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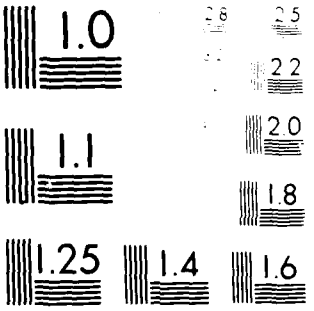
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MISSILE AERODYNAMICS - DIM PAST AND INDEFINITE FUTURE

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14 May 81

ABSTRACT

The present paper covers two distinctly different subjects. The first subject is the efforts of the U.S. Navy and the U.S. Army to develop aerial torpedos during World War I. The Navy team had such prominent engineers as Elmer and Lawrence Sperry and Glenn Curtis. The Army team included Charles F. Kettering and Orville Wright. Despite these eminent personalities, a successful aerial torpedo was not developed for use in World War I.

The second subject area covered in the paper is suggestions for future work in missile aerodynamics. High angle of attack aerodynamics, engine-airframe integration and autopilot-airframe integration are covered. In addition the future of asymmetric vortices, external stores, and computational fluid dynamics are discussed.

INTRODUCTION

It is a privilege to be an invited speaker to the Twelfth U.S. Navy Aeroballistics Symposium. I was pleased when my old friend, Dr. de los Santos, called me and invited me to the Symposium. I was, of course, delighted particularly since this presented an opportunity to choose my material at will. The title of the talk "Missile Aerodynamics - Dim Past and Indefinite Future" is accurate. I chose not to repeat the Wright Brothers Lecture. Rather I am taking this opportunity to divest myself of a number of preprints of the lecture. Anyone who would like a copy should help himself.

The first matter I should like to cover is the efforts of the Navy to develop a flying torpedo during World War I. It is an interesting history. In my Wright Brothers Lecture, I said a number of things about the efforts of the U.S. Army to develop an aerial torpedo in World War I, but gave only brief mention to the U.S. Navy. I now have the opportunity to remedy that shortcoming. At the same time I will expand on the efforts of the U.S. Army. You may want to take sides on the question, "Who invented the first successful guided missile in the United States?" So much for the dim past.

The other general area I would like to address is the never-never land of the indefinite future. I will discuss and make suggestions for future research in a number of areas of missile aerodynamics.

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2. U.S. NAVY MISSILE DEVELOPMENT IN THE WORLD WAR I PERIOD

2.1 INTRODUCTORY REMARKS

The history of the development of the aerial torpedo by the U.S. Navy during World War I is of interest involving, as it did, such engineers as Elmer and Lawrence Sperry, Glen Curtiss, and Carl Norden. The interest of the U.S. Navy followed naturally from the successful automatically controlled underwater torpedo to the aerial torpedo, or flying bomb, as it was variously called. The basic requirement it was envisioned to fulfill was to increase the range of artillery. The history of the development of pilotless aircraft and guided missiles to about 1948 has been summarized by RADM. D. S. Fahrney, USN (ret.) in reference 2.1, and much of the material contained herein has been obtained from this source. I am indebted to Dr. William J. Armstrong, Historian of NAVAIR, for a copy of this document. Its interest, in my view, is such that it should be published as a book. Additional material has been taken from references 2.2 to 2.6.

2.2 BEGINNING OF AERIAL TORPEDO PROJECT

On October 7, 1915 the U.S. Navy set up the Naval Consulting Board to advise the Secretary and Navy Department on matters of scientific and technical natures. A committee of the board was formed on "Aeronautics, including Aero Motors." Among the seven members of the committee were Elmer Sperry and Peter Cooper Hewitt. Hewitt was interested in a flying bomb prior to the creation of the Board and approached Sperry concerning such a device. Sperry had designed successful gyro systems for the automatic control of torpedos over a number of years. Sperry agreed to carry out some experiments if Hewitt supplied the necessary funds, estimated to be about $3,000. These funds went fast and Sperry supplied much more of his own money. To obtain more backing they decided to put on a demonstration for the U.S. Navy.

On September 12, 1916 Lawrence Sperry, son of Elmer Sperry, demonstrated no-pilot automatic control of a hydroplane to Lt. Wilkinson. The pilot took the hydroplane off the water and turned it over to automatic control. The plane thereupon climbed to a predetermined altitude and flew at this altitude a predetermined distance maintaining a given heading the whole time. Lt. Wilkinson recommended that the U.S. Army develop the flying bomb since they were useful for deployment against large targets on land rather than ships on water because of their perceived inaccuracy. However, on April 14, 1917 the Naval Consulting Board recommended to the Secretary of the Navy that $50,000 be made available "to carry on experimental work on the subject aerial torpedos in the nature of automatically controlled airplanes or aerial machines carrying high explosives capable of being initially directed and thereafter automatically managed." Strictly speaking, they were talking about pilotless aircraft.

The next action was for the Secretary of the Navy to set up another committee to make a recommendation on the recommendation. The review committee reported favorably and on May 22, 1917 the Sperry project was approved. The Sperry company received a contract for 6 sets of automatic control gear for aerial bombs at $3,900 apiece. The plan was to install these in N-9 type seaplanes and conduct flight tests.

2.3 FLIGHT TESTS WITH N-9 SEAPLANES

Amityville, Long Island was selected as the site for flight testing the N-9 seaplanes with Sperry automatic controls. In this operation the pilot always takes the plane off before turning the plane over to automatic control. A period of ground testing preceded the flight testing which started in September 1917. Successful flight was made on September 5, 1917, one plane made a run on a target eight miles away with little error in course but 12½-percent error in range. During these tests significant gyro drifts were noted. Elmer Sperry tried to convince the U.S. Navy of the importance of radio control for correcting errors in targeting, but he never succeeded during the entire project, thus delaying the first application of command-updated inertial guidance.

It is of interest that the Chief Signal Officer of the Army witnessed a successful test on November 21, 1917. Later the U.S. Army developed its own aerial torpedo with Charles F. Kettering and Orville Wright on the team.

2.4 PROCUREMENT OF PILOTLESS AIRCRAFT

In the opinion of Elmer Sperry, the top speed of the N-9 was too low, and a special design was needed for the aerial torpedo. By increasing top speed, errors due to gyro drift could be reduced. It was necessary to be able to launch the bird without a pilot, and this specialized problem needed to be worked out.

Sperry had for years worked with the Glenn H. Curtis Company on aeroplane stabilization. He contacted Glenn Curtis concerning the design and manufacture of a pilotless aircraft to act as a flying bomb. A specification was written for such an aircraft by Glenn Curtis as follows:

Payload: 1000 lb of explosive
Empty weight: 500 lbs
Take off: catapult launch
Top speed: 90 mph
Range: 50 miles
Provide for special control equipment
Engine: Should be as light as possible compatible with its duties

The Curtis cost estimate for producing these flying bombs with engines was a minimum of $6,000 apiece and a maximum of $10,000 apiece. A best effort to achieve delivery in 30 days was promised. A contract was signed and the delivery was made within 30 days. A sketch of the Curtis flying [illegible] shown in Figure 2.1.

2.5 LAUNCHING OF THE CURTIS FLYING BOMB

The first launching device tried was a downward sloping wire, with tip wires to hold the wings level. Tests of this device were unsuccessful. Next it was decided to try a launching device which might work aboard ship. Such a device might consist of a launching car on tracks, and a device to give an initial impulse to the car. The device was built but the first tests in December 1917 and January 1918 were unsuccessful. The flying bomb was observed to be tail heavy. It was realized that the flying bomb must first be a

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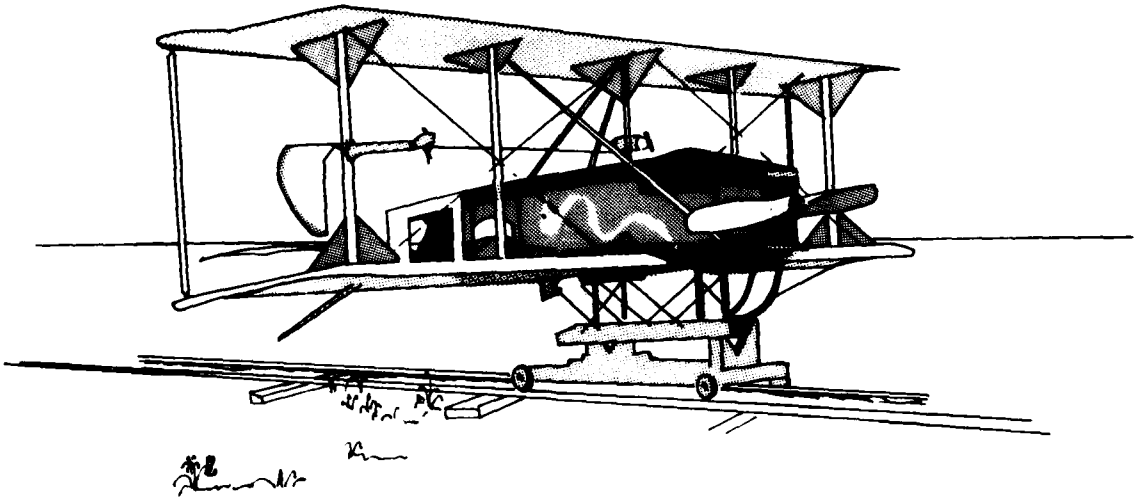


Figure 2.1 - Curtis Flying Bomb.

practical flying machine before it could be demonstrated with automatic control.

In an attempt to correct the stability and control of the flying bomb, a seat to accommodate a pilot was put into the explosive bay. Ski runners were put on the plane and take-offs and landing were practiced by Lawrence Sperry on a bay of ice. He cracked up the plane several times and luckily escaped serious injury.

On March 6, 1918 a successful launch of the flying bomb was made from the track which satisfied all the test objectives. The machine launched successfully, and flew in a straight line climbing steadily. The distance gear cut the throttle at the prescribed distance, 1,000 yards, and the machine spiraled into the water. This flight is said by Admiral Fahrney (ref. 2.1) to be the "first successful flight of an automatic missile in the U.S. and possibly the world."

Another flying bomb on April 7, 1918 was launched successfully but crashed after takeoff. Hereafter the Navy decided to do further work to improve the aerodynamics of the flying bomb as well as its launching.

2.6 SUBSEQUENT HISTORY OF CURTIS FLYING BOMB

Lawrence Sperry entered the Navy Reserve on active duty on January 1, 1918 but became ill and entered the Naval Hospital on March 19, 1918. He was found physically unfit for flying and discharged from active duty subsequently. He formed the Lawrence Sperry Aircraft Co. and started vigorously to solve the flight characteristics of the bomb under automatic control. A special Marmon automobile was used to mount the flying bomb for high-speed ground testing, thus predating sled testing at NWC and Holloman Air Force Base. After some useful experimentation with the flying bomb attached to the automobile, it was decided to put the automobile on railroad tracks and try launching in this mode. The tests were unsuccessful.

At this time the Navy decided to retain Carl L. Norden to design a fly-wheel type of catapult which he successfully accomplished. Tested on a flying bomb, the launching was successful but the flight of the bird thereafter was erratic. The test of another flying bomb was also successful in launch but not in flight.

At this point the Navy decided to launch N-9 seaplanes with the Norden catapult to further test the Sperry control system. At least one successful test was made. It became apparent that if the N-9 had been used on the flying bomb, the project would have reached a successful stage of development much earlier. Elmer Sperry concluded "I feel that we have gone a long way towards completing the development of an extremely significant engine of war, it being nothing short of the coming gun..."

By this time (September 1918) Cdr. McCormick, in charge of the flying bomb development, concluded that future work should be directed to improvements of the automatic pilot and a new design of the flying bomb airplane.

2.7 POST WAR EPILOGUE

The Navy asked Norden to review the design of the Sperry automatic pilot and make recommendations for further work. Specifications for a new flying bomb plane were approved by the Navy and a contract for five planes awarded to the Witteman-Lewis Company. Norden got the contract for design and fabrication of the automatic control gear for the Witteman-Lewis machine. Both Sperry and Curtis were now out of the picture. At the same time plans were made for the Navy to direct the project at the Naval Proving Ground, Dahlgren, Virginia.

The flight tests of the Witteman-Lewis machine in the summer of 1919 by Navy pilots revealed the machine to be too tail heavy with insufficient aileron. The design of the airframe was changed, and flight testing was resumed in the spring of 1920. On August 18, 1920 with the pilotless version, the plane released from the catapult went smoothly, but stalled 150 yards out. Norden stated "No plane has ever been flown under automatic control successfully without previous adjustment after trial flights by a competent pilot," (Reference 2.1). The Navy accepted this and the "pilotless" aircraft were flown by pilots and tuned prior to pilotless launch from the catapult. On October 25, 1920 the next launching was "perfect." The airplane flew in circles all over the sky before it ran out of gas, spun, and crashed. On April 25, 1921 the next launching was also perfect. However, the plane climbed a short distance, but settled into the water and upset due to the fixed landing gear.

The Bureau was losing interest fast.

"The Bureau is not impressed with the practicability of this aerial torpedo (F.B.) for use against vessels, even when they are in Fleet formation, because of the difficulty of controlling the height within sufficient limits to permit a torpedo to be flown at low altitudes, such as would be required for use against a vessel. It is believed they may, however, be of use as aerial targets by installing controls in condemned planes - the question of radio control has been under consideration and is believed to be feasible. The original intention for use of the 'F.B.' was for the distant bombardment from sea of large areas, such as naval stations, fleet anchorages, and fortified towns. It is still believed that this use can be realized with fair success. The tactical value of such a use is, however, believed to be doubtful. Its greatest value for us is probably for use in control of surveyed planes used as targets. In designing the Bureau Controls, allowance was made for possible future fitting of a radio control, which is considered to be quite feasible." This essentially ended the flying torpedo, but the ideas of radio control of airplanes lived on.

3. U.S. ARMY MISSILE DEVELOPMENT IN WORLD WAR I

3.1 THE ARMY GETS UNDERWAY

It is of interest to review the efforts of the U.S. Army to develop an aerial torpedo during World War I. Major General O. Squier had witnessed a flight test of the U.S. Navy's aerial torpedo in Amityville on November 21, 1917. He was so impressed that he got the U.S. Army to start its own project. The principal idea was to get innovative weapons to take to the war in Europe since "wars are won largely by new instrumentalities." Mr. Charles F. Kettering

became Director of the Army's Flying Bomb Project. Mr. Kettering, who with others, had acquired the Dayton Wright Co., enlisted Orville Wright as his aerodynamic consultant. A pair of consultants and manufacturers were obtained for power plants and controls. The Army's aerial torpedo was variously known as an automatic flying machine, automatic carrier, Bug, and Flying Bomb.

3.2 SPECIFICATIONS

Mr. Kettering laid down a number of points for the Army's aerial torpedo:

(1) Simplicity
(2) Easiness of manufacture
(3) Easily assembled in the field
(4) Economy of shipping space
(5) Ease of launching
(6) Reliability
(7) Load carrying aspects
(8) Accuracy

As a result of these points certain specifications were developed:

Total weight: 520 lbs
Biplane wings:
span: 15 ft
chord: 30 ins
dihedral: 10°
Take-off speed: 55 mph
Engine: 4 cylinder, V90°, 2 cycle, 37 HP at 2150 RPM
Altitude control: aneroid barometer
Direction control: Air valve sensing apparatus on gyroscope
Distance control: Air log (propeller revolution count)
Material:
Fuselage: Plywood, paste board
Wings: Muslin, brown paper, dope
Tail surfaces: Paste board

Every effort was made to use materials not needed by the aircraft industry. It was estimated that the total cost including explosive came to about $1.00 per pound.

In contrast to the Navy design, the automatic controls were in the design stage whereas the Navy designs were completed and had been tested in N-9 airplanes.

A sketch of the Kettering "Bug" is shown in Figure 3.1.

3.3 FLIGHT TESTING

The flight tests were started in September 1918. However, numerous changes in design and much testing preceded the flight tests. It should be pointed out that no piloted versions of the flying torpedo were used to test or adjust the automatic controls prior to flight testing. Launch was from a rail-mounted cart powered by the aerial torpedo.

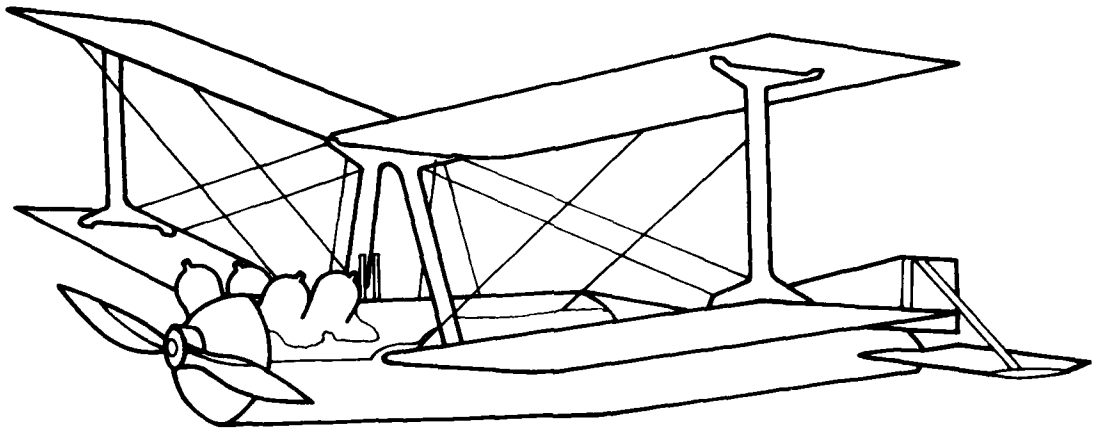


Figure 3.1 - Kettering Aerial Torpedo.

The first flight test put the flying torpedo through a hair-raising series of aerobatics before crashing at the end of an Immelman turn. In the second flight a different series of aerobatics occurred, but the ship corrected itself and flew away. A tendency of the planes to fly in a wide circle as a result of propeller torque was noted.

The status of the flight tests was such that on October 5, 1918, General Squier in a memorandum to the Chief of Staff wrote "The Chief Signal Officer believes that the development of this new weapon, which has now demonstrated its practicability, marks an epoch in the evolution of artillery for war purposes of the first magnitude, and comparable, for instance, with the invention of gunpowder in the fourteenth century. The development is not known to our overseas Forces, nor to the Forces of our allies. It comes as a distinct product of American genius, as applied to our present methods of warfare."

This optimism was on firmer ground when on October 22, 1918 the first perfect test of the aerial torpedo occurred. The altitude was set at 200 feet and the range for 500 yards. The impact was almost exactly on target.

3.4 POST-WAR DEVELOPMENTS

The Army had a number of aerial torpedos made and was preparing to transfer flight operations to the former Navy site at Amityville, Long Island when World War I came to an end. At this point 25 aerial torpedos plus parts were put into storage at McCook Field.

A series of flight tests under Army cognizance of 14 aerial torpedos at Carlstrom Field, Florida were generally unsatisfactory. All the know-how obtained at Dayton was not transferred to the Army test units. As a result of these tests recommendations were made to (1) develop means for launching regardless of wind direction, (2) develop self-propelled launching cars on a catapult, and (3) make improvements in controls, gyroscopes, and engines.

On December 30, 1919 the Adjutant General directed the Air Service to continue the development of the automatic carriers.

In March 1920, plans were laid down for the development of the flying torpedo. They covered three aspects:

(a) Perfection of the automatic controls
(b) Specification of control means
(c) Testing of controls in a piloted aircraft

The firm of Lawrence Sperry Aircraft was given contracts in the first few months of 1920 to construct automatic controls and six "Messenger" airplanes, and to carry out tests under full automatic control. The first flight tests with a standard L-1 airplane with pilot in September 1920 to April 1921 proved out launching and distance control, but the gyroscopes gave problems due to precision, poor bearings, and installation difficulties. Further flight tests with a new gyro were made, but the problems of maintaining a predetermined course was still unsolved because of wind changes and gyro difficulties. At this point the contractor requested permission to use radio control to correct deviations from a predetermined course. Tests were made using radio control up into 1926, initiating more or less successful application of command updated inertial guidance. However, the hand writing was on the wall.

On June 7, 1926 the Chief of the Engineering Division, Major John F. Curry, wrote to the Chief of Air Service expressing the views of his Division "that no torpedo development can be successful if it depends on a system of stabilization alone, but that, in addition, radio control is absolutely necessary. The gyroscopically controlled aerial torpedo, equipped with radio, is necessarily very expensive. A project has been initiated to cover the study of the aerial torpedo as an automatically stable airplane equipped with radio control. --- Due to the shortage of experimental funds, the aerial torpedo development has had to give way to other and more necessary developments. --- Due to other more urgent projects, the allotment of personnel and money is not sufficient to complete the aerial torpedo projects during the coming fiscal year."

This wrote "finis" to the aerial torpedo development until World War II urged the reopening of the development. Little was accomplished before all development was greatly curtailed in the lean years following the stock market crash in 1929.

I am indebted to Mr. Carl Tusch of the AFSC Liaison Offices at Ames Research Center and the Air Force Museum for supplying historical material on the Army aerial torpedo project. Also I wish to thank the Albert F. Simpson Historical Research Center of Maxwell AFB who provided additional material.

At this point I would like to skip over sixty or more years from the dim past to the indefinite future and make some prognostications concerning the future of missile aerodynamics.

4. HIGH ANGLE OF ATTACK AERODYNAMICS

4.1 AREAS OF IMPORTANCE

High angle of attack aerodynamics has been an area of interest among missile aerodynamists for a number of years. The subject embraces a number of areas, some of which are discussed in the following sections. Here we will only treat the subject broadly since specific aspects of the subject will be subsequently discussed. For purposes of discussion let us consider high angles of attack to be those over about 20 degrees.

The general interest in high angle of attack aerodynamics stems from the fact that missiles use higher and higher angles of attack in the search for increased maneuverability. Some particular applications of past and present interest include the bomber defense missile (SRBDM), short-range air-to-air (Agile and ILAAT), and AAW missiles which must turn over quickly from vertical launch. Another application is the high altitude missile which may be unpowered at extreme range and yet be required to have a maneuver capability of two- or three-fold over an evasive target. Also a tumbling missile or missile fragment is another particular application.

4.2 SPECIAL PROBLEMS OF IMPORTANCE

A few special problem areas in high angle of attack aerodynamics are now discussed. First there is the question of air inlets at high angles of attack. It is hard to design an efficient air inlet for a large angle of attack range

(and appears feasible only for bank-to-turn missiles.) Another problem is that aerodynamic controls suffer severe losses of effectiveness at high combined angles of pitch and deflection (Reference 4.1). These losses make it difficult to trim the airframe at high angle of attack thus limiting maneuverability.

There is a severe loss of favorable wing-body interference at high angles of attack and Mach numbers as shown in Figure 4.1. The factor K_W is the ratio of the normal force on the fins mounted on the body to that of the wing alone at the same angle of attack. In this figure a value of K_W greater than unity indicates favorable interference whereas a number less than unity indicates unfavorable interference. The unfavorable effects can be very large at high angles of attack and Mach numbers.

Another problem about which very little is known is nonlinear afterbody effects at high angles of attack. The body section between the missile nose or canards and its empennage can shed vortices at high angles of attack which cause large nonlinearities and greatly reduce tail effectiveness in stabilized missiles.

The above problems represent areas in which additional research is needed.

4.3 PREDICTION METHODS FOR HIGH INCIDENCE

The term "prediction methods" is meant to cover both computational fluid mechanics and engineering prediction methods. The discussion is confined to methods for predicting static forces and moments. The former will be treated in a subsequent section, and the latter will now be discussed with regards to the transonic, supersonic, and hypersonic speed regimes.

Engineering prediction methods for the high angle of attack transonic regime are almost entirely data-base methods (Reference 4.2), which permit little extrapolation out of the test range. This subject is worthy of attention for both transonic and subsonic speeds.

At supersonic speeds there are several methods such as References 4.3 to 4.5. These methods use data bases sometimes combined with rational modeling. In this approach a skeletal but systematic data base is obtained covering a range of the parameters of interest such as angle of attack, roll angle, Mach number, fin aspect ratio, etc. A theoretical model of the flow over the missile is made, and rational mathematical techniques are used to interpolate and extrapolate from the data base. While several high incidence supersonic engineering design methods exist, they cover different configuration spaces with some overlap. The effects of roll angle are generally not included. The effects of aerodynamic controls is an area needing much attention. Force and moment prediction methods for missiles with noncircular bodies and with inlets also need more attention in view of the current interest in bank-to-turn missiles.

For high supersonic and hypersonic speeds the principal methods are based on Newtonian theory, shock-expansion theory, or derivatives of these approaches While for certain simple configurations these methods give good results, no suitable general method exists which applies to more complicated configurations and at the same time handles vortex effects. A need for such a method exists.

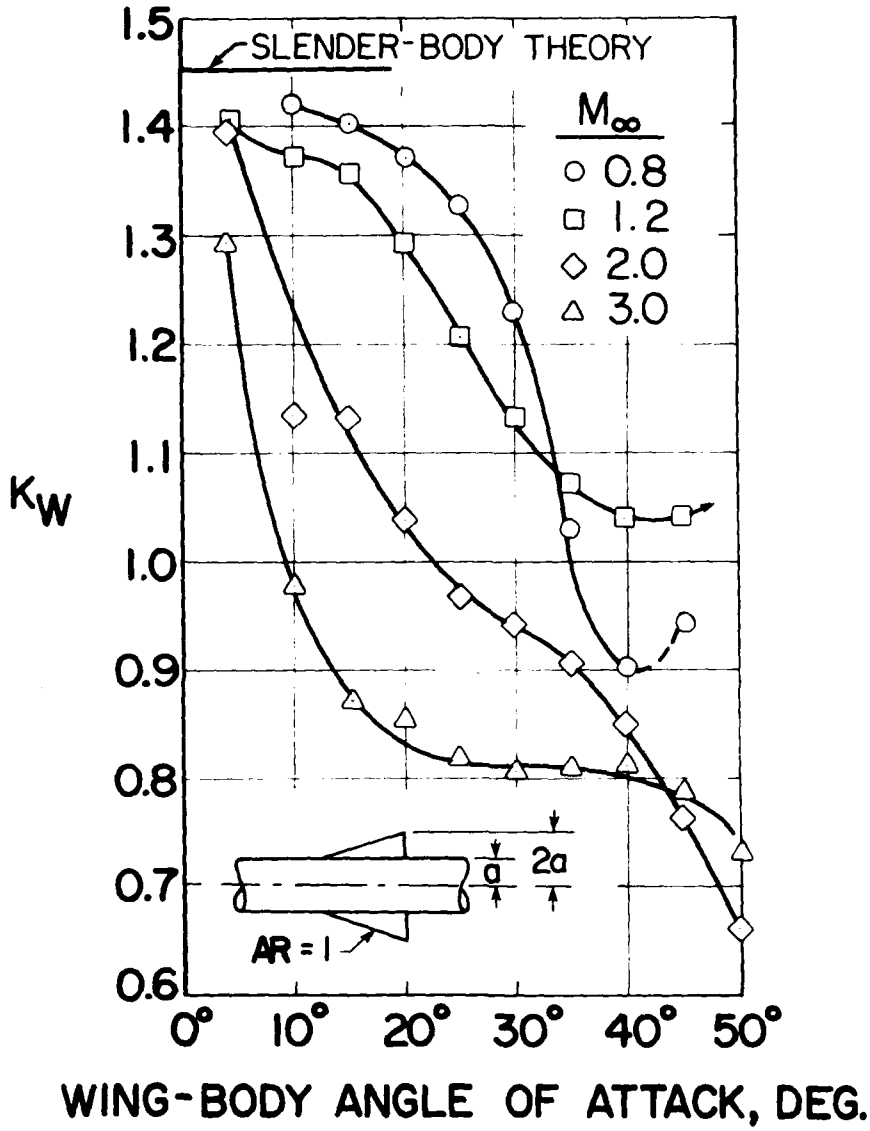


Figure 4.1 - Effect of Angle of Attack and Mach Number on Wing-Body Interference.

4.4 HIGH ANGLE OF ATTACK WIND-TUNNEL TESTING

The need for high α data is not only to obtain design data for particular missiles but it also includes the requirements for high quality data for testing aerodynamic theories and systematic data for rational modeling methods. In the area of data for checking theory there is a requirement for coordinated flow-field measurements, pressure distribution data, and flow visualization.

One of the primary difficulties that makes high angle of attack testing difficult at all speeds is the need to design model supports which will stand the high loads involved and at the same time will minimize the effect of support interference on the quantities being measured. There is much room for ingenuity in the design of such support systems.

A particular speed range of difficulty for high α testing is the transonic range not only because of the well-known wall interference but also because of support interference. The simple case of a body of revolution shows quite different characteristics at high α depending on whether it is supported by a strut or by a sting. An example of this effect is shown in Figure 4.2 as taken from Reference 4.6. It appears that the strut interferes with asymmetric vortex formation. Further experiments are required to develop high α interference-free support systems at transonic speeds.

5. INTEGRATION OF ENGINES AND AIRFRAMES

5.1 Preliminary Observations

The problems of engine-airframe integration are most important for air-breathing missiles, and these comments apply to such missiles. Included in this area are the effect of the airframe on the airflow into the inlet, and the interference of the inlet on the external airflow about the missile. Both effects are important. Generally missiles utilizing air-breathing engines will not be allowed to roll continuously because of the difficulty of maintaining efficient inlet operation under these conditions. Exceptions probably exist. We will address the question of the state of the art concerning CFD methods for inlet design, data available for design methods, and engineering design methods. Suggestions for future work in these areas are also considered.

5.2 COMPUTATIONAL FLUID DYNAMICS (CFD) METHODS

The principal CFD methods available for studying airframe/inlet interference are based on the Navier-Stokes equations, the Euler equations, or on paneling procedures. With regard to Navier-Stokes methods no complete solutions seem to have been carried out for the three-dimensional case. For the subcritical case not even a two-dimensional calculation is available for realistic geometries. The difficulties lie in the lack of powerful enough computing machines and in turbulence modeling, especially in association with boundary-layer shock-wave interaction. Those difficulties will eventually be overcome. Until then other methods must be used in design.

Euler codes have been applied to supercritical inlets with some degree of success. Their applications to subcritical inlet problems are not yet fully demonstrated. The basic problems with the Euler equations for internal flows

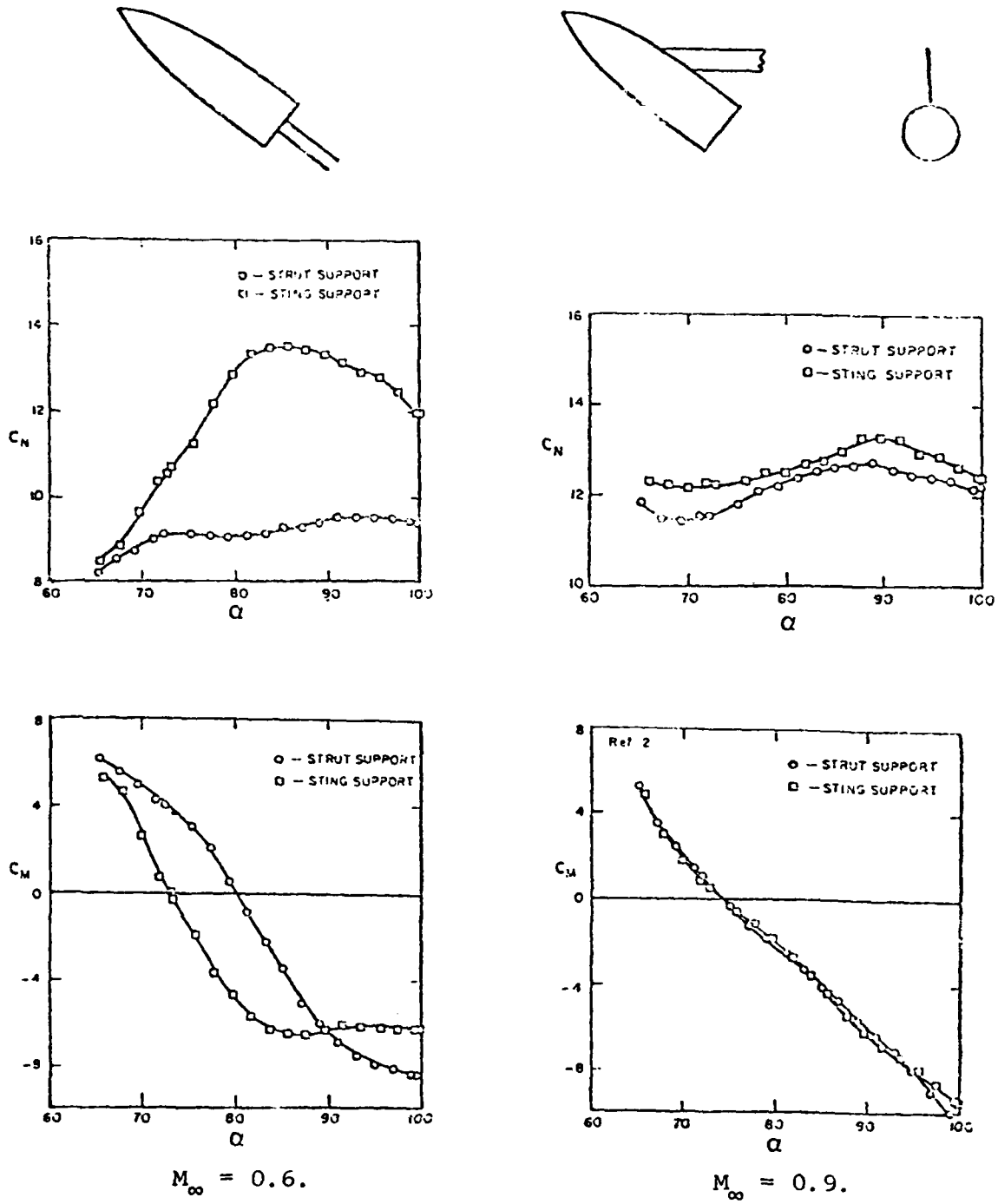


Figure 4.2 - Effect of Support Type on Normal Force and Pitching Moment of Ogive Cylinder at High Angle of Attack and Transonic Speed.

is that boundary-layer, shock-wave interaction causes the internal waves to be in different positions from those predicted for an inviscid fluid as given by the Euler equations. The possibility of using an Euler code together with an embedded boundary-layer analysis appears to be a practical approach which should be attempted to provide a basis for design tools. It is possible to account for body vortices and fin vorticity with Euler codes so that the effect of these quantities on the quality of the flow entering the inlet can be predicted. Also the effect of the inlet on the external flow should be amenable to treatment by Euler codes.

It is possible to treat the effects of variable inlet mass flow with panel methods as has been demonstrated by Dillenius (Reference 8.7). However, a careful comparison between experiment and theory for such an approach has not been made. One would expect to be able to calculate the effect of the inlet on the external aerodynamics by such an approach. It seems worthwhile to determine the accuracy and limitations of panel methods in this connection because of their potential economy.

5.3 STATUS OF THE DATA BASE

Much data exist on engine-airframe integration as a result of testing many specific designs. However, the data are generally not systematic nor consistent with respect to definitions of quantities or terminology. The data on the effects of the inlet system on the external aerodynamics of air-breathing missiles are being assembled into a data handbook. Dr. O. J. McMillan will cover this subject in the last paper of this Symposium.

5.4 ENGINEERING PREDICTION METHODS

The state of the art with regards to engineering prediction methods leaves a great deal to be desired. Existing methods which fulfill the requirement of being cheap suffer from lack of accuracy. In fact, a general method of good accuracy does not exist.

A simple approach to remedying the present unsatisfactory state of the art probably does not exist. It will probably involve a thorough evaluation of the accuracy of present methods to determine their inadequacies, the definition of problem areas where deficiencies exist, and overcoming the deficiencies by systematic experimental tests and the use of rational modeling and computational fluid dynamics.

6. AUTOPILOT-AIRFRAME INTEGRATION

6.1 BACKGROUND

The principal limitations to maneuverability of missiles which are aerodynamically controlled are due to the autopilot. These limitations are often associated with the inability of the autopilot to cope with the cross-coupling of the aerodynamic control functions, largely between yaw and roll. Often the limitations are associated with the variation in the magnitude of the direct control derivatives with angle of attack and roll angle.

6.2 PROBLEMS IN CURRENT PRACTICE

There are a number of factors in current practice which are not conducive to proper autopilot-airframe integration. Frequently the autopilot designer sees the aerodynamics as given or measured, and complicates the autopilot design in an effort to control a missile in the presence of severe aerodynamic nonlinearity. In many companies the aerodynamic and system control groups are separate, and the engineering manager does not exercise the necessary direction to see that cross fertilization occurs. In order to do this, he must have a good knowledge of each discipline. Part of the problem is that undergraduate schools stress linear control theory, leaving nonlinear control theory as an elective course. Yet, as von Kármán said, "It is a nonlinear world in which we live."

Another part of the problem is due to the fact that good engineering methods for predicting control cross-coupling derivatives are lacking. If the aerodynamicist and autopilot designers work together, it seems that better missile maneuverability can be achieved at lower cost; also, the success of efforts to adjust autopilot gains based upon state estimation is enhanced by close coordination. What then can be done to improve the present practice?

6.3 RECOMMENDATIONS

The first step in the process should be to make sure that the aerodynamicist and autopilot designers work together before the airframe design is frozen so that some control can still be exercised over its nonlinearities. Perhaps jointly they could establish specifications for the airframe, allowing for nonlinearities (many of the known classes of airframe nonlinearities are described in Reference 6.1). To accomplish this step will require better aerodynamic methods in some cases for predicting control cross-coupling among other nonlinearities. In many cases the airframe aerodynamics will still need to be determined experimentally, but the test model should be a better approximation to the final design by applying missile aerodynamic prediction methods first. It seems quite feasible that the integrated problem of airframe-autopilot design will become a subject of fundamental research and development to see how a coordinated design effort can synergistically enhance the final product.

7. ASYMMETRIC VORTEX PROBLEMS

7.1 BACKGROUND

Asymmetric vortices are known to form on the leeward side of a body of revolution if the angle of attack is increased beyond a certain limiting value that depends on a number of parameters, the most important of which is probably body fineness ratio. The unexpected phenomenon, first reported by Cooper et al. (Reference 7.1) in 1952 has been termed "phantom yaw." The onset of vortex asymmetry is usually accompanied by large side forces and yawing moments which are undesirable from the standpoints of both stability and control. The precise cause of vortex asymmetry is not clear, but it appears to be associated with a neutrally stable condition of a symmetrical vortex pair depending on its strength and geometric configuration. Then a disturbance can cause it to take

one or another of several asymmetric positions. Slight body geometric asymmetries or wind-tunnel flow disturbances can trigger it one way or the other.

An oversimplified, but useful, diagram which shows the general occurrence of asymmetric vortices is shown in Figure 7.1. Here the $\alpha - M_\infty$ diagram is divided into three regions by the line $\alpha = 25°$ and $M_C = M_\infty \cos\alpha = 0.5$. A typical angle of attack for the onset of asymmetric vortices for a body of moderate fineness ratio is 25°. It is also known that if the crossflow Mach number, M_C, is greater than about 0.5 to 0.6 (Reference 7.2) that the leeward flow changes character. The relatively concentrated vortex pair is now replaced by two large symmetrical elliptical regions of rotational flow, and an asymmetric vortex pair does not occur. It is thus seen that asymmetric vortices are of no significance above the transonic speed range for moderate fineness ratios.

7.2 PARAMETERS AFFECTING PHANTOM YAW

A number of parameters are known to influence phantom yaw. A good survey is contained in Reference 7.3. Increases in body fineness ratio cause the onset of vortex asymmetry at lower angles of attack. Rolling the missile can cause changes in side forces and yawing moments of different magnitudes and sign in a repeatable manner depending on body roughness (departures from circularity). Nose bluntness seems to inhibit asymmetric vortices.

7.3 CONTROLLING OR HARNESSING PHANTOM YAW

While changes in basic geometry to reduce phantom yaw effects are of interest, it is of even greater interest to control or harness phantom yaw by the use of novel ideas. One idea in this category is a rotating nose. Rotating the nose causes an asymmetric vortex pattern to switch as shown in Figure 7.2 taken from Reference 7.2. Increasing the rate of spin may reduce the amplitude of the side force which is oscillatory, not random. I am indebted to Dr. Gary Chapman of NASA/Ames Research Center for these data. Further data are contained in Reference 7.3. If the nose spin rate is above the bandwidth of the autopilot, then the effect of phantom yaw is eliminated.

Another novel idea for harnessing phantom yaw is due to Mr. T. Canning (Reference 7.4) from work performed under an AFATL sponsored experimental study of support interference on the loads on bodies of revolution at transonic speed and high angles of attack. The next two figures (Figures 7.3 and 7.4) show plots of $C_N \cos\alpha$ versus C_Y for an ogive-cylinder at different roll angles of the body. The body had a small piece of tape on the nose at a fixed azimuthal angle. The variations of C_N and C_Y with roll angle were irregular but repeatable. However, paired values of C_N and C_Y formed smooth curves as shown. Note that vortex asymmetry increases both normal force and side force.

Now the maximum resultant force in a plane normal to the free-stream direction is given by $(C_Y^2 + C_N^2 \cos^2\alpha)^{1/2}$. The radius vector from the origin (Figures 7.3 and 7.4) is the value of the maximum force coefficient and its direction is about 30° from the leeward meridian. The nose strip is generally between the leeward meridian and the direction of the maximum resultant force. The data show that resultant forces as much as 35 percent greater than for

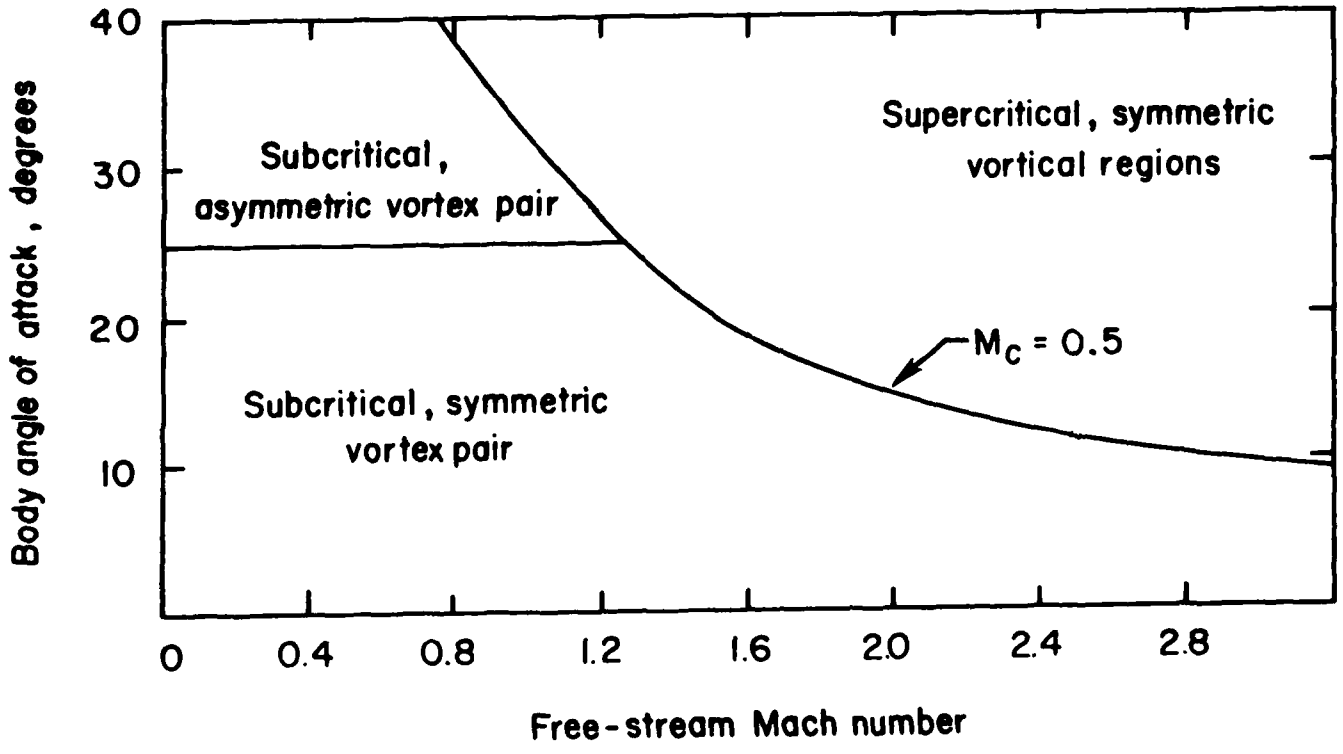


Figure 7.1 - Approximate Regions for Various Types of Body Vortices.

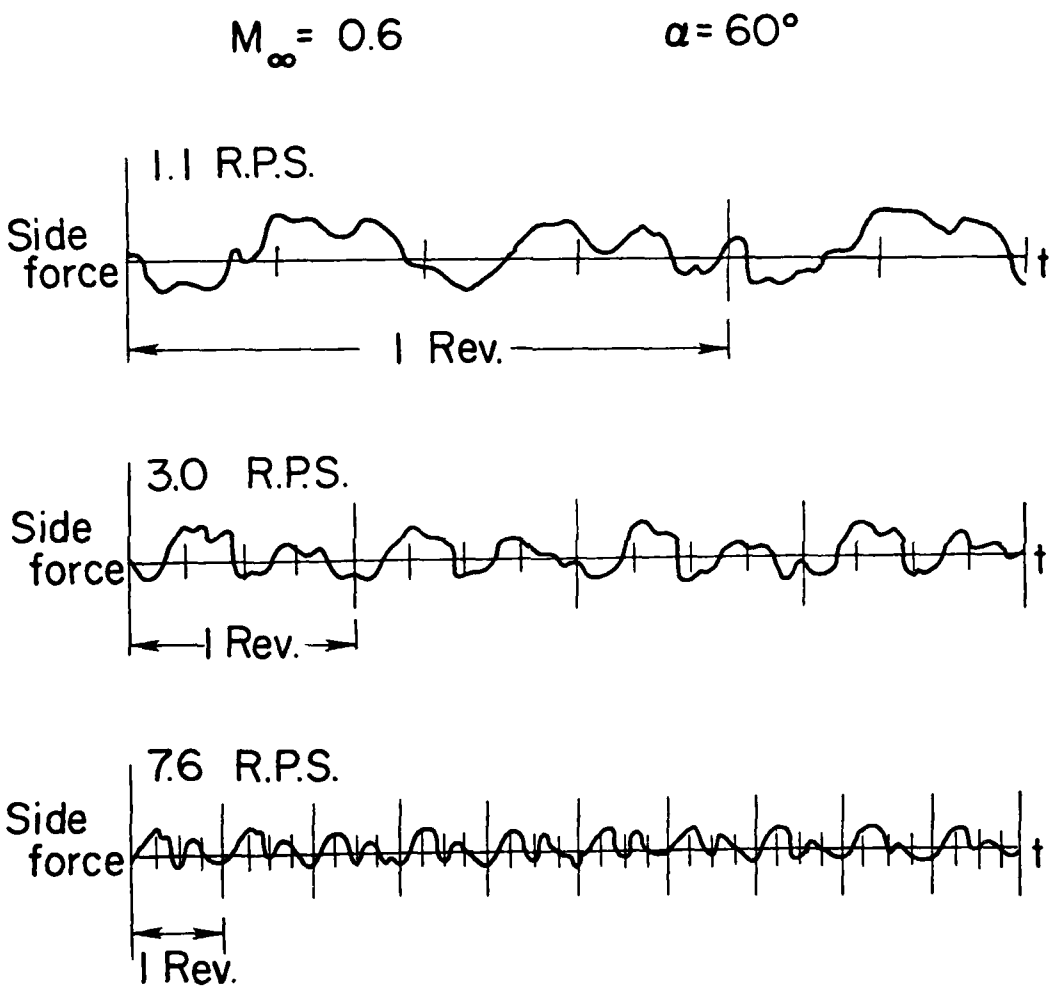


Figure 7.2 - Effect of Spin Rate on Side Forces of 10° Half Angle Cone.

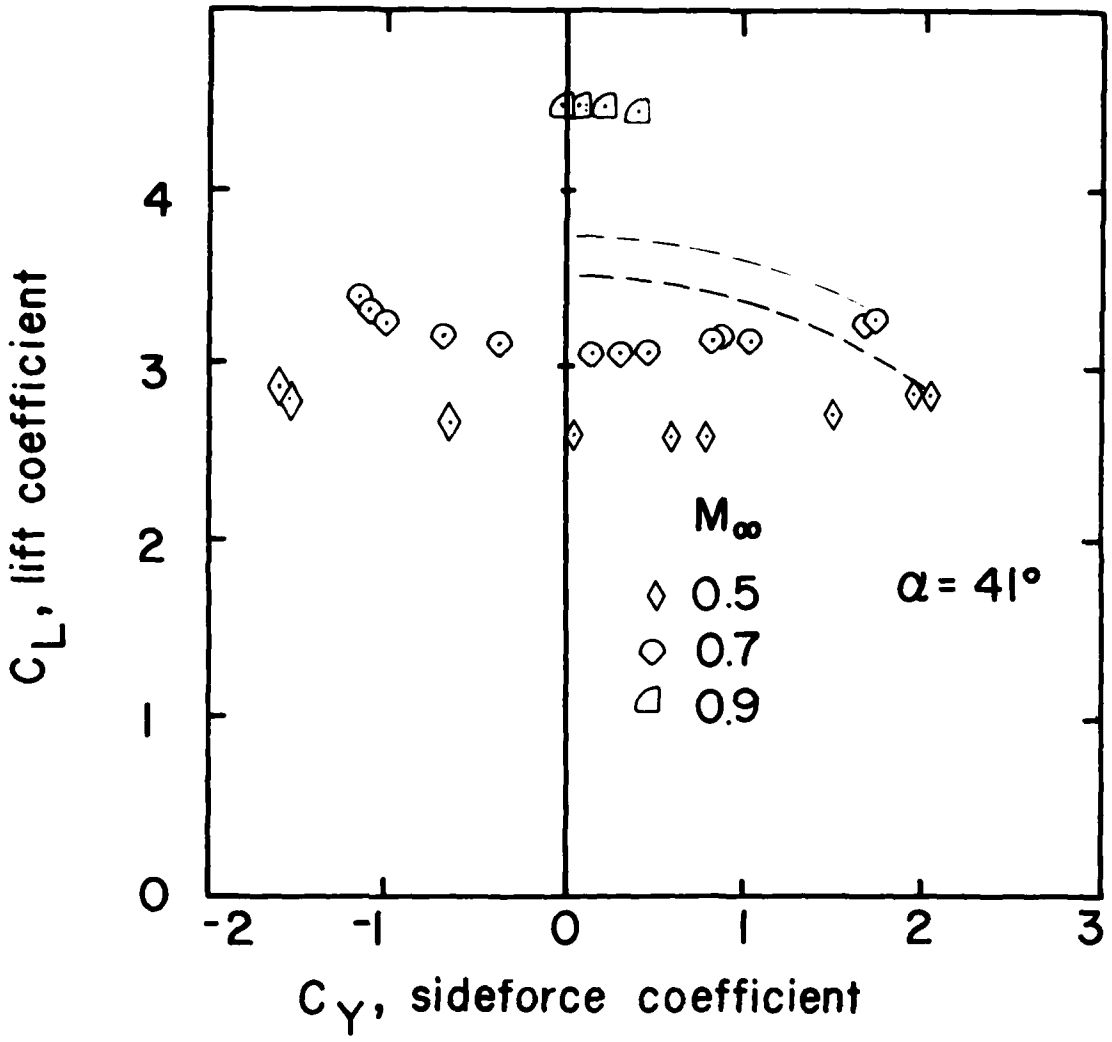


Figure 7.3 - Total Force Available for Maneuvering at Various Roll Positions of a Fineness Ratio 7.5 Ogive-Cylinder; Effect of Mach Number.

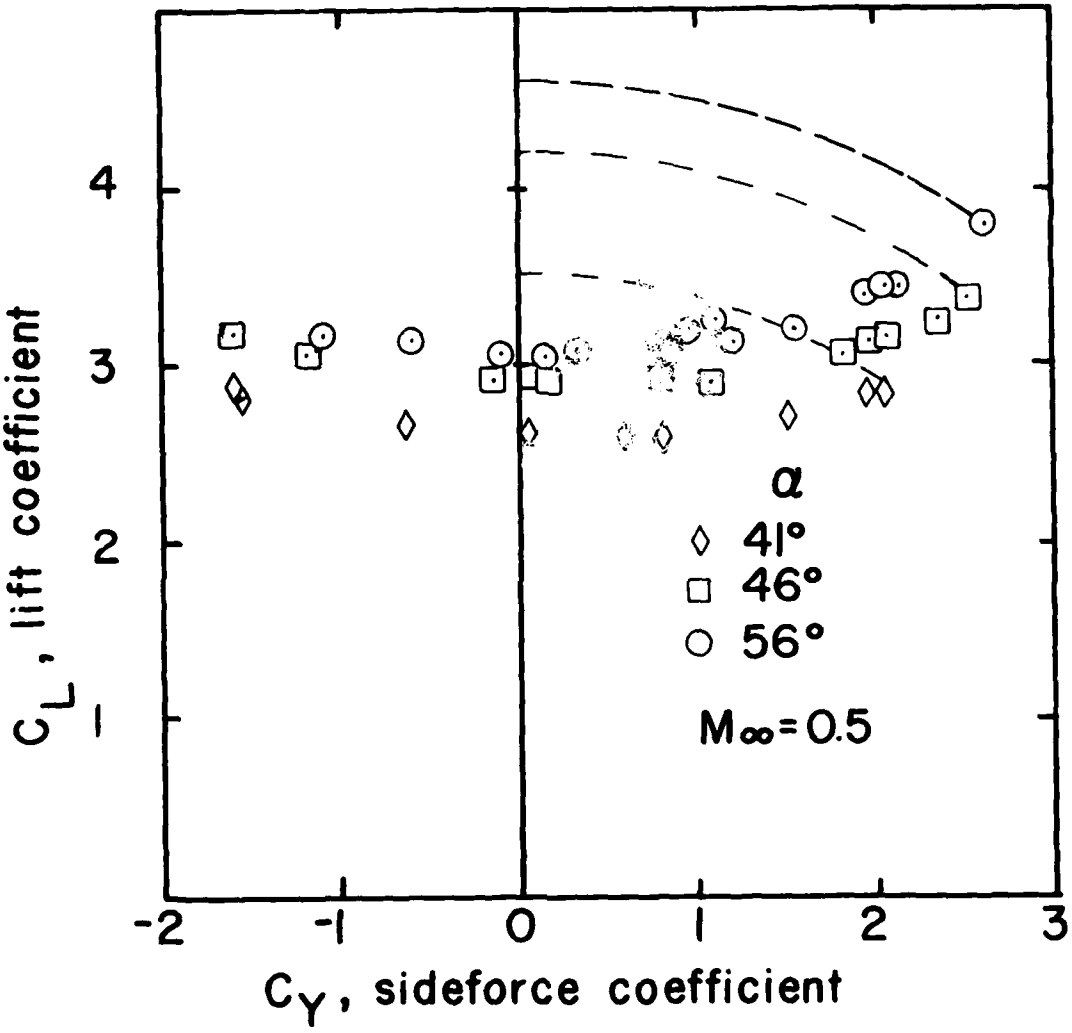


Figure 7.4 - Total Force Available for Maneuvering at Various Roll Positions of a Fineness Ratio 7.5 Ogive Cylinder; Effect of Angle of Attack.

symmetrical vortices can be obtained by harnessing phantom yaw. The gain decreases as the Mach number and angle of attack increase.

For a missile that must pull high accelerations in a transonic turn, it is possible to control phantom yaw by use of roll control and a nose strip and at the same time get greater maneuverability. The design of an actual system to achieve this is an interesting problem.

8. EXTERNAL STORES - LAUNCH DYNAMICS

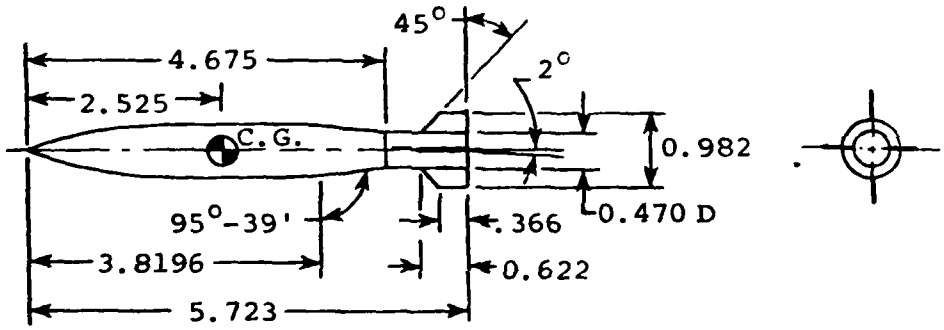
8.1 BACKGROUND

In recent years the addition to certain aircraft of many missiles externally mounted on racks and pylons has resulted in large drag penalties to aircraft optimized for minimum drag without external stores. In fact, missile installations have turned otherwise supersonic aircraft into subsonic ones so that the next generation of combat aircraft were designed with this danger in mind. Up to now the clean separation of external stores from aircraft and the performance penalties due to hanging external stores on aircraft have been investigated principally in expensive wind tunnel and flight tests. The large number of combinations and permutations of aircraft and stores requires extensive testing. For a number of years the power of large-scale computers has been brought to bear on these problems. It is probable that computer analysis of these problems can profitably be greatly expanded.

I am looking forward to what Professor Maddox has to say on the subject in his invited lecture, as well as the other speakers in the external store session. I would now like to make a few remarks about store separation at subsonic, transonic, and supersonic speeds and suggest problems of interest in each speed range.

8.2 SUBSONIC SPEEDS

Much analytical work has been done to develop codes for predicting store separation from fighter-bomber aircraft at subsonic speeds. A particular code developed by Fred Goodwin (Reference 8.1) under Air Force Flight Dynamics Laboratory sponsorship is well known. One of the comparatively recent developments has been the discovery that store loads for an attached store versus one just off the rack can differ markedly. This fact has emerged in wind-tunnel tests by Dix (Reference 8.2) and flight tests at Patuxent River by Maddox (Reference 8.3). These tests stimulated careful wind-tunnel tests (Reference 8.4) to investigate the causes of this phenomenon for stores mounted on a TER rack. Figure 8.1 shows the finned stores tested on the TER rack under a model of the F-4 airplane. Figure 8.2 shows the normal force on the lower finned store of the TER rack in the attached position and for positions beneath the rack. What is of interest is the rapid change in normal-force coefficient for a store displacement of less than a tenth of its diameter. The significance of the results are that special methods are required to predict attached loads. The methods which are adequate for predicting loads for store separation purposes may not be adequate for attached loads. More work is needed in this area.



ALL DIMENSIONS IN INCHES

Figure 8.1 - Store Model Used in TER Force and Moments Tests; S_{MF}

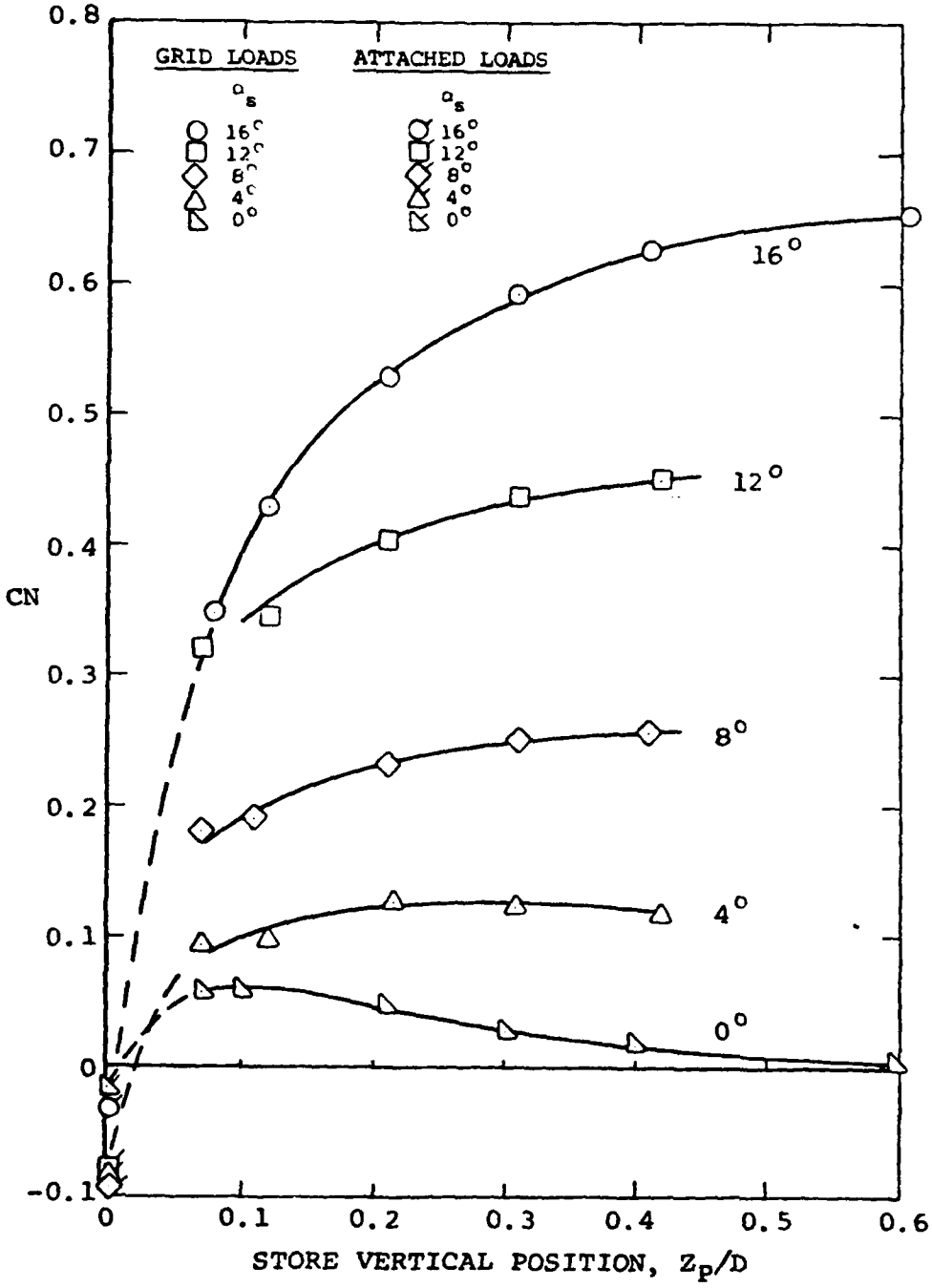


Figure 8.2 - Comparison of Attached and Grid Normal-Force Coefficients at M = 0.6 in Combination With Wing-Body Pylon and TER Rack; Bottom Store.

8.2 TRANSONIC SPEEDS

I would like to make a few general remarks about store separation at transonic speeds. A transonic method has been developed for determining flow fields at store locations (Reference 8.5). This method builds on the subsonic method mentioned in the previous section. Figure 8.3 shows a wing-body-pylon combination under which flow angles were measured in the 4-Foot Transonic Tunnel at Tullahoma. Figure 8.4 shows a comparison between theory and data for the flow angles just below the rack mounted under the wing for $\alpha = 5°$ and $M = 0.95$. What is remarkable is that the linear theory, shown by the dashed line, fits the data so well at this condition. The effect of compressibility on the downwash angle is small as shown, but the effect on sidewash is larger than measured. At higher angles of attack, transonic effects may be more significant.

Figure 8.5 shows a pressure distribution store which was used to determine normal-force axial loading distributions. Loadings for this store directly below the pylon ($Z/D = 2$) are shown in Figure 8.6 taken from Reference 8.6 for $\alpha = 0$ and $\alpha = 5°$. The ability of the linear theory to predict the flow field is better than for calculating loads. Better ways of calculating loads on stores embedded in transonic flow fields are needed.

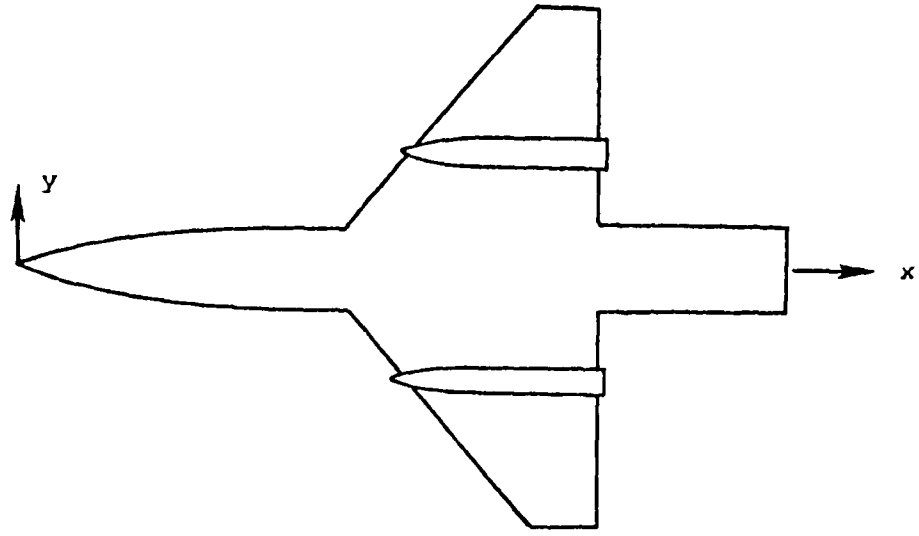
8.4 SUPERSONIC CASES

The delivery of missiles at supersonic speeds has received some attention, and a computer program to compute supersonic store trajectories (Reference 8.7) has been written. Supersonic store separation done with linear methods is not adequate for obtaining store forces and moments during separation. One reason is that the positions of shock waves differs from those for Mach waves as used in linear theory. The difference in position for a wave intersecting a store can introduce significant error with the linear theory calculations of forces and moment. Nonlinear corrections to linear theory are now used in Reference 8.7. Further work in the area of supersonic store separation is needed to understand all the problems involved.

One supersonic problem of particular importance is that of the excessive drag of stores externally mounted on racks. Novel ideas like conformal carriage promise greatly to reduce supersonic store drag. Further progress in this general area is needed with the general theme of designing the stores and airframes as an integral unit.

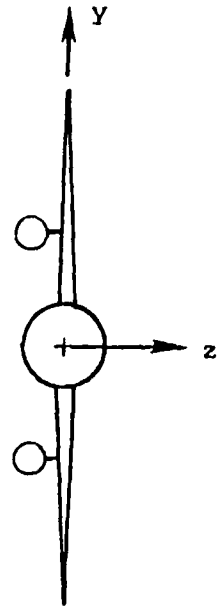
8.5 CONCLUDING REMARKS

With regards to fruitful areas for further analytical studies and computer programs; we can broadly conclude that attached loads need further attention for all speeds. Higher angles of attack need attention for both subsonic and transonic speeds in accordance with current air combat tactics. For subsonic speeds nonlinear wing characteristics must be accounted for, and for transonic speeds the usual transonic nonlinearities must be taken into account. Supersonic store separation involves nonlinearities also despite the fact supersonic linear theory is well established.



(a) BOTTOM VIEW

(b) SIDE VIEW



(c) REAR VIEW

Figure 8.3 - Wing-Body/Pylon/Store Model Configuration for AEDC 4T Tunnel Test Program.

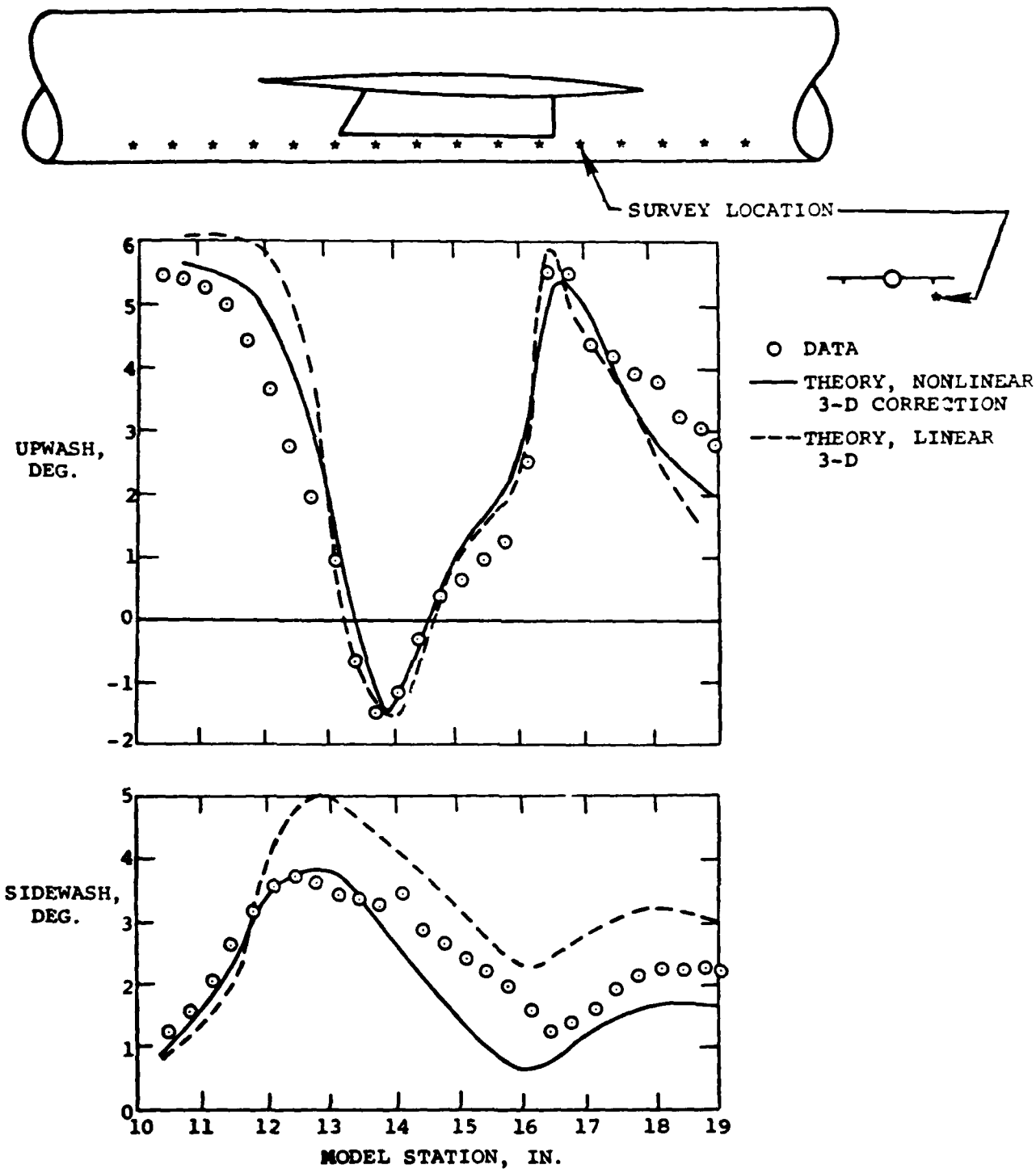


Figure 8.4 - Comparison of Theoretical and Experimental Results for Local Upwash and Sidewash Angles Directly Under the Wing Pylon for Flow Past a Scaled F-14 Wing/Body/Pylon Model at $M_\infty = 0.95$ and $\alpha = 5°$.

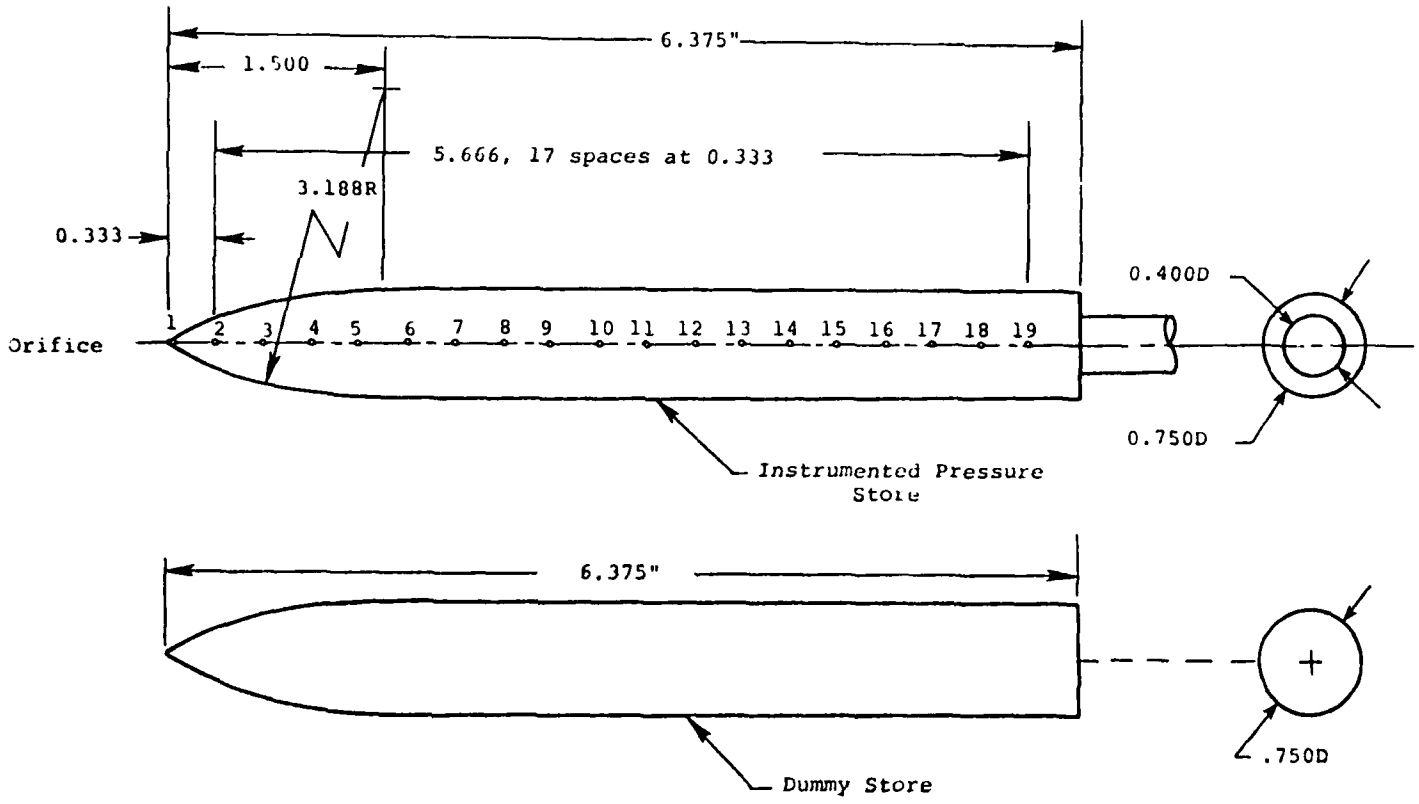


Figure 8.5 - Instrumented and Dummy Store Geometric Details.

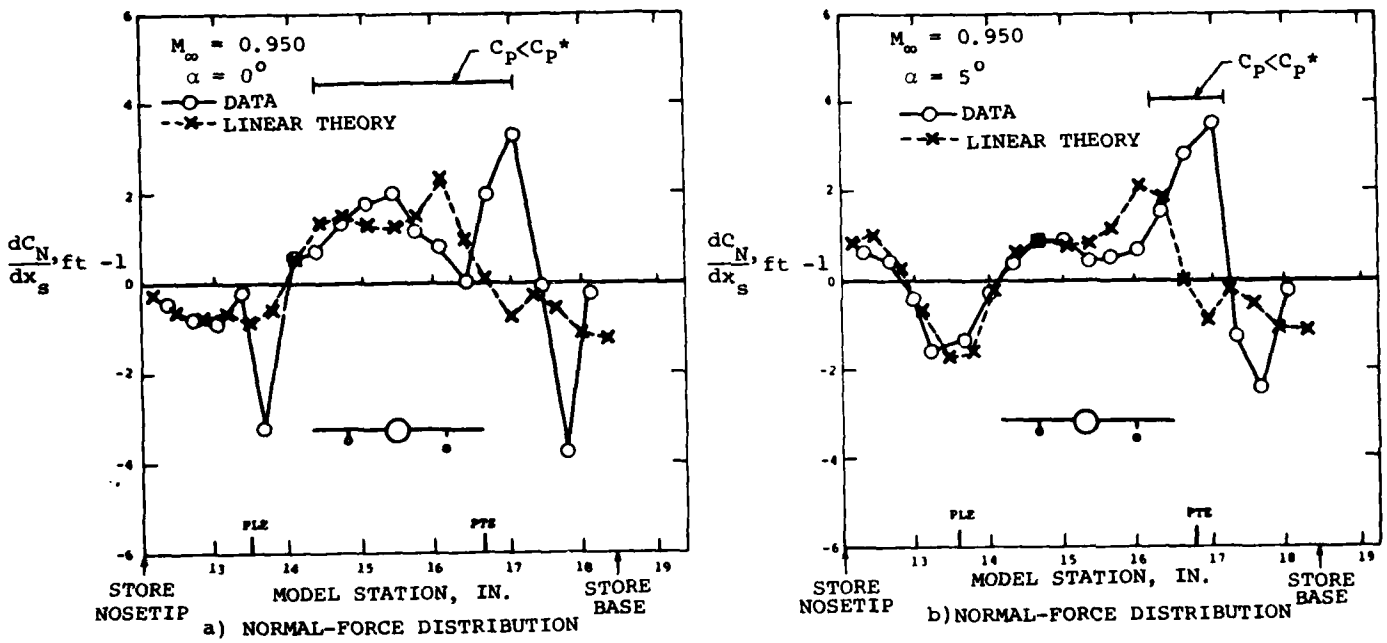


Figure 8.6 - Comparison of Store Theoretical and Experimental Normal-Force Loadings Based on Paneling Method Flow Fields for Store Separated From Wing Pylon at M_∞ = 0.950 and α = 0°, 5°.

One recommendation I have made for a number of years and I would like to repeat it. Since large numbers of aircraft and stores are used in different combinations, it would be useful to compile a data bank of aerodynamic models of those components to use with the subsonic store trajectory program. Such a data bank would eliminate duplication and make it possible to run trajectories with much less effort since the principal effort is usually devoted to modeling airplanes and stores.

9. EXPLOITATION OF LARGE-SCALE COMPUTERS

9.1 INTRODUCTORY REMARKS

The application of large-scale computers to missiles has lagged its application to airplanes for reasons which are not clear to me but which may have to do with aerodynamic efficiency. However, there is increasing emphasis in this area for missiles, an emphasis which will probably increase with the growing interest in airbreathing propulsion. While large-scale computers are not likely to be the principal tool of preliminary design for some time, they provide several important services at the present time. They provide benchmark cases for evaluating the accuracy of more approximate methods. They can also be used to develop data bases for use in approximate methods. They are also useful in verifying final designs. With further improvements in computer capability and reduction in cost, their application will greatly increase.

9.2 LEVELS OF SOPHISTICATION IN COMPUTER PROGRAMS

At least four levels of sophistication can be differentiated in computer programs of interest in missile aerodynamics.

(a) Engineering prediction codes
(b) Potential flow codes, linear and nonlinear
(c) Euler codes
(d) Navier-Stokes codes

After some preliminary remarks about the first two methods, we consider the last two in greater detail.

Engineering prediction methods as referred to here are approximated methods which are based on engineering assumptions and/or data bases. These programs generally do not need large-scale computers, although extensive data bases can be put into core if they are available.

Potential flow codes of the linear type are typified by panel programs for complete configurations (such as Reference 9.1) and for nonlinear potential flow by the Bailey-Ballhaus program, Reference 9.2. For nonlinear programs the present computer capability is taxed for complete configurations and, bigger machines will probably be needed for multi-finned missiles. Their limitations to low angles of attack can be partially overcome by incorporating vortex models into them as in DEMON2 (Reference 9.3). Potential methods break down when strong shock waves are present. Although research is underway to partially alleviate this problem, Euler and Navier-Stokes codes are really required.

9.3 NAVIER-STOKES CODES

It is generally acknowledged that present computer capacity is too limited to solve the flow about complex three-dimensional configurations with Navier-Stokes codes. In the particular cases where a calculation has been made for a body, the computer costs have been prohibitive for preliminary design use. However, if progress in computer development in the future keeps pace with that of the past, it is only a matter of time before the problems of computer capacity and cost will be overcome. I presume the invited lecture of Dr. Ballhaus on the future plans of Ames Research Center, NASA, will contain some interesting material on this subject.

Another limitation in the use of Navier-Stokes codes at the present time is the lack of understanding of turbulence modeling. It turns out that many problems in missile aerodynamics are dependent only on turbulent convection, not turbulent diffusion, so that there is some relief from this limitation. However, when larger machines are available it will be possible to create turbulent models of the required accuracy through an approach called large-scale eddy simulation (Reference 9.4). In large-scale eddy simulation, the unsteady Navier-Stokes equations (filtered) are solved to follow the motion of the eddies down to the smallest scale that can be handled within the capacity of the machine. Smaller eddies are modeled by some universal law. The hope is that large eddies, whose statistics depend on the geometry in question, can all be treated within the capacity of the computer, and the effects of small eddies which follow universal laws can be modeled. For low Reynolds numbers and periodic boundary conditions, predictions by this technique have shown good agreement with experiment. The use of larger computers will permit solutions for higher Reynolds number.

For use with the time-averaged Navier-Stokes equations, eddy viscosity models are usually used. It turns out that there are many classes of flow with different eddy viscosity models. In my Wright Brothers paper I have suggested that NASA create a national data bank of eddy viscosity models.

9.4 EULER CODES

The Euler equations can be used where vorticity convection is important but vorticity diffusion is not. Many missile aerodynamic problems fall within this realm. In these cases the Euler equations will require less computer time than the Navier-Stokes solutions, not only because the viscous terms are not present, but because a fine mesh to resolve the boundary layer is not required.

The problems of the appropriate boundary conditions to use with the Euler equations is still very much an open question. It is through the boundary conditions that the vorticity is shed from the solid boundaries into the flow field. By using a Kutta condition at a subsonic edge it has been possible (Reference 9.5) to discharge vorticity into the flow. Figure 9.1 illustrates the calculated flow field. Also by inputting a separation line location and appropriate boundary condition, it has been possible to calculate flows with primary vortex separation on a body of revolution (Reference 9.6). Figure 9.2 compares vortex strengths calculated by this method with the data of Oberkampf. Much of missile aerodynamics can be predicted with a supersonic marching code for which the calculation times are matters of minutes. However, if the

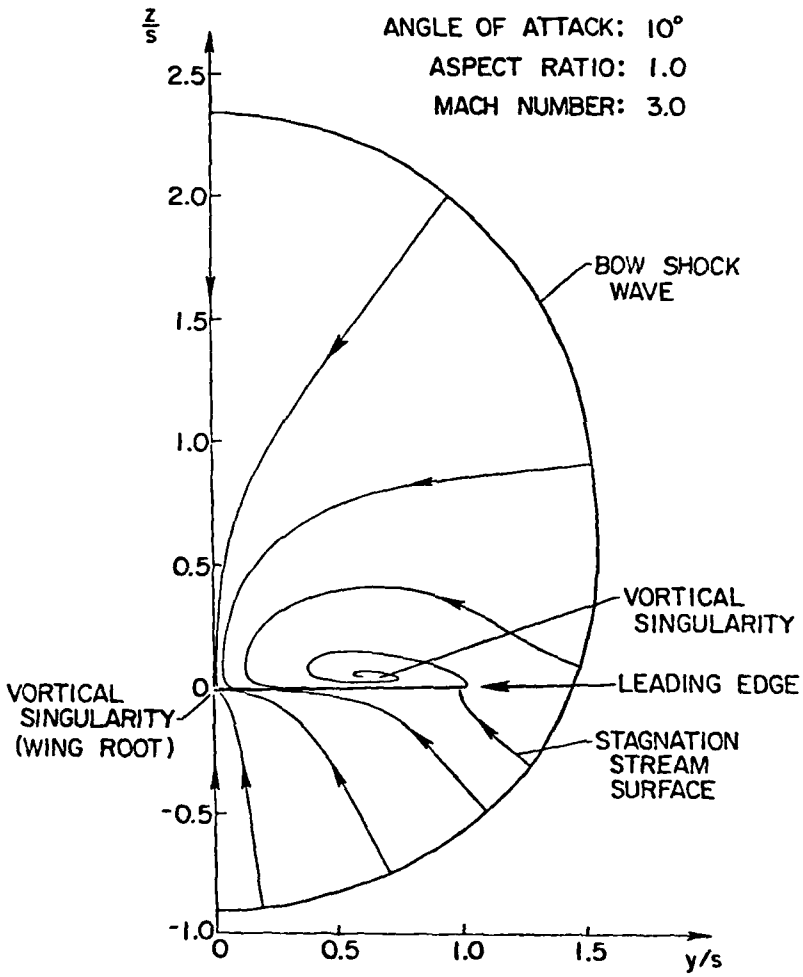


Figure 9.1 - Projection of Streamlines on Crossflow Plane as Determined from Euler Code with Kutta Condition at Leading Edge of Delta Wing.

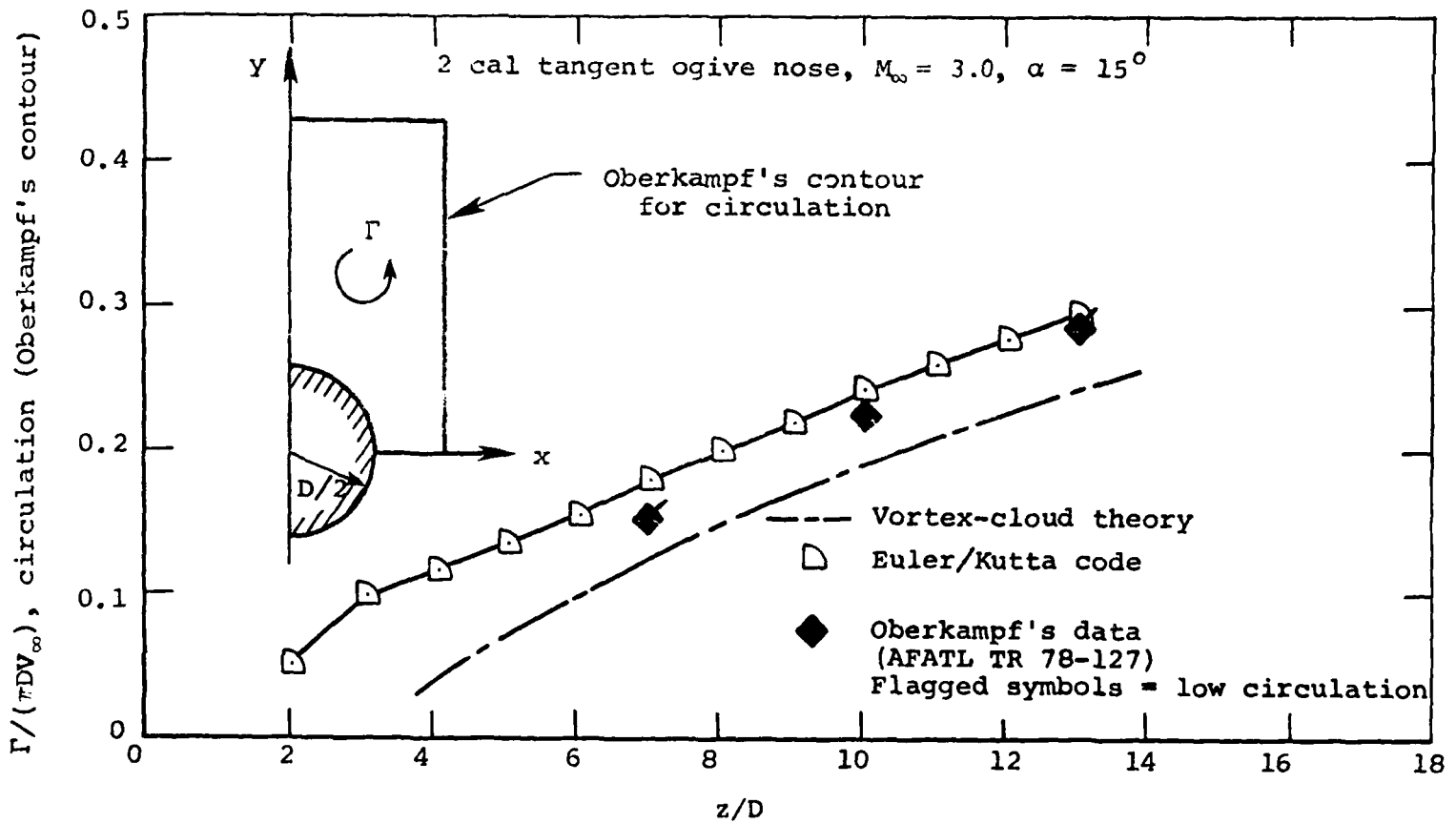


Figure 9.2 - Comparison of Circulation for Euler/Kutta Code, Vortex-Cloud Theory, and Experimental Data.

axial Mach number is subsonic, existing methods of solving Euler equations take much time. Mathematical techniques for overcoming this problem are of considerable interest.

Existing application of Euler equations to missile aerodynamics include bodies alone (Reference 9.6), wings alone (Reference 9.5), and wing-body combinations (References 9.5 and 9.7). In addition, the Euler equations have been applied to missile inlets (Reference 9.8).

I cannot conclude a discussion of large-scale computers without addressing the question of the future role of the wind tunnel versus the computer. Much controversy has surrounded the subject since the thought-provoking paper of Chapman, Mark, and Pirtle (Reference 9.9). In their paper they state: "When a sufficiently advanced computer becomes available, we believe it will displace the wind tunnel as the principal facility for providing aerodynamic flow simulation." There is no doubt in my mind that many measurements now made in the wind tunnel can be calculated just as well on large computers, and that more of the conventional wind-tunnel problems will be tractable on computers in the future. The rate at which this will happen can be argued. However, the above quote does not imply that the wind tunnel will be superseded by the computer. Indeed, it can be argued that the requirements for wind tunnels will be increased. The wind tunnel can reproduce fluid mechanical phenomena for which the physics is not understood and hence which cannot be put into a computer. Also wind tunnels and computers can be used to verify the results of one another. Wind tunnels and computers can reinforce each other in other synergistic ways in such applications as "smart" wind tunnels and conditional sampling. The requirements for both will thus continue and, in my view, will increase.

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8