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1. L'AVENTURE DE L'ESPRIT — 2. L'AVENTURE DE LA SCIENCE



## WHENCE THE LAW OF MOMENT OF MOMENTUM ?

Clifford Truesdell

While mechanics is vulgarly thought a part of physics, it is not through the efforts of physicists that the last two decades have seen a new flourishing of classical mechanics, much in applications, indeed, but even more in basic theory, presented in half a dozen expressly founded journals and a floodlet of books. To the typical physicist, in natural philosophy classical mechanics seems a finished chapter, yet nearly nothing of what he learned about it in the hasty introduction included in his training in theoretical physics is considered generally correct, let alone well done, by the modern theorist of pure mechanics.

The principle of moment of momentum illuminates a fork in method and standpoint as blunt as any professional demarcation today. Let us recall how the principle is presented in a typical textbook of physics. For definiteness, we choose Joos's *Theoretical Physics*<sup>1</sup>, but nearly any other would serve as well, except that the more recent are often less careful to state explicitly what is done. First, in a system of mass-points the equation of motion for the  $k^{\text{th}}$  particle is

$$m_k \ddot{\mathbf{r}}_k = \mathbf{F}_k + \sum_i \mathbf{F}_{ik}, \quad (1)$$

in standard notation. « Newton's third law of the equality of action and reaction » is laid down in the form  $\mathbf{F}_{ik} = -\mathbf{F}_{ki}$ .

1. G. Joos, *Theoretical Physics*, New York, Stechert, 1934. The passages to which we refer are on pp. 101, 103-104, 135-145, 158-160.

Forming  $\dot{\mathbf{H}}$ , where the moment of momentum  $\mathbf{H}$  is defined as

$$\mathbf{H} = \sum_k m_k \mathbf{r}_k \times \dot{\mathbf{r}}_k, \quad (2)$$

shows that

$$\dot{\mathbf{H}} = \mathbf{L} + \frac{1}{2} \sum_{k,j} (\mathbf{r}_k - \mathbf{r}_j) \times \mathbf{F}_{jk}, \quad (3)$$

where  $\mathbf{L}$  is the total torque exerted by the external forces :

$$\mathbf{L} \equiv \sum_k \mathbf{r}_k \times \mathbf{F}_k. \quad (4)$$

If the mutual forces  $\mathbf{F}_{jk}$  are central, so that  $(\mathbf{r}_k - \mathbf{r}_j) \times \mathbf{F}_{jk} = \mathbf{0}$ , then (3) reduces to  $\dot{\mathbf{H}} = \mathbf{L}$ . (5)

the desired law. Joos states the result as follows : « For a system of particles in which the forces between any two particles are in the direction of the line joining these particles, the rate of change of the total angular momentum is equal to the sum of the moments of the applied forces. » No one can criticize this statement or its proof. However, Joos is uneasy about the *relevance* of the result, for he adds in fine print :

« The limitation made above is actually of little importance. From considerations of symmetry, it is difficult to imagine a force acting between two points which does not coincide in direction with the line joining them, for there is no other pre-eminent direction. If the Law of *Biot-Savart* (p. 290) seems an exception, it must be remembered that this law deals with the force between a magnet-pole and an elementary *segment* (i.e., not a point or particle) of an electrical conductor. »

Despite this reservation, Joos applies (5) explicitly to a rigid body, beginning with simple cases and ending with the Euler equations of motion. He does not state anything about the forces among or between the particles, presumably finite in number since (5) was derived by him only for finite systems, that are presumed to make up the rigid body. Then he treats continuous media, with particular reference to elastic bodies in equilibrium. He states that there are « two kinds of force operating ». In addition to « forces like that of gravity », there are « forces operating on the surface particles which are due to the presence of the contiguous particles ». These are

« not proportional to the volume of the element, but to the area of its surface ». Denoting forces of the first kind by  $\mathbf{g}$ , those of the second kind by  $\mathbf{P}$ , Joos claims to apply « the fundamental equations of the mechanics of systems of particles » :

$$\int \mathbf{g} \, d\tau + \oint \mathbf{P} \, d\mathbf{S} = 0,$$

$$\int \mathbf{r} \times \mathbf{g} \, d\tau + \oint \mathbf{r} \times \mathbf{P} \, d\mathbf{S} = 0. \quad (9)$$

From this start he derives various equations of continuum mechanics, such as that asserting the symmetry of stress tensor, in the usual way. He never mentions angular momentum again, and for continuous media he never writes an equation of the form (5). However, he assumes obliquely that the stress tensor remains symmetric in kinetic as well as in static problems of elasticity and linear viscosity<sup>2</sup>.

I repeat that Joos' book was chosen only to give a typical example of development and use of moment of momentum for physicists. The textbook literature is so vast that some variations must exist, but the approach I have met commonly in conversation with practising physicists suggests that their training went along the above line.

Few if any specialists in mechanics think of their subject in this way. By them, classical mechanics is based on three fundamental laws, asserting the conservation or balance of *force*, *torque*, and *work*, or, in other terms, of *linear momentum*, *moment of momentum*, and *energy*<sup>3</sup>. They regard Equation (5)

2. The assumption is buried well. On p. 169 he derives the kinetic equations of linear elasticity from the static ones by adding to the body forces « the inertial forces given by d'Alembert's principle ». On p. 205 he obtains the formula of linear viscosity by saying : « In order to calculate the stresses on a volume element of a viscous fluid we can avail ourselves of the formulae of the theory of elasticity... » We note that the dynamical principle he uses in elasticity is insufficient to obtain the equations of linear viscosity, which he in fact asserts on the basis of a (fanciful) *analogy* to elasticity, not using any mechanical principle whatever.

3. It is largely due to the intensive and widespread study of gas dynamics in the 1940's that the principle of energy came to be understood to some

itself as the statement of the second basic law. They do not regard the principle of moment of momentum as a consequence of the principle of linear momentum, or of « Newton's Laws » or of any other more familiar principle<sup>4</sup>.

Those who cultivate mechanics easily find half a dozen errors of logic and method in the usual approach of the physicists; physicists, in turn, rarely consent to regard (5) as a fundamental law but insist that it is derived, or at least ought to be derived, from Newton's laws or the « Newtonian » equations<sup>5</sup>. Their

extent by a large body of scientists, although it had been used now and then in mechanical problems for a century.

A general and detailed presentation of the three principles is given by C. TRUESDELL and R. A. TOUPIN, *The Classical Field Theories*, Flügge's Handbuch der Physik III/1 (1960). Because of the recent tendency to write specialized monographs, even as textbooks for beginners, it is difficult to cite a general introduction to modern mechanics on an elementary level comparable to Joos' book for physicists. Works on elasticity stress the independence of the first two principles but do little justice, usually, to the third. Works on gas dynamics emphasize the independence of the third principle but often slight the second.

4. Of course, formal rearrangements are possible. *E.g.*, a sufficiently general « variational » principle (*i.e.*, a formal expression in variations, not a true minimum principle) can be made to include the first two laws, or even all three.

5. In this paper I use the term *Newton's Laws* to denote the laws actually stated by Newton: the « Newtonian » equations, to denote the various equations, of which (1) is representative, alleged in physics books as stating Newton's Laws. Examples of the varying usage of physical writers are given in Footnote 8 of the paper by J. C. C. MCKINSEY, A. C. SUGAR, & P. SUPPES, « Axiomatic foundations of classical particle mechanics », *J. Rational Mech. Anal.* 2, 253-272 (1953). I add further examples. W. V. HOUSTON, *Principles of Mathematical Physics*, New York, McGraw-Hill, 2nd ed., 1948, in § 3 of Ch. 2 calls  $F_{ij} = F_{ji}$  « Newton's third law » and infers that it is « a statement of the conservation of [linear] momentum »; in § 5 he states that the additional condition  $(r_i - r_j) \times F_{ij} = 0$  « is satisfied in a great many cases. » In § 2 of Ch. IX, Houston makes it clear he does *not* regard the latter condition as general; see Footnote 26, below. In § 1. 4 of their *Principles of Mechanics*, New York, McGraw-Hill, 2nd ed., 1949, among the laws they assert to be « equivalent to those used by Newton », J. L. SYNGE & B. A. GRIFFITH state  $(r_j - r_k) \times F_{jk} = 0$ , « summed up by saying : action and reaction are equal and opposite ». They do not, however, state that all mutual forces are binary. But these examples are too clear to be typical. Turn now to A. SOMMERFELD'S *Vorlesungen über theo-*

extreme reverence for what they consider Newton's approach to have been seems to furnish the motive for their tendency to brush off any criticism of it or major deviation from it as mere mathematical quibbling.

Elsewhere I design to take up the whole question more thoroughly. Here I wish to open a historical analysis. How did the two viewpoints arise and separate? Whence came the principle of moment of momentum? I have not yet uncovered all I seek on either question, but it seems to me that research on the history of rational mechanics is still enough of a novelty that an example of method during the working, before it is yet known which conjectures are right and which leads point to ends, might not be unwelcome in a circle accustomed to studies of an older, less formally mathematical, natural philosophy.

First we must decide what the question really is. Turning our backs on physics, metaphysics, and prejudice, if we are to get anything solid in this matter we must begin by seeing *how the principle of moment of momentum is used today*. For punctual systems it serves only as a tool in the integration. This fact supports the viewpoint of the physicists that it is a subsidiary, derived principle. For rigid or deformable bodies, it has little use in solution of special problems but instead is one of the two basic principles from which the dynamical equations are commonly derived. For rigid bodies, the principle of moment of momentum yields the Euler equations; for deformable continua, the symmetry of the stress tensor. If we try to follow the physicists here, we see that for them the principle of moment of momentum serves to *obviate the need for specifying the mutual forces among the particles of a rigid or deformable body*. It is enough that the mutual forces contribute nothing to the resultant torque. In this sense we shall understand the

*retische Physik*. 1, *Mechanik*, Leipzig, Akad. Verlagsg., 5th ed., 1955. There in § 1 Newton's own laws are quoted verbatim and taken as axioms; in § 13  $F_{jk} = -F_{kj}$  is inferred from « actio = reactio »; two pages later we are told to conclude that  $r_k \times F_{ik} + r_i \times F_{ki} = 0$  by looking at a figure in which the forces are drawn as central! Such examples could be multiplied indefinitely.

problem. For us, it is not the trivial result of taking the cross-product of (1) by  $r_k$  and summing over  $k$  that will be seen as the principle of moment of momentum, but rather the equation  $\dot{H} = L$  where  $L$  is *the torque of the applied forces only*, whether in the form (4) for admittedly punctual systems, or in more general form for space-filling bodies. Therefore, we shall search the origins of two statements:

A. *The resultant torque of a system of mutual forces is zero, whether as a consequence of Newton's Laws, « Newtonian equations », or otherwise.*

B. *The equation  $\dot{H} = L$  is a fundamental, independent law of mechanics.*

For Statement A a beginner might appeal to Newton's *Principia*, but he would soon note

Datum 1. In Newton's *Principia* there is neither Statement A nor Statement B. Nor does Newton anywhere assert that  $(r_k - r_i) \times F_{ik} = 0$ .

Often I have seen physicists surprised by this conclusion, sometimes to the point of refusing to believe it, but truly it should conform to widespread beliefs in the empirical nature of science. I take it as the first principle of historical research in mechanics that the meaning is to be inferred from the use, since successful application has always preceded statement of the principle being applied. There being in Newton's book no theory of general dynamical systems, of rigid bodies<sup>6</sup>, or of the stress in a continuum, no empiricist ought to be surprised to learn that Newton's system of mechanics was not general enough to yield such theories. Since Huygens was the first to solve a non-trivial problem about rotation, namely, the problem of the center of oscillation, the beginner might try to read the *Horologium Oscillatorium* and other works of Huygens, but he would add

6. This fact does not imply that Newton had never thought about these matters, as is shown by the famous comment following Law I: « A top, whose parts by their cohesion constantly draw each other back from rectilinear motions, does not stop spinning except in so far as it is slowed by the air. » It would be interesting to see if among Newton's papers there is anything mathematical concerning the motion of rigid bodies.

Datum 2. In Huygens' solution of the problem of the physical pendulum there is nothing whatever regarding Statement A or Statement B.

Neither of these data will surprise a historian of science, for, on the basis of his general knowledge, he will not expect any development of mechanics as a whole along « Newtonian » lines by Newton or anyone else before the middle of the nineteenth century, nor will he expect the *general* principle of moment of momentum to be stated before its first great application, namely, the theory of general motion of a rigid body, was discovered. He will more likely start by checking the material as presented in the first systematic treatise on analytical dynamics, the *Mécanique Analytique* of Lagrange, published in 1788 and thus bisecting the period separating Huygens and Newton from the textbook writers at the end of the last century<sup>7</sup>. In the *Mécanique Analytique*, the basic law of mechanics is the principle of virtual work, and from it Lagrange derives easily the general integrals of energy, momentum, and moment of momentum for a system of mass-points. Since any forces that do no work may be left out of the expression for the virtual work, we may infer that in Lagrange's equations of the form  $\dot{H} = L$ , the mutual forces do not contribute to  $L$ , but Lagrange does not say so. When we search for explanation of a concept in Lagrange's writings, usually we search in vain; in this case all we find is that the forces are « those that at the same instant act upon each point of the mass  $m$  along some given directions, that is, the velocities that each of these forces would impress upon the mass  $m$  if they acted separately and equally during the time taken as a unit. However variable may be the action of these forces, nevertheless one can regard it as constant during an instant. » Also he speaks of the acce-

7. *Mécanique Analytique*, Paris, Veuve Desaint, 1788. The general principle is stated in the Seconde Section of the Seconde Partie; the integrals are obtained in the Troisième Section. The history to which we refer is given in the Section Première. In the second and later editions, as reprinted in LAGRANGE'S *Œuvres* II, there are considerable changes in the first and second sections, but they do not materially alter the part of the history to which I refer here, and, while the entire development of the principle of virtual work is recast, it becomes no clearer.

lerating forces as « tending to given centers ». Beside the looseness of statement, recalling d'Alembert's mode of expression, we see here the typical evasive vagueness of Lagrange. In the statement of the principle of areas<sup>8</sup> Lagrange in fact makes us suspect that he does *not* perceive the generality of the integral of moment of momentum, for he adduces only instances where the torque on each several body vanishes : « If the system were not subject to any accelerating force, or if the only forces present all tend to the point we have selected as origin of co-ordinates... » Search of other parts of the book confirms

Datum 3. In the *Mécanique Analytique* there is nothing relevant to Statement B; as for Statement A, from Lagrange's treatment it may be inferred that forces which do no virtual work do not contribute to the resultant torque, but Lagrange gives no evidence of seeing this fact.

The historian will consult the *Mécanique Analytique* for a second reason, namely, that it includes in sections at the beginnings of the various parts the first history of mechanics. He who reads that history will obviate the need to consult the secondary works<sup>9</sup>, since in regard to rational mechanics these do little more than quote, paraphrase, extend, or correct in detail the little sketches by Lagrange.

Lagrange writes that the principle of areas « ... seems to have been discovered at the same time by Messrs. Euler, Daniel Bernoulli, and the Chevalier d'Arcy, but in different forms. According to the two first, this principle consists in the fact that in the motion of several bodies about a fixed center, the sum of the products of the mass of each body by the velocity of circulation around its center and by the distance from that same center is always independent of the mutual action that the bodies can exert upon each other, and it remains constant so long as there is neither exterior action nor an exterior obstacle....

The principle of Mr. d'Arcy... is that the sum of the products of the mass of each body by the area that its radius vector describes about a fixed center is always proportional to

8. Paragraph 7 of the Troisième Section in the first edition.

9. E. MACH, *Die Mechanik in ihrer Entwicklung*, 1883; E. JOUCQUET, *Lectures de Mécanique*, 1908-1909; R. DUCAS, *Histoire de la Mécanique*, 1950.

the time. It is plain that this principle is a generalization of the beautiful theorem of Newton on the areas described in virtue of arbitrary centripetal forces... ».

While Lagrange's book is a good starting place, experience with it has led me to the following working hypotheses :

1. There was little new in the *Mécanique Analytique*; its contents derives from earlier papers of Lagrange himself<sup>10</sup> or from works of Euler and other predecessors.
2. General principles or concepts of mechanics are misunderstood or neglected by Lagrange.
3. Lagrange's histories usually give the right references but misrepresent or slight the contents.

When we read Lagrange's sarcastic comment about d'Arcy, « ... he even made out of it a kind of metaphysical principle, which he calls the *conservation of action*..., as if vague and

10. *E. g.*, the famous Lagrangean equations had been derived before, namely in §§ 7-11 of his « Théorie de la libration de la lune et des autres phénomènes qui dépendent de la figure non sphérique de cette planète », *Nouv. Mém. Acad. Berlin* 1780, 203-309 (1782) = *Œuvres* 5, 5-122. Here Lagrange begins by writing the inertial forces in rectangular Cartesian co-ordinates, while the accelerating forces are expressed in terms of distances from « any centers » (§ 5). He states (§ 13) that the Lagrangean equations hold for « an infinity of particles subject to any forces proportional to functions of the distances »; the meaning of this statement is not certain, but with any meaning I can conjecture it is generally false. In this paper Lagrange stops short of deriving the equations of motion of a rigid body by his method.

Likewise, the general integrals derived in the *Mécanique Analytique* are foreshadowed in the paper, « Remarques générales sur le mouvement de plusieurs corps qui s'attirent mutuellement en raison inverse des carrés des distances », *Nouv. Mém. Acad. Berlin* 1777, 155-172 (1779) = *Œuvres* 4, 401-418; this paper rests on the « Newtonian » equations with a « Newtonian » potential function. While in §§ 4-8 Lagrange derives the integrals of momentum, moment of momentum, and energy, use of special properties of the potential function tends to conceal their meanings. For three bodies, the results are given in § 11 of his « Essai sur le problème des trois corps », *Prix de l'Acad. sci. Paris* 9, 1772 = *Œuvres* 6, 229-324.

Finally, the principle of virtual work for dynamics, on which the entire *Mécanique Analytique* is founded, had been given more than twenty years earlier in § IV of his « Recherches sur la libration de la lune, dans lesquelles on tâche de résoudre la question proposée par l'Académie royale des sciences pour le prix de l'année 1764 », *Prix de l'Acad. sci. Paris* 9, 1764 = *Œuvres* 6, 5-61.

arbitrary names were the essence of the laws of nature and could by some secret virtue raise to final causes some simple consequences of the known laws of mechanics », Hypotheses 3 and 2 suggest that maybe d'Arcy had something. However, this is a bad lead; we find d'Arcy assumes  $\dot{H} = 0$  for bodies that « act on each other in any way, whether by wires, by inflexibles lines, by laws of attraction, etc.<sup>11</sup> ». As a reason he says only, « It is known that a body, all of whose parts are connected together, cannot take on any motion in virtue of their reciprocal actions ».

Turning to Hypothesis 1, we can choose first to follow up Lagrange's own earlier work. Moving slowly backward in his *Œuvres* is a tedious process. The task is lightened by use of a fourth working hypothesis :

4. Lagrange's best ideas in mechanics derive from his earliest period, when he was studying Euler's papers and had not yet fallen under the personal influence of d'Alembert.

Thus we turn at once to his paper of 1760 on least action<sup>12</sup>. Here we find the area integral of kinetics shown to follow from the principle of least action « if the system is entirely free, or if it is constrained to move about a fixed point, and if all the forces acting on the bodies come together at this point... ». The same references to works of Euler, Daniel Bernoulli, and d'Arcy are given here as later in the *Mécanique Analytique*. The law (5) is never mentioned or used<sup>13</sup>.

11. See the argument based on Figure 2 in his « Principe général de dynamique, qui donne la relation entre les espaces parcourus et les temps, quel que soit le système de corps que l'on considère, et quelles que soient leurs actions les unes sur les autres » (read in 1746), *Mém. Acad. Sci. Paris* 1742, 348-356 (1752), and an addition of 1747, pp. 356-361. The paper is not easy to read. Nothing is added in his later « Réflexions sur le principe de la moindre action de M. de Maupertuis », *Mém. Acad. Sci. Paris* 1749, 531-538 (1753).

12. § XII of « Application de la méthode exposée précédente à la solution de différens problèmes de dynamique », *Misc. Taur.* 2<sub>2</sub> (1760-1761), 196-298 [1762] = *Œuvres* 1, 365-468.

13. *E. g.*, in §§ xxxiv-xxxix are incredibly elaborate calculations deriving the equations of motion of a rigid body from the principle of least action. Lagrange uses unsymmetrical notations which complicate the formulae. He does not state that the body he treats is rigid, but the results are not

Since search of other works on general dynamical systems<sup>14</sup> fails to reveal anything toward answering our questions, we must turn to researches on space-filling bodies. At the same time, we quickly see that Lagrange's references to Euler and Daniel Bernoulli bring us to papers where a rigid body plays an essential part. However, rather than creep backward we now jump to the point where, had we not needed to buck the currently widespread belief that the mass-point is the beginning and end of mechanics, we ought to have started, namely, at James Bernoulli's great paper of 1703 on the center of oscillation<sup>15</sup>. As its title suggests, this paper applies *the principle of the lever* to the motion of a pendulum. Now the « principle of the lever » is nothing else than the equilibrium of moments. As is well known, this paper of James Bernoulli contains the germ of the d'Alembert-Euler principle that reversed accelerations are equipollent to forces per unit mass. Combination of these two principles yields (5) at once. Of course, in James Bernoulli's paper we cannot find a general statement of (5) or even an explicit statement in the context of the pendulum, but we recognize the use of the principle to determine the center of oscillation. The question is now, did Bernoulli attempt to prove the

generally correct otherwise. His quantities [P] [Q] [R] are in fact angular components of  $\dot{H}$ , but he never says so. There is no trace of the concept of torque.

14. D'ALEMBERT'S *Traité de Dynamique*, 1743, contains no allusion to moment of momentum, torque, or the area integral, although Chapters II and IV of the *Seconde Partie* concern the integrals of momentum and energy, respectively.

The general « Newtonian » equations of motion for a single mass-point make their first appearance in § 18 of Euler's paper E 112 of 1747, « Recherches sur le mouvement des corps célestes en général », *Mém. acad. sci. Berlin* [3] (1747), 83-143 (1749) = *Opera omnia* II 25, 1-44. In 1744 he had been the first to set up « Newtonian » equations for a system of  $n$  mass-points subject to a particular law of force; see § 26 of E 174, « De motu corporum flexibilium », *Opusculi* 3, 88-165 (1751) = *Opera omnia* II 10, 177-232. For this system he had derived the integrals of linear momentum and kinetic energy but had not noted the integral of moment momentum.

15. « Démonstration générale du centre de balancement ou d'oscillation, tirée de la nature du levier », letter of 13 March 1703, *Mém. Acad. Sci. Paris* 1703, 4<sup>to</sup> ed. (Paris), 78-84 (1705) = 12<sup>mo</sup> ed. (Amsterdam), 96-104 (1707) = *Opera* 2, 930-936.

extended principle of the lever from something else, or did he recognize it as an independent axiom? He inferred it from the « principle of the lever pulled or pushed by powers in motion », which he said was « demonstrated by the late Mr. Mariotte in Prop. 13 of the second part of his treatise *On the impact of bodies*, and there is no one who disagrees with it ». Now Mariotte « demonstrated » this proposition *by experiment*<sup>16</sup>. We may justly doubt that his experiments establish the principle as used by Bernoulli, but only fantasy could suggest that Bernoulli thought of his principle as following from Newton's laws or from any other then known laws of mechanics. Rather, expressly remarking of the special dynamical postulate used by Huygens that « there are plenty of persons to whom this requirement has seemed a little bold and who have never been able to agree that it is obvious », Bernoulli clearly sought and found in the extended principle of the lever a *new and more general method* in kinetics.

This much established, we can follow the idea back a little to James Bernoulli's first and somewhat incorrect attempt<sup>17</sup>, published in 1686 (be it noted, before he could even have seen Newton's *Principia*), which L'Hospital<sup>18</sup> recognized<sup>19</sup> as resting on a principle « which is nothing else than that of the lever ». In this earlier attempt, moreover, Bernoulli does not cite any evidence, either theoretical or experimental, for his principle.

Thus I assert

16. *Traité de la percussion ou choc des corps*, Paris, 1679. The passage in question is reprinted on pp. 135-136 of Jouguet's *Lectures de Mécanique* I, Paris, Gauthier-Villars, 1908.

17. « Narratio controversiae inter Dn. Hugenum et Abbatem Catelanum agitatae de Centro oscillationis quae loco animadversionis esse poterit in responsionem Dn. Catelani, num. 27. Ephem. Gallic. anni 1684, insertam », *Acta Erud. Leipzig*, July 1686, 356-360 = *Opera* 1, 277-281.

18. « Lettre de Mr. le Marquis de l'Hospital à Monsieur Huygens, dans laquelle il prétend démontrer la règle de cet auteur touchant le centre d'oscillation du pendule composé par sa cause physique, et répondre en même temps à Mr. Bernoulli », *Hist. Ouvrages Sçavans* June-Aug. 1690, 440-449 = *Jac. Bernoulli Opera* 1, 454-457 = *Œuvres de Huygens* 9, 403-406.

19. This was no small feat on the part of l'Hospital. Bernoulli's first attempts are always extremely hard to follow. He does say at one point, « since the rod, in this case, can be considered after the fashion of a lever »...

**Conjecture 1.** *The principle of moment of momentum, as an independent law of mechanics and as a generalization to kinetics of the principle of equilibrium of moments in statics, is due to James Bernoulli (1686); it antedates Newton's laws (1687).*

Of course, Bernoulli did not state the principle either clearly or in any generality, nor did he claim that it could not be derived from the laws later published by Newton. We must now follow the way in which the principle was used and developed until it reached the generality in which theorists of mechanics conceive it today.

As Lagrange noted, in 1745, both Daniel Bernoulli and Euler put forward a form of the principle in the course of their solutions of problem of a mass-point constrained to slide within a rigid, rotating tube. The method used by Euler<sup>20</sup> is the same as James Bernoulli's, but it is clearly explained, and Euler gives (5) explicitly in the special case of plane motion about a fixed axis ( $L = I\alpha$ ); his alleged proof is no more than an assertion of the extended law of the lever. While Daniel Bernoulli<sup>21</sup> obtained the same result<sup>22</sup>, he tried to prove it from other principles :

20. E 86, « De motu corporum in superficiebus mobilibus », *Opusc. varii argum.* 1, 1-386 (1746) = *Opera omnia* (II) 6, 75-174. See §§ 42, 45.

21. « Nouveau problème de mécanique résolu », *Hist. Mém. Acad. Sci. Berlin* [1] (1745), 54-70 (1746). See §§ IX-XI.

22. It follows by combining his Lemmas VIII and IX. Then he adds the following characteristic remarks : « This lemma has been proved in several places : For my part, I proved it in a paper *On the motion of bodies arising from excentric percussion etc.*, §§ 4 and 5, which I sent to the Academy of Petersburg some years ago, at a time when nobody had yet thought about this subject, so far as I know. My reason for saying this is certainly not to get myself any credit for it, but rather so as not to be held plagiarist by those who will find the same propositions in other works since printed; and this justification holds also for all the new propositions in my *Hydrodynamics* and my various memoirs on mechanics; nevertheless I do not claim that those who have solved my problems or proved my theorems after me owe the least thing to me in this respect, and I esteem infinitely the wisdom of their solutions and their proofs.

« This property has been noticed also by Mr. Euler, and he pointed it out to me in one of his letters. For my part I could not show that I found it without him. I had used other terms in my calculations, pointing out that the sum of the live forces resulting from the circulating motion of the sys-

« ... the action exerted mutually by the tube and the ball upon each other does not disturb the said [moment of] momentum. » He asserts that the « action » of the tube and ball upon each other is normal to the tube, so he replaces that « action » by a little thread connecting the two. Thus the tube and ball exert on each other equal and opposite forces. Daniel Bernoulli's idea of what he had done is expressed in his letter<sup>23</sup> of February 1744 to Euler :

« ... I have derived and proved the principle of conservation of moment of the rotational motion from the usual principles... » Hence I assert

**Conjecture 2.** *The idea that the principle of moment of momentum follows from the principle of linear momentum in the sense that the mutual actions of a system of bodies exert no resultant torque is due to Daniel Bernoulli (1744).*

tem, divided by the angular velocity of the tube, remains always the same. That comes down to the same thing, and to do justice to Mr. Euler, I have preferred his expression to mine. »

It will be noted that Daniel Bernoulli's generalization is usually false; it is not the general principle of momentum.

23. Published by P.-H. Fuss, *Correspondance mathématique et physique de quelques célèbres géomètres du XVIII<sup>e</sup> siècle*, St. Petersburg, 1843. The letter of Euler to which Bernoulli refers above seems to have been lost.

I have searched the remaining correspondence of the Bernoullis with Euler as published by Eneström, *Bibliotheca Math.* (3) 4, 344-388 (1903); 5, 249-291 (1904); 6, 16-89 (1905); 7, 126-156 (1906-1907). It does not reveal anything further that is conclusive in the present inquiry, though some dozen letters are relevant. It would do the modern Newtonians good to read these great geometers' repeated complaint that *new principles* are needed in order to set up the equations of motion for various simple mechanical systems. Apparently it never entered their heads to apply Newton's Laws or « Newtonian » equations. On 23 April 1743 Daniel Bernoulli wrote to Euler that he wished he could determine the motion in a plane of three bodies linked by threads. « This problem may not be one of the easiest. I believe, however, that if we knew all the universal laws of motion, this problem would become rather easy and would at the same time give the impulse for solving more general problems of this kind. » The remark was prophetic, for Euler shortly did solve this problem, publishing his solution in § 26 of the paper cited in the next footnote. Euler's solution is the earliest example of use of the « Newtonian » equations to set up the differential equations of motion for a dynamical system. For further discussion of the point at issue here, see §§ 26 and 30 of my history of elastic and flexible bodies, *L. Euleri Opera Omnia* II 11, (1960).

Did this idea influence the discovery of the general equations of motion for rigid bodies? Perhaps so. First I note a step in the opposite direction:

**Conjecture 3.** *In a work of 1744, Euler<sup>24</sup> was the first to use the principles of linear and angular momentum as independent mechanical laws for setting up the equations of motion of a system. The system considered was  $n$  linked bars in a plane.*

His special case of the principle of moment of momentum did not lead Daniel Bernoulli to the equations of motion of a rigid body. In a letter of 4 December 1745 to Euler he described the general problem of rotation as « extremely difficult, which will not be solved easily by anybody... One might ask how to determine the axis of rotation by a summation sign, such that the centrifugal forces destroy each other. » While Bernoulli dropped the problem, Euler through exploration of more and more general cases approached the equations of motion of an arbitrary rigid body. He finally achieved them in a paper written in 1750. This paper, *Discovery of a new principle of mechanics*<sup>25</sup>, lays down the principle of *linear momentum*, or the « Newtonian » equation  $F = mr$ , as the axiom which « includes all the laws of mechanics », if applied to every several element of mass  $m$  every body. The end product of the paper is the « Eulerian equations of motion » for rigid bodies. How can this be? Euler states that the forces « include both such external forces as act upon the body from without, and also the internal forces binding the parts of the body to each other, so as to prevent them from changing their relative positions. But it is to be noted that the internal forces mutually destroy each other, so that the continuation of the motion does not require any external forces, except insofar as those forces do not mutually destroy each other ». Euler thereupon obtains the equations of motion by taking moments of the equations of linear

24. E 174, « De motu corporum flexibilibus », *Opusc. [var. arg.]* 3, 88-165 (1751) = *Opera omnia* II 10, 177-232. Presentation date: 5 November 1744. This paper is described and translated in part on pp. 223-229 of my book.

25. E 177, « Découverte d'un nouveau principe de mécanique », *Hist. Acad. Sci. Berlin* [6] (1750), 185-217 (1752) = *Opera omnia* II 5, 81-108. Presentation date: 3 September 1750. See § 22 and § 40-58.

momentum for elements of mass, leaving altogether out of account any mutual forces that may be present. His method, then, is now akin to that of Daniel Bernoulli, but while he sees that mutual forces have no effect, he does not make any special hypothesis about their functional dependences or their directions. In effect, he asserts that (5) follows from the principle of linear momentum when the mutual forces are such as to maintain rigidity<sup>26</sup>. The modern theorist of mechanics, who has constantly in mind the general case of a deformable body, when (5) does not generally follow from the principle of linear momentum, sees the ridgepole that Euler walked here; as usual, he did not slip off.

Euler's book<sup>27</sup> on the dynamics of rigid bodies is so diffuse as to conceal the line of thought in a multitude of special cases, but it seems to rest upon the same approach to the subject.

The general problem was considered next by Lagrange<sup>28</sup>. His « new solution » of 1773 begins, « I consider the proposed body as an assembly of corpuscles or mass-points joined together in such a way as always to conserve their mutual distances... I shall have by the principles of mechanics, since the system is supposed free to move about a fixed point but is

26. One modern author has reverted to a similar view. While in the first edition (1934) of his book Houston gave a derivation of the equations of motion of rigid bodies on the basis of particles exerting central forces, in the second edition, cited in Footnote 5, he abandons the particles in favor of smoothly distributed internal forces per unit volume. He states that the vanishing of their resultant moment « may be taken as part of the definition of a rigid body. It would follow from the assumption that the forces between the parts of the body are central forces, but this assumption lacks generality and seems unnecessarily restrictive. Frequently [the vanishing of the torque] is justified by the statement that the internal forces within a rigid body are in equilibrium among themselves. It may be considered as a law similar to Newton's third law of motion. It describes the observed fact that isolated bodies do not set themselves into rotation. »

27. E 289, *Theoria motus corporum solidorum seu rigidorum ex primis nostrae cognitionis principiiis stabilita et ad omnes motus, qui in huiusmodi corpora cadere possunt, accomodata*, Rostock and Greifswald, Roesse, 1765. 2nd ed., *ibid.*, 1790 = *Opera omnia* II 3 and 4. See Chapter 15.

28. « Nouvelle solution du problème du mouvement de rotation d'un corps de figure quelconque qui n'est animé par aucune force accélératrice », *Nouv. Mém. Acad. Berlin* 1773, 85-120 = *Œuvres* 3, 579-616.

not subject to any further outside force, — I shall have, I say, on the spot, these... equations », and Lagrange writes down the integrals of moment of momentum and kinetic energy for a system of free mass-points. He says they follow from a « known principle » concerning « every system of bodies acting on one another in any manner ». The alleged proof is only a restatement. This typical looseness followed by a cloud of calculation moved Euler to reply<sup>29</sup> : « ... But when I tried with greatest avidity to follow in detail his extremely profound thoughts, truly I could not get myself to go through all his calculations. Even the first lemma so deterred me that on account of my blindness I could not hope in any way to check through all the analytic devices he used ». In this new paper, written in 1775, the old Euler laid down as *fundamental, general, and independent laws of mechanics*, for all kinds of motions of all kinds of bodies, the principles of linear momentum and moment of momentum for each element of mass. In justification, he wrote only, « ... by the principles of mechanics it is necessary that... » The two principles, intergrated forms of which are

$$\mathbf{F} = \dot{\mathbf{P}} \text{ and } \mathbf{L} = \dot{\mathbf{H}}, \quad (7)$$

may justly be called *Euler's laws of mechanics*.

What caused Euler in his last work to revert to the ideas of James Bernoulli and abandon the attempt to regard the linear momentum principle, supplemented by restrictions upon the mutual forces, as sufficient for the whole science of mechanics? The answer is to be sought, as always, in the *practice*, not the philosophy, of the subject. The tenet that Newton's Laws or the « Newtonian » equations in any of their forms, suffice, can be held only by those who limit their attention to mass-points, rigid bodies, and certain other special systems. Any modern theorist knows this and sees at once the evasions used in typical treatments, such as that of Joos above described, to get the form (6) suitable for deformable bodies. In the

29. In the introduction to E 479, « Nova methodus motum corporum rigidorum determinandi », *Novi Comm. Acad. Sci. Petrop.* 20 (1775), 208-238 (1776). Presented 16 October 1775. Reprinted in the 2nd ed., 1790, of the *Treatise on rigid bodies*. The fundamental laws are given in §§ 27-28.

eighteenth century, fluid mechanics was limited, as far as concerns developed theory, to frictionless fluids, where, again, the principle of linear momentum suffices. But in *elasticity* Newton's laws never have been and never can be sufficient : The simplest problems of elasticity rest essentially upon the balance of moments, and if we choose to think of the body as made up of molecules exerting mutual forces, those forces *cannot* be left out of account. In the theory of elasticity, forces described by Joos as « not proportional to the volume of the element, but to the area of its surface » are of the essence, and Joos would have done better to adduce them instead of the law of Biot and Savart as casting doubt upon the generality of binary central forces. A molecular approach to elasticity is fraught with difficulty from start to finish, as anyone knows who has looked at classical and modern theories of the solid state. *Everyone* in the eighteenth century who studied problems of elasticity invoked the principle of moments. Thus, finally, if we are to find the origin of the independent principle of moment of momentum, we must search the history of theories of elastic materials.

Elsewhere I have done so<sup>30</sup>. Throughout his long life Euler studied flexible or elastic lines in various cases, slowly moving towards greater generality. Continually he strove for a *single mechanics* that would include all kinds of systems. He succeeded in showing that for a perfectly flexible line, either the balance of forces or the balance of moments suffices to obtain the equations of equilibrium; for such a system, the two principles are equivalent. For an elastic line, balance of moments had been used since the beginning, and no effort or hypothesis ever enabled him to dispense with torques in favor of forces. Furthermore, from the equation of equilibrium for an elastica obtained by balance of moments, the d'Alembert-Euler principle of reversed accelerations does not lead to equations of motion. What then *are* the equations of motion for an elastica? In 1771, after the publication of his book on rigid bodies but before his statement of the general laws of motion (7), Euler fi-

30. *L. Euleri opera omnia* (II), II<sub>2</sub>, 1960. Also « Outline of the history of flexible or elastic bodies to 1788 », *J. Acoust. Soc. Amer.* 32, 1647-1656 (1960).

nally saw that all the results obtained on the elastica up to that time flowed from special properties of the constitutive equation rather than from the principles of mechanics. In effect reverting to the program of James Bernoulli<sup>31</sup>, he sought the equations of equilibrium and motion for an arbitrary plane continuum, independently of its material constitution. For this, both the balance of forces and the balance of moments are necessary; neither by itself suffices. Application of the d'Alembert-Euler principle to the equations of equilibrium of forces then yields equations of motion, as desired. Euler's final result is<sup>32</sup>.

$$\begin{aligned} \frac{\delta T}{\delta s} + V \frac{\delta \varphi}{\delta s} &= (-F_x + \sigma \frac{\delta^2 x}{\delta t^2}) \sin \varphi + (-F_y + \sigma \frac{\delta^2 y}{\delta t^2}) \cos \varphi \\ \frac{\delta V}{\delta s} - T \frac{\delta \varphi}{\delta s} &= (-F_y + \sigma \frac{\delta^2 y}{\delta t^2}) \sin \varphi - (-F_x + \sigma \frac{\delta^2 x}{\delta t^2}) \cos \varphi \\ \frac{\delta M}{\delta s} - V &= 0. \end{aligned} \quad (8)$$

$T$  is the tension,  $V$  is the shear force,  $M$  is the stress couple,  $F_x$  and  $F_y$  are the components of applied force,  $\sigma$  is the line density,  $\varphi(s, t)$  is the slope angle, and  $x(s, t)$ ,  $y(s, t)$  is the position. These equations correspond to (6), more general in being kinetic but more special in that the continuum is assumed to be a plane curve. Equations (8) furnish the first example of *general equations of mechanics, independent of the nature of mutual forces and materials*.

Thus it may well have been the need to apply (5) as a principle independent of « Newton's laws », in a case to which Newton's laws have never been shown to be relevant or useful, that caused Euler to abandon all reference to mutual forces in

31. « Véritable hypothèse de la résistance des solides, avec la démonstration de la courbure des corps qui font ressort... Lettre du 12 mars 1705 », *Mem. Acad. Sci. Paris*, 1705, 4<sup>th</sup> ed., Paris, 176-186 (1706) = 12<sup>mo</sup> ed., Amsterdam, 230-244 (1707) = *Opera* 2, 976-989. This paper is analysed on pp. 105-109 of my book.

32. E 410, « Genuina principia doctrinae de statu aequilibrii et motu corporum tam perfecte flexibilium quam elasticorum », *Novi Comm. Acad. Sci. Petrop.* 15 (1770), 381-413 (1771) = *Opera omnia* II, 11<sub>1</sub>, 37-61. Presentation date : 14 January 1771.

setting up his general laws of mechanics (7). Hence I assert

**Conjecture 4.** *The general principle of moment of momentum, as independent of the principle of linear momentum and as applicable to every part of every body, was first proposed by Euler in 1775. He was led to it through studies of elastic lines, culminating in 1771 in his general theory of the plane linear continuum.*

Moreover, growth of understanding of the nature and importance of moment of momentum is inextricable from the growth of the concept of stress in a continuum<sup>33</sup>. Here, however, I shall not follow the matter through the researches of Cauchy and down to the present day, when the very definition of moment of momentum is being subjected to scrutiny because, as shown by work of J. L. Ericksen, it needs to be generalized.

Our search of the sources has not yet turned up Statement A, although we have seen steps toward it in work of Daniel Bernoulli (1744), Euler (1752), and Lagrange (1788). The earliest occurrence I have found is a textbook by Poisson<sup>34</sup> :

**Conjecture 5.** *Statement and proof that a system of pairwise equilibrated, central forces exerts no resultant torque is due to Poisson (1833).*

Poisson's proof is that found in modern texts; he mentions « the general law that action always equals reaction », which « always holds in nature », and he says « one always notes » that  $(\mathbf{r}_k - \mathbf{r}_j) \times \mathbf{F}_{jk} = 0$ .

With Poisson we are still some distance, however, from the Newtonians of today. The above remarks occur on page 447 of Volume 2, so it does not seem that Poisson can have considered them fundamental to mechanics as a whole. For example, the theory of rigid bodies is given much earlier. Other than the above-quoted statement of what is often called « Newton's third law », viz,  $\mathbf{F}_{jk} = -\mathbf{F}_{kj}$  and  $(\mathbf{r}_k - \mathbf{r}_j) \times \mathbf{F}_{jk} = 0$ , there seems

33. For this growth, see my « Stages in the development of the concept of stress », *Problems of Continuum Mechanics* (Mushkelsvili Anniv. Vol.), 556-564 (1961).

34. *Traité de Mécanique*, 2nd ed., 2 vol., Paris, Bachelier, 1833. See paragraphs 552-554.

to be in the whole book no other reference to Newton's laws, either as stated by Newton or as found in physics texts today, nor have I seen Newton named.

After all this, we can ask, Who first had the idea that the « Newtonian » equations suffice as a basis for mechanics? Certainly none of the creators of mechanics, except, perhaps, Newton himself<sup>35</sup>: Such an idea is contradictory<sup>36</sup> to the contents of almost any page of the works of the Bernoullis, Euler, d'Alembert, or Lagrange, but it fits in well with views held a century later. In 1867 Kelvin and Tait<sup>37</sup> asserted the supremacy of Newton's laws in terms soon to be echoed by Mach, although after a few preliminary passes they chose instead as their axiom Lagrange's principle of virtual work. Kirchhoff<sup>38</sup> wrote down as comprising the « most general case to be considered in the mechanics of material points » the « Newtonian » equations subject to a particular class of forces, namely, forces that are assigned functions of the positions, velocities, and time, augmented by forces arising from holonomic constraints. Between 1833 and 1867 a fundamental change had occurred in mechanics, not in the subject itself but in the *teaching* of it. To describe this change would require another essay, perhaps a boring one, but nevertheless one that ought to be written, for, unlike most his-

35. I see no reason to think that Newton had any such idea; certainly it is not borne out by examination of the *Principia*. See my summary of the *Principia* in § I of « A program toward rediscovering the rational mechanics of the age of reason », *Arch. Hist. Exact Sci.* I, 3-36 (1960). The Newtonian myth seems to have originated with a group of British disciples, most of whom had insufficient mathematics to follow any proof in the *Principia*. It may account for the fact, at first sight astonishing, that no major result in theoretical mechanics was found in England between 1715 and 1815. Since the beginning there has been a tendency to idolize Newton as a literary saint rather than to follow him as a leader of science.

36. Recall the definition in Footnote 5. Of course, nearly the entire successful effort of mathematical philosophy in the eighteenth century was directed towards the more general subject of « Newtonian mechanics » as distinct, let us say, from peripatetic mechanics or Cartesian mechanics and as extending some of the mechanical theories of Galileo and Huygens.

37. §§ 242-264 and § 293 of their *Treatise on Natural Philosophy*, Oxford, Clarendon Press, 1867.

38. Eq. (17) of Lecture II of *Vorlesungen über mathematische Physik: Mechanik*, Leipzig, Teubner, 1876.

torical studies, it would show us how a subject dies. In all fields except science, modern thinkers tend to doubt their grandparents' myth of « progress ». Perhaps science, too, deserves to turn its doubts inward.

I am obliged to Professor vander Waerden for valuable discussion.

#### APPENDIX: *Recent Views on the Principles of Mechanics*

Since neither historians nor physicists are likely to have had much contact with modern research on mechanics, I append here a sketch of a newer approach toward the ends which seem to be sought in most cases when Newton's laws or « Newtonian » equations are invoked, mentioning its historical origins.

A. Forces and torques are undefined objects such as mass, position, and time. This view seems to be consonant with that of Newton, Euler, and current physics teaching, while opposed to that of d'Alembert, Lagrange, and Mach. However, in contrast to earlier work, it is recognized now that *mathematical axioms* to be satisfied by forces and torques must be laid down<sup>39</sup>, just as real numbers are undefined objects, not regarded as sufficiently described until we have stated the axioms they satisfy.

B. Euler's laws (7), relating the *load* ( $F, L$ ) to the *reaction* ( $\dot{P}, \dot{H}$ ) in inertial frames, express the fundamental axioms of mechanics.

C. All forces and torques arising from the mutual actions of bodies are *indifferent* in the sense that they are invariant under change of observer<sup>40</sup>.

D. From the axioms A and Euler's equation (7) alone may

39. W. NOLL, « The foundations of classical mechanics in the light of recent advances in continuum mechanics », *The Axiomatic Method, with Special Reference to Geometry and Physics*, Amsterdam, North Holland, 1959, 266-281.

40. For discussion of this *principle of material indifference* and for an outline of its history, which goes back to work of Cauchy in 1829, see § 293 of *The Classical Field Theories*.

be proved an action-reaction principle, as follows : The sum of the mutual forces exerted by any two disjoint parts of a body free of singular loads is zero.

E. If the masses are supposed discrete, and if no couples are applied, the strong restriction called « Newton's third law » by some authors but rejected by others<sup>41</sup> can be proved as a consequence of the result stated in D and of Euler's equation (7) : The mutual forces between mass-points are central. Thus, if we are content to confine mechanics to systems of mass-points subject to binary forces obeying the principle of linear momentum, this restrictive « Newton's third law » is *equivalent* to the principle of moment of momentum<sup>42</sup>. I remark that those persons who consider the equation  $(\mathbf{r}_i - \mathbf{r}_j) \times \mathbf{F}_{ij} = 0$  to express the content of Newton's third law or to be otherwise a law of mechanics therefore have no ground for regarding it as either more or less fundamental than the principle of moment of momentum.

F. In Newton's own statement of his third law, there is no explanation of what kinds of « bodies » he had in mind or what he meant by their « actions » on each other. He neither stated that all forces in the universe are binary nor laid down any condition whatever on any other kind of « actions ». Not only in continuum mechanics do actions of a different sort occur. Ericksen has drawn my attention to work of Cauchy<sup>43</sup> in which, apparently, is considered a system of mass-points having a potential energy depending upon the positions of all the masses, not necessarily the sum of pair potentials. Ericksen shows that by aid of Cauchy's theorem on isotropic functions it follows from the requirement of material indifference, stated under C above, that the resultant torque of such a system of forces is zero. Therefore, for binary mutual forces the principle of material indifference is *equivalent* to the restriction to central forces, and the principle of moment of momentum follows. For more general mutual forces, not envisioned in (1) or in the statements about Newton's third law or action and reaction in any physics book I have ever seen, *the principle of moment*

41. Cf. Footnotes 5 and 26.

42. Proofs of this statement and that under D are given by Noll in the work cited in note 37 and are outlined in § 196 A of *The Classical Field Theories*.

43. « Mémoire sur les systèmes isotropes de points matériels », *Mém. Acad. Sci. Paris* 22, 615 (1850) = *Œuvres* (1) 2, 351-386.

*of momentum continues to follow from the principle of material indifference.* Similar theorems hold in the classical theory of finite elastic strain<sup>44</sup> and in the theory of linearly viscous fluids.

G. The foregoing remarks show that all the formulations of mechanics discussed here are equivalent, as far as each goes. The principle of moment of momentum and the principle of material indifference are infinitely more inclusive than any statement about mutual forces between or among mass-points. For sufficiently general systems<sup>45</sup>, they cease to be equivalent to each other.

H. In modern researches on mechanics all the axioms listed under A, B, and C are needed, and for pure mechanics they suffice. This statement holds even when the definition of moment of momentum has to be generalized, as is the case in Grad's theory of polyatomic gases<sup>46</sup> and Ericksen's theory<sup>47</sup> of liquid crystals.

It seems to me that the approach to mechanics outlined by the following steps is a practical one today :

1. The axioms satisfied by forces and torques are motivated, and to some extent verified by experiments, in statics. These axioms are taken over unchanged in kinetics.

2. The principles

$$\mathbf{F} = 0, \mathbf{L} = 0 \quad (8)$$

as necessary and sufficient for equilibrium are motivated, and to some extent verified by experiment, in statics. Following the path opened by James Bernoulli, we assume that motion has the effect of giving rise to apparent forces that are equal, per unit mass, to the negatives of the accelerations in those frames where (8) are the statical equations. Application of this

44. W. NOLL, « On the continuity of the solid and fluid states », *J. Rational Mech. Anal.* 4, 3-81 (1955). See § 16, Coroll. to Th. I.

45. E. g., the tensor  $ek^{mp}d_{sp}^s$ , where  $d$  is the stretching tensor, is invariant

under change of frame but is not symmetric; hence a stress tensor of this form satisfies the principle of material indifference but does not satisfy the principle of moment of momentum in a material subject to no body couples.

46. H. GRAD, « Statistical mechanics, thermodynamics, and fluid dynamics of systems with an arbitrary number of integrals », *Comm. Pure Appl. Math.* N.Y.U. 5, 455-494 (1952). See § 4.

47. J. L. ERICKSEN, « Anisotropic fluids », *Arch. Rational Mech. Anal.* 4, 231-237 (1959), and later papers.

d'Alembert-Euler principle leads to (7) as the general equations of dynamics, including both statics and kinetics.

3. The true and general idea, imperfectly expressed by Newton's third law and the literature concerned with it, is stated by *the principle of material indifference* : The properties of materials (« actions » of bodies on each other) are the same for all observers.