Fluid Dynamics as a Branch of Physics

By ***M. J. Lighthill***

I MUST confess that I have never traveled 5000 miles before to make an after-dinner speech, and I feel warmly appreciative about your having thought of me, 5000 miles away, when you were planning this occasion. To be sure, I know that the difference between an after-dinner speech and any other kind of speech is just a trifle less marked in these longitudes than it is around the meridian of Greenwich, where one can rely on an audience half-seas-over in port and brandy instead of just recovering from the martinis. I was greatly impressed, for example, once at Brown University when a very good dinner indeed was fitted in as a sort of filling in a sandwich between two thick and stodgy talks in a certain adjoining lecture room on "Sound Generated Aerodynamically" and "Shock Wave Boundary Layer Interaction". The audience stayed awake manfully, even in the inner viscous layers, and no snores were generated, aerodynamically or otherwise.

Still, there is inevitably a danger that the universal British admiration for your American stamina may lead us to approach a task like the one with which you have honored me tonight in a spirit of excessive rigor and austerity. It is certainly of crucial importance to check whether this matter of adjournment to a convenient lecture room equipped with facilities for writing differential equations on blackboards is on the program or not! My friend Professor Milne-Thompson will never forget the trauma of having to deliver a speech prepared assuming that it was on an occasion when it wasn't. His embarrassment at having to perform with an extemporized blackboard screened from approximately 43 percent of his audience of hydrodynamicists and wives by the pillars of the Statler Hotel dining room was only tempered by his awe and amazement in having the frequently required blackboard erasures performed entirely by two distinguished admirals of the United States Navy, who each took charge of half the square-footage concerned and regularly interchanged the duster on a ballistic trajectory as the lecture proceeded.

So I said to myself on planning this lecture, "James, the differential equations are out! You must resist the temptation to render anything, even the audience, uniformly valid, and pick rather some imposingly general theme, which even an applied mathematician can handle without visual aids." And here perhaps I would like to make a little parenthesis and reply once for all to a question that has been put to me several times during this visit by saying that changing my job last November has not stopped me being an applied mathematician! I hope you will be able to keep a continuous check going on this point by your usual conscientious study of the voluminous literature of our science. Mind you, the job is having the salutary effect of forcing my attention on to quite a range of subjects quite outside fluid dynamics, such as chemistry, metallurgy, aircraft structures and control systems, electronics and our old friend "space"—but one can, at least, reasonably hope that all *this* can help an applied mathematician to become more and more . . . applied; at least, that is the gen-

M. J. Lighthill, director of the Royal Aircraft Establishment at Farnborough in England, presented this address in Baltimore at a banquet of the Fluid Dynamics Division of the American Physical Society during the Division's annual meeting in November 1960.

eral aim, and only time will show how it works out in practice. Certainly it is a tremendous experience looking after a place bubbling over with vitality like the Royal Aircraft Establishment, every one of whose 8000 employees takes a positive pride in being part of RAE, while its 1400 scientists and engineers continuously throw up ideas throughout the "aerospace" field; and at present I wouldn't give it up for anything!

WELL, I must get around now to the "imposing general theme" that I ended by picking on, namely "Fluid Dynamics as a Branch of Physics". The choice may seem rather too obvious in front of this audience, but it is continuously interesting, I think, to discuss why fluid dynamics remained almost completely unabsorbed by physics until considerably less than a century ago, and to note how the process of absorption has taken place only gradually since then, initially as a result of the work of Rayleigh and Prandtl and von Kármán and G. I. Taylor and the rest, and how it has proceeded with accelerated speed over the last thirteen years, greatly as a consequence of the work of members of the Fluid Dynamics Division of the American Physical Society.

Now possibly you may not agree that before approximately the twentieth century, fluid dynamics failed to be absorbed in physics. Still you are bound to admit, I think, a contrast with the dynamics of solid bodies, which lay at the very heart of physics, was established by direct correspondence between precise measurements and a precise mathematical theory, outstanding scientific minds combining to treat this correspondence with high seriousness. By comparison the scientific study of the dynamics of fluids was regarded as frivolous, a view like that of the biblical authors who thought the wind bloweth "where it listeth", and who lumped together as equally indescribable the way of an eagle in the air, the way of a ship in the midst of the sea, and the way of a man with a maid.

I'd like to remark again that the great physicists from Newton onwards were notable for unceasingly comparing theory and experiment. They tended to drop this serious approach, however, in fluid dynamics, and simply speculate. You may well think me unreasonable to criticize Sir Isaac, bearing in mind that he was breaking completely new ground in everything he did, and in particular in his studies of fluid dynamics. Nevertheless, one does observe some falling off of quality both in the reasoning and in its relation to observation when one plows onward to the part of Book 2 of the *Principia* which treats of the dynamics of fluid media. Two theories are given, of which we can at least say that neither was worthy of being enshrined forever in a book destined to be treated as the scientific equivalent of holy writ. The dense-medium theory rested on the unspoken, undefended, erroneous assumption that fluid motions can be superposed linearly, and predicts correspondingly unrealistic flows. The better-known rarefied-medium theory is put forward in a properly tentative manner, but nevertheless it was treated with exaggerated reverence (as if it were a physical theory of real fluids) for over two centuries, and has recently been resuscitated.

Similar criticisms, couched in the vein of comparing work in this field with work by the same authors in other fields, can be made of the approaches of Euler, Lagrange, Cauchy, and Kelvin to theoretical fluid dynamics. The mathematical reasoning was on a notably high level, but the extent of contact with real fluids was kept to a minimum, and direct attempts at comparison with experiment to a large extent avoided. One reason for this was that the fluid which really interested many of these authors was the aether rather than water or air. Somehow the aether *was* a serious subject, and to speculate on whether vortex motions in it might be the reality underlying atoms was a noble study, unattended by the humdrum observations made necessary in a study of vortices in air.

Alongside all this mathematical abstraction but totally separate from it was a whole volume of empirical fact, amassed by the ballisticians and the hydraulic engineers, and this included already in the seventeen-forties drag values for spheres up to Mach 2, for instance. Obviously the yawning gap between mathematical abstraction and engineering empiricism was most marked of all in this matter of air drag, and certainly it was practically the only point of disagreement of theory and experiment which was seriously taken notice of by mathematicians in the nineteenth century. Writers at that time compared what they called the "older" and "newer" theories, namely the Newtonian and potential-flow theories, and came to the conclusion that the Newtonian must be preferred because it avoided the d'Alembert paradox. This ignored the fact that the fluid motion which it predicted differed fundamentally from observation. It also missed the point that d'Alembert's result might make potential flow a highly desirable end to aim at, since approximations to potential flow might approximate to zero drag, a fact which later made high-speed aeronautics possible.

Looking back in the light of our knowledge that viscous forces, though small, play a crucial part in determining the flow about solid bodies, it is worth looking closely at the nineteenth-century work on viscous flow to see how this point was missed. A certain part of this work must be admitted to have been of outstanding value, notably the experiments on resistance in capillary tubes by Poiseuille and Hager, stimulated by physiology, and Stokes's great paper of 1851 on the effect of viscosity on the motion of pendulums, stimulated by the problems of gravity surveying with pendulums in the rather low vacuum conditions which could then be achieved. All these pieces of work obtained excellent agreement with experiment using the condition of zero relative velocity of fluid at a solid surface, which Stokes showed was also to be expected on physical grounds. Now potential flows do satisfy the equations of motion of a viscous flow although not this boundary condition. Nevertheless, there were no explicit conjectures during the next fifty years that the use of a wrong boundary condition in potential flow theory was the real cause of its deficiencies.

In the meantime, all treatments of the viscous-flow equations continued to neglect the nonlinear inertia terms. This neglect had been just permissible for Stokes's pendulums in air of low density, but the linearized viscous equations were advocated by many well-known mathematicians of this period for flows at high Reynolds numbers on the simple grounds the drag at least isn't zero.

In this field of external aerodynamics, even the appearance of Prandtl's great paper of 1904, which contained all the essentials of the solution to the mystery, was by no means immediately effective in increasing understanding of the physics of the subject among the abstract mathematicians or the empirical engineers. Books do still appear which ascribe flow separation to causes which really are those underlying cavitation! Nevertheless, by the end of the twenties there had been enough university centers of instruction in the physics of fluid motion, notably Göttingen and Caltech, and enough handbooks of the subject like those of Durand, Goldstein, and the German compilations, to have created a recognized and adequately manned discipline which comprehended both theory and experiment.

I could also mention some other examples to emphasize that it was not only *external* aerodynamics which was slow to be integrated within physics. In the general area of vibrations and waves the intimate binding together of theory and experiment was made rather earlier, largely by Rayleigh, whose *Theory of Sound* is, justly, still one of the best-sellers of science. It is interesting, though, that not till well on into this century was it discovered that the mathematical theory of the attenuation of sound by viscosity and heat conduction gave results orders of magnitude too low, actually owing to the time lag in molecular vibrational energy exchange, and it was about the same time that the physical nature of a shock wave first became clear, while the application of surface wave theory to experimental oceanography had to wait for the nineteen-forties.

Nevertheless, at the time when this Fluid Dynamics Division was formed, a clearly defined science had grown up on sound physical premises, with a wide range of applications in aeronautics, naval architecture, acoustics, meteorology, oceanography, mechanical engineering and chemical and explosives engineering. The basic physical premises were a continuum fluid and the simple laws of viscosity and heat conduction. Ideas derivative from these included boundary layers, vortices, turbulence, sound waves, Mach lines and shock waves, actuator disks, free and forced convection, deep and shallow water waves, phase and group velocity, secondary flow, geostrophic and thermal winds. The consistency of inclusion within physics of fluid dynamics at this period is well shown by the success with which Landau and Lifschitz were able soon after this time to include in their nine-volume course on theoretical physics a volume on fluid mechanics, which recently has been translated into English. Although it doesn't refer to experimental results much, the material has been carefully selected from those parts of the theory which do agree with experiment!

WELL, I think I've told half my story now, leaving fluid dynamics firmly installed as one of perhaps a dozen autonomous branches of physics. The second half of the theory is mainly a story of the nineteen-fifties, and deals with the gradually increasing loss of autonomy, and States' Rights and all that sort of thing, and progressive federation and merger with the other states or branches. Now, regarding this development, the views I want to put across are rather complicated, and comprehend much of the ambivalence and passion for checks and balances inherent in the American Constitution.

Let me first state in rather general terms the desirability of cross-fertilization between neighboring parts of science. Why does the Queen Bee fly forth, soaring upwards and flying higher and higher with a wild hum, and why do the drones from all the neighboring hives respond to the signal by ecstatically giving chase, in the urgent competition to catch her first though death may be the consequence? . . . It is because the biological advantages of maximum cross-fertilizing outweigh the inconvenience of aeronautical copulation.

Let me next become more specific about cross-fertilization, and enumerate some of the areas where it has become essential to go beyond the concept of a fluid continuum with a distinctly limited number of properties like viscosity and so on which I outlined earlier.

There is, first, the fascinating study of the dynamics of supercooled vapors and superheated liquids, that is, of condensation and cavitation phenomena, where it is essential to take into account matters such as the spontaneous generation of condensation nuclei, the dependence of surface energy on curvature, and similar matters. Pressing on rapidly, there is next the intriguing appearance of flow patterns in the interstellar gas made visible for us by their own hydrogen emissions, and absorptions, and these flows are undoubtedly affected by the presence of strong young ionizing sources in the form of O stars whose influence propagates outward in the form of a radiation front.

Next, we know that liquid helium below its lambda point exhibits a fluid dynamics considerably different from those to which we are accustomed, and a quantum fluid dynamics has been worked out which indicates that in certain circumstances a proportion of the fluid mass is capable of motion but unable to carry entropy or vorticity. This discovery explained a whole range of phenomena, but there are others, beyond, where the low-temperature physicists are now postulating a peculiar kind of turbulence in the superfluid, and I must say I feel happy to know that C. C. Lin is keeping a watchful eye on their activities!

Next, there are the pinches! Well, perhaps the less said about them the better. Then, there are a whole range of problems of upper-atmosphere aerodynamics, which raises questions of accommodation coefficients and other gas-solid interaction problems, and *may* be particularly interesting around 130 km where the gas pressure and magnetic pressure are equal. Interesting questions of propagation of man-made disturbances could well arise at these altitudes, while higher up we have the far more spectacular disturbances due to natural causes.

Then there's the whole field of what for want of a better word one may call Kantrowitzana! And one must admit that Arky has personally negotiated whole treaties of alliance between some of the different independent areas of physics, marshaling them together in aid of exotic engineering proposals, like his recent one in the field of power generation.

Passing to another magnetic problem there is that of the origin of magnetic fields, such as the earth's. Here the nature of the observed secular variation is a hint that eddies in the liquid core may bear some responsibility, and the work of Elsasser and more recently Bullard has brilliantly shown that they could in principle be responsible for the whole thing.

Well, now, too much further detail would be inappropriate, and it is well enough known, in any case, how amply justified by results have been all these and similar exercises in the combination of fluid dynamical knowledge with physical chemistry, with astrophysics, with quantum mechanics, with nuclear physics, electromagnetism and plasma physics, and so on and so forth.

Nevertheless! . . . yes, you see there's an antithesis coming; the half page of notes, which constituted the first version of my speech, said at this point, "End with a caution against excessive abandonment to the new attitudes of fluid dynamics!"

After some reflection, since writing that note, I think I could put this point a little differently, and say that, from many of the deepest studies that one can make by applying the whole armory of physics to the problems of movement of fluids, one of the most striking conclusions that in a very wide range of cases can be drawn is the simple one, that to an excellent approximation the fluid behaves as a continuum subject to a few simple laws of behavior! And I add to this remark the supplementary one that in the vast majority of these cases a prerequisite absolutely essential before going on from this point to get any further worthwhile information is *to* view the problem from this continuum mechanics approach.

I can give a quick illustration of this point best by a simple challenge. I challenge statistical physicists to demonstrate to me that an assemblage of the usual large number of molecules, which I permit those taking up the challenge to regard in their usual endearing manner as point centers of force, is capable of propagating small-amplitude sound waves which are nondispersive at frequencies small compared with the collision frequency, a condition of the demonstration being that at no stage will a continuum representation of the fluid be permitted. The velocity of propagation should come out as the square root of five-thirds RT. But will they get this, or any other detailed information about distribution of macroscopic properties, without the invaluable intermediate stage of representation by the good old differential equations of continuum mechanics?

Time is short, Mr. Chairman, but I must guard against possible misrepresentation of the point I have just made. I am not speaking against the most careful complete and exact investigation of the fundamental physical background and basis of the branch of fluid dynamics which one is studying, with a view to determining what the relevant differential equations really are. I am not saying that at all, and I am a firm believer in making such an investigation with every possible care and attention to detail. What I am saying is that the conclusion of such an investigation should be the determination of a properly substantiated continuum model—at least if one hopes to go any further to get some really clear-cut results. Examples of the success of this particular approach abound in recent years and need not be enumerated here.

I will add one more point. The excitement inherent in these borderline studies with other branches of physics need not in any way whatsoever blind us to the continued interest to those parts of fluid dynamics that do not lie near any such borderline. This symposium has reminded us of the continued fascination and inexhaustibility of the study of water waves, and of low-speed aerodynamics. I was interested to find that at the wind tunnels of the National Aeronautics and Space Administration, as well as at those of the Royal Aircraft Establishment, it is the low-speed ones, the good old low-Mach-number wind tunnels, that are absolutely crammed with work! The explanation is a very simple one. It was always a tricky subject. From the time of the Wright Brothers it was a full forty years before it was discovered how to design aeroplanes really efficiently for a purely low-Mach-number flight plan, and during that period aerodynamicists were able to bring this off only by concentrating on that single problem. In the subsequent seventeen years or so an enormous range of other kinds of flying machine have been investigated which don't spend the whole of their flight plan in the low-Mach-number range—but almost all have posed new problems of low-speed aerodynamics as well as the problems of high-speed flight on which their designers have tended to concentrate; and as shapes become over the years more and more recherché, the old low-speed aerodynamics has been found more and more inadequate and now we are simply having to start over again to deal with all the exotic types of take-off and landing that are planned. This, then, is one of the many classical fields of fluid dynamics where new ideas are now at a premium.

I could go on much longer, but I will just conclude by thanking you again for inviting me here, and by summing up the argument of my lecture in a single sentence: It needs categorically to be reaffirmed that the continuum mechanics of a fluid innocent of electric current has as vital and exciting a present and future as any other branch of physical science.