Sydney Goldstein

FLUID MECHANICS IN THE FIRST HALF OF THIS CENTURY

BY SYDNEY GOLDSTEIN
Harvard University, Cambridge, Massachusetts

THE FIRST DECADE

The opening decade of the twentieth century was a period of progress in fluid mechanics. Much was begun that was to continue for many years. Most of the great discoverers in the latter part of the previous century were to be with us still during the decade, and other younger men were to come forward. Let us start by listing some of the happenings.

In 1900 and 1901 Bénard published descriptions of his experiments on the convective motion of a fluid heated from below. In 1901 Levi-Civita published his first note on the explanation of the resistance of a solid body held in a stream of fluid by postulating the existence of surfaces of discontinuity of velocity, even when the body is of rounded form, without sharp edges. A paper on lifting forces in streams of fluid, by Kutta, appeared in 1902; this contained a solution for the two-dimensional flow of an inviscid fluid past a solid surface in the shape of a circular arc, at zero incidence, with circulation round the surface and a finite velocity at the trailing edge.

Sir George Gabriel Stokes, who was born in 1819, was chosen as Master of Pembroke College, Cambridge in 1902, and died on February 1, 1903.

Chaplygin's 1902 doctoral dissertation on gas jets, with the hodograph transformation of the equations of steady two-dimensional gas flows, and the application to jets, was published in 1904.

In 1904 Prandtl read his paper "Über Flüssigkeitsbewegung bei sehr kleiner Reibung" to the Third International Congress of Mathematicians at Heidelberg. In the same year Lord Kelvin published three papers on "Waves on water" (with one more in each of the years 1905 and 1906). The classical papers of Sommerfeld and Michell on the hydrodynamical theory of lubrication appeared in 1904 and 1905, and Ekman's paper "On the influence of the earth's rotation on ocean currents" in 1905.

Zhukovskii's famous lift theorem, connecting the lift force with the circulation quite generally for the two-dimensional flow of an inviscid fluid, was published in two notes in 1906, one in Russian and one in French. In 1907 and 1908 the works of Orr and Sommerfeld on the stability of fluid motions appeared. Lanchester's book *Aerodynamics* was published in London in 1907, and a German edition followed in 1909. Near the beginning of 1910 Chaplygin formally enunciated the postulate that out of the infinite number of theoretically possible flows (depending on the magnitude of the circulation) past an airfoil profile with a sharp trailing edge, the flow that is nearest to experiment is the one with a finite velocity at the trailing edge. Also near the beginning of 1910 Kutta finally published his 1902 dissertation, with revisions, and toward the end of 1910 Zhukovskii published a note on the design of airfoil sections, largely devoted to a graphical construction of the profiles.

In 1910 also Rayleigh and Taylor showed clearly how dissipative processes removed infinite gradients but left sharp transitions in shock waves in gases. Controversy, mystification, and hesitation were ended. In the same year Oseen put forward a "cure" for the "paradoxes" of Stokes and Whitehead.

Each professional worker and scholar may find fault with the items I have chosen for my list, but none will fail to recognize the meaning and significance of those I have included. None can deny that it was a fruitful period. It is an interesting game to try to guess now what a similar list written in 2018 would contain for the happenings of the first eleven years of the second half of this century, 1950–1960. But perhaps it is an even more interesting—and certainly more instructive—exercise to ruminate on what the list for 1900 to 1910 would have contained if compiled fifty years ago, in 1918. Much—very much—would have been missing, for not only was there a grievous slowness of communication, but much of what is listed was so new and unfamiliar that it escaped attention or merely induced disbelief or doubt, except in the cases of rare individuals. It was to be a few more years after 1918—when the first world war was just ending—before the new discoveries were to begin to have their full effect.

For the sake of orientation let us take a brief glance ahead from the end of 1910. It was in 1911, with further publications in 1912, that Kármán published his first paper on the vortex street in the wake for two-dimensional flow past a cylinder and the mechanism of resistance. [Bénard had shown evidence of the vortex street in 1908; there had been mention of such vortices before that by Mallock in 1907, by Ahlborn in 1902 and Marey in 1901 (with photographs), and by Reynolds in 1883 (with drawings). Mallock published a second paper in 1910.] Osborne Reynolds lived until 1912, Rayleigh until 1919, and Zhukovskii until 1921.

Rayleigh's Review of Lamb's *Hydrodynamics*—General Remarks

The third edition of Lamb's *Hydrodynamics*, of which the first edition appeared in 1879 and the second in 1895, was published in 1906 and the fourth edition in 1916. Rayleigh reviewed the fourth edition for *Nature*, and his review tells much of the state of the science and is worth quotation. Some excerpts follow.

> That this work should have already reached a fourth edition speaks well for the study of mathematical physics. By far the greater part of it is entirely beyond the range of the books available a generation ago. And the improvement in the style is as conspicuous as the extension of the matter. My thoughts naturally go back to the books in current use at Cambridge in the early sixties. With rare exceptions, such

as the notable one of Salmon's *Conic Sections* and one or two of Boole's books, they were arid in the extreme, with scarcely a reference to the history of the subject treated, or an indication to the reader of how he might pursue his study of it . . .

The progressive development of his subject is often an embarrassment to the writer of a text-book. Prof. Lamb remarks that his "work has less pretensions than ever to be regarded as a complete account of the science with which it deals. The subject has of late attracted increased attention in various countries, and it has become correspondingly difficult to do justice to the growing literature. Some memoirs deal chiefly with questions of mathematical method and so fall outside the scope of this book; others though physically important hardly admit of a condensed analysis; others, again, owing to the multiplicity of publications, may unfortunately have been overlooked. And there is, I am afraid, the inevitable personal equation of the author, which leads him to take a greater interest in some branches of the subject than in others."

Most readers will be of opinion that the author has held the balance fairly. Formal proofs of "existence theorems" are excluded. Some of these, though demanded by the upholders of mathematical rigour, tell us only what we knew before, as Kelvin used to say. Take, for example, the existence of a possible stationary temperature within a solid when the temperature at the surface is arbitrarily given. A physicist feels that nothing can make this any clearer or more certain. What is strange is that there should be so wide a gap between his intuition and the lines of argument necessary to satisfy the pure mathematician. . . .

Naturally a good deal of space is devoted to the motion of a liquid devoid of rotation and to the reaction upon immersed solids. When the solids are "fair" shaped, this theory gives a reasonable approximation to what actually occurs; but when a real liquid flows past projecting angles the motion is entirely different, and unfortunately this is the case of greatest practical importance. The author, following Helmholtz, lays stress upon the negative pressure demanded at sharp corners in order to maintain what may be called the electric character of flow. This explanation may be adequate in some cases; but it is now well known that liquids are capable of sustaining negative pressures of several atmospheres. How too does the explanation apply to gases, which form jets under quite low pressure differences? It seems probable that viscosity must be appealed to. This is a matter which much needs further elucidation. It is one on which Kelvin and Stokes held strongly divergent views. . . .

It would not have accorded with the author's scheme to go into detail upon experimental matters, but one feels that there is room for a supplementary volume which should have regard more especially to the practical side of the subject. Perhaps the time for this has not yet come. During the last few years much work has been done in connexion with artificial flight. We may hope that before long this may be coordinated and brought into closer relation with theoretical hydrodynamics. In the meantime one can hardly deny that much of the latter science is out of touch with reality.

Rayleigh's review is a remarkably interesting review and short essay. In conjunction with my 1900 to 1910 list and the remark, written with the benefit of hindsight, that much in the list was so new and unfamiliar that it escaped attention or merely induced disbelief or doubt, it gives an illuminating picture of the general state of affairs in the science in 1916.

Lamb's textbook was the predominant "high-class" textbook for many years, certainly in England in the 1920's for students of applied mathematics, but we were not really happy with it. Rayleigh may have contrasted it with the "arid" textbooks of the 1860's, but in the 1920's we were complaining that it was impossible to remember while reading Lamb that water is wet. Something of the same dry atmosphere still persists.

By the end of the first half-century there was a stronger and more widespread element of physics in thought and research on fluid mechanics than in the first twenty or thirty years, and this is much more so by now. Several factors and several research workers contributed to this, but the greatest influence has been the example of G. I. Taylor.

Fluid mechanics is a part of applied mathematics, of physics, of many branches of engineering, certainly civil, mechanical, chemical, and aeronautical engineering, and of naval architecture and geophysics, with astrophysics and biological and physiological fluid dynamics to be added. Significant contributions to the theory of airfoils came early in the century, and during the whole of the first half of the century applied aerodynamics was to be probably the major incentive, dealing with questions that were important also in mechanical and civil engineering; but geophysical questions, certainly not without charm and fascination, received much attention.

Curiosity about at least two of the branches of fluid mechanics and their applications has a long and distinguished history, for in the Proverbs of Solomon the son of David, king of Israel, it was stated in the words of Agur the son of Jakeh that "There be three things which are too wonderful for me, yea four which I know not," of which two were "The way of an eagle in the air" and "The way of a ship in the midst of the sea," which I take to be questions of aerodynamics and naval architecture, questions that concern us still.

There is now no dearth of books, some very good and many satisfactory, on the various branches of our science, in which the details of the work done up to 1950 may be read. So I propose not to attempt to be systematic or even to discuss several significant advances, but to plead, in Lamb's words, "the inevitable personal equation of the author, which leads him to take a greater interest in some branches of the subject than in others." Let us go back to 1900.

The Resistance of Fluids. The Surfaces-of-Discontinuity Theory

Probably the outstanding difficulty was still that of accounting for the resistance of a solid body in motion relative to a fluid in which it is immersed. The three known theories were not satisfactory, though there was still considerable difference of opinion about the last of them. According to the ideas of Isaac Newton, the force on a flat plate in a two-dimensional motion would be wholly normal to the plate, and proportional to the square of the sine of the angle of incidence, i.e., the angle between the relative velocity of fluid and solid and the trace of the plate. Later it was held that in an inviscid liquid, because of the theorems of Lagrange and Kelvin, a motion started from rest would be irrotational, and then there would be no force on an isolated body moving at a sufficient distance from any boundaries when the motion is steady, the influence of the motion of the fluid being completely allowed for by a modification of the inertia of the solid. The next theory involved surfaces of discontinuity of tangential velocity beginning at sharp corners, and in particular from the edges of a flat plate in a stream. Lamb writes (pp. 641, 642 of the fifth edition of *Hydrodynamics*, 1924):

> The absence of resistance, properly so called, in such cases is often referred to by continental writers as the 'paradox of d'Alembert.'
> on exact theoretical lines, a result less opposed to ordinary experience is contained in the investigations of Kirchhoff and Rayleigh relating to the two-dimensional form of the problem of the motion of a plain lamina. It is to be noticed that the motion of a fluid in such problems is no longer strictly irrotational, a surface of discontinuity being equivalent to a vortex-sheet.

Rayleigh's 1876 paper "On the Resistance of Fluids" begins by observing that "there is no part of hydrodynamics more perplexing to the student than that which treats of the resistance of fluids." Lamb's paragraph on the "Resistance of Fluids" begins by observing that:

> This subject is important in relation to many practical questions, *e.g.* the propulsion of ships, the flight of projectiles, and the effect of wind on structures. Although it has recently been studied with renewed energy, owing to its bearing on the problems of artificial flight, our knowledge of it is still mainly empirical.

"Resistance" was a term used for the total force, not the drag force, which was "the resolved part of the resistance in the direction of the stream." It is interesting that Rayleigh compared, in part, the results of the theory for a flat plate at angles of incidence between 10° and 90° with measurements by Vince published in the *Philosophical Transactions of the Royal Society* in 1798. Vince had measured the "drag," and Rayleigh divided the measured results by the sine of the angle of incidence to obtain the "resistance," and compared the ratios of the resistance at an angle of incidence of 90° to the resistance at other angles (α) down to 10°. He then remarked:

> The result of Vince's experiments agrees with theory remarkably well and the contrast with $\sin^2 \alpha$ is especially worthy of note. The experiments were made with a whirling machine and appear to have been carefully conducted; but they were on too small a scale to be quite satisfactory. The subject might now be resumed with advantage.

However, the theory was by no means winning complete acceptance. In particular Sir William Thomson, later Lord Kelvin, was quite unconvinced. Rayleigh wrote:

> It was observed by Sir William Thomson in Glasgow that motions involving a

> surface of separation are unstable. This is no doubt the case. . . . But it may be doubted whether the calculations of resistance are materially affected by this circumstance as the pressures experienced must be nearly independent of what happens at some distance in the rear of the obstacle, where the instability would first begin to manifest itself. . . . The formulae proposed in the present paper are also liable to a certain amount of modification from friction which it would be difficult to estimate beforehand, but which cannot be very considerable if the experiments of Vince are to be at all relied on.

Kelvin seems to have been more and more unconvinced. In 1894 he published four notes on the question of resistance in *Nature*, which are reproduced in Volume 4 of his *Mathematical and Physical Papers*, with a note by the editor, Sir Joseph Larmor, that "These communications formed the subject of a prolonged playful controversy between Lord Kelvin and his intimate friend Sir George Stokes, in a series of letters which have been preserved." Kelvin showed that the results of the surfaces-of-discontinuity theory for a flat plate were not in agreement with the experiments of Dines, published in the *Proceedings of the Royal Society* in 1890. The editor of the *Mathematical and Physical Papers* enquired from the Director of the National Physical Laboratory, Dr. T. E. Stanton, about the reliability of Dines's results and about the results of more recent investigations, before republishing Kelvin's papers in 1910. Stanton wrote that, when allowance was made for differences in size of plates, Dines's results compared well with more recent measurements at the National Physical Laboratory, and that the excess in the total resistance over that given by Lord Rayleigh's formula was due to the suction effect of the eddies on the leeward side.

Levi-Civita's first note on the extension of the surfaces-of-discontinuity theory to flows past solid bodies with curved boundaries, for which we note that negative pressures need not enter, was published in 1901, and the general mathematical theory followed in 1906. He sought to justify the mathematical investigation by the hope of practical comparisons, for example to ships, but also by comparison with published experimental results. It is interesting that the observations to which he referred (by Marey and by Ahlborn) are among those which have already been mentioned as showing evidence of vortices in the wake. Levi-Civita in fact remarked that in the actual wake there are vortical and turbulent motions, and that at a certain distance from the body the state of motion of the fluid no longer presents any vestige of discontinuity, but argued that the resistance depends only on the state of motion in the near part of the wake in contact with the body, and that this state of motion is perceptibly the same in both the actual wake and his hypothetical one, so the value of the resistance resulting from his calculations should closely approximate the correct value, in spite of the differences between the actual circumstances and the theoretical model. (This was essentially Rayleigh's 1876 argument, but Levi-Civita's discussion is longer and more detailed.) I shall not discuss the applications and further development of Levi-Civita's method by Cisotti and Villat, and later by others, except to remark that the mathematics was of both considerable analytical difficulty and interest, and also that for a curved boundary the solution is not unique until the positions of separation of the "free streamlines"—the traces of the surfaces of discontinuity—have been determined, and that for this there is only one clue, namely, that it may be required that the curvature of the streamlines at the separation points should be finite, since for general positions of these points this curvature is infinite. When it is finite, it is equal to the curvature of the section of the solid boundary.

There was, then, in the first part of this century some confusion about the validity of the surfaces-of-discontinuity theory for the approximate calculation of resistance. Whatever else it did, however, it introduced the notion of attempting to bring the results of the potential theory of the motion of inviscid fluids into better agreement with observation in fluids of small viscosity by the introduction of discontinuities in the velocity field, in this case vortex-sheets, which was to prove a valuable idealization in spite of instabilities. Of course it exercised also considerable mathematical fascination, and, in England at any rate, it was always learned rather extensively by students of applied mathematics. I do not know how much time, if any, is spent on it now. There is still much to be said for teaching it, but if that is done I hope it is done now, as it was not done earlier, with a full description of the actual circumstances and discussions of its relations to boundary-layer separation and to the instability of vortex-sheets and layers.

Boundary Conditions. Slip

I suppose that it is still correct that for practical purposes in most situations our quantitative knowledge of resistance is mainly empirical. However, much more is understood now of the underlying physical processes, and we may ask what was the main cause of the difficulties and confusion. Certainly it did not lie in any lack of intellectual ability of the very distinguished scientists who wrestled with the problems from Newton to Stokes and Rayleigh. The real trouble was doubt about the boundary conditions to be applied at the dividing surface between a solid and a liquid or gas. In the theory of the irrotational motion of an inviscid fluid, the relative normal velocity at the surface of an impermeable solid must be zero, and no other boundary condition is required or can be imposed. For the motion of a viscous fluid, on the other hand, according to the dynamical differential equations published during the period 1822 to 1845 (Navier, 1822; Poisson, 1829; Saint-Venant, 1843; Stokes, 1845), another boundary condition is required for a solution. There was doubt and vacillation for a long time. On the whole, one thing seems to have been agreed: that there is no slip, i.e., no relative tangential velocity, at the surface of a solid body in the case of a very slow motion in a viscous fluid; but all else was in doubt. A note on the hypotheses and beliefs concerning the conditions at the surface between a fluid and a solid body is printed as an appendix at the end of the second volume of *Modern Developments in Fluid Dynamics*, and what was written there will not be repeated. The discrepancies between the actual motions of a real fluid of small viscosity, when laminar, and the results calculated for the irrotational motion of an inviscid fluid arise mainly, in most cases, from the condition in a real fluid of no slip at a boundary. If a fluid could slip freely over the surface of a solid body it would be a very different world. Those, among them Lamb and Levi-Civita, who have asserted in the past that viscosity cannot be considered a predominant cause of direct resistance, were correct in this sense in most ordinary circumstances.

In his 1904 lecture to the International Congress of Mathematicians Prandtl stated briefly but definitely that by far the most important question in the problem (of the flow of a fluid of small viscosity past a solid body) is the behavior of the fluid at the walls of the solid body. He continued (the original German has been translated):

> The physical processes in the boundary layer (Grenzschicht) between fluid and solid body can be calculated in a sufficiently satisfactory way if it is assumed that the fluid adheres to the walls, so that the total velocity there is zero—or equal to the velocity of the body. If the viscosity is very small and the path of the fluid along the wall not too long, the velocity will have again its usual value very near to the wall. In the thin transition layer (Übergangsschicht) the sharp changes of velocity, in spite of the small viscosity coefficient, produce noticeable effects.

In 1912, in the book on the theoretical bases of aeronautics in which his lectures were reproduced, Zhukovskii pointed out that for an incompressible fluid with a uniform coefficient of viscosity the terms in the differential equations of motion containing the coefficient of viscosity disappear for potential motions, so the influence of viscosity when a potential of the velocity exists can appear only at the walls, where the boundary conditions must be satisfied. He then remarked, as late as 1912, that there are differing opinions on the behavior of the flowing fluid at the walls; some investigators thought there was no motion of the fluid along the walls, while others supposed that the fluid slips along them. For his part he thought (there was no exact experimental confirmation, but it approximated nearly enough to reality) that the fluid velocity is zero at the walls and rapidly increases until it becomes equal to the theoretical velocity, the layer of fluid around the walls being vortical and also very thin.

At the end of the nineteenth century the most satisfactory theoretical progress, other than mathematical, had been made in cases where the boundary conditions at a solid boundary did not greatly affect the results, as for waves on water, or for the one case where the condition of zero slip was accepted, slow motion in a viscous fluid, on the theory of Stokes, for a sphere and also for an ellipsoid, and certain problems of small oscillations. For waves on water, mention may be made of the effect of a local disturbance (Poisson, 1816; Cauchy, 1827; Kelvin, 1887), the theory of group velocity

(Stokes in an examination question, *Smith's Prize Examination*, 1876; Rayleigh, 1881; Osborne Reynolds, 1877), and the effect of capillarity and the minimum wave-velocity (Kelvin, 1871). On this subject much of the later progress has been made somewhat recently.

Rotating Fluids

The results of the theory of flows of inviscid fluids without vorticity were widely at variance with observation and practice over most of the important parts of the velocity field for flow past bluff bodies, because the condition of zero slip at a solid surface was not satisfied. However, the theory was to prove its worth for fair-shaped streamlined bodies such as airfoils, nacelles, and airships, though "improvements" were to be desirable for practical applications after the first calculations. But that is another story, which will be mentioned later. If we assume the existence of vorticity in a diffused form and thereafter neglect viscosity, the motion of a solid body in a fluid possessing such vorticity is amenable to calculation only in a few cases. One such case is motion in a uniformly rotating fluid. In a series of papers on this subject between 1917 and 1923, G. I. Taylor made certain theoretical predictions and reported certain experiments of great interest and importance, not only for themselves and their applications but because predictions were made that either did not depend on the boundary conditions or that gave no slip at a boundary. These predictions were verified by experiment. In his paper on "Experiments with Rotating Fluids" in 1921 G. I. Taylor wrote:

> It is well known that predictions about fluid motion based on the classic hydrodynamical theory are seldom verified in experiments performed with actual fluids. The explanation of this want of agreement between theory and experiment is to be found chiefly in the conditions at the surfaces of the solid boundaries of the fluid.
>
> The classical hydrodynamical theory assumes that perfect slipping takes place, whereas in actual fluids the surface layers of the fluid are churned up into eddies. In the case of motions which depend on the conditions at the surface, therefore, no agreement is to be expected between theory and experiment. This class of fluid motion, unfortunately, includes all cases where a solid moves through a fluid which is otherwise at rest.
>
> On the other hand, there are types of fluid motion which only depend to a secondary extent on the slip at the boundaries. For this reason theoretical predictions about waves and tides, or about the motion of vortex rings, are in much better agreement with observation than predictions about the motion of solids in fluids. Some time ago the present writer made certain predictions about the motion of solids in rotating fluids, or rather about the differences which might be expected between the motion of solids in a rotating fluid and those in a fluid at rest. The predicted features of the motion did not depend on conditions at the boundaries. It was therefore to be anticipated that they might be verified by experiment. The experiments were carried out and the predictions were completely verified.
>
> In view of the interest which attaches to any experimental verification of the-

oretical results in hydrodynamics, and more particularly to verifications of those concerning the motion of solids in fluids, it seems worth while to publish photographs showing the experiments in progress. In the second and third part of this paper further experiments are described in which theoretical predictions are verified in experiments with water.

The same paper contained a proof, by the consideration of the circulation round a circuit, that "if any small motion is communicated to a fluid which is initially rotating steadily like a solid body, the resulting flow must be two-dimensional, though small oscillations about this state of slow motion are possible," with a footnote that "this is practically the same thing as the fact previously noted by Proudman, that small steady motions of a rotating fluid are two-dimensional." The paper closes with the remark that "In a future paper the author hopes to discuss what happens in the case when the boundaries of the fluid move slowly in such a way that three-dimensional motion must take place."

More, then, was to follow from Taylor himself, and much, much more from many others. Motion in rotating fluids, partly but not entirely for geophysical—meteorological and oceanographic—applications, has become a large branch of the science of fluid dynamics. The experimental results predicted and observed by Taylor are striking in the extreme, but they are now adequately described elsewhere, together with further developments. This subject must be abandoned here for a return to matters previously introduced.

Boundary Layers, Boundary-Layer Separation, Vortex Generation

Prandtl's remarks, in his 1904 lecture to the International Congress of Mathematicians, on the boundary condition of no slip and the circumstances in a thin layer of fluid near a solid wall have already been cited. The lecture was reprinted in 1927 in a book *Vier Abhandlungen zur Hydrodynamik und Aerodynamik*, together with Prandtl's two classic papers of 1918 and 1919 on airfoil theory and Betz's paper of 1919 on the screw propeller with minimum energy loss. In this book the paper on the motion of a fluid of very small viscosity occupies less than eight pages. Much of the lecture was devoted to showing to the asssembled mathematicians qualitative diagrams of streamlines and separations for flows past a projecting corner and a circular cylinder, with the rolling up of vortex-sheets, and then experimentally obtained photographs of actual flows in similar cases, with boundary-layer separation and also with boundary-layer suction. Nevertheless, Prandtl found time to derive the result that in a region of closed streamlines where vorticity had been produced by the action of a very small viscosity for a very long time, the vorticity would be constant in a two-dimensional flow, and to announce that it would be proportional to the distance from the axis of symmetry for an axisymmetric flow; to give the boundary-layer equations for steady two-dimensional flow; to mention the difficulty for numerical computation arising from singularities at the wall; to state the form of the "similar" solution for flow along a flat plate, with a first rough computation of the drag; and to discuss boundary-layer separation in the presence of a pressure increasing in the stream direction, with a diagram which has been reproduced ever since. Prandtl wrote (the German has been translated):

> The most important result of the investigation for applications is that in definite cases the fluid flow will separate from the wall at a place completely determined by the external conditions. A layer of fluid, which has been set in rotation by the friction at the wall, makes its way into the free fluid where, causing a complete transformation in the motion, it plays the same part as a Helmholtz surface of discontinuity. When the viscosity coefficient k is altered the thickness of the vortex layer is altered (it is proportional to $\surd(kl/\rho u)$) but everything else remains unchanged, so that one can if one will go over to the limit $k=0$ and obtain always the same flow picture. . . .

Prandtl explained the plausible reason for separation with an unfavorable pressure gradient, and explained that the consideration of a flow must be dealt with in two interacting parts, an inviscid flow obeying Helmholtz's vortex theorems, and transition layers (boundary layers) at the solid boundaries in which the motion will be regulated by the free fluid but which give the free stream its character by the emission of vortex layers.

This was a most extraordinary paper of less than eight pages. In 1928 I asked Prandtl why he had kept it so short, and he replied that he had been given ten minutes for his lecture at the Congress and that, being still quite young, he had thought he could publish only what he had had time to say. The paper will certainly prove to be one of the most extraordinary papers of this century, and probably of many centuries. Of course, to a limited extent the existence and nature of a boundary layer and its connection with frictional drag had been briefly mentioned before (Rankine, 1864; Froude, 1874; Mendeleyev, 1880), but it had amounted to very little compared with Prandtl's contribution; there were no boundary-layer equations and no explanation of separation. The influence of Prandtl's boundary-layer theory has been enormous. It has been used to make clear physical phenomena that were, or would have been, otherwise baffling or at least murky. It formed the basis for approximate methods of computation of practical utility. The ideas were applied to sciences other than fluid dynamics and, in fluid dynamics, to situations other than those involving a small viscosity. After the conclusion of the half-century it was extended and generalized and transmuted, especially by Kaplun, Lagerstrom, and their co-workers, into the theory of singular perturbations for the approximate asymptotic solution of differential equations.

However, for some years after it was published Prandtl's lecture was almost, if not completely, unnoticed. Perhaps this is not surprising. It was so very short, and it was published where no one who was likely to appreciate it might be expected to look for it. In 1908 Blasius published in a more accessible, more conventional, medium of communication a fuller account of the derivation of the boundary-layer equations and a detailed investigation of the flow along a flat plate parallel to a stream, but even after that there was not exactly a rush of acceptance, exposition, or further investigation of boundary-layer theory. Blasius also began the study of the boundary layer at the surface of a circular cylinder started from rest. Boltze, in a Göttingen thesis in 1908, studied boundary layers at the surfaces of bodies of revolution, especially spheres, and in 1911 Hiemenz performed boundary-layer computations with an experimentally determined pressure distribution on a circular cylinder. These also drew little attention.

Lanchester's *Aerodynamics* had been published in 1907. In it, among much else, he found independently that the skin-friction drag would vary as $\nu^{1/2}U^{3/2}$, gave an explanation of separation less detailed than Prandtl's, gave indications that he knew about turbulence in a boundary layer, explained that "a stream-line body is one that in its motion through a fluid does not give rise to a surface of discontinuity," and expected separation to be delayed on one side and hastened on the other side of a rotating cylinder in a stream. This publication presumably drew even less attention.

With the publication in 1921 of Kármán's momentum equation and the Kármán-Pohlhausen approximate method of integration, and the publication in 1924 of the experiments of J. M. Burgers and van der Hegge Zijnen, boundary-layer theory at last became the subject of more attention and acceptance.

The single reference to Prandtl's boundary-layer theory in Lamb's *Hydrodynamics* in 1924 (5th ed.) is interesting. Lamb remarks that in flow past a cylinder

> the central stream-line divides where it meets the surface in front, and then follows the surface for some distance on each side, the motion of the fluid on either hand being fairly smooth and regular. At a certain stage, however, the stream-line in question appears to leave the surface, and can no longer be definitely traced, the space between its apparent continuation and the cylinder being filled with eddies. . . An able attempt to trace this phenomenon mathematically has been made by Prandtl. The region in front of the solid is regarded as made up of two portions, viz. (1) a thin stratum in contact with the solid, with a rapid variation of relative (tangential) velocity in the direction of the thickness, and (2) an outer region in which the motion is taken to be irrotational, being practically unaffected by viscosity. Approximate solutions of the equation of motion are sought, appropriate to these two regions, and continuous with one another at the common boundary. The calculations are necessarily elaborate, but the results, which are represented graphically, are interesting.

There are references to Prandtl's lecture and to Blasius.

The situation was altered appreciably in the sixth edition of *Hydrodynamics*, published in 1932. This contained a new section on boundary-layer theory, with references to Prandtl and Blasius, as before, and to Mises's treatment in 1927, and with Lamb's own contribution to the use of the Kármán-Pohlhausen approximate method.

In the late 1920's and early 1930's there was eagerness to use and study boundary-layer theory, so it was appropriate to print as the motto of *Modern Developments in Fluid Dynamics* an extract from the essays of Sir Francis Bacon (1612): "For when propositions are denied, there is an end of them, but if they bee allowed, it requireth a new worke."

With understanding of boundary-layer separation, coupled with knowledge that when a motion of a solid body is started from rest the fluid does its best to make the initial motion irrotational without circulation (with a vortex-sheet wrapped round the surface of the solid body), and also with some understanding of the rolling-up of vortex-sheets, the way was clear for considerable insight into much that was not clear before—the origin of a large part of the resistance, the process of vortex formation behind a bluff obstacle, and the origin of the circulation round a lifting airfoil.

Kármán pointed out that a theoretical determination of the velocity and spacing of the vortices in a vortex street would require an investigation of the process of vortex formation, and referred to Prandtl's theory of the motion of fluids with small viscosity for an explanation of the formation of vortices even in a fluid of vanishingly small viscosity.

In 1912 Zhukovskii had considered the formation of vortices. There was first the problem of the singularity in the solution at the leading edge of an airfoil of zero thickness, such as a circular-arc airfoil. Zhukovskii believed this would simply lead to a vortical thickening. Kutta had also thought a disturbance arising from the leading edge would not be important. More generally, there was agreement on the desirability of a rounded nose, and there does not appear to have been any widespread uneasiness. On the other hand, Zhukovskii pointed out that vortices could also separate from the trailing edge, and thought that the main cause of all resistances is to be found in such separations.

Meanwhile, in 1910, ideas about resistance had been clarified in reports by Kutta's teacher, Finsterwalder, and by Prandtl himself. Finsterwalder, in a lecture actually delivered the previous year, had discussed Prandtl's theory and mentioned the difficulties, but pointed out that from the theory it could be deduced that in the case of an airship it is not so much the form of the nose that matters as the shape of the tail. Prandtl introduced the idea of the drag being due to two causes, which are not independent of one another, giving rise to surface resistance (skin-friction) and vortex or form drag. (Cf. Stanton's reply to Larmor in connection with the publication of Kelvin's papers). Prandtl stated flatly that the form of the after part of a body has in many cases more importance for drag than the forward part. By suitably forming the after part the drag of an airship hull could be reduced, so that it came near to the theoretical value of zero. Prandtl continued that all theories that try to base drag on what happens in the front must lead to wrong results, and must be rejected. Moreover, he says, the greater or less turbulence of the air is of great importance for the values of the drag.

The concern with the design of airship hulls at that time is important. Also noteworthy is the reference to turbulence, which will be mentioned later.

Exact, or even rough, calculations, were not possible, and the whole process was not completely understood, but some time later the theory began to have considerable influence on design. In fact by the late 1920's and early 1930's "streamlining" was a fashionable word, even if rather completely misunderstood by nonprofessionals. Many things, no matter what their shape in relation to the technical meaning of the term, were referred to as streamlined—for example, fast motor cars, fast railroad trains, and, according to advertisers, agreeable young women at any speed if equipped with the right foundations. The slang use of the term died out, partly perhaps for sociological reasons, but partly perhaps because of the airship disasters of 1933 to 1937, but the art, science, and technology of streamlining in its technical meaning remained of primary importance, at any rate until science fiction started to become engineering fact.

The notion of a cast-off vortex, with a vortex being formed and detached in a short time when a (streamlined) airfoil at lift begins to move, so that a circulation round the airfoil in an opposite sense is left, gave physical substance to the circulation theory of lift. Many striking experiments have been made in fluid mechanics and aerodynamics, and this notion of the cast-off vortex led to one of them. I have never forgotten when William Farren showed me, on a small scale model in a tank with transparent walls, the cast-off vortices when an airfoil was started and then stopped impulsively and the vortex pair with opposite rotations appeared rapidly, and moved sedately downwards perpendicularly to the line joining them, all in accordance with theory. Seeing was certainly believing. I think that a description of the tank, etc., and the experiment apeared in Walker's thesis (1932).

Airfoil Theory

The circulation theory of lift was not widely accepted with any rapidity. In the early 1920's at least one distinguished aeronautical engineer was still expressing scepticism, which produced the experiment of Bryant & Williams (1925) at the National Physical Laboratory. This not only verified the existence of a circulation but confirmed the Kutta-Zhukovskii formula for the lift, even for a real fluid with the presence of a wake, if the contour around which the circulation is taken does not approach the airfoil too closely and cuts the trailing wake at right angles to the direction of the undisturbed relative motion. The problem was then not to explain the lift, but to explain why the formula was so nearly correct in the presence of a vortical wake, and this explanation was immediately provided by G. I. Taylor.

There are three ingredients in inviscid, incompressible, two-dimensional airfoil theory: the lift formula, the condition at the trailing edge, and the conformal mapping of the airfoil contour. As time went on, the second was improved by allowances for the boundary layers and the wake. Two-dimensional airfoil theory was extended by Mises and by Kármán & Trefftz among others. It is noteworthy that the theory of the complex variable was increasingly used for design (of course with the desired physical, mechanical, and geometrical criteria in mind), culminating in the work of Lighthill (1945). The method of singularities was also developed for application to airfoil sections.

Prandtl's two classical papers on three-dimensional airfoil theory were published in 1918 and 1919, leading to calculations for airfoils of large but finite aspect ratio. These papers had been several years in the making, the ideas dating back in part at least to 1910, with the first published reference in publications by Föppl in 1911. (Föppl also referred to Lanchester's *Aerodynamics*, in connection with a pair of trailing vortices which start from the wing tips and make possible, in simply-connected space, the transition from flow around the wing.) Prandtl's papers are classical, not only for aerodynamics, but as part of fluid dynamics generally. Moreover, I remember that when I first read them I formed the strong impression from the way they were written that Prandtl really knew he was writing classical papers.

Much research was going on both before and immediately after the publication of the two papers mentioned above, and anything like a full description is not possible here. The term "induced drag" appears to be due to Munk (1918), who also provided what is now known as "Munk's stagger theorem" (mentioned also in Prandtl's second paper) and an easier and more general proof (with generalizations) that wings with elliptic loading have the smallest possible induced drag. Betz's paper on the screw-propeller with least energy loss appeared in 1919. Trefftz's method of working in the "Trefftz plane" and also of using Fourier series appeared in 1921.

The first practical triumph of the theory was in making sense of experimental results on airfoils of various different aspect ratios.

The news of Prandtl's airfoil theory spread much more rapidly than the news of his boundary-layer theory. In 1921 the National Advisory Committee for Aeronautics in the U.S.A. requested and published a report by Prandtl himself on "Applications of modern hydrodynamics to aeronautics," and in the same year Pistolesi drew attention to the theory in a lecture and publication in Italy. The same author published a more complete exposition in the following year. Also in 1922 Roy published a booklet on the theory in France, and in 1923 the theory was explained, and used, by Glauert and by Low, and verified experimentally by Fage & Nixon, in England. A German textbook by Fuchs & Hopf appeared in 1922, and Glauert's text on *The Elements of Aerofoil and Airscrew Theory* appeared in England in 1926 and rapidly came into very general use.

An appraisal of the contributions of Lanchester and their influence would require at least a complete article. Durand, in his "Historical sketch of the development of aerodynamic theory," spoke of Lanchester's "remarkable physical insight," and of this there is no doubt. Lanchester read a paper to the Birmingham Natural History and Philosophical Society in 1894, sent a revised version of the paper to a Fellow for publication by the Royal Society about two years later, and was, rather curiously, advised to send it to the Physical Society, who rejected it in September, 1897. Apart from taking out a patent, Lanchester made no further effort at publication until *Aerodynamics* was published in 1907 and *Aerodonetics* in 1908. His further contributions to the theory of wings of finite span and airscrews were published in the *Proceedings of the Institution of Automobile Engineers* in 1915, and reprinted as a booklet. A German edition of *Aerodynamics* was published in 1909, and a French edition in 1914, but the publications of 1915 remained largely unknown in continental Europe for some time, and Prandtl stated that he did not become aware of them until 1926.

At a very early stage in each case Lanchester had the fundamental ideas of the circulation theory of lift and of trailing vortices behind an airfoil of finite span. Some attention was soon paid in Europe to the wing theories in *Aerodynamics*. In 1910 Zhukovskii remarked that Lanchester's great merit was in having illuminated the passage from an airfoil of infinite span, for which the field occupied by the fluid is doubly-connected, to the finite wing, for which the field is simply-connected. Föppl's reference in 1911 has already been mentioned. However, no attention was paid until much later to Lanchester's theories in his native England. His works contained but little mathematical development, even though he apologized in the preface to *Aerodynamics* to the nonmathematical reader, "who may find himself out of his depth," for the mathematics that was included. Later, in his Wilbur Wright lecture, he stated that his writings were in plain English, divested of all mathematical ornament, but in fact, plain English or not, they were by no means easy to understand. *Aerodynamics* should still be read, not only for its content but to savor Lanchester's style. Many words he used were his own coinage, and others were used with what is now an obsolete meaning, so a glossary is needed, and is in fact provided in the book. For example, the title of the 1894 paper to the Birmingham Natural History and Philosophical Society was "Stability of an aerodrome." Aerodrome was apparently Langley's word; it was used for a flying machine. The *Oxford Universal Dictionary* says that the word with the meaning of an aeroplane dates from 1891 and became obsolete in 1896, which seems rather hard on Lanchester, who used the word in that sense very firmly in *Aerodynamics* in 1907. In fact, in a footnote he wrote that "the word aerodrome has been grossly misapplied by Continental writers to denote a balloon shed." The *Random House College Dictionary* says that the word (airdrome in the United States) now means an airport; so the change of meaning in what was called the "stability of an aerodrome" must be noted. The contents of the early paper, and several diagrams from it, were reproduced in *Aerodynamics*.

Of course, eventually attention was paid to Lanchester's contributions in England, and he was invited by the Royal Aeronautical Society to deliver the Wilbur Wright Memorial Lecture in 1926, a year before Prandtl.

Prandtl's lecture, the beginning of which he read himself with the reading completed by Major Low, since Prandtl had trouble with the English language, was on "The generation of vortices in fluids of small viscosity" by the action of the boundary layer, not on airfoil theory, because, he said, he thought "it would be preferable to select another subject with which you are possibly less familiar in England, although its beginnings go further back than do those of the aerofoil theory." However, after referring to Froude and stating that "Lanchester developed an approximate theory for steady laminar flow" in connection with boundary layers, Prandtl spoke briefly on the history of airfoil theory. He said:

> In England you refer to it as "the Lanchester-Prandtl theory," and quite rightly so, because Lanchester obtained independently an important part of the results. He commenced working on the subject before I did, and this no doubt led people to believe that Lanchester's investigations, as set out in 1907 in his 'Aerodynamics,' led me to the ideas upon which the airofoil theory was based. But this was not the case. The necessary ideas upon which to build up that theory, so far as these ideas are comprised in Lanchester's book, had already occurred to me before I saw the book. In support of that statement, I should like to point out as a matter of fact we in Germany were better able to understand Lanchester's book when it appeared than you in England. English scientific men, indeed, have been reproached for the fact that they paid no attention to the theories expounded by their own countryman, whereas the Germans studied them closely and derived considerable benefit therefrom. The truth of the matter, however, is that Lanchester's treatment is difficult to follow, since it makes a very great demand on the reader's intuitive perceptions, and only because we had been working on similar lines were we able to grasp Lanchester's meaning at once. At the same time, however, I wish it to be distinctly understood that in many particular respects Lanchester worked on different lines than we did, lines which were new to us, and that we were therefore able to draw many useful ideas from his book. The volume published in 1915 . . . in which Lanchester comes to the same conclusion with regard to the induced drag as we did, was unknown to us until 1926. As it happens the same formula was published by us in 1914 (by Betz) . . . .

It remains to add that the vortex-sheet model of Lanchester and Prandtl formed also the starting point for calculations on "lifting-surface" theory, to which much attention was paid in the 1930's and 1940's and on which work is still proceeding. In 1936 Prandtl introduced the acceleration potential and reduced the downwash integral to a form in which it is taken over the wing surface only. Special planforms, in particular elliptic and circular planforms, were considered [by Kinner (1937), by Kochin (1940), and by Krienes (1940)], and perhaps more importantly, numerical methods came more and more into use.

Prandtl's own presentation was not completely free from intuitional insights, and it has lately been pointed out [e.g. in *Incompressible Aerodynamics*, edited by Thwaites (1960) and in Küchemann's "Ludwig Prandtl Memorial Lecture" (1967)] that what Prandtl himself called a lifting-line theory must be considered an approximate lifting-surface theory in which the chordwise distances involved are small compared with the spanwise distances. In such a case it can be proved that the chordwise loading is the same as in the two-dimensional case and the spanwise distribution of lift is given by Prandtl's lifting-line theory.

The time came when the influence of the compressibility of air at high subsonic speeds could not be ignored, and also theoretical considerations of supersonic flight were taken more and more seriously. Swept wings had to be considered. It appears that the first published suggestion of the use of swept wings was made by Betz in 1940. Slender aircraft became matters of serious consideration only considerably later, but a theory of pointed wings of very small aspect ratio—in contradistinction to the theory for large aspect ratios—was published by R. T. Jones in 1946. In all cases trailing vortices were still with us.

Stability

Among other matters exciting the curiosity and attention of investigators probably the most important were stability, turbulence, and gas dynamics.

Theoretical investigations of stability in the period dealt mainly with stability to infinitesimal disturbances on a linearized theory. Progress in considering finite disturbances was not to come until later.

In 1916 Rayleigh considered, as a question of stability, the convection currents in a horizontal layer of fluid heated below, in connection with observations of Bénard in 1900 and 1901, mentioned in the 1900 to 1910 list at the beginning of this article. Rayleigh's discussion is based on the approximate equations of Boussinesq. He remarked that M. Bénard did not appear to be acquainted with a paper by James Thomson in the *Proceedings of the Glasgow Philosophical Society* for 1881–1882, where a like structure was described in much thicker layers of soapy water cooling from the surface. In the *Scientific Papers* there appears a note added in 1918 about his own work, that "This problem had already been treated by Aichi (*Proc. Tokio Math.-Phys. Soc.* 1907)." The problem was later considered by Jeffreys, Low, Pellew & Southwell, and others. Harold Jeffreys used to call it the "porridge problem", for obvious reasons. In addition to the consideration of finite disturbances, other factors were to be added, such as the influence of surface tension and its variation with temperature, but there was nothing about Rayleigh's stability problem that should be called controversy, and the investigations could be counted as definitely successful.

In 1922 G. I. Taylor published his classical paper on the "Stability of a viscous liquid contained between two rotating cylinders." The results of the theory were definite and correct, in agreement with the experimental results. Simplifications of the mathematics, sufficient for the required purposes, and many further considerations of the flow between rotating cylinders, were to follow, but there could be no controversy.

Jeffreys demonstrated in 1928 the mathematical equivalence of the two stability problems of convection and flow between rotating cylinders, an equivalence which had been suggested by Taylor and Low.

The kind of instability to three-dimensional disturbances for flow over concave curved surfaces, exemplified by the work of Taylor in 1922 and later work on the transition to turbulence for flow between rotating cylinders, was once a cause for some concern in airfoil design. Once upon a time, in connection with the design of low-drag suction wings for which the boundary layer should stay laminar as long as possible, at the National Physical Laboratory we designed an airfoil section with a single slot on each surface, and with favorable pressure gradients everywhere except at the slot. The work was done by Richards. A model was made, and tested in a wind tunnel. In spite of the favorable velocity gradient behind the slot, the flow in the boundary layer became turbulent soon after the slot except at low Reynolds numbers. The airfoil surface was concave to the flow aft of the slot, and the transition to turbulence was undoubtedly connected with the three-dimensional instability in flow over such a surface, and its further development. For boundary-layer flow this instability was studied theoretically by Görtler in 1940, but because of the war we did not know of Görtler's paper, although in fact recollection of Taylor's work on the flow between rotating cylinders should have been enough. Transition to turbulence in the boundary layer on a concave surface was shortly afterwards studied experimentally by Liepmann (1943), of whose results we were informed. As we said at the time, all the laws of nature always continue to work, including those we forget.

As regards the definiteness of theoretical discussions of problems of stability, the state of affairs was different when it involved discussion of the Orr-Sommerfeld equation for parallel flows. Much controversy developed after Heisenberg's discussion in 1924 of the stability of plane Poiseuille flow, which did not die down until C. C. Lin clarified the general theory and gave a detailed calculation of the neutral curve.

Doubt and controversy had appeared before 1924. In 1914 Rayleigh, discussing work by Mises and Hopf on the apparently simpler problem of plane Couette flow, had remarked that "Doubtless the reasoning employed was sufficient for the writers themselves, but the statements of it put forward hardly carry conviction to the mere reader. The problem is indeed one of no ordinary difficulty."

There were even more causes for doubt and controversy over the calculations by Tollmein of the stability of the flow in a boundary layer on a flat plate without a pressure gradient, following previous discussions by Prandtl (1921) and Tietjens [1922 (thesis) and 1925], and themselves followed by further calculations by Schlichting (1933 and 1935). Flow in a boundary layer is not really a parallel shearing flow as assumed in the calculations. Moreover, for some time experimental observations failed to find the neutral or amplified oscillations predicted by the theory, and it appeared that the development of turbulence in a boundary layer depended on the amount of turbulence in the main stream outside the layer. Then in 1940, in experiments by Schubauer & Skramstad at the National Bureau of Standards, under the direction of H. L. Dryden, the theory was verified experimentally. In the introduction to the English edition of Schlichting's *Boundary Layer Theory*, Dryden wrote:

> My own interest in the experimental aspects of boundary-layer flow began in the late twenties. With the appearance of Schlichting's papers intensive attempts were made to find the amplified disturbances predicted by the theory. For 10 years the experimental results not only failed to confirm this theory but supported the idea that transition resulted from the presence of turbulence in the free air stream as described in a theory set forth by G. I. Taylor. Then on a well-remembered day in August, 1940, the predicted waves were seen in the flow near a flat plate in a wind tunnel of very low turbulence. The theory of stability described in the papers of Tollmien and Schlichting was soon confirmed quantitatively as well as qualitatively.

In the theories two-dimensional disturbances are considered. In fact, in 1933 Squire proved that the problem of three-dimensional disturbances of a plane parallel flow is equivalent to a problem with two-dimensional disturbances at a lower Reynolds number, so the minimum critical Reynolds number is given by a two-dimensional analysis.

After the experimental verification of the calculations of Tollmien and Schlichting, difficulties remained. Instability in a laminar flow, even with amplified disturbances, is not the same as transition to a fully turbulent flow; the gap in understanding was still rather large, and attempts have only recently been made to further understanding by considerations of finite disturbances, stability of the disturbed flow, etc. Further, I doubt if it is yet possible to state with certainty how much the appearance of turbulence in a boundary layer, especially over a curved surface with a pressure gradient, as in flow past a sphere, for example, is due to instability in the boundary-layer flow and how much to the effect of impressed disturbances from turbulence in the main stream, or how much they interact.

TURBULENCE

As previously stated, turbulence in boundary layers was mentioned at a fairly early stage by Lanchester and by Prandtl. One of the early triumphs of the ideas of boundary-layer theory was the explanation of the rapid drop of the drag coefficient, for instance of a sphere, with increasing Reynolds numbers in the neighborhood of a certain critical number, which depends on everything that can influence the transition to turbulence in the boundary layer, such as the degree of turbulence in the main stream, roughness of the surface, the method of support, and protuberances, hooks, or other attachments on the surface. In a turbulent boundary layer, because of the more vigorous interchange of momentum between different strata, the retarded fluid can make its way into regions of higher pressure before separation, so that although the friction drag is increased the form drag is considerably decreased. The phenomenon itself was first demonstrated by Eiffel in Paris in 1912 for a sphere, and the explanation was given by Prandtl in 1914, who obtained small drag coefficients even at fairly low Reynolds numbers by inducing turbulence with a wire hoop fixed on a sphere. Many years later stories were still being told in Göttingen about great disagreements in the results experimentally obtained there with those of Eiffel, and how the research on the cause began.

The search for a satisfactory method of forecasting frictional resistance with turbulent flows began early. A complete mathematical theory is not to be expected, and experiments did not deal with high Reynolds numbers. Coupled with formulae for the resistance coefficient is a formula for the distribution of velocity near a wall. In 1913 Blasius put forward interpolation formulae according to which the resistance coefficient varied as the inverse of the 1/4th power of the Reynolds number, and the velocity as the 1/7th power of the distance from the wall. Attempts were made to show that these formulae had a theoretical basis, but when experiments were made at Reynolds numbers above 10^5, it was found that the index 1/7 had to be diminished progressively to 1/8, 1/9, etc.

In 1925 Prandtl put forward what became known as his "mixture-length" theory, and also assumed in that connection that momentum is a transferable property. For forecasts near a wall, this was a valuable advance.

Meanwhile search for satisfactory extrapolation formulae for resistance and velocity near a wall continued vigorously, and, at the Third International Congress of Applied Mechanics at Stockholm, Kármán was the first to announce the famous logarithmic formulae for wall turbulence. He obtained the result from his "similarity theory." Prandtl obtained the results more simply in a paper published shortly afterwards (1933), and it later appeared that other "rational" arguments would produce the same result. However, the announcement in 1930 in Stockholm by Kármán was the first announcement of this famous "law" of wall turbulence.

The late 1920's and early 1930's was a marvelously exciting time for "semi-empirical" theories of turbulence.

In 1932 Taylor published a paper on "The transport of vorticity and heat through fluids in turbulent motion," in which he took vorticity, not momentum, as the transferable property, pointing out that the assumption that momentum is a transferable property involves the assumption that the fluctuations in pressure in the turbulent flow do not affect the mean transport of momentum. Taylor's vorticity-transfer theory in fact dated back to his paper on "Eddy motion in the atmosphere" in 1915 and to his essay for the

Adams Prize awarded the same year. The equations for the velocity distribution are, in general, different on the momentum-transfer and the vorticity-transfer theories, but, more importantly, when there is heat transport, predicted temperature distributions differ. In an appendix to Taylor's 1932 paper, Fage & Falkner reported on the measured temperature distribution in the wake of a heated cylinder, and the results on the vorticity-transfer theory were much closer to the experimental results than those of the momentum-transfer theory. Thereafter many experiments were made in an effort to assess the relative merits of the two theories.

Taylor published his "modified" vorticity-transfer theory in 1935 and 1937 and the application to flow in pipes in 1937. I cannot be sure of my recollection, but I believe he unearthed an old manuscript from among his papers (again part of his Adams Prize essay) when I showed him a (somewhat unsatisfactory) paper of mine on the generalized vorticity-transfer theory, saying that he had previously thought it too speculative to publish. I am more certain about a paper of Taylor's on another subject, stability in stratified fluids with density gradients, which was published in 1931. A manuscript was taken out of storage on my entreaty; it was part of Taylor's Adams Prize essay and had been in storage for some considerable time, its submission for publication having been hindered by the occurrence of the first world war.

Meanwhile research was proceeding on turbulent diffusion, which had its genesis in Taylor's 1921 paper in the *Proceedings of the London Mathematical Society* on "Diffusion by continuous movements."

However, a wholly new direction was given to research by the publication in 1935 of Taylor's "Statistical theory of turbulence" in a series of papers of striking originality, containing many new notions, among them isotropic turbulence, curves of correlation and energy dissipation, the decay of turbulence behind a grid, and correlations and energy spectra as Fourier transforms (1938). Kármán introduced the correlation tensor, depending, in an incompressible fluid, on one scalar function; and derived an equation for changes in that scalar, which can be used to obtain information about the rate of decay, with the assumption that the mean values of triple products of components of velocities at two points could be neglected. Kármán pointed out that if this is incorrect the vortex filaments would have a permanent tendency to be stretched or compressed along the axis of the vorticity, and believed that this could not be the case. However, Taylor showed that it was, the term neglected being shown from certain measurements in one case to be three times a term that is not neglected. There is a tendency for the vortex filaments to be stretched on the average. Turbulence is essentially dispersive. Later, during the second world war, I remarked to Taylor that an army general had just explained that the reason why information and supplies did not always arrive at their destination at the planned time was "the friction of war." "I suppose," replied Taylor, "that supplies are strewn over the countryside because of the turbulence of war."

Kármán & Howarth showed in 1938 that the triple-correlation tensor also involves one scalar function for isotropic turbulence in an incompressible fluid, and carried the discussion further with the hypothesis that the graphs of the correlation functions preserve their shape.

Work continued vigorously. Contributions came in from Loitsyanskii, Millionshchikov, and Kolmogorov, whose contribution proved particularly valuable. The name of the distinguished mathematician Kolmogorov was well known to other distinguished mathematical statisticians, some of whom had hopes of contributing to the theory of turbulence. When they saw the physical, rather than mathematical, nature of Kolmogorov's contribution most of them decided that such research was not for them. Other work, particularly on spectral analysis, also appeared. Meanwhile, experimental and semi-empirical work on shear turbulence was also proceeding.

On the whole the experimental and semi-empirical work flourished. After some time, however, the more fundamental theoretical work served more, after protracted examination, to exhibit clearly the difficulties than to solve them. This, too, was a valuable contribution.

It was at a meeting of the British Association in London in 1932 that I remember that Lamb remarked "I am an old man now, and when I die and go to Heaven there are two matters on which I hope for enlightenment. One is quantum electrodynamics, and the other is the turbulent motion of fluids. And about the former I am really rather optimisitic." (I have quoted from memory, so do not guarantee all the actual words. But the sense is correct. I have heard a similar story since repeated with other names than Lamb and other times and places.) Lamb was correct on two scores. All who knew him agreed that it was Heaven that he would go to, and he was right to be more optimisitic about quantum electrodynamics than turbulence.

Shock Waves, Gas Dynamics, High-Speed Aerodynamics

Let me return to shock waves and gas dynamics. Rankine and Hugoniot had published their papers on shock waves in the nineteenth century, and Ernst Mach and his associates began their experimental observations about the same time as Hugoniot published (1889). However, there was still some doubt and confusion about the theoretical explanation. The discontinuity seemed so very sharp. Was the process to be associated with the theory of an ideal inviscid gas, or were dissipative processes essential? After the publications of Rayleigh and Taylor in 1910, referred to in the opening 1900 to 1910 list, the theoretical ideas rested on a firm foundation and the way was open for further advances, both for more and more complicated problems on shock-wave interactions and on the reflection, diffraction, and refraction of shock waves, as in the work of Polachek & Seeger for example, and on the structure of shock waves, with allowance for bulk viscosity and variations with temperature of the viscosity coefficients (though the correct variation of the bulk viscosity is still uncertain). The investigations of the structure of shock waves required numerical methods. Investigations of shock-wave structure by the kinetic theory of gases also followed later. In the first half of the century the work culminated in J. von Neumann's numerical methods.

The early state of affairs may be illustrated by some quotations from the papers of Rayleigh and Taylor. Rayleigh writes, under the heading "Permanent regime under the influence of dissipative forces," "The first investigation to be considered under this head is a very remarkable one by Rankine 'On the Thermodynamic Theory of Waves of Finite Longitudinal Disturbance' (Phil. Trans. 1870), which (except for a limited part expounded by Maxwell in his *Theory of Heat*) has been much neglected." There is here a footnote in which Rayleigh says "I must take my share of the blame," but adds that "Rankine is referred to by Lamb (*Hydrodynamics*, 1906, p. 466)." In the text he continues "Conduction of heat is here for the first time taken into account and although there are one or two serious deficiencies, not to say errors, presently to be noticed, the memoir marks a very definite advance." Later he refers to "a long and ably written memoir by Hugoniot" and after repeating that "Rankine's investigation is expressly based upon conduction of heat in the gas" remarks that "A wave of this kind is never possible under the conditions, laid down by Hugoniot, of no viscosity or heat conduction. . . . A closer examination of the process by which [Equation 85] was obtained will show that while the first law of thermodynamics has been observed, the second law has been disregarded." The remarkable thing was that both Rankine and Hugoniot ended up with the same equations. Neither had considered viscosity.

Taylor pointed out that "The possibility of the propagation of a surface of discontinuity in a gas was first considered by Stokes" (1848). He later states flatly that "It is evident that a plane of absolutely sharp or mathematical discontinuity cannot occur in any real gas." He refers to the kinetic theory of gases, and continues "This suggests that heat conduction and viscosity are, in the case of a real gas, the causes of the production of dissipative heat; it will be shown that under certain conditions they are also sufficient to produce permanence of type in the layer of transition."

The study of gas dynamics and high-speed aerodynamics proceeded steadily and at a somewhat increasing pace during, say, the first third of the century. Some of the most valuable advances were in the provision and design of high-speed wind tunnels and instrumentation. Towards the end of the 1920's the only lectures on gas dynamics I remember being given in Cambridge, England were a series of eight lectures by G. I. Taylor.

In 1935 came the Fifth Volta Congress, which was indeed an important event. The Schneider Trophy context had had its effect on high-speed aerodynamics, and one of the papers at the Congress was by Wimperis on "The British technical preparation for the Schneider Trophy Contest, 1931," in which he described, as he said, "the combination which achieved success in the 1931 Schneider Trophy Contest." However, in addition, scientists and engineers were already dreaming of supersonic flight. Among the papers at the Congress those that are of interest here were general lectures on high-speed flow by Prandtl, Taylor, Kármán, Busemann, and Pistolesi, on high-speed wind tunnels and experimental technique by Eastman Jacobs, Ackeret, Luigi Crocco, and Panetti, on model airscrews at high speeds by Douglas, and research in France by Villat.

In the last decade of the half-century, particularly after the end of the second world war, the pace of publication and advance in knowledge became rapid, almost hectic. Reports delivered in one place of what had just been done at another were greeted by remarks that "Yes, we've just done that, too." It was all exciting, and great fun, and of serious use, too,—and the war was over.

It seems impossible even to list here the highlights of what was done in the half-century, apart from the provision of high-speed wind tunnels and equipment, but let us bring a few to mind: Prandtl-Meyer expansions; graphical and numerical use of the mathematical theory of characteristics; linearization for subsonic and for supersonic flow, and higher approximations; the Prandtl-Glauert rule; the linearized solution of Kármán and Moore for bodies of revolution; the clearing-up of the difficulties about the Prandtl-Glauert rule for bodies of revolution, etc., by Göthert and Sears, and the appearance of Göthert's rule; methods of successive approximation, first Jansen-Rayleigh and later Hantzsche-Wendt; Taylor's electrical analogy, with the electric field explored in an electrolyte in a shallow tank of variable depth which could be changed by successive approximations; boundary layers in gases at high speeds and frictional resistance; Taylor's calculations for a vortex and a source, forecasting the troubles of transsonic theory; the Taylor-Maccoll nonlinearized solution for supersonic flow past a cone; conical fields, whose study was started by Busemann; the slender-body theory of Jones (and earlier of Munk for low-speed flow) and its development by Ward and others (and later by Mac C. Adams and W. R. Sears); the development of Chaplygin's hodograph method and the alteration in his use of an approximate equation of state, leading to the famous Kármán-Tsien pressure formula, which was so extensively used in airfoil design for subsonic flight both during and soon after the second world war, with the theory later extended by C. C. Lin; the full analytical, mathematical development of hodograph theory by Lighthill and by Cherry (and also, differently, by Bergman) in 1947; Kármán's transsonic similarity rules; the similarity rules for hypersonic flow of Tsien and Hayes; and the work, done independently by Sedov, Taylor, and von Neumann, on blast waves.

In his lecture at the Volta Congress, Kármán had remarked "I have the impression that the possibilities provided by the hodograph method are not yet exploited sufficiently and that it can possibly be used for investigation of the mixed cases, i.e., of flows with partly subsonic, partly supersonic regions." In that lecture he also considered flow at very high Mach numbers, at what he called "ultra supersonic" speeds, and is now called the hypersonic range, and its connection with Newton's conception of air resistance. In 1933 Busemann had studied flow in a shock layer of vanishing thickness and obtained a formula for the surface pressures at the base of the shock layer.

In 1949 Lighthill published papers on the diffraction of blast, and on a technique for rendering approximate solutions to physical problems uniformly valid. The technique in the latter paper has been rather widely used, and there has been considerable discussion of its connection with the theory of singular perturbations, boundary-layer theory, the theory of "inner" and "outer" solutions.

Whitham's paper in the *Proceedings of the Royal Society*, published in 1950, on "The behaviour of supersonic flow past a body of revolution, far from the axis" was first turned down by a referee with a very interesting and rather full discussion of his objection, which, in summary, was the danger of using isentropic methods to study the decay of a shock wave. This had previously been done by Friedrichs in the plane case, so, as the referee pointed out, "Whitham is in good company." The argument of the referee was serious, and this was the genesis of Lighthill's 1950 paper on "The energy distribution behind decaying shocks." Whitham's paper was duly accepted by the Royal Society and published.

Lighthill's first papers were published in 1944, and by 1948 he was invited to deliver a general lecture at the Seventh International Congress for Applied Mechanics. The lecture was on "Methods for predicting phenomena in the high-speed flow of gases." Lighthill spoke then as an applied mathematician or mathematical physicist, and his concluding words are interesting:

> In conclusion, I will say that our understanding of the high-speed flow of gases is growing rapidly; as a mathematician I believe that our endeavors in this field are no waste of effort or of mathematical techniques not only because we are assisting in a great new engineering adventure—supersonic flight—but also since we are getting to grips in this problem with that old bogeyman, the nonlinear partial differential equation, and smelling out his ways in a manner for which our colleagues, in the more fundamental parts of physics, may later be grateful.

Experimental Facilities and Techniques, Numerical Methods

Many improvements in experimental facilities and techniques took place in the first half-century. There were wind tunnels for special purposes, high-speed tunnels and low-turbulence tunnels (with which the investigations of Prandtl and of Taylor on the effect of a contraction on turbulence in a wind tunnel were connected). As time went on, we were on our way to bigger and bigger, and faster and faster tunnels. In instruments, electrical methods, especially for measurements in turbulent flow, came into increasing use:—the hot-wire anemometer, the hot-wire direction-meter, methods of measuring speed variations and correlations in turbulent flow, and of determining energy spectra. The methods of visualizing and photographing fluid motions produced fascinating results, whether the method was smoke from the rotted wood of an apple tree or a cheap cigarette or titanium or stannic tetrachloride, or the results from the chemical coating of a surface, or hot-wire or spark shadows, or Schlieren or interferometer pictures, or the ultramicroscope photographs in water of Fage & Townend.

Numerical methods have previously been mentioned in connection with stability investigations, lifting-surface theory, and the structure of shock waves. In the 1930's Southwell's relaxation method, and other somewhat similar methods, were being used. Digital machines came gradually more and more into use. Later, largely under the inspiration of von Neumann and of Pekeris in the Weizmann Institute in Israel, serious use was to be made of high-speed computing machinery, but that was really a later story, and even now there is still a long way to go.

Prandtl once told me that he had considered building an analogue machine, but came to the conclusion that water itself was the best. It is reported that Neumann once said that there would be no further need for experiment —high-speed computation could take over. I suppose both were incorrect; we need both experiment and computation.

Conclusion

Much has been omitted. I have steadfastly resisted the temptation to write about the arguments concerning education, and the relative importance of mathematics, physics, and engineering. Among other matters omitted are: (*a*) the beginnings of magnetohydrodynamics, Hartmann & Lazarus (1937) on the flow of mercury in a channel in the presence of a magnetic field, Hoffmann & Teller on magnetohydrodynamic shocks, the connection with astrophysics and the publication of Alfvén's *Cosmical Electrodynamics* just after the close of the first half-century; (*b*) whole chunks such as meteorological investigations, oceanography, surface waves and tides, tidal dissipation and its astronomical application (Taylor, 1918 and 1920, and Jeffreys 1920), and shallow-water theory, and geophysics; also the stability of rotating masses of fluid and astronomical applications; (*c*) interesting and important detailed questions, which some of us taught to students with considerable enthusiasm, such as the method of singularities for discussion of the aerodynamic properties and design of airship shapes and the use by Kármán of doublets to allow for crosswinds (1927), and trailing vortices for two-dimensional airfoils in time-variable motion (Birnbaum, 1924; Wagner, 1925; Kármán & Sears, 1938; Sears, 1940; and others); unsteady airfoil motion in high subsonic and in supersonic flow; also the general calculation of virtual inertia, forces and moments in irrotational inviscid motion; and Carrier's modification of Oseen's method; (*d*) the connection of fluid dynamics and heat transfer; (*e*) rarefied gases and relaxation effects—I still remember vividly Arthur Kantrowitz's first demonstration with CO_2 at Langley Field; (*f*) such things as turbulence in round pipes, and the repetitions of Reynolds's experiments by Barnes & Coker, Ekman, and Taylor, and also discussions of the possible wake behind a sphere corresponding to a vortex-street behind a cylinder, with some of us discussing this far into the night in every night-club in Aachen at the time of the Aachen Conference in 1929 and finally "solving" the problem with alcoholically induced euphoria; (*g*) jets and cavities; and (*h*) much else.

Some quotations have been included in this article. Perhaps there are readers who think the article would have been improved if there were more quotations and less other matter. I am reminded of a brief review in *Nature* of a book on education. The whole review was: "Half of this book is quotations; the other half should have been."