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George Batchelor: the post-war renaissance of research in turbulence

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8.1 Introduction

George Batchelor (1920–2000), whose portrait (1984) by the artist Rupert Shephard is shown in [Figure 8.1](#), 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 founder 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 chapter 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 this period, he attracted an outstanding group of research students and post-docs, many from his native Australia, and Senior Visitors from all over the world, to work with him in Cambridge on turbulence. By 1960, however, it had become apparent to him that insurmountable mathematical difficulties in dealing adequately with the closure problem lay ahead. As 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 towards low-Reynolds-number fluid mechanics and

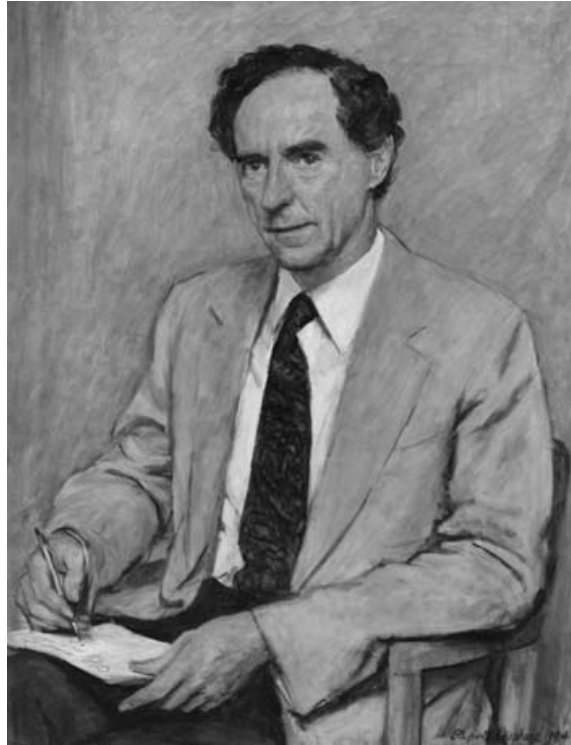


Figure 8.1 Portrait of George Batchelor by Rupert Shephard 1984; this portrait hangs in DAMTP, Cambridge, the Department founded under Batchelor's leadership in 1959.

suspension mechanics, the subject that was to give him a new lease of 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, Canuto & Chasnov 1992) that show the hand of a great master of the subject.

I got to know George Batchelor myself from 1958, when he took me on as a new research student. George had just completed his work to be published in two papers the following year (Batchelor 1959; Batchelor, Howells & Townsend 1959) on the 'passive scalar problem', i.e. the problem of determining the statistical properties of the distribution of a scalar field which 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 fuelled by the publication in 1957 of Cowling's Interscience Tract *Magnetohydrodynamics*. Batchelor had himself 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. George gave me enormous encouragement and support during my early years of research in this area, for which I shall always be grateful.

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

8.2 Marseille (1961): a watershed for turbulence

These frustrations came to the surface at the now legendary meeting held in Marseille in September 1961 on the occasion of the opening of the former Institut de Mécanique Statistique de la Turbulence (Favre 1962). This meeting, of which Batchelor was a key organizer, turned out to be a most remarkable event. Kolmogorov himself 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 Kármán and G.I. Taylor were both there – the great father-figures of pre-war 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, John Laufer, Hans Liepmann, Ian Proudman, Anatol Roshko, 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 (not mentioned in the Proceedings, but published soon after by Grant, Stewart & Moilliet 1962). These were the first convincing measurements showing several decades of a $k^{-5/3}$ spectrum, and providing 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 ‘Russian’ 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 wheresoever the local rate of dissipation of energy 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 as the Kolmogorov scale $(\nu^3/\epsilon)^{1/4}$ is approached, in their 1949 paper. They had noticed the puzzling increase of flatness factor (or ‘kurtosis’) of velocity derivatives with increasing Reynolds number $Re = \langle \mathbf{u}^2 \rangle^{1/2} L/\nu$ in standard notation, a behaviour that is inconsistent with the original Kolmogorov theory (for on that theory, the flatness factor and similar dimensionless characteristics of the small-scale features of the turbulence, should be determined solely by ϵ and kinematic viscosity ν , and so necessarily universal constants, independent of Reynolds number). 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 of the last two decades has been devoted to identifying such concentrated vorticity regions, both in experiments and in numerical simulations. Townsend himself 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 that is described in Batchelor’s (1953) monograph.

I regard 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 link between mathematical analysis and physical understanding that 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 favour of other fields. He had invested huge effort himself in the elucidation and promotion of Kolmogorov’s theory (see below) and regarded it as perhaps the one area of the subject on which reasonable confidence could be placed; to find this theory now undermined at a fundamental level by its originator, and that, ironically, just as experimental ‘confirmation’ of the flawed theory was becoming available, must have been deeply disconcerting, and one may well understand how it was that over the subsequent decade, Batchelor’s energies were more or less totally deflected not only to his textbook, but also to the Editorship of the *Journal of Fluid Mechanics*, which he had founded in 1956 and which was now

in a phase of rapid growth, and equally to the Headship of the Department of Applied Mathematics and Theoretical Physics (DAMTP) in Cambridge, which had been established largely under his visionary impetus in 1959.

8.3 Personal background

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 broke out. He was guided to take up research in Aerodynamics with the CSIR 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 towards 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, particularly those from the 1930s on the statistical theory of turbulence, and resolved that this was what he wanted to pursue as soon as the war ended. He wrote to G.I. offering his services as a research student, and G.I. agreed to take him on. At the same time, and most significantly, George persuaded his fellow-Australian Alan Townsend to join him in this voyage of discovery. I became aware in later years of George's powers of persuasion, of which this was perhaps a first manifestation. Alan described in a later essay (Townsend 1990) his initial encounter with George in Melbourne, and how it was that 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 on his suggesting to Townsend that they should join forces in working 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 obviously the easier to answer; the latter has provoked much philosophical debate over the years! [Batchelor (1953) himself described turbulent flow in simple terms as flow in which the velocity takes random values but whose average properties are uniquely determined by the controllable data (e.g. boundary conditions) of the flow.] In any event, George's answer must have been sufficient to convince Alan on what turned out to be an excellent career move; the partnership between George Batchelor and Alan Townsend, combining brilliant intuition 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.

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

Alan Townsend came independently to Cambridge, and when he and George met G.I. Taylor and talked with him about the research that they would undertake, they were astonished to find that G.I. himself did not intend to resume work on turbulence, but rather to concentrate 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. George and Alan were therefore left more-or-less free to determine their own programme of research, with guidance but minimal interference from G.I. – and they rose magnificently to this challenge!

8.4 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 of Kolmogorov had been published; amazing, because the Academy had been displaced from Moscow to Kazan in the foothills of the Ural Mountains, in the face of the German advance from the west; amazing, 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 even more amazing because it is hard to imagine how any mail, far less 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!]

But the fact is that Batchelor did indeed find these papers and immediately recognised their significance. In his lecture “Fifty Years with Fluid Mechanics” 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 perhaps suited Kolmogorov's minimalist style of presentation, but at the same time made it exceptionally difficult for others to recognize the significance of his work. Batchelor did recognize this significance, 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 both as to 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 VIth International Congress of Applied Mechanics, which was held in Paris in September 1946, provided an early opportunity for Batchelor to announce his findings to a wide international audience. (This sequence of quadrennial Congresses had been established in the 1920s through the forceful initiative of von Kármán, Prandtl, G.I. Taylor and Jan Burgers, but had been interrupted by the war. It was a remarkable achievement to reinstate the sequence so soon after the war, in the chaotic and straitened conditions that must still have prevailed in Paris at that time. Paul Germain told me with a degree of chagrin that the Proceedings of the Congress were duly delivered to Gautiers-Villars following the Congress, but have never yet appeared, perhaps a record in publication delay!)

Batchelor wisely published his contribution to the Congress in a brief communication to *Nature* (Batchelor 1946b). In this, he described the Kolmogorov theory and he simultaneously drew attention to 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 physicists, to Britain at the end of the war, and placed under house arrest in a large country house not far from Cambridge. At their own request, they were allowed to visit G.I. Taylor, no doubt under surveillance (this was probably in August 1945 – see Batchelor 1992) to discuss energy transfer in turbulent flow, and it was in 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".

Kolmogorov himself gave due credit to the prior work of Lewis Fry Richardson, who had conceived the 'energy cascade' mechanism immortalized in a rhyme reproduced elsewhere in this volume. Kolmogorov's signal achievement, on which Batchelor rightly focused, was to identify the mean rate of dissipation of energy per unit mass ϵ and the kinematic viscosity of the fluid ν

as the sole dimensional quantities on which all small-scale statistical properties of the turbulence should depend.

Batchelor's definitive paper on the subject appeared in 1947 in the *Proceedings of the Cambridge Philosophical Society*. There can be little doubt that it was this paper that effectively disseminated the Kolmogorov theory to the Western world. Barenblatt has claimed that in Russian translation it also served to make the theory comprehensible to turbulence researchers in the Soviet Union! The theory is described in [Chapter 6](#) of this volume, and I need not labour the details here. There is however one point 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:

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

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 he chose not to reproduce in his 1953 monograph) still merits study. The result is of central importance because, as pointed out by Frisch (1995), "it is both exact and non-trivial"; indeed it is perhaps the only exact nontrivial result in the whole of the dynamic (as opposed to merely kinematic) theory of turbulence.

Over the following years, Batchelor was to 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. Batchelor kept a research notebook during the late 1940s, in which is found a page significantly entitled "Suggestions for exploitation of Kolmog's [sic] theory of local isotropy"; on the other side of the page appear the following memoranda: "Apply theory to time-delay correlations; establish relation to space correlations; refer to diffusion analysis. . . . Apply to axisymmetric turbulence, e.g. to evaluate dissipation terms and establish tendency to isotropy". These were aspects of turbulence to which Batchelor devoted himself over the subsequent years.

Largely on the basis of his work on the elucidation of Kolmogorov's theory, Batchelor was elected in 1947 to a Fellowship at Trinity College, Cambridge, a position that enabled him to devote himself entirely to research over the next four years, during which he published some 15 papers (some with Townsend) on all aspects of turbulent flow. He took his PhD degree in 1948 (see [Figure 8.2](#)), and was by October 1949 installed as a lecturer in the Faculty of Mathematics, in succession to Leslie Howarth (of von Kármán–Howarth fame) who



Figure 8.2 George Batchelor with his mentor Sir Geoffrey (G.I.) Taylor, PhD graduation day, 1948.

had left Cambridge to take the Chair of Applied Mathematics at the University of Bristol.

Batchelor's PhD Examiners were in fact Leslie Howarth and G.I. Taylor himself (it being still accepted in those days that a Research Supervisor could also act as Examiner – defending counsel one day, prosecuting the next!). Batchelor recounted to me many years later that Howarth had asked him during the oral examination why he had dropped the terms that lack mirror symmetry in his discussion of the form of the spectrum tensor for isotropic turbulence, and what these terms might represent if they were retained. These were two questions to which Batchelor, on his own admission, was unable to give a satisfactory answer! They are in fact the terms that encapsulate the 'helicity' of turbulence, a concept that was to emerge two decades later and find important application in two contexts: the Euler equations (for which helicity is a topological invariant); and turbulent dynamo theory (see below).

The VIIth International Congress of Applied Mechanics was held at Imperial College, London, in September 1948; it was at this Congress that the International Union of Theoretical and Applied Mechanics (IUTAM) was formally established. In the group photograph of some 200 participants, which may be viewed on the IUTAM website, one may detect G.I. Taylor and Theodore von Kármán in the front row, and George Batchelor and Alan Townsend perched together on the plinth of a statue in the back row. I also recognize the unmistakable features of James Lighthill, Keith Stewartson and Michael Glauert, and some others, in this photograph. The Congresses of Paris (1946), London

(1948), Istanbul (1952), Brussels (1956), Stresa (1960) and Munich (1964) provided the opportunity for international contact and exchange of ideas during a period when international meetings in theoretical and applied mechanics were far fewer than they are today; Batchelor attended all of them, and encouraged his collaborators and students to do likewise. (I attended the Stresa Congress myself as a Research Student, and found it wonderfully stimulating and broadening; I have attended every ICTAM ever since, with just one exception; these Congresses are like family gatherings, where announcements of new results are keenly anticipated. G.I. Taylor used to say that he always saved his best results for these great international Congresses, and recommended that others should follow his example!)

8.5 Batchelor and the turbulent dynamo

Batchelor's involvement in magnetohydrodynamics stemmed from a symposium on '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 Kármán 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 Kármán, von Neumann who was there also, and von Weizsäcker! It was in this setting and in his subsequent (1950a) paper that Batchelor developed the analogy between vorticity ω 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 $\omega = \nabla \times \mathbf{u}$ 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 insecure foundation: in particular, the fact that in the ideal fluid limit, the magnetic flux through any material circuit is conserved, like the flux of vorticity in an inviscid non-conducting 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, which is most efficient at the Kolmogorov scale $l_\nu = (\nu^3/\epsilon)^{1/4}$, will intensify it on a time-scale $t_\nu = (\nu/\epsilon)^{1/2}$ 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 attracts credence (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”). This energy level is smaller, it should be noted, by a factor $Re^{-1/2}$, than the overall mean kinetic energy $\langle \mathbf{u}^2 \rangle / 2$ of the turbulence.

There were however two reasons, themselves in mutual contradiction, to doubt Batchelor’s conclusions. On the one hand, intensification of the magnetic field through the stretching mechanism may be expected to occur (albeit relatively slowly) on length-scales much larger than the Kolmogorov scale, and it is therefore arguable (as urged almost simultaneously by Schlüter & Biermann 1950) that the range of equipartition should extend ultimately to the full spectral range of the turbulence. On the other hand, intensification by stretching is naturally associated with decrease of scale in directions transverse to the field and hence with accelerated joule dissipation, and it was argued by Saffman (1964) that this effect would in all circumstances lead to ultimate decay of magnetic energy – as had been previously proved to be the case if all fields are assumed to be two-dimensional (Zeldovich 1957).

Thus, by 1965, all bets were open, and nothing was certain: in the presence of homogeneous isotropic turbulence, an initially random magnetic field with zero mean might grow to equipartition from an infinitesimal level, or might grow under some subsidiary conditions to some significantly lower level, or might grow for a while and then decay to zero. Into the gloom of this unresolved controversy, there penetrated a shaft of light from beyond the Iron Curtain, in the work of Steenbeck, Krause & Rädler published in 1966 in the

East German journal *Astronomie Nachrichten*. Instead of focusing on turbulent distortion of a magnetic field on small scales, these authors considered the possibility that the field might grow on scales *large* compared with the scale of the turbulence (the ‘freedom’ overlooked by Batchelor and others), and they used a two-scale analysis to investigate this possibility, thus giving birth to the subject of ‘mean-field electrodynamics’. This seems so utterly natural now that one can only wonder why no one had thought of it before; perhaps it is always so with a breakthrough, which this undoubtedly was! What emerged was that provided the turbulence has the property of ‘helicity’ (‘schraubensinn’ or literally ‘screw-sense’ in the papers of Steenbeck, Krause & Rädler), then the magnetic field will always grow on a sufficiently large length-scale, this scale being determined by the ‘magnetic Reynolds number’ of the turbulence, which may be arbitrarily *small* (Moffatt 1970). So as it turned out, not only Batchelor, but also Schlüter & Biermann, and Saffman, were all wrong in different ways. The turbulent dynamo problem would never look quite the same again! Nevertheless, as Kulsrud (1999) has argued, arguments of the kind advanced 60 years ago by Batchelor are still relevant to consideration of the small-scale field and of saturation mechanisms when the magnetic Reynolds number is very large, as for example in the interstellar or intergalactic medium.

8.6 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, of 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 process that had previously been investigated by the Soviet scientists Loitsyansky, Millionschikov and Kolmogorov (see [Chapter 6](#)).

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 of Townsend indicated that this skewness factor was approximately constant and independent of 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 $\langle \omega^2 \rangle^{3/2}$. This term taken alone would lead to a blow-up of mean-square vorticity within a finite time. Batchelor & Townsend however stated that “the viscous contribution is always the greater [in comparison with the nonlinear contribution] but the contributions tend to equality as the grid Reynolds number increases”. (The statement that the “viscous contribution is always the greater” clearly does not apply to the initial development of a field of turbulence concentrated at low wave-numbers in wave-number space; for such a field, the nonlinear contribution dominates and leads to initial rapid growth of mean-square vorticity, just as in the model problem treated by Taylor & Green 1937, now familiarly known as the ‘Taylor–Green vortex’.)

There is still to this day great interest in the question as to whether, for an inviscid fluid, the mean-square vorticity (or indeed the pointwise distribution of vorticity) really can exhibit a singularity at finite time. Indeed, this remains a central unsolved problem in the mathematics of the Euler equations; it was debated at length at the meeting celebrating the 250th anniversary of the Euler equations, held in Aussois in the Haute Savoie, in 2008 (Eyink et al. 2008). The argument of Batchelor & Townsend (1947) rests on the semi-empirical observation of apparent approach to constancy of the skewness factor with increasing Reynolds number, but anything approaching a proof of this sort of result is still lacking.

In some ways, it seems now that the intense preoccupation in the post-war years with the problem of homogeneous isotropic turbulence with zero mean velocity was perhaps misguided. The intention was clearly to focus on the central problem of nonlinear inertial energy transfer, but in so doing the most intractable aspect of the problem was addressed, with no possibility (when $Re \gg 1$) of anything like a ‘perturbative’ approach. In shear flow turbulence (as opposed to homogeneous turbulence with zero mean) the (linear) interaction between the mean flow and the turbulent fluctuations provides a valid starting point for theoretical investigation, which is simply not available for the problem of homogeneous turbulence with zero mean. The intense and enduring difficulty of the latter problem 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. Batchelor’s result depends on the assumed behaviour of the energy spectrum at small wavenumbers (Batchelor 1949a), and the later work of Batchelor &

Proudman (1956) revealed an awkward non-analytic behaviour of the spectrum tensor of homogeneous turbulence in the neighbourhood of the origin in wave-number space, induced by the long-range influence of the pressure field. The problem was taken up again 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 $t^{-5/2}$. The form of the spectrum tensor near $\mathbf{k} = 0$ and its consequences for the final period of decay has been the subject of extended debate that continues to this day (Ishida, Davidson & Kaneda 2006). (See [Chapter 12](#) for further discussion of this issue.)

Thus, even for this ‘easiest’ aspect of the problem of homogeneous turbulence, the situation turned out to be far more subtle than originally realized.

8.7 Batchelor’s 1953 monograph, *The Theory of Homogeneous Turbulence*

Batchelor’s slim CUP monograph *The Theory of Homogeneous Turbulence* was published in 1953, and consisted largely of an account of his own work, much of it in collaboration with Townsend, prior to that date. It was the first book wholly and exclusively devoted to the subject of turbulence, and as such gave great impetus to research in this field. It was much later republished in the CUP “Cambridge Science Classics” series, a mark of its enduring value.

Batchelor sets out his general philosophy in the preface as follows:

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.



Figure 8.3 The Fluid Dynamics group at the Cavendish laboratory, 1954; front row: Tom Ellison, Alan Townsend, G.I. Taylor, George Batchelor, Fritz Ursell, Milton Van Dyke. Philip Saffman is in the middle of the back row, Stewart Turner in the top right-hand corner, and Bruce Morton and Owen Phillips third and fifth from the left in the middle row.

Reading between the lines of this quotation, it is apparent that Batchelor already had serious reservations concerning the insight that ‘pure mathematics’, i.e. mathematics based on rigorous proof of precisely stated theorems, might be able to contribute to the problem of turbulence. Although he was by 1953 playing a central role in the Faculty of Mathematics at Cambridge, his wartime background in Aerodynamics still governed his real-world approach to scientific problems; in the spirit of his mentor G.I. Taylor, he would always give preference to an illuminating physical argument over a piece of abstract mathematical analysis; to such analysis, he was even on occasion quite hostile.

Batchelor himself provides a ‘brief history of the subject’ in his introductory chapter. In this, he places the origin of the study of homogeneous turbulence in the papers of G.I. Taylor (1935, 1938) in which “the fact that the velocity of the fluid in turbulent motion is a random continuous function of position and time” was first clearly recognized and developed. It was these papers that had particularly attracted Batchelor to the study of turbulence in the first place. Batchelor’s immense admiration, if not awe, for the achievements of G.I. Taylor shines through his later writings on the subject, and particularly so in his biography *The Life and Legacy of G.I. Taylor*, published by CUP in 1996.

Figure 8.3 shows the Fluid Dynamics group of the Cambridge Cavendish Laboratory in 1954. Prominent in the front row are Townsend, Taylor and Batchelor; also Fritz Ursell, working on surface water waves, who later moved



Figure 8.4 George Batchelor, having been recently elected FRS, in his office at the old Cavendish Laboratory, October 1956.

to the Beyer Chair of Applied Mathematics at Manchester University; and Milton Van Dyke, working on the theory of hypersonic flow, who was that year visiting the Cavendish from NASA Ames. The Cavendish Laboratory was still at that time located in Free School Lane in the centre of Cambridge. G.I. Taylor was based there above the ‘Balfour Room’ which housed the wind tunnel used by Townsend for his experimental work. Batchelor was already planning the *Journal of Fluid Mechanics* (JFM) which was to be launched in 1956; [Figure 8.4](#) shows him in his office at the Cavendish shortly after this launch, and shortly after his election as a Fellow of the Royal Society.

[Figure 8.5](#) shows him in a group at the IXth Congress of Applied Mechanics in Brussels (1956), an important opportunity for disseminating information about the new journal. James Lighthill, whom Batchelor had engaged as one of the first Associate Editors of JFM, is on the right of this photo, one of few photos in which these two great figures of 20th-century fluid mechanics can be seen together. While each recognized the great talents of the other, they were poles apart in personality and style; their ‘sparring’ partnership through JFM was nevertheless amicable and endured for more than 20 years. Lighthill held the Lucasian Chair of Mathematics in DAMTP, Cambridge, from 1969



Figure 8.5 George Batchelor (second from left) at the IXth International Congress of Applied Mechanics in Brussels, 1956; James Lighthill on the right.

to 1978, but was content throughout that period to leave the running of the department to Batchelor, who was Head of the Department from its foundation in 1959 until his retirement in 1983.

8.8 Rapid distortion theory

G.I. Taylor had in 1935 considered the effect of an irrotational rapid distortion on a single Fourier component of a turbulent velocity field, the motivation being 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

Sagaut & Cambon 2008). The paper of Batchelor & Proudman led the way in this important branch of the theory of turbulence.

A closely related problem which 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), interestingly the only paper under their 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 non-isotropic, but axisymmetric, downstream, a behaviour 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 here found useful application.

8.9 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 the hindsight of Kolmogorov’s theory, this result can of course be obtained on dimensional grounds, when the particle separation is in the inertial range.

I have already referred to Batchelor’s famous (1959) paper “Small-scale variation of convected quantities like temperature in turbulent fluid”. It was in this paper that he recognized 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 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 wave-numbers k ; he found this to be proportional to k^{-1} . It is worth remarking that the same technique applied to the corresponding passive *vector* problem leads to exponential growth of the energy of the vector field (Moffatt & Saffman 1964), reflecting in some degree the type of dynamo action that Batchelor had predicted in 1950.

In the companion paper (Batchelor, Howells & Townsend 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 found 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, coupled with those of Corrsin (1951) and Obukhov (1949), have provided the starting point for almost all subsequent treatments of the passive scalar problem, a problem which has attracted renewed attention, with respect to its intermittency characteristics, in recent years – see [Chapter 10](#) concerning Kraichnan’s contributions to this problem. It is worth noting that, although Kraichnan’s model involved a ‘delta-correlated’ velocity field, i.e. one varying infinitely rapidly in time, as opposed to Batchelor’s quasi-steady model, he still found a k^{-1} range when $\nu/\kappa \gg 1$, implying a certain robustness of this result.

I should not leave the topic of turbulent diffusion without mention of the interesting paper of Batchelor, Binnie & Phillips (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 treating the axial diffusion of a scalar in a pipe flow (laminar or turbulent).

8.10 Two-dimensional turbulence

Batchelor anticipated the potential interest of the two-dimensional problem in the final two pages of his 1953 monograph. This situation is very different from the three-dimensional case, because for two-dimensional turbulence, vortex stretching does not occur, and so the enstrophy (mean-square vorticity) as well as the energy are inviscid invariants of the flow. Batchelor recognized an important consequence of this additional constraint, namely that the net effect

of inertial spreading of energy in wave-number space must be to transfer energy towards progressively *larger* scales – what later became known as an ‘inverse cascade’. He further recognized that “from the original motion there will gradually emerge a few strong isolated vortices” and that “vortices of the same sign will continue to tend to group together”, conclusions that have been amply confirmed by numerical simulations more than 30 years later. As Batchelor pointed out, this conclusion had been earlier anticipated by Onsager (1949) on the basis of statistical mechanics.

The issue of two-dimensional turbulence came up in the discussions at the 1961 Marseille Conference, and I believe that these discussions led both Batchelor and Kraichnan to investigate the situation further. Batchelor saw the possibility of numerical simulation, and put his research student Roger Bray to work on the problem; his PhD thesis “A study of turbulence and convection using Fourier and numerical analysis” was submitted and approved in 1966. Bray’s computations were necessarily primitive, but the remarkable thing is that they were carried out at all in those early days when computing power was so low. On the basis of Bray’s numerical results, and stimulated by Kraichnan’s (1967) important paper on the subject (see [Chapter 10](#)), Batchelor finally returned to the problem with his 1969 paper, where he writes 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 nonlinearity 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! An understanding of the phenomenon is essential in the context of atmospheric dynamics, where strong stable stratification tends to confine the velocity field to horizontal planes. Similarly coriolis forces in a rotating fluid, or Lorentz forces in a conducting fluid permeated by a strong uniform magnetic field can constrain turbulence to have a nearly two-dimensional structure.

Batchelor focused in this paper on the fact that in two-dimensional turbulence, it is enstrophy (rather than energy) that cascades towards the high wave-numbers at which dissipation by viscosity takes over. The rate of dissipation

of enstrophy η is now the important dimensional parameter in the inertial (cascade) regime, and dimensional analysis is then sufficient to determine an energy spectrum, $E(k) \sim k^{-3}$, a conclusion that may be subject, as pointed out by Kraichnan, to logarithmic correction.

It must be admitted that Batchelor's conclusion here cannot be universally valid (for all initial fields of two-dimensional turbulence). Saffman (1971) pointed out that, if the vorticity field $\omega(\mathbf{x}, \mathbf{t})$ consists of a random distribution of vortex patches, the vorticity being uniform inside each patch and zero elsewhere, then the vorticity spectrum is dominated for large wave-number k by contributions from the discontinuities in ω , and therefore falls off as k^{-2} ; and correspondingly, $E(k) \sim k^{-4}$ in contrast to Batchelor's prediction. Saffman's conclusion is evidently correct when the vortex patches are far apart compared with their size, since then they interact only weakly, and the vorticity distribution retains its statistical character for a long time. If however the patches are close enough to interact significantly, the situation becomes much more complicated. Vorticity patches tend to merge when they are close enough; if they are of opposite sign this leads to partial or complete annihilation; if of the same sign then merging (involving viscous reconnection of isovorticity contours, or 'isovorts') leads to the growth of scale that is associated with the inverse energy cascade. The nature of the dynamics and resulting spectral form of two-dimensional turbulence is still to this day a matter of continuing controversy and debate.¹

8.11 Later papers

During the next two decades, Batchelor's main research preoccupation was in suspension mechanics, or 'microhydrodynamics' as he himself named the subject. It would be inappropriate to describe his work in this area here except to say that he and the new research team that he gathered around him 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

¹ In the spirit of Richardson, may I propose a rhyme to capture the essence of two-dimensional turbulence:

*An atmosphere stirred by convection
Responds to enstrophic injection:
While filaments fade,
An inverse cascade
Leads to rapid contour reconnection.*

comparison with the inner Kolmogorov scale, so that again (shades of 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 Nu , the result being $Nu = 0.55(a^2\epsilon/\kappa\nu^{1/2})^{1/3}$, where a is the radius of the particle (assumed spherical). The result is in very reasonable agreement with experiment. Batchelor’s great skill in exploiting what is known of the small-scale features of turbulent flow is again evident in this paper. His neglect of the contribution to mass transfer due to particle slip is however debatable, particularly in turbulence that lacks reflectional symmetry, and still calls for further investigation.

Batchelor’s final return to turbulence (in collaboration with V.M. Canuto & J.R. Chasnov) came with his 1992 paper “Homogeneous buoyancy-generated turbulence”. Still, as ever, Batchelor felt most at home in a statistically homogeneous situation. But here a novel variation was conceived, namely a field of turbulence generated from rest by an initially prescribed, statistically homogeneous, random density distribution giving a random buoyancy force. The driven turbulence of course immediately modifies the distribution of buoyancy forces, and an interesting nonlinear interaction between velocity and buoyancy fields develops: the buoyancy field itself drives the flow, so we are faced here with an ‘active’ rather than ‘passive’ scalar field problem. 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”. Alas, Batchelor was himself suffering a lingering decline from Parkinson’s disease which afflicted him with growing intensity from about 1994 until his death at the age of 80 in the millennium year 2000.

8.12 George Batchelor as Editor and as Head of Department

Batchelor’s involvement with the *Journal of Fluid Mechanics* was from the start a very personal one. Prior to 1956, papers in fluid mechanics had been scattered over the literature of applied mathematics and engineering mechanics, and in the journals of the various national academies, including the

Proceedings of the Royal Society. Batchelor saw a need for a journal that would bring together both experimental and theoretical aspects of the subject, and planned *JFM* accordingly, together with a small team of Associate Editors, George Carrier (Harvard), Wayland Griffith (Princeton), and James Lighthill (Manchester). A novel feature of the editorial process, maintained to this day, was that each editor and associate editor should have individual responsibility to accept (or reject) papers submitted to him: they each acted as individuals rather than as members of an editorial board, following agreed guidelines on the scope of the journal and the standards expected. The formula worked extremely well, as may be seen from the quality of the papers published in the early years of the journal.

Batchelor was aided by two Assistant Editors, Ian Proudman and Brooke Benjamin. Their task initially was to prepare accepted papers for the Press, i.e. to carry out the copy-editing process, a process in which George himself led the way, imposing strict rules of style: Fowler's *Dictionary of Modern English Usage* was his constant companion. I served myself as an Assistant Editor from 1961; one of my first tasks was the copy-editing of Kolmogorov's (1962) paper, by no means straightforward! One of Batchelor's guidelines was that the mathematical equations in a paper should be incorporated in such a way as to follow the normal rules of English grammar. Copy-editing was more than simply correcting grammar or punctuation: it sometimes involved complete reconstruction of sentences to clarify an author's intended meaning; of course, this required the author's approval at the proof stage, and extended correspondence could then ensue. But the results of this painstaking procedure are evident in the quality of the early volumes of the Journal, more difficult to maintain in later decades as the rate of publication remorselessly increased (with corresponding increase in the number of Associate Editors).

As I have already indicated, Batchelor was heavily committed, from 1959 on, not only to *JFM*, but also as Head of the new Department of Applied Mathematics and Theoretical Physics (DAMTP); this was his main administrative responsibility from 1959 till 1983, when, under the Thatcher regime, he was induced to take early retirement from his University Professorship. In the five-year period 1959–1964, there were just two Professors in DAMTP, the Lucasian Professor (and Nobel Laureate) Paul Dirac and the Plumian Professor of Astronomy Fred Hoyle. On his appointment as Head of DAMTP in 1959, Batchelor was promoted from his Lectureship to a Readership in Fluid Mechanics, and five years later to a newly established Professorship of Applied Mathematics. These first five years were crucial in the shaping of the

Department. A crisis arose in 1964 when Batchelor's appointment as Head of Department came up for renewal and Hoyle made a bid for the Headship. The matter was put to a vote within the Department, and Batchelor won by a convincing majority, this amounting to a vote of confidence in the quality of his leadership. Hoyle had sought the support of Dirac, but to no avail; in the event, he walked out of DAMTP in a fit of pique, and thereafter devoted his energies and his wayward genius to the foundation of the Institute of [Theoretical] Astronomy on Madingley Road. Dirac spent his time like a hermit in St John's College, above and beyond the fray! In his recent biography of Dirac, *The Strangest Man*, Graham Farmelo well describes Dirac's attitude to the new Department, of which, *faute de mieux*, he was a member: he wanted nothing to do with it, and wanted to be left alone in solitary contemplation in his College rooms! This state of affairs continued until Dirac's retirement in 1969; throughout this decade, his relationship with Batchelor was reserved, but never, so far as I was aware, other than courteous.

After the trauma of 1964, Batchelor settled more securely into his role as Head of Department. On the fluids side, he promoted the emerging field of geophysical fluid mechanics with the help of his brilliant student Adrian Gill, and in parallel with Brooke Benjamin, Owen Phillips and Francis Bretherton. The arrival of James Lighthill, who succeeded Dirac as Lucasian Professor in 1969, led to intense activity in biological fluid mechanics throughout the 1970s. As far as turbulence was concerned, it was MHD turbulence and turbulent dynamo theory, in which I was personally involved, with applications in geophysics and astrophysics, that became a principal focus of effort. Batchelor took a keen interest from the sidelines in these advances, but found it hard to relinquish the vorticity/magnetic field analogy that had been so central to his own thinking twenty years earlier.

8.13 International activity

Batchelor was an enthusiastic adherent of the International Union of Theoretical and Applied Mechanics (IUTAM), founded in 1948, and he soon became a familiar figure at its subsequent quadrennial congresses. For four of these – Stresa (1960), Munich (1964), Stanford (1968) and Moscow (1972) – he served on the Executive Committee of the Congress Committee. During the early 1960s he recognised a parallel need for greater scientific interaction specifically within Europe, and, together with Dietrich Küchemann (of the Royal Aircraft Establishment, Farnborough), he set up a European Mechanics

Committee and served as its founding Chairman from 1964 till 1987 (when he was succeeded in this role by the new Cambridge Professor of Applied Mathematics, David Crighton.)

This Committee established a series of Colloquia, known as ‘EUROMECHs’, the first of which was held in Berlin in April 1965. By 1985 there were approximately 15 EUROMECHs each year; a total of 230 were held during Batchelor’s chairmanship, many in the broad field of turbulence and its applications. His firm hand and meticulous attention to detail can be seen in the notes of guidance written for chairmen of Colloquia: three to five days’ duration, participants from all countries of Europe “extending eastwards as far as Poland and Rumania”, organizing committee of six members for each Colloquium, not more than 50 participants, topics chosen to be of theoretical or practical interest but not devoted to details of engineering applications, introductory lectures to be included, presentation of current and possibly incomplete work to be encouraged, no obligation to publish Proceedings, and so on. These Colloquia were run on a financial shoestring, most participants finding their own individual sources of funding, but this seems to have had little adverse effect; on the contrary, the freedom from the constraints often imposed by grant-giving bodies gave EUROMECHs a freshness and spontaneity that was widely valued. The foundation of EUROMECH following on that of *JFM*, raised the visibility and impact of research in fluid mechanics, and may be recognized as an important part of Batchelor’s great legacy to the subject.

The inclusion of Poland was significant. Through IUTAM, Batchelor was a close friend of Wlodek Fiszdon (of the Institute of Fundamental Technological Research, Warsaw), and had given him encouragement and help in the organization of the biennial Symposia on Fluid Mechanics in Poland that served so well to maintain communication between scientists from East and West during the long cold-war years. The link with Poland developed with the presence of Richard Herczynski, a younger colleague of Fiszdon, as a Senior Visitor in DAMTP in 1960/61.

In 1963 Batchelor attended the Fluid Dynamics Conference held in Zakopane, Poland – one of the first at which any Western scientists were present. He drove across Europe to this meeting with his wife and three young daughters, Adrienne, Clare and Bryony, and took me along also in the last space in the family car! Batchelor’s contributed paper on dispersion of pollutant from a fixed source in a turbulent boundary layer (Batchelor 1964) is a beautiful piece of work, and deserves to be better known. At this conference, he had preliminary discussions with Fiszdon and Herczynski about the need for greater European cooperation in science. He attended several of the subsequent biennial meetings and was involved much later, through Herczynski, in the traumatic

events for Polish scientists during the Solidarity years, always giving moral support when this was most needed.

8.14 Conclusion

Despite the intensive efforts of mathematicians, physicists and engineers over the last 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 towards 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 rigour of the mathematical argument and the depth of physical reasoning that he brought to bear on the various aspects of the problems 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 fifty 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.

Acknowledgements This chapter is an expanded version of my paper entitled “G.K. Batchelor and the homogenisation of turbulence”, published in *Ann. Rev. Fluid Mech.* **34**, 19–35 (2002). I have also drawn extensively from Moffatt (2002); see also Moffatt (2010). I thank Annual Reviews Inc. and the Royal Society for permission to reproduce material from these articles.

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