the body. They attempt to explain this discrepancy between their results and the Pappas and Lee data by stating "the sphere-cone considered by Pappas and Lee[6] ended at $S \sim 5$, and almost all of this body was within the length affected by the discontinuity in surface curvature." If this is the case, then it appears to demonstrate more than anything else the shortcoming of their present theoretical treatment of the sphere-cone junction. In fact, we are rather surprised that they did not modify their treatment of the curvature effect in an attempt to ascertain whether or not their present formulation can indeed accommodate realistic calculations of viscous flows over sphere-cones. Also lacking is any discussion on why they believe that their results would be any more accurate and reliable for longer sphere-cones than for the intermediate-sized sphere-cone used by Pappas and Lee. In spite of such poor agreement with the data, Miner and Lewis unjustifiably claim in their abstract that "The predictions of the present method agreed well with the experimental data," which include those of Pappas and Lee.[6] We believe such statements to be misleading.

To recapitulate, the laboratory data of Pappas and Lee do not substantiate the claimed validity of the analysis of Miner and Lewis on the basis of the comparisons that they presented. Rather, it appears to illustrate once again the desirability of a more complete treatment of the viscous-flow problem by application of the full Navier-Stokes equations.

In closing, it is our considered judgment that Miner and Lewis have not demonstrated the correctness of their ionized, viscous-flow analysis. Therefore, we reiterate that comparison of their theoretical results with the RAM C flight data at higher altitudes (250 kft, 265 kft, 275 kft) is one of the compelling prerequisite tasks which will afford the readers an opportunity to assess the validity of the claims made by Miner and Lewis.

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Reply by Authors to S.-W. Kang and M. G. Dunn

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IN the preceding Comment, Kang and Dunn consistently avoided the central issues which were raised in our present paper.[1] The first issue is: "Given two viscous shock-layer methods, both of which consider thin viscous shock-layer flows, both of which stem from the same governing conservation equations—one set pseudo first-order accurate and the other set second-order accurate; both using methods of solution which are generally recognized as valid—one an approximate integral method and the second an exact finite-difference method, how could they give such radically different results?" The second issue raised by the comparison of results shown in our present paper can be simply stated as: "Is the method of Kang and Dunn appropriate to viscous-layer flows?"

In the preceding Comment it was acknowledged there were some differences between the predictions of the two methods at the stagnation point, but the far larger differences in the heat-transfer distributions were totally ignored. Also ignored were the differences in the shock-temperature distributions, shock-layer thickness distributions, and temperature profiles. None of the three points made in that Comment addressed these issues.

In the development of the method of Kang and Dunn, it appears that the validation of the accuracy of their method was almost totally dependent upon the comparisons with the RAM C electron concentration profiles. In fact, under their point 1, it is claimed that the electron concentration profile data for the RAM C flights represent a standard to which the

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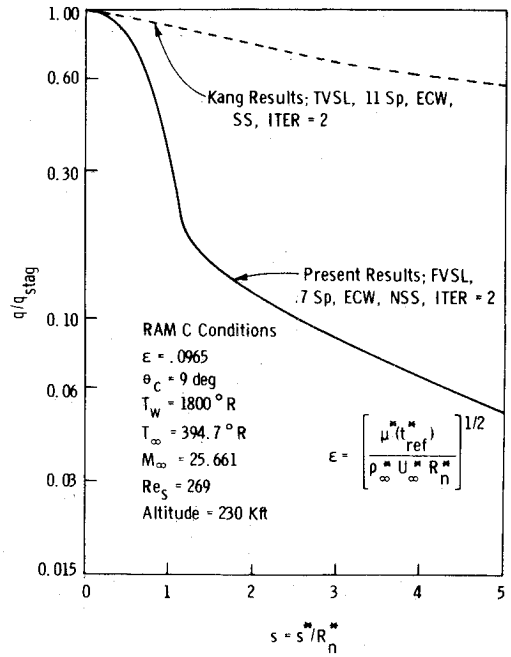

Fig. 1 Comparison of normalized heat-transfer distributions for the RAM C at 230 kft.

accuracy of various calculation techniques should be compared. The apparent implication of this claim is that the electron concentration profiles. as measured during the RAM C flights, form a complete and sufficient standard for comparison. This is equivalent to saying that as long as the desired results are obtained, it doesn't matter whether any of the other results are correct or not. Kang and Dunn seem to ignore the importance of the reaction rates on the magnitude of the electron concentrations predicted. Also, the probes used in the RAM C were fixed at one location on the body, and thus distributions along the body were not experimentally measured. While the reaction rates for single ionizing species air are relatively well known, small changes in the reaction rate data can have significant changes in the predictions of electron concentrations. For example, the electron concentration profiles predicted by the present method with two different, but well used, sets of reaction rate data were shown in Fig. 7 of the present paper.[1] The difference in reaction rate data produced a 50% difference in the peak electron density. Calculations made with another set of reaction rate data (Ref. 24 of present paper[1]) gave nearly an order of magnitude change in the peak electron concentration, but predictions of the surface heat-transfer rate were scarcely affected.

We contend that it is imperative to first have a reliable model and prediction technique for the fluid mechanics before attempting to make predictions of electron concentration profiles. The reliability of the fluid mechanics model might well be tested by making comparison with heat-transfer data. Under point 1 in the preceding Comment, it is stated that surface heat-transfer rates and local static pressures were not measured for the RAM C flights and thus were not available for comparison of calculation technique accuracy. While there were no heat-transfer data obtained with the RAM C flights, there is an abundance of other heat-transfer data in the literature. A particular example is the experimental data obtained by Pappas and Lee[2] at Mach 13 for a 7.5° sphere-cone. The question might be raised though: "Is this a legitimate set of data for comparing the predictions of a viscous shock-layer method?" In the present paper[1] the Reynolds number similarity was pointed out between the RAM C conditions at 230 kft and the experimental conditions of Pappas and Lee. In fact, the shock Reynolds number for the Ames conditions was lower than for the RAM C conditions at 230 kft. Further, the results shown in our present paper[1] in the left portion of Fig. 11 clearly indicated that, within a few percent difference, the predictions for the RAM C and for the Ames conditions were in quite reasonable agreement as demanded by the Reynolds number similarity.

Before further considering a comparison with the experimental data of Pappas and Lee, it is appropriate to look at the precise differences between the predictions of heat-transfer distributions obtained by the present method and the results obtained by Kang and Dunn[3-6] with their integral method. Such a comparison is shown in Fig. 1 (see Ref. 1 for figure nomenclature). The heat-transfer distributions shown were normalized with the stagnation point heat-transfer rate. The difference between the character of the heat-transfer distribution predicted by Kang and Dunn[3-6] and our prediction is immediately obvious. In particular, at $s = 5$, the present method predicted a q/q_0 of approximately 0.08, whereas the method of Kang and Dunn predicted a q/q_0 of approximately 0.6. This is an *order of magnitude* difference. It is differences of this nature and of this magnitude which should be the central issue; however, in the preceding Comment, Kang and Dunn chose to ignore such differences.

In point 3 of the preceding Comment, Kang and Dunn claim that the predictions of the present viscous shock-layer method agreed poorly with the experimental data of Pappas and Lee.[2] We do not claim that the predictions of the present method agree exactly with the experimental data. It is, however, very fair to question what is good and poor agreement. In Fig. 2 of this reply, the two heat-transfer distributions shown in Fig. 1 are added to the left portion of Fig. 11 of our present paper,[1] using predictions from our 7 species air model instead of dissociating oxygen. As previously noted, the present predictions of heat-transfer distributions for the RAM C and for the Ames conditions were in reasonably good agreement despite the difference in the sphere-cone half angles. Both distributions agreed reasonably well with the experimental data but did not agree as well as did the results of Lewis, Adams, and Gilley.[7] In contrast, the predictions of Kang and Dunn[3-6] for the RAM C conditions differed from the present prediction of the heat-transfer distribution by an order of magnitude at $s = 5$ and thus by an order of magnitude from the boundary-layer theory prediction of Lewis, Adams, and Gilley[7] and the experimental data of Pappas and Lee.[2] It would seem to be

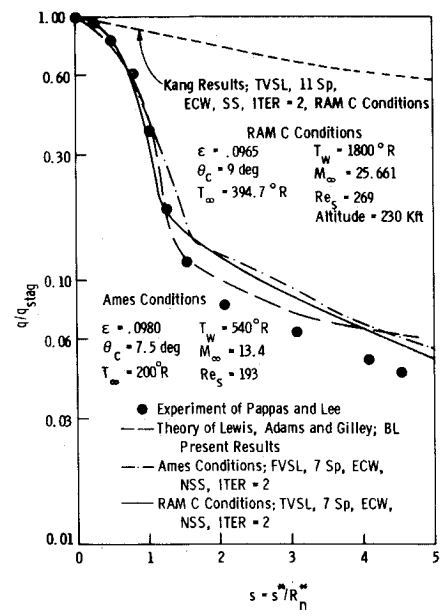

Fig. 2 Predicted heat-transfer distributions for the RAM C and Ames conditions compared with experimental data.

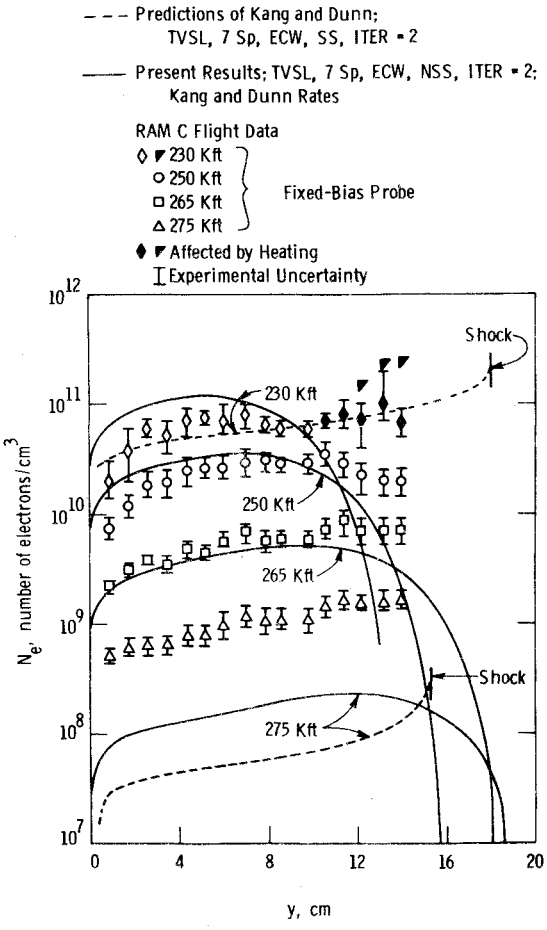

Fig. 3 Present predictions of electron concentration profiles without shock slip compared with experimental data and predictions of Kang and Dunn, RAM C conditions, $s^*/R_n^* = 8.8$.

extremely clear that if the present predictions agreed poorly with the experimental data, as was claimed in the preceding comment, the prediction of Kang and Dunn[3-6] for the heat-transfer distribution was in far worse agreement.

In the preceding Comment, Kang and Dunn did acknowledge differences in the heat-transfer rate at the stagnation point. They provided no explanation except they had compared their results with Cheng's perfect gas theory[8] and had found agreement within 20% to 30%. They state that they do not understand the contradictory results. but they have checked their calculations and remain convinced that the results presented in Fig. 2 of their Comment are correct. The appropriate question appears to be not whether the results presented in Fig. 2 of their comment are correct, but what their results should have been for the multi-component gas model. As shown in Fig. 2 of their comment, at the value of Cheng's $K^2 \simeq 90$, their value of the heat-transfer coefficient was approximately 0.05. The value obtained by Cheng[8] was approximately 0.065. Their result was approximately 70% of Cheng's value. Referring to Table 1 of our present paper,[1] their heat-transfer rate at the stagnation point was 70% of the value of the stagnation point heat-transfer rate which we obtained for a perfect gas viscous shock-layer model. This would put our perfect gas result in exact agreement with the perfect gas result of Cheng. Such was indeed the case. It should further be noted that Davis[9] compared with the results of Cheng[8] and obtained essentially exact agreement. Further, and crucially, the results of Cheng[8] were for a perfect gas. Thus, the perfect gas results of Davis[9] and of the present method were in excellent agreement with the results of Cheng and in fact these perfect gas results agreed substantially better with the results of Cheng than did the results of Kang and Dunn.[3-6] The question then becomes, "What should the stagnation-point results have been?" The data shown in Table 1 of our present paper[1] indicate that for either dissociating oxygen or for multi-component air the stagnation point heat-transfer rate for a viscous shock layer with equilibrium catalytic wall should have been 50 to 100% higher than the heat-transfer rate obtained for a perfect gas shock-layer flow. In contrast, the results of Kang and Dunn[3-6] were only 70% of the perfect gas value. Thus it is apparent that their results for an 11 species gas model at the stagnation point were only one-half or less of what they should have been.

In summary, our response to point 1 of the preceding Comment is that we have demonstrated our results are valid in the ionized viscous flow regime. Further Kang and Dunn stated that the presentation of comparisons for only a lower altitude is misleading and raises questions as to the appropriateness of the present approach for ionized viscous flows. It is true that good agreement with predictions of boundary-layer theory for one or two cases is inadequate to fully validate the applicability of a viscous shock-layer method; however, it is *also* true, that extremely poor agreement with predictions of reliable boundary-layer methods and experimental data are sufficient to *invalidate* the applicability of a viscous shock-layer method. We submit the present viscous shock-layer method passes all tests without difficulty and the method of Kang and Dunn fails most tests.

The applicability of the present finite-difference, viscous shock-layer method can further be illustrated by the electron concentration profiles shown in Figs. 3 and 4. The present predictions are for thin viscous shock-layer flows with and without shock slip at the 4 altitudes considered, 230, 250, 265, and 275 kft. Included in Figs. 3 and 4 are the experimental data and the predictions of Kang and Dunn. In Ref. 4, Kang and Dunn noted that at 230 kft the electron concentration profiles predicted with a single ionizing species (seven total species) and with 5 ionizing species (eleven total species) were essentially the same, but that at 275 kft the five ionizing species chemistry model predicted electron concentrations an order of magnitude higher than the single ionizing species (NO^+) model. Thus, the predictions of Kang and Dunn as shown in Figs. 3 and 4

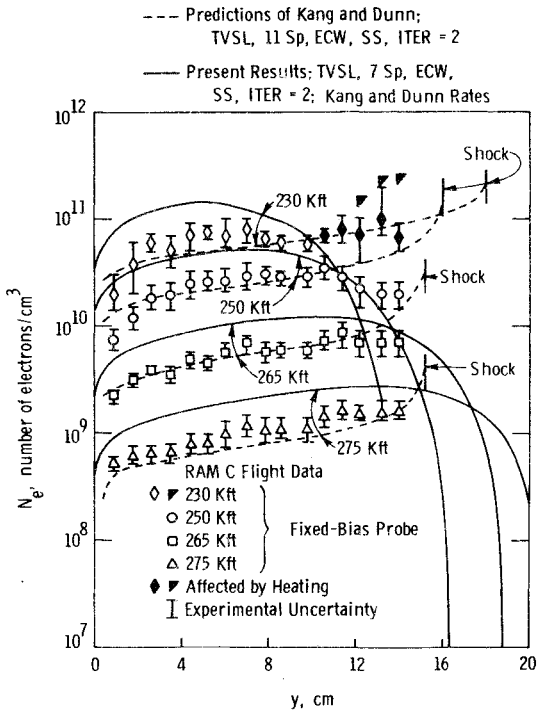

Fig. 4 Present predictions of electron concentration profiles with shock slip compared with experimental data and predictions of Kang and Dunn, RAM C conditions, $s^*/R_n^* = 8.8$.

are for the five ionizing species model except at 230 and 275 kft where they included the single ionizing species model results as well. The present predictions were for the single ionizing species model only. Comparing our predictions without shock slip with Kang and Dunn's predictions with shock slip as shown in Fig. 3, the present predictions agreed reasonably well as to level of ionization with the predictions of Kang and Dunn[3-6] with the NO^+ model. The agreement between the present predictions and the experimental data was reasonably good at 230, 250, and 265 kft, but at 275 kft the present method, without shock slip, significantly underpredicted N_e. In contrast, the present predictions of electron concentration profiles, *when shock slip was included,* as shown in Fig. 4, agreed quite well with the expermental data, even at 275 kft. Without shock slip, the species concentrations behind the shock were the same as in the freestream and thus $C_{NO^+} = C_{e^-} = 0$. With shock slip, however, a finite concentration of NO^+ and thus e^- was permitted behind the shock and diffusion carried the ions to the shock zone. While the electron density at the shock was quite low with shock slip, the nonzero value of C_{NO^+} behind the shock raised the electron density profile, especially at the higher altitudes. Also at 275 kft, with shock slip we found a thicker viscous shock layer, an increased static temperature and a decreased density distribution from the maximum values in the layer to the shock. We thus found an increased ionization due to the higher temperature and decreased deionization due to the lower density.

Further, the present predictions showed correctly two trends not shown by the predictions of Kang and Dunn[3-6]: 1) the viscous-layer thickness increased with altitude, and 2) the present predictions showed that the peak of the electron concentration profile occurred within the viscous layer and not at the shock. In Ref. 4, the explanation given by Kang and Dunn for the peak of the N_e profile occurring at the shock was that the temperature immediately behind the shock was quite low, with an accompanying high density. Thus, it was reasonable for the peak electron concentration to occur at the shock since with shock slip there was a finite electron concentration at the shock. If this were modified to state that the peak occurred near the shock, this would appear to be reasonable. In fact, such results were indicated by the present predictions at 265 and 275 kft. However, it appears totally unreasonable that the electron concentration profiles would show a peak *at* the shock, much less such a strong upswing toward the shock as predicted by Kang and Dunn.

In the preceding Comment, Kang and Dunn try to raise some other issues. For example, they claim in the present day it is much more appropriate to use the full time-dependent Navier-Stokes equations in a viscous shock-layer regime. As part of their argument, they point out that the present finite-difference method required a large amount of computing time, e.g., 35 min for a 9° sphere-cone with multi-component air for a length of 50 ft (to $s^*/R_n^* = 100$). What they did not indicate was the probable time that would be required for a typical time-dependent Navier-Stokes method. Such calculations could easily require several hours on a CDC 7600 (a computer much faster than our IBM 370/158) to compute the entire flowfield with finite-rate chemistry. Further, whether the Navier-Stokes methods should be used or not is not relevant. The viscous shock-layer equations and the boundary-layer equations result from a proper expansion of the Navier-Stokes equations retaining terms of the appropriate order. As far as the governing equations are concerned, the equations of the present viscous shock-layer method, accurate to second order, should be considerably more accurate in a viscous shock-layer regime than the equations used by Kang and Dunn[3-6] which were very similar to first-order boundary-layer equations with the inclusion of a normal momentum equation but with the longitudinal pressure gradient term omitted which they denoted in Ref. 4 as a higher-order term. Further, the normal momentum equation which they used was identically zero on the conical afterbody as would be the case for boundary-layer equations and was nonzero only for flow over the spherical cap. Thus their discussion of the advantages of methods using the full Navier-Stokes equations is misleading and tends to divert attention away from the comparisons which we made with their predictions and experimental data.

Further, it should be noted that Kang and Dunn[4] compared their predictions of the electron concentration profiles with the experimental data at 214 kft. The agreement they obtained appeared quite reasonable. They noted, however, that the good agreement was "perhaps fortuitous" since they had excluded the longitudinal pressure gradient term as a higher-order effect. This implied that even though they were using boundary-layer-like equations, in fact first-order boundary-layer equations, their method would not be valid in a regime where first-order boundary-layer theory could very reasonably be expected to apply. It is entirely reasonable to infer from this contradictory situation that in the development of the integral method of solution of the equations, restrictions were made in the technique which did not permit the method of Kang and Dunn[3-6] to be used at the lower altitudes in the "boundary-layer regime." The comparisons which we have made with their predictions further indicate that it is highly probable that in making these restrictions the applicability of their method was severely impaired. It further appears that all of their agreement with the electron concentration profiles was strictly fortuitous and was not based upon accurate predictions of the viscous flowfield over spherically blunted cones.

Based upon the comparisons which we have made with the results of Kang and Dunn,[3-6] it appears that the central issue in the exchange of Comment[10] and Reply[11] in the January 1974 issue of the *AIAA Journal* and also raised in the present paper,[1] the preceding Comment, and in this Reply to the Comment should be; "Is the integral method of Kang and Dunn,[3-6] as previously published, applicable to viscous shock-layer flows?" This issue Kang and Dunn have studiously avoided, apparently with good reason, since the obvious appears to be unequivocably that their method is *not* applicable to viscous shock-layer flows and most certainly not to flows in a boundary-layer regime. The latter they admit. It becomes somewhat of a problem then; "Are the RAM C conditions at 230 kft boundary-layer or viscous shock-layer conditions?" The results of Evans et al.,[12] which we discussed in the present paper, and the comparison which we made with those results, indicated that at 230 kft the conditions could be considered as boundary-layer-like conditions. If this is the case, and this appears quite reasonable, then the conclusion is that the method of Kang and Dunn is not applicable at 230 kft for the RAM C. Considering the similarity between their results at 230 kft and at 250, 265, and 275 kft it would appear that their method is not applicable at the latter altitudes either. In fact, it becomes apparent that not only was the agreement they obtained for electron concentration profiles fortuitous at 214 kft (in the boundary-layer regime) but at 230, 250, 265, and 275 as well.

Finally, they state under point 2 of their Comment that their principal requirement was for reasonably good predictions of electron density distributions at the RAM C experimental conditions. If it were not for the distinct upswing in the electron concentration profiles at the shock, we feel they achieved this one goal. However, it would seem to be abundantly clear that the other results which they presented for the 9° sphere-cone at 230 kft, such as shock-temperature distributions, shock-layer thickness distributions, and temperature profiles were completely unrealistic and contradictory. Thus, the conclusion must be that Kang and Dunn indeed obtained the result in which they were interested, namely apparently good agreement with the experimental electron concentration profiles at one location on the body, while the remaining results which they presented for the 9° sphere-cone, especially at 230 kft, such as shock-temperature distributions, heat-transfer distributions, and temperature profiles were incorrect. It then seems quite clear *why* Kang and Dunn did not address themselves to the central issue of the differences between their predictions and the present viscous shock-layer results.

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Comment on "Buckling in Segmented Shells of Revolution Subjected to Symmetric and Antisymmetric Loading"

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REFERENCE 1 contains an interesting study of its title subject based on an extension of previous work by Bushnell.[2] The only two numerical problems considered in the paper, viz: pure bending and symmetric lateral pressure on cylindrical shells, are well known in the literature.

It must be noted that a subsequent work by Bushnell[3] has not been quoted by the authors. In that paper, an approximate method is given for calculating the buckling load of axisymmetric structures subjected to nonuniform lateral loads. Bushnell determines the meridian having the "worst" disturbance of stress, from a linear prebuckling analysis, and then performs an axisymmetric buckling analysis, assuming this distribution to be uniform in the circumferential direction. This method is expected to yield buckling loads lower than those by a more rigorous approach, such as the one in the authors' paper.[1] Since the authors have intended to amplify Bushnell's work, a comparison of these two methods would have been appropriate and is of practical interest for people using the BOSOR4 code developed by Bushnell.[3] In their conclusions, the authors have stated that the mode of buckling under symmetric loads is either symmetric or antisymmetric, and both should, therefore, be considered to determine the lower eigenvalue. It is not clear to the present writers whether both the Fourier cosine and sine series have been considered in the buckling state variables for the numerical problems quoted and, if so, what differences have resulted.

It should also be noted that the lower limit of the harmonics considered in the Fourier series of the buckling state variables, need not be fixed at 0 or 1, as in Eq. (13), particularly for problems involving nonuniform lateral pressure, since the "effective" Fourier coefficients can start at any higher value depending on the shell geometry[4] etc. If K_L, the lower limit of the harmonic series, is fixed at 0, then K_U, the upper limit of the series, may have to be a high value for convergence; the stability determinant will then be unduly large, without significant influence on the eigenvalue that would be obtained when only the "effective" Fourier coefficients are considered. Hence, it appears to be more convenient to let $K_U = K_L + K_E$, where K_E is a small fixed number corresponding to the number of effective Fourier coefficients (minus one), and to study the variation of the eigenvalue as a function of K_L.

References 5–8 are concerned with cylindrical shells subjected to nonuniform pressures. Since these works deal with basically a similar problem, the writers have listed them here, as they may be of some interest to the workers in this subject. In all these works, the stability criterion used is based on the second variation of the energy that is expressed in terms of prebuckling strains and the virtual displacement components. The prebuckling analysis is carried out using a linear theory. In Ref. 5, only simply supported shells are considered, hence a Fourier series representation in the axial co-ordinate is made as in Ref. 4. In Refs. 6–8, the writers have used algebraic polynomials in the axial co-ordinate for the virtual displacement components and obtained good results for various combinations of different boundary conditions at the circular ends. Supporting experimental evidence is contained in Ref. 8.

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