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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a two-dimensional step. In the axisymmetric case, the pressure rises through the reattachment region to a value higher than the final recovery pressure and subsequently falls again. The existence of this overshoot leads to higher values of N (to use the terminology of Ref. 3) than in two-dimensional supersonic flow. However, as Wazzan is no doubt aware now, Roshko and Thomke have since produced evidence, even from axisymmetric tests, that for small values of θ/h, the base pressure falls below the values predicted by Korst.

By and large, Wazzan's remarks are merely an elaboration of those in Ref. 8 and in view of the later comments of Roshko and Thomke[8] his criticisms would seem to have little basis.

References

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Reply by Author to J. F. Nash

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IN an article concerned with turbulent base pressure Nash[1] reviewed the theory of Korst[2] and presented arguments to point out its shortcomings. Nash sums up his criticisms by stating "the really important point which emerges from a study of the complete base-flow solution is, however, as follows: The form of the variation of base pressure with boundary-layer thickness derived from the theory indicates that the *limiting* base pressure cannot be estimated successfully by an extrapolation from measurements made at small but finite values of the ratio of boundary-layer thickness θ_s to base height h" and that "a linear extrapolation of the curve from position of finite boundary-layer thickness could result in a serious over estimation of the limiting base pressure."

It is, therefore, evident that Nash's criticisms mainly are based on his allegation that Korst's theory, and its extension to account for the effect of the approaching boundary layer, fails to predict what he considers to be the correct limiting base pressure.

The note by Wazzan[3] was not the least concerned, as alleged by Nash in the preceding comment, with the so-called hypothetical limiting base pressure nor with whether it has any physical significance or not. Rather, it was merely "an attempt to point out first, the difficulties in using the preceding arguments (referring to Nash's arguments against Korst's theory) to discredit Korst's theory, and second, some of the difficulties associated with the theory of Nash and their effect on base pressure."

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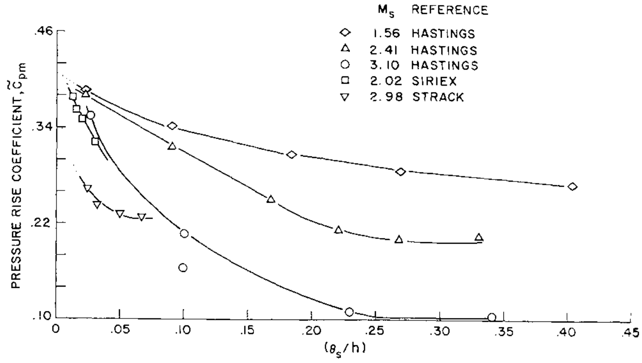

Fig. 1 The effect of θ_s/h on reattachment pressure rise coefficient $\tilde{C}_{pm}$.

Nash's theory is an improvement over earlier theories[4,5] insofar as his expression for the base pressure included the parameter θ_s/h. However, the theory had many shortcomings, which were discussed in Ref. 3.

The work of MacDonald[6,7] and Roberts[8] were not discussed in Ref. 3 because first, none of these were available at the time of submitting the article to the AIAA Journal, and second, although Nash's comment and Ref. 5 only were available at the time of revising the article, their contents were not discussed because the object of the note was merely a discussion of Nash's work (see last paragraph of the introduction of Ref. 3).

Experiments at Mach numbers as large as three were presented and discussed in Ref. 3. More data[9–11] in the Mach number range of 1.56 to 3.10 in support of my previous criticisms will be given here.

Nash's comment contends that in Ref. 1 he merely intended to show that base pressures lower than the limiting values of Korst were measured at finite values of θ_s/h. I certainly do not contest this statement, rather I contend (Sec. 1 of the discussion of Ref. 3) that in general the values of base pressures cited by Nash[1] were obtained either for geometries or under conditions not strictly applicable to Korst's theory.

Nash's comment also states that "as evidence to support his criticisms, Wazzan refers to the test of Roshko and Thomke[13] which were done on a body of revolution with a step in the surface." In reply to this remark, the following should be noted: 1) In support of my criticisms, data from five different experiments[4,12–15] were presented (Sec. 2 of discussion of Ref. 31). Only one, that of Roshko and Thomke,[12] was obtained from tests on a body of revolution, whereas the rest[4,13–15] were obtained from tests on two-dimensional backward facing steps, which is the model of the Korst theory. Furthermore, the results of Ref. 12 were used with reservations that were clearly indicated. Therefore, in view of the foregoing and of the discussion presented in Sec. 1 of Ref. 3 on why results from tests on three-dimensional bodies should not be used to test the theory of Korst, and in view of Nash's own statement that "it is dangerous to argue about two-dimensional base flows on the basis of axisymmetric models," perhaps, it would be best to disregard all results from tests on three-dimensional bodies in this discussion. On the other hand, the two-dimensional results[4,13–15] strongly support[3] the theory of Korst.

In the last paragraph of his note, Nash states that "by and large Wazzan's remarks are merely an elaboration of those in Ref. 8 (Ref. 8 is Ref. 12 of this note) and in view of the later comments of Roshko and Thomke his criticisms would seem to have little basis." In reply to these allegations I should