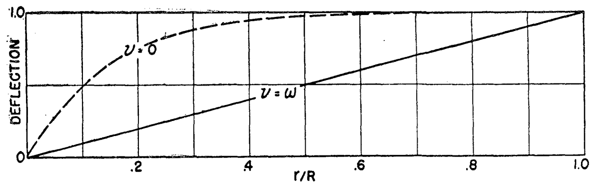


FIG. 8. Rigid blade mode and static mode of rotating blade at ω = 223 r.p.m., beamwise.

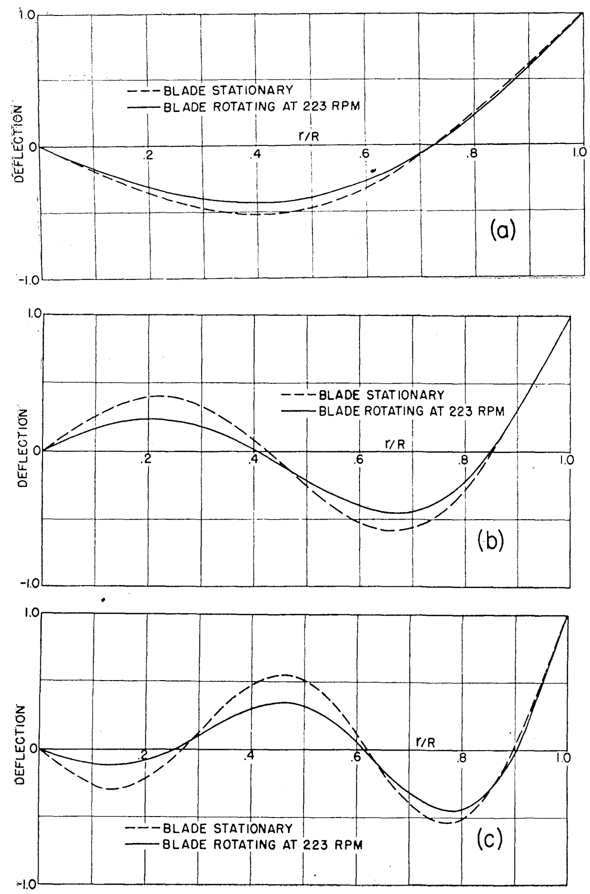


FIG. 9. Beamwise natural modes. (a) Fundamental mode, (b) second mode, (c) third mode.

undamped hinge moment M_H. The moment that must be supplied at the hinge to shake the blade tip with unit amplitude is $-M_H - jM_d$, and has the magnitude

$$\mathfrak{M} = \sqrt{M_H^2 + M_d^2} \tag{28}$$

The hinge slope φ_H is plotted vs. ν in Fig. 6; $\mathfrak{M}$ and, for comparison, also M_H and M_d, with signs disregarded, are plotted in Fig. 7a (nonrotation) and Fig. 7b (rotation at 223 r.p.m.) for a damping constant of D = 400 lb.ft. per rad. per sec.

$\mathfrak{M}$ is always equal to, or larger than, M_H and M_d. Points of tangency occur only where φ_H or M_H vanish. It is noted for the rotating blade that up to about $\nu = \omega$ the damper absorbs little vibration. Nevertheless, this small damping plays an important role in preventing ground vibrations. At the higher resonance frequencies M_d is extremely effective in limiting the vibration amplitudes to small values. It is noted that the resonance frequencies ($\mathfrak{M}$ = minimum) are almost entirely unaffected by the presence of the damper. (For exceedingly large damping constants, not achievable in practice, this would no longer be true.) The same is found true also for the deflection curves.

BEAMWISE NATURAL MODES

In Fig. 8 the rigid mode, $\nu_0 = \omega$, and the static deflection curve, $\bar{\nu} = 0$, are shown for the rotating blade. The first, second, and third beamwise natural modes, nonrotating and rotating at 223 r.p.m., are shown in Figs. 9a–9c. The discrepancy between the rotating and the nonrotating modes seems to increase with the order of the mode.*

* A somewhat different conclusion is reached by Simpkinson, Eatherton and Millenson,[5] for cantilever blades.

REFERENCES

[1] Myklestad, N. O., *Vibration Analysis;* McGraw Hill Book Company, Inc., New York, 1944.

[2] Prohl, M. A., *A General Method for Calculating Critical Speed of Flexible Rotors,* Journal of Applied Mechanics, Vol. 12, p. A-142, 1945.

[3] Horvay, G., *Stress Analysis of Rotor Blades,* Journal of the Aeronautical Sciences, Vol. 14, p. 315, 1947. Errata to the above paper, ibid., Vol. 14, p. 544, 1947.†

[4] Den Hartog, J. P., *Mechanical Vibrations,* p. 310; McGraw Hill Book Company, Inc., New York, 1944.

[5] Simpkinson, Eatherton, Millenson, *Effect of Centrifugal Force on the Elastic Curve of a Vibrating Cantilever Beam,* N.A.C.A. T.N. 1204, 1947.

† In correction (15) of this reference, $(\zeta)_4$, which is the coefficient of the sin 2ψ term of ζ, should read $-2\omega A_2$.

Letter to the Editor

Dear Sir:

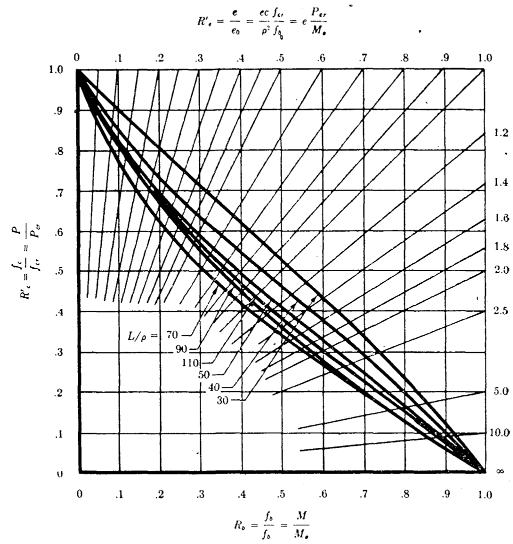
The following notes were suggested by Professor Wang's paper "Inelastic Column Theories and an Analysis of Experimental Observations" (JOURNAL OF THE AERONAUTICAL SCIENCES, May, 1948).

In Eqs. (16), (18), and (19) the "coefficient of fixity" was used as a multiplying factor for the critical column load in the inelastic range. If C were actually used in this manner, the predicted loads would be much too high. It is unfortunate that the coefficient of fixity was ever introduced into the column literature. Since end restraint reduces the effective column length, its effect should be taken care of by a factor that operates on the column length, not on the load or stress. Such a factor has been used by many authors, in the form, $k = L_0/L$. I am heartily in favor of dropping C entirely and substituting an effective length factor. (Incidentally, I can claim priority over Wang on this error, having done the same thing in my book, *Basic Structures!*)

The second point concerns the use of the term "neutral axis." In his 1909 thesis von Kármán pointed out (in a footnote) that the neutral axis as he used it was not "stress free," since axial compressive stresses were also acting over the entire cross section. If the curve of actual stress distribution is known, the "neutral axis" is found at the point of intersection of this curve with a horizontal line representing the average axial stress. For the elastic case this coincides with the neutral axis for pure bending, but in the inelastic case it does not; neither does it determine the location of the resultant axial force for a constant stress distribution (this remains at the centroid). If the bending is permitted to proceed simultaneously with the increase of axial load, the "neutral axis" will not be stationary but will move away from the centroid as the loading increases. The only physical meaning that could be attached would be that it determines a line along the length of the column for which there is no change of axial stress. But this *requires the assumption of a constant average axial stress during bending*, the very assumption that is now agreed to be wrong and on which the double-modulus theory was based. It would seem desirable to restrict the definition of the term "neutral axis" to pure bending. This would force realization that its use in combined bending and axial load-ing involves treating the bending separately.

Professor Wang has attempted to improve the simple tangent modulus formula by introducing two modulii, one higher and one lower than the tangent modulus. This is done to obtain a better approximation to the true stress distribution. These modulii are then used in the original double-modulus formula, again introducing the original assumption of a neutral axis. Furthermore, the use of two modulii aE_t and bE_t requires a further assumption as to *how much* differential strain (bending) is involved. If we use the idealized column as a basis for analysis, we must assume, as Wang has assumed, an "infinitesimally small" disturbance. This implies that the differential strain approaches zero as a limit, bringing us back to the tangent modulus again.

From the correspondence that followed the publication of my paper, "Inelastic Column Theory," I can see that much of the difficulty associated with buckling theories comes from a confusion of "orders of abstraction" (to borrow a term from Korzybski's *Science and Sanity*). Since it is impossible to analyze an actual column, we must create a mental picture or abstraction that may be more or less idealized. The so-called ideal, centrally loaded column in the elastic range represents a high order of abstraction and leads to the original Euler formula. A closer approach to reality is obtained by replacing the elastic modulus by the tangent modulus, but the idea of the "perfect" column is still retained. The degree of eccentricity or disturbance that must be assumed in such an analysis is truly infinitesimal; it is of the order of the disturbance needed to upset a needle if we could succeed in making it stand on end! The introduction of a *finite* eccentricity or "disturbance" places the problem on an entirely different level of abstraction. It must then be attacked by methods similar to those used by von Kármán in his 1909 paper. Most of the arguments about the effects of small eccentricities on the "column theory" can never be resolved because they are not arguments at all, simply confusions of the orders of abstraction involved.



Reduced Interaction Curves for Combined Bending and Axial Loading

Fig. 15

I do not believe that any additional theories or modifications are needed to explain the fact that test values often exceed the values predicted by the tangent modulus theory in the region above the knee of the stress-strain diagram. The remarks about Fig. 10, in my original paper, together with Dr. von Kármán's discussion, should make this quite clear. By the same reasoning, it can be seen that, in the region of the knee of the stress-strain diagram, where the tangent modulus decreases rapidly with increasing strain, the inevitable initial eccentricities and imperfections will have their largest effect and test data will tend to fall below the tangent modulus values.

Finally, I should like to suggest that the problem of finite eccentricities be attacked by an entirely different engineering procedure, the use of the *interaction curve* for combined axial loading and bending. In a recent A.S.C.E. paper ("Applied Column Theory," not yet published) I showed how this could be done by adding lines of constant eccentricity ratio to an interaction chart. This method covers the entire range from pure axial loading to pure bending, is dimensionless, and can be reduced to a simple form as shown herein by Fig. 15 from the above paper. This chart is based on *N.A.C.A. Technical Note No. 307*, in which the tests were made by transverse loading at the third points. For eccentric columns (bending moment applied at ends) the curves would show more sag. I am now attempting to collect test data on eccentric columns for which the bending modulus of rupture has also been determined and would appreciate receiving any information of this type.

F. R. Shanley
Consultant on Structures
Los Angeles, Calif.