The Normal Modes of Nonlinear n-Degree-of-Freedom Systems

R. F. Henry. This interesting paper necessitates some comment on differences in practice between American and European engineers in the definition and use of the term “normal modes” as applied to nonlinear systems. For a linear system it is possible to find a coordinate system, the well-known unique set of “normal coordinates,” which uncouples the equations of motion. Given any particular motion, the components of it attributable to the various normal coordinates are completely independent of one another and are consequently termed normal modes. The author lists some properties of linear normal modes in his paper; an additional important feature is that they form a complete set. Since the principle of superposition applies in linear systems, this means that the free motions (which are all periodic) can be other hand, completely in terms of the normal modes.

In free nonlinear systems, periodic or nonperiodic motions may occur (depending on the initial conditions), and in addition solutions cannot be superposed. Inevitably, there is no simple equivalent to the independent normal modes of linear systems; the best that can be done is to choose a coordinate system which allows some particular feature of the system to be seen clearly. In the writer’s country, the custom has been to regard the problem as that of defining some convenient coordinate system which simplifies to some extent the discussion of periodic motion but yet does not prevent or unnecessarily complicate discussion of nonperiodic motion. For the case of systems with linear components in the spring forces, it has been found that the applied mathematicians’ standard technique of using “normal coordinates” (i.e., the linear normal coordinates obtained by temporarily neglecting all nonlinearity) simplifies subsequent examination of the effect of nonlinearity on periodic and nonperiodic motion. Here, the term normal mode is then used in the nonlinear case only to indicate the components of the general motion attributable to each of the linearly normal coordinates just described. Independence is no longer implied. Since these normal modes are merely displacements defined in a particularly suitable coordinate system, any motion of the system, periodic or nonperiodic, can be regarded as a combination of the normal modes. With this definition, it is then in order to speak of the normal modes as “varying in amplitude, frequency or relative phase,” “being coupled,” or “disturbing one another,” as the case may be.

On the other hand, the author has chosen a logical alternative and introduced a different definition of normal modes, intended, so far as the writer interprets it, to simplify particularly the treatment of periodic motions. Apparently there is some loss of generality in that the limitations imposed complete or possibly preclude subsequent discussion of nonperiodic motions, but, on the other hand, clarity of definition is achieved between systems with and without linear components in the spring forces. An important point may be raised regarding this proposal; there is a wide range of possible periodic motions which cannot be described in terms of the normal modes which the author has defined. As an example, the quasi-linear system

\[ \ddot{q} + k_1q_1 + \epsilon q_1^2 + q_1q_2 = 0 \]
\[ \ddot{q} + k_2q_2 + \epsilon q_2^2 + q_1q_2 = 0, \]

a particular case of equations (2) in the paper under discussion may easily be shown to possess periodic solutions with the fundamental terms

\[ q_1 = A \cos \omega t \]
\[ q_2 = B \sin \omega t \]

Similar periodic motions, i.e., with relative phase angles not equal to 0 or \( \pi \), can be shown to occur in a wide class of systems of engineering interest, such as vibration-isolating suspensions, vibration absorbers, and thin disks. Now \( q_1 \) and \( q_2 \) cannot simultaneously be zero, thus it is clear that such a motion cannot be described in terms of normal modes if the latter cannot be superposed and are defined such that the masses all “pass through the equilibrium position at the same instant,” as stipulated by the author.

In view of the earlier sacrifice of generality, it seems to the writer that it is not unreasonable to require that normal modes, however defined, should be sufficiently general to allow discussion of at least the periodic motions. Perhaps the author might care to consider and comment on the possibility of extending his method to include the other cases mentioned in the foregoing.

L. S. Jacobsen. The author has succeeded in obtaining an elegant, general analysis of a problem that will have wide application to practice in the future. His discussion of the normal-mode concept enlarges the reader’s understanding of linear systems and prepares him for an extension to the nonlinear ones.

The author’s definition of a normal mode for a nonlinear system states:

(a) All masses execute periodic motions with the “same period.”
(b) All masses pass through their static-equilibrium positions at the “same instant.”
(c) The positions of all masses at any time \( t \) are uniquely defined by the positions of any “one” of them.

It might be added—for the sake of the physically minded student—that the “same period” obviously does not mean a period independent of magnitude of motion.

The author’s use of the potential function \( U \) in the \( (\xi_1, \xi_2, \ldots, \xi_n) \) space to represent the modal properties of linear as well as of nonlinear systems is clearly explained and appeals to the theoretician as well as to his more earthbound, practical brother. Thus, for a three-degree-of-freedom linear system, the Euclidean space \( U \) function will be an ellipsoid, and the three straight modal lines will be its principal axes, while for a three-degree-of-freedom nonlinear system the ellipsoid changes to an ovaloid, the modal lines may or may not be straight, but they will intersect the \( U \) surface normally.

It is to be expected that this general paper—a distinct pioneering contribution—will be well received and will form a basic starting point for numerous specific studies of the fascinating multidegree-of-freedom nonlinear problems since it enables the investigator to decouple at least some of the various modes.

2 Department of Engineering, University of Cambridge, Cambridge, England.
6 Professor of Mechanical Engineering, Stanford University, Stanford, Calif.
D. G. MAGIROS. The author examines a nonlinear system of $n$-degrees of freedom, where the nonlinearities are taken as odd functions of the spring deflection, for the purpose of determining the “normal modes” of the system. The equations of motion of the system are normalized in the sense of unit masses, and the discussion of the normalized system is based on a mathematical formulation of the physical definition of normal modes as “amplitude restrictions” of the oscillations. The calculation of normal modes, by using the “potential function” of the normalized system, is reduced to a geometrical problem, which is analyzed.

The following comments may be found useful for a better understanding of this important paper.

1. Given a physical system and using “normalizing conditions” the resulting “normalized systems” are specific cases of the physical system, and then a discussion on normalized systems may be not equivalent to that of the corresponding physical system.

The discussion of the normal modes of a physical system can be applied to the normalized system, but the converse may be not true, depending on the normalizing conditions.

The kind of normalization used in the paper keeps the amplitude of the oscillations unrestricted; then its definition of normal modes holds for both the physical and normalized systems. However, if the normalizing conditions were amplitude-restrictions, the normal modes of the normalized systems are not the same thing as the normal modes of the physical systems.

It is, therefore, reasonable in connection with the definition of normal modes to start with the physical system.

One can escape the possible difficulties, if he calls these particular motions of the physical system “principal modes,” and the corresponding motions of the normalized system “normal modes.”

As a result, the term normal modes of the title of the paper more appropriately should be called principal modes.

2. A correctly stated physical problem can be idealized as a well-formulated mathematical problem, which, in the case of motion, is an initial-value problem, of which any function can be taken as a trial solution. If the trial solution can be calculated in such a way that it satisfies the differential equations and the data of the problem, the solution will become a formal solution. The formal solution must satisfy some additional requirements, say the Hadamard's postulates, in order to be an “actual solution” of the physical problem. This is the procedure of the modern applied mathematicians for calculating mathematical solutions of the physical problems, solutions which interpret the reality in an adequate way.

In the paper the “nature” of the trial solution taken is not clear; and only a formal mathematical solution of the corresponding physical problem is discussed. There is no systematic attempt for discussion of the existence of the solution, the uniqueness with the data, and the continuous dependence with the data, properties which permit for a formal solution to be or not to be an actual solution of the physical problem.

3. Speaking on principal modes of a system, an important question is the following: Can a procedure be found which makes use of the principal modes of a system in order to get information about other motions of the system? The answer is “yes” in linear systems, since any motion of a linear system can be considered as an appropriate linear combination of the principal modes of the system.

The answer is “no,” in general, in nonlinear systems. It is a big unsolved problem to answer the foregoing question in a general way, if the system is nonlinear.

Author's Closure

The comments of all three discussers are very much appreciated—in both meanings of this word; they will be taken up in order.

Mr. Henry questions the use of the term “normal mode” for the type of periodic motion examined in the paper under discussion. He is not alone in questioning it as shown, for instance, by Mr. Magiros' discussion and by several personal communications received by me. On the other hand, I am not the only one to favor it, as shown by several recent papers on these theories [1-4].

The ultraconservative (and Mr. Henry is not among these) demands that any set of solutions worthy of being called “normal modes” constitute a complete set of linearly independent solutions having the property that linear combinations of these yield again solutions. He lives in a linear world and he concludes, necessarily, that the nonlinear systems under discussion here do not possess solutions in normal modes. One cannot find fault with this “linear man,” for he has merely exercised his prerogative of finding one definition revolting and another acceptable. To pacify him, it may be pointed out that one can let the systems of the subject paper approach the linear case in a continuous manner. This is done by letting the $a_i$'s continuously for $j = 3, 5, \ldots$, when the equations of motion are (4), or by letting $k \to 1$ continuously when the equations of motion are (33). In either case, the solutions called “normal modes” will, in the limit, coincide with his own definition. Thus, having been offended perhaps, he has at least not been violated.

Mr. Henry takes a more liberal view, although not quite as liberal as he did in the past. In an earlier paper [8] he agreed that the use of the term “normal mode” could be applied whenever the solution in “normal modes” can be expressed “in terms of a single coordinate.” This view coincides with mine. Now, however, Mr. Henry defines as “normal modes” those which are found by “temporarily neglecting all nonlinearity.” This defines a coordinate system, Mr. Henry says, in which any motion may “be regarded as a combination of normal modes.” Clearly, this definition denies the existence of normal modes in all non-linearizable systems (i.e., in those where the spring forces have no linear components), and this is the central point of his discussion.

As I understand Mr. Henry's comments and papers, I believe that he does not, in fact, define normal modes in the manner described in his comments. Since his work has, so far, been restricted to systems having only two degrees of freedom, my comments will also be restricted to these. However, they can be generalized readily to systems having any finite number of degrees of freedom, even though this generalization is neither self-evident nor trivial.

Consider a system of the class examined in the paper under discussion and having the two degrees of freedom $x, y$. Then, any configuration of the system is uniquely defined by a point in the $xy$-plane (the configuration space), and any motion whatever corresponds to a curve in that configuration space. If the system is linearizable, any motion in a sufficiently small neighborhood of the origin of the configuration space coincides with that of the linearized system, obtained by “temporarily neglecting all nonlinearity.” This linearized system has, indisputably, normal modes in the conventional sense, and they can be found by the methods used by Mr. Henry and first applied to strongly non-linear systems by Kauderer [9]. These normal modes are two straight lines passing through the origin of the $xy$-plane and intersecting each other orthogonally. These two lines may be thought of as constituting a new system of coordinate axes $\alpha, \beta$, and any configuration of the system may be defined by its $\alpha$ and $\beta$-components. I take this to be the meaning of Mr. Henry's statement that any motion "can be regarded as a combination of

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Footnotes:

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normal modes.' But these coordinates "simplify subsequent examination" of the motion only in two instances: either configurations near the origin only are considered; or the \( x, y \)-axes themselves are special solutions (called by me "modal lines") everywhere in the \( xy \)-plane (with \( |x|, |y| < \infty \)). The nonlinear systems examined by Mr. Henry are, in fact, such that the \( x, y \)-axes are solutions in the entire domain, and he uses Kauderer's method to find them.

I have shown, for \( n \) degrees of freedom in the paper under discussion and for two degrees of freedom in an earlier paper [6], that there exists a nonlinearizable class of systems, called homogeneous, for which a set of noncoincident straight lines, all passing through the origin, are solutions in the entire (finite) \( xy \)-plane. One can also show readily that, when every motion is bounded, the set is complete. In general, these lines do not intersect each other orthogonally. Taking Mr. Henry's point of view, these lines may now be regarded as a set of oblique coordinates. They are quite as useful for defining uniquely any given configuration as orthogonal coordinates.

A similar situation exists in the general problem, linearizable or not, for which no straight lines exist that are solutions to the problem. It was shown for \( n \) degrees of freedom in the paper under discussion, and for two degrees of freedom in the earlier paper [6], that in this case there exist single-valued curved lines passing through the origin which are solutions in the entire (finite) domain. At the origin, these curved lines are tangent to straight lines, and these tangents may be found by ignoring temporarily all but the lowest powers in the spring forces. If that lowest power is 1, the curved lines intersect each other orthogonally, otherwise not. When every motion is bounded, it can be shown that this set of single-valued curved lines is complete. Hence this set of curves, called in my paper "modal lines," may be used as curvilinear coordinates to specify any configuration of the system. It appears, then, that one can put the modal lines to the use to which Mr. Henry wants to put them, whether straight or curved and whether intersecting orthogonally or obliquely. It is not quite clear, therefore, what Mr. Henry means with "sacred line of generation." It seems to me that, if Mr. Henry will generalize his own notions of coordinates so as to admit oblique and curvilinear ones, he will find that generality has been gained, rather than sacrificed by the proposals contained in the paper under discussion.

The comments of Professor Jacobsen are deeply appreciated. He is entirely correct in saying that "the same period" does not imply that the motions are isochronous. The intended implication is that every element of the system undergoes periodic motion, and the period of motion of any one of them is the same as that of every other. In fact, the frequency is amplitude-dependent, as pointed out by Professor Jacobsen.

Mr. Magiros raises three points. In the first, he states his preference for the term "principal modes" as opposed to "normal modes" for the periodic motions examined in the paper under discussion. The argument in favor of his preference is the observation that the dependent variables in the equations governing the system differ from the physical displacements of the masses by constants. The point seems well taken; certainly, there is no objection to such a change in nomenclature.

Mr. Magiros' second point deals with the question whether the problem is properly posed. It is possible that he has misinterpreted some features of the paper under discussion, for there was no question of a "trial solution." Inasmuch as the solutions presented reduce the governing differential equations to identities, they are exact. These governing equations are (30). They are nonsingular everywhere in the closed domain \( D \) bounded by the surface \( U + U_e = 0 \). Moreover, they satisfy easily the conditions required for solutions to exist, to be unique under specified initial conditions, and to be continuous in the initial conditions and the system parameters because the partial derivatives \( \partial U/\partial \alpha \) are analytic in the variables \( \alpha \), and in the system parameters \( \nu \alpha \) and \( \omega \). Hence all mathematical solutions (including those in principal modes) are real solutions of the physical problem as well. A much more difficult question is that of existence of principal modes. For the existence of these we require (sufficient, but perhaps not necessary) that \( U \) be negative definite everywhere in \( D \), and that the surface \( U + U_e = 0 \) be star-shaped with respect to the origin. In that case, it can be shown from arguments of continuity in initial conditions that solutions in principal modes do exist.

Mr. Magiro's third point deals with the important question whether knowledge of the complete set of solutions in principal modes permits the construction of solutions for arbitrary initial conditions. As he says, "it is a big unsolved problem." However, some light can be shed on this question. For instance, it is known in the case of Riccati's equation in a single degree of freedom how one can construct new solutions from known ones. In Hamiltonian systems having many degrees of freedom, knowledge of at least three first integrals permits the construction of an entire Lagrangian of such integrals (by means of Jacobi's theorem for Poisson systems). Finally, it has been shown [10] that for nonlinear (and even nonlinearizable) systems there exist "superposition in the wrong sense." This term implies that a certain linear combination of normal mode solutions can yield the general solution; however, it is not true that every linear combination of solutions is itself a solution. It is for this reason that this type of superposition has been termed "weak."

References


It seems possible that the second of these conditions could be relaxed.

Similitude in Package Cushioning

G. E. NEVILL, JR. In addition to their presentation of improved scaling laws for cushion systems, it is believed that the

2 Senior Research Engineer, Department of Mechanical Sciences, Southwest Research Institute, San Antonio, Tex. Assoc. Mem. ASME.