of the approximate solution if one blindly extended the integrations to $t=28$ sec using the same values of t_l and N as used for smaller values of t?

We believe that Ref. 4, when properly interpreted, is concerned with how one can achieve an approximate solution to motion problems when the differential equations can not be solved exactly. This has always been possible through the method of weighted residuals or the principle of virtual work. Indeed, Professor Bailey's applications of his Eq. (4) are simply Galerkin method approximate solutions of Lagrange's equations of motion after some integration by parts. Professor Bailey's presentation certainly serves to call attention to the need in his Eq. (4) for $(\partial T/\partial \dot{q}_i)\delta q_i$ when $\delta q_i \neq 0$; but the content of Eq. (4) is not one whit different from the Galerkin solution. Furthermore, as mentioned in Ref. 3, Hamilton's "Law of Varying Action" as presented in 1834 and 1835 is quite different from what Professor Bailey describes with the same title in his Eq. (4). His procedure is not "contrary to the state of energy theory found in textbooks and in the variational calculus,"[4] and he has not cleared up a 140 year mystery on how to achieve direct solutions. His contribution has been to provide some very excellent and informative examples of the application of Galerkin's method, demonstrating the accuracy possible for certain problems.

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Reply by Author to C.V. Smith Jr. and D.R. Smith

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FIRST, let us express our sincere appreciation to the Professors Smith for providing this opportunity. The tone of glee with which the professors choose the time and the place is duly noted.

It is impossible, within the page limitations imposed by the editors, to reply in detail to the very strong statements and serious charges made by the Professors Smith. However, let us begin.

The Professors Smith mention "concepts" in their second paragraph; but, they choose to ignore a large part of Ref. 1 and ignore completely Ref. 2. In Ref. 2 the following statements appear: "Not discussed in this paper are the concepts of the author..." and, "If there is no question about the origin of Hamilton's integral or about how and why the operator δ can be used as it has been used here..."

Obviously, there is now a question about the concepts because the Professors Smith have stated in their third paragraph, "This is wrong." Our theory indicated that it could be done. Our calculations prove that it is correct. A definitive paper on our concepts is yet to be written. As pointed out in Ref. 3, the reasons for this are set forth in Ref. 2 and Ref. 4. A copy of Ref. 4 is available from either the NASA Langley Research Center or from the author.

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The Professors Smith have presented specific examples and referenced definite equations in our paper which, according to their concepts, are "wrong" and "inconsistent." We must point out that the Professors Smith have not only ignored most of our published work, but also have not produced, with their concepts, direct analytical solutions to nonstationary systems from energy considerations alone. The key to this part of the problem lies in the last sentence of Paragraph Three of the Smith's comment. It is addressed further in Ref. 3. A complete explanation of this important point will be made, "...at another time in another place."[5]

In their seventh paragraph, they address another part of the problem – the term $(\partial T/\partial \dot{q})\delta q|_{t_0}^{t_1}$. In every text and every paper which we have examined, this term is set to zero at the outset, or it is ultimately set to zero because we are taught only Hamilton's principle which applies only to stationary systems.[6] The author of Ref. 6 is no exception. He chooses to ignore Hamilton's words in Hamilton's papers and thus credits Hamilton with the principle of least action. For emphasis, we refer the Smiths to the quote in Ref. 7 which was taken directly from Hamilton's paper. It begins thusly, "When this well known law of least, or as it might better be called, of stationary action,..."[8] This point is of importance because neither the Smiths nor anyone else has produced, nor have we been able to find, any evidence to indicate that direct analytical solutions to nonstationary systems have ever been achieved from energy considerations alone with the concepts which we have all been taught. Derivation of the differential equations of force equilibrium, which may then be solved (at least in principle) to obtain the time-space path and/or configuration, is not the same thing. Such a procedure introduces all of the complexities of the theory of solution of such equations. It is a matter of record that relatively few such equations associated with significant dynamical systems have ever been solved exactly.

Further along in their comment, when they illustrate their ideas for achieving an approximate solution, the Smiths demonstrate very effectively a major reason for the confusion[3] that surrounds the use of energy methods. Based on *a priori* knowledge of the solution, they find a special function, Eq. (6) of their Comment, which when substituted in Eq. (1) or Eq. (2), yields the already known result. By their own admission, generality is lacking and from their demonstration, simplicity most certainly is missing. We are surprised that the professors do not demonstrate the conversion of this simple initial value problem into an equivalent boundary value problem, or at least mention that this can and has been done. What does not surprise us is that they ignore our challenge to produce directly from Eq. (4), Ref. 7, without the necessity of finding special functions or converting to an equivalent mathematical system, the solution. Simplicity and generality as demonstrated in our published papers is not contained in the concepts employed by the Smiths; but, perhaps these features, particularly simplicity, are no longer of importance.[1]

The excellent accuracy, about which the Smiths comment, is not the result of the concepts of "approximate" and choice of "trial" functions as used by the Professors Smith. The results, of their concepts have been too adequately demonstrated over the years through the application of both the Rayleigh-Ritz and Galerkin methods; e.g., in Ref. 9 a trial function which satisfies all of the boundary conditions is used to obtain the lowest frequency and mode of a tapered, cantilever beam by the concepts of Rayleigh. The exact solution to the differential equation yeilds 2.4796. The judiciously chosen trial function, which satisfies *all* of the boundary conditions, yields 2.8127, which is 13% in error. A single term of a power series, which satisfies *only* the displacement boundary conditions and nothing else, yields 2.4980, which is 0.74% in error. This is not an accident. Reference 10 shows why.

In Ref. 11, the time-space displacement function is generated for a linear oscillator with initial conditions which produce the cosine function (a nonlinear oscillator is given in Ref. 2). This generated, analytical function is then differentiated by the exact, formal rules of the calculus to produce the sine function. After 100 full cycles of motion, the error was .0066% (Ref. 11 contains the result for only 25 cycles). Undergraduate students generate either of these functions, among others, as an exercise using 1, 2, or 3 terms of a power series. With only three terms, the results for the sine function are

$$(0 \leq \tau \leq 1.0 \text{ rad.}); \ \tau = 0.2, f(\tau) = 0.1990, \ 0.17\% \text{ error};$$
$$\tau = 0.6, f(\tau) = 0.56599, \ 0.24\% \text{ error}; \ \tau = 1.0,$$
$$f(\tau) = 0.8435, \ 0.25\% \text{ error}$$

With such results from only three terms, for the reasons given in Ref. 1 and Ref. 10, the results in our published work, where ten terms are standard, becomes understandable.

We are in no way constrained to the use of power series. The power series has served to prove our theory. However, with it we generate the functions needed over finite time and space domains as demonstrated in our published papers. The reason for these results has nothing to do with "trial" functions and "ficticious" displacements. With respect to accuracy, we find this passage in Hamilton's second essay,[12] "This general method is founded chiefly on a combination of the principles of variations with those of partial differentials... When applied to the integration of the equations of varying elements, it suggests, as is now shown, the consideration of a certain Function of Elements, which may be variously chosen, and may either be rigorously determined, or at least approached to with an indefinite accuracy..." Our published work and the many other examples in our files demonstrate the truth of this statement.

We humbly beg to differ with the Professors Smith about singularities. Singularities, in fact, do not occur in mechanical systems (don't cite shock waves or any other physical phenomena. We have spent too many hours of our life observing such so-called discontinuous phenomena). All change in nature requires a change in time and/or displacement, which is change in time-space. Singularities are the result of mathematical models of physical phenomena. The mathematical model, whether or not it contains sinugularities, for the purpose of the analyst, may be adequate. But an exact mathematical solution to the mathematical model is just that, an exact solution to the mathematical model. The adequacy of the model must ultimately be judged by comparison of the *numbers* produced from its solution to the *numbers* produced by a valid experiment. It should be noted that in four years of application of our concepts, no singularity has been encountered when the mathematical model is made sufficiently realistic. A case in point is the complete cone. The direct solution is compared to the "exact" solution as an example in Ref. 13.

When the Professors Smith address the problem of the axial wave propagation in a rod, they truly speak from their frame of reference. Axial propagation of transverse waves is shown in Ref. 14. Axial propagation of axial waves poses no particular problem in time-space.

In their final paragraph, the professors interpret the results of our work on the basis of their concepts. It is our sincere belief that it was such interpretations of Hamilton's Law of Varying Action from the frame of reference of the variational calculus and of force equilibrium that has produced the confusion[3] which is so adequately demonstrated throughout the literature. The confusion of the Smiths is apparent when they state, "...the content of Eq. (4) is not one whit different from the Galerkin solution." The difference is pointed out in Ref. 14.

The concepts associated with Galerkin's method as with the concepts associated with the Rayleigh-Ritz method have obviously inhibited the use of these methods, particularly the Rayleigh-Ritz method.[15] Reference 11 demonstrates for the Smiths that it is not the Galerkin method, but the theory of Ritz which applies when our concepts of time-space continuity are employed. Yet, we find the theory of Ritz, except for its association with the approximation concepts of Rayleigh in the principles of minimum potential energy and minimum complementary energy, has been virtually ignored by the mathematician. We have been unable to find where it has ever been successfully applied to nonstationary systems through the concepts set forth by the Professors Smith.

Finally, the Smiths state, "Hamilton's Law of Varying Action as prescribed in 1834 and 1835 is quite different from what Professor Bailey describes with the same title in his Eq. (4)." Our response is, by whose authority? For a very sound reason, we have omitted the term which involves the variation of time.[2] We do not omit the term which provides the key to direct analytical solutions to nonstationary systems from energy considerations alone; but, this term, as pointed out, is not the whole story. Our concepts are different.[2] To obtain a solution from the differential equation of force equilibrium, it is *necessary* to apply continuity conditions. To obtain a solution from Hamilton's law, it is *sufficient* to apply continuity conditions; application of the equilibrium conditions are not necessary.[1,2,10,11,14]

Again, we thank the Professors Smith for their comments. In particular, we appreciate the statements, "This is wrong," and, "There are internal inconsistencies...;" because, we are not wrong, and there are no inconsistencies. The definitive paper mentioned in this reply and in Ref. 3 has now been submitted.[16]

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