LETTERS, OPINIONS, AND COMMENTS

The author responds

[David Levinson's review of *Spacecraft attitude dynamics* appeared in AMR v 39 n 8 (Aug 1986) p 1190.]

Although the review was, as a whole, not negative, it is the book's alleged weaknesses that move me to respond.

"Weakness" No. 1 is a purely semantic issue dealing with what one means, precisely, by a "reference frame." Concerning "Weakness" No. 2 ("no clear operational definition of angular velocity"), I had assumed that the elementary concepts of velocity and angular velocity were already familiar to my readers; nevertheless, a precise operational definition was, in fact, given by Eq. (12) on page 24.

As to "Weakness" No. 3, it is alleged that my "treatment of the energy sink method is of concern, for it is such as to lead readers to believe that this method is accepted universally as a dependable technique, whereas it is, in fact, a difficult matter to determine the extent to which the results of an energy sink analysis can be trusted." What I *actually* said was the following: "energy dissipation will be represented heuristically by the Energy Sink Hypothesis in Chapters 5 and 7" (p 3); "The effects of energy dissipation are studied below in several stages. The first analysis, based on the so-called energy sink hypothesis, is semiquantitative in nature" (p 140); "... the results are in general agreement with, but more precise than, the energy sink results" (p 193); "It should be remembered that in spite of its importance in providing guidance on how directional stability may generally be achieved, the energy sink hypothesis does not produce an exact analysis and therefore cannot be relied upon for precise stability conditions" (p 217); "The preceding two analyses (energy sink and subjoined damping matrix) have the virtue of simplicity but are not especially rigorous" (p 385). It seems clear that the reviewer and I are, in fact, basically in agreement on this subject.

"Weakness" No. 4 (mercifully, the last) essentially refers to a modern quasi-theology about how motion equations should be formulated. Although the reviewer is a disciple of this new theology, I confess to being as yet an infidel, paying homage still to those ancient deities, Newton, Euler, and Lagrange. The reviewer's prophet is Thomas Kane of Stanford, who has propounded a thoughtfully worked-out new procedure for deriving equations of motion, a procedure that is meant to combine the best features of the various Newton–Euler–Lagrange methods (ie, of classical mechanics) and thereby to supplant these methods. Leaving aside the question of whether or not this lofty goal has in fact been achieved, I would like to point out two facts that will provide perspective to all but "true believers." First, the formulational part of the problem is only one small part of a much larger process of engineering dynamics analysis. (Less than 4% of my book was spent on formulation-dependent material; the other 96% was spent on aspects that I believe to be more important, including modeling assumptions, physical context, flight data, etc.) Second—and I believe this to be a very important factual statement—the reviewer's "Weakness No. 4" is a "weakness" of *every book on dynamics ever written*, except for the dynamics books written by Kane. This does not prove, of course, that Kane's formulations are not superior, but it does indicate rather dramatically that the reviewer has at least a smidgen of bias when almost 30% of his remarks are devoted to what is currently a partisan issue.

My last point is not addressed to the review itself but to the procedure used by *Applied Mechanics Reviews* in acquiring it. The only other book on the market which could reasonably be considered a competitor to my own (*Spacecraft attitude dynamics*, Wiley, New York, 1986) is *Spacecraft dynamics*, by Kane, Likins, and Levinson (McGraw–Hill, New York, 1983). With all the experts available on this subject, why choose an author of the only competing book to write a review?

In any case, the subject of spacecraft attitude dynamics is sufficiently important and complex that a mere two books devoted to it are not enough. I have recommended (and will continue to recommend) that all my graduate students get "Kane, Likins, and Levinson" in addition to my own book if they are to be serious students of the subject.

Peter C Hughes
University of Toronto
Institute for Aerospace Studies
4925 Dufferin Street
Downsview ON M38 5T6, Canada

AMR's response: We see nothing intrinsically wrong with a reviewer being the author of a competing book. You can sometimes get very interesting reviews that way. But it would have been better if we had mentioned the fact. All reviews are, of course, subject to editing.

