Technical Comments

Comment on "Application of Hamilton's Law of Varying Action"

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In a recent Comment[1] on our paper,[2] Professor Bailey made several statements to which we responded in our Reply.[3] That Reply closed with the remark that further comment on Professor Bailey's procedure would be more appropriate at another time in another place; now is the time, and this is the place. Many of the concepts in Ref. 1 are repeated in Ref. 4, so that of necessity some of our statements in Ref. 3 will be repeated in this Comment.

Professor Bailey's Eq. (3) in Ref. 4 is an incorrect statement of Hamilton's principle for general application. Just below that equation, one reads that "it is understood that W is the work of both conservative and nonconservative forces," and then Eq. (12) purports to offer a work function when there exist dissipative forces and time dependent forces. This is wrong. By definition, a work function does not exist for nonconservative systems since a work function is conventionally merely the negative of the potential energy, and potentials do not exist for nonconservative forces.

There are internal inconsistencies between Eq. (2), Eq. (12), and what is actually used to get the numerical results of Figs. 3 and 4. Assume that $W = W(q_i, \dot{q}_i)$ as shown in Eq. (12). Then Eq. (2) implies that

$$Q_i = \frac{\partial W}{\partial q_i} - \frac{d}{dt}\left(\frac{\partial W}{\partial \dot{q}_i}\right)$$

with no indication of what happened to the term

$$\frac{\partial W}{\partial \dot{q}_i}\delta q_i \Big|_{t_0}^{t_1}$$

However, using Eq. (12) for W, this definition of Q_i gives, for example,

$$Q_2 = -2k_2 q_2 + T_2(t) - m_2 g L_2 \sin(q_1 + q_2)$$

This is surely not what was used to obtain Figs. 3 and 4 since there is no damping force; and, we suspect, there is an unwanted factor of 2 in the spring force term.

The correct expression for Q_2 is

$$Q_2 = -c_2\dot{q}_2 - k_2 q_2 + T_2(t) - m_2 g L_2 \sin(q_1 + q_2)$$

which then raises the question of how this relation is derived by substituting Eq. (12) into Eq. (4). It can be derived only by an improper variational operation on W in which $W(q_i, \dot{q}_i)$ is treated differently from $T(q_i, \dot{q}_i)$. Incorrect and inconsistent treatment of terms has led to a correct answer, but this provides a shaky foundation for any theoretical development.

One final observation on this point. For any force system, one can always impose a virtual displacement and calculate the work done by those forces. This work might be denoted by δW. However, this symbolism of δW does not mean that there necessarily exists a function W whose (proper) first variation is equal to the virtual work.

Professor Bailey's Eq. (4), with the obvious typographical correction, contains a term $(\partial T/\partial \dot{q}_i)\delta q_i|_{t_0}^{t_1}$. He then states "The term $(\partial T/\partial \dot{q}_i)\delta q_i$ does not in general vanish at t_1, as taught by the variational calculus." First it should be stated that the variational calculus does not "teach" that $(\partial T/\partial \dot{q}_i)\delta q_i = 0$ at t_1. There may be very sound reasons for considering $\delta q_i = 0$ at $t = t_1$ (for example, if something must be true for all δq_i then it must certainly be true for those δq_i which vanish at t_1; but it is not a tenet of the calculus of variations that $\delta q_i = 0$ at t. The variational calculus teaches how to determine the variations of functionals and how to solve optimization (or minimum, or maximum) problems arising in many areas. However, the variational calculus does not set the problem. The functional is established by the physics of the problem, and the physics establishes what one does with the variation of that functional.

Reference 4 shares with Ref. 1 the confusion between real and virtual displacements. As pointed out in Ref. 3, the analyst has the freedom to make $(\partial T/\partial \dot{q}_i)\delta q_i$ equal to zero at any time simply by selecting $\delta q_i = 0$ at that time. Virtual displacements do not have to be variations of (have the same form as) real displacements.

With the limited information available when preparing Ref. 3, we were able to state that Professor Bailey's application of what he calls the law of varying action "is simply an incremental, weighted residual (in particular, Galerkin's method) approximate solution to the differential equations of motion, with trial functions which satisfy initial conditions." We see no reason to change this statement after reading Ref. 4.

One integration by parts of Professor Bailey's Eq. (5) will give

$$\int_{t_0}^{t_1} (F - kq - c\dot{q} - m\ddot{q})\delta q dt = 0 \tag{1}$$

This expression is more simple in form than Eq. (5); and, with Professor Bailey's smoothness requirements on the real displacements, the two equations are *completely* equivalent for *any* continuous, piecewise differentiable virtual displacements. If virtual displacements are selected which vanish at t_0 and t_1 (and this is a perfectly valid choice), then Eq. (5) becomes

$$\int_{t_0}^{t_1} [m\dot{q}\delta\dot{q} + (F - kq - c\dot{q})\delta q] dt = 0 \tag{2}$$

Now, Professor Bailey may prefer the point of view which states that his Eq. (5) can be used to obtain a solution to the problem "without any reference to or knowledge of differential equations."[4] However, our Eq. (1) makes it clear that, in reality, one actually is working with the differential equation of motion. One might absolutely refuse to invoke the fundamental lemma of the calculus of variations and extract the differential equation, but it is still there in the integrand.

Suppose the differential equation is known, but it is impossible to obtain an exact solution in terms of functions which have been well-studied. Then an approximation could be obtained by beginning with a trail solution in the form of

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Professor Bailey's Eq. (6). As he observes, the conditions at t_l are not known in advance; but this need have nothing to do with the virtual displacements at t_l.

Two interpretations can be given to the use of our Eq. (1) to continue the approximate solution. If δq is expressed as a linear combination of independent functions, then one has an application of the mathematical procedure known as the method of weighted residuals. Or, some might prefer a more physical interpretation which recognizes that Eq. (1) is a statement of the principle of virtual work including inertia forces. In either case, δq need have no relation to q.

These ideas can be illustrated by the following simple example. Consider a particle of mass m, constrained to linear motion under the influence of a force $F = F_0 + F_l t$. The differential equation is given by

$$\ddot{q} = (1/m)(F_0 + F_l t) \tag{3}$$

with the solution of

$$q = q_0 + \dot{q}_0 t + (F_0/2m)t^2 + (F_l/6m)t^3 \tag{4}$$

where q_0 and $\dot{q}_0$ are the displacement and velocity, respectively, at $t=0$. This result can be obtained "without any knowledgment of or use of the theory of differential equations"[4] as follows. Assume

$$q = q_0 + \dot{q}_0 t + \sum_{i=2}^{N} A_i t^i \tag{5}$$

$$\delta q = \sum_{j=2}^{N} B_j (t^j - t t_l^{j-1}) \tag{6}$$

Note that the virtual displacement is not the variation of the real displacement, and $\delta q = 0$ at $t=0$ and $t=t_l$. Substitution of Eqs. (5) and (6) into Eq. (1) or Eq. (2) or into Professor Bailey's Eq. (5) leads (with the arbitrariness of the B_j) to the following set of equations

$$A_2 + \frac{2(j+1)}{j+2} t_l A_3 + \sum_{i=4}^{N} \frac{(i-1)(j+1)}{i+j-1} t_l^{i-2} A_i$$

$$= \frac{F_0}{2m} + \frac{F_l}{3m}\frac{j+1}{j+2} t_l, \tag{7}$$

with $j = 2,3,\ldots, N$. The solution is given by

$$A_2 = (F_0/2m),\ A_3 = (F_l/6m),\ A_i = 0 \text{ for } i = 4,5,\ldots N, \tag{8}$$

and Eq. (4) has been obtained.

Now assume

$$\delta q = \sum_{j=2}^{N} B_j t^j$$

as used by Professor Bailey. Since $\delta q \neq 0$ at t_l, our Eq. (2) is no longer applicable (which means, incidentally, that Eq. (5) is not a suitable trial function for evaluation of the action and direct use of Hamilton's stationary principle). However, Eq. (1) or Professor Bailey's Eq. (5) can be used to give

$$\frac{F_0}{m}\frac{1}{j+1} + \frac{F_l}{m}\frac{t_l}{j+2} = \frac{2}{j+1}A_2 + \frac{6}{j+2}t_l A_3$$

$$+ \sum_{i=4}^{N} \frac{i(i-1)}{i+j-1} t_l^{i-2} A_i,\ j = 2,\ldots,N \tag{9}$$

with solution given once again by Eq. (8).

For this very simple example, any reasonable choice of virtual displacements and the use of Eq. (1) lead to the correct answer because the trial family of Eq. (5) includes that exact answer. For more complicated problems, the result may be only an approximation of the exact answer, with the adequacy of the approximation dependent upon how many terms are included in the trial family and how long the time interval from t_0 to t_l. Which leads to the observation that Professor Bailey did not supply information on how many terms he used with the indicated values of t_l for each problem solved; it would be interesting to learn what was necessary to achieve the excellent accuracy he reports.

Figure 5 of Ref. 4 could be interpreted to mean that the system truly reaches the indicated final configuration in a finite time. This, of course, cannot be exactly true, although the accelerations may be so small at $\tau = 120$ that the system is essentially in static equilibrium.

Professor Bailey's smoothness requirements on the real displacements are restrictive and exclude various useful mathematical models of shock phenomena or singularity propagation phenomena. An engineer might say that singularities do not truly occur in nature. However, simple mechanical and mathematical models often predict singularities; and this information is useful to the engineer in such problems as longitudinal wave propagation in a rod. The theory of differential equations allows one to study such problems. Trial functions of the form of Professor Bailey's Eq. (6) will not provide the true solution no matter how small a value for t_l or how large a value of N. The method can not provide a "complete and general analytical solution" for all initial value problems in the mechanics of solids, and the approximate solutions generated can not "yield all information available from the laws of mathematics, without reference to the theory of differential equations."

As a further illustration of possible shortcomings of the solution procedure presented in Ref. 4, consider the differential equation given in Fig. 1, recorded as follows

$$(0.25 - 0.01t)\ddot{q} + (0.001 + 0.01t)\dot{q} + (0.05 + 0.005t)q$$

$$= F_0 - (0.25 - 0.01t)g \tag{10}$$

The theory of differential equations indicates[5] that $t = 25$ sec (or $\tau = 6.25$) is a regular singularity of Eq. (10), suggesting the possibility of "unusual" behavior of the solution in the neighborhood of $t = 25$ sec. Of course, for this problem, there are physical reasons for recognizing $t = 25$ as a special point and restricting the solution to $t \leq 25$ (avoid negative mass). It can be shown by the theory of differential equations that in the neighborhood of $t = 25$ sec the solution to Eq. (10) is bounded but cannot be expanded in a Taylor series. This true solution can never be represented by Professor Bailey's Eq. (6).

One final example is a modification of Eq. (10), as follows

$$(0.25 - 0.01t)\ddot{q} + (0.20 - 0.01t)\dot{q} + (0.05$$

$$+ 0.005t)q = F_0 - (0.25 - 0.01t)g \tag{11}$$

It is recognized that Eq. (11) might represent an unrealistic physical system since the damping becomes negative for $t > 20$ sec. However, the differential equation makes sense mathematically; and any solution procedure claiming to "yield all information available" must be able to show that the system response is in general unbounded in the neighborhood of $t = 25$ sec. It is not enough to merely know from the physics of this relatively simple problem that zero mass and negative damping will result in unbounded motion. Once again, Professor Bailey's Eq. (5) and (6) can never give the true solution. There may be some indications of growing amplitude as t approaches 25 (it would be very instructive to see a plot of the approximate solution), but this is not the same as knowing the exact solution. Suppose the engineer did not realize that a singularity existed, which might be possible for more complicated problems. What would be the appearance 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.

References

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[3] Smith, D. R. and Smith, C. V., Jr., "Reply by Authors to S. F. Felszeghy and C. D. Bailey," *AIAA Journal*, Vol. 13, Nov. 1975, pp. 1540-1541.

[4] Bailey, C. D., "Application of Hamilton's Law of Varying Action," *AIAA Journal*, Vol. 13, Sept. 1975, pp. 1154-1157.

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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 boun-