$$+ 173537.28\beta + 7078579.2\} + \{3386.88\beta^3 - 101606.4\beta^2 + 3657830.4\beta - 85349376\} = 0.$$

where $\chi = ml^4\omega^2/EI$ and $\beta = Pl^2/EI$.

For comparison, the same three terms of the power series yields the following characteristic equation for the stationary solution to the *conservative* cantilever beam-column

$$\chi^3 - \chi^2\{218.4\beta + 14464.8\} + \chi\{5443.2\beta^2 + 630766.08\beta + 7078579.2\} - \{13547.51995\beta^3 + 1828915.2\beta^2 + 39016858.05\beta + 85349376\} = 0.$$

From these simple and simply obtained three-term solutions, one finds for $\beta = -2.0$, 0.0, +2.0, respectively, χ_0 (nonconservative) = 14.27, 12.37, 10.79, respectively, and χ_0 (conservative) = 2.502, 12.37, 21.23, respectively. Hence, the behavior of the nonconservative system is seen to be entirely different to that of the conservative system. The exact solution, when rounded to four places for these three prescribed values of β, will be, χ_0 (nonconservative) = 14.24, 12.36, 10.78 and χ_0 (conservative) = 2.499, 12.36, 21.22, respectively.

Again, let us express our appreciation to Professor Smith for his discussion. However, we will continue with the application of our concepts to the equation which Hamilton called the "Law of Varying Action." We find it very much in existence. Nature is pleased with simplicity and affects not the pomp of superfluous causes [12].

References

4 Bailey, C. D., "Application of the General Energy Equation: A Unified Approach to Mechanics," Final Report, NASA Grant NGR 36-008-197, Aug. 1975.

5 Bailey, C. D., "Hamilton's Principle and the Calculus of Variations," submitted to the *AIAA Journal*, May 31, 1977.

6 Smith, D. R., and Smith, C. V., Jr., "When is Hamilton's Principle an Extremum Principle?," *AIAA Journal*, Vol. 12, No. 11, Nov. 1974, pp. 1573–1576.

7 Bailey, C. D., "A New Look at Hamilton's Principle," *Foundations of Physics*, Vol. 5, No. 3, Sept. 1975, pp. 433–451.

8 Bailey, C. D., "Hamilton and Nonstationary Systems," submitted to the *Foundations of Physics*, June 1977.

9 Bailey, C. D., "Reply by Author to C. V. Smith, Jr.," to appear in the *Journal of Sound and Vibration*, Sept. 1977.

10 Bailey, C. D., "Application of Hamilton's Law of Varying Action," *AIAA Journal*, Vol. 13, No. 9, Sept. 1975, pp. 1154–1157.

11 Bailey, C. D., "The Method of Ritz Applied to the Equation of Hamilton," *Computer Methods in Applied Mechanics and Engineering*, Vol. 7, No. 3, 1975, pp. 235–237.

12 Yourgrau, W., and Mandelstam, S., *Variational Principles in Dynamics and Quantum Theory*, 3rd ed., W. B. Saunders Company, Philadelphia, Pa., 1968, p. 8.

Timoshenko Beam Theory Is Not Always More Accurate Than Elementary Beam Theory[1]

R. M. Christensen.[2] The Brief Note by Nicholson and Simmonds on the accuracy of Timoshenko beam theory, along with the ensuing discussion are both stimulating and important. Of course, beam theory is just a one-dimensional specialization of plate theory, the latter of which is the subject of this discussion note. Very brief comments will be given here relative to a means of classifying the order of various plate theories. The relevance of this classifying scheme to the subject of the discussion will then follow.

A common means of deriving plate theories involves the use of power series expansions in z, for stresses and/or displacements such as

$$u = \sum_{i=1,3}^{N} (z)^i U_i(x, y)$$

$$v = \sum_{i=1,3}^{N} (z)^i V_i(x, y)$$

$$w = \sum_{i=1,3}^{N} (z)^{i-1} W_i(x, y) \quad (1)$$

where z is the thickness coordinate and the usual notation in rectangular Cartesian coordinates is employed. Expansions (1) are restricted herein to the case of antisymmetrical deformation about the middle plane, appropriate to flexural motions of plates. The governing field equations and boundary conditions to be used to determine U_i, V_i, and W_i for a specified value of N are obtainable from variational principles.

It follows from the completeness property of power series expansions that given enough terms in (1), these expressions can very nearly represent any smooth, antisymmetrical field. There does not seem to be a mathematical proof of the following hypothesis, yet it appears to be realistic: the more terms taken in the expansion (1), the more accurate the corresponding theory. At least for purposes of this discussion, let us accept the premise just stated. Accordingly, the order of any theory under consideration is just given by the level N at which the series in (1) are truncated, and a higher-order theory is expected to be more accurate than a lower-order one.

Consider next, the relationship of classical plate theory and the shear deformation plate theory, for example as derived by Reissner [1].[3] According to the foregoing scheme of classifying the order of plate theories, both classical theory and shear deformation theory are of the same order, $N = 1$. The only difference in these two theories is that the classical theory imposes additional assumptions or constraints into the shear deformation theory; namely, that

$$U_1(x, y) = \frac{-\partial W_1(x, y)}{\partial x} \quad \text{and} \quad V_1(x, y) = \frac{-\partial W_1(x, y)}{\partial y}. \quad (2)$$

These constraints do not alter the fact that the two theories are of the same order. Naturally, these special constraints reduce the order of the final system of governing differential equations. However, the order of the final system of differential equations is a completely different measure from the order of the plate theory, as defined herein. In fact, the lack of distinction between the order of the theory and the order of the differential equations appears to be the source of considerable confusion on the subject.

The situation of the classical plate theory being of the same order as shear deformation theory, but involving constraints, bears some resemblance to the linear theory of elasticity, with and without the constraint of incompressibility. Both of these latter two theories are considered to be of the same order of approximation with respect to the fully nonlinear theory, yet one is more general than the other.

With both classical plate theory and shear deformation plate theory being of the order $N = 1$, it is completely consistent that Nicholson and Simmonds were able to find an example which demonstrates that shear deformation theory is no more accurate than classical theory. Probably many more such examples exist. Certainly, it is helpful to have this situation pointed out and known. However, as a practical matter, one must expect to degrade the generality of a theory by imposing special assumptions or constraints into the theory, especially when these constraints are physically rather than mathematically motivated. Such is the case in restricting shear deformation theory to the classical theory form using (2). Thus I agree with Professors Koiter and Reissner that the practical usefulness of the shear deformation theory remains intact. It is simply a matter of having two theories of the same order in a mathematical sense, but of different degrees of generality or completeness. The more complete theory should in general be preferable, even though counterexamples exist.

Finally, it may be noted that to make an improvement in the order

[1] By J. W. Nicholson and J. G. Simmonds, and published in the June, 1977, issue of the JOURNAL OF APPLIED MECHANICS, Vol. 44, TRANS. ASME, Vol. 99, Series E, pp. 337–338.

[2] Lawrence Livermore Laboratory, P.O. Box 808, Livermore, Calif.

[3] *Numbers in brackets designate References at end of Discussion.*

of plate flexure theory over that of the classical and shear deformation level, $N = 1$, it is necessary to develop a theory of the order of $N = 3$ in (1). A theory of this level has been given by Reissner [2]. A different general theory of this order will be published shortly [3].

References

1 Reissner, E., "On Bending of Elastic Plates," *Quarterly of Applied Mathematics*, Vol. 5, 1947, p. 65.

2 Reissner, E., "On Transverse Bending of Plates, Including the Effects of Transverse Shear Deformation," *International Journal of Solids and Structures*, Vol. 11, 1975, p. 569.

3 Lo, K. H., Christensen, R. M., and Wu, E. M., "A High-Order Theory of Plate Deformation. Part 1: Homogeneous Plates," JOURNAL OF APPLIED MECHANICS, Vol. 44, TRANS. ASME, Vol. 99, Series E, Dec. 1977, pp. 663–668.

R. Schmidt.[4] Although the authors refer to the problem under consideration as a "beam" problem, in fact it is not. Hence, their conclusions apply only to their problem, and not to the theory of beams.

Let us examine some of the authors' assumptions and results. In view of the presence of *large longitudinal* distributed shearing *loads*, applied to the upper and lower edges of the bar, the authors' loads cannot be regarded as "reasonable" for a beam, as no longitudinal forces on long edges are permitted in the beam theory. As a matter of fact, the authors' final conclusion would have been difficult, if not impossible, in the absence of these *applied* longitudinal shears, for, with the aid of (18*a*) and (14*c*),

$$[k - (1 + \nu)]\gamma = 2(1 + \nu)[k - (1 + \nu)]\tau$$

in (19). Hence, if the longitudinal shear (load) $\tau(x, \pm H) = \tau(x,y) = \tau(x)$ were zero, $\beta(x) = -\eta'(x)$, (k could be any number),

$$u = -\eta'(x)y - (\epsilon^2/3)g'(x)(y^3 - 3y/5),$$

$$v = \eta(x) + \epsilon^2 g(x)(y^2 - 1/5),$$

which imply no shear deformation.

Furthermore, in no familiar reference to Timoshenko's beam theory (e.g., [4][5]) is the term $H[\tau(x,H) + \tau(x, -H)]$ present in equations corresponding to the authors' (6) and (3*b*).

It seems to the writer that the authors chose a seemingly "reasonable" displacement field, but, alas, "reasonable" displacement functions are not always in accord with the prescribed boundary conditions for stresses. And it is the boundary conditions on stresses that often determine the classes of problems encountered in the theory of elasticity. For a bar is a bar, and it can be bent geometrically not only by couples and transverse loads but also by longitudinal shearing surface forces as well as longitudinal body forces. Unfortunately, the latter case (or an inseparable combination of the two) is not considered to be a beam.

As an aside, it may also be said that, although the shear deformation and the rotary inertia had separately been introduced into the engineering beam theory [5,6] long before Timoshenko, it was he who emphasized the importance of the transverse shear deformation in vibrations [7,8], and, for that reason, and as a brief, convenient way to designate the inclusion of these two effects in the classical elementary theory of beams, his name has been associated with this theory.

References

4 Timoshenko, S., Young, D. H., and Weaver, W., Jr., *Vibration Problems in Engineering*, 4th ed., Wiley, New York, 1974, p. 433.

5 Rankine, W. J. W., *A Manual of Applied Mechanics*, 1st ed., Richard Griffin & Co., London, 1858, pp. 342–344.

6 Rayleigh, J. W. S., *Theory of Sound*, 2nd ed., Vol. 1, Macmillan, London, 1894; reprinted by Dover Publications, New York, p. 258.

7 Timoshenko, S. P., "On the Correction for Shear of the Differential Equation for Transverse Vibrations of Prismatic Bars," *Philosophical Magazine*, Vol. 41, Series 6, 1921, pp. 744–746.

8 Timoshenko, S. P., "On the Transverse Vibrations of Bars of Uniform Cross Section," *Philosophical Magazine*, Vol. 43, Series 6, 1922, pp. 125–131.

M. Levinson.[6] Since this Brief Note, the ensuing discussion [9][7] and the author's closure [10] all appeared simultaneaously, the present writer takes the liberty of discussing these as one entity.

Nicholson and Simmonds, van der Heijden, and Koiter all all correctly note that Timoshenko was not the first to consider the additional deflections of beams due to transverse shear for the static case. What is usually called Timoshenko beam theory was devised by Timoshenko [11, 12] to study the vibrations of beams and the purpose of his work was to show that if one considered the rotatory inertia, as had Rayleigh, that one should also consider the shear effect; in fact, for a particular case of a vibrating beam, Timoshenko demonstrated that shear had an effect on the natural frequency of about four times that of rotatory inertia. It is this dynamical theory, considering both shear and rotatory inertia effects, which is, quite appropriately, called Timoshenko beam theory. Furthermore, we note that Timoshenko's concern was with an overall property, namely, the spectrum of natural frequencies of a vibrating beam, and not with pointwise behavior of the beam.

The fact that the Timoshenko beam theory has no asymptotic basis in linear elastodynamics leads to the point that *ad hoc* engineering theories (or approximations), which must always be used judiciously, frequently must be justified by experience and experiment. The good engineer is one who learns the limits of the *ad hoc* theory and, thus, knows when to use it and, equally, when not to use it. The reader should recall that linear elasticity is a physical theory, with a proper mathematical structure, the validity of which rests on comparison with experience and not on its mathematical integrity alone. The more limited physical validity of the Timoshenko beam theory, with its own valid mathematical structure, rests on how well it compares with our experience of the phenomena it attempts to describe. One would like to have, of course, an asymptotically valid dynamical beam theory which would be no more complicated than the Timoshenko beam theory.

References

9 van der Heijden, A., et al., Discussion, JOURNAL OF APPLIED MECHANICS, Vol. 44, TRANS. ASME, Vol. 99, Series E, June 1977, pp. 357–359.

10 Nicholson, J. W., and Simmonds, J. G., Authors' Closure, JOURNAL OF APPLIED MECHANICS, Vol. 44, TRANS. ASME, Vol. 99, Series E, June 1977, pp. 359–360.

11 Timoshenko, S. P., "On the Correction for Shear of the Differential Equation for Transverse Vibrations of Prismatic Bars," *Philosophical Magazine*, Vol. 41, 1921, pp. 744–746.

12 Timoshenko, S. P., "On the Transverse Vibrations of Bars of Uniform Cross Section," *Philosophical Magazine*, Vol. 43, 1922, pp. 125–131.

T. Leko.[8] It is remarkable that Dr. A. van der Heijden, Professor W. T. Koiter, and the authors emphasize the importance of the end conditions for a built-in cantilevered beam. This brings us back to the problem of validity of the Saint-Venant principle. The principle has been challenged earlier in several papers. Most recently, the writer presented a paper, June 1, 1977, at the GAMM meeting in Denmark, to be published in the *Zeitschrift für Angewandte Mathematik und Mechanik*, which shows unambiguously clearly that a particular

[4] Professor of Engineering Mechanics, Department of Civil Engineering, University of Detroit, Detroit, Mich. Mem. ASME.

[5] Numbers in brackets designate References at end of Discussion.

[6] Professor of Engineering Mechanics, Department of Civil Engineering and Engineering Mechanics, McMaster University, Hamilton, Ontario, Canada L8S 4L7.

[7] Numbers in brackets designate References at end of Discussion.

[8] Mechanical Engineer, Potomac Electric Power Company, Washington, D. C. Mem. ASME.

statically equivalent end condition produces a large effect throughout the length of a cantilevered beam. This supports the contention of the paper under discussion that it is senseless to refine the differential equations of the beam theory if it is not possible to specify the boundary conditions in more detail than in elementary beam theory.

Authors' Closure

We thank Dr. Christensen, Professor Schmidt, Professor Levinson, and Dr. Leko for their comments and we shall reply to them as best we can.

We do not believe that Dr. Christensen's classification of the order of beam (or plate) theories is useful because it does not explicitly account for the beam's thinness, H/L, which is obviously the relevant small parameter. Away from edges, i.e., in the interior of the beam, it seems more natural to seek asympotic expansions of all variables in powers of (H/L). This is the approach pioneered by Goodier [13][9] and developed by Johnson and Reissner [14], Friedrichs and Dressler [15], and Goldenveiser [16], among others.

If solutions are expanded in powers of z, there is no guarantee that, say, an $N = 3$ solution will be any more accurate than an $N = 1$ solution. Moreover, if (1) is used with $N = 1$ to compute strains, then one arrives at the erroneous conclusion that the transverse normal strain is zero, whereas one should be able to conclude, to this degree of approximation, that it is the transverse normal stress that is zero. In particular, if the $N = 1$ approximation were used in conjunction with the principle of minimum potential energy, the resulting field equations would contain a relative error of $O(1)$.

Finally, we believe that it is incorrect to characterize equation (2) as being "physically rather than mathematically motivated." In the asymptotic method, (2) would fall out from the exact plane stress equations as a first approximation.

Professor Schmidt feels that our results do not apply to "beam theory" because we permit "*large longitudinal* distributed shearing *loads*" to be applied to the upper and lower faces of the bar. This objection is easy to refute. First, the shear loads are *not* large. According to the scaling introduced in our equation (12), the shear stress is $O(\sigma_0 H/L)$, where σ_0 is of the order of magnitude of the dominant axial stress. If conventional beam theory permits interior shear stresses of this order, why is it inconsistent to have the shear stress take on values of this magnitude at the boundaries of the beam?

Second, it is easy to produce an alternate two-dimensional displacement field that results in a shear stress that varies parabolically through the thickness and vanishes at the upper and lower faces, namely,

$$u(x,y) = -\eta'(x)y + \epsilon^2\gamma(x)y + \epsilon^2 f(x)(y^3 - 3y/5) \quad (1)$$

$$v(x,y) = \eta(x) + (\epsilon^2/2)\{\nu[\eta''(x) - \epsilon^2\gamma'(x)] + \epsilon^2\zeta(x)\}(y^2 - 1/5), \quad (2)$$

where

$$12f(x) = -5\gamma(x) - 2\nu\eta'''(x) + 2\nu\epsilon^2\gamma''(x) - 2\epsilon^2\zeta'(x) \quad (3)$$

and η, γ, and ζ are arbitrary functions, save that they must be chosen so that $u(0,y) = v(0,y) = 0$. One finds that in this case

$$V(x) = \eta(x) + \epsilon^2[(5k/6(1+\nu)]-1\} \times \int_0^x \gamma(\xi)d\xi - \nu(1-\nu^2)^{-1}\epsilon^2 \int_0^x (x-\xi)\zeta(\xi)d\xi \quad (4)$$

$$v(x,0) = \eta(x) - (\epsilon^2/10)\{\nu[\eta''(x) - \epsilon^2\gamma'(x)] + \epsilon^2\zeta(x)\} \quad (5)$$

$$w(x) = \delta(x) = \eta(x). \quad (6)$$

Again, no choice of k will make (4) agree with (5) or (6).

We agree with Professor Levinson's comments regarding the care with which one must apply *ad hoc* theories. In regard to his comment that "Timoshenko's concern was with an overall property, namely, the spectrum of natural frequencies of a vibrating beam, and not with pointwise behavior of the beam," we wish to point out that Cowper [17] has compared the lowest natural frequency predicted by Timoshenko's equations against that predicted by exact plane stress theory for a simply supported beam. He finds excellent agreement providing one interprets the vertical deflection of the Timoshenko theory as the average of the exact vertical deflection across the thickness and sets

$$k = \frac{10(1+\nu)}{12+11\nu} \quad (7)$$

The reference cited by Dr. Leko concerning Saint-Venant's principle is not available to us at this time so that we cannot comment on its relevance to our Brief Note.

[9] Numbers in brackets designate References at end of Closure.

References

13 Goodier, J. N., "On the Problem of a Beam and a Plate in the Theory of Elasticity," *Transactions of the Royal Society of Canada*, Vol. 32, 1938, pp. 65–88.

14 Johnson, M. W., and Reissner, E., "On the Foundations of the Theory of Thin Elastic Shells," *Journal of Mathematics and Physics*, Vol. 37, 1958, pp. 375–392.

15 Friedrichs, K. O., and Dressler, R. F., "A Boundary-Layer Theory for Elastic Plates," *Comm. Pure and Appl. Math.*, Vol. 14, 1961, pp. 1–33.

16 Goldenveiser, A. L., "The Principles of Reducing Three-Dimensional Problems of Elasticity to Two-Dimensional Problems of the Theory of Plates and Shells," *Proceedings of the 11th International Congress of Applied Mechanics*, Springer-Verlag, 1966, pp. 306–311.

17 Cowper, G. R., "On the Accuracy of Timoshenko's Beam Theory," *Journal of Engineering Mechanics Division, Proceedings, ASCE*, Vol. 94, No. EM6, 1968, pp. 1447–1453.