where

$$I_z = \iint x^2\,dx\,dz, \quad I_x = \iint z^2\,dx\,dz, \quad A = \iint dx\,dz$$

and

$$\iint x\,dx\,dz = \iint y\,dx\,dz = \iint xy\,dx\,dz = 0$$

Substitution of Eqs. (1–3) into Eq. (6) of Ref. 1 yields

$$T = \frac{1}{2}\int_0^l \{\rho(\dot{\xi}^2+\dot{\zeta}^2+\dot{\eta}^2)+\mu' I_z \dot{\xi}_y{}^2+\mu' I_x \dot{\eta}_y{}^2\}\,dy + (\Omega^2/2)\int_0^l \left[\rho\left\{\xi^2+2y\dot{\zeta}-y\int_0^y \{(\xi_y{}^2(y',t)+\eta_y{}^2(y',t)\}\,dy'\right\}+ \mu' I_z \xi_y{}^2+\mu' I_x \eta_y{}^2\right]dy+\Omega\int_0^l(\rho\xi\dot{\zeta}-\mu' I_z\dot{\zeta}_y)\,dy+ \Omega^2\mu'(I_z+l^3/3)/2 \quad (7)$$

where $\rho = \mu' A$. Equation (7) may be simplified at this point: first, by making the customary assumption that effects of rotary inertia may be ignored (i.e., $I_z = I_x = 0$); and next, by assuming that the neutral line is inextensible which then implies that all terms in ζ and its derivatives may be ignored. Further it may be shown via integration by parts (in a manner similar to that in Ref. 3) that,

$$\int_0^l y\left\{\int_0^y f(y')\,dy'\right\}dy = \frac{1}{2}\int_0^l (l^2-y^2)f(y)\,dy \quad (8)$$

With Eq. (8) and the above simplifications, Eq. (7) becomes

$$T = \frac{1}{2}\int_0^l \{\rho(\dot{\xi}^2+\dot{\eta}^2)-\tfrac{1}{2}\rho\Omega^2(l^2-y^2)(\xi_y{}^2+\eta_y{}^2)+\rho\Omega^2\xi^2\}\,dy + \Omega^2\mu'\{I_z+l^3/3\}/2 \quad (9)$$

Equations (6) and (9) may then be combined with Eq. (1) of Ref. 1, to construct motion equations. Routine execution of the variation operation, and integration by parts, leads to

$$EI_z\xi_{yyyy}-\tfrac{1}{2}\rho\Omega^2\{(l^2-y^2)\xi_y\}_y-\rho\Omega^2\xi+\rho\ddot{\xi}=0 \quad (10)$$

$$EI_x\eta_{yyyy}-\tfrac{1}{2}\rho\Omega^2\{(l^2-y^2)\eta_y\}_y+\rho\ddot{\eta}=0 \quad (11)$$

Equations (10) and (11) are in accord with Eq. (50) of Ref. 1, and corresponding equations derived by other means in Ref. 4. Further, the concepts underlying Eqs. (1–6) are in accord with established results.[5,6]

In comparing the above derivation with that of Ref. 1, the following points may be noted. The key difference lies in the assumed form for $v(x, y, z, t)$. The required "steady-state solutions about which to linearize oscillatory deformations" of Ref. 1 are avoided herein. The terms in Eqs. (10) and (11) contributing centrifugal stiffening arise quite naturally from the kinetic energy expression. There is no need to resort to the concept of an effective applied load, $P(y)$, or any other artifice. The "cascade of approximations" referred to in Ref. 1 is systematized to a significant extent via a consideration of quadratic-order terms in T and V.

I am not in accord with the author's conclusion that, "except for a small class of very special cases, the continuum mechanics model is barren of useful results," particularly in view of the above discussion. Continuum mechanics techniques similar to those of Ref. 4 or above for handling elastic beams, thin-wall sections, shells, membranes, trusses, etc., are applicable and useful in this class of problems. Further, it is my view that spacecraft will, in the future, be designed with deliberate efforts to ensure structural simplicity in order to maximize reliability, to the point where continuum modeling will be applicable to an appreciable extent.

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Reply by Author to F. R. Vigneron

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We appreciate Dr. Vigneron's thoughtful comments and constructive criticisms. We agree that he has provided an alternative path to our radial beam result, and that his derivation has the important advantage of providing a more systematic treatment for the beam. We accept his contention that there are other special cases of practical interest, such as simple membranes, plates, and perhaps trusses, for which derivations parallel to his radial beam derivation could be devised.

However, we wish to extend the dialogue with Dr. Vigneron on two specific issues. First, we regret that we gave him the impression that we resort to the concept of an effective applied load as an integral part of our derivation; we left him (and perhaps others) with this impression because in our desire to conform to journal standards of brevity we made a comparison of our results with an established (Meirovitch) textbook treatment in the middle of the textbook derivation, rather than at the true end point established by the equations of motion. The textbook uses the "effective load" concept, and thereby bypasses the use of nonlinear strain-displacement equations (which are used by Dr. Vigneron and in our paper). The essential point of this section of our paper was to illustrate that *without* the distasteful "effective load" concept one must resort to nonlinear strain displacement equations to obtain linear equations of vibration. Once we obtained expressions for kinetic and potential energies without the use of an effective load [see our Eqs. (39) and (45)], we could certainly have solved for the nominal extension v_o and applied the rules of variational calculus to obtain equations of motion from our Eq. (1). This is what we did originally, but in writing the paper we truncated the development by comparing our potential energy in Eq. (45) to that presented in terms of effective load in the Meirovitch text; this made it permissible to record the final equations [Eq. (50)] without further derivation. On this point we hope Dr. Vigneron will accept our rebuttal.

The second issue we must raise will probably remain controversial; this is the role of the "steady-state solutions about which to linearize oscillatory deformations," which is at the heart of our method in the original paper, but which is not employed by Dr. Vigneron. It's important to remember that in our paper we advanced this method as an approach to the most general

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continuum, and not as the ideal approach for the radial beam; Dr. Vigneron's method is both more elegant and more efficient than ours for the beam, and the wholly linear derivation in the referenced text by Meirovitch is much simpler than either of these options. We used the radial beam in our paper merely as an example. We fail to see in Dr. Vigneron's Comment a general method, applicable to any continuum; his crucial equations (1–3) make sense to us only for the beam. Until we learn more about the problem, we must retain the impression that for the general problem of the rotating elastic continuum one must first find a steady-state deformation and then linearize about this solution.

Now we come to the final paragraph of Dr. Vigneron's Comment in which he expresses a personal opinion that seems to be at variance with our own. We suggest to the reader that he avoid sweeping conclusions concerning the relative utility of continuum models (characterized by partial differential equations) and discretized models (characterized by ordinary differential equations). It is certainly true that there exists a class of problems for which continuum models are superior; whether this is "a small class of very special cases" or not depends on the context in which an individual is operating, and the reader must judge for himself. He must remember, however, that Dr. Vigneron's Comment deals only with the question of how you obtain the partial differential equations of continuum vibration; he doesn't address the problem of extracting useful information from these linear but generally variable-coefficient equations. Having recently labored with a very good student over the modal analysis problem for the relatively simple cases of the rotating radial beam[1,2] and the rotating circular thin plate,[2] at least one of the three authors of our original paper must cling to his belief that the partial differential equations of rotating elastic continua are more difficult to analyze than the corresponding equations for a finite element model.

References

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