Then Eq. (8) shows that

$$r' = 2 + \frac{1}{S^2} - \frac{\mathrm{erf}(S)}{8S^4(St')} \tag{9}$$

Equations (8) and (9) seem particularly simple to use in Eqs. (4) and (1) to give the heat transfer rate to spheres in free molecule flow.

Two other references have recently come to my attention with errors in the recovery factor. One is V. P. Shidlovskiy, *Introduction to Dynamics of Rarefied Gases*, American Elsevier, New York, 1967. On page 41, Eq. (2.52), the factor in front of erfs in the numerator should be divided by 2. The second is S. A. Shaaf," Mechanics of Rarefied Gases," in *Handbuch Der Physik*, Vol. III, No. 2, Springer-Verlag, Berlin, 1963. On page 605, Eq. (7.6), the last factor in the denominator should be multiplied by S^2.

Acknowledgment

The authors' colleague G. E. Caledonia pointed out the error in r' in Ref. 4 and suggested writing this Comment.

References

[1] Hall, D. R., "Hypersonic Free-Molecular Heating of Micron Size Particulate," *AIAA Journal*, Vol. 16, Aug. 1978, pp. 857-859.

[2] Schaaf, S. A. and Talbot, L., "Handbook of Supersonic Aerodynamics," Navord Rept. 1488, Feb. 1959.

[3] Schaaf, S. A. and Chambre, P. L., "Flow of Rarefied Gases," *Fundamentals of Gas Dynamics*, edited by H. E. Emmons, Princeton University Press, Princeton, N.J., 1958, Sec. H.

[4] Schaaf, S. A., "Heat Transfer in Rarefied Gases," *Developments in Heat Transfer*, edited by W. M. Rohsenow, The M.I.T. Press, Cambridge, Mass., 1964, Chap. 7.

[5] Hayes, W. D. and Probstein, R. F., *Hypersonic Flow Theory*, Academic Press, New York, N.Y., 1959, Chap. 10.

[6] Sauer, F. M., "Convective Heat Transfer from Spheres in a Free-Molecule Flow," *Journal of the Aeronautical Sciences*, Vol. 18, May 1951, pp. 353-354.

[7] Oppenheim, A. K., "Generalized Theory of Convective Heat Transfer in Free-Molecule Flow," *Journal of the Aeronautical Sciences*, Vol. 20, Jan. 1953, pp. 49-58.

[8] Stalder, J. and Zurick, V., "Theoretical Characteristics of Bodies in a Free Molecule Flow Field," NASA TN 2423, 1951.

[9] Henderson, C. B., "Reply by Author to M. J. Walsh," *AIAA Journal*, Vol. 15, June 1977, pp. 894-895.

Reply by Author to N.H. Kemp

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THE author is happy that his Technical Note[1] has inspired Dr. Kemp to correct many erroneous printings of the free-molecular heating and drag equations which were published and republished by many people over the last two decades. The error in the recovery factor does, as he implied, have negligible effect on the predicted temperatures of Ref. 1. In particular, for a speed ratio of 13, the recovery used there is ≅0.2% higher than it would be using the correct recovery factor equation of Dr. Kemp. As s becomes bigger the error becomes smaller. On the other hand, if the tests were conducted near speed ratios of 2 to 4 the author would have made obvious temperature mispredictions.

Received Dec. 8, 1978.

Index categories: Supersonic and Hypersonic Flow; Radiatively Coupled Flows and Heat Transfer.

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References

[1] Hall, D. R., "Hypersonic Free-Molecular Heating of Micron Size Particulate," *AIAA Journal*, Vol. 16, Aug. 1978, pp. 857-859.

Rebuttal to Reply by Author to "Comment on 'Flutter of Flat Finite Element Panels in a Supersonic Potential Flow' "

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DR. Yang's criticisms in his Reply[1] to my Comment[2] on his paper[3] were primarily limited to the structural aspects of the problem, and he replied to points most of which were conceded in the Comment. The matter of aerodynamic influence coefficients (AICs), however, deserves a few additional remarks. The AICs, as Revell and I have defined them,[4] constitute an aerodynamic finite element counterpart to the structural displacement method since they both relate forces to displacements. Nelson and Cunningham's work[5] certainly was published before my thesis, but they solved the stability problem by a routine application of the Galerkin method. By 1956, however, the validity of the Galerkin method had been called into question by a number of investigators because of an apparent paradox in analyzing the membrane panel (i.e., a panel with no bending stiffness) which is stable. Hence, the novelty of my thesis was found in a completely different approach, i.e., using both the structural and aerodynamic influence coefficients, which demonstrated the correctness of the Nelson and Cunningham Galerkin solution and contributed to a better understanding of the Galerkin method. Dr. Yang would not concede that my two-dimensional panel AICs preceded his (by 20 years) or that they were based on a more efficient computational algorithm. The point of referencing my thesis, or better yet, the *AGARD Manual on Aeroelasticity*[6] which discussed my AICs, is that this is where the first use of AICs appears in panel flutter analysis and the AICs included the *second order* frequency term.

Dr. Yang depreciates my treatment of the second order term when he replies: "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 so crude." The remark on the crude structural model is, of course, irrelevant to any aerodynamic discussion, but Dr. Yang does not recognize the fact that he did not simply use a trapezoidal rule. He used the trapezoidal rule and then *an additional averaging* between grid points that increased his accuracy to be comparable to my "sophisticated" integration, but at some computational

Received July 1977; revision received Sept. 1978.

Index categories: Aeroelasticity and Hydroelasticity; Structural Dynamics; Supersonic and Hypersonic Flow.

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expense. The fact that he did not find the cost of a Mach Box flutter analysis prohibitive does not mean that the computation is not relatively costly.

However, panel flutter aside, Dr. Yang brings up a more fundamental question in his Reply when he states, "Although Dr. Rodden claimed that his approach was the finite element method, clearly it was not." There seems to be a self-proclaimed group of experts on the Finite Element Method who officiate on what does and what does not constitute a finite element development. When I expressed disappointment at a recent meeting that the subsonic Doublet-Lattice Method[7] was not mentioned in Dr. Pian's AIAA reprints,[8] one of the Group let me know that it was a lattice method and not a finite element method! Since we need some guide on the question, I suggest reference to the late Prof. Martin, who was a coauthor of the paper[9] that Dr. Yang regards as the *first* finite element method. Chapter One of Prof. Martin's last book[10] places the matter in historical context. It is entitled, "A Brief History of the Development of Finite Element Theory," Two of its paragraphs are quoted below:

> "The earliest use of finite elements was probably that of the geometer. More than two thousand years ago, mathematicians were interested in such problems as determining the perimeter and area of a circle. Exact solutions had to await the discovery of the calculus. In the meantime, amazingly accurate results were found by introducing approximate problems employing finite elements. In the case of the circle, it is quite obvious that a regular polygon can be chosen as a substitute problem. The straight line then becomes the finite element. For this simple case, the important characteristic associated with the element is simply its length. In terms of this length it becomes possible to approximate both the circumference and area of the circle; in other words, to estimate the value of π.
>
> "Archimedes, one of the greatest of the early mathematicians, used finite elements for determining the volume of solids. His work brought him to the very threshold of the calculus. As it was, the ultimate fulfillment of his early steps had to await the day of Newton and Leibnitz some twenty centuries later."†

Clearly, then, my "old, obsolete flexibility influence coefficient method," characterized as a Force Method by Argyris[11] (among others‡), qualifies as a finite element method and Dr. Yang's challenge on priorities is historically inaccurate. One of the original elements in NASTRAN[12] is a general element called GENEL and it is specified by the user in terms of its flexibility§ matrix and the rigid body modal matrix relative to the flexible system supports. The NASTRAN user community does not regard it as any older than the beam element (BAR), and its frequency of usage suggests that neither is it obsolete.

Experimental verification deserves final mention in this controversy. Ground vibration tests measure flexibility and *not* stiffness, as can be readily seen from the following equations for restrained systems. The flexibility matrix $[a]$ is:

$$[a] = \sum_{n=1}^{N} (1/\omega_n^2 M_n)\{h_n\}\{h_n\}^T \tag{1}$$

and the stiffness matrix $[k]$ is:

$$[k] = \sum_{n=1}^{N} (\omega_n^2/M_n)[M]\{h_n\}\{h_n\}^T[M] \tag{2}$$

in which ω_n is the frequency of the nth mode, M_n is its generalized mass, $\{h_n\}$ is its vibration mode, $[M]$ is the mass matrix (consistent or not), and N is the number of degrees of freedom in the finite element model. Equation (1) is given, e.g., in Ref. 13, and Eq. (2) was suggested as its stiffness counterpart in Ref. 14. The convergence characteristics of Eq. (1) have been investigated in Ref. 13; Eq. (2) obviously has poorly behaved convergence, i.e., the $(N-1)$ term series is not even a close approximation to the N term result. Hence, in spite of the best efforts of Structural Systems Identifiers to the contrary (see the survey of Flanelly and Berman[15]), the flexibility of a system is more easily identified than its stiffness. In this connection, flexibility methods can never become obsolete.

What is needed in this controversy is more precise language, because there is no single finite element method. Dr. Yang combined the Direct Stiffness Method of Turner et al.[9] with an (expensive) imitation of my force AICs (as distinguished from pressure AICs or velocity potential AICs) to solve the panel flutter problem.¶ The Doublet-Lattice Method was precisely named to show its difference from the steady Vortex-Lattice Method of Falkner,[16] which itself gives a precise description of the aerodynamic finite element idealization employed. (In contrast, we have the NASA Kernel Function Method[17] which tells us nothing about the method inasmuch as any solution to the aerodynamic pressure-downwash integral equation must involve its kernel function.) If we cannot achieve precision in our language, perhaps we should simply associate names with contributions, such as the Argyris Displacement Method, the Turner Displacement Method, the Argyris Force Method, Rodden and Revell AICs, the Guyan Reduction,[18] Watkins' Subsonic Lifting Surface Method, and so on, since words are signs of the things understood. In any case, the distinction between lumped parameters and finite elements is only to be found in the eyes of the beholder.

References

[1] Yang, T. Y., "Reply by Author to W. P. Rodden," *AIAA Journal*, Vol. 15, May 1977, pp. 756-757.

[2] Rodden, W. P., "Comment on 'Flutter of Flat Finite Element Panels in a Supersonic Potential Flow,'" *AIAA Journal*, Vol. 15, May 1977, pp. 755-756.

[3] Yang, T. Y., "Flutter of Flat Finite Element Panels in a Supersonic Potential Flow," *AIAA Journal*, Vol. 13, Nov. 1975, pp. 1502-1507.

[4] Rodden, W. P., and Revell, J. D., "The Status of Unsteady Aerodynamic Influence Coefficients," Fairchild Fund Paper No. FF-33, Institute of the Aeronautical Sciences, Jan. 1962.

[5] Nelson, H. C. and Cunningham, H. J., "Theoretical Investigation of Flutter of Two-Dimensional Flat Panels with One Surface Exposed to Supersonic Potential Flow," NACA TN 3465, July 1955, superseded by NACA TR 1290, 1956.

[6] Johns, D. J., "A Panel Flutter Reivew," *AGARD Manual on Aeroelasticity*, Vol. III, May 1969, Chap. 7.

[7] Albano, E., and Rodden, W. P., "A Doublet-Lattice Method for Calculating Lift Distributions on Oscillating Surfaces in Subsonic Flows," *AIAA Journal*, Vol. 7, Feb. 1969, pp. 279-285, and Errata, Nov. 1969, p. 2192.

[8] Pian, T.H.H., ed., *Finite Element Methods in Structural Dynamics*, AIAA Selected Reprint Series, Vol. XVII, 1976.

[9] Turner, M. J., Clough, R. W., Martin, H. C., and Topp, L. J., "Stiffness and Deflection Analysis of Complex Structures," *Journal of the Aeronautical Sciences*, Vol. 23, Sept. 1956, pp. 805-823.

[10] Martin, H. C. and Carey, G. F., *Introduction to Finite Element Analysis*, McGraw-Hill Book Co., 1975.

[11] Argyris, J. H., "The Impact of the Digital Computer on Engineering Sciences," Twelfth Lanchester Memorial Lecture, *The Aeronautical Journal*, Vol. 74, Pt. 1, Jan. 1960, pp. 13-41, Pt. 2, Feb. 1970, pp. 111-127.

[12] MacNeal, R. H., ed., *NASTRAN Theoretical Manual*, NASA SP-221(01), April 1972.

‡Argyris' Lanchester Lecture is cited because it also contains a discussion of aeroelastic analyses by influence coefficient methods.

§Current versions permit specification of a restrained stiffness matrix as an alternative user input.

¶Dr. Yang's calculated results were correct, as could have been anticipated, but the curves faired through his data points were not correct at low supersonic Mach number. This point was emphasized in my Comment but was ignored in Dr. Yang's Reply. The incorrect curves might be regarded as "a new solution."

[13] Rodden, W. P., "A Method for Deriving Structural Influence Coefficients from Ground Vibration Tests," *AIAA Journal*, Vol. 5, May 1967, pp. 991-1000.

[14] Natke, H. G., private correspondence with W. P. Rodden, July 11, 1966.

[15] Flanelly, W. G., and Berman, A., "The State of the Art of System Identification of Aerospace Structures," *System Identification of Vibrating Structures*, Reprints of Papers presented at ASME Winter Meeting, 1972, pp. 121-131.

[16] Falkner, V. M., "The Calculation of Aerodynamic Loading on Surfaces of any Shape," R&M 1910, British Aeronautical Research Council, 1943.

[17] Watkins, C. E., Woolston, D. S., and Cunningham, H. J., "A Systematic Kernel Function Procedure for Determining Aerodynamic Forces on Oscillating or Steady Finite Wings at Subsonic Speeds," NASA R-48, 1959.

[18] Guyan, R. J., "Reduction of Stiffness and Mass Matrices," *AIAA Journal*, Vol. 3, Feb. 1965, p. 380.

Reply by Author to Rebuttal by W. P. Rodden

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IN Ref. 1, this author combined a numerical method similar to that by Nelson and Cunningham[2] for computing an aerodynamic matrix with a finite element consistent mass formulation for flutter analysis of panels under initial stresses. Nelson and Cunningham's results were used for comparison. Such results were published earlier than the identical results given in Dr. Rodden's thesis. Although Nelson and Cunningham did not use the terminology AIC, they indeed used *aerodynamic matrices* [C_{mn} as defined in Eq. (19)] with amplitudes (generalized coordinates) of modes as degrees of freedom. This author regrets that he does not feel he should reference Dr. Rodden's thesis. Bolotin[3] also only referenced Nelson and Cunningham's work and did not mention Dr. Rodden's thesis. It appears to be an international practice that when two solutions are identical, it is only necessary to reference the earlier one.

The widespread use of finite element method is the by-product of computers. This author finds it difficult to be excited about what went on more than 2000 years before the computer generation.

Dr. Rodden challenges the words "the kind of old, obsolete flexibility influence coefficient method he used" and thus, once again, finds a chance to tell a long story about his past work with the implication that his structual beam model without the slope degrees freedom is not obviously inferior when used in the type of panel flutter problems dealt with in Ref. 1. All this author can do is to suggest that Dr. Rodden include the slopes in his structural beam model next time.

In Figs. 5 and 8 of Ref. 1 the curves at low M are connected through the correct data computed at $M=1.3$, $\sqrt{2}$, 1.56. Had data been computed for $M<1.3$ and $1.3<M<\sqrt{2}$, the curves connecting these points would have been drawn slightly differently. Therefore, Dr. Rodden's comment that the shape is incorrect is not pertinent to those data computed and presented.

The rest of the comments by Dr. Rodden do not appear to be relevant to the research being discussed. Since this author has a large number of tasks confronting him, he regrets that he can hardly afford the time to help Dr. Rodden achieve the credit he feels is due him. Nor can this author afford the time to participate, by commenting, in Dr. Rodden's earlier disagreements with "one of the self-proclaimed group of experts" that he brings up in his lengthy comments.

References

[1] Yang, T. Y., "Flutter of Flat Finite Element Panels in a Supersonic Potential Flow," *AIAA Journal*, Vol. 13, Nov. 1975, pp. 1502-1507.

[2] Nelson, H. C., and Cunningham, H. J., NACA TR 1280, 1956.

[3] Bolotin, V. V., *Nonconservative Problems of the Theory of Elastic Stability*, edited by G. Herrmánn, translated by T. K. Lusher, MacMillan Co., New York, N.Y., 1963, pp. 200, 231, 235.

Received March 7, 1978; revision received Oct. 2, 1978.

Index categories: Aeroelasticity and Hydroelasticity; Structural Dynamics; Supersonic and Hypersonic Flow.

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