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The Boundary Layer and I

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Nomenclature

A	= amplitude of Tollmien-Schlichting waves or Görtler vortices; also Van Driest's damping-length constant
A_0	= amplitude at neutral stability
c	= constant in velocity distributions of the form $u_e = cx^m$
C_{D_p}	= profile drag coefficient
F''_W	= transformed value of $\partial u/\partial y$ at the wall in the Falkner-Skan equation
k	= height of a roughness
m	= exponent in velocities of the form $u_e = cx^m$
n	= measure of amplification ratio, $n = \ln(A/A_0)$
Pr	= Prandtl number
r	= longitudinal radius of curvature for Görtler vortices
R_c	= chord Reynolds number
R_θ	= Reynolds number based on momentum thickness
u	= velocity in x direction
u_e	= edge velocity
x,y	= coordinates, generally along and perpendicular to the wall
β	= Hartree's boundary-layer profile parameter, note $\beta = 2m/(m+1)$
γ	= intermittency factor
δ	= thickness of a boundary layer
δ^*	= displacement thickness
ϵ	= turbulent eddy viscosity; ϵ_i, inner, ϵ_o, outer
θ	= momentum thickness of a boundary layer
κ	= von Kármán's constant
ν	= kinematic viscosity
ρ	= mass density
Λ	= Pohlhausen's $\Lambda = (\delta^2/\nu)(du_e/dx)$

I. Early Motivation

MY story begins in June 1945, in a basement at the Göttingen Aerodynamische Versuchsanstalt (AVA), Germany, at the end of World War II. But first let us back up slightly. In 1937 and 1938, I took a course, called AE 267 - Real Fluids, at Caltech from C. B. Millikan. This course dealt with laminar and ordinary viscous flows. AE 269 dealt with turbulent flows. AE 267 was rather similar to the laminar and viscous portions of Goldstein's *Modern Developments in Fluid Dynamics* and Prandtl's contribution in Durand's *Aerodynamic Theory*, Vol. III. It was a very good course and Millikan was probably the clearest and best organized teacher I ever had. I know that he always conscientiously reviewed his next day's lectures the day before. However, I was mainly interested in becoming a preliminary designer, and at that time, boundary layers and viscous effects seemed quite long-haired and abstruse.

Part of the course dealt with the Orr-Sommerfeld equation governing the stability of laminar boundary layers and attempts at the solution of the equation. This was long before Schubauer and Skramstad verified its applicability to the process of boundary-layer transition, so the analysis was a kind of academic exercise. At one time in some general discussion, Millikan mentioned that suction should change the shape of the boundary-layer profile and probably make it

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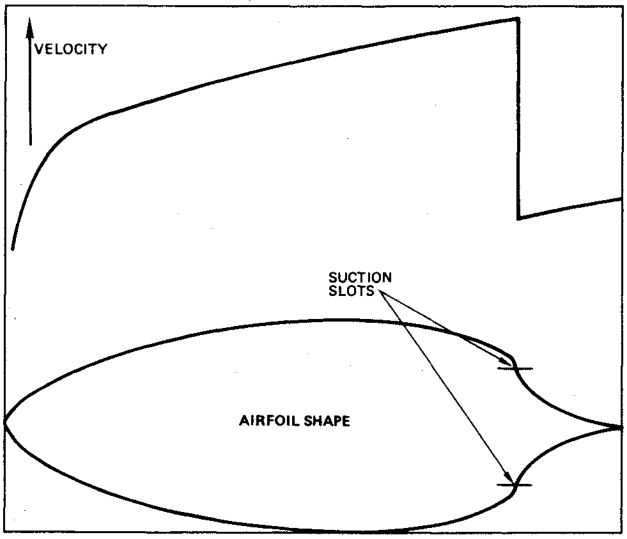

Fig. 1 A typical thick symmetric Griffith airfoil, $\alpha = 0$ deg.

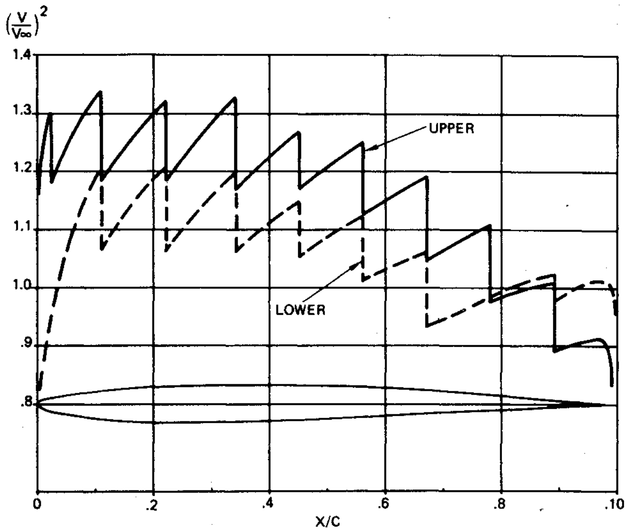

Fig. 2 The DESA-2 airfoil and its pressure distribution, 6.6% thick, design $C_L = 0.1$.

more stable, but nothing was known about how much suction it would take or whether there could be net gains in efficiency, such as net gains in total required horsepower. After leaving Caltech, I did not remember much about this theory but did remember his conjecture regarding stabilization by suction and the question about how much suction might be required.

My work at Douglas Aircraft from 1938 to 1945 was on general airplane design and ordinary aerodynamics, and I never thought about the subject of laminar-flow control, although my Master's thesis had been on high-lift boundary-layer control. However, in 1945 at the end of the war, I was appointed a member of the U.S. Naval Technical Mission in Europe (NAVTECMISEU) to help investigate the German aeronautical developments. One part of my work involved exploring the Göttingen AVA and Kaiser Wilhelm Institute (KWI) complexes. I got into their reprint room in a basement and browsed through many interesting new and old and famous classical papers. One that I came across was by Schlichting and Bussmann.[1] This paper solved the Falkner-Skan equation for suction and blowing type of boundary conditions on the wall. The authors found that the stability would be changed tremendously by very small amounts of suction. Thus, it answered the question posed by Millikan seven years before and caused me to pay serious attention to laminar-flow control as a possibility for drag reduction. However, I did not immediately try to do any work in this area and nothing happened, except that I followed Pfenninger's Swiss work. Then it was my good luck to be in Washington in December 1947 at the time Sidney Goldstein gave his Wright Brothers lecture about the Griffith airfoils developed in England. A typical Griffith airfoil is illustrated in Fig. 1. It has long, continuously favorable pressure gradients on each side. In order to close the airfoil, there must be a pressure jump toward the rear, and after that, the flow again accelerates. At the pressure jump, suction is applied to prevent separation. One of the unique features is that, behind the slots, the shape must be concave as seen in the illustration in order to develop the higher pressures. While listening to the lecture, it occurred to me that an airfoil could be designed with several small pressure jumps instead of one large one on each side—perhaps 10 per side. My design would lead to an airfoil that had a more conventional shape, and in case suction failed, its performance should not deteriorate catastrophically. I wanted to find something better than Pfenninger's approach of using a large number of slots (40 per side or more) and its resultant mechanical complications.

But as already noted, when I learned of the results of Ref. 1, I did not immediately try to start work on laminar flow airfoils. In 1948, I believe, I attended a Rand Corporation briefing on ways to extend the range of aircraft. One way was by aerial refueling. This seemed very wild to me and if people were that desperate to extend the range, then laminar flow control should surely be looked into as an alternate. After that briefing I began active work in this area, mainly around my idea. Then in attempting to design practical and successful Griffith type airfoils I was forced into experimental and theoretical boundary-layer work rather extensively.

II. DESA-2 Airfoil and Theoretical Developments

This airfoil was the type I conceived at Goldstein's lecture. It was not successful, but it was a very basic stimulus to much of my work in the field, so I feel it deserves separate mention. We first designed and tested a preliminary model, the $G0010_7$ airfoil. It was symmetrical and had a flap. We obtained 98% laminar flow to the capacity of the wind tunnel, $R_c = 4.25 \times 10^6$. At that time this was the highest Reynolds number for full laminar flow that yet had been reached. The tests were a complete success. Therefore, we began designing a thin, cambered and more realistic shape for tests up to $R_c = 40$ or 50×10^6 in the NACA two-dimensional pressure tunnel. It was 6.6% thick. The shape and pressure distribution are shown in Fig. 2. Just behind each pressure jump is a short concave region. At the scale of the figure, the waviness and concavities required to generate the sawtooth pressure distribution do not show. Moreover, the pressure jumps are so low that, in case of suction failure, the shape should act like an ordinary airfoil. In wind tunnel tests, both the $G0010_7$ and the DESA-2 did so behave.

The model has two unique features: 1) very strong stabilization due to the very favorable pressure gradients and 2) several short regions of substantial concave curvature behind the slots. The concave curvature was substantial, but not so great as to invalidate boundary-layer theory. By this, I mean that, in the basic equations, δ/r is still small. Here, δ is a measure of the boundary-layer thickness, and r the longitudinal concave radius of curvature. Moreover, the regions were very short. In thinking about this situation as well as the favorable gradient regions, it did not seem reasonable to me that transition would be determined by an instantaneous value of some parameter. To change the flow, some process had to act for a finite period, because physical processes are never truly impulsive, and there is nothing to give an impulse. It seemed to me that any analysis of the process should involve an integration, not just a limiting value, unlike an allowable stress in the science of strength of materials.

The concave regions would develop Görtler vortices, which if too strong, would cause transition. Görtler's work dealt only with neutral stability; however, in our problem, there would be strong instability, but only for a very short time. Hence, we wanted to know rates of growth, and Görtler's paper gave no indication of rates. Thus, this peculiarity of the DESA-2 directly involved me deeply in boundary-layer theory and made me feel that an amplification ratio was more significant than the maximum value of some parameter, e.g., Görtler's $R_\theta(\theta/r)^{1/2}$. Here θ is momentum thickness, and R_θ is the usual momentum thickness Reynolds number. Furthermore, it should be noted that the stability equations yield rate parameters, not strength parameters. Strength of a disturbance is some kind of integral of rate times distance, or rate times time.

During the design of the DESA-2 airfoil, I often stated that I was not sure the airfoil would work, but at least I was determined not to have to apologize for a poor job of designing it. Therefore, I decided to try to calculate the growth rates of Görtler vortices. For me this was a substantial effort, because, up to that time at Douglas, I had hardly used any mathematics beyond ordinary algebra. I studied various methods of solving the eigenvalue problem, including the basic mathematics. I remember studying Pauling and Wilson's *Introduction to Quantum Mechanics* and Margenau and Murphy's book, *The Mathematics of Physics and Chemistry*. Also, you might say Ince's *Ordinary Differential Equations* was my bible. In the boundary-layer field, I studied both Meksyn's work, G. I. Taylor's, and Görtler's own. In fact, we solved several cases by Görtler's own method for neutral stability, but it did not seem to have the power needed for solving the problem we wished to solve, which involved amplification rates rather than just neutral stability. As will be seen, we did find what we thought was a more powerful method.

However, one problem bothered me. I could not tie time and distance together, as can be done with Tollmien-Schlichting waves, and of course, Görtler's and Meksyn's analyses were for wave growth in time. I pondered this problem for some months and one weekend, while laying flagstones in my backyard, saw the solution. Instead of using a disturbance expression that grew as exp$t$, the problem should be reformulated into one that grew as exp$x$.

At about the same time, late 1949 I believe, I talked with Douglas Dill, head of our Douglas El Segundo Flutter Group. I was looking for help on methods of calculation. He suggested that I look into Galerkin's method. I did and found it so powerful that it could nicely handle the kind of problem we wished to solve. This method involves solution of a matrix. Mechanical automatic calculators, which were good at matrices, were just arriving, and this was more incentive for using Galerkin's method.

Accordingly, we worked hard on deriving the extended equations and studying their structure, order of magnitude of various terms, methods for calculation of eigenvalues, etc. Frazier, Duncan, and Collar's book, *Elementary Matrices*, was my source book for matrix methods and theory. I was forced to gain a great deal of knowledge or I would never have solved our problem. The unusual nature of the DESA-2 and the high-performance stakes were the stimulus. At the design $R_c = 50 \times 10^6$, the predicted effective drag coefficient was $C_{D_p} = 0.00064$. For comparison, wind tunnel tests show the drag of the NACA 66,1-015 airfoil to be $C_{D_p} = 0.0050$ at this same Reynolds number. Therefore, as you see, the DESA-2 airfoil and its accompanying stability problem caused me to pay much more attention to boundary layer and applied mathematics literature.

As an aside, it should be noted that the calculations we ended up doing can only be called "heroic." While we studied accuracy with lesser matrices, for our final results, we set up a 12×12 matrix system, expecting the IBM machine to give us the answers. When we gave the operator the first case, we started solving it on a desk calculator as a check. The process required inversion of the matrix and repeated multiplication by a column to get the eigenvalue. Also, the matrix was nearly singular, i.e., very difficult. The man who attacked the problem with a desk calculator was D. A. Callin. The IBM operators ran into trouble with accuracy. In the meantime, Callin plugged along and got our first answer. So we started on the next case, and the IBM operators still had trouble, but Callin got the second answer, and so it continued until we finally calculated 26 different cases, 25 by Callin. The IBM machines finally gave us two answers, which only checked the hand calculations already completed. Each case required just over 100 h of continuous hand calculation, often involving double-precision arithmetic. You can see that the calculations took over a year of continuous work by one man. Callin could do all this effortlessly, often working two days without a mistake. If we had not been so fortunate as to have his help, I doubt if we would have been able to finish the problem, certainly not by the time we needed results. Thus, whenever I think of him I think of him as the virtuoso of the Marchant.

The calculations and work were successful, and by the spring of 1952, we had most of the answers to the Görtler stability question. At the same time we applied our results and found that an amplification ratio correlated data much better than Görtler's parameter $R_\theta(\theta/r)^{1/2}$ at transition. In fact we found that Görtler type transition occurred about when $\ln A/A_0 = 10$, A/A_0 being the amplification ratio. This work was nearly complete in early 1952, but was all classified. The internal Douglas report, ES 17110, was published in March 1953. We later studied and correlated further data which ended up as my paper on Görtler stability, including the e^n method of predicting this kind of transition. As noted, transition was found to occur when $\ln A/A_0$ was about 10. Thus, you see that the DESA-2 airfoil had much to do with development of the amplification ratio method, first stimulated by the Görtler problem. For the very short highly concave regions, it did not stand to reason that a high value of Görtler's parameter, acting for an exceedingly short time, should cause transition. Rate and total growth of the disturbance somehow must be involved. For the DESA-2 airfoil in the worst concave region, Görtler vortices grew across it by a ratio $\ln A/A_0$ of 5 at $R_c = 60 \times 10^6$ so we thought we were safe.

Naturally we wondered just how rapidly the convex regions would damp out the Görtler vortices. We tried stability calculations several times for the convex regions, but never could get convergence to an eigenvalue.

The other considerable effort was on the question of Tollmien Schlichting stability. It seemed to me that amplification rate and ratio for this kind of problem also gave the most rational guide to transition, but at the time, the e^n method had not been fully developed. In fact, the thinking was something of a carry over from the Görtler problem. The prediction of transition was in a primitive state. As I remember, the best way of predicting transition was to use some value of R_θ as the criterion. We needed our guidance around 1950 or 1951, and R_θ was the only guide. Significant papers on the subject such as Gazley's and Granville's did not come out until 1953. Michel's paper appeared in 1951, but we did not know about it until after the DESA-2 was tested.

R_θ at transition was not then known as a function of the pressure gradient or Pohlhausen's Λ. From NACA airfoil tests, we of course knew there was, indeed, an effect of pressure gradient, and we wanted a method of analysis that at least took into account some of the changes in stability caused by it. Pretsch's charts were a great help on this question and on the airfoil design. I do not remember all our thinking at the time, but in the report, the airfoil is very nearly designed as if the e^9 criterion was applicable. That is, at $R_c = 50 \times 10^6$, the calculated amplification ratio for the entire airfoil is $e^{9.5}$ on the upper surface and $e^{7.2}$ on the lower surface. In fact, in the report,[2] the design conditions are stated to be as follows.

1) Airfoil thickness to be 6.6% in order to be usable on the F4D-1 airplane, if desired.

2) Design lift coefficient $=0.1$.

3) $C_{m.25}$ to be zero at $C_L=0.1$.

4) At $R_c=50\times10^6$ for Schlichting waves the amplification ratio at the trailing edge must be less than exp 10.

5) At $R_c=50\times10^6$ the amplification ratio for Görtler vortices must be less than exp 10 and preferably as low as possible (for each concavity).

6) From the practical standpoint, the design should have as few slots as possible.

Thus, it is seen that the stimulus for the e^9 method lay in the DESA-2 airfoil. By 1952, a large number of amplification ratios at transition had been calculated from the various test data available, and while I believed they formed the most logical method of prediction, there was a large amount of scatter in the calculated values, so no rational method of prediction was evident. That was not so true for the Görtler problem. In it, for the available test data, the calculated amplification ratios at transition were fairly uniform, at a value $\ell n A/A_0=10$. Once, probably in 1953, I showed our studies of test data to a co-worker, Deane Morris, who later moved to the Rand Corporation. He saw the large scatter in calculated amplification ratios at transition and suggested that, instead of worrying about it, we just take some average value and see how well we could actually predict the transition point. We did and found the point was predicted well and rationally. Thus was born the e^n method, at least my contribution. After further studies and rounding out the work, I presented my paper about it in 1956. For test data at transition, the calculated amplification ratio is often a sensitive parameter. Conversely, the position of transition is relatively insensitive to the value of the amplification ratio.

It should be noted that this development was not started with the expectation of finding that an amplification factor alone would be a suitable criterion, because it is only something that multiplies an initial disturbance. Transition must depend on disturbance strength, not just a ratio. However, calculating the amplification ratio was a well-defined problem, and so it was done first. We then expected to study turbulence inputs at the neutral point. Some work was done, such as multiplying percent turbulence times amplification ratio to get a disturbance strength to see if we could predict the effect of turbulence on flat-plate transition. This proved no good; the problem is far more complicated. However, two things happened, the DESA-2 contract ended, and thanks to Deane Morris's suggestion, we found that A/A_0 by itself was a good parameter. That is about where the prediction of low speed transition stands to this very day.

A lesser development, but also stimulated by the peculiar pressure distribution of this airfoil, was my piecewise method of approximate laminar boundary-layer calculation.[3] The sawtooth pressure distribution gave me the idea that ficticious origins could be used for similar flows of the type $u_e=cx^m$, from which approximate solutions quickly could be patched together. We especially wanted to use similar solutions because Pretsch's stability charts were for various values of Hartree's β. As I remember it, this idea came up in some discussion one afternoon, and by the end of the day, we had made our first general calculation. A preliminary form is in the DESA-2 report.[2] We gradually assessed the method, refined it, and published it in 1956.

Another event in connection with the DESA-2 airfoil was an attempt at finite-difference boundary-layer calculation. To answer stability questions, we wanted to know very accurately the boundary-layer profiles between slots, and integral methods are inadequate. I studied various methods of writing a finite-difference system and selected one that seemed the best. I then gave it to the head of computing, W. C. Schlieser, and asked for an estimate of the machine time required to calculate 20 or so stations. He came back with an estimate of 2000 machine hours! That stopped my interest in finite-difference methods for the time being. The computer was the IBM Card Programmed Calculator—a semimechanical computer.

In tests on the DESA-2, we could get 100% laminar flow only to a Reynolds number of about 6×10^6, so the attempt and my airfoil idea were unsuccessful. I never understood why until 1978. Not knowing any better, I had assumed that the much longer convex regions between the concave regions would damp out the Görtler vortices so much that, at each new concavity, the flow would essentially start out afresh; Tollmien-Schlichting waves behave this way. When they encounter a sufficiently favorable pressure gradient, they damp out. Besides, since the airfoil has significant thickness, there was more convexity than concavity. In 1978, Professor Eli Reshotko pointed out to me the principle of exchange of stabilities. According to this principle, when the flow encounters a concavity, Görtler vortices amplify; but when they reach the convex portion, they do not damp, they remain neutral in stability. Mathematically speaking, it works out that, for concave flows, the exponential giving the eigenvalue has both a real and imaginary part, the real part being a measure of the amplification rate. However, for convex flows, it is a pure imaginary, meaning an undamped fluctuation. Hence, the behavior of Görtler vortices is considerably different from that of Tollmien-Schlichting waves. In my original concept of the entire amplification process, I visualized it as a series of climbs and descents; but according to the principle of exchange of stabilities, the process would be something like a staircase: a climb, a level region, a climb, a level region, etc. So the total amplification ratio for the airfoil would be the product of the amplification ratios for each individual concavity. Also, this neutral stability for convex surfaces probably was the cause of our inability to converge to eigenvalues in our Görtler calculations when we tried such surfaces.

Therefore, the airfoil was not a practical success, but it was an indirect success because, with its peculiar sawtooth pressure distribution, it was a great stimulus to development of boundary-layer methods.

Another study was of the size of roughness that could trip a laminar boundary layer, done for the U. S. Navy. While our work on developing a laminar-flow airfoil had stopped, we still had our knowhow, and in 1954, the U. S. Navy asked us to look into the practical problems of laminar boundary-layer control. It seemed to me that one of the most severe practical problems was that of roughness tripping the boundary layer. We made a large wind tunnel model and very carefully and extensively looked into this problem. Our interest was the height of roughness that first affects a long laminar run. There had been many tests by others, e.g., Dryden, Tani, and Potter, on the roughness required to bring transition to the roughness but not on the height of roughness that first affects transition. About the time we had obtained all our data and were studying various ways of correlating it, I talked to Professor Hans Liepmann. He commented that he and Frank Goddard were having very good luck on the problem of turbulent skin friction of rough surfaces at high Mach numbers by correlating test data with really local conditions at the bottom of the boundary layer. We followed suit and obtained very successful correlation by using the true Reynolds number of the roughness, $u_k k/\nu_k$ where k is the height of the roughness, u_k the velocity in the boundary layer at the top of the roughness and ν_k the kinematic viscosity at the same point. Both on this problem and many others, a great many results were all explained by correlating with the true local flow at the roughness. Hence our advance was in the correlation method, and it was due a good deal to the conversation with Hans Liepmann.

The Navy did nothing more than ask us to look into the practical problems of laminar-flow control. Thus, the idea of tests of roughness were entirely our own, in fact, my own as I remember. Also, the idea of the DESA-2 project was entirely my own. There was no spirit of competition on this problem or trying to out-do others, except very indirectly as an improvement over Griffith airfoils. On both the transition and piecewise method of boundary-layer calculation, one might say we were working in a vacuum. The procedures just seemed quite logical, so we explored and implemented them.

III. Some Comments on the Practical Side of Getting Work Done

The two wind tunnel models were rather expensive, and a great deal of theoretical work was involved that took man-hours. Experienced readers may wonder how I managed to get the work authorized. I do not really know because I did not have any problems like those in later years. For exploratory theoretical work there was a substantial budget of indirect time (called 9603 time at Douglas) that one was relatively free to charge to in the late 1940's. Gradually, due to tighter budgetary controls, this fund became smaller and smaller over the years.

I do not remember any specific battles to sell authorization of funds for the $G0010_7$ or DESA-2 airfoils although I do remember talking about them and their principles. The work was authorized as amendments to the XF4D-1 tailless airplane contract as peripheral studies with the idea that results might be applicable to the airplane in the future. So you see that funds did not have to be contributed by the company, they came from the Navy. I have always found the Navy alert to developments that might lead to better airplanes. Because the work was under amendments to a classified military contract, it too was classified until the end of 1955.

The computer mainly used at this time was the IBM Card Programmed Calculator (CPC). It was an electromechanical relay type and a great advance over a Marchant-type desk calculator, but almost infinitesimally slow compared with modern calculators. A set of IBM cards was properly punched, using machine language, and put in a hopper to instruct the machine on what to do. It was a considerable advance over earlier ones for which a plugboard had to be wired. Storage was limited and mechanical. Computing rate was governed by rate of feed of the card deck. I did not observe the operation of the CPC closely when we tried to solve the Görtler problem but I did on another problem.

The original table of solutions to the Falkner-Skan equation were calculated by D. R. Hartree, using a Bush-type mechanical differential analyzer which yielded between three- and four-place accuracy. In our airfoil and other work we used his solutions so extensively that it seemed to be a good idea to work out more complete and more accurate solutions. Now that a much better and digital type of computer was available, it did not seem to be a large job.

Accordingly, we started working, charging to the general 9603 indirect charge number. (At Douglas, computer use was free just like a desk calculator, to foster their use; so in the good old days the only problem was access.) In a moment of optimism I decided to make tables of five-place accuracy. Only later did I gradually realize that developing that accuracy for certain on a nonlinear equation is a formidable undertaking. We solved the equation by a Taylor series extrapolation, beginning the work at Santa Monica because El Segundo's computer was not yet operable. After a while the work was transferred to El Segundo, where I often ran the machine myself, using the shooting method to meet boundary conditions.

If about 100 steps were taken and with the proper number of terms in the series to assure five-place accuracy, a run with given initial conditions would require about 15 min. One looked at the tab sheet to see whether the boundary-layer profile went above or below the asymptote. Then a new initial condition was punched into a card and a new try made. If one had uninterrupted use of the machine, a proper solution for a particular value of Hartree's β could be made in about half a day. So when I was doing this work I was mostly standing around, watching the tab sheet being printed, making plots to estimate the new value of Hartree's F''_W, etc. I hope this story gives you an idea of what automatic computing was like in its early days. The tables were published by the Institute of Aeronautical Sciences as a Sherman Fairchild Fund paper in 1954 but all the work was done in 1951.

One other aspect should be mentioned. When the program was transferred to El Segundo from Santa Monica, I naturally duplicated some runs. The printout was eight-decimal places, I believe. For a number of steps the new and old tab sheets would check exactly. But then after a while there would be a gradual drift; first the eighth place would not check, then the seventh, and finally down to the sixth or fifth, so that I could not be certain I met my goal of five place accuracy. I talked and worked with the computer staff on the problem for about two weeks and no explanation was found. The discrepancy could not be tolerated, so in desperation I sat down at a Marchant and, using exactly the same mathematics and roundoff procedures, set out to duplicate a run by hand. After about a day and a half of steady calculating I found my first disagreement. This gave some guidance as to where to look for the trouble. The deck of cards containing the program for this problem was about 0.9-in. thick. In order to reduce the frequency of reloading the deck in the hopper, eleven duplicate decks were made, making a dozen in all. What we found when we traced through all the decks was that one card had been misplaced so that eleven times instructions were correct but were wrong the twelfth time. The erroneous card determined the value of one of the last terms in the Taylor series extrapolation so the error was small. This experience has made me extremely cautious about trusting the output of a large scale computer on a complicated problem, because there are so many possibilities for error.

I was not sure of the propagation and growth of roundoff errors, so after finding all the solutions, I checked by rerunning them again with two more terms in the Taylor series and longer steps. Everything checked. I hope this chronicle gives you the flavor of automatic computers in the early days.

IV. Developing a General Method for Calculating Laminar Boundary Layers

After my success with the panel method of solving the Neumann potential flow problem, I chose as a home study project an attempt to solve the inviscid transonic flow problem. I studied at home for about a year without success. I considered all kinds of methods, but did not consider pure finite-difference calculations because the computing machines were too slow.

Thus you see that my sole interest was not boundary layers, but after about a year of frustration, I turned back to them. Now my goal was not to control the boundary layer but just to try to tell what it would do on its own. I wanted a method that would be accurate enough for stability calculations and that would be quite general. At the time, some methods had accuracy but were not sufficiently general, and other methods had generality but were not sufficiently accurate. In fact, Table 1, taken from report ES 40446,[4] is an excellent summary of the methods available at the time and their limitations. All, except the last method which was our development, had some sort of limitation. It should be said that Oscar Seidmann of the U. S. Navy supported our work, but he questioned whether our proposed work was any advance, and for this reason, the rather elaborate chart was prepared.

In trying to find a truly general and accurate method, I had no idea what to do. I read the literature extensively, both at Douglas and at home. I studied various forms of equations. I studied various methods of solution. There are many types of transformations, Crocco's, von Mises', Falkner and Skan's, Görtler's, Cope and Hartree's, and many others, not to mention all sorts of integral methods. For most transformations, I found some drawback. After looking at these

Table 1 Summary of principal methods for solving the laminar boundary-layer equations

NO.	METHOD	WORKING FORM OF EQUATION	IMPORTANT CONTRIBUTORS	ACCURACY AS MEASURED BY ERRORS IN τ, THE SKIN FRICTION	ADVANTAGES	CRITICISMS	PRINCIPAL REASONS FOR REJECTION AS BASIC METHOD FOR PRESENT INVESTIGATION
1.	Momentum Integral	Von Kármán's momentum integral equation	Von Kármán (Ref. 1) Pohlhausen (Ref.1) Walz (Ref.1)	3 figure accuracy possible in the most favorable cases. Greater than 100 percent error in adverse cases.	Very fast and convenient. In well conditioned problems accuracy is good*.	Accuracy entirely unsatisfactory for problems of boundary layer stability. Method insensitive to shape of upstream boundary layer. It considers only thickness.	Completely unable to handle many problems of importance. Poor, even on well conditioned problems in compressible flow.
2.	Correlation	None, it is based on empirical correlation of available exact solution.	Thwaites (Ref. 13)	About the same as above.	About the same as above.	About the same as above.	Completely unable to handle many problems of importance. Extensive compressible solutions essential for correlation are not now available.
3.	Local Similarity	Based on similar solutions, as of Falkner-Skan equation.	Smith (Ref. 16)	About the same as Nos. 1 and 2.	About the same as Nos. 1 and 2.	About the same as Nos. 1 and 2.	Method has promise as an approximate procedure, but little work has been done. At best it will be completely unable to handle many problems of importance.
4.	Infinite Series	Various transformations of momentum equation where x, y remain essentially as the independent variables.	Blasius (Ref. 1) Howarth (Ref. 1) Tifford (Ref. 14) Görtler (Ref. 1) Cope and Hartree (Ref. 15)	5 to 8 figure accuracy possible in the most favorable cases, as near stagnation point. Several hundred percent error in adverse cases.	Exact solutions can be obtained easily for certain fortuitous edge velocity distributions.	Cannot handle ordinary pressure distributions such as those of airfoils.	Unable to handle the most common type of pressure distribution. In high speed flow there are so many variables, that calculation of universal functions is impractical.
5.	Dorodnitzin	Transformed momentum equation in which x and y remain essentially as the independent variables	Dorodnitzin (Ref.17) Pallone (Ref. 17)	Theoretically exact, but insufficient work has been done to establish accuracy-computing time relation. High accuracy will probably require lengthy computation	Principal advantage is ability to give much better answers than No. 1 with reasonable labor. Method is essentially a generalized Pohlhausen method.	For high accuracy a high order system of simultaneous first-order differential equations must be solved. Starting requires special procedure.	Computing and setup time appears unreasonably long when high accuracy is desired as for B.L.Stability investigations.
6.	Finite Difference, Explicit	Unmodified momentum and energy equations.	Wu (Ref. 18) Görtler (Ref. 1)	Theoretically exact, but insufficient work has been done to establish the accuracy, computing time relation.	Simple and sufficiently fast for low accuracy. Theoretically can handle any boundary layer problem exactly in limit.	Boundary layer thickness in this system varies greatly, causing computing complications. Computing time excessive where high accuracy demanded. Has numerical stability problem.	Accuracy-computing time relation is poor, if high accuracy demanded. Starting the solution is a problem. Numerical stability question is a disadvantage.
7.	Finite Difference, Explicit	Crocco's transformed equation, in which x, u are independent variables.	Baxter and Flügge-Lotz (Ref. 19) Raetz (Ref. 20)	Theoretically exact. Accuracy high (3 to 5 figure) Computing time is long.	Theoretically exact and convenient formulation, because boundaries are at u = 0, u = 1, and Crocco's equation is second order.	Computing time great where high accuracy demanded. Crocco's equation not suitable for problems involving ''overshoot''. Has numerical stability problem.	Computing time too long, ''overshoot'' difficulty eliminates Crocco's equation as basis for a completely general method.
8.	Finite Difference, Implicit	Crocco's transformed equation, in which x, u are independent variables.	Kramer and Lieberstein (Ref. 21)	Theoretically exact, but insufficient work has been done to establish accuracy-computing time relation. Computing time should be short.	Same as No. 7 above plus rapid computing time. Computing is fast because a ''trick'' good for second order equations has been used. No numerical stability problem.	Crocco's equation not suitable for problems involving ''overshoot''.	''Overshoot'' difficulty eliminates Crocco's equation from consideration.
9.	Hartree-Womersley Finite Difference, Implicit	Simple transformation of momentum equation in which x, y remain essentially as independent variables.	Hartree (Refs. 4, 5, 6) Smith and Clutter	Theoretically exact. Accuracy high (3 to 5 figure). Computing time now medium, but could be reduced greatly.	Theoretically can handle any boundary-layer problem, exactly in limit. Computing time is good. Has best starting procedure of any exact method. No numerical stability problem.	Probably slower than No. 8 in cases where No. 8 can handle the problem.	

*By "well conditioned" is meant that the problem to be solved is similar to those for which the method is at its best. N.B. References are from original report.

various forms for some time hoping to find something that would give an easy solution but not finding it, common sense got the better of me, and I realized that what I wanted was in the full partial-differential Falkner-Skan equation. It did not distort the x axis and had no pathological problems such as with overshoot profiles, or starting. (An overshoot profile is one in which the velocity ratio u/u_e may exceed unity before decaying to its final asymptotic value. Accelerating flows past hot walls can generate them.) We gradually focused on this equation, but for a considerable time, we considered Howarth- or Görtler-type universal solutions for a series representation of the edge velocity. We studied this at Douglas. Both velocity and temperature distribution were represented by series expansion but I do not remember all the conditions being represented. Hajimu Ogawa, now a professor of mathematics, worked on this problem, and he obtained universal solutions that were far more general than any in existence, such as those of Tifford. It was a major accomplishment and easily could have been an important paper.

However, I got to thinking. To have an adequate supply of universal solutions for the many variables of a general problem, a very extensive library of universal solutions would be needed. I recalled the old saying, a table of one variable requires a page, two variables a book, three an encyclopedia, and four a large library. I realized that the number of differential equations that must be solved to provide the necessary universal functions would be far more than if I solved the basic Falkner-Skan partial differential equation directly for each individual problem that came up—at least in my lifetime. Besides, the universal-series-type solutions are poor for airfoil-type pressure distributions. Therefore, after accomplishing all this good work, we dropped it cold.

Because I knew Galerkin's method very well from the Görtler stability problem, we tried this form of solution, as far as writing out the system of simultaneous equations; but it was nonlinear, and we did not like it. Another that we studied was Picard's method, the method of successive substitutions. We tried it on Hartree's ordinary differential equation, and it was fast and converged beautifully for all values of β. I have already mentioned that I solved this same equation some years before by the shooting method, to construct the tables of solutions of the Falkner-Skan equation for various values of β.

With our success using Picard's method, we obtained Navy support to try to solve the full partial-differential equation. To do that, we planned to represent the x-derivative terms by finite-difference approximations which then gave us an ordinary differential equation at such x station to solve in the y direction. Hartree and Hartree and Womersley had used this method successfully with the old Bush mechanical differential analyzer to solve a number of problems.

Hence, in getting under contract with the Navy to solve the full equations, we had confidence because of our success with Hartree's ordinary differential equation. The additional x-derivative terms looked small, and we did not see them as any problem. In fact, we considered the flows more or less as locally similar, and the x term would merely be a slight correction.

However, that is not the way it worked. Picard's method now would not converge, and try as we might with various alterations, it still would not converge. The contract time was running out, and we were getting desperate. So one day, we dropped Picard's method and switched to the old shooting method that had been used without trouble on the Hartree β profiles. It worked fine. That is how our original method was born. It was hardly a cold, deliberate, and logical study that then gave birth to a successful method.

The original work was on the momentum equation alone. It is the worst problem, because the equation is nonlinear. After success with it, it was fairly easy to extend the same method to include the energy equation. Further extension was made to handle the species equation for such things as nonequilibrium flows. A summary of this work is in a Pergamon Press publication.[5] This method could truly produce high-accuracy answers for any two-dimensional or axisymmetric problem that arose. Any $Pr(x,y)$ (Prandtl number), any edge velocity, any suction distribution, any wall temperature distribution, or any heat transfer distribution could be analyzed. The only restriction we encountered was a sensitivity to short x steps toward the rear of the body, but that was overcome. Our original Douglas report was issued in July 1961.[4] While the method has been largely superseded, it still remains as one that is both accurate and simple to write out and program.

V. Other Laminar Work

About 1965, Professor Marten Landahl and R. E. Kaplan came out with their purification scheme of solving the Orr-Sommerfeld equation. Because this broke a bottleneck in the numerical solution, we decided to get active in this area and I talked it over with R. S. Shevell, Director of Aerodynamics, pointing out the gains in knowledge and the possibility of getting research-type contracts. He approved getting into the field; then I proposed work on it to Professor A. R. Wazzan, a consultant. He studied and learned the problem and the field and began calculations. Shortly after we began our work, a new book by Bellman and Kalaba, entitled *Quasilinearization and Nonlinear Boundary Value Problems,* came out in 1965. Bellman and Kalaba had very good reputations for contributing useful methods, so I bought the book and gave it to Tom Okamura in my group to see if it had anything useful in it. He discovered Gram-Schmidt orthogonalization which was a more rigorous and powerful generalization of Landahl and Kaplan's purification technique. I had nothing to do directly with its application but do claim enough vision to buy Bellman and Kalaba's book. I do not know how we stand timewise with respect to the orthogonalization procedure and other workers. For instance, Radbill et al. came out with the method at about the same time, but at least we discovered it independently. Eigenvalues are basically found numerically by calculating independent solutions of the Orr-Sommerfeld equation and then combining them to meet boundary conditions. One of the solutions used is a weakly growing exponential and the other is strongly growing. Due to roundoff the strongly growing solution can find its way into the weakly growing solution. Then, at high Reynolds numbers after calculating for some distance in y, one does not have the necessary two independent solutions needed for meeting boundary conditions. They are proportional. Gram-Schmidt orthogonalization keeps them independent.

After learning and exploring the Gram-Schmidt orthogonalization procedure which now made it possible to reach almost any Reynolds number, I thought a very useful contribution would be to modernize Pretsch's old Falkner-Skan stability charts and do both spatial and temporal amplification calculations. We did this, and the charts were first published in 1968.

The original e^9 method used Pretsch's charts, hand calculation, and interpolation between charts. We now had the capability of solving the stability equation at each x station for the exact profile at that station, supplied by the exact solution of the partial-differential boundary-layer equations as described earlier. Okamura, Jaffe, and Wazzan carried out this work which became a general and highly accurate method of calculating amplification ratios, frequencies, etc. The method then became rather dormant at Douglas Aircraft, because transports have no laminar flow.

Around 1974, the Navy first found out about this work and that it could analyze both two-dimensional and axisymmetric flows, the effect of heating and cooling in both water and air, suction, etc. Because such calculations were of little direct interest to Douglas, I proposed to the Navy that we pull together our potential flow calculations that gave pressures, our boundary-layer calculations that gave boundary-layer profiles, and our stability calculations that gave amplification rates. They bought the proposal, and it was pulled together by A. E. Gentry with A. R. Wazzan handling the stability aspect. Gentry named the program TAPS for transition analysis program system. It is now in widespread use by various Navy organizations doing preliminary design studies of torpedoes and other bodies.

One of the reasons the stability program was of interest to the Navy was that it had been extended to handle the problem of water flow past warm walls. This modification began in the middle sixties while we were still developing our boundary-layer and stability methods. I was aware that accurate solutions of boundary-layer flows in water with heat transfer did not exist so I suggested them as a kind of homework exercise for developing our methods, Douglas having no basic interest in water flows. These were worked out and Professor Wazzan made a simple but accurate modification of the Orr-Sommerfeld equation to analyze their stability. Both the boundary-layer calculations and the stability analysis were presented in 1967 at an ASME meeting. The stability analysis revealed a profound effect, a very small amount of warming the walls increases the stability very, very much, almost as much as suction. At about the same time (1968), Hauptmann discovered the effect, using approximate methods, whereas our methods were highly accurate. We had no knowledge beforehand of Hauptmann's work.

VI. Turbulent Boundary Layers

At least for airplanes, laminar boundary layers are not the common case, turbulent layers are. Recognizing this fact and now having success with the laminar case, I began to think about the turbulent problem, not in general, but only for boundary layers. It seemed to me there were certain basic relations that applied regardless of the pressure distribution—the law of the wall, Prandtl's mixing length relation, the velocity defect law, intermittency at the edge of the boundary layer, and Van Driest's modification of the law of the wall. Previous attempts at calculating turbulent boundary layers had all used approximate forms of Prandtl's equations or even none at all. I wondered what kind of answers we might get if we incorporated these rather universal laws into the exact equations now being solved regularly for laminar flows. Incorporation as we did it just meant adding an algebraic turbulent eddy-viscosity term to the equations. Of course it was not a constant.

More specifically, after examining the available literature and information on the subject, such as Townsend's and Hinze's books, I used an inner eddy-viscosity formula, an outer eddy-viscosity formula, and a decay law. For the inner eddy-viscosity formula, Prandtl's mixing length formula basically was satisfactory, but I liked Van Driest's improvement of it, which had a certain amount of physical justification. The formula used was

$$\epsilon_i = \rho\kappa^2 y^2 (1 - e^{-y/A})^2 \frac{\partial u}{\partial y}$$

Here κ is von Kármán's constant. The $\rho\kappa^2 y^2$ quantity is just Prandtl's mixing-length formula. Van Driest's modification brings solutions properly to the very wall. For the outer region, we used a form suggested by F. Clauser, but with a slightly different constant, namely,

$$\epsilon_o = 0.0168 \rho u_e \delta^* \gamma$$

As you can see, at any particular station ϵ_o is a constant viscosity except for γ, the intermittency factor. Finally, as one gets to the outer edge, intermittency sets in and the turbulent transport processes fade away. We used Klebanoff's expression for γ to describe this fadeout. So you see that, at the very wall, the turbulent viscosity was zero, but then it grew mainly as y^2, but as y^4 at the very wall, which is theoretically correct. Then when it reached the value indicated by the outer formula, we would switch to it and finally fade everything out as the edge was reached. This formulation is quite simple but the results were in reasonable agreement with the experimental data that was available. While numerous extensions of the method have been made, it is interesting that my original formulation has never been changed, not even the constants.

It is interesting to note that I often received a lot of "static" from O. R. Dunn, a later Director of Aerodynamics, about our laminar efforts. He would ask, "Why are you wasting your time on laminar boundary layers when the DC-8 is all turbulent?" One day in connection with this turbulent extension when he again asked me this question, I went to his blackboard and wrote out the full incompressible equation including the turbulent viscosity term ϵ. He could see that the structure was the same for both the turbulent and laminar cases, and that work on the laminar was a kind of training exercise for work on the turbulent case. After that, he became a big booster for all of it.

I did not have a high degree of confidence that incorporating accepted basic turbulent properties in the full equations of motion would give good answers for all kinds of boundary-layer flows because an algebraic eddy-viscosity model has obvious flaws. Instead, initially I regarded the effort as one merely to find an answer to the question, "If we use the best available algebraic transport properties plus the exact equations of motion, how good are the answers?" Our first efforts were just to use the described eddy-viscosity formulation and introduce it into the equations of our shooting method. We then solved a number of cases by this method to see what kind of accuracy was indicated and found it to be very good for a considerable variety of problems. Our first paper about this method was given in 1966.[6]

The studies seemed to answer my question favorably, so we went ahead with development. The shooting method was very slow; the eddy viscosity was a variable function of the boundary layer itself, so a double iteration was needed. Hence I started out to find a better method. After reading Fox's book, *The Numerical Solution of Two-Point Boundary Problems,* I settled on a five-point finite-difference method. We were developing and applying this method at the time T. Cebeci joined my group at Douglas. He soon got into this work and made the new finite-difference method operational. For several years this appeared to be the leading all-around method for boundary layers. Dr. Cebeci devised several ingenious extensions to the eddy-viscosity formulations to account better for suction and blowing, low Reynolds number, heat transfer, etc. This extended version did very well at the Stanford Symposium in 1968.

The method is still used throughout the country but, it has been considerably superseded by Cebeci's extension to Keller's Box Method, a powerful and more universal numerical method than my five-point method. This is only a mathematical method of solution; the equations are just the same. There now are numerous turbulent methods, some far more sophisticated than mine. Sometimes their predictions are better and sometimes worse, so on the average our method is still holding up for ordinary boundary layers. When Cebeci and I wrote our book about turbulent boundary layers, I feared it might become obsolete rather fast because there was great activity in developing various transport equation methods. They definitely are more logical, but still lack a basic ingredient—a sounder description of turbulence—and so seem rather bogged down. For ordinary boundary layers, our simple algebraic eddy-viscosity formulation seems as good as any and much easier to calculate. Ordinary boundary layers are the important kind on aircraft.

When I began to try to calculate the turbulent boundary layer, I was not aware of any similar effort. More accurate calculation of turbulent boundary layers was just a desirable goal. After a while, I became aware of Mellor's work which used a similar approach, and of Bradshaw and Ferriss's work, but not at first, when the course of development was being set.

VII. Closure

A final contribution in the field of turbulent boundary-layer theory was my extension to the axisymmetric case of Stratford's method of predicting separation. Stratford's method is an approximate procedure that is easy to use and reasonably accurate and so is very useful. At the time I did this work, the main methods of predicting this kind of separation were by Presz and Pitkin or else by some method like the Cebeci-Smith that involves the full partial-differential equations. These are rather long, and I wondered if one could not find a shorter method—an extension of Stratford's. I was at UCLA at that time and had some free time before a research contract on torpedo-like bodies came in. So I decided to use this time by seeing if I could work out the extension. I managed to. Stratford's method involves a transformation of boundary layers from one position to another. It is interesting that the old DESA-2 airfoil report materially helped me on this problem because, in it, I had worked out a related transformation for the boundary layer on each side of a suction slot. A paper about the Stratford extension was published in *ZAMP* and dedicated to Professor Nicholas Rott on his 60th birthday.

In thinking back about my contributions, I find a very high percentage of my really basic ideas were conceived at home, not at work. Generally I was not seriously thinking about a problem, but still it was in the back of my mind, and suddenly I saw a solution. Another process that is very common is as follows. I would work on some problem rather intensively all day long, without satisfaction. I would usually be trying to implement some line of action but without full success. During the day, the problem was studied thoroughly and learned in intimate detail. Then frequently, when I had quit for the day, given up thinking, and freed my mind from the course followed all day, I would suddenly see the satisfactory solution I had been searching for. I think my subconscious brain is smarter than my conscious one. Also, I am more or less a loner. I have never been especially good at theoretical work and have not been very good at theoretical exchanges and mutual creation of ideas. The only way that has been successful for me has been continuous and persistent study by myself in some area. I am rather slow to understand a subject and, consequently, have to work on it very had. However, when I finally understand it, I understand it well.

While working on research within the engineering department of an aircraft manufacturing company (Douglas Aircraft Co.) has its disadvantages, there are certain advantages. One is that a person is more intimately exposed day to day to real life problems and directly feels the great pressure for better and practical answers. This fact was often in the background of my contributions. I would see some area where there were problems, then gradually, I would see the essence of the problem, e.g., general turbulent boundary-layer calculations. Then my group and I would figuratively go off to the side and work on a fundamental solution. Being a research group, we could take a longer range approach than the design engineer who probably needed an answer the next day.

In general, at least for the most significant or important problems, I had little trouble getting the support of management. Of course I had to show clearly the goals and gains that would occur if a project was successful. We were rather selective about seeking government support, going after it only when we thought we could accomplish the job and at the same time benefit the company. This is why we solicited considerable work from the Navy in hydrodynamics, because much hydrodynamics is just incompressible aerodynamics. Also, I carefully watched another aspect—to be sure our work was useful to the company. I have seen a number of industrial research organizations founder, probably because their work was too far out or too unrelated to the company's more immediate problems. So we only worked on problems for which we could make a strong defense if attacked, the Neumann program, for example. I had a simple motto "there are far more problems than we can possibly solve, so we might as well pick on ones that are useful."

One final comment is in order about why I gradually drifted into research. I suppose the ambition of most young engineers is to be a chief engineer. It was one of mine and I attained it very early—at Aerojet. However, I soon found it was not what I wanted. I found myself just interpreting other people's input which at times I believed was biased. Gradually I realized I preferred creating information rather than interpreting something given to me by others. This is just a way of saying I preferred the technical side or more specifically creating information, which is just research. So after going back to Douglas I stuck to the strictly technical.

I hope this confession is of interest and helpful to others. Also, I would like to make it clear that if I had not had a group of very capable people at Douglas to help me implement the methods, much less would have been done. To them, who are too many to name, I feel much gratitude; I was lucky to have such good help.

Acknowledgment is made, with thanks to Richard S. Lee for suggesting the title.

References

[1] Schlichting, H. and Bussmann, K., "Exakte Losungen fur die Laminare Grenzschicht mit Absaugung und Ausblasen," Schriften der Deutschen Akademie der Luftfahrtforschung, May 7, 1943.

[2] Smith, A. M. O., "Design of the DESA-2 Airfoil," Douglas Aircraft Co., Rept. ES17117, AD 143008, Nov. 1952.

[3] Smith, A. M. O., "Rapid Laminar Boundary Layer Calculations by Piecewise Application of Similar Solutions," *Journal of the Aerospace Sciences,* Vol. 23, Oct. 1956, pp. 901-912.

[4] Smith, A. M. O. and Clutter, D. W., "Solution of the Incompressible Laminar Boundary Layer Equations," Douglas Aircraft Co., Rept. ES 40446, AD 266271, July 1961.

[5] Jaffe, N. A. and Smith, A. M. O., "Calculation of Laminar Boundary Layers by Means of a Differential Difference Method," *Progress in Aerospace Sciences,* Pergamon Press, 1972, pp. 49-211.

[6] Smith, A. M. O., Jaffe, N. A., and Lind, R. C., "Progress in Solving the Full Equations of Motion of a Compressible Turbulent Boundary Layer," presented at the Seventh BOWACA Symposium on Aeroballistics, U. S. Naval Missile Center, Point Mugu, Calif., AD634988, June 1966.