References

Kane, T. R., and Levinson, D. A., 1988, "A Method for Testing Numerical Integrations of Equations of Motion of Mechanical Systems," ASME JOURNAL OF APPLIED MECHANICS, Vol. 55, pp. 711–715.

Kane, T. R., and Levinson, D. A., 1990, "Testing Numerical Integrations of Equations of Motion," ASME JOURNAL OF APPLIED MECHANICS, Vol. 57, pp. 248–249.

Author's Closure[18]

I became aware of Kane and Levinson's (1990) corrective note, to their earlier (1988) paper, late in the spring of 1990; by then I had already completed my paper, sent it, and revised it (Dec. 1989). I believe, however, that Papastavridis (1991) deserved publication anyway for the following prominent reasons:

1 It is formulated in terms of the most general nonholonomic and rheonomic system variables and constraints, and is thus in line with standard advanced dynamics;

2 It makes explicit the energetic effects of nonholonomic (= nonintegrable) rheonomic constraints; and

3 Since it is clearly motivated and fairly self-contained, it can serve as a timely theoretical tutorial for both students and researchers.

All these advantages are absent from Kane and Levinson's "raw" and structureless treatments. In fact, even if Kane and Levinson's (1988) results were correct, it would still take a paper to demonstrate their equivalence with mainstream dynamics.

Dynamics of Nonholonomic Mechanical Systems Using a Natural Orthogonal Complement[19]

John G. Papastavridis[20]. The paper contains a number of theoretical and historical inaccuracies:

1 The opening statement, "The theory of nonholonomic systems arose when the analytical formalism of Euler and Lagrange was found to be inapplicable to the very simple mechanical problems of rigid bodies rolling without slipping on a plane," is eminently false. It is well known that the method of Euler, i.e., the principles of linear and angular momentum, applies to *any* mechanical system, including rolling ones. As for the method of Lagrange, his "Principle of Lagrange" (LP) of 1764 holds for any mechanical system subjected to Pfaffian constraints, holonomic (H) or not. The entire constrained system mechanics rests on LP. Higher-order (or nonlinear) constraints, e.g., control/servo-constraints, require the related higher principles of Gauss/Gibbs, Mangeron/Deleanu, etc.

The original Lagrangean equations of motion of 1780 were indeed restricted to H constraints. However, that limitation of Lagrange's equations, but not of Lagrange's Principle, was easily removed by Routh in 1877: By applying the method of "Euler-Lagrange Multipliers" to LP, he obtained Lagrange-type equations for systems under Pfaffian constraints—H or not. Also, Lagrange himself states in his "Méchanique Analitique" of 1788, p. 46"... in general we shall represent, by $dL=O$, $dM=O$, ..., the equations of condition [i.e., constraints] between these differentials, whether these equations are themselves exact or not, as long as the differentials do not appear except linearly."

2 The fundamental concept of nonholonomicity (NH) was indeed made precise in 1894 by Hertz; although special such problems had been successfully studied by Ostrogradsky in 1858, Minding in 1864, Ferrers in 1873 (see Fig. 1), C. Neumann in 1885, Vierkandt in 1892, et al. But nowhere in their article do the authors supply even a hint as to the precise meaning of NH. Clearly not every rolling constraint is NH; while (outside of control/servo problems) every constraint is or can be brought to Pfaffian form. How does one detect, analytically or otherwise, the NH (≡nonintegrability) or not of a given velocity constraint? Such properties affect critically the equations of motion and their solutions, say, in configuration space; and also their time-integral variational principles. I suspect that the authors (contrary to standard terminology followed by the masters of mechanics for the last hundred years) use the terms kinematic, velocity, and NH synonymously.

3 The relationships among the various equations of motion in NH coordinates and/or NH constraints are not explained. What are their similarities/differences and strengths/weaknesses? And how does one pass from one of them to another?

The complete history of this fascinating topic has never been written. Within the limits of this Discussion I can only say that these equations fall into (i) those based on the kinetic energy T, and (ii) those based on the Gibbs/Appell acceleration energy S. (A third group, initiated by Nielsen in 1935, is based on the gradients of $\dot{T}\equiv dT/dt$. No account of it exists in English.) Both T and S-equations are further subdivided into those in H and NH, or quasi-variables. The most general NH variable T-equations are due to Hamel (1903, 1904), appropriately dubbed by him "The Lagrange-Euler Equations." They uncouple naturally into reaction-free (or kinetic), and reaction containing (or kinetostatic) ones, and, although unmentioned by the authors, constitute the theoretical pinnacle of Lagrangean dynamics. The equations of Ferrers (1872, 1873—what Whittaker (erroneously) calls Ferrers' equations are in reality the Routh equations), C. Neumann (1885), Chaplygin (1895, 1897), Volterra (1898), Appell (1899—not his other famous S-equations), Carvallo (1900, 1901), Voronets (1901), Poincaré (1901), Boltzmann (1902), McMillian (1936) et al., are all special cases of Hamel's; and all result from LP. Figure 1 shows them in increasing order of conceptual power, simplicity, and generality (→). The most general H-variable T-equations are due to Maggi (1896, 1901). A special variable (kinetic) case of them is due to Hadamard (1895, 1899); it was rediscovered, in 1973, by Huston and Passerello. The kinetic and kinetostatic Maggi equations result, respectively, by projection of the Routh equations on the local virtual plane (or null space—the authors' "natural orthogonal complement") and on the normal to it (or range space); Maggi's method also applies to Hamel's equations under additional constraints.

The S-equations, also based on LP (and related to the T-equations by kinematical identities), subdivide into those in H and NH variables. The earliest S-equations in NH variables, but without constraints, are due to Gibbs (1879); and for general Pfaffian constraints and special quasi-variables are due to Appell (1899, and several papers afterwards, until 1925). (The so-called "Kane's equations" of 1961 are theoretically identical to them; and the more general Kane's equations of 1966, are nothing but a special case of the very general nonlinear NH constraint, Schaefer equations of 1951; and all are raw forms of general Hamel results from 1927, 1938, 1949).

4 The statement, "Neimark and Fufaev (1967) gave the first comprehensive and systematic exposition of the mechanics of nonholonomic systems ...," would have been correct had the authors added to its end the words "in English." The earliest systematic reference is due to Heun (1906, in German);

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[19]By S. K. Saha and J. Angeles and published in the March 1991 issue of the JOURNAL OF APPLIED MECHANICS, Vol. 58, pp. 238–243.

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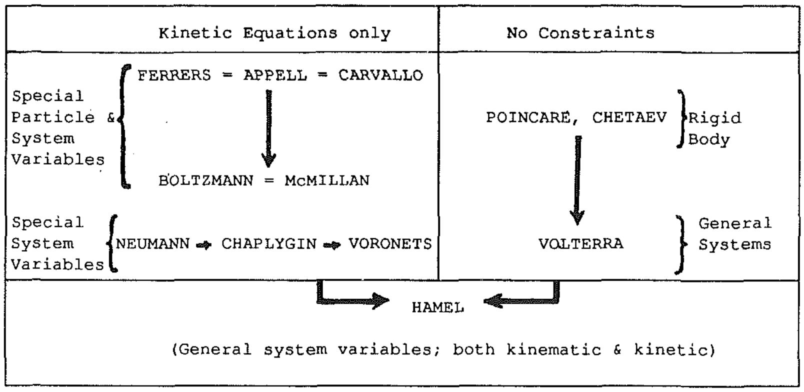

Fig. 1

other classic treatments are due to Prange (1935, in German), Hamel (1949, in German), and Lur'e (1961, in Russian; 1968, in French). (See references of Papastavridis, 1990).

5 The frequent term "nonworking" must be replaced by "virtually nonworking," otherwise, constraint reactions may be working.

6 The term "generalized speed ($-s$)" is incorrect; speed is the (non-negative) magnitude of velocity. The precise term is (contravariant) nonholonomic component ($-s$) of the velocity vector (or nonholonomic velocity (ies), or quasivelocity (ies)); just as the familiar $\dot{q}_k$ are its holonomic components—see e.g., Schouten (1954).

7 The authors' term "scleronomic" ("rheonomic") must be replaced by the correct one "catastatic" ("acatastatic") (= linear and homogeneous (nonhomogeneous) in the velocity components but with, possibly, time-dependent coefficients); i.e., homogeneous constraints may still be rheonomic. And although Pfaffian constraints can indeed always be brought to castastatic form, it is not explained how this is done and what it means. The authors' rolling constraint (31) is catastatic because their plane (and coordinates used) are inertial. If it had a given motion their constraint could be made catastatic, but it would in general be rheonomic. And how does one handle acatastatic constraints if one must?

8 The authors' Eqs. (17), (35) are very special cases of Hamel's equations; also, in Eq. (35), ω should be $\dot{\omega}$.

9 Finally, below Eq. (18), why not use the standard dynamics term gyroscopic, instead of the (continuum mechanics sounding)" convective inertia terms?"

References

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Schouten, J. A., 1954, *Tensor Analysis for Physicists*, 2nd ed., (corrected printing 1959), Clarendon Press, Oxford, U.K., (Reprinted by Dover, 1989), pp. 194–195.

Author's Closure[21]

We appreciate the special interest with which Professor Papastavridis read the paper under discussion. The discussion of a paper, as we understand it, is intended to set the record straight on confusing or erroneous concepts, leaving aside details like unavoidable misprints and omissions. Professor Papastavridis' discussion helps very little in this regard, as we remark below, taking up his discussion pointwise.

[21] In the absence of the first author, this Closure was written solely by the second author, Professor J. Angeles, Department of Mechanical Engineering, McGill University, Montreal, Quebec H3A 2K6, Canada.

1 We could not agree more with Professor Papastavridis that the principles laid down by J. L. Lagrange in his "Mécanique Analytique," misspelled in the draft of the Discussion that was forwarded to us, hold for any mechanical system, whether holonomic or nonholonomic. We would even go as far as saying that those principles do hold for continuous systems as well. Note that we never said the opposite, either in our opening statement or anywhere else. We did say that Lagrange's formalism, i.e., the procedure whereby a system of governing equations of motion is derived by invoking the independence of the generalized coordinates in use, is not, as such, directly applicable. We believe that this statement is correct. What is not correct, and adds confusion to the discussion, is Professor Papastavridis' allusion to the "method of Euler, i.e., the principles of linear and angular momentum" (sic). We have not found in Euler's works any reference to a method, but are familiar with Euler's *principles* of conservation of momentum and angular momentum (Truesdell and Toupin, 1960), that are applicable to *any* mechanical system. Mixing up methods with principles does not particularly help to set any record straight. We are talking inaccurancies...

2 We did not elaborate on the criteria to identify a set of constraints as nonholonomic because our paper is not of a tutorial nature, but gave some references where the reader can find details. A more modern approach to the characterization of holonomic constraints is given in terms of *involutive distributions* in Isidori (1985). So, where is the inaccuracy here?

3 Again, we could not agree more with Professor Papastavridis in that the history of nonholomic systems has not been written. Neither did we intend to write it in the Introduction of our six-page paper! A more detailed historical account, although by no means a comprehensive one, can be found in Saha (1991). Again, where is the inaccuracy here?

4 We still believe that Neĭmark and Fufaev's is the first *comprehensive and systematic* exposition of the theory of nonholonomic systems, but we are ready to accept that Professor Papastavridis may have a very different concept of what we understand by the above emphasized phrase. Heun (1906) gives a rather short account of nonholonomic systems, but he should not be chastised for that, since his book was meant as an elementary textbook of engineering mechanics at the turn of the century. Even nowadays undergraduate texts of mechanics are not expected to go into much detail when it comes to nonholonomic systems. Unfortunately, Professor Papastavridis does not provide a full reference for Prange's book, as cited in his Discussion. The other books that he cites devote, at most, one chapter to nonholonomic systems. On the contrary, Neĭmark and Fufaev devote their whole book to such systems, including examples with nonmechanical nonholonomic constraints, namely, in the realm of electromachinery. So, again, where is the inaccuracy here?