Doing Little Science with Dhawan

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When I joined the Department of Aeronautical Engineering, Indian Institute of Science, Bangalore, in 1953 for doing a Diploma (as it was then called), the 'star' in the Department was undoubtedly Prof Dhawan. His labs, his classes, his attitudes all made an immediate and deep impression on the twelve new students who joined that year and enthusiastically yielded to the spell of his personality. Here was a tall, young, handsome bachelor who drove up in a sporty little red MG, raced up the staircase that arches over the 7 ft × 5 ft tunnel, and came into the classroom uttering a cheerful and smiling "Good Morning" – all this at a time when the general atmosphere in the Department was rather stern and formal (although some of the other faculty were also pleasant enough, led by the German Head of the Department, Prof O G Tietjens).

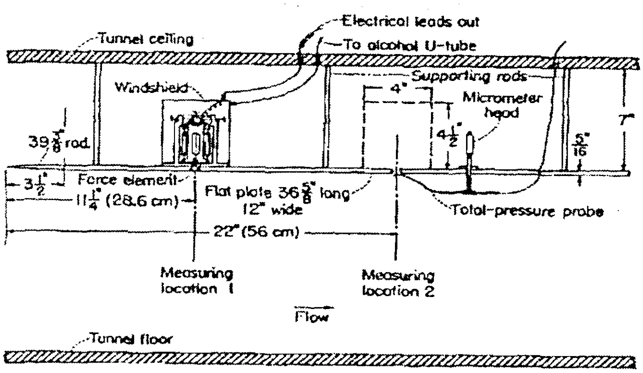
Dhawan's lectures were advanced, simple and elegant all at the same time, and quickly gave students a sense of confidence. Students liked his classes very much indeed, and for a variety of reasons; the first of these was, as I have already remarked, Prof Dhawan's general cheerfulness in his approach to the subject as well as to the students. He took his teaching very seriously, and supplied his classes with plenty of notes, data sheets, diagrams and so on. He worked hard on all these – one would often see him in his office late at night – and he expected the students to work just as hard – which many of them cheerfully did. Another reason for the great popularity of his classes – last but not least, as they say! – was that if he was convinced you had understood what was going on, he was generous with his grades!

Keywords
Satish Dhawan, skin friction, supersonic wind tunnel.

It was in the High Speed Aerodynamics Laboratory that he had set up at the suggestion of Tietjens, however, that his personality was most forcefully expressed. First of all, everything there looked different, and worked well. The laboratory somehow

Box 1. Direct Measurements of Skin Friction

A fluid flowing past any surface, such as for example an aircraft wing, exerts a shear stress on the surface due to the action of viscosity; this stress, which varies from point to point, is in part responsible for the 'drag' of the surface, i.e. the resistance offered to its motion by the fluid. In the first half of the 20th century, the introduction of the idea of the boundary layer by Prandtl led to new and rational estimates of the skin friction, which was shown to depend on the Reynolds number (which is a non-dimensional number given by (flow velocity × characteristic length scale of the body)/the kinematic viscosity of the fluid). Although various experiments had confirmed the usefulness of the boundary layer idea, no direct measurement of the skin friction on a surface had been made, till Dhawan devised a floating element balance for making such a measurement. In this balance a small strip of the surface is isolated from the rest of it and is allowed to 'float', i.e. it is free to move against the stiffness of appropriate elastic flexures (springs). The deflection of the element is a measure of the skin friction it experiences. In the balance the measurement is made by a nulling technique by which the element is brought back to its original position by applying a force which then provides a measure of the skin friction. Dhawan measured the deflection through a linear variable differential transformer; the size of the element was 2 mm in the direction of the flow × 20 mm across, the movement of the element being of the order of a few thousands of an inch. The figures here show (a) design of the instrument and (b) the results of the measurement compared with the theories available at the time. Numerous such floating element balances were made after Dhawan's work [3].



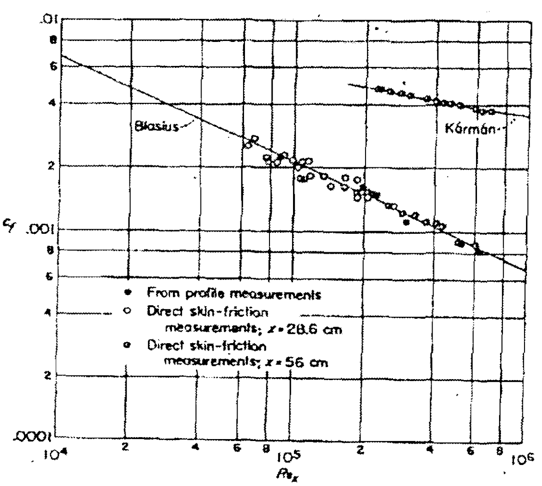


Figure 1a: Sketch of flat-plate installation in GALCIT 2½ by 2½ foot correlation tunnel.

Figure 1b: Local skin friction in incompressible flow.

Box 2. Shock/Boundary-layer Interaction

In supersonic flow, the fluid can encounter sharp discontinuities in flow variables such as normal velocity, pressure, temperature, etc. at what is known as a shock wave. If a shock wave hits a surface, then ideal fluid theory lays down how the parameters of the reflected shock are related to those of the incident shock. (Shocks are nonlinear waves, so the laws of reflection are not as simple as in classical optics.) However it is well known that no matter how small the viscosity of the fluid (or more precisely how high the Reynolds number) the effects of viscosity will be felt in a thin boundary layer near the surface, and so shock reflection is not quite the process that this ideal fluid theory describes. The experiments of Liepmann, Roshko and Dhawan [4] showed that surface pressure does not exhibit a discontinuity at the shock impingement point; there is a smearing out which is wider when the boundary layer is laminar than if it is turbulent (*Figure* 2a). In fact the flow can even go from laminar to turbulent in the neighbourhood of the shock impingement point and so the reflection process can be quite complicated (*Figure* 2b).

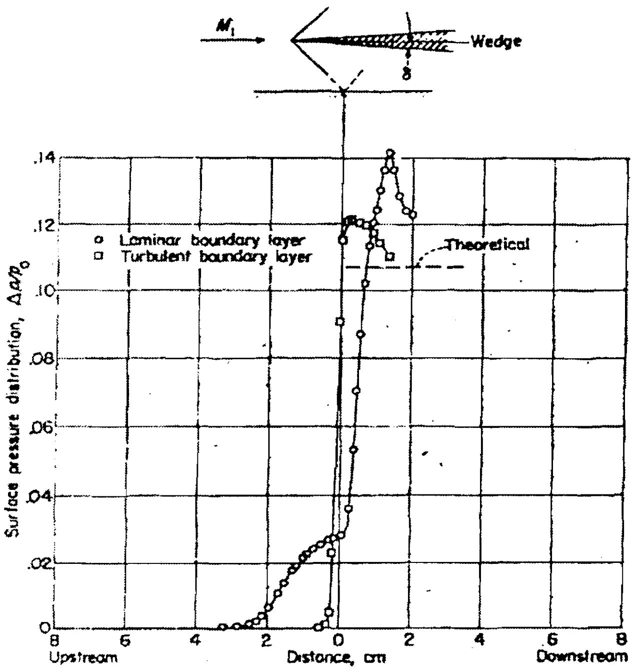


Figure 2a. Reflection of shock wave from flat surface. **$\delta = 3^0$; M_1 = 1.4; $R = 0.9 \times 10^4$.**

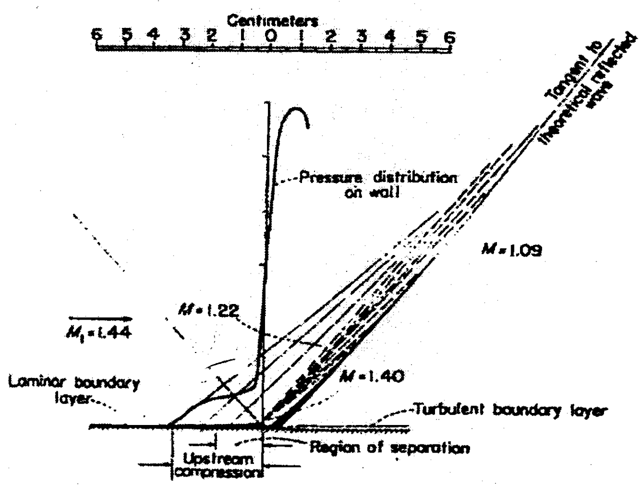


Figure 2b. Model of shock-wave reflection from flat surface with laminar boundary layer. Incident wave **$\delta_t = 4.5^0$; $M_1 = 1.44$; $R = 0.9 \times 10^6$.**

managed to convey an impression of both science and engineering; it had 100 hp compressors running supersonic wind tunnels, as well as lenses and galvanometers measuring what was going on in those tunnels. Much of the equipment and instrumentation were made locally, for they could not be easily borrowed or bought: clearly the idea was to get on with the experiments whichever way they might best be conducted. I vividly recall how a small 1 in × 3 in supersonic wind tunnel was calibrated, with the help of all hands that could be mustered at any given time, to open valves, ring bells, take readings, click cameras, etc. – to a young student like me it was all very dramatic and modern. (Not that the number of people so mustered was very large: the Department was still small at that time.)

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There was also an earlier 1 cm × 1 cm supersonic wind tunnel, which ran on compressed air from two war-time surplus oxygen tanks from a Dakota – complete with a schlieren system which quickly demonstrated shock and expansion waves to students, and made them real at a time when, to most people, they were no more than fancy ideas in fancy foreign books.

While I was working in the High Speed Lab, a low-turbulence boundary layer tunnel was getting ready, and as I joined as a research student in 1955 I moved to the new Boundary Layer Lab which housed this tunnel.

Both laboratories had a variety of ingenious little devices, rigged up by Dhawan with great and obvious pleasure, to make things clearer for the student or easier for the experimenter. Among these "gizmos", as he loved to call them, I remember a pretty little thing for electroplating 5 micron tungsten wires with copper, so that they could later be soldered for making hot wire probes – I started my life in the laboratory, like so many students of fluid dynamics everywhere in the world at the time, struggling to make these probes for turbulence measurements. I still recall Prof Dhawan teaching me how to make these probes, telling me about the strict 'ritual' one had to follow – "like doing pooja", he would say. The fine wires we needed for these probes

Box 3. Transition

Fluid flow can exhibit either of two states, laminar (generally smooth and regular) or turbulent (generally irregular, chaotic, apparently random). In flow past any body or surface the state of fluid motion is usually laminar near the nose or leading edge, and undergoes 'transition' to turbulence further downstream. For a long time there were two contending pictures of how transition actually takes place: one seeing it as abrupt, the other as gradual – what experimental evidence there was could be used to support either view. It turns out that this transition does not occur across a sharp front but rather over a region, which can some times be quite extensive; the flow within this region is turbulent only part-time. The two pictures were reconciled when it was discovered that transition is characterized by islands of turbulence, called spots, which have sharp boundaries; the flow is fully turbulent only when the spots grow so big that they entirely cover the surface. The fraction of time that flow is so turbulent at any point is called the intermittency. The diagram here shows that, irrespective of the agent causing transition, measurements made at IISc and the National Bureau of Standards all follow a universal curve (*Figure* 3) [5].

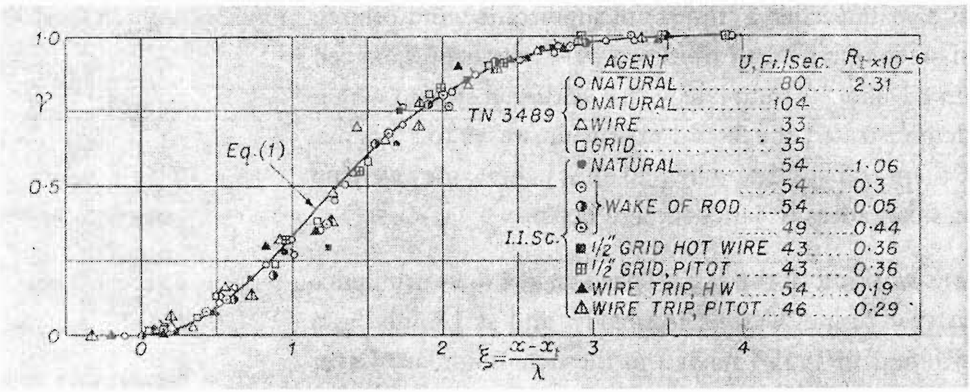

Figure 3. Universal distribution of γ vs ξ with transition due to different agents.

were not easily available, and Dhawan got them from friends in the United States: various bits of platinum and tungsten wire arrived by mail, stuck on the back of letters written to him (we used to hoard them like misers).

He would often rig up small experiments very fast, with whatever was available. For example, I remember a tiny vertical wind tunnel made out of a drawing sheet rolled into a tube, with a little fan driving air through it from the bottom. Placed across the tube at the top of the tunnel was a 'test cylinder', which was just a fat wire, and students were shown the famous Karman vortices in the wake of that cylinder by the use of hot wire probes.

Dhawan thought at the time that we needed to do something on transition from laminar to turbulent flow, because the wind speed and model size for the Department's 5 ft × 7 ft tunnel left flow on the wing of a typical aircraft model in an awkward transitional state. It was characteristic of him to identify the nature of the problem with the tunnel, and so start a *basic* research programme to figure out if anything intelligent could be done about it. That is how I started studying transition for my Associate's thesis.

When I needed a hot wire amplifier for my research it had to be built from scratch (this was long before commercial sets became available in the market, and in any case we had very little money, especially in foreign exchange), so Dhawan hired a graduate from the Electrical Communication Engineering Department (P B Krishnaswamy) to help me on the project. We struggled for a year to put together a high-gain vacuum tube amplifier, operating on a 90 V dry-cell battery pack (of the kind that was still being used in the 1950s to run radio sets in villages). The amplifier seemed to suffer from every known instability (and some unknown ones as well – or so it seemed) before we had it working satisfactorily. Then we had to take photographic traces from an oscilloscope to measure intermittency – no cameras were available in the Department, and the one person who had it on campus would not dream of loaning it to us. So one day Dhawan asked, 'Can't we stick a film on a piece of wood and draw it across the oscilloscope?' So I made a 'linear' oscilloscope camera, with a length of unrolled 36 mm film stuck on a sheet of plywood. This sheet was then pulled through a black box with a slit facing the oscilloscope. As the velocity of the film was hardly uniform, equal intervals of length on the film did not correspond to equal intervals in time. So I used beam intensity modulation to provide time markers and counted time intervals manually after the film was developed in a make-shift room in the lab But it worked – worked so well, in fact, that we could make good sense of the data, and even write a paper for the *Journal of Fluid Mechanics*, but more importantly I learnt how, with some ingenuity, one can overcome what seem like insuperable difficulties.

Another pair of related incidents I must recall, because they show the way Dhawan worked with his students. As the boundary layer tunnel was calibrated and the hot wires began to work, Dhawan one day dropped on my desk a NACA report [1] on turbulent spots, which immediately seemed to provide a framework for understanding the transition zone. There was something strange in this work, however, because while it confirmed the value of the concept of a turbulent 'spot' that had been originally proposed by Emmons [2], the quantitative prediction of his theory was not compared with the experimental results that had been obtained. I was surprised by this apparent omission, but quickly found out why: when I made the comparison the disagreement was huge.

As my thesis work began making some progress, I finally thought I had found a clue to the interpretation of measured intermittency distributions. Dhawan was convinced about the soundness of this work, and encouraged me to publish a note in the *Journal of Aeronautical Sciences*. Although it took me some time to figure out the reason for the huge disagreement, and where the theory had to be modified, in time the new hypothesis began to sound so reasonable – indeed obvious – that I was a little surprised that nobody else had thought of it. It so happened that at that time Dhawan had a stream of friends visiting him from abroad, and he would often leave them with me in the Boundary Layer Lab to discuss our transition work. One such was an Indian scientist working in the US, who found it virtually impossible to believe that we might have done something new, and hinted how very unlikely it was that (with my crude, home-made dry-cell hot wire amplifiers) I might be right. I failed to convince him (although he seemed to have no specific scientific objections), and towards the end of our conversation he was well on the way to shaking my confidence. When I duly reported on the encounter to Dhawan, he was appropriately dismissive, so my confidence was restored. Fortunately some months later the famous Kurt Friedrichs from the Courant Institute visited Dhawan, who again dropped him off in the BL Lab. (Friedrichs was co-author, with Courant, of a book on supersonic flow and shock waves – a book that was very well known but was so mathematical that to most engineers it was Greek and Latin.) After my earlier experience I started a little cautiously, but Friedrichs bought the argument right away. Indeed he was fascinated by what we were doing, and was quickly asking rather deep questions to which I had no answers at all, but the very fact that he *thought* we might have them wiped out the effect of the other encounter.

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A very strong point in the Department's programme at that time was the way that it combined, under Dhawan's leadership, a concern for science with a responsiveness to the country's needs. I am sure that all of Dhawan's students were impressed by how, on the one hand, he was intrigued by the fundamental scientific problems in one's own subject, and, on the other, driven by his desire to use one's knowledge towards applications on a larger scale.

In a very real sense I think Dhawan established, at IISc and – by example – elsewhere in this country, a tradition of scientific research on engineering problems.

Suggested Reading

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[2] H W Emmons, *J. Aero Sci.*, Vol.18, p.490, 1951.

[3] S Dhawan, NACA Report 1121, 1953.

[4] H W Liepmann, A Roshko and S Dhawan, NACA Report 1100, 1952.

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