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Readers' Forum

Brief discussion of previous investigations in the aerospace sciences and technical comments on papers published in the AIAA Journal *are presented in this special department. Entries must be restricted to a maximum of 1000 words, or the equivalent of one* Journal *page including formulas and figures. A discussion will be published as quickly as possible after receipt of the manuscript. Neither the AIAA nor its editors are responsible for the opinions expressed by the correspondents. Authors will be invited to reply promptly.*

Comment on "Flexural Behavior of a Rotating Sandwich Tapered Beam" and on "Dynamic Analysis for Free Vibrations of Rotating Sandwich Tapered Beams"

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ACCURATE determination of the vibration characteristics of rotating beams is very important for the design of helicopter blades, wind turbine blades, and many other systems. The subject papers by Ko[1,2] attempt to analyze the vibration characteristics of tapered, rotating beams with sandwich construction. Although most engineering applications do not use beams of the construction treated in these papers, it is reasonable to develop such idealized models. Such developments are usually undertaken for at least two reasons. First, one usually hopes to obtain insight into the effects of certain assumptions (say, Euler–Bernoulli vs Timoshenko-type models) which will eventually lead to the identification of the necessary features of more general-purpose analyses. Second, with simplified, idealized models one can easily study qualitative behavior associated with certain design features (say, the effects of the core stiffness on the natural frequencies).

In the subject papers,[1,2] partial differential equations of motion are derived and solved. These equations were intended to predict the behavior of both axial and transverse (out of the plane of rotation) motions due to stretching, bending, and transverse shear. Numerical solutions were obtained by finite difference analysis, but the claim in Refs. 1 and 2 of closed-form solutions led the author of this Comment to look more deeply at the work. Although there are several errors in these works, in this Comment only four will be discussed.

Centrifugal Stiffening Effect

The most serious error in Refs. 1 and 2 is the absence of the centrifugal stiffening term from the partial differential equation for free vibration of a rotating beam out of its plane of rotation. From the very earliest work on rotating beams, and now after decades of research, it has been universally recognized that the dominant effect of rotation is contained in the centrifugal stiffening term. This term's importance is not debatable.

Just for simplicity, let us write this equation for a beam made of homogeneous, isotropic material, rotating about an axis fixed in inertial space. This equation can be found in any textbook which treats rotating beams (e.g., Refs. 3 and 4) and is given by

$$(EIw_0'')'' - (Tw_0')' + m\ddot{w}_0 = 0 \tag{1}$$

where w_0 denotes displacement of the beam centroidal axis out of the plane of rotation, m is the mass per unit length, EI denotes the bending rigidity, T denotes the axial force in the beam due to its rotation, $()'$ denotes the partial derivative with respect to the axial coordinate x, and $(\dot{\ })$ represents the partial derivative with respect to time. For small strain T is independent of the deformation and can be obtained from the axial force equilibrium equation by spatial integration of

$$T' = -m\Omega^2 x \tag{2}$$

where Ω is the angular speed of the frame to which the beam is attached. For a beam of length L with x measured from the center of rotation, the resulting expression for T, with m constant, is

$$\begin{aligned} T &= \Omega^2 \int_x^L mx\,\mathrm{d}x \\ &= (m\Omega^2/2)(L^2 - x^2) \end{aligned} \tag{3}$$

Much has been written over the years about the derivation and effects of the variable-coefficient term $-(Tw_0')'$ in Eq. (1). The following facts about this term have been established over decades of research.

1) Without this dominating term, accurate prediction of the free-vibration characteristics of rotating beams is impossible, as confirmed by years of experience in the helicopter industry and an abundance of experimental evidence. Some of the references cited by Ko make this point quite clearly.

2) The presence of this variable-coefficient term precludes a closed-form solution of the form offered in Ref. 2, which is appropriate only for constant-coefficient equations. Rather, only series solutions of the Frobenius type have been found, apparently first offered by Refs. 5 and 6, the latter of which was cited by Ref. 2. A simple comparison of results from a special case of the analysis of Ref. 2 with the exact solutions is sufficient to show that the analysis of Ref. 2 is in error.

3) This term guarantees that the free-vibration frequencies associated with out-of-plane (flapping) motion of an outwardly directed beam will increase as a function of Ω and will never be less than Ω, for which there is, again, an abundance of experimental evidence. Obviously, then, the results in Tables 1 and 2 of Ref. 2 are wrong and, in fact, meaningless. Along these same lines, Ref. 2 alleges the existence of instabilities, involving the fundamental frequency of transverse vibration going to zero, "which have never been reported in [the] literature." These alleged instabilities are not noted in the literature because they do not exist, since the fundamental free-vibration frequency cannot go to zero for outwardly directed rotating beams, see Refs. 7 and 8.

4) The derivation of this term must be handled in a special way. It does not fall out from straightforward linear analysis. One can capture this important term in the linear equations in three known ways—all of which have a sort of nonlinear "flavor" to them. a) Derive the equations of motion using at least the linear and quadratic terms in a set of nonlinear strain-displacement relations, and linearize the resulting equations about a nontrivial equilibrium configuration. Solve the axial equilibrium equation for the axial force in terms of an integral over the outboard portion of the beam of certain inertial terms [see Eq. (3a)]. The resulting linearized equation for transverse vibration is a linear equation identical to that which one would obtain from a purely linear analysis plus a linear term

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involving the axial force, which cannot be obtained from a purely linear analysis. This method is used by Ref. 9. b) Break up the axial displacement variable into two parts—one due to stretching and one due to bending. Then, use at least the linear and quadratic terms in the displacement field to calculate the energy and virtual work of applied loads. This resulting equation, when linearized, will contain the same axial force term as one obtains when applying method 1. This method is used by Ref. 10. c) Introduce the centrifugal force as a separate applied force, the virtual work of which must be calculated based on the deformed beam configuration. This method is used by Ref. 3.

Total Section Rotation Variable

Another serious error in Refs. 1 and 2 has to do with the kinematical boundary conditions. These boundary conditions will affect the entire derivation through the variational procedure. One cannot simply "turn the crank" with variational methodology without understanding and applying basic kinematics. The confusion in the subject papers seems to stem from the faulty schematic of the deformation in Fig. 1 of Ref. 1 and concomitant errors in the interpretation of the angle θ and its relationship to the curvature.

Before reading further as the details of these errors are discussed, the reader may want to consult a correct presentation of Timoshenko beam theory, with detailed treatment of the section rotation and kinematical boundary conditions. Such treatments can be found in many places, for example, see Ref. 11. A detailed comparison with this work shows beyond doubt that the subject papers have confused certain kinematical quantities related to the section rotation.

Since the assumed displacement field is an important part of the derivation of beam equations, let us apply elementary vectorial mechanics to obtain the longitudinal component of the sectional displacement u. Using Ko's notation and sign convention and letting plane sections remains plane, one can express u as a displacement at the reference line u_0 minus the total sectional rotation times a distance y. This is seen in Eq. (16) of Ref. 1:

$$u = u_0 - y\left(\psi + \frac{\partial w_0}{\partial x}\right) \tag{4}$$

(It is far easier to extract some meaning from this simple equation than to comment on all the conceptual errors in Sec. II of Ref. 1.) Clearly, the total section rotation, which is denoted here as θ, can be identified from this equation as

$$\theta = \psi + \frac{\partial w_0}{\partial x} \tag{5}$$

Note that ψ is not the total section rotation; rather, ψ is the part of the section rotation due to transverse shear strain, serving as the transverse shear strain measure for Timoshenko beam theory, see Eq. (19) of Ref. 1.

In accordance with Timoshenko beam theory, only the total section rotation θ can be kinematically restrained at an end. The transverse shear strain measure, ψ, however, cannot be kinematically set at a boundary; to set ψ at a boundary requires some statement about stress, beyond the scope of kinematics. Thus, the proper boundary conditions at the root (the cantilevered end) are

$$u_0 = w_0 = \psi + \frac{\partial w_0}{\partial x} = 0 \tag{6}$$

which is correct. Proper mode shapes for cantilevered Timoshenko beams, then, have nonzero ψ and $\partial w_0/\partial x$ at the root.

However, boundary conditions in the subject papers at the root are

$$u_0 = w_0 = \frac{\partial w_0}{\partial x} = \psi = 0 \tag{7}$$

which is incorrect.[1,2] Thus, the kinematic boundary conditions of Refs. 1 and 2 at the beam root overconstrain the section at the root by prescribing zero displacement (w_0), zero slope of the reference line $\partial w_0/\partial x$, and zero shear (ψ). In contrast to the erroneous Eqs. (43) and (49) in Ref. 1. and Eq. (4) in Ref. 2, ψ cannot be zero because the shear force is not zero at the root. Furthermore, from elementary considerations, the number of boundary conditions is limited to six for this problem; however, the subject papers have imposed seven, four at the root and three at the tip. It seems that old errors continue to be repeated.[12]

Axial Instability

Reference 1 discusses an instability related to axial deformation. Equation (81) of Ref. 1 (repeated here for convenience)

$$u_0 = \frac{\sin \alpha x}{\alpha \cos \alpha L} - x \tag{8}$$

blows up when $\alpha L = \Omega\sqrt{\rho L^2/E}$ reaches the lowest critical value $\pi/2$. However, this equation is supposedly derived based on Hooke's law, in which the strain is small compared to unity. Thus, its validity is severely limited. For small strain $u_0' \ll 1$, and a simple perturbation analysis reveals that

$$u_0' = \frac{\cos \alpha x}{\cos \alpha L} - 1 \approx \frac{\alpha^2}{2}(L^2 - x^2) + \cdots \tag{9}$$

Thus, the maximum strain is $\mathcal{O}(\alpha^2 L^2/2) \ll 1$, and as Ω is increased from zero, which in turn increases αL, Hooke's law ceases to be valid long before the supposed instability at $\alpha L = \pi/2$ ever comes into play. In other words, the theory predicts instability in a regime where the theory is not valid.

The behavior of such a system when the strain is anything other than small compared to unity depends on physical nonlinearities, as pointed out by Brunelle.[13] For homogeneous, isotropic, prismatic rods with $u_0' > 0$ one can postulate a strain energy function of the form

$$U = \int_0^L EA\left(\frac{1}{2}u_0'^2 + \frac{\beta}{3}u_0'^3\right)\mathrm{d}x \tag{10}$$

where β is a nondimensional constant which depends on the material and section geometry. (To determine β one would most likely need to resort to an experiment, since higher order elastic constants are not readily available for most materials.) Assuming this strain energy function to be valid at sufficiently large values of strain, one can show that whether or not an instability of the type discussed by Ref. 1 exists depends on the value of β. If β is sufficiently large, no instability is possible; thus, the very existence of the instability cannot be ascertained on the basis of a linear theory.[14]

Dissipative Forces

Reference 1 states that "the variational principle" is being applied, but it is also claimed that dissipative forces are present. As is well known, this is impossible. An extended form of Hamilton's principle (essentially a time integration of the principle of virtual work) is really being used, which is not a variational principle. This is a relatively minor point.

The major problem concerning the virtual work of dissipative forces in Ref. 1 is that the forces depend on the inertial velocity. This is quite an unrealistic situation. If Ko were attempting to account for the beam moving through a fluid, he would need to apply an aerodynamic theory. This was obviously not intended. On the other hand, if he was trying to account for material damping, then the terms involving Ω in Eq. (36) of Ref. 1 are obviously incorrect, because they exhibit damping forces in the absence of dynamic deformation! By elementary physical arguments, material damping forces must be zero for static deformation.

Closure

It should be clear that the subject papers[1,2] contain errors of nontrivial significance. It is hoped that this discussion will motivate young researchers to carefully examine the claims of published papers and veteran researchers to review submitted papers more rigorously.

Acknowledgment

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Reply by the Author to Dewey H. Hodges

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I WOULD like to thank Hodges for his comments on my work, which was presented in Refs. 1 and 2. Hodges considered four items relating to these analyses to be significant errors and indicated that these papers should not have been published. Therefore, I would like to discuss these four issues from a different point of view.

Centrifugal Stiffening Effect

Hodges indicated that governing equations derived in Refs. 1 and 2 were in serious error because the centrifugal stiffening term was absent from those equations. His argument was primarily based on the assumption that the governing equation [Eq. (1) of Hodges' Comment] for a rotating homogeneous beam found in many textbooks, such as Refs. 3 and 4, was absolutely correct. In fact, this particular equation was derived by using the assumption of zero axial deformation and by using a free body of a beam element which was deformed into an arbitrarily sheared element without rotation and curvature as shown in Fig. 9.2 of Ref. 3. Since the bending-curvature relationship used for this derivation was based on Euler–Bernoulli's beam theory, using an arbitrarily sheared element as the free body was actually incorrect because it violated the basic assumption of the theory. As was stated and discussed by Fletcher,[5] the theory assumes that "sections initially planar and perpendicular to the neutral axis remain planar and perpendicular to the deformed neutral axis." A beam element which is consistent with this assumption can only be deformed into a circular differential section with curvature and with rotation but without shear strains. If one uses such an element as the free body to derive equilibrium conditions, governing equations for both the axial and the transverse vibrations will become nonlinear. In addition, terms due to the gravitational force and the static displacement will appear in these equations. Furthermore, the governing equations for the steady-state behavior also will be nonlinear. These complex results also will happen to governing equations for the vibration of nonrotating beams if the same free body is used for the derivation. If both the steady-state and the dynamic nonlinear equations are linearized, the linearization process will imply that both the angle of rotation and the curvature of the element should be neglected. If these two entities are specified to be zero, this deformed beam element will reduce to the same nondeformed free body as those widely utilized in linear mechanics, especially those used for deriving governing equations of nonrotating beams. The linear governing equations for the vibration of rotating beams derived in Refs. 1 and 2 actually are consistent with those derived by applying Newton's mechanics to such a nondeformed free body.

It can be shown that results reported in Ref. 2 were consistent with those obtained by solving governing equations derived by applying Newton's mechanics to a nondeformed free body. The natural frequencies of the beam decrease with increasing rotating speeds for both the axial and the transverse vibrations. If one uses a free body which is consistent with assumptions of Euler–Bernoulli's beam theory and linearizes all governing equations by invoking conflicting assumptions as suggested by Hodges, one can show that the natural frequencies for both the axial and the transverse vibrations are coupled. In addition, frequencies of these coupled modes dominated by the axial vibration can be determined to decrease with increasing rotating speeds. For those modes dominated by the transverse vibration, natural frequencies can be shown to increase with increasing rotating speeds; however, this phenomenon is primarily due to retaining those linear terms which were assumed to be negligibly small in the linearization process. Reference 2 was intended to report results based on a rational analysis consistent with the classical beam theory. If experimental results indicate a completely different behavior for a rotating beam, these results can mean that the classical beam theory is not accurate enough for analyzing the vibration problem of a rotating beam. Regarding the invocation of conflicting assumptions in linearizing the nonlinear equations or using a deformed shape not consistent with the basic assumptions of the classical beam theory as the free body in order to predict experimental results, these approaches can be more irrational mathematically than using the classical linear beam theory. The proper method for this problem might be to solve the complete nonlinear differential equations, such as those derived by using a deformed element with curvature and rotation. However, it is also well known that the nonlinear response of a vibrating structure can be completely different in nature from its linear oscillatory behavior. The major difference lies in the dependency of the natural frequencies on the amplitude of the structure undergoing the nonlinear vibration.

Hodges indicated that there were three proper ways to solve the vibration of rotating beams. In fact, I have found all three methods involve invoking irrational assumptions in their analyses. Therefore, I would like to describe my view of each approach in the same order as they appeared in Hodges' Comment.

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