Readers' Forum

Brief discussions of previous investigations in the aerospace sciences and technical comments on papers published in the AIAA Journal *are presented in this special department. Entries must be restricted to a maximum of 1000 words, or the equivalent of one* Journal *page including formulas and figures. A discussion will be published as quickly as possible after receipt of the manuscript. Neither the AIAA nor its editors are responsible for the opinions expressed by the correspondents. Authors will be invited to reply promptly.*

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Comment on "Stability Aspects of Diverging Subsonic Flows"

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STABLE calculations of subsonic, inviscid flows in a divergent duct can be performed using time-dependent techniques. If the flow evolves asymptotically into a steady state, the accuracy of the results can be tested, and it is found to be very high. (Errors in stagnation pressure, for example, are of the order of 10^{-4} or less.) A full discussion of the problem, both for quasi-one-dimensional flows and two-dimensional flows, requires a full-length paper. The reader interested in details will find them in Ref. 2, a report which also has been submitted to the *AIAA Journal* for publication. Since the prior statement, however, does not agree with Ref. 1, a brief comment seems to be in order.

The error growth analysis, as presented in Ref. 1, can hardly be accepted. The author tries to justify it as a "heuristic argument"; we object that the stability analysis of partial differential equations cannot be based on a finite-difference calculation, since different discretizing schemes applied to the same equations may have different ranges of stability. The fallacy of the analysis is proven pragmatically by the existence of stable calculations that tend asymptotically to that very state that is "proven" to be unstable.

I disagree with the author's statement (end of col. 2, p. 535) that his chosen boundary conditions are "correct for a well-posed problem." In brief, u cannot be specified at the inflow boundary; the correct parameter to be prescribed is the stagnation pressure.[3,4] When u is prescribed, spurious pressure waves propagate into the region of interest, and may remain trapped within. If u and p are specified from the exact solution, mesh-dependent wiggles appear. If the density is also specified, entropy waves develop.

Similar considerations can be advanced for the outflow boundary, both for quasi-one-dimensional flows and for two-dimensional flows. Some conceptual subtleties, that are not developed in Refs. 3 and 4, are necessary to understand the arguments in depth.

The most disturbing feature of the paper is the implication that erratic results of a defective numerical analysis can be used to imply physical instabilities of a flow (last paragraph in the first column of p. 537). Whatever physical "instability" (such as onset of turbulence or, with a good deal of semantic stretching, boundary-layer separation) may occur in a viscous flow, that has nothing to do with the appearance of mesh-dependent wiggles in an inviscid calculation.

References

[1] Cline, M. C., "Stability Aspects of Diverging Subsonic Flow," *AIAA Journal*, Vol. 18, May 1980, pp. 534-539.

[2] Moretti, G. and Pandolfi, M., "On the Computation of Subsonic Flows in Ducts," Polytechnic Institute of New York, Rept. M/AE 80-18, June 1980.

[3] Serra, R. A., "Determination of Internal Gas Flows by a Transient Numerical Technique," *AIAA Journal*, Vol. 10, May 1972, pp. 603-611.

[4] Pandolfi, M. and Zannetti, L., "Some Permeable Boundaries in Multidimensional Unsteady Flows," *Proceedings of the VIth International Conference on Numerical Methods in Fluid Dynamics, Lecture Notes in Physics*, Springer-Verlag, Vol. 90, 1978, pp. 439-446.

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Reply by Author to G. Moretti

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THE first conclusion of my paper[1] is that subsonic, diverging flows will separate for small angles of divergence and, therefore, inviscid methods, unable to predict or properly treat separation, cannot be expected to produce good results regardless of whether the calculations are stable or not. The second conclusion is that inviscid, subsonic, diverging flow is physically unstable. This second conclusion and its supporting research are the subject of Moretti's Comment. There are four objections presented in Moretti's Comment and these are discussed below.

The first objection (Moretti's last paragraph) concerns the implications I am using viscous flow phenomena to justify inviscid flow results. The viscous flow results,[1] which were, in the diverging case, unsteady but not unstable, were included to illustrate the accuracy, not stability, shortcomings of the inviscid equations. All of the stability discussions, results, and conclusions were intended to pertain only to the inviscid equations (see the fourth paragraph in this Reply). The term "physically unstable," used in Ref. 1, denotes the instability of the inviscid differential equations as opposed to "numerically unstable," which applies to the finite-difference equations. I must admit that the last sentence of the second paragraph of page 539[1] may be somewhat misleading. This sentence was included because at first I was surprised to find that inviscid, diverging, subsonic flow was unstable. On the other hand, after seeing how poorly the inviscid equations model the true viscous flow, I was less surprised by this result. However, I never intended there be any claims in Ref. 1 that boundary-layer separation, onset of turbulence, or any other viscous flow phenomena has any bearing on the stability of inviscid flows.

The second objection (Moretti's second paragraph) concerns the validity of my one-dimensional (1-D) stability analysis. To verify the finite-difference procedure used to solve the stability equations, two different spatial

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discretizations (no time differencing is involved here) were tried, many different grid spacings were used, and, finally, the spatial discretization was applied to equations with known stability properties. While one can never be positive this analysis is correct, I still feel that when used in conjunction with other supporting research, this type of stability analysis can be useful.

The 1-D stability analysis[1] was performed to determine the stability of the inviscid, source, and sink flows. However, in Ref. 1 this analysis showed that source flow is unstable, while sink flow is conditionally stable; that is, some dissipation is required to stabilize the sink flows. I gave two possible explanations for this lack of unconditional stability of the sink flows and the existence in nature of stable, converging, mostly inviscid, subsonic flows that approximate sink flows. (There are no flows in nature that approximate the high-divergence angle, inviscid, subsonic source flows.) The first was concerned with the fact that in nature dissipation is always present due to molecular viscosity and the effects of turbulence (last paragraph in the left column of page 537). The second was that as Δx in the stability analysis goes to zero, the amount of dissipation required to stabilize the flow decreases. I now feel that this second explanation is the correct way to explain the sink flow conditional stability. By reducing Δx several times, one can show that the dissipation required to stabilize the sink flow goes to zero and the inviscid flow becomes unconditionally stable.

The third objection (Moretti's third paragraph) is Moretti's statements "I disagree with the author's statement that his chosen boundary conditions are correct for a well-posed problem" and "u cannot be specified at the inflow boundary; the right parameter to be prescribed is the stagnation pressure." The specification of u, v, and ρ at the inflow boundary was proven to be correct for a well-posed problem by Oliger and Sundstrom (Ref. 2 of Ref. 1). Other papers describing the derivation and/or use of u as an inflow boundary condition are Refs. 2-6. Moretti does not take issue with any of these references, but instead states that "when u is prescribed, spurious pressure waves propagate into the region of interest, and may remain trapped within." Note that the u, v, and ρ inflow boundary conditions specify only three of the four dependent variables. The pressure is free to adjust and, therefore, allow waves to interact with the boundary conditions. Still, it is true that whenever any of the dependent variables are held constant at a subsonic boundary, waves will be reflected back into the interior and may become trapped. However, I have always considered the "trapping of waves" to be a numerical problem and not a mathematical proof that these boundary conditions are incorrect. The reflection of waves at the boundaries are affected by different algorithms to calculate the unspecified variables at the boundaries, different initial conditions, and different interior point algorithms. A proper numerical solution should use the above procedures in such a way as to avoid this "trapping of waves." In the converging flow cases, after the initial waves died out, the flow became steady for all time. However, in the diverging case, after the initial waves died out, instability would set in. I also used p_T, stagnation temperature T_T, and flow angle θ boundary conditions (see discussion under the fourth objection) proposed by Serra (Ref. 3 of Moretti's Comment). Indeed, specifying p_T instead of u does reflect less, thereby reducing the "trapping of waves" and improving the convergence to steady state. This improved convergence led me to use p_T in Refs. 7 and 8. However, I cannot see where Moretti's arguments invalidate the u, v, and ρ boundary conditions.

Moretti implies in his Comment and states in his Ref. 2 that I "believe that overspecifying exact conditions at the boundaries is necessary to obtain a correct solution." This statement is completely false. Nowhere in Ref. 1 did I state that one must overspecify exact conditions (or for that matter any conditions) at the boundaries to obtain a correct solution.

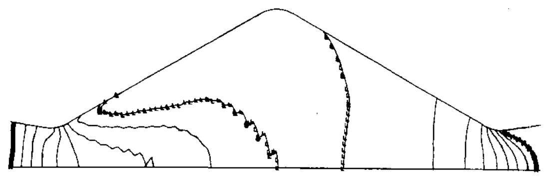

Fig. 1 Mach number contours for 30 deg diverging flow with supersonic sections on each end.

I only discussed overspecifying boundary conditions to explain how Griffin and Anderson (Ref. 4 of Ref. 1) were able to get stable calculations. I then went to great lengths to show how dangerous overspecifying boundary conditions can be by showing a calculation where reasonable, but not exact solution boundary conditions produced unreasonable results.

The fourth objection (Moretti's first paragraph) is my statement that inviscid, subsonic, diverging flow is unstable. Because I have found an error in my original work, I will briefly restate my original work and then discuss the error and how it affects my original conclusions. I first calculated subsonic source and sink flows with angles of 30 and 45 deg with midplane inlet Mach numbers M of 0.5-0.9 (only the 30 deg, $M=0.5$ cases were shown in Ref. 1) specifying u, v, and ρ at the inflow boundary and p at the outflow boundary. The sink flow calculations were stable while the source flow calculations were unstable. Next, converging sections were placed on each end of the diverging source flows. Therefore, the unstable diverging flow was located between regions of subsonic flow that by themselves were stable. All of these flow calculations were also unstable.

Calculations were also made specifying p_T, T_T, and θ at the inflow boundary. Flows with 30- and 45-deg angles and M of 0.5-0.9 were calculated. Again, all the diverging flow calculations considered were unstable. Because the p_T, T_T, and θ inflow boundary conditions have not been shown to my knowledge to be correct for a well-posed problem, these results were not discussed in Ref. 1.

The next step was to completely remove the subsonic boundaries by using the geometry shown in Fig. 1. The flow entering from the left is supersonic and, therefore, all variables are specified. The flow then decelerates to sonic flow in the first throat section, becomes subsonic in the first diverging section, and accelerates back up to sonic flow in the following converging section. Finally, the flow exiting on the right is supersonic and, therefore, all variables are extrapolated. Figure 1 shows the Mach number contours with flow instability beginning in the first diverging section. This flow calculation also became unstable and was not presented in Ref. 1 because it is a flow that probably would not occur in nature (which is true for all high-divergence angle, inviscid, subsonic flows) and because it confirmed other included results.

The last step was to perform a linear, variable coefficient stability analysis on the 1-D differential equations. Both the u, ρ, and p_T, T_T inflow boundary conditions were considered. Based on the results of the above numerous calculations and the stability analysis, I concluded that inviscid, subsonic, diverging flow was physically unstable.

While rechecking some of my calculations using the p_T, T_T, and θ boundary conditions, I found the 30-deg source flow with $M=0.5$ to be stable. However, above I stated the opposite was true. It is not clear where I made the error; the original calculations were made three years ago and the printed output has been thrown out. However, the 30-deg source with $M=0.7$ and 0.9 and the 45-deg source with $M=0.5$ were still unstable. (Again, as in the case of the u, v, and ρ boundary conditions, these calculations are stable if we overspecify the boundary conditions.) These unstable flows have a larger pressure gradient than the stable 30-deg source flow with $M=0.5$. (The 45-deg case has the same inlet height as the 30-deg case and, therefore, the source point is closer to the inlet resulting in a larger pressure gradient for the same M.) Therefore, I extended the stability analysis and found that instead of inviscid, diverging, subsonic flow being unstable, it was conditionally unstable depending on the size of the pressure gradient. The fact that the u, v, and ρ, and p_T, T_T and θ boundary conditions are unstable at different pressure gradients may be because they model different upstream conditions.

In a personal conversation, Moretti stated he had solved the higher Mach number and divergence angle flows and found them to be stable. Because our numerical procedures appear to be similar (provided I use the p_T, T_T, and θ boundary conditions), I cannot explain this discrepancy. Moretti feels this is due to an error in my code.

Therefore, based on my work (p_T, T_T, and θ calculations; u, v, and ρ calculations; supersonic inflow/outflow calculations; 1-D stability analysis) inviscid, diverging, subsonic flow is conditionally unstable. Moretti argues that based on his work, these flows are stable (recall he discounts my u, v, and ρ calculations and 1-D stability analysis). Considering that my conclusions, as well as Moretti's, are based on numerical computations, there is a considerable margin for error.

References

[1] Cline, M. C., "Stability Aspects of Diverging Subsonic Flow," *AIAA Journal*, Vol. 18, May 1980, pp. 534-539.

[2] Crocco, L., "A Suggestion for the Numerical Solution of the Steady Navier-Stokes Equations," *AIAA Journal*, Vol. 3, Oct. 1965, pp. 1824-1832.

[3] Chu, C. K., "Numerical Solution of Navier-Stokes Equations: Survey and Some Observations," Symposium on Numerical Fluid Dynamics, NASA Langley Research Center, Hampton, Va., Oct. 1972.

[4] Ganz, A. A. and Serra, R. A., "Boundary Conditions and Uniqueness in Internal Gas Dynamic Flows," *AIAA Journal*, Vol. 12, March 1974, pp. 263-264.

[5] Rudy, D. H., Morris, D. J., Blanchard, D. K., Cooke, C. H., and Rubin, S. G., "An Investigation of Several Numerical Procedures for Time-Asymptotic Compressible Navier-Stokes Solutions," Conference on Aerodynamic Analyses Requiring Advanced Computers, NASA Langley Research Center, Hampton, Va., March 1975.

[6] Steger, J. L., Pulliam, T. H., and Chima, R. V., "An Implicit Finite Difference Code for Inviscid and Viscous Cascade Flow," AIAA Paper 80-1427, AIAA 13th Fluid & Plasma Dynamics Conference, Snowmass, Colo., July 1980.

[7] Cline, M. C., "Computation of Steady Nozzle Flow by a Time-Dependent Method," *AIAA Journal*, Vol. 12, April 1974, pp. 419-420.

[8] Cline, M. C., "Computation of Two-Dimensional, Viscous Nozzle Flow," *AIAA Journal*, Vol. 14, March 1976, pp. 295-296.

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Marangoni-Number-Dependent Bubble Velocity

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A RECENT article by Papazian et al.[1] concerning bubble behavior during solidification in low gravity. One part of their paper deals with the effects of an imposed temperature gradient on bubble motion in the melt. A temperature gradient parallel to the phase boundary of two fluids induces a gradient of interfacial tension which generates convection. Young et al.[2] have shown that in the case of fluid particles in a fluid medium the interfacial tension driven convection can cause linear particle motions. Papazian et al.[1,3] applied the formula of Young et al. for estimates of particle velocities in their space experiments. Their experiments, however, yielded bubble velocities that were much lower than calculated (<1%). The authors discussed possible explanations for this discrepancy and finally concluded that "reasonable doubts exist as to whether thermocapillary forces will cause bubble motion in low gravity."[1] We think that this statement calls for a comment: Aside from the influence of contamination of the phase boundary (which has been thoroughly tested by the authors[1]), a reason for the discrepancies relates to the magnitude of the Marangoni numbers, which in the experiments of Papazian et al. were between 1 and 5000.[4] In connection with Marangoni numbers the authors mention the criterion of Pearson[5] which says that a critical value of $Ma_c = 80$ must be exceeded for convection cells to be induced in a horizontal geometry. In such a situation sufficiently large Ma are required to maintain original variations in surface temperature. However, the application of Pearson's criterion in connection with Young's theory of particle motion can be misleading. The following considerations will show that low Marangoni numbers, as in the experiments of Young et al.[2] and Coriell et al.[6] ($10^{-4} < Ma < 10^{-2}$), are prerequisite for Young's calculation to be valid.

The particle velocity according to Young et al.[2] is:

$$v_p = -[2R/(6\eta_e + 9\eta_i)] \text{ grad}\gamma \tag{1}$$

with $\text{grad}\gamma = \text{constant}$, and η_e, η_i are the viscosity of medium and particle, respectively, R is the radius, and γ the interfacial tension.

When a constant gradient of temperature is imposed and the temperature distribution depends on conduction only the velocity is:

$$v_p = [2R\kappa_e/(2\eta_e + 3\eta_i)(2\kappa_e + \kappa_i)](\partial\gamma/\partial T)\text{grad}_0 T \tag{2}$$

where κ is heat conductivity, and $\text{grad}_0 T$ is the imposed gradient.

For bubbles in a surrounding liquid we have $\eta_e \gg \eta_i$ and $\kappa_e \gg \kappa_i$ and the velocity is:

$$v_p = -(R/2\eta_e)(\partial\gamma/\partial T)\text{grad}_0 T \tag{3}$$

But as soon as interfacial tension driven convection sets in the temperature distribution is distorted, since convective heat transport must also be taken into account. Under stationary conditions the temperature distribution is now determined by:

$$\vec{v} \text{ grad } T = \chi\Delta T \tag{4}$$

where χ is the thermal diffusivity.

The ratio of convective and conductive heat transport is measured by the Péclet-number ($P_e = vR/\chi$).

For $Pe \rightarrow 0$, the left-hand side of Eq. (4) vanishes. This however, is one of the basic assumptions underlying the derivation of Eqs. (2) and (3), respectively. Taking into account that the flow velocity v involved in Young's model is of the same order as the particle velocity v_p, the Péclet number for interfacial tension driven convection may be identified with the Marangoni number:

$$Ma = [R^2(\partial\gamma/\partial T)\text{grad } T]/(\eta\chi)$$

In conclusion, it can be stated that bubble velocities calculated with Young's formula are in good agreement with observations provided that the Péclet number is small ($<10^{-2}$). In the experiments of Papazian et al. the Marangoni number, and by consequence also Pe, was higher by at least a factor of 100, so that only substantially reduced velocities (compared to

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