should be recognized that the correlation requires satisfying, with least-square errors, $I(I - 1)/2$ simultaneous relations for I coefficients, not an isolated equation such as that examined in the comment.

Professor Libby questions "the need for the approximation when detailed calculations of boundary-layer flows involving multicomponent diffusion are being performed." It would seem that the primary consideration here is the tradeoff between accuracy and computational conveniences (in particular, speed, storage, and input requirements). Although not having performed large-scale boundary-layer computations utilizing the approach suggested by Professor Libby, our experience to date leads us to believe that substantial computation convenience is achieved with little loss of accuracy. The boundary-layer equations proposed in our paper have recently been programmed[2] and solutions have been obtained considering 30 molecular species.[3] Philco 212 computer time required for a nonsimilar boundary-layer solution over an ablating body in this case was 6 min. This included the evaluation of edge conditions, a similar solution at the stagnation point, and nonsimilar solutions at 19 additional stations. However, as pointed out in our paper, the primary motivation for introducing the approximation was to develop a transfer-coefficient model to be used in lieu of large-scale boundary-layer computations. In constructing a film coefficient model considering unequal diffusion effects, a convenient alternative approach of equivalent accuracy does not exist.

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Comment on "Dynamics of a Radiating Gas with Application to Flow Over a Wavy Wall"

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IN his excellent exposition of the application of the spherical-harmonic approximation to the radiation-transport equation, Cheng found [Ref. 1, Eq. (6.11)] the velocity potential for the linearized flow of a nonscattering grey gas over a small-amplitude wavy wall to be

$$\varphi(x_1,x_2) = \frac{U_\infty l}{2\pi} Re\left\{\sum_{j=1}^{2} A_j \exp\left[\frac{2\pi}{l}(c_jx_2 + ix_1)\right]\right\}$$

$$= U_\infty\epsilon \sum_{j=1}^{2} e^{-2\pi\delta_j(x_2/l)}\left[a_j \cos 2\pi\left(\frac{x_1 - \lambda_j x_2}{l}\right) - b_j \sin 2\pi\left(\frac{x_1 - \lambda_j x_2}{l}\right)\right]$$

where the wall shape was given by

$$X_2 = \epsilon \sin 2\pi x_1/l$$

and A_j, c_j, δ_j, λ_j, a_j, and b_j could be found from his Eqs. (6.8–6.10).

From this velocity potential, it should follow that the normal velocity perturbation may be written as

$$\frac{u_2'}{U_\infty} = \frac{1}{U_\infty}\frac{\partial\varphi}{\partial x_2} = -\frac{2\pi\epsilon}{l}\sum_{j=1}^{2} e^{-2\pi\delta_j(x_2/l)} \times \left\{(a_j\delta_j - b_j\lambda_j)\cos\frac{2\pi}{l}(x_1 - \lambda_j x_2) - (a_j\lambda_j + b_j\delta_j)\sin\frac{2\pi}{l}(x_1 - \lambda_j x_2)\right\}$$

However, Cheng, in writing this expression in his Eq. (6.13b), included only the first term; the second, sine, term was omitted. In addition, through a typographical error, the coefficient of the cosine term was incorrectly given as $(a_j\delta_j + b_j\lambda_j)$.

It should be noted that where $x_2 = 0$, the coefficient of the sine term is identically zero. This is a consequence of Cheng's Eq. (6.9):

$$\sum_{j=1}^{2} A_j c_j = 2\pi\frac{\epsilon}{l}$$

Thus, on the wall surface, the omitted term equals zero. However, since Cheng's solution should be valid throughout the flow, the omitted sine term must be included in any calculation of u_2' for x_2 not identically zero.

Reference

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Comment on "Computation of Stress Resultants from the Element Stiffness Matrices"

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THE general form of Eq. (1), Ref. 1, to take into account not only continuous distributed loads, but also discontinuous and concentrated loads applied outside the nodes, was presented in a previous work.[2] The partitioning of Eq. (5) of Ref. 2 relative to the structural elements of the structural system results, for each element, in an equation exactly like Eq. (3), Ref. 1 (with the exception of the inertia term which was not considered in Ref. 2). The generalized forces $-\{Q_i\}$[1] are precisely the component submatrices of [s] [Eq. (5) of Ref. 2]. The generalized forces $-\{Q_i\}$ of the second equation of the example are simply the fixed-end bending moments and shear forces of the elements. This concept was already introduced in Refs. 2 and 3.

From the final results of the example and the final paragraph of the Note a misleading conclusion may be drawn that the method discussed is approximate. In reality the method is theoretically an exact one as the generalized displacements are obtained from the equilibrium equations relative to the nodes, and these are exactly satisfied (except for computational er-

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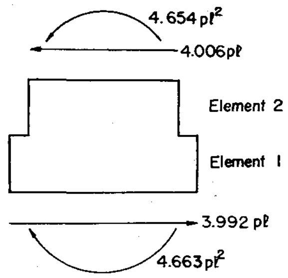

Fig. 1 Equilibrium of the node.

rors); i.e., there are no stress resultant discontinuities at the nodes. This is true for the structural system of the example or for any other system.

Parenthetically, it is worthwhile to mention that the stiffness matrix and the generalized forces of the structural elements of the example are exact since the third-order polynomial is the exact deflection pattern for a straight flexural beam element with constant (EI). This is not the case when approximate deflection patterns are used as, for example, in the work on shells of revolution by the Massachusetts Institute of Technology group.

Consider the node joining elements 1 and 2 of the example (Fig. 1) with the stress resultants taken from columns 1 and 2 of the last equation of the Note. The discontinuities in the bending moments ($0.009pl^2$) and in shear forces ($0.014pl$) are exclusively due to computational errors. Theoretically there should not be such discontinuities since this situation corresponds precisely to the satisfaction of the node equilibrium conditions. The writer worked out this example by a standard matrix structural analysis FORTRAN program. It was found that the stress resultants, from each side of the nodes, agreed with the exact solution with five significant figures.

In conclusion, contrary to the assertion of the Note that "although this example is too simple to draw general conclusions from . . .," it can be conclusively asserted that the method is an exact one even for shells of revolution. For this type of structural system the only approximation is due to the use of approximate deformation functions for the shell elements. However this does not cause any stress resultant discontinuities at the nodes.

References

[1] Stricklin, J. A., "Computation of Stress Resultants from the Element Stiffness Matrices," *AIAA Journal*, Vol. 4, No. 6, June 1966, pp. 1095–1096.

[2] Filho, F. V., "Matrix Analysis of Plane Rigid Frames," *Transactions of the American Society of Civil Engineers*, Vol. 126-II, 1961, pp. 214–227.

[3] Filho, F. V., "Discussion on Generalized Displacements in Structural Analysis, by J. L. Meek," *Journal of the Structural Division, Proceedings of the American Society of Civil Engineers*, Vol. 88, ST6, 1963, pp. 303–316.

Reply to F. V. Filho

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PROFESSOR Filho has studied my technical note in detail and retained more significant figures in his calculations. Although the equations used in my technical note[1] and Ref. 2 of Professor Filho's comments are the same, our objectives were quite different. In particular my objective was to obtain the stress resultants after computation of the generalized displacements. My assertion about not being able to draw general conclusions from a simple example has been verified by more recent studies on shells of revolution.[2] It was found that my method yields good results when the geometry of the shell is accurately represented but yields poor results when the shell is represented by conical segments. Based on this more recent data we can say that the method is theoretically correct but sensitive to geometric approximation.

The statement "since the third order polynomial is the exact deflection pattern for a straight flexural beam element with constant (EI)," is not correct. For example, the exact deflection pattern for a uniform beam under a uniform loading is a fourth-order polynomial. In summary, we used the same equations to reach different objectives. Otherwise, I believe the limitation as stated in Ref. 1 should remain.

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Torque on a Satellite in a General Gravitational Field

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SCHLEGEL[1] has properly pointed out that I was careless (he was kind enough not to use the word) in an earlier derivation of the gravitational torque on a satellite in a noninverse square field.† He provides a correct development and, in particular, gives forms for the body components of torque in the neighborhood of the oblate earth. His results are expressed partly in terms of body orientation parametrized by Euler angles, though portions of the equations are simplified considerably by his leaving them in terms of the direction cosines, which he denotes by $m_{\alpha\beta}$ $(\alpha,\beta = 1,2,3)$.

In this Note, I would like to give an alternative derivation which is somewhat more compact and is more straightforward, in my opinion, in regard to the development of the torque components from the potential. It leads to a compact matrix form for the result that is more general in several particulars; a form that also is extremely convenient for the numerical computation of these torques for the purposes of digital simulation.

Denote by $U(\mathbf{R})$ the specific potential at the center of mass of a material system due to the gravitational field in which it is immersed, $\mathbf{R}$ locating this center of mass with respect to the center of the earth or other central body. Denote by $U(\mathbf{R} + \rho)$ the specific potential at a nearby point. Integrating the latter over the material system and using a well-known expansion from vector analysis, valid when the gradient operator is expressed in rectangular coordinates, the spe-

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† Several others have pointed out to me the fallacy of my original development, and I wish to take this opportunity to express to them my genuine appreciation.