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.

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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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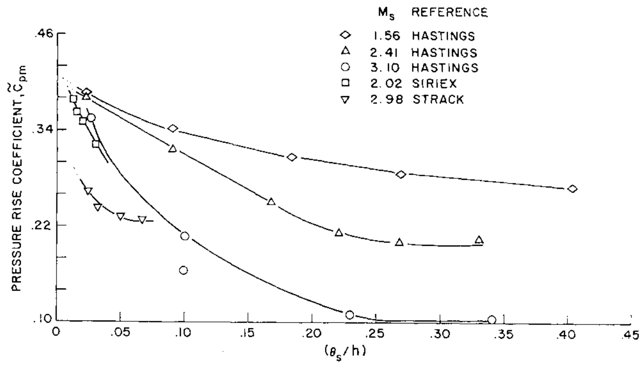

Fig. 1 The effect of θ_s/h on reattachment pressure rise coefficient $\tilde{C}_{pm}$.

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 like to submit the following. Wazzan's criticisms were presented in three separate sections under the heading of discussion. The first is a discussion of the flow over a two-dimensional step vs the flow over an isolated airfoil section. The second discussed the manner of extrapolating base pressures from finite values of θ_s/h to $\theta_s/h = 0$. The third discussed the effect of approaching boundary layer on base pressure within the framework of the theory of Nash. Only the second, and shortest section, made use of and discussed the results of Roshko and Thomke, whereas the material of the first and third sections were completely foreign to their work and their remarks. Yet, Nash would like to have us believe that our criticisms are merely an elaboration of those of Roshko and Thomke.

Although it has been shown that the criticisms of Ref. 3 are almost entirely independent of the remarks by Roshko and Thomke, it should be noted next that their later remarks[16] referred to by Nash, concerning the extrapolation to $\theta_s/h = 0$, do not invalidate my previous contentions on this issue.

Figure 11 of Ref. 16 is a plot of $\tilde{C}_{pm}$, on axisymmetric and two-dimensional bodies, as a function of $(\theta_s/h)^{1/2}$ where $\tilde{C}_{pm}$ is the pressure rise coefficient at reattachment. On the basis of this plot, which contains data from Refs. 9–11 and 16, they state that "it appears that Nash's criticism of earlier methods of linear extrapolation from relatively high values of θ_s/h is well founded." For the reasons discussed earlier we shall disregard the axisymmetric results of Ref. 16, and we shall concern ourselves with the two-dimensional results of Refs. 9–11 only. These results are presented in Fig. 1 as a function of θ_s/h. It is seen that on this linear scale for abscissa the data do not seem to extrapolate to values far from the Korst value of about 0.385. Of course, we have shown in our previous note[3] (Sec. 2) that, e.g., the Korst value falls somewhat below the extrapolated value, whereas the values predicted by Nash fall considerably above the extrapolated ones. The plot of $\tilde{C}_{pm}$ vs $(\theta_s/h)^{1/2}$ in Ref. 16 perhaps greatly exaggerates the steepness of the curve at small values of θ_s/h. In fact, had Roshko and Thomke plotted $\tilde{C}_{pm}$ vs $(\theta_s/h)^{1/4}$, e.g., the extrapolated values for C_{pm_0} (subscript 0 indicates value at $\theta_s/h = 0$) for the data of Strack ($M_s = 2.98$) and Hastings ($M_s = 2.41$ and 3.10) would be larger than unity!

Admittedly, it is quite difficult to ascertain the appropriate mode of extrapolation to $\theta_s/h = 0$. However, in the case where the limiting value of base pressure or C_{pm_0} is not known a priori, either from theoretical and or physical considerations as in the present case, a linear extrapolation from small values of θ_s/h to $\theta_s/h = 0$ is a common practice and perhaps more meaningful than other methods of extrapolation.

In conclusion, it is demonstrated that the comments by Nash are not well founded.

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Errata: "Application of Pohlhausen's Method to Stagnation-Point Flow"

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In the Nomenclature, the definitions for δ^* and θ should read as follows:

$$\delta^* = \text{displacement thickness } \delta \int_0^1 \left(\frac{U-u}{U}\right) d\eta$$

$$\theta = \text{momentum thickness } \delta \int_0^1 \frac{u}{U}\left(\frac{U-u}{U}\right) d\eta$$

Also, Eq. (2) should read $\tau_0/\rho = (d/dx)(U^2\theta) + \delta^* U(dU/dx)$.

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