In "Applied Computational Aerodynamics," Vol. 125, edited by P. A. Henne, American Institute of Aeronautics and Astronautics (1990)

Chapter 1

The Panel Method: Its Original Development

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Introduction

THE panel method is an extremely general method for solving Laplace's equation that governs low-speed inviscid flow. The flow being solved for may be about a body of any shape or past any boundary. Furthermore, the solution can be subject to nearly any kind of boundary condition, not just those imposed by the flow of a fluid. If the body is two dimensional or axisymmetric, the profile is approximated by a many-sided inscribed polygon. If it is three dimensional, it is approximated by flat quadrilateral elements. The name "panel method" comes from these treatments of the body shape. We at Douglas did not invent this rather appropriate name; instead, we called the calculation method either the Neumann method or Neumann program because it was solving the classical second boundary-value problem, that is, the Neumann problem. The method as generally programmed solves mainly for the kind of kinematic boundary values determined by a fluid flow. The method has also been adapted for purely arbitrary boundary conditions or ones determined by other kinds of physical situations, such as heat flow. About the only restriction is that basic existence proofs for solution do not exist when there is a discontinuity in boundary conditions, as exists for sharp corners on a body immersed in fluid flow. But in practice it has been found that any kind of convex corner can be handled with a high degree of accuracy. There was some trouble with concave sharp corners such as the hinge line on the bottom side of a lowered plain flap. However, ways were found to minimize this problem.

Conventional computational fluid dynamics (CFD) methods require calculation for the entire three-dimensional field about the body. While the panel method also can calculate the entire three-dimensional field, it requires only calculation over the surface of the body, that is; a two-dimensional calculation. Therefore, it inherently requires much less calculation than CFD, especially if flow values only on the surface of a body are sought.

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Getting into the Problem

About 1952 Richard Whitcomb had just introduced the Area Rule. All the design aerodynamicists were then converting their airplanes into equivalent bodies of revolution. But converting an airplane to an equivalent body of revolution leads to a rather bumpy type of body. Rather naturally design aerodynamicists wished to calculate the flow; that is, the velocity distribution about these bumpy equivalent bodies. But the methods available seemed unable to handle the more obnoxious shapes like that shown in Fig. 3.

One day late in 1952, I was approached by my superior, K. E. Van Every, Chief of the Aerodynamics section at the old El Segundo plant of the Douglas Aircraft Company. He asked me if I would look at the methods currently available for calculating the flows about bodies of revolution and recommend the best, if possible. This was my motivation; the primary concern originally was bodies of revolution, not airfoils or other two-dimensional bodies. Because bodies of revolution do not have lift, at least inviscidly, I was not particularly interested in vortex flow. That is a major reason why our original panel method (i.e., Neumann) was so strongly source biased.

Preliminary Work

I was certainly no expert on the various methods of calculating flows about bodies of revolution, but I probably was more knowledgable than others in the aerodynamics section. For instance, in connection with the inlet design problem on the D-558-1 research airplane first flown in Aug. 1947, a helper, Sue Hart, and I had worked up a large plot of the flowfield due to a ring source. Our plot was on 1-m wide and 2-m long millimeter roll graph paper. We went both axially and radially to five-ring radii. With this size plot plus the accuracy we maintained in our calculations, our results could be read to three decimal places. This work was done during the early design of the airplane in late 1944 or early 1945. At the end of World War II, while on a U.S. Navy mission in Europe, I met Dr. Dietrich Kuchemann in Germany. In 1940 he had completed a paper on the flow due to both ring sources and ring vortices.[1] But an interesting fact is that his tables also were to three decimal places and extended the same distance as my graph—five radii in both directions. This work was one bit of background; the large graph was a substantial project.

Later, in 1946, I found that I could analytically integrate an axial line source that had a quartic longitudinal variation in strength. A quartic variation can generate many kinds of Fuhrmann-type bodies, from ones with cusped noses and tails to ones with very blunt noses and tails. Therefore, we worked up a full report on nose and tail shapes with varying degrees of bluntness and cuspiness. Everything was solved to four-place accuracy. A report was issued, most of the work being done by G. Brazier,[2] presenting 11 nose and tail shapes plus 1 complete body, which was used for our first test case of the panel method several years later.

Still later, about 1948, I believe, I wondered if anything could be learned about supersonic delta wings by looking at the flowfield of a flat triangular shaped source, whose shape was approximately that of the delta wing. Nothing came of this, but I did find that I could indeed integrate such a three-dimensional distribution, and this knowledge came in handy later when we undertook to solve the three-dimensional Neumann problem since it, too, basically uses triangular elements.

The Aerodynamics Section was currently using the British method of Young and Owen[3] for analyzing bodies of revolution. I probably looked at this report first, but I also looked at the methods of von Kármán,[4] von Wijngaarden,[5] R. H. Smith,[6] and two methods of Kaplan.[7,8] There also were articles about disk sources such as the one by van Tuyl[9] that might lead to blunter bodies generated by the inverse (Fuhrmann) method. Other geometric arrangements of sources were also apparent, such as point sources off the axis. In fact, for awhile I wondered about point sources or ring sources that were located slightly below the surface of a body. But still nothing looked very promising. Every method seemed inadequate, so I kept looking and reading. I am sure I passed on my opinions and findings periodically to Van Every, but since I regarded his question as a general assignment that could last a long time, I kept looking and thinking.

Getting on to the Method

The last report about a method for the direct calculation of flow about an arbitrary body of revolution that I studied was one by Prof. L. Landweber.[10] I read it through slowly. It looked rather good, but at one point I came to a relation I could not understand, try as I might. I decided I did not know enough about potential flow and theory and decided to take some time off from looking at specific methods and instead read Kellogg's *Foundations of Potential Theory*[11] to learn more about the theory. (As I slowly read through Landweber's report I forgot about its title and took the method to be exact. A year or two later I happened to glance at his report and noticed the word "elongated" in the title. This explains why I could not understand a certain relation. But the report was very useful in an indirect sort of way because it caused me to read Kellogg.)

About two-thirds of the way through the book I came to the following equation, but rewritten here in the notation we were to use:

$$\frac{\partial \phi}{\partial n^-}(p) = 2\pi\sigma(p) + \iint_S \sigma(q)\,\frac{\partial}{\partial n}\left(\frac{1}{r}\right)\mathrm{d}S, \qquad \text{region } R$$

$$\frac{\partial \phi}{\partial n^+}(p) = -2\pi\sigma(p) + \iint_S \sigma(q)\,\frac{\partial}{\partial n}\left(\frac{1}{r}\right)\mathrm{d}S, \qquad \text{region } R'$$

Figure 1 will be used to show some notation and to help explain the equation. It is taken from the original Smith and Pierce report (1958), where it is identified as Fig. 13. Now consider the figure. The term ϕ is, as usual, the potential. One equation applies to the region R' external to the body, the other to region R, internal to it. The $+$ and $-$ subscripts to $\partial\phi/\partial n$ indicate the direction of the normal derivative. The term $\partial\phi/\partial n$ is the component of the freestream or onset flow that is normal to the boundary. It is the boundary condition. The term σ denotes the area density of a sheet of sources covering the complete surface of the shape being analyzed. It is the unknown to be found. The term p represents a point where a flow is being calculated, and q is a variable point. The term r is the distance between points p and q as shown on the figure, and n represents the normal to the surface at any point. S represents surface area, and the two equations tell us that the indicated operations are done over the entire surface of the body. The figure will be referred to periodically. Although this kind of solution can be found more or less buried in Lamb's *Hydrodynamics*, this was the first time I became aware that a pure surface source-sink method had possibilities of solving for the kinds of flow we were interested in. Before this discovery I had not known that sources and sinks could be right on the surface.

Furthermore, the equation looked tractable, being a Fredholm integral equation of the second kind, since high-speed computers were becoming available. Upon further study I could see no special restrictions. The surface shape could be entirely arbitrary, and the $(\partial/\partial n)(1/r)$ term required nothing more than directional differentiation and was just a function of the geometry of the body. The pair of equations shows that an internal problem is really no different from an external one except for the change in sign of the $\sigma(p)$ term. Of course, conservation of mass is a requirement for the internal flow.

Besides looking solvable and quite general, Kellogg makes the following statements on pages 311 and 314, respectively: "II. The Neumann problem is solvable for the infinite region R' for any continuous values of the normal derivative on the boundary." "V. The Neumann problem is solvable for a single one of the bounded regions R: under the essential condition that the integral over the bounding surface of the values assigned to the normal derivative vanishes."

Although not immediately obvious, it soon became apparent that for external flows there could be more than one body, thus solving interference problems and the like. As I looked more and more into this kind of solution, it became clearer and clearer that the two statements made by Kellogg really said a mouthful. Seldom in aerodynamics are to be found such unqualified statements accompanied by equations that look as if they can be solved. (We aerodynamicists indeed have other essentially unqualified statements, like the Navier-Stokes equation, but it cannot be solved in general.)

There are essentially no restrictions on the kinds of derivatives $(\partial\phi/\partial n)p$ that can exist. Low body slopes or linearization of any kind are not even mentioned in the two statements given by Kellogg. The quantity $\partial\phi/\partial n$ can apply for all kinds of conditions in fluid flow, from rectilinear flow, porous flow, vortex flow, rotating bodies, onset flows caused by other bodies, etc. If Laplace's equation applies to different physical situations, $\partial\phi/\partial n$ can be determined for that situation too, for example, temperature distribution in a solid. Moreover, the equation applies equally to two-dimensional, axisymmetric, and three-dimensional shapes. I can think of very few situations where a governing equation has essentially no restrictions,† yet seemed fairly readily solvable using machine calculation. In actual application it was found that even discontinuous boundary conditions which are excepted in Kellogg's statement II could be calculated accurately as noted in the introduction.

Trying to Solve the Equations

Serious work to solve the equations began in the summer of 1953. I told Van Every that it looked as if we might be on to something, and he supported us by authorizing the necessary budget. There are, of course, many approaches to solving the two equations, but most have some kind of limitation. Many methods will handle only very regular solutions, not our bumpy body problem. I thought about the solution and got more familiar with the equation for several months, including considerable time looking in the literature for methods of solution. In particular, I remember spending one entire day in the California Institute of Technology physics and mathematical libraries trying to find leads to a method of solution. The first and second boundary-value problems are famous problems, and there is extensive literature about them. But I got no help. All I found were proofs of the existence and uniqueness of the solution, in line with Kellogg. I found nothing on practical methods for solving the problem. I came away with a mild irritation at the mathematicians. Of course, knowing that a solution exists and is unique is important because one would not find himself following a blind alley. But I already knew that from Kellogg.

The Solution

Although there are numerous ways of evaluating the integration required by the equation (e.g., Martenson[12] and Jacob[13]), after a few months of looking around and thinking about the problem I rather quickly settled on a method. Because the calculations would be lengthy, only one method was worked out at the time, although theoretical refinements were seen. The method chosen was one I thought was the simplest possible that would converge to the exact answer for the case of an infinite number of panels. It had the following main features:

1) The body would be divided into a series of sections, later called panels (see Fig. 1).

2) Continuous source density distribution over each of these sections would be used; i.e., no point or line sources would be used.

3) These sections would be flat in profile, i.e., panels.

4) The panels might each have a different source density σ, but the density would be constant for each panel.

5) The indicated integration would be replaced by a simple summation.

Now we will discuss these assumptions in order and the thinking behind them.

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†That is, where Laplace's equation applies. More generally there are indeed restrictions; the flow cannot be compressible or viscous.

1) Because our interest was bumpy, i.e., nonfair bodies, no kind of continuous or analytic treatment seemed likely to work, yet Kellogg's statements were truly applicable to nonfair bodies. Hence, as in numerical quadrature, it seemed that the best course was to divide up the body into a number of sections as in Fig. 1.

2) In dealing with a continuous surface distribution, when the variable point q approaches the control point p (see Fig. 1), the normal flow $\partial\phi/\partial n$ due to the sheet of sources takes on the very finite value $2\pi\sigma$. If the source material were concentrated in lines or points, then when q approaches p, i.e., $r \to 0$ in the figure, the velocity becomes infinite. Therefore, in order to avoid all kinds of singularities, it seemed to me to be imperative to use a continuous surface source distribution.

3) A flat element in profile seemed much the easiest for integration of the $(\partial/\partial n)(1/r)$ term applicable to each panel. In fact, for two-dimensional flow the integration across a strip is analytic. It also is analytic for flat quadrilateral elements in three-dimensional flows that were solved later. Only in the axisymmetric case where each element is a frustum of a cone (see Fig. 1) is it nonanalytic, but even here accurate results can be obtained by a combination of numerical integration and series expansion. Using flat elements essentially amounts to solving for the flow about a polygon inscribed inside the true shape. By using flat elements and assuming constant σ on each, it became possible to perform the first integration of the indicated double integration analytically or at least in advance.

4) Having constant source density σ on each panel made integration simpler and helped lead to a simple system of linear algebraic equations.

5) It is probably apparent by now what will be the system for solving this Fredholm integral equation of the second kind. Referring to Fig. 1 and the equation, it is seen that the double integral is approximated by breaking the body into a sum of values for strips, each one having an unknown value of σ. The $\sigma(q)$ is the unknown, but the $(\partial/\partial n)(1/r)$ term is directly calculable from the geometry of the body. Then a system of linear equations can be formed. On the other side of the equal sign is the $\partial\phi/\partial n$ term. This quantity is a given by the boundary conditions. If the flow being analyzed is a rectilinear kind, $\partial\phi/\partial n$ is just proportional to the sine of the local slope of the body.

What is being done is really simple and straightforward. At any point p we are writing an expression for the contribution of all the ring sources for the entire body (see Fig. 1). It seemed to me at the time that the $(\partial/\partial n)(1/r)$ value at p for an inscribed frustum would differ exceedingly little from the value that would exist for the exact shape, thus further justifying the use of flat elements. On the other side of the equation we have $(\partial\phi/\partial n)p$. This is evaluated at the middle of each element; hence, it is a central difference calculation and should have good accuracy. Note that the equation is represented by a simple sum of terms; there were no quadrature formulas, etc.

Approximating a body shape by an inscribed polygon was a rather bold step. At the center of a two-dimensional strip where σ values are calculated, there is indeed no self-induced tangential flow, but there is some for cone frustums. But at the edge of any strip there is a logarithmic infinity that exists because 1) the next strip has a different σ, and 2) there is usually an angle between any two adjacent strips. However, we felt that by working in the middle of elements (strips) this unruly trait of the elements would not cause a problem. Yet the only way to know for sure was to program the method and run some cases.

Programming and First Results

All of the formulas or algorithms were worked out around the middle of 1954. Programming using the IBM/701 calculator was begun in the fall. The programming was going along smoothly enough but in the middle of it I had an assignment that took me to Europe from January 2 to March 10, 1955. During this trip I often wondered how the programming was coming along. At this early stage the programming was all in machine language; no FORTRAN, etc., was used. When I returned I found the program was working, and as far as I knew no special bugs had been encountered.

The original program was written for 24-point solutions, and the first case to be run was a Fuhrmann-type body taken from Ref. 2 for which we knew the accuracy to four places. The accuracy of our new 24-point solution turned out very good, and this very first case is shown as Fig. 29 in the long Hess-Smith paper, but is shown here for convenience as Fig. 2 taken from the Smith and Pierce report. Various cases were then run. We made extensive use of Milne-Thomson's book *Theoretical Hydrodynamics* to find theoretical solutions of various kinds to serve as test cases.[6] Also, two special cases come to mind. One was Fig. 2 of the Smith and Pierce report, shown here as Fig. 3. It was a very severe test; in fact, at first I contemplated making a wind-tunnel model of the same shape to help check our method. But the calculation turned out to be so accurate that I dropped the idea. The dashed line in Fig. 3 identified as the conventional method was calculated by use of Ref. 3, which was the method being used by the Aerodynamics Section at that time. As you can see, the improvement in accuracy was substantial, to say the least. The conventional method completely ignored the bump.

Another special body was in Figs. 1 and 27 of the Smith and Pierce report. This body was chosen 1) because it had a flat nose that up to then could not be calculated by any existing method and 2) because there were test data on the shape. The shape and velocity distribution are shown by Figs. 4 and 5. More will be mentioned about Fig. 4 later.

As can be seen, the original program was for bodies of revolution, but after we found that it was working we began programming the two-dimensional variation. By December 7, 1955, we had run a 150-point solution for the flow about a circular cylinder. Extensions like this generally came easy. Of course, I enjoyed seeing good results occur with each new try or extension, but it was almost more enjoyable to see the pleasure and enthusiasm that Jesse Pierce showed with each successful new case. More will be said about Pierce a bit later.

Our first report was the Smith and Pierce Report, in April 1958. In June 1958 I gave a shortened version of the paper at Brown University. The method was first revealed at this meeting. At the meeting Prof. Irmgard Flugge-Lotz attended my lecture. Afterward, she got up and commented that she had used the surface method in some work she had done on airships at yaw. This was the first I knew that the basic idea had been used before. Later, I learned that Dr. W. Prager had published a paper[14] in 1928 also using surface sources. Flugge-Lotz's paper[15] helped Hess in his crossflow work, but Prager's paper was too primitive to be of any assistance, except that it called attention to the fact that a surface source treatment was a possibility.

Up to 1958 all of this work was financed by Douglas, but we could see much more work to be done, and extensions were proposed to the Office of Naval Research (ONR). An interesting coincidence developed. The very first illustration in the Smith and Pierce report was that of the flat-nosed body, Fig. 4, mentioned earlier. The man in charge of the Fluid Mechanics Branch at ONR at the time was Phillip Eisenberg. He had tested the flat-nosed model himself in a water tunnel at the David Taylor Model Basin in 1947. In contacting ONR we had sent in advance a copy of the Smith and Pierce report; thus, when he looked at the figure it immediately caught his eye. He arranged for a meeting with representatives of the Model Basin, the principal one being Dr. Avis Borden. We told her and the others what we thought we could do, and shortly thereafter they chose to give us a contract to extend the method to three-dimensional nonlifting flows, which would be directly applicable to ships. The work would also be directly useful to Douglas, since free surface effects were not being considered under this contract.

Other Early Extensions

After the original body of revolution program including inlets, the first extension was to the nonlifting two-dimensional problem. The next extension of significance as far as I remember was Hess' extension of the body of revolution problem to crossflow. This extension allowed bodies of revolution to be analyzed at angles of attack just like airfoils. At low angles of attack this method is quite useful, only losing its validity at higher angles of attack when separation develops. The next development was the three-dimensional nonlifting problem already mentioned.

At about the same time we began working on incorporating lift into two-dimensional flows. A lifting airfoil has circulation; hence, at first we simply put a vortex inside the airfoil and calculated for still another kind of onset flow. Algebraically it makes no difference where the vortex is located inside the airfoil, but computationally it made a great difference. A point vortex generally gave the correct gross pressure distribution and lift coefficient but created two bumps in the velocity distribution near wherever we put the vortex. After a number of tries with various vortex treatments, it was found that a vortex onset flow created by turning all the source strips into vortex strips worked very well and eliminated the irregularities in the pressure distribution. J. P. Giesing did most of the airfoil work. But since then further refinements made in years subsequent to this chronology have made the results with lift even better. Our method of solving most of the lifting problem with sources may seem peculiar to many, but the source method was working so well that it seemed natural to try to extend it. Also, of course by the very generality of the method of solution, the extensions to cascades, multielement airfoils, and ground effect were very simple. Still another extension was to find added mass or inertia coefficients for arbitrary bodies of revolution.

Another early extension was to solve the hydrofoil problem subject to a linearized free surface condition. Except for complications caused by this linearized surface condition, a hydrofoil could be just as arbitrary in shape as an airfoil. I became interested in this problem because of our continuing relation with the David Taylor Model Basin. A very complicated elementary singularity that automatically satisfies the linearized free-surface condition is called the Havelock source. We hoped to apply it to the three-dimensional problem but were unable to perform one integration. However, we could and did do it for the two-dimensional case—the hydrofoil case. Although I started this line of action, Giesing did most of the work; thus, he is the first author in the Giesing-Smith paper. Hydrofoil and other free-surface work had a lower priority because Douglas was an airplane company, not a ship company.

There were other extensions, such as suction over part of a body, rotating bodies, bodies in shear flow, effects of compressibility by use of the Goethert rule, nonlinear unsteady airfoil theory, and applications in physics to problems involving superconductivity. Most of this work is written up in the long Hess-Smith paper. The early work is considered to end with this publication. Much more work has been done by now; for instance, the three-dimensional problem with lift, exact propellor calculation (except that the location of the wake is not precisely known), higher-order solutions using curved instead of flat elements, more use of vortex elements to eliminate some final problems near the trailing edge of airfoils, and, finally, good inverse solutions.

Four Key Helpers

My first helper was a retired mathematics professor, Dr. Jesse Pierce. His son, E. W. Pierce, was assistant chief of the Aerodynamics Section where I worked. Jesse Pierce was 67 years old when he was transferred to my group. He had been head of the mathematics department at a small school, Heidelberg College, in Tiffin, Ohio. He was still interested in working and got a job in the computing section at Douglas, in El Segundo, California. After awhile, about the middle of 1954, I believe, the manager of computing felt that he would be more useful in my group, especially since I needed help. Accordingly, he was transferred to work mainly but not exclusively on the Neumann Program. He advised the actual programmers, mainly George W. Timpson and William E. Moorman, worked up details of test cases, and performed various other duties.

Most of the theory was all worked out before Dr. Pierce joined our group, but he did contribute one important observation. He noticed that the flow of interest is always on the left as one traverses a shape. Thus, on a closed body of revolution, if the coordinates of the body are written for the top side from nose to tail, the flow will be the conventional external flow. If the coordinates for the bottom side are used instead, then in loading in the deck the flow calculated will be one internal to the body, if that is possible. Jesse Pierce worked in my group from 1954 to 1958. He helped a great deal in preparing the Smith and Pierce report, writing about half of it. About the time it was finished he decided to quit work for good, since he was 71 years old by then.

John Hess came into my group in July 1956. He had fulfilled all of the requirements for a PhD in Applied Math at the Massachusetts Institute of Technology, except for the research requirements. I hired him mainly because I needed more help on a variety of projects. At first he did indeed perform a variety of duties, but showed an interest in the potential flow work that Pierce and I were doing. In fact, he was soon giving proofs and demonstrations of relations we thought were true, but where questions existed.

Then when Jesse Pierce quit it seemed only natural for Hess to take over. In retrospect, it was a good decision, as evidenced by all of the papers and extensions of the method that he has made. My decision was confirmed very early by his extension of the body of revolution problem to the crossflow case. I had practically nothing to do with this problem. Then when we collaborated on the ONR three-dimensional flow contract, besides doing things like developing the quadrilateral element input system, he worked out a simplified method for evaluating the flowfield of a quadrilateral source element. He applied a multipole expansion frequently used in electrostatic problems. If the p point in Fig. 1 is at some relative distance from the q point, the lengthy exact formula could be bypassed, thus saving much computing time. Obviously, if p is sufficiently far from q, a quadrilateral source could be treated as a point source. I only mention these two points because Hess' more recent contributions are well known. Since he was handling the potential flow work very well, perhaps better than I could, I gradually eased out of it, turning most further development over to him.

Joe Giesing had been in our group earlier but went back to school to get his Master's degree in Aeronautics at the California Institute of Technology on a Douglas scholarship. He returned to us in June 1962 and immediately began working on the airfoil and cascade problem, studying various distributions and locations of vortices to get lift. I am not sure who gets credit for the final quite successful method, converting all of the source elements into vortex elements to give lift. It rather grew out of discussions among Hess, Giesing, and myself, but in any case Giesing wrote the final report. Later he did most of the work on the hydrofoil problem. In May 1968 he was offered a better position in the structures group at Douglas, and he left my group.

Sue Schimke, née Faulkner, was also instrumental in the early development of the method. Sue was a University of California, Los Angeles, graduate in meteorology but never practiced it. She came to Douglas in

1957 and transferred to our group in October 1958. She made herself very useful; being able to find things and knowing where everthing was, was one of her strong points. Although there was nothing formal about it, she gradually became John Hess' assistant. She soon became very adept at running any sort of Neumann problem case. All that was necessary was to tell her the general problem, and often she would work out the formulas for it if needed. Then she would compute the necessary body coordinates and finally run the case on the IBM/704, 7090, 7094, 360, or whatever. It could be said that after awhile Hess did the basic theory and Sue would turn it into practice.

References

[1]Küchemann, D., "Tafeln für die Strömfunktion und die Geschwindigkeits componenten von Quellring und Wirbelring," *Jahrbuch der Deutschen Luftfahrforschung*, Rufus Oldenburg, Munich and Berlin, 1940, pp. 1547–1564.

[2]Brazier, J. G., and Smith, A. M. O., "Development of Nose and Tail Shapes in Incompressible, Irrotational Flow," Douglas Aircraft Co., Long Beach, CA, Rept. E.S. 20875, June 1947.

[3]Young, A. D., and Owen, P. R., "A Simplified Theory for Streamline Bodies of Revolution, and its Application to the Development of High-Speed Low-Drag Shapes," Aeronautical Research Council, London, R & M 2071, 1943.

[4]von Kármán, T., "Calculation of Pressure Distribution on Airship Hulls," NACA TM-574, 1930.

[5]von Wijngaarden, A., "Écoulment Potential Autour d'un Corps de Revolution," *Colloques Internationaux du Centre National de la Recherche Scientifique, XIV. Methods de Calcul dans des Problèms de Mécanique*, Paris, 1948.

[6]Smith, R. H., "Longitudinal Potential Flow about Arbitrary Body of Revolution with Application to Airship 'Akron'," *Journal of the Aeronautical Sciences*, Vol. 3, No. 1, Sept. 1935, pp. 26–31.

[7]Kaplan, C., "Potential Flow about Elongated Bodies of Revolution," NACA Rept. 516, 1935.

[8]Kaplan, C., "On a New Method for Calculating the Potential Flow Past a Body of Revolution," NACA Rept. 752, 1943.

[9]van Tuyl, A., "On the Axially Symmetric Flow around a New Family of Half Bodies," *Quarterly of Applied Mathematics*, Vol. 7, No. 4, 1950, pp. 399–409.

[10]Landweber, L., "The Axially Symmetric Potential Flow about Elongated Bodies of Revolution," David W. Taylor Model Basin, U.S. Navy, Washington, DC, Rept. 761, 1951.

[11]Kellogg, O. D., *Foundations of Potential Theory*, Ungar, New York, 1929; also Dover, New York.

[12]Martenson, E., "Berechnung der Druckverteilung an Gitterprofilen in Ebner Potentialströmung mit einer Fredholmschen Integralgleichung," *Archives of Rational Mechanics and Analysis*, Vol. 3, No. 3, 1959, pp. 235–270.

[13]Jacob, K. W., "Some Programs for Incompressible Aerodynamic Flow Calculations," California Inst. of Technology Computing Center, TR-122, Feb. 1964.

[14]Prager, W., "Die Druckverteilung an Körpern in Ebener Potentialströmung," *Physikalische Zeitschrift*, Vol. XXIX, 1928, pp. 865–869.

[15]Flügge-Lotz, I., "Calculation of Potential Flow Past Airship Bodies in Yaw," NACA TM-675, July 1932.

Bibliography

Geising, J. P., and Smith, A. M. O., "Potential Flow about Two-Dimensional Hydrofoils," *Journal of Fluid Mechanics*, Vol. 28, Pt. 1, April 1967, pp. 113–129.

Hess, J. L., and Smith, A. M. O., *Calculation of Potential Flow about Arbitrary Bodies*, Vol. 8, Progress in Aeronautical Sciences, Pergamon, Oxford, UK, 1966, pp. 1–138.

Smith, A. M. O., and Pierce, J. "Exact Solution of the Neumann Problem. Calculation of Non-Circulatory Plane and Axially-Symmetric Flows About or Within Arbitrary Boundaries," Douglas Aircraft Co., Long Beach, CA, Rept. E.S. 26988, April 1958 (this is the first report about the method); also available as Armed Services Technical Intelligence Agency, Rept. AD 161445. (A shortened version of the paper but with the same title can be found in *The Proceedings of the III U.S. National Congress of Applied Mechanics*, 1958, p. 807.

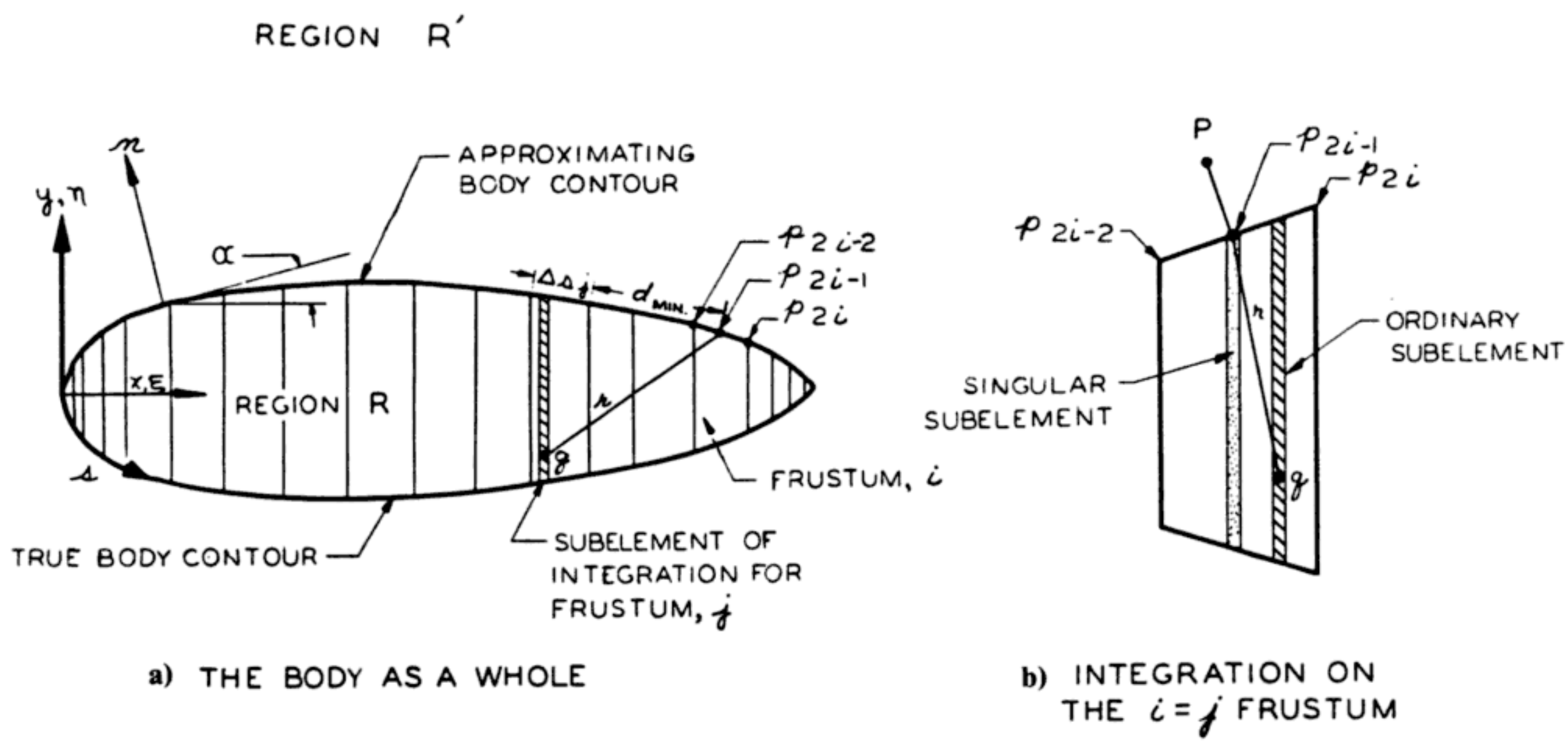

Fig. 1 Notation and method of approximating a body of revolution (Fig. 13 of Smith and Pierce report).

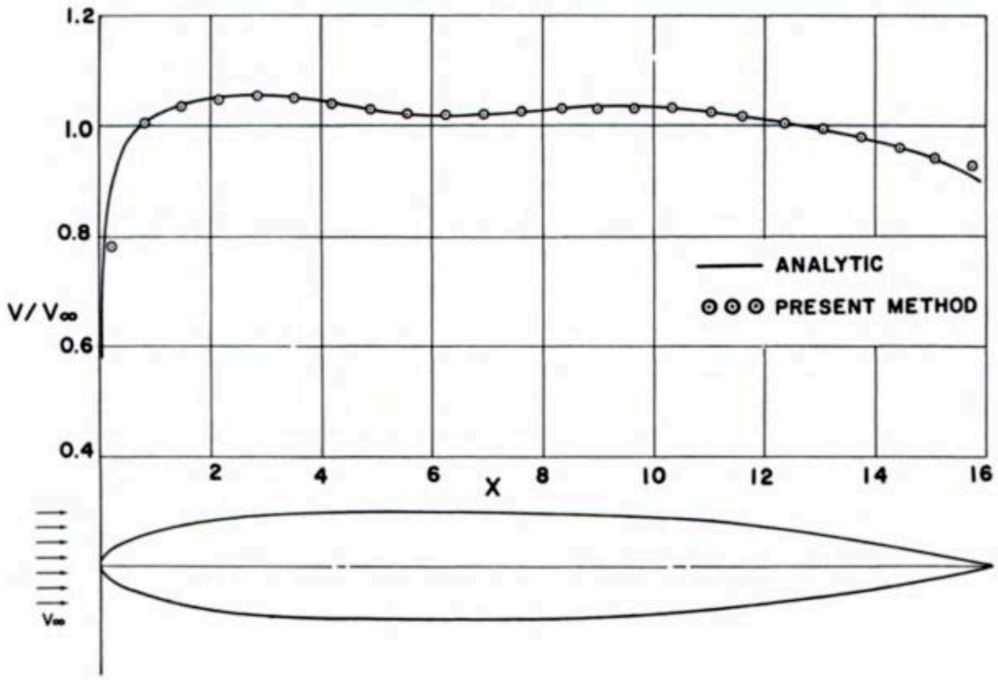

Fig. 2 Very first case run (Fig. 28 of Smith and Pierce report).

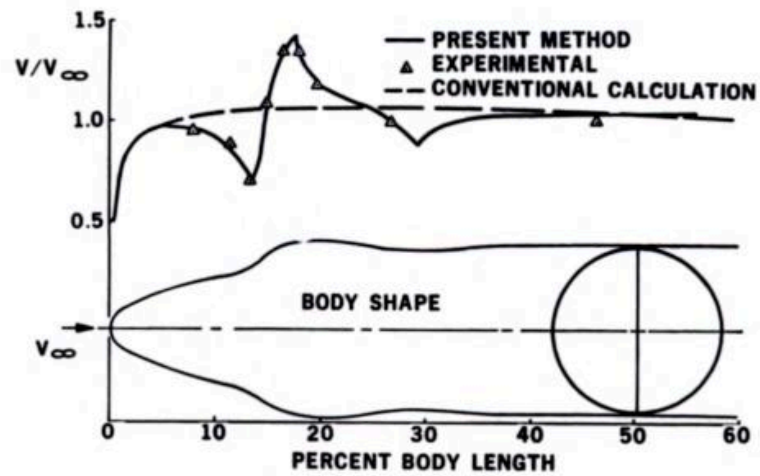

Fig. 3 A bumpy body of revolution (Fig. 2 of Smith and Pierce report).

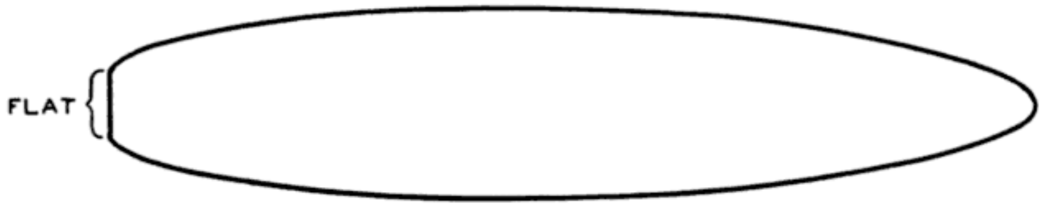

Fig. 4 A flat-nosed body of revolution (Fig. 1 of Smith and Pierce report).

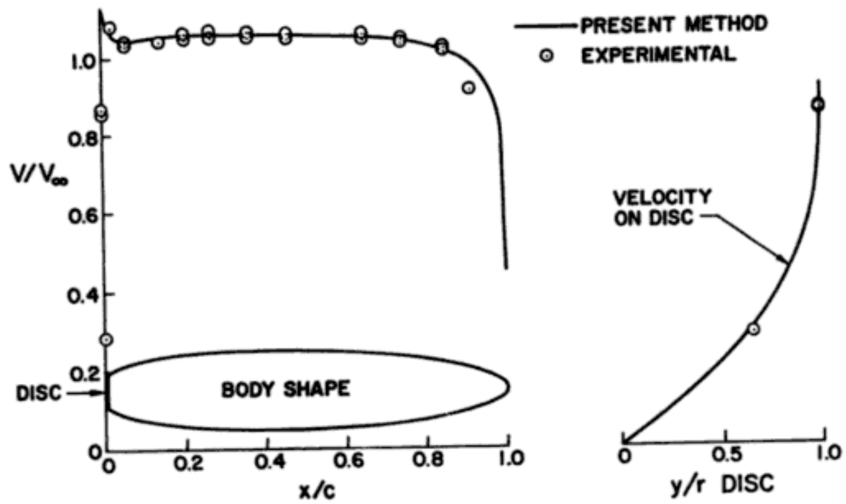

Fig. 5 Further data on the flat-nosed body of revolution (Fig. 27 of Smith and Pierce report).