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FORCES IN NEWTONIAN DYNAMICS AND FORCES IN EULERIAN DYNAMICS

RUMYANA STOYANOVA, IVAN TCHOBANOV

Ab haedis segregare oves
St. Matthew's Gospel XXV, 32

Румяна Стоянова, Иван Чобанов. СИЛЫ В НЬЮТОНОВСКОЙ ДИНАМИКЕ И СИЛЫ В ЭЙЛЕРОВСКОЙ ДИНАМИКЕ

Существенное отличие между ньютоновской динамики массовых точек и эйлеровской динамики твердых тел состоясь в том, что пока первая основывается на одной только фундаментальной динамической аксиомы (Ax N или закон Ньютона о количестве движения массовой точки), то вторая основывается на двух независимых динамических аксиомах (Ax 1E и Ax 2E или законы Эйлера о количестве движения и о кинетическом моменте твердого тела). На этой основе в работе предлагается анализ понятия силы в этих двух главных ветвей аналитической механики. В то время, как реальные стандартные векторы являются адекватным орудием для математического представления сил в динамике массовых точек, в динамике твердых тел они для этой цели недостаточны. Необходимым и достаточным инструментом для адекватного математического представления сил в динамике твердого тела являются стрелы (иными словами, упорядоченные пары взаимно перпендикулярных векторов). Рассмотрения проведены на историческом фоне критических моментов в развитии динамики массовых точек и твердых тел и последствия динамической традиции Даламбера–Лагранжа.

Rumyana Stoyanova, Ivan Tchobanov. FORCES IN NEWTONIAN DYNAMICS AND FORCES IN EULERIAN DYNAMICS

The essential distinction between the Newtonian mass-point dynamics and the Eulerian rigid body dynamics consists in the fact that while the former is based upon, and is developed from, a single fundamental dynamical axiom (Ax N or Newton's law of momentum of a mass point), the latter is based upon, and is developed from, two independent dynamical axioms (Ax 1E and Ax 2E or Euler's laws of momentum and of moment of momentum of a rigid body). On this basis in the present paper an analysis of the notion of force in these two main branches of analytical mechanics is proposed. While the forces in mass-point dynamics are represented mathematically by real standard vectors, these vectors are found to be insufficient for the adequate mathematical representation of the forces in rigid body dynamics, for which the arrows (scilicet ordered pairs of normal real standard vectors) prove to be the necessary and sufficient mathematical tool. The considerations are implemented upon the historical background of the crucial moments in the developments of mass-point and rigid body dynamics and of the after-effects of D'Alembert-Lagrangean dynamical tradition.

This paper proposes, among other things, a short excursion in the not too distant past of that part of rational mechanics to which traditionally the adjective *analytical* is ascribed. Let us therefore first see how the land stands. In other words, let us clarify the meanings of the terms *Newtonian dynamics* and *Eulerian dynamics*.

Making a long story short, we may at once say that the questions "What does Newtonian dynamics mean?" and "What does Eulerian dynamics mean?" are answered quite simply if they are answered at all. Newtonian dynamics means *mass-point dynamics* and Eulerian dynamics means *rigid body dynamics*.

In such a manner one would know what does Newtonian dynamics mean if one knew what does mass-point dynamics mean, and to such a degree one would know what does Eulerian dynamics mean if one knew what does rigid body dynamics mean. Does one, however, really know the meanings of these two last terms, or are those answers only *qui pro quo*, *alias* transfer of the points of application of perplexities from one *locus* into another one?

However regrettable it may seem, the *nuda veritas* is that those are questions which are much more easily put than answered — if they are answered at all. By the way, this state of affairs is due to the idiosyncrasies of the true history of rational mechanics — the genuine one, we mean, not the popular, folklorish, mythological one, the one that reminds the fairy-tale of Newton's apple and has given Truesdell a good reason to complain about "the universal ignorance of the true history of mechanics" [1, p. 117].

By an irony of historical fate Rational Mechanics — a granddaughter of Astronomy, a daughter of Geometry, and the blood mother of Analysis — lapsed into the sorry plight of a poor kinswoman in the fabulously rich family of mathematical sciences. This aristocrat of mind was struck down in the sinister days of the French Revolution never to recover again through a genuine restoration act. The only attempt at her rebirth was made at the beginning of this century by Hilbert who included in his famous list of mathematical problems [2] Nineteenth Century bequeaths to Twentieth, as Problem Number Six, the problem of axiomatical consolidation of the logical foundations of rational or mathematical mechanics, "but this problem, like all those he proposed concerning the relation between mathematics and physical experience, has been neglected by mathematicians" [1, p. 336]. As a matter of fact, the subsequent development is even a negative one, since it only simulates axiomatics by ignorant profanations. Since this paper is alien to any polemic tendency whatever, the said above is enough and to spare. But Hilbert erred in his faith in Twentieth Century. His Sixth Problem is left to the next one, if God will permit it.

There are great similarities in the fates and stories of mechanics and chemistry. Both are as antique as Methuselah. Both are every nook and corner around us. Both are as mazy as labyrinth. Both are parts of physics, though small parts of it. Both have gone through millenial fallacies. Last but not least, in any of them there was a single man who all of a sudden, ultimately, and completely, was beginning to see through it.

Lavoisier in chemistry. Newton in mechanics.

One may read in traditional treatises on analytical mechanics — like [3] *exempli gratia* — statements like "The subject of particle dynamics was founded by Galileo early in the seventeenth century" (p. VII), and one may swallow the bait — hook, line, and sinker. They belong, however, to mechanical mythology, to historical enthusiasm, *s'il vous plaît* — by no means to historical reality, though. There is no dynamics at all to be found in Galileo's works, in general, and in his *Two New Sciences* [4], in particular. The most one may find there is kinematics — mathematical & experimental. As regards Galileo's mathematical kinematics, its originality is disputable; some words in this connection are said immediately below. As regards Galileo's experimental kinematics, its originality seems to be indubitable, and it ranks Galileo first in the line of the founders of the experimental scientific method — if one disregards the millenial works of alchemists.

If one puts trust in Truesdell's *Essays* [1], "Historians of letters had meanwhile created the myth of the 'Renaissance'. According to this myth, in the Middle Ages man hibernated beneath a pall of scholastic repetition, borrowed from Aristotle and enforced by the Church; the Renaissance, casting all this aside, opened its eyes and discovered man and the world by personal sensation. While for the arts this theory may be tenable as an expression of taste, in science other than anatomy it is not: The 'early' Renaissance, 1450–1500, stands in the front rank of competition for the most sterile half-century of Western mathematics and physics, and the only exact science of the 'late' or 'high' Renaissance, 1500–1550, is algebra, which grew, not from open-eyed wonder at the world, but rather from bookish study of Arabian mathematics. Where is the empirical science which should have crowned the 'rebirth' of knowledge? Galileo, whom some physicists enthrone as founder of the empiricism they claim as their own, came one hundred years later, in the late Mannierist and early Baroque periods ... such historians of science as then there were — a paltry scatter of semi-philisophers and science teachers — found several of Galileo's ideas, more or less, in Leonardo's notes. Previously, historians had believed that Galileo thought these things out of 'genius' applied to thin air. In 1788 Lagrange ... had written of Archimedes and Galileo, 'the interval separating these two great geniuses disappears in the history of mechanics', and this simple view, as satisfying as the account of creation given in Genesis, was good enough for Mach a century later ... 'Discovery' of Leonardo transferred the point of application of the same theory a century backward. He, too, had the same material to work with: 'genius' and thin air, and the remaining problem for this group of historians was only to see how Leonardo's ideas got to Galileo, thus making the latter a true grandson, if not son, of Renaissance" (pp. 25, 27).

Incipit Pierre Duhem:

"A scientist, as a sound historian of science must be ... Whatever a litterato may consider a reasonable activity for a 'genius', few scientists will believe that a thousand pages of mechanics [written by Leonardo's own hand], right or wrong, have *ever* sprung from the application of genius to thin air. The question Duhem asked himself was, where did Leonardo learn all this, and what fraction of it did he modify or add from his own thought or experience? In the attempt to answer these questions, Duhem became the first person since the Renaissance actually to read what the mediaeval schoolmen wrote on mechanics and physics. In a real sense, he discovered the science of Middle Ages, and he added, attached to concrete and definite discoveries and theorems, a long list of names never before encountered in histories of science: Jordanus de Nemore, Jean Buridan, Nicole Oresme, Richard Swineshead, William Heytesbury, and others. In the midst of his immortal historical discoveries Duhem had already reached the conclusion that ... 'In the mechanical work of Leonardo da Vinci there is no essential idea that does not come from the geometers of Middle Ages.'

Later studies by a school of painstaking historians, along with the publications of the texts Duhem used and others unknown to him, have revised much of the detail but little of the general aspects of the picture of mediaeval achievement drawn by Duhem. The now published sources ... prove to us, beyond contention, that the main kinematical properties of uniformly accelerated motions, still attributed to Galileo by the physics texts, were discovered and proved by scholars of Merton College — William Heytesbury, Richard Swineshead, and John of Dumbleton. Their work distinguished *kinematics*, the geometry of motion, from *dynamics*, the theory of causes of motion. Their approach was mathematical. They succeeded in formulating a fairly clear concept of instantaneous speed, which means that they foreshadowed the concept of function and derivative, and they proved that the space traversed by a uniformly accelerated motion in a given time is the same as that traversed by a uniformly motion whose speed is the mean of the greatest and least speeds in the accelerated motion. In principle, the qualities of Greek physics were replaced, at least for motions, by the numerical quantities that have ruled Western science ever since. This work was quickly diffused into France, Italy, and other parts of Europe. Almost immediately, Giovanni da Casale and Nicole Oresme found how to represent the results by geometrical graphs, introducing the connection between geometry and the physical world that became a second characteristic habit of Western thought — a habit so deep-seated that it is known to every carpenter and passes unremarked only in certain highly specialized professions.

... these ideas, which originated in England and France in the early fourteenth century, were discussed back and forth in periods of varying activity and inactivity in France, the Empire, and Italy in the latter half of the same century and were taught in Italian universities in the next one, at the end of which a flood of printed books opened the subject to everyone — everyone who could understand Latin and mathematics" (*ibid.*, 25, 27, 29–31).

In such a manner it becomes clear that a considerable part of the mathematical information included in *Giornata terza* and *Giornata quarta* of Galileo's *Discorsi* [4] and *eo ipso* attributed to him through ignorance, mental indolence, adoration of authority, or simply from force of habit, has been known quite a while ago, just like Euclid did not invent all the mathematics his books present; and that, not unlike Euclid, Galileo is worthy of praise for the organization, systematization, and propagation of this knowledge.

There is something more, however. Although uniformly accelerated motions were studied *as such* almost three centuries before Galileo, nevertheless to all appearances they have not been as yet connected with particular physical phenomena. In other words, scholastics came to know some properties of uniformly accelerated motions *if such existed*, but they could not point out a practical example of a concrete natural motion of uniform acceleration. Ten to one Galileo was the first not only to see, but also to prove, that the free fall of bodies (*Movimenti Locali*) is such an example, and accomplished this by means of an experimental demonstration. (*Gott sei Dank* that physical measurements are always false: failing this Galileo could not make his discovery, since in Nature there does not exist such a miracle like a constant force.)

And although Galileo undoubtedly had intuitive ideas about forces and motions as causes and effects, in his work there is not the slightest hint at all of any quantitative connection between these mechanical entities.

As regards some dynamical pre-Galilean ideas, Truesdell has drawn the following picture:

"A precursor of the later ideas of inertia, momentum, and energy may be found in the theory of 'impetus' put forward by Jean Buridan at Paris during the period when kinematical concepts were developed at Oxford. Rejecting Aristotle's view on the morion of projectiles, Buridan adopted an idea mentioned by John the Grammarian some 800 years earlier, namely that 'some incomporeal motive force is imparted by the projector to the projectile'. Buridan replaced this assertion of quality by a statement of quantity: '... by the amount more there is of matter, by that amount can the body receive more of the impetus and more intensely' and 'by the amount the mover moves the moving body more swiftly, by the same amount it will impress a stronger impetus'. Thus Buridan comes close to defining as 'impetus' what we now call 'momentum'. That a quantity of this kind measures the tendency of a body to persevere in motion, Buridan infers from what he calls 'experiences' — observations of simple phenomena concerning..." (*ibid.*, 31).

If we quote such vast excerptions from Truesdell's *Essays*, it is because of the extreme importance of the historical information they include and by reason of our aversion to making concealed plagiarism by giving a restatement of them. As far as our knowledge goes, in the whole span of mechanical literature before Newton one cannot find the haziest notion of that greatest scientific induction which is contained in Newton's dynamical *Lex* II from *Principia* [5]:

Mutationem motus proportionalem esse vi motrici impressae, et fieri secundum lineam rectam qua vis illa imprimitur.

Or, just the same, in Motte's Translation [6]:

The change of motion is proportional to the motive force impressed; and is made in the direction of the right line in which that force is impressed [7, I, p. 13].

The cardinal importance of this physical law (with the traditional school formulation *mass by acceleration equals force*) for the whole spectrum of natural sciences is trivially known in order to be underlined here. Much more interesting is the question of the psychological motives for this imprecedented scientific discovery. How did, in other words, Newton attain this achievement? Which considerations did he take into account in order to formulate his *Lex*? What did he conjecture, how did he guess, what did he fancy making his thoroughfare through the jungle of most multifarious motions and forces?

One cannot be sure. One may only guess.

One of the most annoying slips a historian of science may make is to antedate ideas. The ancient dictum *temporis filia veritas* may quite equitably be paraphrased in *temporis filia idea*. Velocity and force did not mean for Newton and his contemporaries the same things these terms mean for you and me. For us these notions have undergone a tricentenarial embryonal as well as extrauterine evolution. Their vectorial character is a much more ulterior discovery. Let us *exempli causa* peek into Newton's two first *Definitiones*:

Def. I. *Quantitas Materiae est mensura ejusdem orta ex illius Densitate et Magnitudine conjunctim.*

Def. II. *Quantitas motus est mensura ejusdem orta ex Velocitate et quantitate Materiae conjunctim.*

Or in Motte's Translation [7, I, p. 1]:

Definition I. *The quantity of matter is the measure of the same, arising from its density and bulk conjointly.*

Definition II. *The quantity of motion is the measure of the same, arising from the velocity and quantity of matter conjointly.*

In his commentaries explaining the meaning of Definitions I & II Newton says:

"It is this quantity [of matter] that I mean hereafter everywhere under the name of body or mass. And the same [quantity of matter] is known by the weight of each body, for it is proportional to the weight..." & "The [quantity of] motion of the whole is the sum of the [quantities of] motions of all the parts [of the body]..." [7, I, p. 1].

One can immediately see irreconsilable logical contradictions already in these initial statements of *Principia*. No subtle perspicacity is needed, indeed, to comprehend that if Newton's treatise is mechanics at all, it is exlusively mass-point dynamics. As Truesdell notes, "while Newton had used the word 'body' vaguely and in at least three different meanings, Euler realized that the statements of Newton are generally correct only when applied to masses concentrated at isolated points" [1, p. 107]. For mass-points, however, Newton's Definition I becomes meaningless, since for a point *densitas* and *magnitudo* disappear *conjunctim* like water at heating *ad totalem evaporationem*. But if Newton's bodies are mass-points, then the phrase "all the parts" in the second comment becomes meaningless too. And so on, and so forth, etcetera.

Not this is, however, the problem now. Let Newton's moving objects be mass-points; let velocities and forces have scalar, rather than vectorial, nature; let, for the sake of simplicity, the motion be rectilinear, as in the case of a free fall along the vertical. What made Newton think that mass by acceleration gives force?

Seeking a plausible answer of this question, let us first consult Newton's *Lex* I:

Corpus omne perseverare in statu suo quiescendi vel movendi uniformiter in directum, nisi quatenus a viribus impressis cogitur statum illum mutare.

Or in Motte's Translation [7, I, p. 13]:

Every body continues in its state of rest, or of uniform motion in a right line, unless it is compelled to change that state by forces impressed upon it.

In such a manner, Newton had three keys in his hand:

1. Buridan's idea that the "impetus" is increased when both the mass and velocity are increased.

2. No force — no acceleration.

3. Galileo's discovery that constant force (weight) generates uniformly accelerated motion.

Now (by our humble opinion) Newton's psychological motives:

Buridan's "impetus" being identified with Newton's "quantitas motus", the simplest formula connecting it qualitatively (causal nexus) and quantitatively with the idea of force — as vague, pale, and dim in Newton's head as it is in our heads today — and conformable to items 2 and 3 just now stated above consists in equalization of *vis* with *mutatio* of *impetus* as formulated in *Lex* II, "change" meaning, as it has been traditional in Newton-Leibniz days, differentiation (with respect to the time in this particular, *dynamical*, case). (Strange though it may seem, as far as our knowledge goes, nobody has hitherto made out that Newton's *Lex* I parasitizes in the umbra of *Lex* II. Indeed, put "force equal zero" in *Lex* II and you obtain *Lex* I. The hypothesis that Newton did not see this simple fact is unthinkable. Why did he not take it away? Heaven only knows. Some authors are inclined to see in *Lex* I an implicit definition of the term *materiae vis insita* or *innate force of matter.* Such an explanation is, however, untenable, since Newton's *Definitio* III is explicitly dedicated to this last term, as well as his *Definitio* IV is explicitly dedicated to the term *vis impressa* or *impressed force.* Let us note at that that the idea of an *implicit* or *axiomatical* definition is at least two centuries younger than Newton's *Principia* being Hilbert's invention.)

Any child would now see that the experimental and observational backing of *Lex* II by means of Galileo's law of free fall is a rather brittle one. Its fragility is menaced not only by its solitary confinement, the proclamation of *Lex* II in the capacity of an universal physical law on the basis of one and only natural precedent being as bold, daring, and venturesom logical act as the inductive inference from 1 to ∞. No, it is threatened also by the fact that even the free fall in the earth atmosphere is not a pure phenomenon, the resistance of the air exceeding in some cases the active force of gravity. That is why Newton badly needed new confirmations of his law — and found them in the celestial motions, sometimes even with the aid of forged data. (One may accept as a practical joke the affirmation that if the Earth's sky was as cloudy as Venusian heaven, then mechanics would never be created; but this is no joke at all. Celestial motions are "pure" in that sense that no unknown reactions of geometrical constraints accompany them; that is why Newton's *Law* II could be verified by their aid. On the contrary, any terrestrial movement is generated and simultaneously generates reactions of the constraints; being unknown — moreover, demanding dynamical determination themselves — these reactions admit no experimental verification of *Law* II as such, *an sich*, and *in se*.)

From a physical point of view mass-point and rigid body dynamics are phenomenological sciences. It is a tritiness to say that nothing can be proved concerning Nature, but this vapidity is especially true for the phenomenological approach. *Voilà* a rather impressive instance, as notoriously known as commonly ignored. Anybody writes H_2O, but does anybody know why? Why not HO? Or HO_2? Or $H_{999}O_{1001}$? Are there many professors of chemistry who know why they write H_2O? The most probable answer you may expact (screening the *nuda veritas* that they do so because others do so) is that it is proved. Is it really? Phenomenologically, at least (in other words, by virtue of the phenomenological chemical and physical laws, like those of Richter (1792–1802), Proust (1799–1806), Dalton (1802–1808), Gay-Lussac (1805–1808), Avogadro (1811), Mitscherlich (1818–1819), Dulong and Petit (1819), Faraday (1834), Hess (1840), Cannizzaro (1858) etc.), one can prove nothing of the kind. If we must speak the truth and shame the devil, then we must confess that the formula H_2O is a matter of faith rather than a point of proof.

In the light of those explanations one must realize crystal-clearly that this *enfant naturel*, this fruit of a not quite legal liaison of logic and psychology — Newton's *Lex* II — stands beyond proof: it has the mathematically social status of a postulate, or a hypothesis, or an axiom in Hilbertian sense of the word.

And that in Newton's original formulation, quoted above in English as well as in Latin in order to leave place for no uncertainty, this law represents a classical case of a *Circulus Vitiosus*. In other words, this original formulation of *Lex* II implies that it is true if it is true, or rather that if it is true, then it is untrue.

In order to penetrate into the nucleus of this mathematical fact, let us suppose that $Oxyz$ and $\Omega\xi\eta\zeta$ are orthonormal right-handed orientated Cartesian systems of reference with unit vectors $\boldsymbol{i}$, $\boldsymbol{j}$, $\boldsymbol{k}$ and $\boldsymbol{\xi}^\circ$, $\boldsymbol{\eta}^\circ$, $\boldsymbol{\zeta}^\circ$ of the axes Ox, Oy, Oz and $\Omega\xi$, $\Omega\eta$, $\Omega\zeta$, respectively, and let $\Omega\xi\eta\zeta$ be moving in some manner with respect to $Oxyz$. Let by definition $\boldsymbol{r}_\Omega = \boldsymbol{O\Omega}$ and $\boldsymbol{r} = \boldsymbol{OP}$, $\boldsymbol{\rho} = \boldsymbol{\Omega P}$ for any point P. Dots denoting derivatives with respect to the time t with regard to $Oxyz$, i.e.

$$\dot{\boldsymbol{r}} = \dot{x}\boldsymbol{i} + \dot{y}\boldsymbol{j} + \dot{z}\boldsymbol{k} \tag{1}$$

provided

$$\boldsymbol{r} = x\boldsymbol{i} + y\boldsymbol{j} + z\boldsymbol{k}, \tag{2}$$

let

$$\boldsymbol{\omega} = \frac{1}{2}(\boldsymbol{\xi}^\circ \times \dot{\boldsymbol{\xi}}^\circ + \boldsymbol{\eta}^\circ \times \dot{\boldsymbol{\eta}}^\circ + \boldsymbol{\zeta}^\circ \times \dot{\boldsymbol{\zeta}}^\circ) \tag{3}$$

be the instantaneous angular velocity of $\Omega\xi\eta\zeta$ with respect to $Oxyz$. Besides, let $\frac{\delta}{\delta t}$ denote by definition the local derivative with respect to t of any vector function with regard to $\Omega\xi\eta\zeta$, i.e.

$$\frac{\delta \rho}{\delta t} = \dot{\xi}\xi^\circ + \dot{\eta}\eta^\circ + \dot{\zeta}\zeta^\circ \tag{4}$$

provided

$$\rho = \xi\xi^\circ + \eta\eta^\circ + \zeta\zeta^\circ. \tag{5}$$

Then, as it is well-known, for any vector function $\boldsymbol{a}$ the identity

$$\dot{\boldsymbol{a}} = \boldsymbol{\omega} \times \boldsymbol{a} + \frac{\delta \boldsymbol{a}}{\delta t} \tag{6}$$

holds.

Under these conventions the identity

$$\boldsymbol{r} = \boldsymbol{r}_\Omega + \boldsymbol{\rho} \tag{7}$$

implies

$$\dot{\boldsymbol{r}} = \dot{\boldsymbol{r}}_\Omega + \boldsymbol{\omega} \times \boldsymbol{\rho} + \frac{\delta \boldsymbol{\rho}}{\delta t} \tag{8}$$

and

$$\ddot{\boldsymbol{r}} = \ddot{\boldsymbol{r}}_\Omega + \dot{\boldsymbol{\omega}} \times \boldsymbol{\rho} + \boldsymbol{\omega} \times (\boldsymbol{\omega} \times \boldsymbol{\rho}) + 2\boldsymbol{\omega} \times \frac{\delta \boldsymbol{\rho}}{\delta t} + \frac{\delta^2 \boldsymbol{\rho}}{\delta t^2}. \tag{9}$$

On the other hand, if P represents a mass-point with mass m, then its *motus* is $m\dot{\boldsymbol{r}}$ and $m\frac{\delta \boldsymbol{\rho}}{\delta t}$ with regard to $Oxyz$ and $\Omega\xi\eta\zeta$, respectively, and its *mutatio motus* is $m\ddot{\boldsymbol{r}}$ and $m\frac{\delta^2 \boldsymbol{\rho}}{\delta t^2}$ with regard to $Oxyz$ and $\Omega\xi\eta\zeta$, respectively, the mass m being, by a fundamental postulate of Newtonian dynamics, a constant with respect to the time t.

Let P be under the action of the *vis* $\boldsymbol{F}$ and let *Lex* II hold for both $Oxyz$ and $\Omega\xi\eta\zeta$. Then

$$m\ddot{\boldsymbol{r}} = \boldsymbol{F}, \qquad m\frac{\delta^2 \boldsymbol{\rho}}{\delta t^2} = \boldsymbol{F}. \tag{10}$$

Now (9), (10) imply

$$\ddot{\boldsymbol{r}}_\Omega + \dot{\boldsymbol{\omega}} \times \boldsymbol{\rho} + \boldsymbol{\omega} \times (\boldsymbol{\omega} \times \boldsymbol{\rho}) + 2\boldsymbol{\omega} \times \frac{\delta \boldsymbol{\rho}}{\delta t} = 0 \tag{11}$$

for any t. The condition (11) is, however, by no means obligatory for the motion of $\Omega\xi\eta\zeta$ with respect to $Oxyz$. In other words, there are systems $\Omega\xi\eta\zeta$ for which (11) is violated.

It is proved that a necessary and sufficient condition for the observance of the identity (11) for any mass-point P and for any force $\boldsymbol{F}$ acting on it is

$$\ddot{\boldsymbol{r}}_\Omega = \boldsymbol{0}, \qquad \boldsymbol{\omega} = \boldsymbol{0} \quad (\forall t). \tag{12}$$

Any motion of $\Omega\xi\eta\zeta$ with respect to $Oxyz$, satisfying the condition (12), is called a *rectilinear uniform translation* of $\Omega\xi\eta\zeta$ with respect to $Oxyz$.

In such a manner, if *Lex* II is true for the system of reference $Oxyz$ and the motion of the system $\Omega\xi\eta\zeta$ with respect to $Oxyz$ is not a rectilinear uniform translation, then *Lex* II is not true for $\Omega\xi\eta\zeta$. *Quod erat demonstrandum.*

There is a formula of Roman law: *Impossibilium nulla est obligatio.* Another formula reads: *Ignorantia juris nocet, ignoratio facti non nocet.* An ancient saw preaches: *Saeculi vitia, non hominis.* To charge Newton with flaws that could be seen only centuries later would be as ridiculous as to charge canibals with lack of democracy. But it is high time now things to be put on their right places. A modern formulation of *Lex* II may read as follows.

Ax N (*Newton's fundamental axiom* (*postulate, hypothesis, law*) *of mass-point dynamics*). There exists such a rigid Cartesian system of reference S that, all derivatives being taken with respect to S, for any mass-point P and for any system of forces $\underrightarrow{F}$ *acting* on P, the derivative with respect to the *time* of the momentum of P equals the resultant of $\underrightarrow{F}$.

Df N. Any system of reference satisfying Ax N is called *inertial according to Newton.*

Ax N is an axiom in the spirit and letter of Hilbert's axiomatical principle. All mathematical terms it involves are susceptible to strict definitions save two of them (printed above in italics): *acting* and *time*. Their meaning is defined implicitly namely by means of Ax N (along with other axioms of Newton's mass-point dynamics). If *acting* and *time* were definable too, then Ax N would be a (true or false) theorem rather than an axiom and would need a proof or a disproof.

The definable terms of Ax N are: *Cartesian system of reference*, *rigid* (Cartesian system if reference), *derivative* (of a vector function with respect to a Cartesian system of reference), *mass-point*, *system of forces*, *resultant* (of a system of forces), and *momentum* (of a mass-point with respect to a Cartesian system of reference). Now the question quite naturally arises: how are these things defined?

This question is similar to two other questions. As it is well known, any term of classical real and complex analysis is defined. How? The answer is: in the frames of the *fields R* and *C of all real* and *of all complex numbers*, respectively. It is by no means accidental that Landau entitled *Foundations of Analysis* his book [8] dedicated to the axiomatical approach to the whole, rational, irrational, complex numbers.

In the same way, any term Ax N contains save *acting* and *time* can be defined in the frames of the *real standard vector space V*; for the axiomatical definition of the latter see, for instance, the article [9], as well as [10] for a complex version.

We are now in a position to answer the question "What does *mass-point dynamics* mean?", and the answer is: *The mathematical theory based upon* Ax N (and other appropriate mechanical axioms).

We can also say what do *forces* in Newtonian dynamics mean. The answer is: *functions* in V, the meaning of this term being specified in the process of evolution of mass-point dynamics.

There is a point here that must not be left unmentioned. All things considered, Newtonian forces prove to be *real standard vectors*. Now, not any real standard vector is a force, as well as not any number is a length, or an area, or a volume, or a mass, etc. Length, for instance, is defined as a number — a number, however, connected with a certain curve line and satisfying specific conditions. In the same manner, a Newtonian force is defined as a real standard vector — but a vector in a dynamical context, obeying Ax N, say. The essential point is that the mathematical nature of this dynamical entity is vectorial.

And here lies the great difference between a Newtonian and an Eulerian force.

Before we enter this last point, let us, by the way, say some words concerning the developments immediately following the publication of *Principia*.

Although the following statement may sound extremely unpopular in the ears of a public hungry for fetiches, the undisguised under fig-leaves truth is that the mechanical content of *Principia* is improperly poor. "Except for certain simple if important special problems, Newton ***gives no evidence of being able to set up differential equations of motion for mechanical systems*** ... the cold fact is the equations are not in Newton's book... In Newton's *Principia* occur no equations of motion for systems of more than two free mass-points or more than one constrained mass-point; Newton's theories of fluids are largely false; and the spinning top, the bent spring, lie altogether outside Newton's range" [1, pp. 92–93].

The consequences are that "a large part of the literature of mechanics for sixty years following the *Principia* searches various principles with a view to finding the equations of motion for the systems Newton had studied and for other systems nowadays thought of as governed by the 'Newtonian' equations... The year in which the 'Newtonian equations' for celestial mechanics were first published is not 1687 but 1749... The first formulation of the general problem of celestial mechanics ... by means of a system of differential equations occurs in a paper of Euler, written in 1747 and published in 1749" [1, pp. 90, 92–93, 114].

Meanwhile a new actor comes on the mechanical stage. In 1743 D'Alembert published his dynamical treatise [11] destined not to remain unnoticed. The nuisance is not that this author goes back on his words, pouring out bountiful pledges on the title page of the book; the calamity is that in this *Traité* a new mechanical philosophy is developed, foreordained to play in the future havoc with the natural development of solid dynamics. Moreover, this book marks the beginning of an era when logical vices penetrate into mechanical morals.

D'Alembert is one of the most muddle-headed authors that ever entered the field of mechanics — in this respect the only one who could fittingly rival him is maybe Maupertuis. It is true that great historical generosity is needed sometimes to appreciate writings from this epoch, but D'Alembert's dynamical treatise goes beyond all bounds. It is pointless to quote even a single line from it, since any page simply swarms with nonsenses. The book may be used as a detector of hypocrisy: any dissimulator will insist that he makes any sense of it. To cap it all, the arrogance of its composer is unbreakable. To get some idea about his effrontery, it is sufficient to point out that he claims to solve, on a single page at that, the following *Problême general*, and to proclaim his "solution" in the capacity of a "general principle for the determination of the movements of several bodies which interact in an arbitrary manner":

Soit donné un système de Corps disposés les uns par rapport aux autres d'une maniére quelconque; et supposons qu'on imprime à chacun de ces Corps un Mouvement particulier, qu'il ne puisse suivre à cause de l'action des autres Corps, trouver le Mouvement que chaque Corps doit prendre.

A mere glimpse at D'Alembert's "solution" is sufficient to find it as ingenious as Immaculate Conception. As such it appears half a century later in Lagrange's *Méchanique Analitique* (sic) [12] under the name of "principle of D'Alembert". For the history of rational mechanics this has been a genuine catastrophe.

The Great Fault that gave rise to it consists in D'Alembert's outlook on the mathematical nature of the rigid body concept. Borrowing his own view-point from D'Alembert's ideas, Lagrange figures to himself a rigid body as "an assembly of corpuscules or mass-points joined together in such a way as always to conserve their mutual distances" [13, cited according to 1, p. 259]. Strange enough, more than two centuries it came to nobody's mind to put this mechanical philosophy to a mathematical test. In other words, no one asked himself whether all this is possible or not, alias, what will keep these "corpuscules or mass-points" at constant mutual distances. In point of fact this check-up is as easy as shelling peas.

If it is possible to maintain n mass-points at constant distances for any n, then the same should be true for $n = 2$. Let P_ν be free mass-points with masses m_ν and radius-vectors $\boldsymbol{r}_\nu$, respectively, supported at constant distance by the forces $\boldsymbol{F}_\nu$, $\nu = 1, 2$. In other words, there exist by hypothesis such forces $\boldsymbol{F}_\nu$ that the conditions

(13) $$m_\nu \ddot{\boldsymbol{r}}_\nu = \boldsymbol{F}_\nu \quad (\nu = 1, 2; \quad \forall t),$$

(14) $$\frac{d}{dt}(\boldsymbol{r}_1 - \boldsymbol{r}_2)^2 = 0 \quad (\forall t)$$

are satisfied. Now (14) is equivalent with

(15) $$(\boldsymbol{r}_1 - \boldsymbol{r}_2)(\boldsymbol{v}_1 - \boldsymbol{v}_2) = 0 \quad (\forall t),$$

provided by definition $\boldsymbol{v}_\nu = \dot{\boldsymbol{r}}_\nu$, $\nu = 1, 2$. Especially, for $t = 0$ (15) implies

(16) $$(\boldsymbol{r}_{10} - \boldsymbol{r}_{20})(\boldsymbol{v}_{10} - \boldsymbol{v}_{20}) = 0,$$

provided by definition $\boldsymbol{r}_{\nu 0} = \boldsymbol{r}_\nu(0)$, $\boldsymbol{v}_{\nu 0} = \boldsymbol{v}_\nu(0)$, $\nu = 1, 2$.

But (16) is impossible — it is an absurdity! Indeed, it imposes restrictions on the initial conditions of a dynamical problem involving *free* mass-points contrary to all good mathematical manners and customs. This unthinkable conclusion is an immediate logical corollary from the hypothesis that there exist forces realizing constant distance between the two mass-points, ergo Lagrange's mental picture of a rigid body as "an assembly of corpuscules or mass-points" belongs to the sphere of scientific fiction.

Let Σ be a system of mass-points $P_\nu(m_\nu, \boldsymbol{r}_\nu)$, $\nu = 1, \ldots, n$, subject to holonomical geometrical constraints, and let q_λ, $\lambda = 1, \ldots, l$, be mutually independent parameters determining their positions in space. Let $\boldsymbol{F}_\nu$ and $\boldsymbol{R}_\nu$ be respectively the active and passive force (reaction of the constraint) acting on P_ν, $\nu = 1, \dots, n$. Then it is proved that

$$(17) \qquad \sum_{\nu=1}^{n} \left(\frac{d}{dt}(m_\nu \boldsymbol{v}_\nu) - \boldsymbol{F}_\nu - \boldsymbol{R}_\nu \right) \frac{\partial \boldsymbol{r}_\nu}{\partial q_\lambda} = \frac{d}{dt}\frac{\partial T}{\partial \dot{q}_\lambda} - \frac{\partial T}{\partial q_\lambda} - Q_\lambda^{(a)} - Q_\lambda^{(p)},$$

$\lambda = 1, \dots, l$, where by definition $\boldsymbol{v}_\nu = \dot{\boldsymbol{r}}_\nu$, $\nu = 1, \dots, n$, and

$$(18) \qquad T = \frac{1}{2}\sum_{\nu=1}^{n} m_\nu v_\nu^2, \quad Q_\lambda^{(a)} = \sum_{\nu=1}^{n} \boldsymbol{F}_\nu \frac{\partial \boldsymbol{r}_\nu}{\partial q_\lambda}, \quad Q_\lambda^{(p)} = \sum_{\nu=1}^{n} \boldsymbol{R}_\nu \frac{\partial \boldsymbol{r}_\nu}{\partial q_\lambda},$$

$\lambda = 1, \dots, l$.

The equations (17) are *identities*. In other words, they are derived only on the basis of the definitions of the corresponding mathematical entities involved, without use of any dynamical hypothesis whichever.

If now one accepts Newton's *Lex* II, i.e.

$$(19) \qquad \frac{d}{dt}(m_\nu \boldsymbol{v}_\nu) - \boldsymbol{F}_\nu - \boldsymbol{R}_\nu = \boldsymbol{0}, \quad \nu = 1, \dots, n,$$

in the case considered, and if the geometrical constraints are *ideal*, i.e.

$$(20) \qquad \sum_{\nu=1}^{n} \boldsymbol{R}_\nu \frac{\partial \boldsymbol{r}_\nu}{\partial q_\lambda} = 0, \quad \lambda = 1, \dots, l,$$

then (17), (18) imply

$$(21) \qquad \frac{d}{dt}\frac{\partial T}{\partial \dot{q}_\lambda} - \frac{\partial T}{\partial q_\lambda} - Q_\lambda^{(a)} = 0, \quad \lambda = 1, \dots, l,$$

i.e. Lagrange's equations for the system S. As it is seen from (17), the left-hand sides of Lagrange's equations for a discrete system of mass-points are purely and simply linear combinations of the projections of the left-hand sides of Newton's equations (19) on appropriate axes, defined in direction by means of the vectors $\frac{\partial \boldsymbol{r}_\nu}{\partial q_\lambda}$, $\lambda = 1, \dots, l$; $\nu = 1, \dots, n$.

After this excursion through the thickets of Newtonian mass-point dynamics, let us turn our attention to Eulerian rigid body dynamics. As a matter of fact, the whole of it is a creation of Euler's own hands. Amongst hundreds of articles he dedicated to problems of rational mechanics we shall quote only two: the paper [14] containing the first version of his renowned dynamical equations describing the motion of a rigid body around its mass center, and the work [15] containing the first in the whole history of mechanics realization of the fact that the same two dynamical laws — the principles of momentum and of moment of momentum — which govern the motion of any solid, become in the same time, if properly reformulated, those mathematical canons that rule the dynamical behaviour of any mechanical system whichever, no matter rigid or elastic, no odds liquid or gaseous. At that the substantive "principles" are by no means fortuitously used above: those laws are, by their very essence, mathematical postulates or axioms, in other words, indemonstrable assertions which one may accept or cast aside, but not prove or disprove. They are not liable to discussions, too, by virtue of the ancient dogma *de principiis non est disputandum.*

Applied to rigid bodies, in a modern formulation these laws read as follows:

Ax 1 E (*Euler's first dynamical axiom* or *postulate* (*hypothesis, law*) *of momentum of a rigid body*). There exists such a rigid Cartesian system of reference S that, all derivatives being taken with respect to S, for any rigid body B and for any system of forces $\underset{\rightarrow}{F}$, *acting* on B, the derivative with respect to the *time* of the momentum of B equals the basis of $\underset{\rightarrow}{F}$.

Df E. Any system of reference satisfying Ax 1 E is called *inertial according to Euler.*

Ax 2 E (*Euler's second dynamical axiom* or *postulate* (*hypothesis, law*) *of moment of momentum* (*kinetical moment*) *of a rigid body*). S being an inertial according to Euler system of reference and all derivatives being taken with respect to S, for any rigid body B and for any system of forces $\underset{\rightarrow}{F}$, *acting* on B, the derivative with respect to the *time* of the moment of momentum of B equals the moment of $\underset{\rightarrow}{F}$, both moments being taken with respect to the origin of S.

The same remarks may be made apropos of the mathematical terms involved in Ax 1 E and Ax 2 E as those already made apropos the terms involved in Ax N. Therefore we shall spare them, confining our exposition to the remark that the only indefinable entities are again *acting* and *time.*

There is a great difference, though, between Ax N, on the one hand, and Ax 1 E and Ax 2 E, on the other hand, apart from the fact that in the rigid body case there are two dynamical axioms rather than a single only. Indeed, if in mass-point dynamics forces are represented mathematically by real standard vectors, the elements of V are already insufficient for representing forces in rigid body dynamics.

It turns out, however, that V remains, if indirectly, the repository, where the requisites of rigid body dynamics are preserved. Let the set W be defined by

$$W = \quad \{(\boldsymbol{s}, \boldsymbol{m}) \in V^2 : \boldsymbol{s} \neq 0, \boldsymbol{sm} = 0 \vee \boldsymbol{s} = \boldsymbol{m} = 0\} \tag{22}$$

and let the elements of W be called *real standard arrows.* Now any force in rigid body dynamics is an arrow in the same manner as any force in mass-point dynamics is a vector.

For a developed algebraic theory of arrows see, for instance, the articles [16–18]. We shall restrict ourselves to the following concise information. If

$$\overrightarrow{\boldsymbol{F}} = (\boldsymbol{F}, \boldsymbol{M}) \tag{23}$$

is a *force* (i.e. $\overrightarrow{\boldsymbol{F}} \in W$), then $\boldsymbol{F}$ is called the *basis* and $\boldsymbol{M}$ is called the *moment* of $\overrightarrow{\boldsymbol{F}}$. In Euler's dynamics finite systems of arrows are considered. If $\underset{\rightarrow}{F}$ is such a system, then the *basis* and the *moment* of $\underset{\rightarrow}{F}$ are defined as the sums of the bases and the moments, respectively, of all the elements of $\underset{\rightarrow}{F}$. Euler's mechanical philosophy is antipodal to Lagrange's. Euler regards a rigid body a specific mechanical entity or integrity, in the same way as a geometer considers a pyramide, say, an autonomous geometrical entirety or wholeness rather than the aggregate, or totality, or set of its points. Let the solid B be under the action of the active forces

$$\overrightarrow{\boldsymbol{F}}_\mu = (\boldsymbol{F}_\mu, \boldsymbol{M}_\mu), \quad \mu = 1, \ldots, m, \tag{24}$$

the moments $\boldsymbol{M}_\mu$, $\mu = 1, \ldots, m$, being taken with respect to the origin O of an inertial according to Euler system of reference $Oxyz$; let B be subject to geometrical constraints generating passive forces

$$\overrightarrow{\boldsymbol{R}}_\nu = (\boldsymbol{R}_\nu, \boldsymbol{N}_\nu), \quad \nu = 1, \ldots, n, \tag{25}$$

the moments $\boldsymbol{N}_\nu$, $\nu = 1, \ldots, n$, being also taken with respect to O; let by definition

$$\boldsymbol{F} = \sum_{\mu=1}^{m} \boldsymbol{F}_\mu, \quad \boldsymbol{R} = \sum_{\nu=1}^{n} \boldsymbol{R}_\nu, \quad \boldsymbol{M} = \sum_{\mu=1}^{m} \boldsymbol{M}_\mu, \quad \boldsymbol{N} = \sum_{\nu=1}^{n} \boldsymbol{N}_\nu; \tag{26}$$

if P denotes any point of B, $\boldsymbol{r} = \boldsymbol{OP}$, $\boldsymbol{v} = \dot{\boldsymbol{r}}$, let by definition

$$\boldsymbol{K} = \int \boldsymbol{v}\, dm, \quad \boldsymbol{L} = \int \boldsymbol{r} \times \boldsymbol{v}\, dm \tag{27}$$

be respectively the momentum and the moment of momentum of B with respect to $Oxyz$. Under these conventions, the mathematical formulations of Ax 1 E and Ax 2 E read

$$\dot{\boldsymbol{K}} = \boldsymbol{F} + \boldsymbol{R}, \quad \dot{\boldsymbol{L}} = \boldsymbol{M} + \boldsymbol{N}, \tag{28}$$

respectively.

Read any textbook, treatise, or monograph on analytical dynamics, say [3], where Lagrange's dynamical equations (21) or some of their multitudinous versions are derived. What do you find there? Do you find any rigid body? No, you find nothing of the kind. The most you can find is a finite system Σ of mass-points $P_\nu(m_\nu, \boldsymbol{r}_\nu)$, subject to holonomic geometrical constraints, giving rise to mutually independent parameters q_λ, $\lambda = 1, \ldots, l$, and to passive forces $\boldsymbol{R}_\nu$, $\nu = 1, \ldots, n$; if now $\boldsymbol{F}_\nu$ are active forces, acting on P_ν, respectively, $\nu = 1, \ldots, n$, you find in those writings routine mathematical operations based on cockabundy of the sort

$$\sum_{r=1}^{N} (m_r \ddot{x}_r - X_r)\delta x_r = 0 \tag{29}$$

[3, Eq. (3.1.1)] — quite solemnly, without a grain of humour, called "the fundamental equation for a dynamical system ... a generalization both of the principle of Virtual Work in statics, and of d'Alembert's principle for a single rigid body" [3, p. 28]; and as an end product you see Lagrange's equations (21). **Nota Bene**: $\boldsymbol{F}_\nu$ and $\boldsymbol{R}_\nu$ are vectors! And these equations claim to rule the motions of a solid!

Ex nihilo nihil. Nobody — Hindoo fakirs inclusive — is in a position to restore Euler's dynamical axioms (28) out of Lagrange's dynamical equations (21).

At last, let us say some words about something Lagrange, strange though it may seem, never knew, namely a mathematical inference of Lagrange's equations for rigid bodies. Let the system of reference $\Omega\xi\eta\zeta$ be invariably connected with the solid B; let $\boldsymbol{\rho} = \boldsymbol{\Omega P}$; let the mass m and the mass center G of B be defined by

$$(30) \qquad m = \int dm, \quad \boldsymbol{\rho}_G = \frac{1}{m}\int \boldsymbol{\rho}\, dm,$$

provided $\boldsymbol{\rho}_G = \boldsymbol{\Omega G}$; at last, let by definition

$$(31) \qquad \boldsymbol{L}_\Gamma = \int \boldsymbol{\rho} \times (\boldsymbol{\omega} \times \boldsymbol{\rho})\, dm - m\boldsymbol{\rho}_G \times (\boldsymbol{\omega} \times \boldsymbol{\rho}_G),$$

provided (3). It is proved that (28) are equivalent with

$$(32) \qquad \dot{\boldsymbol{K}} - \boldsymbol{F} - \boldsymbol{R} = 0, \quad \dot{\boldsymbol{L}}_\Gamma - \boldsymbol{M} - \boldsymbol{N} = 0.$$

If now q_λ, $\lambda = 1, \ldots, l$, are independent parameters of the rigid body B, then it is proved that, similarly to (17), the equations

$$(33) \quad (\dot{\boldsymbol{K}} - \boldsymbol{F} - \boldsymbol{R})\frac{\partial \boldsymbol{r}_G}{\partial q_\lambda} + (\dot{\boldsymbol{L}}_\Gamma - \boldsymbol{M} - \boldsymbol{N})\frac{\partial \boldsymbol{\omega}}{\partial \dot{q}_\lambda} = \frac{d}{dt}\frac{\partial T}{\partial \dot{q}_\lambda} - \frac{\partial T}{\partial q_\lambda} - Q_\lambda^{(a)} - Q_\lambda^{(p)},$$

$\lambda = 1, \ldots, l$, where by definition $\boldsymbol{r}_G = \boldsymbol{r}_\Omega + \boldsymbol{\rho}_G$ and

$$(34) \qquad T = \frac{1}{2}\int v^2\, dm, \quad Q_\lambda^{(a)} = \sum_{\mu=1}^{m} \boldsymbol{F}_\mu \frac{\partial \boldsymbol{r}_\mu}{\partial q_\lambda}, \quad Q_\lambda^{(p)} = \sum_{\nu=1}^{n} \boldsymbol{R}_\nu \frac{\partial \boldsymbol{r}_{m+\nu}}{\partial q_\lambda},$$

$\lambda = 1, \ldots, l$, $\boldsymbol{r}_\mu$ and $\boldsymbol{r}_{m+\nu}$ being the radius vectors of points of B coinciding in the moment of time t with the directrices of the forces $\boldsymbol{F}_\mu$ and $\boldsymbol{R}_\nu$, respectively, $\mu = 1, \ldots, m$; $\nu = 1, \ldots, n$, i.e.

$$(35) \qquad \frac{d}{dt}(\boldsymbol{r}_\nu - \boldsymbol{r}_\Omega) = \boldsymbol{\omega} \times (\boldsymbol{r}_\nu - \boldsymbol{r}_\Omega), \quad \nu = 1, \ldots, m+n,$$

$$(36) \qquad \boldsymbol{r}_\mu \times \boldsymbol{F}_\mu = \boldsymbol{M}_\mu, \quad \boldsymbol{r}_{m+\nu} \times \boldsymbol{F}_\nu = \boldsymbol{N}_\nu,$$

$\mu = 1, \ldots, m$; $\nu = 1, \ldots, n$.

Like (17), equations (33) are *identities*. In other words, they are derived on the basis of the definitions of the corresponding mathematical entities involved by means of purely identical transformations, without the use of any dynamical hypothesis whichever.

If now one accepts Euler's dynamical axioms Ax 1 E and Ax 2 E or, just the same, their mathematical equivalents (32), and if the geometrical constraints are *ideal*, i.e.

$$(37) \qquad \sum_{\nu=1}^{n} \boldsymbol{R}_\nu \frac{\partial \boldsymbol{r}_{m+\nu}}{\partial q_\lambda} = 0, \quad \lambda = 1, \ldots, l,$$

then (33), (34) imply (21), i.e. Lagrange's equations for the solid B. As it is seen from (33), the left-hand sides of Lagrange's equations for a rigid body B are, purely and simply, linear combinations of the projections of the left-hand sides of Euler's axioms (32) on appropriate axes, defined in direction by means of the vectors $\dfrac{\partial \boldsymbol{r}_G}{\partial q_\lambda}$ and $\dfrac{\partial \boldsymbol{\omega}}{\partial \dot{q}_\lambda}$, $\lambda = 1, \ldots, l$.

The equations (17) and (33) are called the *fundamental identities of Lagrangean formalism* — for discrete systems of mass-points and for rigid bodies, respectively, or, just the same, for Newtonian and Eulerian dynamics respectively.

Summa summarum: Newtonian mass-point dynamics and Eulerian rigid body dynamics are two entirely independent mathematical theories, with different basic objects and based upon different dynamical axioms. Forces in Newtonian dynamics are represented mathematically by real standard vectors, while forces in Eulerian dynamics are represented mathematically by real standard arrows. There is no certitude *a priori* that inertiality according to Newton is inertiality according to Euler and vice versa, or that time in Newtonian dynamics is the same thing as time in Eulerian dynamics. It is desirable to be so, but *ab posse ad esse non valet consequentia*: being possible, these things must be secured by means of special dynamical axioms. Is it, or is it not, pedagogically advantageous to expose *conjunctim*, as Newton sais, Newtonian and Eulerian dynamics, is a question that could be debated, but logically such a unification is not predeterminated. The tendency, modern today, towards "general mechanics" is not solely a mathematical snobbery of the first water, paying tribute to the vacuous motto *la généralité pour la généralité* and derided by the formula "Be wise, generalize!" of practical mathematical moralism — moreover, it may be harmful. Dose makes poison. The price of megalomania is as high in mathematics as in everydays life, and there is a certain limit of enterprise in both, across which enlarging tendencies become contraproductive.

D'Alembert's and Lagrange's faith that solid dynamics may be deduced from mass-point dynamics is a scientific phantom like Phlogiston and Thermogène — a popular fallacy which strangled the great Newton–Bernoulli–Euler's dynamical tradition *ab incunabilis* and caused immensurable damages to rational mechanics. No progress toward any solution of Hilbert's Sixth Problem, in one sense or another, is possible unless and until these ideological abberations are burried in the past — where they belong.

REFERENCES

1. Truesdell, C. Essays in the History of Mechanics. Berlin — Heidelberg — New York, 1968.
2. Hilbert, D. Mathematische Probleme. Vortrag, gehalten auf dem internationalen Mathematiker-Kongreß zu Paris 1900. Aus den *Nachr. der. K. Ges. der Wiss. zu Göttingen*, Math.-phys. Klasse, 1900, Heft 3. Mit Zusätzen des Verfassers. *Arch. d. Math. u. Phys.*, III. Reihe, 1, 1901, 44–63, 213–237.
3. Pars, L. A. A Treatise on Analytical Dynamics. London, 1965.
4. Discorsi e dimostrazioni matematiche, intorno à due nuoue Scienze Attentati alla Mecanica & i Movimenti Locali; del Signor Galileo Galilei Linceo, Filosofo e Matematico primario del Serenissimo Grand Duca di Toscana. Con vna Appendice del centro di grauità d'alcuni Solidi. In Leida, Appresso gli Elsevirii. M.D.C.XXXVIII.
5. Philosophiae Naturalis Principia Mathematica. Autore Js. Newton, Trin. Coll. Cantab. Soc. Matheseos Professore Lucassiano, & Societatis Regalis. Sodali. Imprimatur S. Pepys, Reg. Soc. Praeses. Julii 5, 1686. Londini, Jussu Societatis Regae ac Typus Josephy

Streater. Prostat apud plures [in the second issue, also dated Julii 5, 1686: Prostant Venales apud Sam. Smith ad insignia Principis Walae in Coermitio D. Pauli, aliosq; nonnullos]. Bibliopolas, Anno MDCLXXXVII.

6. The Mathematical Principles of Natural Philosophy. By Sir Isaac Newton. Translated into English by Andrew Motte. To which is added: The Laws of the Moon's Motion, According to Gravity. By John Machin, Astron. Prof. Gresh. and Secr. R. Soc. In two volumes. London: Printed for Benjamin Motte, at the Middle Temple-Gate, In Fleet-street. MDCCXXIX.
7. Sir Isaac Newton's Mathematical Principles of Natural Philosophy and his System of the World. Translated into English by Andrew Motte in 1729. The translation revised and supplied with a historical and explanatory appendix, by Florian Cajori. Volume One: The Motion of Bodies. Volume Two: The System of the World. University of California Press, Berkeley and Los Angeles, 1966.
8. Landau, E. Grundlagen der Analysis (Das Rechnen mit ganzen, rationalen, irrationalen, komplexen Zahlen). Leipzig, 1930.
9. Christova, I, I. Chobanov. On the definition of vector multiplication. *Год. Соф. унив., Фак. мат. мех.* , **70**, 1975–1976, 281–289.
10. Chobanov, I. Complex standard vector spaces. *Год. Соф. Унив., Фак. мат. мех.*, **81**, 1975, 3–26.
11. Traité de Dynamique, dans lequel les loix de l'Equilibre & du Mouvement des Corps sont réduites au plus petit nombre possible, & démontrées d'une maniére nouvelle, & où l'on donne un Principe générale pour trouver le Mouvement de plusieurs Corps qui agissent les uns sur les autres, d'une maniére quelconque. Par M. d'Alembert, de l'Académie Royale des Sciences. A Paris, Chez David l'aîné, Libraire, rue Saint Jacques, à la Plume d'or. MDCCXLIII. Avec Approbation et Privilege du Roi.
12. Méchanique Analitique; par M. de la Grange, de l'Académie des Sciences de Paris, de celles de Berlin, de Pétersbourg, de Turin, etc. A Paris, Chez la Veuve Desaint, Libraire, rue du Foin S. Jacques. MDCCLXXXVIII. Avec Approbation et Privilège du Roi.
13. Lagrange, J.-L. Nouvelle solution du problème du mouvement de rotation d'un corps de figure quelconque qui n'est animé par aucune force acceleratrice. Nouv. Mém. Acad. Berlin, 1773, 85–120.
14. Euler, L. Découverte d'un nouveau principe de mécanique. *Hist. Acad. Sci. Berlin*, **6**, 1752, 185–217.
15. Euler, L. Nova methodus motum corporum rigidorum determinandi. *Novi Comm. Acad. Sci. Petrop.*, **20**, 1775, 208–238.
16. Hristova, I., R. Stoyanova, I. Tchobanov. Introduction to an algebraic theory of arrows. I. *Год. Соф. Унив., Фак. мат. мех.*, **71**, 1, 1976–1977, 5–53.
17. Chobanov, I. Introduction to an algebraic theory of arrows. II. *Год. Соф. Унив., Фак. мат. инф.*, **82**, 1, 1988, 63–117.
18. Chobanov, I. Introduction to an algebraic theory of arrows. III. *Год. Соф. Унив., Фак. мат. инф.*, **82**, 1, 1988, 127–172.

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