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Reply by Authors to K. W. London

T. R. Kane*
Stanford University, Stanford, California
R. R. Ryan†
University of Michigan, Ann Arbor, Michigan
and
A. K. Banerjee‡
Lockheed Missiles & Space Company, Sunnyvale, California

As was stated at the outset in the paper[1] that is the subject of Lips' Comment, the paper was written with two objectives in mind, namely 1) to present an algorithm that can be used to predict the behavior of a cantilever beam when the base to which the beam is attached undergoes general three-dimensional motions, and 2) to draw attention to fundamental limitations of certain publicly available multibody computer programs. Because we regard the second of these as more important than the first, we shall deal mainly with those of Lips' statements that appear to be relevant to this issue.

To determine whether or not it was desirable at the time our paper was published to attempt to dispel the notion then widely held in the aerospace industry that one could use certain publicly available computer codes to simulate correctly the motions of multibody systems containing flexible bodies, it is only necessary to recall that this paper and, in particular, the beam spin-up problem described therein, provided, at least in part, the motivation for convening a workshop[2] at the Jet Propulsion Laboratory, Pasadena, California, to examine the role and status of multibody codes. Now, to prevent confusion from arising in connection with issues that were clarified at the workshop, misconceptions underlying the comments made by Lips under the heading of "Role, Status of Multibody Codes" should be pointed out. In particular, attention needs to be refocused on the fact that the flawed multibody codes lead to dynamic softening at *all* rotational speeds at which dynamic stiffening is to be expected, which makes it impossible to determine a priori whether or not a given simulation performed with such a code will be acceptable. This point is so important that we feel obliged to pursue it briefly in the context of a specific example that will allow us to deal incisively with Lips' comments on this issue.

Figure 1a shows a spacecraft supporting two cantilever beams. In Fig. 1b, the spacecraft is represented by a rigid body B; the beams are replaced with massless rods of length l; each rod carries at its end a particle of mass m, and is attached to B at a distance b from B^* (the mass center of B); and linear torsion springs connect the rods to B, each spring having a spring constant of σ units of moment per unit of rotation.

Considering only motions of B during which B rotates about a fixed axis passing through B^*, this motion being characterized as a function of time t by the angle $\phi(t)$, one finds that the exact, fully nonlinear differential equation governing the angle $\theta(t)$ (which plays the role of a beam deflection function) is

$$\ddot{\theta} + \left(\frac{\sigma}{ml^2}\theta + \frac{b}{l}\dot{\phi}^2\sin\theta\right) + \ddot{\phi}\left(1 + \frac{b}{l}\cos\theta\right) = 0 \tag{1}$$

and linearization of this equation in θ leads to

$$\ddot{\theta} + \left(\frac{\sigma}{ml^2} + \frac{b}{l}\dot{\phi}^2\right)\theta + \ddot{\phi}\left(1 + \frac{b}{l}\right) = 0 \tag{2}$$

which is analogous to the equations of motion presented in Ref. 1. By way of contrast, the equation of motion formulation process underlying "conventional" multibody codes, yields the equation

$$\ddot{\theta} + \left(\frac{\sigma}{ml^2} - \dot{\phi}^2\right)\theta + \ddot{\phi}\left(1 + \frac{b}{l}\right) = 0 \tag{3}$$

which differs from Eq. (2) as regards the coefficient of the $\dot{\phi}^2$ term. In Eq. (2), this term has a *stiffening* effect, whereas in Eq. (3) it produces *softening*. Finally if one drops this term altogether, one arrives at what we shall call the "crudely

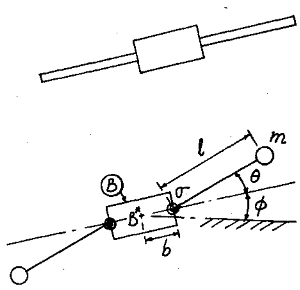

Fig. 1 Spacecraft supporting two cantilever beams.

Received Sept. 26,1988.

*Professor of Applied Mechanics.

†Assistant Professor of Mechanical Engineering. Member AIAA.

‡Senior Staff Engineer. Member AIAA.

linearized" equation

$$\ddot{\theta} + \frac{\sigma}{ml^2}\theta + \ddot{\phi}\left(1 + \frac{b}{l}\right) = 0 \tag{4}$$

Lips, after raising the question "When is foreshortening significant and when is it not?", avers that "the impact of this effect can be expected to be quite small for the majority (but not all!) of applications in space," and cites as a basis for this statement work done with the program TREETOPS on spacecraft slewing maneuvers. Now, TREETOPS is a program in which foreshortening is *not* taken into account. Equation (2) corresponds to taking foreshortening into account, whereas in Eqs. (3) and (4) this effect is neglected. Let us, therefore, examine the response predicted by each of Eqs. (1-4) when B undergoes a slewing maneuver during which ϕ changes in time T from zero to a final value ϕ_f, and is kept constant thereafter, which can be accomplished by taking

$$\phi = \frac{\phi_f}{T}\left(t - \frac{T}{2\pi}\sin\frac{2\pi t}{T}\right) \tag{5}$$

for $0 \leq t \leq T$ and setting $\phi = \phi_f$ for $t > T$.

For a 90 deg slew performed in 4 s, that is for $\phi_f = \pi/2$ rad and $T = 4$ s, and with $b = l = 1$ m, $m = 1$ kg, and $\sigma = \pi^2$ Nm/rad, the solutions of Eqs. (1-4) are plotted as four curves in Fig. 2. The curve identified by the symbol □ represents the solution of the *exact* equation of motion [Eq. (1)], and so it furnishes a perfect basis for assessing the merits of Eqs. (2-4).

During the first quarter of the slewing motion, that is, throughout the first second of the motion, the four curves are indistinguishable from each other. Clearly visible differences come into evidence during the second quarter of the slew ($0 \leq t \leq 2$ s), but here it is a bit difficult to tell which of Eqs. (2-4) leads to results that approximate best those obtained with Eq. (1). At $t = 3$ s, however, this determination can be made at a glance: the solution of the correctly linearized equation [Eq. (2)], identified by the symbol ○, furnishes a much better approximation to the exact solution of the problem than do the solutions of the prematurely linearized equation [Eq. (3)] (symbol △), and the crudely linearized equation [Eq. (4)] (symbol •). Moreover, at least at this instant, the crudely linearized equation produces a better result than does the prematurely linearized one. These conclusions also are seen to be valid throughout the remainder of the slewing motion, as well as subsequent to its conclusion, that is, for $t > 4$ s, where a striking phenomenon can be observed, namely that the solution of the prematurely linearized equation, that is, the equation corresponding to "conventional" multibody theory, is *180 deg out of phase* with those of Eqs. (1) and (2). In the light of these observations, it is noteworthy that, for $t > 4$ s, the absolute value of θ is at all times small (less than 0.05 rad), certainly smaller than the largest value of θ during the slew, which shows conclusively that "smallness" is no guarantee of validity. In other words, an inadequate theory should not be expected to reveal its own inadequacy. Lips' statement quoted earlier is misleading because his statement implies that a theory which is suspect can be self-validating. Lastly, the results at hand bring to light the fact that a crude theory, that is, one involving equations analogous to Eq. (4), can be expected to lead to results better than those obtained with a "conventional" theory, i.e., one in which foreshortening is handled improperly. We included Eq. (4) and the associated curve to make this point because it has direct, practical implications: Certain existing multibody codes could be modified along this line, and their utility would be enhanced by such modifications.

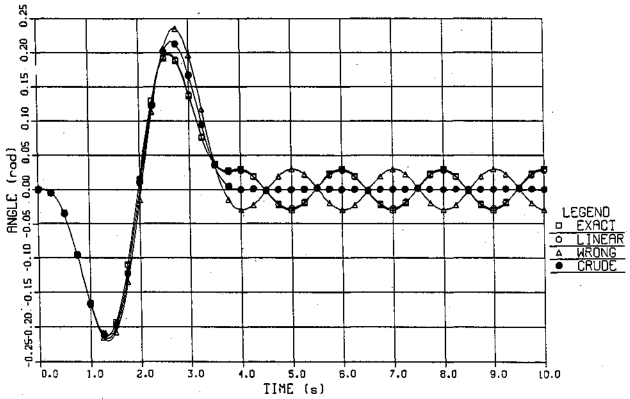

Fig. 2 Comparison of four solutions to the slewing problem.

As regards Lips' comments under the heading "Treatment of Kinematic Nonlinearity," these seem to be attributable to misunderstandings that, we hope, are not shared by other readers; and, since Lips does not appear to question the validity of our results, we shall respond only briefly.

Equation (19) of Ref. 1 is a relationship that follows directly from the definitions of the symbols appearing in the equation; the three elastic displacements are represented by s, u_2, and u_3, not by five variables; Eq. (41) yields space-dependent, not explicitly time-dependent modal integrals, and ζ creates no problems whatsoever computationally; consistency in linearization makes it unnecessary to consider the effect of foreshortening on elastic strain energy.

Finally, a few words in reply to Lips' rather strong comments under the heading of "Treatment of the Literature." The charge that we attempted "to lump together much of the work on beam dynamics under the umbrella of the 1974 review paper" by V. J. Modi is not only ill founded, but seems to denigrate a useful paper, which is unfortunate. Next, Lips takes issue with our statement that Hodges' Note, cited by Lips as Ref. 20, and the Kaza-Kvaternik paper he lists as Ref. 2 involve "analyses performed in connection with aircraft dynamics." Dealing with large deflections of beams, Hodges addresses issues of direct concern in connection with helicopter rotor blade analysis; the fact that this Note, to use Lips' words, "does not deal with any specific configuration" in no way invalidates our statement; and it is obvious that the rotating beams in the Kaza-Kvaternik paper are meant to be helicopter rotor blades. As for what we said about the Likins and Vigneron papers cited by Lips as Refs. 22 and 9, we continue to believe that these papers are of interest regarding the effect of vehicle elasticity on attitude motions, which is all we said about them.

Meirovitch's treatment of the topic of foreshortening requires separating inertial forces into two types, namely, those involving motions relative to a rotating reference frame and those involving motions of the rotating frame itself. For any problem more complicated than that of a simple beam spinning with a constant angular speed in a plane, it is not explained how to carry out this separation or how to obtain a solution without doing so. Therefore, this approach reasonably may be characterized as suitable "for the particular end or case at hand without consideration of wider application," which is the Webster's dictionary definition of ad hoc, a term we do not regard as disparaging. Concerning Refs. 21 and 23 of our paper, we believe Lips missed the point that so-called gyroscopic and centrifugal modes do not provide the means to model *arbitrary* beam motions. Lastly, we chose not to mention the papers listed by Lips as Refs. 10–16 and 26 because, dealing primarily with the deployment of appendages, they did not seem to us to have a direct bearing on the subject of our paper.

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