Robert T. Jones

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ROBERT T. JONES, ONE OF A KIND

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■ **Abstract** His contemporaries saw R.T. Jones as one of the notably creative aerodynamicists of the twentieth century. This essay reviews his remarkable life and career, including his years as a farm-country boy, college dropout, and fledgling airplane designer in Missouri, his time as an elevator operator and self-directed student in Washington, D.C., and his long professional career as an aerodynamicist at the Langley and Ames Aeronautical Laboratories and Stanford University. The focus in his career is on his fundamental discovery of the benefits of sweepback for the wings of high-speed airplanes. This includes speculation about his highly intuitive thought processes in arriving at his creative ideas. I also give an account of his work on blood flow and the mechanical heart, his avocational accomplishments as a maker of telescopes and violins, and his philosophical interest in human affairs.

INTRODUCTION

Robert Thomas Jones, one of the premier aerodynamicists of the twentieth century, was born on May 28, 1910 in Macon, Missouri and died on August 11, 1999 in Los Altos Hills, California. This piece reviews some aspects of his remarkable life.

All of us who knew Jones—"R.T." to his friends, of whom I was fortunate to be one—agreed that he stood in some way head and shoulders above the rest of us. For him, more than anyone else, the word "genius" entered our conversation. We would have been hard pressed to define what we meant, but we knew he was different from the rest of us in the circumstances of his life and the novelty of his work. I hope I can give some feeling for these differences.

Recollections by R.T. himself appeared as one of these opening articles in an early volume of the *Annual Review of Fluid Mechanics* (Jones 1979). In reviewing his life and work here, I have also depended on a number of sources: an unpublished autobiographical piece by R.T. (in University Archives at Stanford University), the introduction by William R. Sears (Sears 1976) to R.T.'s collected works as of 1976, histories of the NACA's Langley Aeronautical Laboratory by James R. Hansen (Hansen 1987) and of aerodynamics by John D. Anderson, Jr. (Anderson 1997), and various unpublished patent-related materials. I have also drawn on my own and others' memories, especially for R.T.'s later years, and on reading of R.T.'s technical reports. I shall not attempt detailed references from within these sources; I shall, however, give individual reference to R.T.'s reports.

EARLY YEARS

Macon, Missouri, where R.T. was born, was a typical farming-country town of the American Midwest. The circumstances of his family, though typical of the time and place, differed markedly from things today. His grandfather, Robert N. Jones, immigrated to this country at age 17 and joined the gold rush to California. With some gold in hand, he then went east to Missouri, where he married and settled on a farm (apparently not his own) near a coal mine. Here he farmed during the summer months and mined coal during the winter.

R.T.'s father, Edward S. Jones, grew up on the farm and aspired to be a lawyer. He attended the University of Missouri to study law but decided he could educate himself by reading law and dropped out of the university after one year. After passing the bar examination and establishing his practice in Macon, he bought a farm, where R.T. spent time assisting the family who lived there in running it. R.T. writes as follows about the lack of indoor plumbing: "Most farms had outhouses, or you just went behind the barn. Young people nowadays seem to believe that living closer to nature can lead to an expanded spiritual consciousness. However, I do not remember having a single uplifting thought while trudging through the snow to answer a call of nature." Later, while running for public office, R.T.'s father traveled the dirt roads of Macon county in a buggy behind a single horse. R.T. contrasts this with his own experience flying nonstop from London to San Francisco over the polar regions behind engines of 50,000 horsepower.

Writing about his days in Macon High School, R.T. pays tribute to "a wonderful mathematics teacher, Iva S. Butler, who took us along the intricate path through exponents, logarithms, and trigonometry." Like most boys of his time, he and his friends strung wires from the house to the barn, wound coils on oat boxes, obtained Model T spark coils from junk yards, and made spark-gap transmitters. With these, he says, "We showered the ether with noisy dot-dash signals that could be heard clear across the country and beyond." For a time, he also helped a friend sell and install some of the early battery-powered radios, which were such a boon to lonely farmers.

"My consuming interest," R.T. writes, "however was aviation." He built rubber-band-powered model airplanes from the scale drawings and kits of the Ideal Model Airplane Supply Company and "devoured eagerly" the technical articles in *Aero Digest* and *Aviation* magazines. He also ordered technical reports, at 10 or 15 cents each, from the National Advisory Committee for Aeronautics (NACA), little suspecting that for most of his life it (and its successor, NASA) would be his employer.

Following high school, R.T. attended the University of Missouri, but found it unsatisfying and, like his father, dropped out after one year. A stunt-flying group typical of the time turned out to have more appeal. A Macon taxi driver, Charles Fower, had bought a wartime-surplus Standard J1 training plane, learned to fly it, and gave stunt-flying exhibitions. In doing so, Fower met Marie Meyer, another exhibition pilot, and together they formed the "Marie Meyer Flying Circus." R.T. writes that "It was quite a thrill to see Charley fly upside down over Macon and come out of loops seemingly below ground level in a deep railroad cut." R.T. joined the Circus, where he received flying lessons in exchange "for carrying gas and patching wing tips," though he would not fly solo until more than fifty years later. He never returned to the university, and his only college degree would be an honorary doctor's degree.

In 1929, the small Nicholas-Beazley Airplane Company in nearby Marshall was beginning production on the Barling NB3, a new type of three-place low-wing all-metal (except fabric-covered) monoplane designed by Walter Barling, a British engineer of some note. Barling had recently left the company, which found itself without an engineer. Charley Fower, who was a friend of one of the company's owners, recommended R.T. for the job, saying that he "knew everything there was to know about airplanes." As R.T. puts it, "I was hired immediately; 19 years old, a college dropout, and chief (or only) engineer at a salary of 15 dollars a week." Later the company hired Thomas Kirkup, a more qualified engineer from England, and Kirkup taught R.T. about airplane design, especially stress analysis. For several months he also "worked from early morning until midnight" in the design of a small racing plane for the 1930 air races. These experiences apparently fostered R.T.'s ambition to become a skilled engineer. Nicholas-Beazley was successful for a time, selling as many as one plane a day. However, in the early 1930s in the Great Depression, people stopped buying personal airplanes, and the company, like many others, went out of business.

WASHINGTON, D.C.

R.T. returned home to Macon, where he spent his time studying books on aerodynamics, such as Hermann Glauert's *Aerofoil and Airscrew Theory* and Max Munk's *Fundamentals of Fluid Mechanics for Aircraft Designers*. Needing work, however, he obtained a ride with neighbors to Washington, D.C., where his local congressman provided him with a "wonderful" job as an elevator operator in the House Office Building. With typical dry humor, he writes that the "ups and downs of this job" gave him an opportunity to observe the inner workings of the government. Realizing that he would need to know considerable mathematics to be a successful engineer, R.T. used his spare time in the nearby Library of Congress studying original works on various topics. These included Sir William Hamilton's quaternions, an esoteric element in four-dimensional vector analysis actually of negligible use in applied mathematics. He also struck up an acquaintance with A.F. Zahm, a well-known aerodynamicist in charge of the Library's aeronautics collection and an ex-member of the NACA. One day a Maryland congressman, David J. Lewis, got on R.T.'s elevator and asked if R.T. would tutor him in mathematics. Congressman Lewis had asked Zahm for help with some mathematical problems of his own devising, and Zahm had referred him to the elevator operator in the Congressman's building. Congressman Lewis was then age 65 and completely self-taught, with no formal education of any kind. R.T. brought him through algebra and up to calculus, "learning a lot from him on the way."

After arriving in Washington, R.T. also visited the brilliantly creative but difficult Max M. Munk. Munk had received his doctorate in aerodynamics with Ludwig Prandtl at Göttingen and worked for the NACA at its laboratory at Langley Field. He was now without a formal job but working as a consultant and patent attorney and giving evening graduate courses in aerodynamics at Catholic University. When told that R.T. had studied his book on fluid mechanics (he was surprised it had penetrated as far as Missouri), he suggested that R.T. take his courses at the University. When R.T. said he was probably not qualified because he had no undergraduate degree, Munk asked him to define the derivative of a function. He succeeded and Munk proclaimed him qualified. R.T. took Munk's courses for three years. They would have a profound influence on his later achievements.

THE LANGLEY LABORATORY

In 1934 the new Public Works Program, started by President Roosevelt to help combat the depression, made available a number of nine-month positions as Scientific Aid at the NACA's Langley Aeronautical Laboratory in Virginia. With the recommendations of Zahm, Munk, and Congressman Lewis, R.T. obtained one of these positions. By the time his nine months were up, his exceptional talents had become apparent, and his supervisors retained him at subprofessional levels by temporary and emergency reappointments. A permanent professional appointment as Engineer at the initial civil-service grade, however, required a Bachelor's degree. A professional appointment thus seemed impossible until someone noticed that the next higher grade, usually attained by promotion from the initial grade, had no such stated requirement. Therefore, in 1936 he was promoted directly to that level and could see himself as having achieved his ambition to become an engineer. He writes, "At that time the inflation of the language had not yet reached the point where we were called 'scientists.'" Thus began R.T.'s career with NACA and its successor NASA, which, except for a period in the 1960s, would occupy him until his retirement in 1982 from the Ames Research Center in California. As the late William Sears observed, "The fates had their way: It required a sequence of unlikely events and remarkable people, but Robert T. Jones was embarked on a career in research in the NACA. . .."

R.T.'s work in his first ten years with the NACA dealt mostly with airplane stability and control, in which he became a recognized authority. This took place under the supervision of, and sometimes in collaboration with, the practical-minded stability-and-control expert Fred E. Weick, Assistant Chief of Aerodynamics at

Langley. Although he quickly became sophisticated mathematically, R.T. does not hesitate to write about his naiveté early on. He tells about how he got the wrong sign in what he thought was a "clever" theory for the yawing moment due to roll, the correct result for which had been found back in 1918. When an associate noticed the error, R.T. rethought his incorrect resolution of the local velocities relative to the wing sections and got the correct result. Near the beginning of their work, Weick also asked R.T. to calculate the transient motions of an airplane following deflection of the ailerons. Because no one in their group knew how to solve the differential equations analytically, R.T. set up step-by-step numerical calculations, carried out laboriously by human operators with desk calculating machines. When Weick wondered what would happen if they doubled the aileron deflection, R.T. had the calculations repeated with twice the original input. He writes: "In spite of the complex interactions of rolling, yawing and sideslipping, every number in the new tables was exactly twice that in the first table! Afterward when anyone told me that a system was 'linear,' I knew exactly what he meant."

R.T.'s lack of knowledge of applied mathematics rapidly disappeared. He quickly became a pioneer in the application of Oliver Heaviside's operational methods in the theoretical analysis of the transient motion of airplanes following a transient disturbance. In this he introduced some perceptive, ingenious, and mathematically sophisticated procedures. By 1944 he had published, alone or occasionally in collaboration, about twenty reports on stability and control, mostly theoretical but some involving discussion of related wind-tunnel and flight results. One of these was an exhaustive resumé and analysis of NACA lateral-control research written in collaboration with Fred Weick (Weick & Jones 1937). Weick also asked R.T. to see if he could design a satisfactorily maneuvering airplane with simplified controls on the assumption that it should not require both hands and feet to move them. The main question was whether to do this by dispensing with the rudder or the ailerons. R.T.'s analysis showed that two-control operation would best be achieved by controlling the ailerons instead of the rudder, preferably with a small amount of rudder movement linked directly to the aileron motion. This scheme had been anticipated by the Wright brothers, with wing warping instead of ailerons. After leaving Langley, Weick used the concept to design the famously successful two-place low-wing Ercoupe, which went into production in 1940. R.T. would own an Ercoupe late in life, when he finally learned to fly.

In his first ten years with the NACA, only three of R.T.'s publications were related to aerodynamics, all concerning incompressible flow. He says that he was "no expert on compressible flow." In the mid-1940s that would quickly change. He would spend the remainder of his career mostly in high-speed (i.e., compressible) aerodynamics, coming up at the outset with a fundamental concept for the aerodynamic design of high-speed aircraft.

This idea originated in 1944 in the course of a wartime assignment to help develop guided missiles. The Ludington-Griswold company of Saybrook, Connecticut was designing a dart-shaped glide bomb conceived by a Russian émigré, Michael Gluhareff, and having a wing of narrow triangular planform. The company's engineers had calculated the aerodynamic lift of the wing using Ludwig Prandtl's time-honored lifting-line theory; they had misgivings, however, because the theory was devised for wings wide relative to the flight direction. In a visit to Langley, the president of the company asked R.T. if he could think of a better way to calculate the characteristics of a long and narrow triangular wing, pointed in the direction of flight. While puzzling about the problem, R.T. remembered a paper from 1924 "by my teacher Max Munk" in which the forces on a long, narrow body of revolution (an airship hull) at angle of attack had been analyzed on the assumption that the flow in planes perpendicular to the flight direction could be treated as two-dimensional. With this assumption, the only novelty for the flat wing, though far from a trivial one, was how to satisfy the Kutta condition at the trailing edge—i.e., the condition that there be no flow around the sharp edge. R.T. devised a way to do this, and the rest was relatively simple. So simple, in fact, that he thought "nobody would be interested in it," put the analysis in his desk drawer, and temporarily forgot about it. (I shall attempt later to characterize R.T.'s mode of thinking. For the present we will simply trace the sequence of events and ideas.)

R.T.'s slender-wing analysis, like Prandtl's lifting-line theory, used the linear equations of incompressible (i.e., low-speed) flow. A few months later, in early 1945, while exploring the more complex nonlinear equations of compressible flow, he realized that introducing the approximation of the long, narrow wing gave him the same results as his incompressible analysis. This implied that for slender wings there was no effect of Mach number. In particular, there was none of the undesirably large increase in drag characteristic of straight wide-span wings at flight speeds approaching and exceeding the speed of sound. This result brought to R.T.'s mind a 1938 paper by H.S. Tsien of Caltech showing that the lift of slender bodies of revolution at angle of attack exhibited no effect of compressibility at supersonic speeds. The result for wings nevertheless came as a surprise.

The surprise, however, did not stop there. In trying to understand physically why the slender wing should show no effect of Mach number, R.T. wondered if it might have to do with the large sweepback of the wing's leading edge. Again he remembered another paper by Max Munk, this one dealing, in 1924, with the effect of dihedral and sweepback on the performance of wings at low speeds. In it, Munk assumed, as sufficiently obvious to need no discussion, that the air forces on a swept wing of large span and constant chord depend only on the component of flight velocity perpendicular to the leading edge and are independent of the component parallel to it. R.T. wondered if this independence principle might not apply also to the components of the flight Mach number (i.e., in high-speed flight) and decided that it did. Thus, the effective Mach number, on which the air forces depend, decreases continuously with increasing sweep; it follows that, even at supersonic flight speeds, the air forces can be made to have the advantageous properties found at low subsonic Mach numbers simply by introducing sufficient sweepback. In particular, that the enormously increased drag of conventional unswept wings at supersonic speeds can be reduced to subsonic levels. R.T. thus discovered the theory of high-speed sweepback, which William Sears describes as "certainly one of the most important discoveries in the history of aerodynamics." The planform of every high-speed transport one sees flying overhead embodies R.T.'s idea. When it became known, it came as a mind-boggling surprise to the rest of us working in aerodynamics.

With his new insights, R.T. quickly resurrected his incompressible slender-wing analysis, modified it to start from the compressible-flow equations, and added his reasoning about sweepback. As customary at Langley, the resulting report was then submitted, in April 1945, to an editorial committee, chaired in this case by the laboratory's top theoretician, Theodore Theodorsen. To R.T.'s surprise, the committee accepted his special slender-wing theory but rejected his general finding about sweep. Subsonic and supersonic flow were conceived at that time as of an entirely different nature, and Theodoresen could not accept that an essentially subsonic result could be obtained in a supersonic free stream. He thought that R.T.'s reasoning was too intuitive and called for less "hocus-pocus" and more "real mathematics." He described the finding about swept wings in particular as "a snare and a delusion." He insisted that R.T. remove the part about sweep from the paper. This was done and Langley transmitted two separate reports, one on slender wings and one on sweep, together with the editorial committee's opinion, to NACA's Washington headquarters for publication. The report on slender wings appeared promptly as an unrestricted NACA Report dated at Langley Field in May 1945 (Jones 1946a). Headquarters, however, accepted the negative recommendation of Theodorsen and his committee on the sweep report and delayed publication.

With his usual practical outlook, R.T. had in early March suggested to Langley's chief of research, Gus Crowley, that high-speed experiments be run on swept wings. This was done by one of the stop-gap means used for such work before the development of transonic wind tunnels: wings were attached to a spring balance inside a heavy cylindrical body that was dropped axis-wise from an airplane at high altitude and the balance readings transmitted to the ground. The results became available at the end of May and showed 45-degree swept wings to have much less drag than straight wings. This convinced headquarters to issue the sweep analysis, which appeared initially in circulation-restricted reports in late June and early July of 1945 and as an unrestricted report in 1946 (Jones 1946b). R.T.'s reports on slender wings and sweep are among the most consequential in the history of aerodynamics.

At the time of R.T.'s work, no one in the United States appears to have been aware that the German aerodynamicist Adolf Busemann had used the independence principle to examine the high-speed possibilities of sweep in his lecture to the important Volta Congress in Rome in 1935. Eastman Jacobs, a colleague at Langley, and Theodore von Kármán and Hugh Dryden, likewise from the United States, had attended the meeting but failed to recall Busemann's discussion, which was somewhat obscure and combined with a number of other topics. At the time of the meetings of the editorial committee, one of R.T.'s colleagues came upon a British translation of Busemann's lecture, which, however, had considered only supersonic flight speeds and sweep angles for which the effective Mach numbers remained supersonic. Busemann's lecture apparently gave Theodorsen, if indeed he learned of it, no cause to change his objection to R.T.'s more general view of sweep. In early May 1945, while the events at Langley were taking place, a group of American engineers investigating German wartime research also came upon a large collection of swept-wing data from high-speed subsonic wind tunnels at Busemann's institute at Braunschweig. R.T.'s idea of high-speed sweepback occurred independently of German thinking, however, and he and Busemann are credited jointly with the concept.

THE AMES LABORATORY

In early 1946, R.T. transferred from Langley to the NACA's Ames Aeronautical Laboratory, just south of San Francisco. It was there that I came to know him when we occupied offices across the hall from each other in the building for the new 1-by-3-foot Supersonic Wind Tunnel, the NACA's first sizeable supersonic facility. We spent numerous hours together in the next few years laying out what we hoped would be optimum swept-wing configurations for testing in the tunnel. From this we became friends and learned a great deal about the details of swept-wing design. Except for seven years in the 1960s, when he went elsewhere and worked outside aeronautics (as described later), R.T. was employed at Ames until his formal retirement in 1981. After that he served until 1997 as a consulting professor in the nearby Department of Aeronautics and Astronautics at Stanford University, at the same time maintaining an informal relationship with Ames.

R.T.'s work at Ames and later at Stanford dealt mostly with sweepback. However, his nonsweep concerns also have fundamental importance. One of these dealt, by means of a sophisticated analysis, with a basic mathematical singularity that occurs when thin-airfoil theory is applied to an airfoil having a rounded leading edge (Jones 1950). Previously unnoticed, it is essential for correct calculation of the airfoil's drag. Another contribution put the "area rule" for flight speeds near the speed of sound, which transforms the pressure-drag problem for a wing-body combination into that for an equivalent body of revolution, on a firm theoretical foundation and extended it to supersonic flight speeds (Jones 1956a). Both these topics appear in any complete text in aerodynamic theory.

R.T's extensive work on sweep after his move west shows a wide range of concerns. For the first ten or so years, his papers dealt mostly with the theory, elaborating its foundations and techniques and exploring its results. Typical titles are "The Minimum Drag of Thin Wings in Frictionless Flow" (Jones 1951) and "Theoretical Determination of the Minimum Drag of Airfoils at Supersonic Speeds" (Jones 1952). All but one of the papers employed linearized (thin-wing) theory of a frictionless fluid. The exception examined the effects of sweep on the laminar boundary layer, where he discovered another, more qualified independence principle (Jones 1947). In 1957, he and Doris Cohen incorporated these and the findings of others into an important 241-page section with the title "Aerodynamics of Wings at High Speeds" for the Princeton series *High Speed Aerodynamics and Jet Propulsion* (Jones & Cohen 1957).

In the second half of the 1950s, R.T.'s concerns began to shift from sweep theory itself to the implications of sweep and other theoretical findings on the design of airplanes for high-subsonic and supersonic flight. He voiced his ideas especially in papers at meetings—at Brooklyn and Göttingen in 1955 (Jones 1955, 1956b) and Madrid in 1958 (Jones 1959). In various writings of this period and earlier, including the section in the Princeton series and the paper from the talk in Madrid, he mentioned the idea of an oblique (or yawed) wing, though mainly as a matter of theoretical interest rather than a practical configuration. Following his return to Ames from his absence in the 1960s, however, this would change, and he would devote himself wholeheartedly to the startlingly unconventional concept of the oblique-wing airplane and its potential as a useful device.

The planform of a conventional swept wing, as we see it flying overhead, has bilateral (mirror) symmetry, that is, it is swept back on both sides of the fuselage (or plane of symmetry). But nothing in the concept of sweep requires such configuration. Sweep can equally well be embodied in a wing that is swept back on one side and forward on the other, that is, a wing that is oblique to the line of flight. In fact, there was reason to suspect, and R.T.'s theoretical work had shown, that such an oblique wing would have superior aerodynamic performance at high speed to one of conventional planform. Because both the conventional and the oblique wing present problems at the much reduced speed of landing, however, there may be virtue, especially for the large sweep needed for supersonic flight, in having a wing adjustable at landing to the zero sweep of low-speed aircraft. Such adjustment would be mechanically and structurally easier for an oblique wing with a single pivot atop the fuselage than for a conventional swept wing with its required pair of pivots, one on each side. However, besides its potential aerodynamic and mechanical virtues, the oblique wing raised questions about stability and control and the resulting handling (piloting) qualities and about aeroelastic deformation and hence structural design. The oblique wing thus presented a wealth of problems for study.

Whether the concept of the oblique wing was original with R.T. is not clear. The idea was current in the sweep developments in Germany during the war. The first mention in the United States is in a NACA report, dated in mid-1946, by John P. Campbell and Hubert M. Drake on stability-and-control tests of an oblique-wing airplane model in the Langley low-speed free-flight wind tunnel (Campbell & Drake 1947). The report states in typical NACA impersonal fashion that "it has been proposed" that such an airplane be flown, but R.T. has confessed that he promoted the tests, though somewhat secretively from Langley management. Whether he had heard of the German work before these tests we do not know. In any event, the idea was—and still is—startling because, as R.T. wrote, "Artifacts created by humans show a nearly irresistible tendency for bilateral symmetry."

A large body of work has grown up concerning the oblique-wing airplane in the past half century, much of it at Ames and Stanford under R.T.'s inspiration. The first high-speed tests of the configuration were run in the Ames 11-foot transonic wind tunnel in 1958, even before R.T.'s absence from Ames in the 1960s. These tests compared an oblique wing with a conventional sweptback wing, both swept at 40° and mounted on a fuselage, and found, as expected, that the oblique wing had much the smaller drag at sonic flight speed. In the early 1970s, R.T. was a coauthor with wind-tunnel experimentalists of a number of other tests in the 11-foot tunnel. In several papers on design in the 1970s, he also used his sweep and slender-wing theories to examine the relative merits of different airplane configurations at supersonic speeds. One such paper, on a study done by Boeing under NASA contract, was coauthored with James W. Nisbet, a Boeing engineer (Jones & Nisbet 1974). The Boeing results for five different configurations were also useful in several other papers to show the superior performance of the oblique wing. Throughout his papers on design, R.T. also gave careful consideration to stability and control and aeroelasticity. However, aerodynamic theory was not discussed much beyond citing its results—for R.T., the theory had served its purpose. In 1971–72, to gain experience in stability and control, R.T. also designed and built (but had someone else fly) radio-controlled flying models of oblique-winged airplanes, "taking advantage of the highly sophisticated techniques and equipment developed in recent years by model-airplane builders" (R.T.'s words). To these he necessarily added a mechanism for pivoting the wing in flight. In the 1980s, Ames and the NASA Dryden Flight Research Center in southern California, with R.T.'s design advice, built and flew an oblique-wing single-seat aircraft in low-speed tests of its control response and pilot feel (Figure 1). (This airplane is now on exhibit at the Hiller Aviation Museum at San Carlos, California.)

The foregoing work on the oblique wing concerned a wing with a fuselage and tail. R.T.'s interests following his formal retirement at Ames centered on the even more startling concept of the oblique flying wing. This consists simply of an oblique wing without fuselage or tail and large enough to carry its load internally. Here the variable sweep must be achieved by aerodynamic means, and the wing-mounted engines must be pivoted so they can be pointed in the direction of flight. The concept had been in R.T.'s mind for some years; he included it in his mention of the practical advantages of sweep in his 1958 talk in Madrid. This he accompanied with a striking demonstration of the low-speed stability of an oblique flying wing by means of a balsa-wood glider. R.T took special delight in later years in flying such gliders before his invariably skeptical audiences (Figure 2).

STANFORD UNIVERSITY

Though he had mentioned the idea earlier in several papers, R.T.'s work on the flying wing took place mainly in the late 1980s and early 1990s, much of it in his association with Stanford. His own writing on the subject was limited to a paper of 1991 (he was 81 at the time) to bring the configuration and its advantages and problems to a broad aeronautical audience (Jones 1991). Mostly, however, he

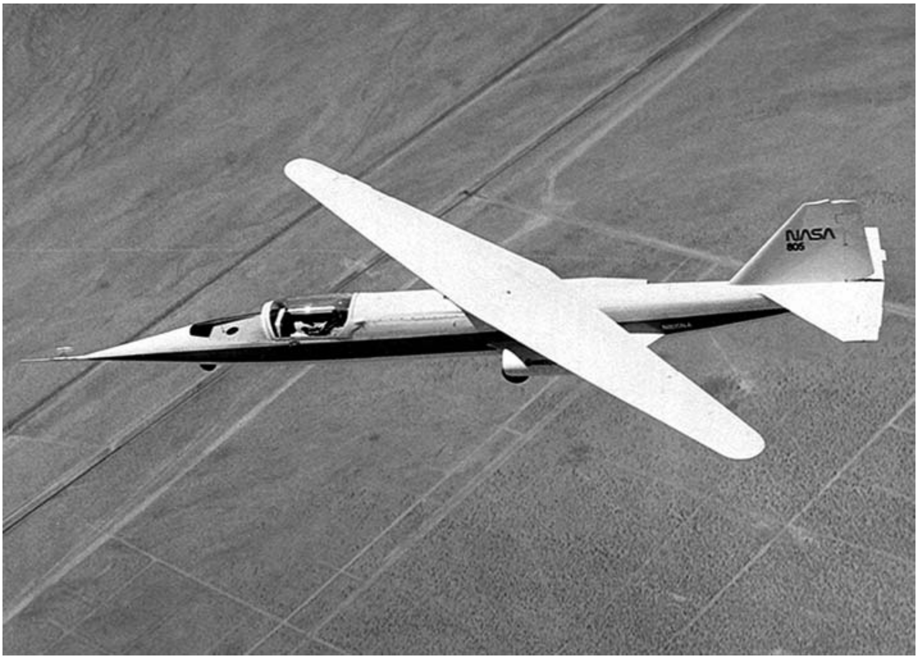

Figure 1 The NASA subsonic pivoted-wing AD-1 aircraft in flight at the Dryden Flight Research Center.

served as an advisor to people at Ames and other local research groups and to Professor Ilan Kroo and his doctoral students at Stanford. Kroo and his students, with R.T.'s inspiration and help, studied the aerodynamic-design and control-system problems in detail. The students also built and flew a radio-controlled model to become familiar with the low-speed flight characteristics. (The model is now also at the Hiller Aviation Museum.) Kroo and three engineers at local research companies did a comprehensive design layout to examine the packaging within the aircraft of the payload, fuel tanks, landing gear, and other internal components and their strong coupling to the exterior aerodynamic geometry. The resulting Mach 1.6 aircraft would accommodate 440 passengers inside a wing of 400-foot span (Figure 3).

Thus, thanks to R.T.'s impetus and vision, there now exists a large body of knowledge of possible oblique-wing airplanes in both the pivoted and flying-wing versions. In the course of this work, the stability-and-control and aeroelastic problems that accompany the oblique wing have been solved. Sears, writing in 1976 with regard to the pivoted wing, said "I, for one, fully expect to see future transport airplanes with 'Jones oblique wings.'" Though aircraft companies have studied the possibilities, what Sears expected has not, for a complex of reasons, come to the pass with either version. The crystal ball for the future is unclear.

Figure 2 R.T. flying his balsa-wood oblique-flying-wing glider in his backyard at Los Altos Hills, California.

Figure 3 Painting of possible oblique-flying-wing aircraft.

In addition to advising doctoral students, R.T. offered an occasional quarter-long lecture course at Stanford on problems in aerodynamic theory. In relation to this, in 1990 he published a book entitled simply *Wing Theory* (Jones 1990). Sears, on the flap of the dust jacket, calls it "surely. . .one of the most important books on aerodynamics written in our time." In 200 pages with numerous figures and frequent comparison with experiment, R.T. focuses on the basic principles and principal findings of the theory at both subsonic and supersonic speeds. To do so, he uses a minimum of mathematics and a great deal of the intuitive physical thinking characteristic of all of his work. It is a book that only R.T. could have written.

As I have tried to make clear, R.T.'s work at Langley, Ames, and Stanford showed a wide range of concerns. In much that I have described, R.T. gave attention not only to theory but to its interrelation with experiment and design. Even his predominantly theoretical papers usually elaborate the mathematical techniques and explore the theory's consequences. His most mathematically sophisticated papers are generally intuitively grounded, and some of his theoretical studies cite experimental results by other investigators to strengthen his theoretical findings. An example of the latter is his mainly mathematical paper on the effects of sweep on the behavior of a laminar boundary layer, where he includes the results of wind-tunnel tests of oblique circular wires (Jones 1947). In a purely experimental paper, he also appears as coauthor with two wind-tunnel experimentalists, presumably as collaborator in planning the tests and analyzing the experimental data (Graham, Jones & Boltz 1973). And in his design-oriented papers he used his sweep and slender-wing theories to examine the relative merits of different airplane configurations. An example here is the paper with James Nisbet, the Boeing engineer (Jones & Nisbet 1974). R.T. clearly enjoyed an uncommon range of interests and abilities. (This range is further emphasized by papers shortly before and after his retirement at Ames on such miscellaneous topics as the motion of ultralight aircraft in vertical gusts, the dive recovery of hang gliders, the aerodynamics of flapping wings, and the efficiency of small transport aircraft.)

INTUITION AND EXPLANATION

For fluid-mechanics readers, it is tempting to speculate about how R.T. arrived at his ideas. However they arose, all of us who knew him agreed that he thought differently than the rest of us. I recall a committee meeting in Washington in the late 1950s when the chair called on him to explain one of his new ideas. R.T. started by saying, "It's really very simple," whereupon the chair interrupted with, "Whoa! Do you mean simple or Jonesian simple?" Let us look more deeply at the beginnings of R.T.'s most influential ideas.

In the initial publication of his slender-wing theory (Jones 1946a), R.T. states his basic assumptions—two-dimensional flow in planes perpendicular to the flight direction and how to introduce the Kutta condition at the trailing edge—without physical or mathematical argument. With regard to the first of these, he asserts simply that "The flow [about the wing] may be considered two dimensional when viewed in cross sections perpendicular to the longitudinal axis." Though he did later attribute the assumption to remembering the airship analysis by Munk, in his initial report he gives only incidental mention of Munk as having dealt with a related problem. In any event, Munk provided no justification either. They both seem to have considered the assumption self-evident. Regarding his innovative assumption corresponding to the Kutta condition, R.T. does give an explanation, but—for me at least—an incomplete and unsatisfying one. And years later, in his *Wing Theory*, he still finds no need to explain either assumption. Sears apparently agrees with my puzzlement when he writes, "The rest of us have never found that the extension [of Munk's airship theory], involving as it does the recognition and treatment of the trailing vortices that lie behind all trailing edges, is at all 'obvious.'" R.T. appears, however, to have had no problem with the assumptions or deep-seated need to explain them.

In his sweep work, R.T.'s approach is strikingly different. His initial publication (Jones 1946b) provides—for his own understanding, I imagine, as well as ours—three physical explanations and one mathematical derivation. (His later writings do more or less the same.) The physical explanations in the order in which they appear in the report are as follows:

(*a*) The first is the "independence principle" described earlier, though R.T. did not call it that at the time. Over the years, he obviously considered this the primary explanation, probably because it indicates how to proceed analytically. The statement of it in his initial report is less clear than the following from *Wing Theory* (Jones 1990, p. 93):

> Suppose that [an infinitely long wing of constant cross section] is initially in a stream with the oncoming flow velocity at right angles to its leading edge. It is clear that if the fluid is frictionless and all sections of the wing are alike, an axial or endwise motion [of the wing] may be introduced without causing any additional motion of the fluid, since the axial motion results only in a sliding motion of the surface parallel to itself. The combination of crosswise and lengthwise velocities results in an oblique relative motion. Conversely, in the oblique motion of a cylindrical wing, the flow will be determined solely by the component of velocity perpendicular to the leading edge.

Though R.T. would later credit inspiration for this idea once again to a paper by Munk, in his initial report on the subject he gives an even more incidental mention of Munk than in the slender-wing case. In the paper that R.T. refers to, Munk again gave no physical explanation; R.T. obviously felt that such explanation was essential in this case.

(*b*) R.T.'s second explanation is more a plausibility argument than an indication of how to proceed. As had long been understood, elementary pressure signals propagate at the speed of sound relative to a fluid. In a subsonic oncoming stream, the disturbance from an unswept wing (or any obstacle) therefore propagates upstream against the flow and causes the streamlines to begin to curve to avoid the obstacle, thus keeping the drag to a desirably low value. In a supersonic stream, by contrast, signals are confined to a Mach cone originating at a point of disturbance. R.T., in figure 2 of his report, reproduced here as our Figure 4, considers an infinitely long wing immersed in a supersonic stream of speed V and swept at different angles in relation to a Mach line (the trace of a Mach cone in an axial plane). For angles of sweep β less than that of the Mach line (Figure 4*a*), the leading edge cannot affect the approaching flow, which thus impacts the airfoil undisturbed, resulting in a large drag. For angles of sweep greater than that of the Mach line (Figure 4*b*), to quote R.T., "although the fluid directly upstream from a given section can receive no pressure signal from this section, the flow behaves as though it did receive such signals because of the successive influence of similar sections farther upstream along the airfoil. The streamlines will thus be caused to curve and follow paths appropriate to a subsonic flow, although the speed is everywhere supersonic." The drag can be expected to be correspondingly low.

This Mach-line explanation appears alone in a patent statement witnessed and dated on February 27, 1945. This document is R.T.'s earliest

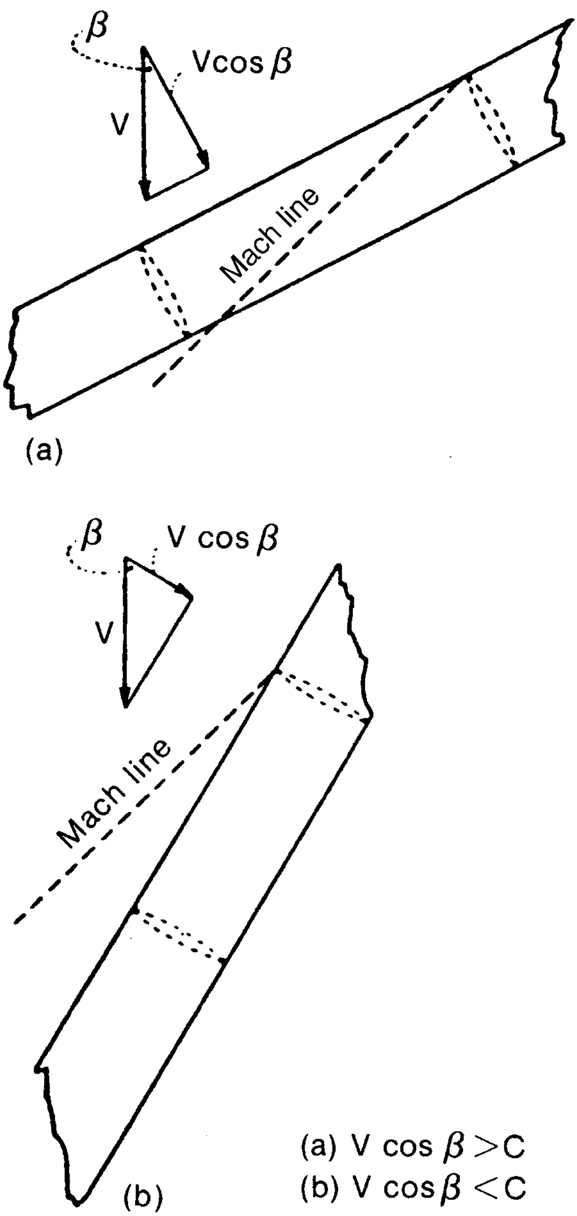

Figure 4 Angles of sweep in relation to Mach line (from figure 2 of Jones 1946b).

known statement of his sweep concept. The independence principle appears added as a second explanation in a revision in which the figures are dated March 9, 1945. One can only speculate as to which explanation occurred first in R.T.'s mind.

(*c*) The report's third argument, what might be called a consistency analysis, traces the path of streamlines over a wing having biconvex cross section and swept behind the Mach line. R.T. then argues qualitatively that a streamtube defined by adjacent streamlines, when viewed jointly in plan and section, follows the contraction-expansion behavior well-known from one-dimensional subsonic-supersonic flow. I find this argument as R.T. explains it here (and in later writings) difficult to follow. This may be an example of what the committee chair meant by "Jonesian simple."

The mathematical derivation that accompanies these three physical explanations considers a long wing at an angle of yaw in a subsonic or supersonic stream. Starting from the full three-dimensional equation for inviscid compressible flow, R.T. introduces the usual small-disturbance approximations for a thin wing plus the additional assumption that variations in the lengthwise direction are negligible. He then shows that the equation reduces to the subsonic thin-airfoil equation in the plane perpendicular to the length if the free-stream component normal to the leading edge is subsonic and to the supersonic thin-airfoil equation if the normal component is supersonic. The lengthwise assumption is, of course, consistent with the physical situation of the independence principle. In his paper, R.T. puts this mathematical derivation ahead of the physical explanations and begins his discussion of the first of these, the independence principle, by saying that the mathematical results are "a special case of a more general statement." One wonders if the mathematics might actually have come into R.T.'s mind after the physical concepts and been inserted in the report in response to Theodorsen's call for less "hocus-pocus" and more "real mathematics."

Of course, one cannot say for sure how anyone produces ideas. In the foregoing work, as throughout R.T.'s writings, insightful simplifying assumptions and multiple physical explanations are characteristic. Those who worked with him had no doubt that his thinking was basically intuitive. He could use highly sophisticated mathematics when necessary, but he did so mostly to support his ideas and explore their details and consequences. His physical explanations of his ideas could be distressingly obscure at times. Things that seemed clear and obvious to him often caused the rest of us difficulty and struggle to master. As Sears has written, "Lesser aerodynamicists often find his arguments too concise and the literature of the field includes papers in which authors redo Bob's work providing longer proofs, and discover again Bob's results." And I recall hearing Max Heaslet, a colleague at Ames, tell how he had listened to a vigorous conversation between R.T. and Adolf Busemann, who was visiting the laboratory, about their current ideas. They talked mostly in terms of metaphors and physical analogies and seemed to understand each other perfectly. Max said he could hardly tell what they were talking about.

R.T.'s interests, however, went beyond simply aerodynamics. From his boyhood days in Missouri, his passion was for airplanes. His papers frequently include airplane-performance calculations as affected by his aerodynamic ideas; an occasional paper focuses entirely on comparative performance calculations. Such design-focused papers predominate in his work on the oblique wing. In his devotion in his later years to the flying wing, he was, apart from his *Wing Theory*, completely focused on a type of airplane. As pointed out, this concern for airplane-design problems went beyond performance to include stability and control as well as structural matters.

However he arrived at his ideas, R.T.'s way of looking at things was typically that of an engineer, albeit an unusal one. His concern for aerodynamic theory was deep and abiding, but at bottom it was as a tool for the design of high-performance airplanes, not an end in itself. That is, his ultimate goal was to facilitate the design of a practical device. This is typically an engineering occupation.

OTHER MATTERS

R.T.'s achievements in fluid mechanics went beyond aerodynamics. In his absence from Ames from 1963 to 1970, he worked at the Avco Everett Research Laboratory in Massachusetts, where he was chair of the Laboratory's Medical Research Committee. As such, he studied the characteristics of blood flow in the human body and the application of such knowledge to the design of cardiac-assist devices. He also served as a codirector of studies directed toward developing one of the first artificial hearts. These efforts led to a number of articles in medical and biomechanical publications (e.g., Jones 1970) and to a survey article entitled simply "Blood Flow" in Volume 1 (1969) of the *Annual Review of Fluid Mechanics*.

R.T.'s studies also went beyond fluid mechanics. Pieces by him appeared at various times in physical journals under such titles as "Analysis of Accelerated Motion in the Theory of Relativity" (Jones 1960), "Conformal Coordinates Associated with Space-Like Motions" (Jones 1963), and "Relativistic Kinematics of Motions Faster than Light" (Jones 1982). These writings fall outside the present writer's ability to explain or assess.

As his construction of radio-controlled flying models of pivoted-wing airplanes might suggest, R.T. also had a talent for craftsmanship. In his spare time in the 1950s, he took up the study of optics and learned to grind spherical mirrors. He also devised and constructed an improvement on a type of reflecting telescope and published a number of related articles (e.g., Jones 1957). In 1957, he and his wife formed the Vega Instrument Company employing several technicians. The company made and sold some 40 six- and eight-inch telescopes of this kind (though it didn't make much money).

Also in the 1950s, R.T.'s daughter Patty reached the point in her violin studies that she needed a better but discouragingly expensive instrument. With characteristic resourcefulness, R.T. undertook to make her one. After putting together equipment for electronic acoustic testing, he made a first attempt that turned out to be disappointing. His second effort, however, was a notable success. Patty has since played it in recitals and as a member of the La Jolla Civic Symphony. Over the years, R.T. built more than a dozen fine violins and violas. He also made an electronic violin that a physicist and violinist friend in Berkeley "thinks is very good" (R.T.'s words).

If there was any doubt about the matter, R.T's passion for airplanes became unmistakably visible in the mid-1980s (R.T. was in his mid-70s), when he obtained a pilot's license and bought the two-place Ercoupe mentioned earlier. This airplane was billed as being stall-resistant. R.T. found that this was indeed so when he flew alone. When he first tried to demonstrate this to a passenger, however, they were startled to find the stall frighteningly attainable. Back on the ground, R.T., by adjusting the horizontal controls, corrected this and made the plane live up to its billing fully loaded. In line with such interests, he also went frequently (though not in his Ercoupe) to the annual Experimental Airplane Association Fly-In at Oshkosh, Wisconsin, where he gave occasional talks on airplane aerodynamics.

Besides his interests in airplanes, telescopes, and violins, R.T. read widely and thought seriously about human affairs. This is exemplified by a letter he wrote to *The New York Times* in 1986 giving an unusual, historically based view of why the "Star Wars" ballistic-missile defense was unlikely to succeed (Jones 1986). A second example is an extraordinary piece entitled "The Idea of Progress" that he contributed to *Astronautics & Aeronautics*, the journal of the American Institute of Aeronautics and Astronautics, on the occasion of the 50th anniversary of the Institute in 1981 (Jones 1981). In it he recounts something of the advances in his lifetime of technology, especially aeronautics, plus the concurrent increases in food production, improvement in living standards, and the related and troubling increase in population and appearance of the nuclear bomb. Along the way he contrasts all this with the lack of change in politics and the general conduct of human affairs. He cites Gerard O'Neill as telling how a scientist brought to life from 200 years ago would be completely bewildered by what he saw, whereas a politician would recognize perfectly well the kind of thing that was going on. In the course of the discussion, R.T. mentions or draws on the ideas and writings of such a disparate group as (in the order in which he calls on them) R.A. Millikan, Frederick Soddy, Malthus, Hendrick Willem van Loon, Jay Forrester, Paul Ehrlich, Marx, J.B. Bury, Giordano Bruno, Gerard O'Neill, Voltaire, Machiavelli, and Max Born. He ends with the conclusion that ". . . the idea of progress, in the form responsible for the revolution in science, must somehow find its way into political thought." The mainly technical audience for *Astronautics & Aeronautics* can only have been startled by what they encountered.

R.T. was elected to both of the United States national academies, the National Academy of Engineering in 1973 and the National Academy of Sciences in 1981. His many other honors included the Sylvanus Albert Reed Award of the Institute of the Aeronautical Sciences in 1946, the Prandtl Ring of the Deutsche Gesellschaft für Luft und Raumfahrt in 1978, the Langley Medal of the Smithsonian Institution in 1981 (an award shared with such aviation notables as the Wright brothers and Charles Lindbergh), and the Fluid Dynamics Prize of the American

Physical Society in 1986. His lone college degree—the honorary doctorate mentioned earlier—came from the University of Colorado in 1971. (This is the institution at which Adolf Busemann had become a professor in 1964. R.T. wrote the memorial tribute to Busemann for the National Academy of Engineering in 1987, the year after Busemann's death.)

I cannot think of a more fitting way to close than to repeat what I have written elsewhere: R.T.'s friends knew him as a modest, considerate person of absolute integrity. According to Ilan Kroo of Stanford, "Those of us privileged to call him a colleague . . . were continually surprised and inspired by this maverick scientist who contributed so much to our understanding of flight. In addition to his well-known technical contributions . . . , he captivated a generation of students with fresh insights and new ways of looking at problems ranging from hang-glider dynamics and optimal bird flapping to supersonic aircraft." Most important for his various activities, he seemed to have a quiet confidence that he could accomplish whatever he set out to do—even if it was to make a fine violin. We do not see his like very often.

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