nine degrees-of-freedom per span, the first four natural frequencies and vibration modes of the panels were found to have sufficient accuracy and no mass coupling was necessary. Finally, we come to the less conventional matter of AICs. Equation (11) of Ref. 1 [i.e., Eq. (21) of Nelson and Cunningham[9]] which relates the panel deflections to the pressures, led to three terms in the AICs in the thesis.[2] The first term in Eq. (11) of Ref. 1 gives the static pressure and the AICs involve numerical differentiation; this is the important term at high Mach nuumber. The numerical differentiation in Ref. 1 has only cubic accuracy, whereas Refs. 2 and 7 have quartic accuracy. The AICs for the second term are diagonal. The AICs for the third term require numerical integration, which is as routine a matter in the field of numerical analysis as is differentiation. However, Prof. Yang regards it as a source of difficulty and remarks, "It is perhaps, this difficulty that hinders the finite element workers from employing the exact linearized flow theory"[†]; and later, "The inclusion of the higher-order frequency terms causes much complexity[‡] in the formulation of an aerodynamic matrix." Equation (16) of Ref. 1 bases the third term on the trapezoidal rule with only linear accuracy, whereas Refs. 2 and 7 used an integration formula with cubic accuracy; we also note that the third term is the most important term at low Mach numbers. The final determination of the AICs in Eq. (17) of Ref. 1 is an additional averaging of the pressures over the length of the finite elements which increases the accuracy of the numerical differentiation and integration. Although this averaging increases the accuracy of the AICs probably to be comparable to Refs. 2 and 7, it does so at some computational expense.[§]

So, what's new? In terms of accuracy of formulation, we see that Ref. 1 offers us about the same, but requires a larger computational effort. In terms of results, Ref. 1 does not address the low Mach number problem per se, except at the isolated Mach number $M = 1.3$, and therefore shows the wrong trend for thickness requirement in its Figs. 5 and 8; the correct trend in thickness requirement at low Mach number was shown by Lock and Farkas (Ref. 12, Figs. 9 and 10), and a maximum occurs near $M = 1.3$. Reference 1 also shows some effects of tensile stresses, but their stabilizing effects are of little interest; it is the effects of compressive stresses that are a practical concern. In short, Prof. Yang has contributed nothing to improve upon the state-of-the-art of 1965.

To date, over 500 papers on panel flutter have been published, including a number of surveys, two of which are contained in the *AGARD Manual on Aeroelasticity,* and one textbook devoted entirely to the aeroelastic problems of plates and shells. The bibliography of these surveys is listed as Refs. 14-20. Reference 21 is added since it is a recently completed computer program (ZYNAPF) for routine design analysis of arbitrary three-dimensional panels including the effect of the boundary layer.

It is hoped that this Comment and the associated references will be useful to researchers and designers concerned with the panel flutter problem, and will discourage any further renaissance of the pressing problems of fifteen years ago.

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[†]We should point out that Ref. 7 also discussed three-dimensional AICs for flat and cylindrical panels, but only in a preliminary manner.
[‡]It does: the numbers are complex rather than real!
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Reply by Author to W. P. Rodden

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DR. RODDEN misread the first sentence of the abstract, which states that "A finite element formulation and *solution procedure* are developed for the flutter analysis of

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..." Dr. Rodden insisted that "solution procedure" should be read as "a new solution." Thus, the author's intention of developing a finite element procedure was misinterpreted as claiming a new solution. By twisting such key words, Dr. Rodden found himself a chance to tell a long story in the *AIAA Journal* about his Ph.D. thesis.

Dr. Rodden asked "What's new?" between the finite element displacement method the author used and the kind of old, obsolete flexibility influence coefficient method he used. The answers are as follows:

1) The finite element displacement model in this paper has displacement and slope degrees-of-freedom at nodal points and provides a 4×4 stiffness matrix. In the examples, four elements and six degrees of freedom are used. Dr. Rodden included only the displacements at the nine collocation control points, neglected the important slope degrees of freedom, and formulated the 9×9 flexibility matrix for the ten-segment span. Although Dr. Rodden claimed that his approach was the finite element method, clearly it was not. Not all the matrix methods are finite element methods!

2) In this paper, the consistent mass matrix is used. Dr. Rodden used a lump mass diagonal matrix. The obvious difference in the resulting accuracy between the consistent mass and the lumped mass diagonal matrices for the case of beams or infinite plates was pointed out by Archer (Ref. 13 in the Comment). In the dynamic eigenvalue problems of beams or infinite plates, it is possible that one can use an inferior flexibility matrix and an inferior lumped mass matrix simultaneously and be satisfied with the results due to the compensation of modeling errors.

3) The incremental stiffness matrix in this paper can accurately account for the important effect of initial in-plane stresses. This point is studied by the author. Examples are performed and results are presented in Figs. 6-8. Such effect was not considered by Dr. Rodden in his thesis.

4) The beauty of a numerical method does not necessarily lie in its sophistication. When the simple trapezoidal rule can achieve excellent accuracy in approximating the aerodynamic pressure, the use of sophisticated higher-order numerical integration method is of no value, especially when the structural model is crude.

Since their appearance in 1956[1] the finite element methods have sometimes been criticized for the emphasis on the methodology rather than new theory. The methods have gradually gained widespread acceptance because of their powerfulness in solving the practical problems which cannot be solved otherwise. In the development of each new finite element method, it is necessary to choose some examples with known solutions for comparison and evaluation. Once the method is evaluated, it can be used for more general and practical cases. In this paper, Cunningham's examples and solution in Ref. 7 were chosen for such an evaluation purpose. Reference 7 was published earlier than Dr. Rodden's thesis. It is absolutely pointless to reference a thesis later published by Dr. Rodden which provides the same solution as Ref. 7.

This paper establishes a basic procedure of extending the finite element method (displacement models) to include the aerodynamic effects for flutter analysis. Previous similar attempts have been made by Olson (Refs. 1 and 2 of original paper), Kariappa et al.[2] (Refs. 3 and 4 of original paper), and Sander et al.[2] The basic development in this paper has recently been extended to include the effect of thermal buckling and geometric nonlinearlity[3] and three-dimensional supersonic unsteady potential flow.[4] Contrary to a footnote in the Comment, it has been shown in Ref. 4 that the computing expense is not prohibitive in using the supersonic Mach box method.

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Comment on "Localized Diamond-Shaped Buckling Patterns of Axially Compressed Cylindrical Shells"

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I READ with great interest several articles on the isometric buckling of shells.[1] However, I am unable to trace how the vital coefficients K_I, K_I are derived so perhaps El Naschie could give the derivation of these coefficients in detail. This would be of great help to the reader.

I would also like to ask about the connection to the work of Yoshimura.[2] El Naschie does not refer to this work although it seems to me to deal with similar ideas.

Finally, great interest has been awakened in an engineering approach to shell buckling along similar lines since the publication of El Naschie's first work. This is mainly due to the recent works of Edlund,[3] Fritz and Wittek,[4] and Croll.[5] Perhaps El Naschie could comment on these works and their interrelationship. Such a comparison would help to lessen the confusion arising from the numerous shell buckling theories.

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Reply by Author to P. Kaoulla

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I WOULD like to thank P. Kaoulla for his interest in the work and for his relevant remarks. As for the derivation of

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