Reply by Author to A. N. Tifford

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THE paper published by the author[7] in the February issue of AIAA J. is part of the results of a long range research program on rheology conducted by the author. It has two purposes, namely, to discuss the possible similarity solutions and to apply the group-theoretic method to such an analysis.

The selection of an infinite flat plate as a problem and the importance of the power law model in non-Newtonian flow need no defense. The fact that some authors[11,12] treat infinite and semi-infinite flat plate problems together does not mean the infinite flat plate itself does not constitute a research problem. The importance of the power law model of the Ostwald-de Waele model can be shown by simply counting the number of publications in recent years on this model. Although it is empirical, it has been observed that a large number of non-Newtonian fluids behave according to this simple law. The discusser's so-called "more general laws of viscosity," Eqs. (1, 2 and 3) in the Comment, is nothing more than one of an infinite number of possible mathematical functions. Any discussion on them will be against the "Plea, and Clarion Call" of the Editor.[10]

The group-theoretic method used in the original paper[7] is a method based on the concepts developed from the theory of transformation groups. It was first given by Birkhoff[1] and then by Morgan[6] and is discussed in detail in a recent book by Hansen.[2] Kline's new book[4] has also an excellent discussion on it. The author would like to point out that the method used in the paper by the discusser (Ref. 9 in his Comment) is not the group-theoretic method as he claims.

The author is happy to know that Wells[12] obtained the same results by the usual free-parameter method. This result came to the author's attention after the paper was published. Even so, the author does not regret publishing this paper. Checking the same results by entirely different methods is also worth doing. Two recent works on the group-theoretic method are of this nature. One of them is a recent report (1963) by Manohar,[5] of the Mathematics Research Center of the University of Wisconsin, in which the reresults of Schuh (1955) on the unsteady boundary-layer flow[9] and those of Hansen (1958) on three-dimensional boundary-layer flow[3] are checked by the group-theoretic method. The results in the original works were obtained by the free-parameter method. The other is the recent work (1965) by Rao[8] in which he was trying to justify the form of the well-known solution of von Karman's problem of a rotating disk (1921) by the method of group theory. The contribution made in these two reports to our understanding of similarity solutions is the same as that in the original works.

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Comment on "Perturbation Solutions for Low-Thrust Rocket Trajectories"

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THE solutions presented by D. P. Johnson and L. W. Stumpf (Ref. 1) to a specific form of the problem of low-thrust trajectories, namely trajectories arising from the application of low thrusts at constant angle to the radius vector, suffer, as the authors themselves recognize, from the disadvantage of being truncated series solutions with undefined convergence properties. Thus, in the specific example treated, even the second-order theory yields results whose accuracy is doubtful and a priori unascertainable beyond the first revolution of the trajectory.

The method described in Refs. 2–4 can in fact quite simply be extended to solve, to a very good approximation, the problem, treated in Ref. 1, generalised to remove the limitations of constant satellite mass and initially circular parking orbit and extended to cover a considerably larger stretch of the trajectory.

Thus, referring to Eqs. (3) of Ref. 4, these, in the circumstances described in Ref. 1, become

$$[(d^2u)/(d\theta^2)] + u = (1/p^2) - \{[(\cos(\psi + \beta)]/[(A - \tau)u^2p^2 \cos\beta]\} \quad (1)$$
$$(dp^2)/(d\theta) = (2 \sin\psi)/[(A - \tau)u^3]$$

where β is the elevation of the trajectory, and ψ is the constant angle of the thrust vector to the radius vector. If the elevation β is small, an approximate form of the first equation in (1) is

$$[(d^2u)/(d\theta^2)] + u = (1/p^2) - \{\cos\psi/[(A - \tau)u^2p^2]\} \quad (2)$$

We note that, if ψ is little different from $\pi/2$, the error involved in using $\cos\psi/(A - \tau)$ for $[\cos(\psi + \beta)]/[(A - \tau)\cos\beta]$ on the right-hand side of (2) is of order higher than $1/A$ and, hence, need not affect significantly any general solution derived in that range of ψ. If, on the other hand, ψ is significantly different from $\pi/2$, then this substitution is fully justified provided, as is assumed, β is small throughout the portion of the trajectory considered. Thus, such trajectories must originate from parking orbits of small eccentricities. The second equation in (1) is exact with ψ = const and the

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solution of Ref. 4, i.e.

$$\left.\begin{aligned} 1/\bar{p} = p_0/p = 1 + (w'c/g_0)\log[1 - (\tau/A)] \\ \theta = ap_0(\tau - \alpha' A\{3[X(\log X - 1) + 1] + \\ 3a'[X(\log X - 1)^2 + (X - 2)] + \\ a'^2[X(\log X - 1)^3 + 3X(\log X - 1) - (2X - 6)]\}) \end{aligned}\right\} \quad (3)$$

where, $w' = w \sin\psi$, $X = 1 - \tau/A$, $a = wc/g_0$, and $a' = w'c/g_0$, applies to the secular part of the trajectory. If the satellite is of constant mass, the parameter τ can be eliminated by proceeding to the limit as $c \to \infty$ and $\dot{m} \to 0$, and the two equations (3) reduce to the single equation

$$p_0/p = 1 - \sin\psi\{1 - [1 - (6\theta/Ap_0)]^{1/2}/3\} \quad (4)$$

Following the argument of Ref. 4, the periodic component is assessed by determining a suitable function u of θ such that the initial conditions, when $\theta = \tau = 0$, are

$$\left.\begin{aligned} (u)_{\theta=0} &= 1 \\ (du/d\theta)_{\theta=0} &= 0 \\ [(d^2u)/(d\theta)^2]_{\theta=0} &= -1/p_0^2[e + (\cos\psi/A)] \\ &= -e'/p_0^2 \end{aligned}\right\} \quad (5)$$

where, $e' = e + (\cos\psi/A)$, assuming that the trajectory starts from an apse of the parking orbit. The last equation is obtained from (2) at $\theta = \tau = 0$. From a comparison of the set (5) and the corresponding set in Ref. 4, the complete trajectory is determined to a very good approximation as

$$u = (1 - \cos\psi/A + e' \cos\theta + 2\bar{p}^{p_0/2a} \sin\psi \sin\theta/A)p^2 \quad (6)$$

with $\bar{p}$ given by (3), and, if the variation in satellite mass is ignored ($a \to \infty$), (6) reduces to

$$u = (1 - \cos\psi/A + e' \cos\theta + 2 \sin\psi \sin\theta/A)/p^2 \quad (6a)$$

with p given by (4). If, in addition, the initial parking orbit is circular (6a) reduces further to

$$u = (1 - \cos\psi/A + \cos\psi \cos\theta/A + 2 \sin\psi \sin\theta/A)/p^2 \quad (6b)$$

with

$$1/p = 1 - \sin\psi\{[1 - (1 - 6\theta/A)^{1/2}]/3\} \quad (7)$$

Equations (6) and (3) determine the complete trajectory of the satellite to an accuracy of $1/A^2$ of e/A whichever is larger.

Equations (6b) and (7) have been used to compute the trajectories evaluated in Ref. 1 namely, trajectories corresponding to $\psi = 0°$, 45°, and 90° with $A = 100$ ($A = 1/\alpha$), for comparison purposes, and this shows excellent agreement with the second-order theory up to $\theta \approx 350°$, beyond which point the two trajectories diverge markedly thus confirming the convergence difficulties anticipated in the series solution. The limitation imposed on the solution (6) and (3) is that β be small in its range of validity.

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Comments on "Review of Recent Developments in Turbulent Supersonic Base Flow"

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THE note by Wazzan[1] appears to be concerned chiefly with the hypothetical limit to which the base pressure tends as the ratio of the boundary-layer momentum thickness θ to the base height h approaches zero. In the last paragraph of the note, Wazzan concedes that the modifications to the original theory of Korst[2] and others, suggested in Ref. 3, represented a significant improvement as regards predicting the variation of base pressure with θ/h. Presumably, he would be even more impressed by the work done by various authors,[4–6] since Ref. 3 was issued, but of which he does not seem to be aware.

Arguments as to the precise limit to which the base pressure tends as θ/h is reduced to zero are, to a large extent, academic. This point is emphasized by Roshko and Thomke[7] in a paper written subsequent to the one[8] by those authors referred to by Wazzan. A turbulent base flow with zero boundary-layer thickness at separation is physically unrealistic (the shear layer springing from the separation point would be laminar, initially). Nor, in any practical case, can infinite values of θ/h be reached by increase of h. The "limiting base pressure" is a figment of the theory rather than a quality having any physical significance, and the values ascribed to it depend on the method of extrapolation from conditions of small but finite θ/h.

The discussion presented in Ref. 3 was intended to show that base pressures lower than the values predicted by Korst's method (which are "limiting" values) had indeed been measured for finite values of θ/h. Wazzan casts doubt on the evidence from transitional base flows; he makes no attempt to discredit the more important evidence from measurements at low supersonic speeds ($1.0 < M < 1.4$).

The main objects of Ref. 3 were to draw attention to the shortcomings of existing theories (principally as regards the reattachment criterion), and to indicate a modified criterion, which enabled the theory to predict base pressures more accurately over a range of (finite) θ/h. However, it is generally agreed that the reattachment criterion proposed was still inadequate. Soon after Ref. 3 was published it became apparent that the pressure rise up to the reattachment point was not a constant fraction N of the over-all pressure recovery, but that this fraction varied significantly with the boundary-layer thickness at separation. However, no evidence has been produced yet to show that the value of N reaches unity for $\theta/h = 0$. If a crude assumption is required, it is better to assume that the pressure recovery occuring downstream of the reattachment point is independent of the boundary-layer thickness. On the other hand, there would seem more prospect of success in methods that compute the downstream pressure rise from an examination of the changes in the boundary-layer profile during rehabilitation.

As evidence to support his criticisms, Wazzan refers to the tests of Roshko and Thomke, which were done on a body of revolution with a step in the surface. These tests appeared to indicate that the base pressure approached Korst's values as the boundary-layer thickness was reduced. It is dangerous to argue about two-dimensional base flows on the basis of tests on axisymmetric models because, even when the radius is large for the latter, the fact that the stream tubes are annular still leads to streamline shapes and pressure distributions that are characteristically different from those downstream of

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