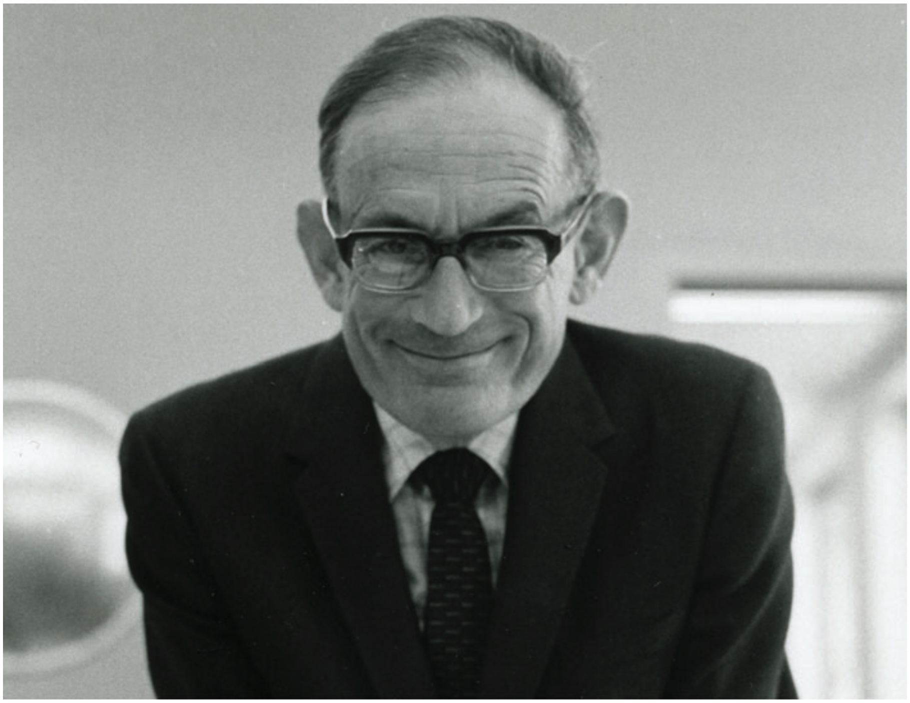

Hans W. Liepmann

Hans W. Liepmann, 1914–2009

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Abstract

This article presents a brief account of the life and work of Hans W. Liepmann, a distinguished fluid dynamicist, an outstanding teacher and leader, and the third Director of the Graduate Aeronautical Laboratories, California Institute of Technology.

INTRODUCTION

Hans Wolfgang Liepmann, a distinguished fluid dynamicist, outstanding teacher, and the third Director (1970–1985) of the Graduate Aeronautical Laboratories, California Institute of Technology (GALCIT), passed away on June 24, 2009, at his home in Flintridge, La Cañada, near Pasadena, California. His passing marked the end of an era that began with World War II, saw the development and growth of a fluid mechanics driven by aeronautical applications, and (with the launch of Sputnik in 1957) expanded rapidly into such new branches as rarefied gas dynamics, hypersonics and high-velocity flows, "real-gas" dynamics, magnetohydrodynamics (MHD), and plasma physics.

During this period, Liepmann's research interests changed from wartime work on instability, transition to turbulence, and transonic flows to low-density flows, strong shocks, and finally to liquid helium, although turbulence remained an invariant interest throughout.

EARLY YEARS, EDUCATION

Much material in this section and some of the stories about his experiences, both professional and personal, have been taken from a manuscript that Liepmann kept writing off and on from 1992. The material was collected together into a 64-page document titled "May You Live in Interesting Times" (Liepmann 2004).

Liepmann was born in Berlin on July 3, 1914, just about a month before the beginning of World War I. His parents had met in a faculty ball in the small university town of Halle. His father was an outstanding doctor and academic, and his mother was the daughter of an outstanding surgeon and academic. The family eventually moved to Berlin. His father was deeply interested in Goethe, so when the first son was born he was named Hans-Wolfgang, slightly modernizing Goethe's first name (Johann-Wolfgang). His mother had wanted to study medicine and could not, but she became very knowledgeable as an assistant to her husband when he was performing surgery.

Liepmann had a natural inclination toward natural sciences and engineering but had to go to a severely classical gymnasium because of his father's passion for the humanities. He eventually transferred to and graduated from a more lenient classical high school in 1933, the same year in which Hitler came to power.

Liepmann intended to study physics, but jobs were not easy to get for physicists at that time, so his parents did not encourage him. To do engineering, he had to spend a year as an apprentice with some industry. He did so at the well-known company of Siemens and Halske (roughly similar to GE in the United States). What he learned during that year stood him in good stead later on as he often made his own experimental apparatus.

As Nazi power grew in Germany (and Liepmann was classified as an Aryan half-Jew), his parents left first for Switzerland and then in 1933 for Kemal Pasha's new Turkey, where his father accepted the position of head of the department of gynecology in the reorganized University of Istanbul. Among the 50 or so other scholars from Europe who went to Istanbul as faculty around that time were Richard von Mises and William Prager. After his year at Siemens, Liepmann joined his parents in Istanbul in 1934. Here he had his first encounter with physics and mathematics in the classroom—the latter with von Mises as instructor being the less successful, because of Liepmann's "atrocious background in math" from his classical high school. In 1935 he took the Orient Express to Prague, spending a term there at the German University and learning about Fourier series in a Kaffeehaus. His encounter with theoretical physics was not much more successful than with math, but he soon discovered that he had a great aptitude for experimental physics.

Liepmann's life in Turkey left an indelible impression on him; among the few pictures he had on the walls of his office at Caltech, even in much later years, was one of the Hagia Sophia (another one was of G.I. Taylor).

In the fall of 1935, Liepmann went to Zürich to study physics and made another discovery: He actually knew more physics than he had imagined. He was taken immediately as a research student by Prof. R. Bär, and started working on the application of light-scattering methods to the measurement of sound velocity in liquid oxygen.

By 1938 it was clear to Liepmann that Europe was heading for a crisis, and triggered by Chamberlain's talk of "peace in our time" but fearing the exact opposite, he decided to emigrate. In February 1938, while celebrating his PhD in physics and mathematics in a Zürich pub, he was asked by the head of the department, Edgar Meyer, what his future professional plans were. High on a glass of beer, Liepmann answered "hydrodynamics." This was enough to win a recommendation from Meyer to von Kármán (an old acquaintance from Aachen), and perhaps another from von Mises, and took Liepmann to Pasadena (which he first had to spot on a map) and Caltech. Before getting there, he visited Hugh Dryden's group at the National Bureau of Standards (NBS), Washington, at von Kármán's instance. He arrived in New York on July 31, 1939, on a boat from Le Havre, surrounded by passengers who did not believe war was imminent (it broke out about a month later). After he finally arrived in Pasadena, he quickly took to Caltech "like fish to water," and spent the rest of his life there.

THE WAR YEARS

Instability and Transition

The first area in which Liepmann's work at GALCIT made a significant impact was a study of instability and transition in the boundary layer on curved surfaces (Liepmann 1943). (This was secret wartime work, and R.N. remembers Liepmann telling him that he had to have an armed guard accompany him whenever he went to work at night.) This work was influenced by the remarkable discovery, by Schubauer and Skramstad at the NBS labs directed by Dryden, of the so-called Tollmien-Schlichting (T-S) instability waves predicted by linear theory in the laminar boundary layer on a flat plate. In the late 1930s, most fluid dynamicists outside Germany (including, in particular, G.I. Taylor) did not believe in the T-S waves, as they had never been observed experimentally. In an account of the early days of boundary-layer transition research at GALCIT, Liepmann (1997) recounted how, sometime during 1941, Clark Millikan handed a sheaf of papers to von Kármán with the words, "It is a complete German victory!"—a victory not on the battlefield but in the NBS laboratories, where the German T-S waves had at last been observed in a boundary layer excited by appropriate periodic forcing in the quiet NBS tunnel. Liepmann had great regard for the NBS in general and Dryden and Schubauer in particular, and always insisted that T-S should stand for Tollmien-Schubauer. The GALCIT investigation was part of a National Advisory Committee for Aeronautics program on transition research, farmed out to NBS, MIT, and Caltech.

Liepmann's investigations consisted of studies on a flat plate, on the concave and convex sides of a curved plate with a radius of curvature of 20 ft, and on the convex side of another plate with a radius of curvature of 30 inches. He reported that transition on the convex side was similar to that on the flat plate at NBS. On the concave surface, however, the boundary layer appeared unstable to streamwise vortices of the kind theoretically predicted by Goertler in Germany. Interestingly, Liepmann noted the presence of large fluctuations in the transition region and attributed them to sudden changes in the mean velocity distribution, which according to him alternated between a Blasius-type laminar profile and a Kármán-type turbulent profile (as explicitly shown in figure 9 of Liepmann 1943). Indeed he noted that "the first appearance of turbulent bursts—that is, occasional sudden changes from a laminar profile to a turbulent profile—is taken as a transition criterion" (Liepmann & Fila 1947). As was found later by H.W. Emmons, this kind of alteration in mean velocity profile can be explained if transition occurs through spots of turbulence, the flow being turbulent within the spot and laminar outside.

Around this time Liepmann's interest began to move away from transition to high-speed flows, but it seems clear that he never completely abandoned the subject. In his Dryden lecture of 1972, unfortunately never published (but a draft of the lecture is available), he made a survey of all the developments that had taken place since his own early work. In a style that is characteristic of much of his work, he derived the main results of stability theory that were relevant to transition by simple arguments emphasizing the physical phenomena. But he returned to the subject in 1982 in two papers (Liepmann et al. 1982, Liepmann & Nosenchuck 1982). In these papers, it was first shown how hot film gauges could be used to activate laminar instability waves by periodic heating. With this technique, it was possible to excite waves from such gauges to cancel out those that were already there by amplification of waves generated earlier. This way it was shown that, using surface gauges along the plate, laminar-turbulent transition could be actively controlled by inducing suitably phase-shifted waves using film gauging in the stress-sensing mode to provide signals that could be used in a feedback active-control system.

Interestingly, the last thesis that Liepmann supervised was that of Steve Schneider (1989) on the effects of controlled three-dimensional (3D) perturbations on boundary-layer transition. So transition was always on Liepmann's mind throughout his career.

High-Speed Flows

Another area that became very important during the war years was transonic and supersonic aerodynamics. In 1942 Liepmann got involved in GALCIT's evolving program in this area, to which von Kármán had already made significant contributions. With a 10-ft, low-speed wind tunnel having already established GALCIT as a center for aerodynamic testing and research, getting into the high-speed regime seemed natural. In about 1941–1942, a 2.5 inch $\times$ 2.5 inch transonic tunnel was assigned to Allen Puckett, a graduate student from Harvard working for his PhD with von Kármán. In 1943, two US Navy lieutenant commanders wrote a progress report on "The Theory, Design and Performance of a Two Dimensional High Speed Induction Type Wind Tunnel." Some three years later, another contract for a transonic facility was assigned to Liepmann. Liepmann also was involved, along with Puckett, in giving a series of lectures on compressible flow to engineers at aircraft companies in Southern California. These lectures were also Liepmann's introduction to the subject, and eventually resulted in the book *Aerodynamics of a Compressible Fluid* (Liepmann & Puckett 1947). It had meanwhile been found that shock waves appear in transonic flow over airfoils and cause the boundary layer to separate, a phenomenon called the "compressibility burble." A continuous-flow wind tunnel 20 inches high $\times$ 2 inches wide was set up to investigate such phenomena in 2D flows over circular arc airfoils 2 inches in span. (The 20-inch height was necessary to reduce the severity of the well-known problem with wall interference in transonic flows.)

While all of this was happening, Millikan was already busy with a plan for the Southern California Cooperative Wind Tunnel, which would reach near-sonic velocity in a test section of approximately 7 ft $\times$ 11 ft. Things were clearly moving very fast indeed.

Beginning with Julian Cole's engineer's thesis in 1947 on transonic flow past thin airfoils, there were 12 others at GALCIT dealing with high-speed flows one way or another during the next seven years. Some of this work was on boundary layers, as discussed above, but one of the most interesting was probably on shock–boundary layer interaction (Liepmann et al. 1951b). When a shock wave impinges on a flat plate and is reflected, the no-slip boundary condition at the surface alters the classical inviscid flow picture dramatically. In particular, the surface pressure distribution exhibits an influence that is felt as far as 50 boundary-layer thicknesses upstream when the boundary layer is laminar, but this is reduced to 5 boundary-layer thicknesses when the boundary layer is turbulent. Liepmann et al. (1951b) pointed out that one may ask "why a complicated phenomenon such as an interaction between a shock wave and boundary layer is investigated before the boundary layer in uniform supersonic flows has been studied carefully"! Clearly the research was driven by wanting to understand, even if only qualitatively, the way that strong viscous diffusion effects near the wall where flow is subsonic would modify the largely inviscid picture that prevails away from it.

TURBULENCE

Research on turbulence had in fact preceded Liepmann's arrival in Pasadena, as von Kármán had published a paper with Howarth on the statistical theory of isotropic turbulence (von Kármán & Howarth 1938), in which governing equations for correlation functions were derived. Von Kármán was interested to see how various aspects of his theory compared with experimental results: He was looking for confirmation of isotropy, data on correlation functions, the rate of decay of turbulence energy, etc. Experiments made in 1938 in a special open-circuit wind tunnel did not compare satisfactorily with existing data; this led to a visit by Dryden who brought his own hot-wire equipment from Washington. Although there was some indication of isotropy "within experimental scatter," the situation was not satisfactory. One of the assignments given to Liepmann, who had just arrived as a postdoctoral fellow (but without a stipend), was in fact to get the project going. The tunnel built for this purpose was 20 inches × 20 inches × 20 ft long, had turbulence levels as low as 0.02%, and was used to measure energy decay in grid turbulence.

In 1942, Liepmann's first research student, Stanley Corrsin, made measurements in grid turbulence for an aeronautical engineer's thesis. In 1951, Liepmann wrote a report with John Laufer and Kate Liepmann on the spectrum of isotropic turbulence (Liepmann et al. 1951a). It is interesting that the first paragraph in this report talks about turbulence and shocks in the same breath: Both are seen as striking effects of nonlinearity, containing a mechanism for steepening vorticity or velocity gradients until they are balanced by diffusion. So this simple but compelling argument about what is essentially different in Navier-Stokes dynamics seems to have been responsible for the fact that Liepmann's research for the next four decades was dominated by turbulence and shocks.

Liepmann et al. (1951a) presented spectra and correlations at Reynolds numbers of up to 10^5 based on the grid mesh, found a range of frequencies where it is nearly of the power-law form due to Kolmogorov, and supported the work of Dryden on the approximate form of the spectrum at lower frequencies. An interesting side result had to do with estimating the microscale by counting the number of zeros in the fluctuating velocity time series—a subject on which Liepmann (1949) had earlier written.

Although Liepmann continued to mention isotropic turbulence for some time, it is clear from the research carried out in his group that the subject was quickly abandoned. (A note unearthed by M.G., listing items for a talk by Liepmann, has an entry on the promise of and eventual disappointment with homogeneous, isotropic turbulence.) The argument appears to have been that isotropic turbulence was almost never found in nature and was also hard to create in the laboratory. Turbulent shear flows, conversely, were certainly closer to real life and moreover fascinating to study. In fact, this rapid change in perspective is quite visible in the subjects chosen for his students' theses. Corrsin, who had studied grid turbulence in 1942, wrote his PhD thesis on round turbulent jets in 1947. John Laufer studied mixing layers in 1947 (Liepmann & Laufer 1947), wrote his PhD thesis on 2D channel flow in 1947 (Laufer 1947), and continued with pipe flows (Laufer 1954). In 1951, Satish Dhawan wrote a thesis on the direct measurement of skin friction on flat plates (Liepmann & Dhawan 1951, Dhawan 1953). (Interestingly, this was seen as providing an independent test of boundary-layer theory, as it did not depend on any of the assumptions underlying the theory.) The work was continued by Donald Coles in his thesis on supersonic boundary layers in 1953, and he followed it with extensive analyses of the characteristics of turbulent boundary layers at both low and high speeds in succeeding years. Anatol Roshko (1952) obtained his PhD for a thesis "On the Development of Turbulent Wakes from Vortex Streets." (Incidentally, this actually started out as a project on isotropic turbulence, the idea being to find out how much of the imprint of the wire mesh would remain in the turbulence as it evolved downstream. Roshko started out with a single wire and found the vortex wakes so fascinating that he never got beyond it.) He went on later to carry out a wide variety of investigations on the near and far wakes of bluff bodies over the next several decades. Thus, in about six years, virtually all the shear flows that today would be considered canonical had been investigated by Liepmann's students.

Among these, Liepmann & Laufer (1947) reported a much referenced experiment on a mixing layer at high Reynolds number. At the time, there were two "solutions" for the self-similar mixing layer, one by Tollmien using mixing-length theory and the other by Goertler using eddy viscosity. One objective of the experiment was to compare those differences, presumably to decide which one fit the data better. Neither profile shape could be fitted exactly, but best fits could be made by adjusting σ in the similarity variable $\eta = \sigma y/x$. The values of σ were 12.0 and 11.0 for Tollmien and Goertler, respectively. The conclusion was that the theories are "based on invalid assumptions," and it was further noted that "the overall characteristics of a turbulent mixing process can be obtained by dimensional reasoning without any assumptions about the physical mechanism of turbulent motion." The careful measurements of the mean velocity field and the convincing demonstration of self-similarity made Liepmann & Laufer's (1947) paper a starting point for many investigators over the following years.

Looking at those pioneering years in retrospect when he wrote an obituary of Satish Dhawan, Liepmann (2002) said, "It was a marvelous time! Almost everything we touched was new and exciting"—turbulence, shock waves, or both.

There was also a short program on aerodynamic noise during the period from the mid-1950s to the early 1960s. This program included investigations of noise radiated by jets, the vibration and acoustic radiation of thin-walled cylinders caused by internal turbulent flow, and aerodynamic noise on a flat plate in supersonic flow. Liepmann also proposed a theory (in an unpublished report) that attributed noise to fluctuations in the instantaneous displacement thickness of the radiating turbulent shear flow. However, this effort did not sustain itself and gave way to the more central issues of shock waves and turbulence in succeeding years.

However, even as Liepmann maintained close involvement with turbulent shear flow research, his own publications on the subject were usually in the form of broad reviews. The first of these was a two-part survey on "Aspects of the Turbulence Problem" (Liepmann 1952a), which reviews much of the work done at GALCIT and elsewhere. It emphasizes the importance of large-scale structures in turbulent shear flow, which became evident from the wartime work of Corrsin (1943), and later that of Townsend (1947). These experimental studies showed that the outer part of a jet or wake was only intermittently turbulent. Liepmann incidentally also noted that "there do exist a number of cases in which a regular or nearly regular motion of large scale superimposed upon turbulent flow has been observed." He saw the evidence for this in the work of Pai (1943) (one of Liepmann's first assignments on arrival at Caltech had been to make some changes in Pai's thesis) and of Roshko (1952). He noted that these "large eddies" were important because of their role in the bulk transfer of turbulent flow and that their size (comparable to the dimension of the whole flow) made it difficult to account for their behavior in terms of local quantities.

Liepmann (1962) returned to this issue in his lecture on "Free Turbulent Flows" at the Marseille Colloquium in 1961. He made the interesting point that although the analogy of turbulent transport with the kinetic theory of gases could legitimately be criticized, there was also a kinetic theory analog in free-molecule flow, for which the sudden expansion of a gas cloud into vacuum could be described through the Navier-Stokes equations with a diffusion coefficient proportional to the time from the instant at which the expansion begins (Narasimha 1962) (the relation here is local with respect to the velocity gradient, but global with respect to the implied viscosity). From the picture that Liepmann (1962) constructed of turbulent motion, he introduced the concept of a "turbular" fluid, whose instabilities could lead to the large-scale structure observed in many flows, for example, in the Kármán vortex street that Roshko saw at a Reynolds number of 10^7. [The work of Brown & Roshko (1974) on mixing layers showing the presence of strikingly well-organized vortices in turbulent mixing layers was still more than 10 years away.] Liepmann saw the turbular fluid as non-Newtonian with a viscosity increasing with the rate of strain and discussed the relation of these ideas to the work of Townsend and the simulations made by Abernathy & Kronauer (1962) using a finite number of point vortices representing two plane vortex sheets leading to large-scale Kármán-type vortices. He also made brief comments about how the much simpler 2D vortex motion gives one "a fascinating glimpse into rapid randomization by vortex interaction and a demonstration of the statistical character of turbulence proper!" He even made some admittedly speculative comments drawing thermodynamic analogies. It is interesting that these comments were made apparently without any knowledge of the statistical hydrodynamics of point vortices initiated by Onsager (1949), and well before either the demonstration of dynamical chaos in any system of more than three point vortices or the discovery of highly organized motion in mixing layers by Brown & Roshko (1974).

His third review of this type, titled "The Rise and Fall of Ideas in Turbulence" (Liepmann 1979b), was written when the concept of coherent structures had become well established through work done at GALCIT. He summarized this work by saying, "As a problem in statistical mechanics, 2D shear flow turbulence looks like an ensemble of interacting gyroscopes." Another result that he included is derived from the linearized equations for perturbation vorticity in 2D parallel flow (these are the basis for the Orr-Sommerfeld stability theory): $\langle v\zeta \rangle = -\partial \langle uv \rangle / \partial y$. He made the interesting interpretation that this relation (which appears in Taylor 1915) could imply that the vorticity flux can be thought of as the source term for the Reynolds shear stress.

THE POST-SPUTNIK YEARS

With the launch of Sputnik in October 1957, and the decision in the United States to undertake a major initiative in space technology, many new branches of fluid mechanics began to attract a great deal more attention. Most of these can be grouped under the category real-gas dynamics, a term we use here to include low-density, high-velocity, chemically reacting, MHD, and a variety of other related flows going beyond the scope of classical incompressible and compressible fluid dynamics.

Low-Density Gas Dynamics

The first subject that Liepmann began to pursue was rarefied gas dynamics, which was a natural extension of his interest in the kinetic theory of gases and the Boltzmann equation as a physicist.

This interest must have begun in Zürich, as Meyer had gifted him a copy of Boltzmann's lectures on kinetic theory (now with R.N.). Characteristically, Liepmann (1961) set out to "find one simple and experimentally realizable flow problem for which both the Euler limit at infinite Reynolds number and the free molecular limit at zero Reynolds number are well defined and, at least in principle, theoretically understood, if not worked out in detail." The choice he finally made was that of flow through an orifice. He built an elaborate piece of apparatus, virtually all in glass and put together by himself, to study the problem over the Reynolds number range 5×10^{-2} to 5×10^{2}. He analyzed both limits in Liepmann (1961). He first provided a simple estimate of the correction to the classical Knudsen value for the mass flow through an orifice in the free-molecule limit [more precisely calculated later by Narasimha (1961a)]. Then he went on to consider the Euler limit in 2D flow and, based on earlier work by Guderley, showed that there is a maximum flow through the orifice between the two limits; i.e., the transition between $\mathrm{Re} \to 0$ and $\mathrm{Re} \to \infty$ is not monotonic and in fact has an overshoot around a Reynolds number of 40–50. Liepmann made the interesting side comment that in engineering work, "the transonic character of the flow has not been appreciated, e.g., the large pressure ratios necessary to obtain maximum mass flow come as a surprise to many workers in the field."

Liepmann's second major effort in rarefied gas dynamics concerned the shock wave. Liepmann et al. (1962a) presented a nearly exact numerical solution of the Bhatnagar-Gross-Krook model of the Boltzmann equation for the structure of a shock. This had become possible because of the advent of what was then the most powerful computer available, namely the IBM 7090, at the Jet Propulsion Laboratory. It was immediately realized that this would enable the solution of the full BGK equation and therefore get a structure for the shock layer from a model that had the correct behavior at both limits, namely Navier-Stokes and free-molecule flow; this had become clear from work on orifice flow (Narasimha 1961b). The shock-structure solutions thus obtained were unexpected and revealed for the first time how different dynamical regimes play out within the shock.

Section 2 of Liepmann et al. (1962), ostensibly on the Navier-Stokes solution for the shock but in fact containing an analysis of where the strongest deviations from it could be expected, was largely the work of Liepmann himself. The analysis was relatively simple and was based on the examination of two variables: the ratio of the normal stress τ to the pressure p and the ratio of the heat flux q to τu, where u is the flow velocity. It makes clever use of the fact that if $q = \tau u$, then the stagnation enthalpy is constant across the shock. This enables approximate model-free estimates, leading to the conclusion that the critical region for the failure of the Navier-Stokes approximation is upstream of the maximum stress point within the shock. The major point of the analysis was that it divided a strong shock into regions where the Navier-Stokes approximation might be useful and those where it would not. This shed new light on the way that one approaches the shock-structure problem. The analysis was in part triggered by the numerical solutions that had just been computed by Chahine at the Jet Propulsion Laboratory at the suggestion of Narasimha and Liepmann. These showed spectacular differences from Navier-Stokes in the temperature profile on the upstream side of the shock but not on the downstream side. Describing the structure of the shock wave by just a thickness characterizing the density profile was shown to be inadequate to reveal the changing dynamical regimes within the shock.

The 17-Inch Shock Tube

Work done on the gas kinetics of orifice flow and on the structure of shock waves was accompanied by the conceptualization of a shock tube for experimental studies in rarefied gas dynamics (Liepmann et al. 1962b; it was an all-faculty effort). The specifications for the shock tube were determined by the requirement that the length scales and timescales of such dissipative flows as boundary layers, shocks, and reaction zones should be sufficiently large to enable accurate measurements. For example, if shock waves were to be up to 1 cm thick, the tube diameter would have to be larger by a factor of approximately 50. Furthermore, at the densities required for the purpose, the shock-contact separation becomes asymptotically proportional to the diameter for a given shock Mach number; this occurs when balance is achieved between the new mass ingested by the propagating shock and the mass loss to the boundary layer on the tube's inner wall. Eventually, the designed shock tube had a 17-inch inner diameter and was 67.9 ft long in the driven section. It was made of welded stainless steel pipes, honed inside. Although the tube itself was conventional, it carried many innovations, such as a specially designed cruciform blade cutter for diaphragm rupturing and a design that enabled the shock tube as a vacuum system to have a leakage of less than 0.01 μm Hg per hour (corresponding to continuous leakage through a 0.2-μm hole). The whole facility could be operated by one person. The tube was suspended from the ceiling, saving floor space for other equipment (including, as the paper points out, "a ping-pong table sometimes used for unsponsored research in low-speed aerodynamics").

This facility was used for a series of investigations by various members of the GALCIT fluid mechanics group. A review (Coles et al. 1969) concluded that theory and experiment were "now in substantial agreement" on the structure of a shock wave in a monatomic gas. Interesting studies were made of shock wave reflections from real walls, and much progress had been achieved in more complicated systems, such as reacting gases and gas mixtures.

Magnetohydrodynamics

For a period of about a decade, Liepmann's students wrote approximately a half-dozen theses on problems in MHD. These covered MHD surface waves, MHD flow past bodies and the associated drag, radiative dissipation in shock waves, and ionization precursors ahead of strong shocks. Although there were interesting issues, Liepmann himself seems to have written little about it.

CRYOGENIC SHOCKS AND SUPERFLUID TURBULENCE

As he noted in a review (Liepmann & Laguna 1984), the motion of fluids is described by a nonlinear field theory whose "most striking manifestations . . . are shock waves and turbulence, corresponding to nonlinear wave and vortex interaction respectively." Liepmann's lifelong interest in these two phenomena found a new outlet in the fluid dynamics of liquid helium, and he and his students proceeded to study both.

First, all the work in rarefied gas dynamics described above led to the idea of a cryogenic shock tube (Liepmann et al. 1973), based on the following argument. The shock strength obtained in a shock tube depends on the ratio of the temperature in the driven gas to that in the driver gas. One obvious way to get stronger shocks is therefore to increase the latter, but another way is to keep it fixed and decrease the former. Thus, using helium as the working gas, the temperature of the driven gas can be as low as 2 K; even if the driver is at room temperature, it is feasible to obtain shock Mach numbers of the order of 50. Furthermore, it turns out that with helium, even the gas behind a shock reflected from the end wall can be no hotter than 3,600 K, at which temperature it still behaves like a perfect gas. It is thus possible to achieve huge hypersonic Mach numbers without the "real-gas effects" that would accompany such Mach numbers in more conventional atmospheric gases. Two such cryogenic shock tubes were actually built and used for research; shock Mach numbers of 40 were achieved in a perfect gas by this technique (Cummings 1974).

Turning now to turbulence, Liepmann began to look at the fluid dynamics of liquid helium itself. He presented the results of this work at a symposium in honor of Sydney Goldstein on his 70th birthday in Haifa, Israel, in 1973, and in Liepmann & Laguna (1984). His motivation for pursuing this line of work has already been quoted.

The resulting research made good use of the experience that Liepmann's group had in the study of turbulent shear flows in classical fluids. It was partly based on the recognition that a disproportionate share of that knowledge of turbulent shear flows had come from what today would be called canonical flows (e.g., mixing layers, jets, wakes, and flat-plate turbulent boundary layers), all first studied at GALCIT in the 1940s and 1950s. Furthermore, free shear flows had played an especially strong role in that effort as the absence of walls made them simpler and promised more immediate insight into the essential mechanics of a turbulent flow. (After all, the subject of Liepmann's lecture at the famous 1961 Colloquium on the Mechanics of Turbulence at Marseille had also been on free turbulent shear flows.)

The precise flow selected for this purpose was what had come to be called counterflow jets, going back to the work of Kapitza (1941). The flow configuration here involves a well-defined jet of normal fluid issuing from the mouth of a counterflow channel, accompanied by a superfluid flow that enters the same channel through the same mouth. This can produce large relative velocities between the superfluid and the normal fluid, leading to mutual friction. According to a well-known model (Gorter & Mellink 1949), this mutual friction is proportional to the cube of the relative velocity. A consequence of this model is an axial temperature gradient along the flow. Experiments at GALCIT (Dimotakis & Broadwell 1973), however, had found that there was a significant temperature gradient only within the channel and none in the free jet beyond one diameter from the channel mouth. These and other measurements by Laguna showed that there was a serious problem in the prevailing understanding of such counterflow jets. There were two possible explanations. One was that the mutual friction is not a volumetric force acting in the fluid but rather one that depends on the walls. The other explanation attributed the findings to entrainment. The decisive experiment here investigated the scattering of the so-called second sound by the turbulent jet. A simple calculation based on the methods used in an early study of Liepmann (1952b) for Douglas Aircraft yielded results that were very similar to those found experimentally. This supported the entrainment hypothesis and suggested that the jet was turbulent and that the mutual friction away from solid boundaries played a role somewhat like of that of viscous dissipation in an ordinary fluid. The authors "hoped that an ambitious theorist will attempt a more realistic calculation of the flow field"! This work showed that without an adequate understanding of classical turbulent shear flows, there was the possibility that more would be attributed to quantum effects than could be justified.

LIEPMANN AS TEACHER, LEADER

Liepmann was universally admired as an outstanding teacher—in the classroom as well as with his research students. He took great trouble with his classes and invariably worked his lecture out in the early hours of the morning (he was an early riser); derivations were written down neatly in black notebooks. It was a matter of pride for him that he gave his lectures without the benefit of any notes (although he did keep a few cards in his pocket for emergencies should he get stuck at some point in a derivation!). There was a permanent hint of a smile on his face while he lectured, as if the way he was deriving his results was fun because it was so simple and offered such rich insights.

It is therefore no wonder that he was the coauthor of two texts on compressible flows that between them taught the subject to generations of students in the United States and elsewhere over a period of more than 60 years. The first was *Aerodynamics of a Compressible Fluid* (Liepmann & Puckett 1947). This was felt to be in need of updating by the early 1950s. Around 1953, Liepmann asked Roshko (by then a research fellow investigating bluff bodies and their near wakes) whether he would be interested in being coauthor of the update. Four years later the *Elements of Gasdynamics* was published by Wiley (Liepmann & Roshko 1957). Its only edition was reissued as a Dover paperback in 2001. It has been translated into Russian, French, Spanish, Chinese, and Japanese. Its strong point has been the emphasis on fundamentals; a companion book on applications, which was to have been written by Millikan and Puckett, never happened.

As an adviser to his research students, Liepmann was always available and helpful; one did not have to make an appointment. He did not tell his students what they should do in great detail and was happy if they came up with their own suggestions. [R.N. remembers how flattered he was when he was asked (before he got his degree) to join a group for dinner to discuss next year's research agenda.] At the same time, he was tough and unsparing in his assessment of what anyone (in particular himself) could actually learn from the student's work. Once that work crossed some threshold that was quite well defined in his mind (but not necessarily in that of the students), they were basically through with their theses and degrees. More than anything else he strove to teach his students taste in scientific research. One part of it was that the experimental work carried out in his group valued and was characterized by innovation in defining the apparatus and instrumentation used, often specifically designed for the problem in hand. He believed that experimental research must relate to theoretical foundations in question, so he encouraged close associations with applied mathematicians, and also with other Caltech faculty and visitors. He was instrumental in the establishment of the applied mathematics option at Caltech in 1967. He also had a large role in the establishment of the applied physics option in 1974 and held a professorship there. Tony Leonard was brought into the group to look closely at vortex dynamics, and his presence had a significant impact on the thinking of the experimenters. All this was in support of a GALCIT research program that was driven by a quest for insight. A powerful group of experimental fluid dynamicists investigating a variety of flows seemed to Liepmann to have provided an "unfair advantage" to GALCIT, and he was going to make the best use of it!

He clearly had his own distinctive criteria for judging the quality of any work, and his judgment often disagreed sharply with the consensus in the community. A.R. has a copy of a letter dated August 2, 1999, addressed "Dear George," almost certainly intended for Batchelor; in this letter Liepmann dismissed a well-known body of work on the "isotropic homogeneous state of turbulence" in the words "I do not believe that it helps in understanding the physics and I am afraid that much of it is dry water." His general view of turbulence research is succinctly and trenchantly summarized in the first part of this letter, reproduced in Appendix 1. He thought of RANS-type computational fluid dynamics as something industry needed and found useful for lack of anything better at the present time, but unlikely to be of lasting value ["much of this huge [RANS] effort will be of passing interest only" (Liepmann 1979b)]. He did not therefore consider it either appropriate or necessary for GALCIT to get into it. He saw the future for applications as lying with large-eddy simulations (see the letter to George in Appendix 1), and this was consistent with his concept of a "turbular fluid." His view on this subject might well turn out to be not only justified but also far seeing.

Outside of fluid mechanics in some form, the subject that Liepmann most spoke or wrote about was education. That was the theme of a talk he gave on the fiftieth anniversary of GALCIT (Liepmann 1979a), an Aachen Jubilee Symposium (Liepmann 1981), a Caltech commencement address (Liepmann 1982), a SURF kick-off dinner (Liepmann 1989), and a piece he wrote as a chapter in a book called *The Dynamics of Technology* (Liepmann 2003). His ideas about what education should be in general and at Caltech in particular (which he thought of as an unusual institution) were clear. Technology was changing rapidly, and one needed a group of people who were engineers that should be “capable of specializing rapidly, rather than being specialized.” (He was himself a living example of this ability because, as shown above, his own areas of research kept changing—on a roughly decadal timescale—during his professional career.) This ability required that such engineers have a strong background in basic science, especially physics and mathematics. He saw the PhD as a degree that should broaden rather than narrow a person’s scientific outlook. It is possible that the crossing line for the student in his march toward a PhD came when he was seen as having penetrated “the subject to the state of the art, experiencing both the frustration and exhilaration in trying for new understanding.” Research and teaching were the raison d’être of a university because creative teaching was impossible without a deep interest in research. He went on to say, “Very few research results per se can compare with the lasting impact of forming the next generation through teaching and example.” Not surprisingly he saw natural science and technology as an indivisible part of human culture.

In the Caltech commencement address, he talked about building “an enclave of excellence but also of trust”; this was clearly the driving force behind the way that he created and led the GALCIT fluid mechanics group. He often said that the one thing that an administration should do—almost the only thing—was to protect those who could do from those who could not.

One reason for his extraordinary success as a teacher and leader may have been his sharp wit, often displayed in “proper” English delivered in a distinctive German accent. This was often at his own expense, but sometimes also at the listener’s (he was known as a master of the friendly insult). If you told him you are preparing a talk, his immediate response would often be “Cut it in half.” A typical greeting might be “and what is on your little mind this morning?,” followed by good natured laughter. A.R. tells the story of an unexpected meeting with his longtime friend Bill Sears on a conference breakfast line. Sears exclaimed, “Oh my God!” Liepmann looked nervously over his shoulder, “Shh, not so loud!” R.N. recalls that on the telephone call he usually got from Liepmann on the eve of a new year, the greeting “How are you, Hans?” would elicit the reply (in later years), “These days when I wake up I say ‘Ha! I am still alive!’” A sign often seen on his desk said, “In research there must be some disorder.” The boxes on his desk for sorting mail would be labeled “IN,” “OUT,” and “WAY OUT.”

A list of his students (see Appendix 2) is enough to show how they went on to become leaders in industry, national agencies, and academia across the world. One aspect of his leadership was his special view of world history and the way that different cultures had contributed to it. His time at Istanbul had made him particularly sensitive to the achievements of Islamic art and science. At Aachen, he spoke of how “the cultural elite in ancient Athens or during the height of Islamic splendour in Damascus, Cordova or Samarkand would have ostracized anybody lacking in appreciation of mathematics and astronomy.” When in India for a conference, he made a special point of visiting the mausoleum of the Moghul Emperor Akbar, of whose vision in religion and politics he was a great admirer.

CONCLUSION

It appears that Liepmann’s research agenda at GALCIT was dominated by turbulence and shock waves, presumably because he saw these as the major features of the Navier-Stokes nonlinear velocity-field theory that distinguish it from other field theories in physics. Much of his philosophy is seen in the work that his students did and in the occasional reviews he wrote as some significant progress had been achieved in the understanding of any particular subject by the work in his group. Apart from setting the agenda and standards, his involvement in his students’ work left them with considerable freedom. It is possible that it was this attitude that made him the outstanding teacher and adviser he was. He had, however, strong likes and dislikes, and he had no hesitation in letting people know about them. He seemed driven by a desire to create at GALCIT what he liked to think of as an enclave of excellence and trust, and in this objective he was entirely successful. His broad and liberal grasp of world history enabled him to have wonderful friends from every part of the world: Asia, Africa, Latin America, and, of course, Europe. He wanted his students to see science as fun ("Have fun" was often a parting substitute for "Bye" after conversation with a student), and he had lots of funny stories to tell, about scientists he knew and others. On the whole, his influence on fluid dynamics was far greater than the direct impact of his publications. This was consistent with his belief in the extraordinary value of combining research and teaching, and making an impact on "forming the next generation through teaching and example," and his emphasis on the pleasure of learning, the joy of understanding, and the importance of the urge to contribute. We believe that the more than 70 students who got their research degrees from him, the several thousands who listened to his lectures, and the even larger number who read his books (sometimes in their own languages) carried these messages to different parts of the world. Liepmann's enthusiasm, passion, and leadership impacted the larger aeronautics, fluid mechanics, and physics communities through the numerous positions of leadership that he held on national and international professional societies, government panels, and advisory boards.

Liepmann was widely honored for his many accomplishments. He was a member of the National Academy of Engineering and the National Academy of Sciences, and received the National Medal of Technology and the National Medal of Science from US presidents. He was awarded the Ludwig Prandtl Ring by the German Society for Aeronautics and Astronautics and the Guggenheim Medal in the United States. He was an Honorary Fellow of the Indian Academy of Sciences. And he won several awards as a distinguished teacher.

APPENDIX 1

Dear George:

8/2/99

The turbulence community is a peculiar part of the physics-engineering interface. Most physicists who got interested in the problem disregard any results from the engineering literature as below their dignity and for many one [*sic*] the other side the jargon of the physics community is hard to take and the aim of the research difficult to understand. This split does not apply to the top people e.g., when Feynman started working on turbulence, eh [*sic*] asked me for the empirical results on turbulent pipe flow and attempted to get the results from first principles. He did not get very far and true to his high standards did not publish anything. To me the crux of the turbulence problem are the fluxes of momentum, energy and mass. to [*sic*] put it more drastically: as long as we cannot predict pipe flow or the drag of a sphere for all Reynolds numbers we have no real theory.

The largest part of the turbulence literature today deals with semiempirical computations based on very dubious physical assumptions reminiscent of equations of state in times past. The only approach with more physical comment [*sic*] seems to me the so called "large eddy simulation." Here the large-scale motion is treated in a deterministic manner and only the small-scale motions are handled as statistical vortex interactions.

The preoccupation of the turbulent physics community seems to me the statistics of vortex interactions that is, of course, a very important aspect of fluid physics but the usual limitation to homogeneous and sometimes isotropic conditions as well, limits the connection to realistic flow problems. It is by now quite clear that unlike the equilibrium Boltzmann distribution of perfect gases, the isotropic-homogeneous state of turbulence cannot be used as the first step in a perturbation procedure a la Enskog-Chapman.

. . .

As ever

APPENDIX 2: LIEPMANN'S STUDENTS

1. Stanley Corrsin, 1942 (AE)
2. Keirn Zenn, 1943 (AE)
3. James F. Parker, 1947 (AE)
4. George R. Kronmiller, 1947 (AE)
5. Stanley Corrsin, 1947 (PhD)
6. Julian D. Cole, 1947 (AE)
7. Nateson Srinivasan, 1948 (AE)
8. Frank E. Marble, 1948 (PhD)
9. John Laufer, 1948 (PhD)
10. Dean R. Chapman, 1948 (PhD)
11. Norman Charles Peterson, 1949 (PhD)
12. Richard M. Head, 1949 (PhD)
13. Satish Dhawan, 1949 (AE)
14. David T. Barish, 1950 (AE)
15. Harry I. Ashkenas, 1950 (AE)
16. Jay William Stuart, 1951 (AE)
17. Antony J.A. Morgan, 1951 (PhD)
18. Satish Dhawan, 1951 (PhD)
19. Anatol A. Roshko, 1952 (PhD)
20. Martin S. Robinson, 1952 (AE)
21. Donald Earl Coles, 1953 (PhD)
22. William W. Willmarth, 1954 (PhD)
23. Thomas Vrebalovich, 1954 (PhD)
24. Raimo Jaakko Hakkinen, 1954 (PhD)
25. William Jeffris Williamson, 1955 (AE)
26. George Tolmie Skinner, 1955 (PhD)
27. Richard J. Magnus, 1955 (PhD)
28. Merwin Sibulkin, 1956 (AE)
29. Philip Lamson, 1956 (PhD)
30. George Stuart Campbell, 1956 (PhD)
31. Frank E. Goddard, 1957 (PhD)
32. Paul Frederik Robert Weyers, 1959 (PhD)
33. Wilfried Stockmair, 1959 (AE)
34. Bradford Sturtevant, 1960 (PhD)
35. Jacek Piotr Gorecki, 1960 (PhD)
36. William S. Chen, 1961 (AE)
37. Roddam Narasimha, 1961 (PhD)
38. Werner Preukschat, 1962 (AE)
39. Douglas S. Johnson, 1962 (AE)
40. David P. Hoult, 1962 (PhD)

41. Erik Slachmuylders, 1963 (AE)
42. Viviane Claude Rupert, 1963 (AE)
43. Harlow G. Ahlstrom, 1963 (PhD)
44. Gerold Yonas, 1966 (PhD)
45. F.Y. Sorrel, 1966 (PhD)
46. Robert Marcus Bowman, 1966 (PhD)
47. Bob Hiro Suzuki, 1967 (PhD)
48. James A. McGill, 1967 (AE)
49. Alan F. Klein, 1967 (PhD)
50. Alan Lowell Hoffman, 1967 (PhD)
51. William McKinley Robinson, 1969 (PhD)
52. Bruce Meno Lake, 1969 (PhD)
53. Samuel Ernest Logan, 1972 (PhD)
54. Viviane Claude Rupert, 1973 (PhD)
55. Javier Jimenez, 1973 (PhD)
56. Paul E. Dimotakis, 1973 (PhD)
57. Glenn A. Laguna, 1975 (PhD)
58. John R. Shea, 1976 (PhD)
59. Kwasi Kete Bofah, 1976 (PhD)
60. Lambertus Hesselink, 1977 (PhD)
61. Jack LeRoy Wise, 1979 (PhD)
62. Philip Louis Rogers, 1979 (AE)
63. Robert Edward Breidenthal, 1979 (PhD)
64. Timothy Neal Turner, 1980 (PhD)
65. Bernd Otto Trebitz, 1982 (PhD)
66. Daniel Mark Nosenchuck, 1982 (PhD)
67. John Robert Torczynski, 1983 (PhD)
68. Douglas Marion Moody, 1983 (PhD)
69. Thomas Rosgen, 1985 (PhD)
70. Stephen Taylor, 1986 (PhD)
71. H.F. Robey, 1986 (PhD)
72. Steven Philip Schneider, 1989 (PhD)

Abbreviations: AE, Aeronautical Engineer's thesis; PhD, Doctor of Philosophy.

DISCLOSURE STATEMENT

The authors are not aware of any biases that might be perceived as affecting the objectivity of this review.

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