaps contributed to that sense of frustration that afflicted Batchelor (and many others) from 1960 onward.

2. MARSEILLE (1961): A Watershed for Turbulence

These frustrations came to the surface at the now legendary meeting held in Marseille (1961) to mark the opening of the former Institut de Mécanique Statistique de la Turbulence (Favre 1962). This meeting, for which Batchelor was a key organizer, turned out to be a most remarkable event. Kolmogorov was there, together with Obukhov, Yaglom, and Millionshchikov (who had first proposed the zero-fourth-cumulants closure scheme, in which so much work and hope had been invested during the 1950s); von Karman and G.I. Taylor were both there—the great father figures of prewar research in turbulence— and the place was humming with all the current stars of the subject—Stan Corrsin, John Lumley, Philip Saffman, Les Kovasznay, Bob Kraichnan, Ian Proudman, and George Batchelor himself, among many others.

One of the highlights of the Marseille meeting was when Bob Stewart presented results of the measurement of ocean spectra in the tidal channel between Vancouver Island and mainland Canada (subsequently published by Grant et al. 1962). These were the first convincing measurements to show several decades of a $k^{-5/3}$ spectrum and to provide convincing support for Kolmogorov's (1941a,b) theory, which had been published 20 years earlier. But then, Kolmogorov gave his lecture, which I recall was in the sort of French that was as incomprehensible to the French themselves as to the other participants. However, the gist was clear: He said that quite soon after the publication of his 1941 papers Landau had pointed out to him a defect in the theory, namely, that wherever the local value of ϵ is larger than the mean, there the energy cascade will proceed more vigorously, and an increasingly intermittent distribution of $\epsilon(\mathbf{x},t)$ is therefore to be expected. Arguing for a log-normal probability distribution for ϵ, a suggestion that he attributed to Obukhov, Kolmogorov showed that the exponent $(-5/3)$ should be changed slightly and that higher-order statistical quantities would be more strongly affected by this intermittency.

This must in fact have been no real surprise to Batchelor because, as indicated above, it was he and Townsend who had remarked on the phenomenon of intermittency of the distribution of vorticity in their 1949 paper, The Nature of Turbulent Motion at Large Wave-numbers. They had noticed the puzzling increase of flatness factor (or "kurtosis") of velocity derivatives with increasing Reynolds number, a behavior that is inconsistent with the original Kolmogorov theory. They interpreted this in terms of a tendency to form "isolated regions of concentrated vorticity," and it is interesting to note that much of the research on turbulence from the past two decades has been devoted to identifying such concentrated vorticity regions, both in experiments and in numerical simulations. Townsend thought in terms of a random distribution of vortex tubes and sheets (Townsend 1951b) in his theory for the dissipative structures of turbulence; a theory described in Batchelor's (1953) monograph.

I still see the 1961 Marseille meeting as a watershed for research in turbulence. The very foundations of the subject were shaken by Kolmogorov's presentation; and the new approaches, particularly Kraichnan's (1959) Direct Interaction Approximation, were of such mathematical complexity that it was really difficult to retain that essential link between mathematical description and physical understanding, which is so essential for real progress.

Given that Batchelor was already frustrated by the mathematical intractability of turbulence, it was perhaps the explicit revelation that all was not well with Kolmogorov's theory that finally led him to abandon turbulence in favor of other fields. He had invested huge effort in the elucidation and promotion of Kolmogorov's theory (see below) and regarded it as perhaps the one area of the

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subject in which reasonable confidence could be placed; to find this theory now undermined at a fundamental level by its originator, and having this occur, ironically, just as experimental confirmation of the flawed theory was becoming available, must have been deeply disconcerting. Thus, one may well understand why, over the subsequent decade, Batchelor's energies were more or less totally deflected to his textbook, to the Editorship of the *Journal of Fluid Mechanics*, which he had founded in 1957 and which was now in a phase of rapid growth, and to heading the Department of Applied Mathematics and Theoretical Physics (DAMTP) in Cambridge, which had been established largely under his visionary impetus in 1959.

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3. EARLY DAYS

But first, some early background. George Batchelor was born on 8 March 1920 in Melbourne, Australia. He attended school in Melbourne and won a scholarship to Melbourne University where he studied mathematics and physics, graduating at the age of 19 in 1939 just as World War II was breaking out. He then took up research in aerodynamics with the CSIRO Division of Aeronautics. Throughout the war, he worked on a succession of practical problems, which were not of great fundamental interest but which served to motivate him toward the study of turbulence, which he perceived not only as the most challenging aspect of fluid dynamics, but also as the most important in relation to aerodynamic applications.

In the course of this work, he read the papers of G.I. Taylor from the 1930s on the statistical theory of turbulence and resolved that this is what he wanted to pursue as soon as the war ended. He wrote to Taylor offering his services as a research student, and Taylor agreed to take him on. At the same time, and most significantly, Batchelor persuaded his fellow Australian, Alan Townsend, to join him in this voyage of discovery. I became aware in later years of Batchelor's powers of coercion. Townsend describes in a later essay (Townsend 1990) his initial encounter with Batchelor in Melbourne and how he was induced to switch from research in nuclear physics to experimental work on turbulence. In his last published paper, Research as a Life Style, Batchelor (1997) relates that, after suggesting to Townsend that they should join forces to work on turbulence under G.I. Taylor, Townsend responded that he would be glad to do so, but he first wanted to ask two questions: Who is G.I. Taylor and what is turbulence? The former question was easier to answer; the latter would provoke much philosophical debate over the following decades. In any event, Batchelor's answer must have been sufficient to convince Townsend to pursue what turned out to be an excellent career move. The partnership between George Batchelor and Alan Townsend, combining brilliance in both theory and experiment, was to endure for the next 15 years during which the foundations of modern research in turbulence were to be established.

Batchelor married Wilma Raetz, also of Melbourne, in 1944. In January 1945, they set off on an epic voyage to Cambridge, via New Zealand, Panama, Jamaica, and New York, then in a convoy of 90 ships across the Atlantic to the Tilbury docks in London, and finally to Cambridge where they were destined to spend the rest of their lives. Batchelor was then just 25 years old.

Alan Townsend came independently to Cambridge. When he and Batchelor met G.I. Taylor and talked with him about the research they would undertake, they were astonished to find that Taylor himself did not intend to work on turbulence, but rather on a range of problems—for example, the rise of large bubbles from underwater explosions or the blast wave from a point release of energy—that he had encountered through war-related research activity. Batchelor and Townsend were, therefore, left more or less free to determine their own program of research, with guidance but minimal interference from Taylor—and they rose magnificently to this challenge!

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4. BATCHELOR AND THE KOLMOGOROV THEORY OF TURBULENCE

Batchelor spent his first year in Cambridge searching the literature of turbulence in the library of the Cambridge Philosophical Society. There, he made an amazing discovery—he came upon the English-language editions of the 1941 issues of *Doklady*, the Comptes Rendus of the USSR Academy of Sciences, in which the seminal papers by Kolmogorov had been published. This was amazing because, in the face of the German advance from the West, the Academy had been displaced from Moscow to Kazan in the foothills of the Ural Mountains; because it is hard to imagine how the Academy could have continued to produce an English language edition of *Doklady* in the crisis situation then prevailing; and because it is hard to imagine how any mail, much less any consignments of scientific journals, could have found their way from the USSR to England during those dreadful years. [Barenblatt (2001) relates that bound volumes of *Doklady* and other Soviet journals were used as ballast for supply ships making the dangerous return journey from Russia through Arctic waters to the West.]

Nonetheless, Batchelor found those papers and immediately recognized their significance. In his lecture, Fifty Years with Fluid Mechanics, presented at the 11th Australasian Fluid Mechanics Conference (Batchelor 1992), he said: "Like a prospector systematically going through a load of crushed rock, I suddenly came across two short articles, each of about four pages in length, whose quality was immediately clear." Four pages was the normal limit of length imposed by the USSR Academy for papers in *Doklady*, a limit that may have suited Kolmogorov's minimalist style of presentation but at the same time made it exceptionally difficult for others to understand the implications of his work. Batchelor did understand these implications and proceeded to a full and thorough discussion of the theory in a style that was to become his hallmark: The assumptions of the theory were set out with the utmost care, each hypothesis being subjected to critical discussion in terms of both its validity and its limitations; and the consequences were then derived and illuminated with a penetrating physical interpretation at each stage of the argument.

The resulting 27-page paper was published in the *Proceedings of the Cambridge Philosophical Society* in 1947. Batchelor (1946b) had earlier announced some of his findings at the 6th International Congress of Applied Mechanics held (remarkably) in Paris in September 1946, where he also drew attention to the parallel lines of enquiry of Onsager, Heisenberg, and von Weizsäcker. There is an interesting historical aspect to this: Both Heisenberg and von Weizsäcker were taken, together with other German theoretical physicists, to Britain at the end of the War and placed under house arrest in a large country house not far from Cambridge. G.I. Taylor visited them at their request [probably in August 1945; see (Batchelor 1992)] to discuss energy transfer in turbulent flow, and it was during a subsequent discussion between Taylor and Batchelor that the link with the work of Kolmogorov was recognized. But, as Batchelor said, "The clearest formulation of the ideas was that of Kolmogorov, and it was also more precise and more general."

There can be little doubt that it was Batchelor's (1947) paper that effectively disseminated the Kolmogorov theory to the Western world. I understand that a Russian translation of it also served to make the theory comprehensible to turbulence researchers in the Soviet Union. The theory is now so well known that there is no need for me to go into details here. There is just one point, however, that does deserve mention: Batchelor draws particular attention to Kolmogorov's derivation of the four-fifths law for the third-order structure function in isotropic turbulence:

$$B_{ddd}(r) = \left\langle \left(u'_d - u_d\right)^3 \right\rangle = -\tfrac{4}{5}\epsilon r,$$

where, in Kolmogorov's notation, the suffix d indicates components of velocity parallel to the separation between points $\mathbf{x}$ and $\mathbf{x}' = \mathbf{x} + \mathbf{r}$, r is in the inertial range, and ϵ is the rate of dissipation of energy per unit mass of fluid. Batchelor's careful re-derivation of this result (which strangely was not reproduced in his book) still merits study. The result is of central importance because, as pointed out by Frisch (1995), "it is both exact and nontrivial."

Over the following years, Batchelor would be increasingly preoccupied with exploiting the new insights that the Kolmogorov theory provided in a range of problems (e.g., the problem of turbulent diffusion) for which the small-scale ingredients of the turbulence play a key role. Figure 2 shows a page from the research notebook that Batchelor kept during the late 1940s, significantly entitled, Suggestions for Exploitation of Kolmogorov's Theory of Local Isotropy. On the other side of the page appear the following suggestions: "(2) Apply theory to time-delay correlations; establish relation to space correlations; refer to diffusion analysis. (3) Apply to axisymmetric turbulence, e.g., to evaluate dissipation terms and establish tendency to isotropy."

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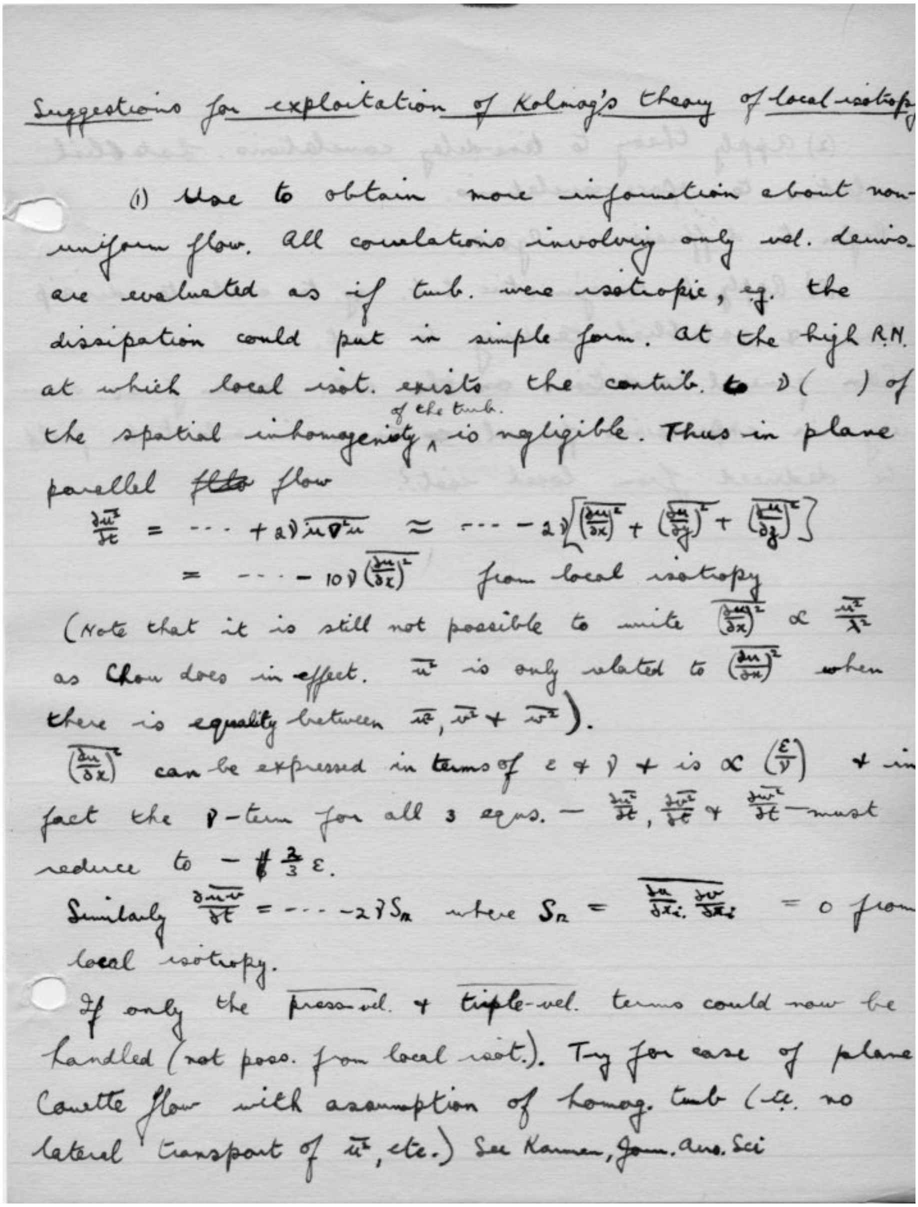

Suggestions for exploitation of Kolmog's theory of local isotropy

(1) Use to obtain more information about non-uniform flow. All correlations involving only vel. derivs. are evaluated as if turb. were isotropic, eg. the dissipation could put in simple form. At the high R.N. at which local isot. exists the contrib. to ν( ) of the spatial inhomogeniety of the turb. is negligible. Thus in plane parallel ~~flo~~ flow

$$\frac{\partial \overline{u^2}}{\partial t} = \cdots + 2\nu\overline{u\nabla^2 u} \approx \cdots - 2\nu\left[\overline{\left(\frac{\partial u}{\partial x}\right)^2} + \overline{\left(\frac{\partial u}{\partial y}\right)^2} + \overline{\left(\frac{\partial u}{\partial z}\right)^2}\right]$$

$$= \cdots - 10\nu\overline{\left(\frac{\partial u}{\partial x}\right)^2} \quad \text{from local isotropy}$$

(Note that it is still not possible to write $\overline{\left(\frac{\partial u}{\partial x}\right)^2} \propto \frac{\overline{u^2}}{\lambda^2}$ as Chou does in effect. $\overline{u^2}$ is only related to $\overline{\left(\frac{\partial u}{\partial x}\right)^2}$ when there is equality between $\overline{u^2}, \overline{v^2}$ + $\overline{w^2}$).

$\overline{\left(\frac{\partial u}{\partial x}\right)^2}$ can be expressed in terms of ε + ν + is $\propto \left(\frac{\varepsilon}{\nu}\right)$ + in fact the ν-term for all 3 eqns. — $\frac{\partial\overline{u^2}}{\partial t}, \frac{\partial\overline{v^2}}{\partial t}$ + $\frac{\partial\overline{w^2}}{\partial t}$ — must reduce to $-\frac{2}{3}\varepsilon$.

Similarly $\frac{\partial \overline{uv}}{\partial t} = \cdots - 2\nu S_n$ where $S_n = \overline{\frac{\partial u}{\partial x_i}\frac{\partial v}{\partial x_i}} = 0$ from local isotropy.

If only the press-vel. + triple-vel. terms could now be handled (not poss. from local isot.). Try for case of plane Couette flow with assumption of homog. turb (i.e. no lateral transport of $\overline{u^2}$, etc.) See Karman, Journ. Aero. Sci

Figure 2 A page from George Batchelor's research notebook; this dates from approximately 1947.

5. BATCHELOR AND MAGNETOHYDRODYNAMICS

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Batchelor's involvement in magnetohydrodynamics stemmed from the symposium, Problems of Motion of Gaseous Masses of Cosmical Dimensions, arranged jointly by the International Astronomical Union and the newly formed International Union of Theoretical and Applied Mechanics and held in Paris in August 1949. It is interesting to go back to the proceedings of that symposium (Batchelor 1951) to see what he said there. His paper starts: "It is not a very enviable task to follow Dr. von Karman on this subject of turbulence. He explained things so very clearly and he has touched on so many matters that the list of things which I had to say is now torn to shreds by the crossings out I have had to make as his talk progressed. But there is one point on what ought to be called the pure turbulence theory, which I should like to make; this point concerns the spectrum and will be useful also for Dr. von Weizsäcker, in his talk. After having made that point, I want to plunge straight into the subject that some of the speakers have lightly touched on and then hastily passed on from, namely the interaction between the magnetic field and the turbulence. That will perhaps give us something to talk about. I shall be thinking aloud so that everything may be questioned." It takes some courage to think aloud in an international gathering of this kind; and here was Batchelor, at the age of 29, thinking aloud and indeed leading the debate, in the presence of such giants as von Karman, von Neumann (who was also present), and von Weizsäcker.

It was in this setting and in his subsequent (1950a) paper that Batchelor developed the analogy between vorticity $\boldsymbol{\omega}$ in a turbulent fluid and magnetic field $\mathbf{B}$ in a highly conducting fluid in turbulent motion. The analogy is one that has to be used with great care because it is an imperfect one: Vorticity is constrained by the relation $\boldsymbol{\omega} = \text{curl}\,\mathbf{u}$ to the velocity field $\mathbf{u}$ that convects it, whereas $\mathbf{B}$ is free of any such constraint. Nevertheless, some valid results do follow from the analogy, despite their slightly dubious foundation. One in particular is the fact that in the ideal fluid limit the magnetic flux through any material circuit is conserved, similar to the flux of vorticity in an inviscid nonconducting fluid.

Batchelor applied the analogy to the action of turbulence in a highly conducting cloud of ionized gas on a weak "seed" magnetic field. He argued that, provided $\eta \lesssim \nu$ (where η is the magnetic diffusivity and ν the kinematic viscosity), stretching of the field that is most efficient at the Kolmogorov scale $l_\nu = (\nu^3/\epsilon)^{1/4}$ will intensify the field on a timescale $t_\nu = (\nu/\epsilon)^{1/2}$ until it reaches a level of equipartition of energy with the smallest-scale (dissipation-range) ingredients of the turbulence, i.e., until

$$\langle \mathbf{B}^2 \rangle / \mu_0 \rho \sim (\epsilon \nu)^{1/2}.$$

I note that this estimate still survives [see (Kulsrud 1999) who writes "there is equipartition of the small-scale magnetic energy with the kinetic energy of the smallest eddy" in conformity with Batchelor's conclusion that "a steady state is reached when the magnetic field has as much energy as is contained in the small-scale components of the turbulence"].

Whether or not this conclusion concerning the small-scale field in, say, a galactic cloud is correct, it must be admitted that the argument on which it was based is really unsound. As I have indicated, it fails to take account of the fact that a far wider range of initial conditions is available to the magnetic field **B** than to the (severely constrained) vorticity field $\boldsymbol{\omega}$. The way to handle this evolved later, quite slowly, with the work of Parker (1955), Braginskii (1964), and, most notably, Steenbeck et al. (1966) who developed the two-scale technique and the concept of mean-field electrodynamics. We now know that Batchelor's criterion $\eta < \nu$ for the growth of magnetic energy in turbulent flow is incorrect. Even when $\eta \gg \nu$, magnetic energy can grow on large scales, the critical requirement being that the turbulence must lack reflexional symmetry (Moffatt 1970). And yet, as Kulsrud (1999) has argued, debates of the type advanced 50 years ago by Batchelor are still highly relevant to consideration of the small-scale field and of saturation mechanisms.

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6. THE DECAY OF HOMOGENEOUS TURBULENCE

Much of Batchelor's early work was concerned with the idealized problem of homogeneous turbulence; that is, turbulence whose statistical properties are invariant under translations. The central problem addressed by Batchelor & Townsend (1947; 1948a,b) concerned the rate of decay of homogeneous turbulence, the kind that could be produced by flow through a grid in a wind tunnel. It was perhaps natural to focus on this problem, which is so strongly influenced by the nonlinear transfer of energy from large to small scales.

The 1947 paper, Decay of Vorticity in Isotropic Turbulence, by Batchelor & Townsend is of particular interest in this context. They note that the rate of change of mean-square vorticity in isotropic turbulence is proportional to the mean-cube of vorticity, which in turn is related to the skewness factor of the velocity derivative. Measurements by Townsend indicated that this skewness factor was approximately constant and independent of the Reynolds number during decay of the turbulence, with the consequence that the contribution from the nonlinear inertial terms of the equation of motion to the rate of change of mean-square vorticity is apparently proportional to $(\overline{\omega^2})^{3/2}$. This term taken alone leads to a blow-up of mean-square vorticity at finite time. Batchelor & Townsend pointed out that "the viscous contribution is always greater [than the nonlinear contribution] but the contributions tend to equality as the grid Reynolds number increases."

There is, of course, great current interest as to whether, for an inviscid fluid, the mean-square vorticity really can exhibit a singularity at finite time. Even to this day, this remains a central unsolved problem in the mathematics of the Euler equations. The argument of Batchelor & Townsend (1947) rests on the semi-empirical observation of constancy of the skewness factor, but anything approaching a proof is still lacking.

In some ways, it now seems that the intense preoccupation in the postwar years with the problem of homogeneous isotropic turbulence was perhaps misguided. The intention was clearly to focus on the central problem of nonlinear inertial energy transfer, but in so doing, the most difficult aspect of the problem was in some way isolated. In shear flow turbulence (as opposed to homogeneous turbulence), the interaction between the mean flow and the turbulent fluctuation (which is linear in these fluctuations) provides a valid starting point for theoretical investigation, which is simply not available for the problem of homogeneous turbulence. The intense and enduring difficulty of the problem of homogeneous turbulence is associated with the fact that all linearizable features have been stripped away, and the naked nonlinearity of the problem is all that remains.

Batchelor & Townsend nevertheless recognized (1948b) that nonlinear effects become negligible during the "final period of decay" when eddies on all scales decay through direct viscous dissipation. They determined the asymptotic decay of turbulent energy (proportional to $t^{-5/2}$) during this final period. Even this result however has been subject to subsequent controversy. It rests on the behavior of the energy spectrum at small wavenumbers (Batchelor 1949a), and the later work of Batchelor & Proudman (1956) revealed an awkward nonanalytic behavior of the spectrum tensor of homogeneous turbulence in the neighborhood of the origin, induced by the long-range influence of the pressure field. The problem was later taken up by Saffman (1967) who showed that, under plausible assumptions concerning the means by which the turbulence is generated, the energy spectrum function could have a k^2 (rather than k^4) dependence near $k = 0$, with the consequence that the energy decays in the final period as $t^{-3/2}$ instead of $t^{-5/2}$. Thus, even for this "easiest" aspect of the problem of homogeneous turbulence, the issue was far more subtle than originally realized.

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7. RAPID DISTORTION THEORY

In 1953, G.I. Taylor considered the effect of an arbitrary rapid distortion on a single Fourier component of a turbulent velocity field; the motivation was to understand the manner in which wind tunnel turbulence might be suppressed by passage through a contracting section. Batchelor & Proudman (1954) took up this problem and, by integrating over all the Fourier components of a turbulent field, determined the manner in which anisotropy is induced in an initially isotropic field of turbulence. If the distortion is sufficiently rapid relative to the timescale of the turbulent eddies, then a linear treatment is legitimate. This realization, and the fact that a linear treatment is distinctly better than no treatment at all, has led to many subsequent developments of rapid distortion theory and application in a wide range of contexts (see for example Savill 1987). The paper by Batchelor & Proudman (1954) led the way in this important branch of the theory of turbulence.

A closely related problem that may also be treated by linear techniques concerns the effect on wind tunnel turbulence of a wire gauze placed across the stream. This problem was treated by Taylor & Batchelor (1949) and is, interestingly, the only paper under this joint authorship. When the turbulence level is weak, the velocity field on the downstream side of the gauze is linearly related to that on the upstream side; and so, the spectrum tensor of the turbulence immediately downstream of the gauze can be determined in terms of that on the upstream side. The transverse components of velocity are affected differently from the longitudinal components so that turbulence that is isotropic upstream becomes nonisotropic, but axisymmetric, downstream—a behavior that was, at least qualitatively, confirmed by Townsend (1951a). Batchelor (1946a) had previously developed a range of techniques appropriate to the description of axisymmetric turbulence, techniques that now found useful application.

An account of both of these theories may be found in Chapter 4 of *The Theory of Homogeneous Turbulence* (Batchelor 1953). Having referred several times to this book, I now quote the following statement that Batchelor made in the preface, which is revealing as regards his general philosophy: "Finally, it may be worthwhile to say a word about the attitude that I have adopted to the problem of turbulent motion, since workers in the field range over the whole spectrum from the purest of pure mathematicians to the most cautious of experimenters. It is my belief that applied mathematics, or theoretical physics, is a science in its own right, and is neither a watered-down version of pure mathematics nor a prim form of physics. The problem of turbulence falls within the province of this subject, since it is capable of being formulated precisely. The manner of presentation of the material in this book has been chosen, not with an eye to the needs of mathematicians or physicists or any other class of people, but according to what is best suited, in my opinion, to the task of *understanding the phenomenon*. Where mathematical analysis contributes to that end, I have used it as fully as I have been able, and equally I have not hesitated to talk in descriptive physical terms where mathematics seems to hinder the understanding. Such a plan will not suit everybody's taste, but it is consistent with my view of the nature of the subject matter."

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8. TURBULENT DIFFUSION

Batchelor first addressed the problem of turbulent diffusion in a paper (1949b) published by the *Australian Journal of Scientific Research*, under the title, Diffusion in a Field of Homogeneous Turbulence. This paper was clearly inspired by the seminal paper of Taylor (1921) in which the dispersion of a particle in a turbulent flow relative to a fixed point had been first considered. Batchelor extended this treatment to three-dimensions and, more importantly, showed that the mean concentration for a finite volume of marked fluid satisfied a diffusion equation with a time-dependent diffusion tensor, this being a generalization of Taylor's diffusion coefficient. In a subsequent series of papers (Batchelor 1950b, 1952a; Batchelor & Townsend 1956), Batchelor considered the relative diffusion of two particles and established a theoretical link with Richardson's law of diffusion whereby the rate of increase of mean-square separation is proportional to the two-thirds power of the mean-square separation. With hindsight provided by Kolmogorov's theory, this result can be obtained on dimensional grounds when the particle separation is in the inertial range.

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I have already referred to Batchelor's famous (1959) paper, Small-scale Variation of Convected Quantities like Temperature in Turbulent Fluid. In this paper, he recognizes the critical importance of the Prandtl number ν/κ, where κ is the molecular diffusivity of the convected scalar field. He argued that, when ν/κ is large, scalar fluctuations persist on scales that are small compared with the Kolmogorov scale, in fact down to the "Batchelor scale" $(\epsilon/\nu\kappa^2)^{1/4}$. On scales between the Kolmogorov scale and the Batchelor scale, the velocity gradient is approximately uniform; and on this basis, Batchelor was able to determine the spectrum of the fluctuations of the scalar field in the corresponding range of wavenumbers k. He found this to be proportional to k^{-1}.

In the companion paper (Batchelor et al. 1959), the small Prandtl number situation was considered. In this case, the "conduction cut-off" occurs at wavenumber $(\epsilon/\kappa^3)^{1/4}$ [as previously determined by (Obukhov 1949)], and the scalar spectrum was determined in the range of wavenumbers between the conduction cut-off and the viscous cut-off (the $k^{-17/3}$-law). These two papers have provided the starting point for almost all subsequent treatments of the passive scalar problem—a problem that has attracted renewed attention, with respect to its intermittency characteristics, in recent years.

Before leaving the topic of turbulent diffusion I must mention the interesting paper by Batchelor et al. (1955) in which it was shown that the mean velocity of a fluid particle in turbulent pipe flow is equal to the conventional mean velocity (averaged over the cross-section). This result was tested experimentally using small neutrally buoyant spheres injected into a pipe flow. This was perhaps the first Lagrangian measurement in turbulent flow. The paper is closely related in spirit to Taylor's famous (1954) paper that describes the axial diffusion of a scalar in a pipe flow (laminar or turbulent).

9. THE LATER PAPERS ON TURBULENCE

Computation of the Energy Spectrum in Homogeneous Two-dimensional Turbulence (Batchelor 1969) is a highly significant paper, in that it presents one of the first (perhaps the first) numerical simulations of turbulent flow, albeit restricted to two dimensions. These computations (by R.W. Bray, a research student under Batchelor's supervision from 1961 to 1964) were undoubtedly motivated by discussions at the 1961 Marseille meeting (see above). Kraichnan (1967) had earlier responded to the same challenge. Batchelor wrote as follows: "There appears to be an impression in the literature of fluid dynamics that the known differences between motion in two and three dimensions are so great that two-dimensional turbulence (which cannot normally be reproduced experimentally in our three-dimensional world owing to instability) bears no relation to three-dimensional turbulence. It is indeed true that particular properties of a turbulent motion depend strongly on the number of space dimensions; but the two basic properties of randomness and non-linearity are present in two-dimensional turbulence, and these should ensure the applicability of concepts such as a cascade transfer process and statistical decoupling accompanying transfer between Fourier components. My own view is that there are enough common general properties of two- and three-dimensional turbulence to justify using a numerical investigation of two-dimensional turbulence as a means of testing the soundness of some of the plausible hypotheses about turbulent flow.” Such justification for a study of two-dimensional turbulence would hardly be needed now. In this paper, Batchelor describes the enstrophy cascade and the corresponding counter-flow of energy toward small wavenumbers. The numerical computation was necessarily primitive, but the remarkable thing is that such a computation was carried out at all in these early days.

During the next two decades, Batchelor’s main research preoccupation was in suspension mechanics, or microhydrodynamics as he named the subject. This phase of Batchelor’s career deserves a complete article in its own right, and I will not attempt to describe his contributions in this area, except to say that he and the new research team that he gathered virtually created a new field of study during this period. Batchelor did return to turbulence twice during this period, on each occasion with a highly original contribution. The first of these was his 1980 paper, Mass Transfer from Small Particles Suspended in Turbulent Fluid. Here, “small” means small in comparison with the inner Kolmogorov scale, so that again (as with his 1959 paper) he was able to represent the far field as a uniform gradient velocity field. Batchelor assumed that the Péclet number was large, and he solved the advection-diffusion equation in the concentration boundary layer around the particle, in order to calculate the net rate of mass transfer from the particle. He identified two contributions to this mass transfer: The first related to the translational motion of the particle relative to the fluid, and the second related to the local velocity gradient. Batchelor argued that the first of these, for reasons associated with reflectional symmetry, is zero “in common turbulent flow fields,” and he determined the second contribution in terms of a Nusselt number N; the result is $N = 0.55(a^2\epsilon/\kappa\nu^{1/2})^{1/3}$, a being the radius of the particle (assumed spherical). This is in very reasonable agreement with experiments. Batchelor’s great skill in exploiting what is known of the small-scale features of turbulent flow is again evident in this paper.

Actually, his inference that the contribution to mass transport associated with “particle slip” is zero for flows in which the mean helicity is nonzero may now require further investigation. Batchelor showed that the component of particle velocity relative to the fluid in the direction of the local vorticity is what is most relevant to this first contribution to mass transfer. As far as I know, the mean value of this quantity has not been calculated, but there is no reason to believe that it should be zero in turbulence that lacks reflectional symmetry. It would be of interest to evaluate this contribution and to compare it with the strain contribution evaluated by Batchelor.

Batchelor’s final return to turbulence (in collaboration with V.M. Canuto and J.R. Chasnov) resulted in his 1992 paper, Homogeneous Buoyancy-generated Turbulence. Still, as ever, Batchelor felt most at home in a statistically homogeneous situation. Here, a novel situation was conceived, namely a field of turbulence

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generated from rest by an initially prescribed statistically homogeneous random density distribution giving a random buoyancy force. The driven turbulence immediately modifies the distribution of buoyancy forces, and an interesting nonlinear interaction between velocity and buoyancy fields develops. As the authors state in their abstract, "the analytical and numerical results together give a comprehensive description of the birth, life and lingering death of buoyancy-generated turbulence."

10. CONCLUSION

Despite the intensive efforts of mathematicians, physicists, and engineers over the past half century and more, turbulence remains, to paraphrase Einstein, "the most challenging unsolved problem of classical physics." The 1950s was a period when, under the inspiration of George Batchelor, there was a definite sense of progress toward a cracking of this problem. But he, like many who followed him, found that the problem of turbulence was challenging to the point of total intractability. Nevertheless, Batchelor established new standards, both in the rigor of the mathematical argument and the depth of physical reasoning that he brought to bear on the various aspects of the problem that he addressed. His 1953 monograph is still widely quoted as the definitive introduction to the theory of homogeneous turbulence, and his later contributions, particularly in the field of turbulent diffusion, have stood the test of time over the subsequent 40 years. There can be no doubt that any future treatise on the subject of turbulence will acknowledge Batchelor's major contributions to the field, coupled with those of his great mentor G.I. Taylor. It must be admitted nevertheless that the great appeal of the problem of turbulence has survived into the twenty-first century and that its challenge will still persist for another few years yet.

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