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Early days of turbulence research in Cambridge

'Early days' and 'turbulence' are vague terms which cover much ground and the following recollections concern the period 1945–1956, referring mostly to experimental work done in G. I. Taylor's old room at the Cavendish Laboratory. My arrival there can be blamed entirely on George Batchelor. We had both been working in the C.S.I.R. Aeronautical Laboratory in Melbourne, George in the Aerodynamics Section and I in the Instruments Section. My work was devising and building gadgets for use in other sections, for wind-tunnel tests, engine tests and so on. One was a very primitive amplifier for hot-wire signals but I did not use it. At the same time, George had been studying the Taylor papers on the statistical theory of isotropic turbulence, and extending it to axisymmetric turbulence. He had in mind coming to Cambridge at the end of the war as a research student of G. I. Taylor and asked me to consider doing the same. I had interrupted my research in nuclear physics in 1939, intending to return and finish my scholarship, but I found myself signed on to do experimental work on turbulent flows. Why I was accepted is still a mystery, as my ignorance of fluid dynamics, let alone turbulence, was almost total.

On arrival in 1945, I found myself the sole occupant of a large room on the ground floor of the Old Cavendish Laboratory in Free School Lane, quite near the 'garage' where I had worked on β-ray spectra during 1938–9. It held a low-turbulence wind tunnel (according to rumour designed by G. I. Taylor and W. Farren while punting to Grantchester), much of the apparatus used by G. I. over the past twenty years, and plenty of dust. The sections of the tunnel had to pass in through a standard door, and its size was the maximum for the room. If the control rods for the traverse gear were fully withdrawn, they nearly hit the window glass and only a thin man could pass between the far end and the wall. Whether or not the room was spacious, most of it was soon filled with racks of primitive hot-wire electronics, a distortion tunnel, a water channel almost as long as the room, and three or four humans.

Perhaps because he foresaw the coming congestion, G. I. had moved to a room of similar size down the corridor where he worked with Wally Thompson, his personal assistant. Wally found time to do much work for me, notably the construction of a distorting tunnel, possibly the only one ever made from tin plate and held together by soft solder. George Batchelor was put in a very small room about ten yards away. I remember meeting Owen Phillips there on his arrival in Cambridge and, more vividly, Sidney Goldstein. He nonplussed us by declaring as simple fact than no one as young could possibly appreciate the nature of the distinction between flows with zero viscosity and flows with very small viscosity. Perhaps I still do not.

An event of some importance was the arrival in Cambridge of L. S. G. Kovasznay from Hungary on a British Council award. The Council rang to say that they had there a Hungarian who was to work with us on turbulence. As anyone who knew him would expect, he was an instant success even before we fully understood his Central European accent and use of German syntax with English. He had worked on the Kármán vortex street and began to determine the velocity pattern from velocity correlations between two hot-wire anemometers. This must be almost the earliest use of conditional sampling, a technique developed more fully at Johns Hopkins. For that work, he made a low-speed, heated cylinder anemometer that could be used for air speeds as low as 30 cm/s. It was still in use five years ago. If I am critical of his accent on arrival, he was much more intelligible than von Kármán who came to talk with G. I. from time to time.

By 1945, research on turbulent flow had changed in emphasis from its earlier concentration on mean flow properties to attempting to describe and understand the turbulent motion itself. The inspiration was the statistical theory of isotropic turbulence developed by G. I. Taylor, coupled with the development of the hot-wire anemometer by Dryden, Simmonds, Hall and others. While ignorant in the field of fluid dynamics, I had used and made many electronic gadgets and it was no problem to design amplifiers and analogue devices for statistical analysis of hot-wire signals.

After a few months, we had equipment to measure turbulent intensities and two-point velocity correlations and, not long after, statistical distributions of velocity fluctuations and their time derivatives. At that time, I was reliant on George to suggest the measurements of grid turbulence that could be useful. He had found the work of A. N. Kolmogorov on local isotropy of the small-scale eddies, which seemed to open a route to the understanding of inhomogeneous and shear flows. A test of the prediction of form for the structure function in the inertial range could not be made in our small tunnel, and verification (in terms of the spectrum function) had to wait on Bob (R.W.) Stewart's work in the ocean. We did show that the large-scale structure in grid turbulence and wakes was independent of the Reynolds number, although few, and certainly not G. I. Taylor, had doubted it.

The decisions on what to measure were made initially by George. He and I, once I understood it, were excited by the concept of local isotropy for the smaller eddies, independently formulated by Kolmogorov, by Weiszacker and Onsager. It was the first plausible prediction valid for all kinds of high-Reynolds-number flows. The Reynolds numbers obtainable in our tunnel were too small to allow confirmation of the inertial subrange, but a variety of correlation and spectrum measurements (especially by Stewart) showed that the essential differences between flows with similar geometry at different Reynolds numbers were confined (nearly) to those motions most concerned with viscous dissipation. Of course, this notion of similarity had always been an element of Taylor's concept of turbulence but measurements of the fluctuations were a step to understanding its limitations.

The other interest was predicting the development of turbulent flows by assuming similarity in form of mean value distributions of mean velocity and Reynolds shear stress for shear flows, and of similarity of the two-point correlation functions for isotropic turbulence. An assumption of complete similarity implies that turbulent intensity should vary as the inverse of decay time measured from some origin, and, needless to say, our measurements were consistent with that variation. At this time, it was being realized that the Loitsianski invariance condition precluded that decay law and required one with a $-\frac{10}{7}$ power. George then showed that, for small turbulent Reynolds numbers, decay would follow a $-\frac{5}{2}$ power law. Work at Johns Hopkins by Corrsin and others confirmed the $-\frac{10}{7}$ decay for larger Reynolds numbers and Stewart in our tunnel demonstrated that the final period decay law for all three velocity components although the motion became strongly isotropic. In fact, grid turbulence (without massaging) is not even crudely isotropic and many grids do not produce homogeneous turbulence.

To further complicate the issue, I started a series of spectrum measurements of grid turbulence to test the validity of the Heisenberg inverse seventh power spectrum for the far viscous range. In a moment of inspiration, I decided that the power could be obtained by measuring the flatness factors of a number of high-order velocity derivatives, and I managed it for the first four. The kurtosis increased both with order of the derivative and with the Reynolds number of the turbulence. I then realized that the measurements said nothing about the spectrum, but they did show a considerable departure from the predictions of local similarity. The spatial intermittency of small-scale motion is now well know. Our trouble in writing up the work was to explain why we had done the measurements in the first place, pride preventing use of the words, 'In a moment of exceptional stupidity, we measured ... '.

Fritz Ursell inserted himself into the wind-tunnel room to use a small towing tank that G.I. had once used for a Christmas lecture at the Royal Institution. He verified by experiment that trapping wave modes occur on sloping beaches unbounded in the off-shore direction. A more serious threat to our accommodation was the arrival from Canada of R. W. Stewart, well over six feet tall and broad with it. His work on velocity correlations for grid turbulence went very well, even though a second, distorting tunnel made its appearance. We got on well, discussing the various sports which we played. He played lacrosse for the University until he broke a leg – not that that kept him out of the laboratory for long. When Bob Stewart returned to Canada, Chris Nicol kept the connection going with a study of velocity and temperature fluctuations in a boundary layer with a heated wall. He was able to observe that with a large stable temperature gradient, turbulent motion 'collapses' to a state resembling the nocturnal inversion.

In the summer of 1947, George and I submitted for Ph.D. and underwent oral examinations. I think George had Howarth and G. I. Taylor, while, in consequence of pre-war work on β-ray spectra, I had N. Feather, a nuclear physicist, and G.I. They spent much time explaining to each other the meaning of their questions and I had little to do. Afterwards, G.I. took me off on his boat to sail first down the Channel to Brixham, then to Brittany, Guernsey and home. It was my first experience of sailing on the open sea and I had much to learn. G. I. Taylor was very patient and it was a wonderful trip.

An important event was the International Conference on Applied Mechanics, held at Imperial College, London, in 1948. George and I both gave papers, for my part with some misgivings with so many famous men in the audience. G.I. was of course used to that sort of thing and produced a paper on the swirl atomizer. He began by criticizing the current engineering theory, then developed what he claimed to be the correct hydrodynamic theory, and paused. Several of his distinguished colleagues seemed to think that the end had come and were about to clap politely. G.I. uttered what I regarded as his motto – 'So I did some experiments' – and proceeded to show (*a*) that the hydrodynamic theory did not describe the action of a real atomizer, and (*b*) that, erroneous though it was, the engineering treatment gave a substantially accurate description. The reason for success of the engineering assumption of constant core diameter was that boundary layers within the swirl chamber modified the effective shape.

In contrast, G. I. Taylor had given a few lectures on the statistical theory of turbulence to a few research students, encouraged to do so by George. My memory is of him deriving on the blackboard and in detail the mean-value ratios of velocity gradients, using the methods of his 1935 paper. Obviously, he saw no point in deriving them in another way to get the same results. On the other hand, if the subject was of current interest, he could be interesting and instructive.

G.I.'s work of this time that I recall was on longitudinal diffusion in turbulent pipe flow and on the swimming of spermatazoa. For the diffusion, he tested his calculations by injecting a plug of saline water into a long pipe installed on the wall of the corridor outside and, with some assistance from Tom Ellison, observing the changes in electrical conductivity as the plug passed by. For the spermatazoa, he used a mechanical model working in golden syrup and water-glass as liquids of sufficient viscosity to model swimming at laboratory scale of his mechanical spermatazoa.

The first coordinated account of our work took the form of the monograph *The Theory of Homogeneous Turbulence* (Cambridge University Press 1953) in which George put together current theoretical and experimental developments in the statistical description of homogeneous turbulence. At this distance, it may seem to be nearly the end of purely statistical approaches before concepts of eddy structure began to assume importance. If I were to select the piece of work that impressed me most, it would be George's paper on extension of material lines and surfaces. It excited me greatly because, unlike manipulations of the spectrum and transport equations, it offered a comprehensive model for discussing turbulent mixing of scalars and especially for the production of turbulent vorticity by stretching. At that time, I had tried to trace fluid particles by 'marking' them with heat spots. While this worked well in laminar flow, the spots cooled too rapidly in turbulent flow. The Batchelor paper offered a starting point to calculate the maximum rate of cooling by enhanced diffusion, obtained if the principal axes of strain remain fixed with respect to the fluid. To my surprise, the measured rate was very close to the calculated rate for times comparable with the timescales of the large eddies, showing that the transfer from large scales to microscales is a continuous process, and that the 'cascade' model should not be taken too literally.

My interest in the far wake of a cylinder began after a visit to R.A.E. Farnborough with George, where we saw work on achieving laminar flow over wing sections both in flight and in wind tunnels. A thin wire stretched normal to the wing about a chord length upstream was found to induce a wedge-shaped region of turbulent flow with a surprisingly sharp point. Curious to find the width of the disturbing wake, I stretched a 1.5 mm diameter wire across the entrance to the tunnel and started to measure turbulent intensities and velocity correlations. Significant levels of correlation existed over nearly the whole wake, showing strong, long-range interactions.

My next stage was the measurement of the terms in the Reynolds equation for the turbulent kinetic energy. Not only was much energy being transported by turbulent convective movements but, in some parts of the flow, the transport was up the intensity gradient rather than down. To account for the observations, it was suggested that there exist 'large eddies' spanning the wake and largely responsible for the convection of energy. Now they would be 'coherent structures'.

Similar measurements were made in the boundary layer on the floor of our wind tunnel, giving rise to similar conclusions. A difference is that a large eddy capable of convection in the outer layer will induce near the wall only motions parallel to the wall and so be ineffective for the transport of both momentum and kinetic energy. Other, smaller wall eddies with centres close to the wall do the transport but much of the motion within the constant-stress region is swirling and 'inactive'.

This work convinced me that, in spite of its apparent simplicity, homogeneous turbulence poses a much more difficult problem than shear flows which are subject to more constraints. For a free turbulent flow, the condition of momentum conservation is something that is missing in homogeneous flows. My views on turbulent eddy structures in shear flows were set out at length in *The Structure of Turbulent Shear Flow* published in 1956 by Cambridge University Press. It was fortunate in some respects that much experimental work to refute me remained undone.

After his work on material lines and surfaces and its applications to magnetohydrodynamics, George spent much time and energy on the closure problem for homogeneous turbulence. At first, the assumption of joint normality of the various order covariances seemed promising, but that was shown by Kraichnan to violate energy conservation. In the end and to my regret, George abandoned active work on turbulence to organize the Department of Applied Mathematics and Theoretical Physics in Cambridge. It was certainly a most worthwhile achievement for fluid dynamics not just confined to turbulence!

Looking over the preceding text, I see I have omitted reference to many colleagues whose work was mainly theoretical, Ian Proudman, Philip Saffman, Owen Phillips among others. They were very much a part of the group and I apologize. My intention was to describe what I knew best, the old room where I measured turbulent intensities, etc., and learnt about turbulence from George Batchelor and G. I. Taylor.

A. A. TOWNSEND