Remarks on high-Reynolds-number turbulence experiments and facilities

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Abstract

A summary is given here of remarks made at the Workshop on the need for research in high-Reynolds-number turbulence and the possibility of using a helium tunnel for the purpose.

The main text

In an eloquent assessment that deserves to be read even today, von Neumann (1949) stated: 'The great importance of turbulence ... requires no further emphasis. ... Turbulence undoubtedly represents a central principle for many parts of physics, and a thorough understanding of its properties must be expected to lead to important advances in many fields. ... turbulence represents *per se* an important principle in physical theory and in pure mathematics. ...These considerations justify the view that a considerable ... effort towards a detailed understanding of the mechanism of turbulence is called for.' A few further remarks may be in order, especially espousing the need for experiments at high Reynolds numbers.

It has for long been clear that fluid turbulence is a significant engineering problem but, in spite of von Neumann's remarks, its place in mainstream physics has been ambiguous: turbulence has generally been thought to be too difficult and unfocused. The situation appears to be changing in recent years, and turbulence appears to be inching towards the important position in nonlinear physics that is its due.

The equations governing fluid turbulence have been known for long, but the information extracted from them has not been overwhelmingly large. To the extent that turbulent flows are nonlinear systems with many degrees of freedom which are strongly coupled, we have very little intuition about them. I cannot help but quote von Neumann again: ...our intuitive relationship to the subject is still too loose..., (and) we are still disoriented as to the relevant factors...' We are thus exploring new avenues all the time. The point I wish to make is that much progress in turbulence has eluded us at least partly because of the compromises that typical (that is, most) experimentalists are forced to make: Without proper tools, one cannot arrive at definitive facts. Without definitive facts, definitive theoretical ideas – which in turn feed the type of measurements that one should best make – cannot be developed. The atmosphere now seems right for this type of feedback.

I argue that several issues can be settled by first-rate experiments in high-Reynolds-number turbulence. Traditionally, most experimental research in universities has been carried out at moderate

Reynolds numbers, and the strong belief has been that the precise value of the flow Reynolds number is irrelevant as long as it is 'sufficiently high'. Most research wind tunnels thus cluster around a small region in Reynolds number. Many high-Reynolds-number experiments have been made in atmospheric and oceanic turbulence, and much has been learnt from them especially concerning the universality of small scale turbulence. The general feeling appears to be that a judicious combination of moderate-Reynolds-number experiments under controlled conditions and high-Reynolds-number experiments in geophysical flows serves our purpose for the most part. One might therefore wonder if there is indeed a compelling case to be made for expensive experiments at high Reynolds numbers. In other words, are there some specific and crucial issues to be settled for which one or more dedicated high-Reynolds-number research facilities are essential?

One might draw upon the few previous experiences when high Reynolds number facilities were made accessible to the turbulence community, albeit under sub-optimal conditions. For example, one has learnt something new and substantial from experiments such as those of Roshko (1961) and Kistler & Vrebalovich (1966), behind a cylinder and in grid turbulence respectively, made during the last days of the Southern California Cooperative Wind Tunnel before it was dismantled. Such focused measurements cannot be made in natural flows with uncontrollable experimental conditions and inherent non-stationarity. For example, it is impossible to make long-time measurements such as those required to determine high-order moments accurately. Indeed, there are suggestions that such moments diverge (Mandelbrot 1974, Schertzer & Lovejoy 1985).

One can also argue, simply from the point of view of providing hard data to which theoretical ideas have to be moored, that a well-executed experiment at high Reynolds numbers is worth more than many that are done under compromising conditions of Reynolds numbers, development length, resolution, etc. As in all asymptotic analyses, progress follows from an understanding of the high Reynolds number domain even if many (if not most) applications might pertain only to moderate Reynolds numbers. Yet, it is worth asking specifically: What critical issues need to be settled by new experiments at high Reynolds numbers? What intellectual and practical gains justify investing in them? Attempting an answer to these questions is too serious a task to be accomplished here, but a few points will nevertheless be made. They range (in arbitrary order) from the practical to the fundamental.

For almost fifty years now, Kolmogorov's (1941) ideas and their later modifications (Kolmogorov 1962, Obukhov 1962) have ruled the horizons of research in turbulence, and yet we are unclear about their status as a true theory. As an example, different theoretical ideas of recent origin predict that the celebrated –5/3 law is exact (e.g., Nakano 1986, 1988; Chorin 1988), approximate and needs a positive correction (e.g., Monin & Yaglom 1971), and approximate and needs a negative correction (Yakhot et al. 1989). Which of them is correct? What is the best way of experimentally settling this question? The situation is worse in sheared turbulence. How much of the structure one sees in shear flows is a manifestation simply of the shear? How do the hairpin vortices oriented at 45° to the mean flow direction survive at very high Reynolds numbers if the Kolmorogov cascade, grinding inexorably towards the isotropy of small scales, is the principal dynamical mechanism? What are the asymptotic properties of the probability densities of turbulent fluctuations? What is the nature of turbulent mixing at very high Reynolds numbers? Do the small scales of a passive marker attain an asymptotic state independent of the shear? What role does intermittency play in all these issues? The uncertainties surrounding these questions are unsettling to students and professionals alike, unless mere familiarity renders one immune to them.

Let me proceed along this line a bit further: What is the asymptotic value of the drag coefficient for a high-aspect-ratio circular cylinder? What is the correct extrapolation formula for the skin-friction coefficient in a two-dimensional boundary layer? Is the logarithmic region in the turbulent boundary layer asymptotically consistent with the dynamics? Does the momentum transport in high-Reynolds-number turbulent boundary layer occur principally in the form of intermittent events occupying relatively small amounts of space and time? High-Reynolds-number experiments are especially critical in the boundary layer because of the well-known difficulty of separating inner and outer scales at low Reynolds numbers.

These (and other similar) questions can be narrowed down and perhaps resolved by means of high-quality experiments at high Reynolds numbers. One way of attaining such high Reynolds numbers is to build *substantially* bigger and better wind tunnels, water tunnels, pipe flows, or some such standard facility. (An experience concerning a wind tunnel being built at IIT, Chicago was discussed at the Workshop by H.M. Nagib.) A different suggestion is to build a helium tunnel whose virtues were the subject of this Workshop. It is trivial to argue that a helium tunnel of *modest* size can provide high enough Reynolds numbers that can be used for much of the standard model testing. The Workshop revealed several instances where the feasibility and advantages of the helium tunnel have been demonstrated. Much of the needed technology appears to be available. With such a facility one can address several of the questions raised above (such as the drag on a flat plate and a circular cylinder, or others such as decay rates of turbulence behind grids).

The situation, however, is unclear from the point of view of research in small-scale turbulence. To decide on the feasibility of using helium tunnel for this purpose, one first has to address questions such as: how can we resolve small scales of turbulence (e.g., vorticity, dissipation)? Can one obtain meaningful spectral data? How much quantitative flow imaging will be possible? What are the intrinsic limitations of resolution in experimental diagnostics? In short, how much new technology needs to be developed before a helium tunnel becomes a good tool for turbulence research? These enquiries are interesting in their own right.

Concluding remarks

I have been principally concerned in my remarks about turbulence research at high Reynolds numbers. I have briefly outlined some thoughts without the benefit of a thorough and quiet study. It is, however, clear that some thinking is required about the 'optimality' of the present mode of experimental research in turbulence. In this context, the potential of the helium tunnel should to be addressed adequately.

If one ignores the perspective of turbulence studies, it is clear that helium tunnels can extend the Reynolds number range accessible to most experimentalists. In particular, if they can be built without extraordinary expense so as to be accessible also to the university community, they acquire an added value.

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Helium