can be shown simply for any system with analytic nonlinearities that when a single mode is in motion, it is by itself orbitally stable, being in this and in other respects like a single-degree-of-freedom system. However, the other mode, the one at rest, is parametrically excited by the mode in motion and may begin to vibrate if it is unstable. For detailed discussion of these points in the particular case of a system with small cubic nonlinearities in the restoring springs, reference may be made to a recent paper.

For unsymmetrical systems, it may be shown directly from the equations of motion that it is not possible for one mode to be at rest while the other is vibrating. Consequently, no “uncoupling” of the equations of motion is permissible in general.

Author’s Closure

I would like to thank Mr. Henry for his discussion which illuminates some “misconceptions” and “invalid results.” Other than his statement that the misconceptions and invalid results are mine, not his, he takes issue with what he alleges to be some of my views by saying:

(i) “...artificial uncoupling of the equations of motion [and] ... subsequent (my italics) investigation of stability cannot yield information about any mode other than the one in motion.”

(ii) “The hypothesis that the crossing of the ... backbones is of significance (my italics) is not born out by analysis...” and he states that

(iii) “...when a single mode is in motion, it is by itself orbitally stable ... [while] the one at rest is parametrically excited.”

(iv) “[in] unsymmetrical systems it may be shown... that it is not possible for one mode to be at rest while the other one is vibrating.”

The first two points may be disposed of by suggesting that Mr. Henry reread with some care the papers which have aroused his interest.

Mr. Henry’s statement (i) might lead the unwary reader to believe that in references [3] and [1] (his reference numbers are used throughout) the stability of solutions was examined by means of the uncoupled equations. While it is true that the stability of solutions was examined subsequent to the discussion of the solutions themselves, this was, of course, not done on the uncoupled equations. For instance, it is clearly stated in [3] that a perturbed solution was substituted in (6), and (6) are the coupled equations of motion. However, it did not escape Mr. Henry’s attention that we did find orbital instabilities. This observation alone should have told him without further reading that we could not have examined instability on the uncoupled equations. For instance, it is stated in [3] that a perturbed solution was substituted in (6), and (6) are the coupled equations of motion. However, it did not escape Mr. Henry’s attention that we did find orbital instabilities. This observation alone should have told him without further reading that we could not have examined instability on the uncoupled equations, for the latter are those of a conservative single-degree-of-freedom system moving in free vibrations, and even a novice in nonlinear vibration theory knows that such vibrations are never orbitally unstable.

The unsuspecting reader may gather the impression from Mr. Henry’s remark (ii) that we attached some special significance to our observation, quoted, here from [3], that “That particular [stability] threshold amplitude is identical with the ... amplitude ... at which the backbones ... intersect.” If he were to read [3], instead of Mr. Henry’s contribution, he would discover that we deduced the crossover amplitude by one method, and the stability threshold amplitudes by a totally different one. We then noted the coincidence of the former with one of the latter, and we reported this observation. Had we attempted to explain the mechanism of the instability by means of the crossover amplitude, or had we used one of these amplitudes to deduce the other, we would indeed have endorsed this coincidence with special significance; this, however, is not the case. While we made no special claims regarding the significance of this observation, we do claim that it is correct. Mr. Henry’s belief that “it seems improbable from the outset” may cast some doubt on its correctness as well. Yet, it can be deduced without difficulty from his own paper [5] which bears some gratifying resemblances to [3].

Mr. Henry’s statement (iii) is not at variance with our own views. It is merely a different interpretation of the basic instability phenomenon discussed by us in [3] and later by him in [5]. Suppose a nonlinear system having degrees of freedom x and y and solutions $x = x(t)$, $y = y(t)$. For the systems examined in [3], there is one set of initial conditions for which $y(t)/x(t) = 1$, and another for which $y(t)/x(t) = -1$. The first is called mode 1 in [5] and e-mode in [3], while the second is called mode 2 in [5] and o-mode in [3]. Each represents a straight line in the xy-plane passing through the origin at 45 deg to the x-axis; evidently, these lines intersect each other orthogonally. Using Mr. Henry’s terminology, let us assume that mode 1 vibrates while mode 2 is at rest. Then, as has been shown in [1], the line $y = x$ may be regarded as the trajectory of a unit mass oscillating back and forth along that line. Let us now suppose that mode 2 is parametrically excited. In that case, the unit mass will no longer oscillate along the line $y = x$ because a component normal to that line will contribute to its motion. Then, we say that motion along $y = x$ is unstable, or mode 1 is unstable, while Mr. Henry insists that it is actually mode 2 which is unstable.

Mr. Henry’s statement (iv) is not quite clear because he does not say what he means by “unsymmetrical.” If he uses that term as defined in [4], his statement is certainly incorrect. In [4] and [1], a class of unsymmetrical systems has been treated, and it was shown that the existence of normal vibrations is identified with the existence of real roots of a transcendental equation. In [4], it was shown for a subclass that two distinct real roots exist, and upper and lower bounds were given for each.

Mr. Henry’s statement (iv) is a repetition of a conclusion reached in his own paper [5]. In that paper he agrees that the term “normal mode” may be used when the motion can be defined in terms of a single co-ordinate—that is, when the equations of motion can be decoupled—and his method of normalizing them is strikingly similar to that found in the well-known, but unrefined, book by Kauderer. But Kauderer restricts himself to linearizable systems in which the potential energy of sufficiently small motions is given by the quadratic terms only. Kauderer concludes correctly that, as in linear systems, the intersection of the normal trajectories at the origin is orthogonal. Hence, if these trajectories are straight lines (“linear normal co-ordinates” in the nomenclature of [5]), they are orthogonal everywhere. It is evident that Kauderer’s procedure excludes all systems for which the “linear normal co-ordinates” are not orthogonal. Mr. Henry, having used essentially the same method as Kauderer, is in essentially the same position. But, the unsymmetrical systems of [4] and [1] are not linearizable, and in [1] it was demonstrated that their “linear normal co-ordinates” do not intersect orthogonally; in consequence, their existence escaped Mr. Henry’s analysis. However, failure to discover should not be confused with demonstration of nonexistence.

Minimum Transfer Time for a Power-Limited Rocket

F. N. Edelbaum

One of the current problems in the development of electric propulsion systems is whether the improvements in performance due to thrust modulation are worth the requisite increases in system complexity. It is well known that theoretical
The Effect of a Longitudinal Gravitational Field on the Super-cavitating Flow Over a Wedge


The author has given an elegant solution to a difficult problem of practical importance. It is worth noting that within the framework of the linearized free-streamline theory he has been able to account exactly for the influence of gravity. This is the first exact solution which has been obtained under such general conditions (we are excluding certain well-known but very special solutions from the nonlinear theory of cavity flows).

In his introductory remarks the author indicates the existence of some work by the writer in which the effect of a gravity field normal to the direction of a cavity flow has been given a simplified treatment. Although this simplified treatment is not capable of describing cavity flows with large buoyant effects, the resultant approximation is consistent with the scheme of linearization and should give results valid for that Froude-number range in which the effect of gravity, like the effects of the body, are of first order in smallness. In this linearized theory the solution is obtained by summing a number of fundamental singularities in much the same manner as has been illustrated in the present paper. Therefore the writer will resist the temptation to present any of these details. However, since his findings tend to complement those of a fully cavitated flat-plate hydrofoil in a flow of unlimited extent. The closed-cavity linearized theory was used to consider the effect of gravity acting in the downward direction.

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Fig. 1 Effect of gravity on calculated cavity shapes

Fig. 2 The effect of gravity on lift coefficient