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LAGRANGE OR EULER?
PART TWO: THE PRESENT

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> The truth, Mr. Blake, has a habit of making itself known. Even after many years.
>
> Agatha Christie: *Five Little Pigs*

Георги Чобанов, Иван Чобанов. ЛАГРАНЖ ИЛИ ЭЙЛЕР? ЧАСТЬ ВТОРАЯ: НАСТОЯЩЕЕ

Эта работа является второй частью серии статьей по вопросу о динамических традиций Эйлера и Лагранжа, первая часть [1] которой была опубликована в том же самом *Ежегоднике*. Настоящее состояние вещей в аналитической механике можно характеризовать как эклектическое совместное существование двух противоположных тенденций — динамических традиций Эйлера и Лагранжа. В этой статье анализируются самые типичные черты этих традиций и делаются соответствующие заключения. Согласно последных, невозможна какая-нибудь альтернатива динамических аксиом Эйлера (законов или принципах о количестве движения и кинетическом моменте твердого тела), а динамические уравнения Лагранжа являются лишь их проекциями на подходящих осей. Эта работа является предварительной подготовкой для решения шестой проблемы Гильберта об аксиоматической консолидации логических основ аналитической механики.

Georgi Chobanov, Ivan Chobanov. LAGRANGE OR EULER? PART TWO: THE PRESENT

This paper represents the second part of a series of articles on the Eulerian and Lagrangean dynamical traditions, the first part [1] of which was published in this *Annual*. The present state of affairs in analytical mechanics may be characterized as an eclectic coexistence of two opposite tendencies — the Eulerian and the Lagrangean dynamical traditions. The most typical features of both are analised in this article and corresponding conclusions are drawn. According to them, no alternative of the Eulerian dynamical axioms (the laws or principles of momentum and of kinetical moment of rigid bodies) is possible, the Lagrangean dynamical equations being only their projections on appropriate axes. This paper is a preliminary preparation for the solution of Hilbert's sixth problem concerning the axiomatical consolidation of the logical foundations of analytical mechanics.

Several years ago we have published the first part [1] of a series of articles dealing with the Eulerian and Lagrangean dynamical traditions. This is a topic we have worked out in the course of a clear decade, see for instance the article [2]. The paper [1] contains a brief historical account on the conception, birth, and flourish of the Lagrangean-dynamical tradition, antagonistic to the Bernoullis-Eulerian one. The *modus operandi* of the Lagrangean approach toward analytical mechanics has been aimed at the strangulation *in-cunabula*, if not in embryo, of the Eulerian dynamical tradition. Its after-effects on the supervenient development of rational mechanics, in general, and of analytical dynamics, in particular, resulted in an utmost formalism that transmuted into the domineering mechanical philosophy of the twentieth century. This philosophy is still in its climax in the current mechanical literature even of recent time.

Our initial intention, as regards this second part, has been to proclaim the anticlimax of the Lagrangean dynamical tradition. This is obviously not an easy task, especially if one aims at fundamentalistic goals, as we did, namely the solution of Hilbert's sixth problem concerning the axiomatical consolidation of the logical foundations of analytical mechanics — a problem, at the face of which the Lagrangean dynamical tradition is entirely helpless. In order to expose clearly the rise and fall of the Lagrangean dynamical tradition we had to replough a virgin soil, since never before, in the course of a well-nigh a century, has rational mechanics been even in the slightest touched by that critical spirit which made contemporary mathematics what it now is. The results of these efforts are reflected in the present paper — some of them, at least. In order to attain them, however, we badly needed time. This is the main reason for the ten years' delay of the pnblication of this second part of the series in question.

In the meantime a considerable list [3 — 24] of publications dealing with the interplay of the Eulerian and Lagrangean dynamical traditions has been printed in this *Annual*. Now some words about these articles are, to a not unconsiderable degree, necessary. As a matter of fact, the problems therein discussed are cornerstones on the meandering way one has to follow in order to throw light upon the very essence of the Lagrangean dynamical tradition.

What does, by the way, this term mean? What do the authors of mechanical writings bear in mind when using the word combination "Lagrangean mechanics"?

The Lagrangean dynamical tradition has been conceived in the moment when Lagrange's notorious mechanical work *Méchanique Analitique* (sic!) [25] has seen the light of day. In the *Avertissement* of this *Traité* which is the only part of his treatise well known nowadays by the mathematicians he has written:

"On a déjá plusieurs Traités de Méchanique, mais le plan de celui-ci est entiérement neuf. Je me suis proposé de réduire la théorie de cette Science, et l'art de résoudre les problèmes qui s'y rapportent, à des formules générales, dont le simple développement donne toutes les équations nécessaires pour la solution de chaque probléme. J'espère que la manière dont j'ai tâché de remplir cet objet ne laissera rien à desirer.

Cet Ouvrage aura d'ailleurs une autre utilité: il réunira et présentera sous un même point de vue les différents principes trouvés pour faciliter des questions de Méchanique, en montrera la liaison et la dépendance mutuelle, et mettra à portée de juger de leur justesse et de leur étendue.

Je le divise en deux Parties: la Statique ou la Théorie de l'Équilibre, et la Dynamique ou la Théorie du Mouvement, et chacune de ces Parties traitera séparément des corps solides et des fluides.

On ne trouvera point de Figures dans cet Ouvrage. Les méthodes j'y expose ne démandent ni constructions, ni raisonnements géométriques ou méchaniques, mais seulement des opérations algébriques, assujetties à une marche régulière et uniforme."

All these statements of Lagrange's and all the claims he has laid to the mechanical performances the *Méchanique Analitique* contains have been taken by his contemporaries and by all coming generations of mathematicians and mechanicians at their face value. Not only Hamilton has praised the *Méchanique Analitique* as "a kind of scientific poem", but even today there are authors of mechanical writings who maintain that "the whole of analytical dynamics is based upon, and is developed from, the theorem of Lagrange that I call the fundamental equation" [26, p. VII]. The only critical notes apropos of Lagrange's mechanical achievements as a whole we have come across in all our professional days may be found in Truesdell's *Essays* [27] where one may read:

"At the end of the [eighteenth] century there was a dismaying tendency to turn away from the basic problems, both in mechanics and in pure analysis. Directly contrary to the great tradition set by James Bernoulli and Euler, this formalism grew rapidly in the French school and is reflected in the *Méchanique Analitique*. Much of the misjudgement that historians and physicists have passed upon the work of the eighteenth century comes from unwillingness to look behind and around the *Méchanique Analitique* to the great work of Euler and the Bernoullis which is left unmentioned. As its title implies, the *Méchanique Analitique* is not a treatise on rational mechanics, but rather a monograph on one method of deriving differential equations of motion, mainly in the special branch now called, after it, analytical mechanics" (p. 134).

And later, as a general estimation of Lagrange's mechanical performances:

"1. There was little new in the *Méchanique Analitique*; its contents derive from earlier papers of Lagrange himself 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.

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

Granted its more modest scope, estimates of Lagrange's performance must remain a matter of taste. In music, in painting, in literature, tastes have changed in the past century. Why should they not also change in mechanics? The historians delight in repeating Hamilton's praise of the *Méchanique Analitique* as "a kind of scientific poem", but it is unlikely that many persons today will find Hamilton's recommendations in non-scientific poetry congenial" (p. 134 – 135, 246 – 248).

We give expression to our greatest regret that we cannot agree with this standpoint of the author of the excellent *Essays in the History of Mechanics*, but the acceptance of Lagrange's main dynamical pretension — his famous dynamical equations of motion — is by no means a matter of taste, even if a bad one: for analytical mechanics the problem *Lagrange or Euler?* is not a question of an alternative. It turns out that the lawsuit *Lagrange versus Euler* is a struggle for existence. The opposition of Lagrange against the Eulerian dynamical tradition has begun five years after Euler's death and nowadays the battle-field lies entirely in the hands of the Lagrangeanists. The result is a total obscurantism, both ideological and technical, in a vast domain of human knowledge, and no glimmer of hope is at hand for the time being.

But let us not anticipate events. Before answering the question what does Lagrangean dynamical tradition mean and which are its most characteristic features, let us first pose the problem: are Lagrange's statements in his *Avertissement* true?

Is it, in other words, true that Lagrange succeeded in his *Méchanique Analitique* "de réduire la théorie de cette Science, et l'art de résoudre les problèmes qui s'y rapportent, à des formules générales, dont le simple développement donne toutes les équations nécéssaires pour la résolution de chaque problème"?

Is it true that the *modus operandi* Lagrange applied in order "de remplir cet objet ne laissera rien à désirer"?

Is it true that "cet Ouvrage ... réunira et présentera sous un même point de vue les différents principes trouvés jusqu'ici pour faciliter des questions de Méchanique, en montrera la liaison et la dépendance mutuelle, et mettra à portée de juger de leur justesse et de leur étendue"?

Is it true that the statical and dynamical parts of the *Méchanique Analitique* treat "des corps solides"?

Is it true that analytical dynamics may be exposed by means of "méthodes ... ne démandent ni constructions, ni raisonnements géométriques ou méchaniques, mais seulement des opérations algébriques, assujetties a une marche régulière et uniforme"?

Is all this true?

Not a word, with the only exception that "on ne trouvera point de Figures dans cet Ouvrage". There are none, indeed.

All these claims are false in the utmost degree, and one of the main aims of the present paper is to prove this in an unambiguous way.

To begin with, let us first analyse Lagrange's pretensions that the *Méchanique Analitique* presents "les différents principes trouvés jusqu'ici pour faciliter des questions de la Méchanique".

Why ask the Bishop when the Pope is around? In *Section première: Sur les différents principes de la dynamique* of *Second partie: La dynamique* of his *Méchanique Analitique* Lagrange writes:

"... auparavant, il ne sera inutile d'exposer les principes qui servent de fondement à la Dynamique, et de presenter la suite et la gradation des idées qui ont le plus contribué à étendre et à perfectionner cette science ... les principes ou théorèmes connus sous les nomes de *conservation des forces vives*, de *conservation du mouvement du centre de gravitè*, de *conservation des moments de rotation* ou *Principe des aires*, et de *Principe de la moindre quantuté de l'action*."

Apropos of the principle of *conservation des forces vives* Lagrange states:

"Le premier de ces quatre principes, celui de la conservation des forces vives, a été trouvé par Huygens, mais sous une forme un peu différente de celle qu'on lui donne présentement... Ainsi le principe de Huygens se réduit à ce que, dans le mouvement des corps pesants, la somme des produits des masses par les carrés des vitesses à chaque instant est la même, soit que les corps se meuvent conjointement d'une manière quelconque, ou qu'ils parcourent librement des mêmes hauteurs verticales ... Jean Bernoulli ... donna ainsi a ce principe le nom de *conservation des forces vives*, et il s'en servit avec succès pour résoudre quelques problèmes qui n'avaient pas encore été résolus et dont il paraissait dificile de venir à bout par des méthodes directs. Daniel Bernoulli a donné ensuit plus d'extension à ce principe et il en a déduit les lois du mouvement des fluides ... Enfin il l'a rendu très général ... en faissant voir comment on peut l'appliquer au mouvement des corps animés par des attractions mutuelles quelconques ou attirés vers des centres fixes par des forces proportionnelles à quelques fonctions des distances que ce soit."

In order to penetrate this text of Lagrange's, some detailed explications are unavoidable. Let us suppose that a rigid body B is acted on by the *active forces*

$$\vec{F}_\mu = (\boldsymbol{F}_\mu,\ \boldsymbol{M}_\mu) \qquad (\mu = 1, \ldots, m), \tag{1}$$

all moments $\boldsymbol{M}_\mu$ $(\mu = 1, \ldots, m)$ being taken with respect to the origin O of an orthonormal right-hand orientated Cartesian system of reference $Oxyz$. Besides, let B be subjected to certain geometrical constraints generating *passive forces* or *reactions of the constraints*

$$\vec{R}_\nu = (\boldsymbol{R}_\nu,\ \boldsymbol{N}_\nu) \qquad (\nu = 1, \ldots, n), \tag{2}$$

the moments N_ν $(\nu = 1, \ldots, n)$ being taken again with respect to O. For the sake of brevity let by definition

$$\vec{F}_{m+\nu} = \vec{R}_\nu \qquad (\nu = 1, \ldots, n), \tag{3}$$

i. e.

$$F_{m+\nu} = R_\nu, \; M_{m+\nu} = N_\nu \qquad (\nu = 1, \ldots, n) \tag{4}$$

provided

$$\vec{F}_{m+\nu} = (F_{m+\nu}, \; M_{m+\nu}) \qquad (\nu = 1, \ldots, n). \tag{5}$$

Moreover, let P_ν $(\nu = 1, \ldots, m+n)$ be points of B, coinciding at the moment of time t with the directrices of $\vec{F}_\nu$ $(\nu = 1, \ldots, m+n)$ respectively. In other words, if

$$r_\nu = OP_\nu \qquad (\nu = 1, \ldots, m+n), \tag{6}$$

then the following two conditions are, by hypothesis, satisfied. First,

$$\frac{d}{dt}(r_\nu - r_\Omega) = \varpi \times (r_\nu - r_\Omega) \qquad (\nu = 1, \ldots, m+n), \tag{7}$$

where

$$r_\Omega = \overline{O\Omega}, \tag{8}$$

Ω denoting the origin of an orthonormal right-hand orientated Cartesian system of reference $\Omega\xi\eta\zeta$ invariably connected with B, and

$$\varpi = \frac{1}{2}(\bar{\xi}^\circ \times \dot{\bar{\xi}}^\circ + \bar{\eta}^\circ \times \dot{\bar{\eta}}^\circ + \bar{\zeta}^\circ \times \dot{\bar{\zeta}}^\circ) \tag{9}$$

being the instantaneous angular velocity of $\Omega\xi\eta\zeta$ with respect to $Oxyz$; $\bar{\xi}^\circ$, $\bar{\eta}^\circ$, $\bar{\zeta}^\circ$ in (9) denote the unit vectors of the axes $\Omega\xi$, $\Omega\eta$, $\Omega\zeta$ respectively with respect to $Oxyz$. Second,

$$r_\nu \times F_\nu = M_\nu \qquad (\nu = 1, \ldots, m+n). \tag{10}$$

Further, let

$$T = \frac{1}{2}\int v^2 dm \tag{11}$$

denote the *kinetic energy* of B with respect to $Oxyz$. In other words, if P denotes any point of B and

$$r = OP, \tag{12}$$

$$(13) \qquad v = \frac{dr}{dt},$$

the derivative in (13) being taken with respect to $Oxyz$, then the integral in the right-hand side of (11) is expanded over the space occupied by B, dm denoting elementary mass of B.

A classical theorem of analytical mechanics reads

$$(14) \qquad dT = \sum_{\nu=1}^{m+n} F_\nu dr_\nu.$$

The equation (14) is usually called the *kinetic energy theorem*. Let us now see how is this theorem proved?

Let by definition

$$(15) \qquad F = \sum_{\nu=1}^{m+n} F_\nu, \quad M = \sum_{\nu=1}^{m+n} M_\nu$$

be the *basis* and the *moment* (with respect to O) of all forces, both active and passive, acting on B. Then (10) and the second relation (15) imply

$$(16) \qquad M = \sum_{\nu=1}^{m+n} r_\nu \times F_\nu.$$

Besides, let

$$(17) \qquad v_\Omega = \frac{dr_\Omega}{dt},$$

the derivative being taken with respect to $Oxyz$. Now the relations (7), (15) – (17) imply

$$(18) \qquad \sum_{\nu=1}^{m+n} F_\nu \frac{dr_\nu}{dt} = \sum_{\nu=1}^{m+n} F_\nu (v_\Omega + \bar{\omega} \times (r_\nu - r_\Omega))$$

$$= (v_\Omega + r_\Omega \times \bar{\omega}) \sum_{\nu=1}^{m+n} F_\nu + \bar{\omega} \cdot \sum_{\nu=1}^{m+n} r_\nu \times F_\nu,$$

i. e.

$$(19) \qquad \sum_{\nu=1}^{m+n} F_\nu \frac{dr_\nu}{dt} = (v_\Omega + r_\Omega \times \bar{\omega}) F + \bar{\omega} \cdot M.$$

On the other hand, P being a point of B, the relations (12) and (13) imply

$$(20) \qquad v = v_\Omega + \bar{\omega} \times (r - r_\Omega).$$

Now (20) and the definition (11) imply

$$\frac{dT}{dt} = \int v w dm = \int (v_\Omega + \overline{\omega} \times (r - r_\Omega)) w dm \tag{21}$$

$$= (v_\Omega + r_\Omega \times \overline{\omega}) \int w dm + \overline{\omega} \cdot \int r \times w dm,$$

provided

$$w = \frac{dv}{dt}. \tag{22}$$

Let by definition

$$K = \int v dm, \qquad L = \int r \times v dm \tag{23}$$

denote the *momentum* and the *moment of momentum* (*kinetical moment*) of B with respect to $Oxyz$. Then *Euler's dynamical axioms* or the *laws* (*principles*) *of momentum* and *of moment of momentum* (*of kinetical moment*) governing the motion of any rigid body read

$$\frac{dK}{dt} - F = O, \qquad \frac{dL}{dt} - M = O. \tag{24}$$

The relations (22), (23) imply that the laws (24) may be written in the form

$$\int w dm = F, \quad \int r \times w dm = M. \tag{25}$$

By virtue of (25), the relation (21) may be written in the form

$$\frac{dT}{dt} = (v_\Omega + r_\Omega \times \overline{\omega}) F + \overline{\omega} \cdot M. \tag{26}$$

The right-hand sides of (19) and (26) obviously coincide. Hence (14), *q. e. d.*

In such a manner it is clear that the kinetic energy theorem (14) is unprovable unless Euler's dynamical laws (24) come to its help. Lagrange, however, does not mention these principles among his "principes qui servent de fondament à la Dynamique". This is, by the way, a topic we shall later come back to. Let us now see how the kinetic energy theorem (14) may be used in solving dynamical problems.

Strange though it may seem, in the general case the plain answer is: *not the least bit.*

In order to explain this somewhat disappointing conclusion, let us write down (14) in the equivalent form

$$dT = \sum_{\mu=1}^{m} F_\mu dr_\mu + \sum_{\nu=1}^{n} R_\nu dr_{m+\nu} \tag{27}$$

by virtue of (4). Now (27) clearly displays that the right-hand side of the kinetic energy theorem consists of two addends. The first one, namely

$$\sum_{\mu=1}^{m} \boldsymbol{F}_\mu d\boldsymbol{r}_\mu \tag{28}$$

is well known for any position of the rigid body B consistent with the geometrical constraints imposed on it. Indeed, the bases $\boldsymbol{F}_\mu$ $(\mu = 1, \ldots, m)$ of the active forces (1) are known since the forces (1) themselves are known: an active force is, by definition, a force which is wholly defined in the conditions of the dynamical problem. As regards $\boldsymbol{r}_\mu$ $(\mu = 1, \ldots, m)$, the only conditions imposed on these quantities are (7) and (10). In such a way the quantity (28) may be regarded as given.

The case with the second addend

$$\sum_{\nu=1}^{n} \boldsymbol{R}_\nu d\boldsymbol{r}_{m+\nu} \tag{29}$$

is a quite different one. Indeed, nothing is known about the reactions (2) of the constraints save that their directrices pass through the corresponding points of contact of the rigid body with the geometrical constraints.

In such a way, the rigid-hand side of the relation (14) is unknown, in the general case at least. Consequently, the kinetic energy theorem (14) implies no dynamical integral, unless additional hypotheses are made in the condition of the dynamical problem in question.

As a rule, these additional hypotheses reduce to the condition

$$\sum_{\nu=1}^{n} \boldsymbol{R}_\nu d\boldsymbol{r}_{m+\nu} = 0. \tag{30}$$

Obviously, (30) is satisfied trivially if the rigid body is *free*. Another case when (30) holds good is especially favoured by the Lagrangeanists. It is the case of the so-called *ideal constraints.*

Geometrical constraints are called *ideal* if the work the reactions of these constraints accomplish is equal to zero. The ideal geometrical constraints are called also *frictionless.* Since the work in question is, by definition, represented by the quantity (29), the geometrical constraints under consideration are ideal if, and only if, the condition (30) holds good. In such a case the kinetic energy theorem (14) becomes

$$dT = \sum_{\mu=1}^{m} \boldsymbol{F}_\mu d\boldsymbol{r}_\mu. \tag{31}$$

The condition (30) is called the *postulate of ideal constraints.*

This form of the kinetic energy theorem is much more hopeful than (14), since the right-hand side of (31) is, as already emphasized, wholly determined. Alas, these hopes are premature ones: in the general case (31) given also no dynamical integral.

Indeed, in the general case the active forces (1) are functions of the position of the rigid body B, of its velocities, and of the time t, rather than of its position only, so that the differential relation (31) cannot be integrated. Let us discuss this question somewhat closer.

Let the rigid body B possess l degrees of freedom; in other words, let any of its positions in space consistent with the geometrical constraints imposed on B be determined by the aid of exactly l mutually independent quantities

$$q_\lambda \qquad (\lambda = 1, \ldots, l) \tag{32}$$

(*independent parameters*, or only *parameters*, of B). The very definition (32) implies that, if nothing else is required in the condition of the dynamical problem under consideration, then not only the parameters of B, but also their derivatives

$$\dot{q}_\lambda \qquad (\lambda = 1, \ldots, l) \tag{33}$$

with respect to the time t (*generalized velocities*, or only *velocities*, of B) are mutually independent. Moreover, the *canonic parameters* of B, namely the Cartesian coordinates x_Ω, y_Ω, z_Ω of Ω with respect to $Oxyz$ and the Eulerian angles ψ, φ, θ between $Oxyz$ and $\Omega\xi\eta\zeta$, are functions of (32) and of t:

$$x_\Omega = x_\Omega(q_1, \ldots, q_l, t), \quad y_\Omega = y_\Omega(q_1, \ldots, q_l, t), \quad z_\Omega = z_\Omega(q_1, \ldots, q_l, t), \tag{34}$$

$$\psi = \psi(q_1, \ldots, q_l, t), \quad \varphi = \varphi(q_1, \ldots, q_l, t), \quad \theta = \theta(q_1, \ldots, q_l, t). \tag{35}$$

Now in the general case the active forces (1) are wholly determined functions

$$\vec{F}_\mu = \vec{F}_\mu(q_1, \ldots, q_l; \dot{q}_1, \ldots, \dot{q}_l, t) \qquad (\mu = 1, \ldots, m) \tag{36}$$

of (32), (33), and possibly of t. This implies that the bases $\boldsymbol{F}_\mu$ of $\vec{F}_\mu$ $(\mu = 1, \ldots, m)$ are completely determined (i.e. given) functions

$$\boldsymbol{F}_\mu = \boldsymbol{F}_\mu(q_1, \ldots, q_l; \dot{q}_1, \ldots, \dot{q}_l, t) \qquad (\mu = 1, \ldots, m) \tag{37}$$

of q_λ, $\dot{q}_\lambda$ $(\lambda = 1, \ldots, l)$, and of t. On the other hand, the very definition of the quantities (6) implies that they are wholly determined functions

$$\boldsymbol{r}_\nu = \boldsymbol{r}_\nu(q_1, \ldots, q_l; t) \qquad (\nu = 1, \ldots, m+n) \tag{38}$$

of the parameters q_λ $(\lambda = 1, \ldots, l)$ of B and possibly of the time t (in the case when the geometrical constraints imposed on B are *rheonomic*, rather than *scleronomic*). Now (38) imply

$$d\boldsymbol{r}_\nu = \sum_{\lambda=1}^{l+1} \frac{\partial \boldsymbol{r}_\nu}{\partial q_\lambda} dq_\lambda \qquad (\nu = 1, \ldots, m), \tag{39}$$

provided, for the sake of brevity, by definition

$$q_{l+1} = t. \tag{40}$$

Then (39), (31) imply

$$dT = \sum_{\lambda=1}^{l+1} Q_\lambda dq_\lambda, \tag{41}$$

where by definition

$$Q_\lambda = \sum_{\mu=1}^{m} \boldsymbol{F}_\mu \frac{\partial \boldsymbol{r}_\mu}{\partial q_\lambda} \qquad (\lambda = 1, \ldots, l+1). \tag{42}$$

The quantities (42) are called the *generalized active forces* acting on B. It is immediately seen that, in the general case, the generalized active forces are wholly determined functions

$$Q_\lambda = Q_\lambda(q_1, \ldots, q_l; \dot{q}_1, \ldots, \dot{q}_l; t) \qquad (\lambda = 1, \ldots, l) \tag{43}$$

of the parameters (32) of the rigid body B, of its generalized velocities (33), and possibly of the time t. For that reason, in the general case, the kinetic energy theorem (31) does not, even when the rigid body is free or the postulate of ideals constraints (30) is satisfied (the rigid body being not free), lead to a dynamical integral.

A necessary condition for the integrability of (41) is

$$\frac{\partial Q_\lambda}{\partial \dot{q}_\mu} = 0 \qquad (\lambda = 1, \ldots, l+1; \ \mu = 1, \ldots, l). \tag{44}$$

In other words, Q_λ $(\lambda = 1, \ldots, l+1)$ must be independent of the generalized velocities of the rigid body. In such a case the generalized active forces (42) become wholly determined functions

$$Q_\lambda = Q_\lambda(q_1, \ldots, q_l; t) \qquad (\lambda = 1, \ldots, l+1) \tag{45}$$

of the parameters of B and of t only, and (41) becomes

$$dT = \sum_{\lambda=1}^{l+1} Q_\lambda(q_1, \ldots, q_{l+1}) dq_\lambda. \tag{46}$$

We were sent from pillar to post, our woes have, however, not yet finished. The Pfaffian form (46) leads to no conclusion. We are compelled to hypothecate still, pledging the generality of the dynamical problem. Namely, we are coerced to hypothesize that the right-hand side of (46) springs from a potential function

$$U = U(q_1, \ldots, q_{l+1}) \tag{47}$$

of the parameters of B and possibly of t by virtue of (40), in other words, that

$$Q_\lambda = \frac{\partial U}{\partial q_\lambda} \qquad (\lambda = 1, \dots, l+1) \tag{48}$$

or, just the same, that the right-hand side of (46) is complete differential

$$dU = \sum_{\lambda=1}^{l+1} Q_\lambda dq_\lambda. \tag{49}$$

In such a case, and in such a case only, the kinetic energy theorem (46) leads to a dynamical integral (the *kinetic energy integral*)

$$T = U + h, \tag{50}$$

h being an arbitrary constant with respect to t. The quantity (47) is called a *function of forces*, and the quantity

$$V = -U \tag{51}$$

is called a *potential*. Now (50) and (51) imply

$$T + V = h, \tag{52}$$

in other words, the quantity $T + V$ remains constant. It is called the *full mechanical energy* of B, and (52) implies that it does not change during the motion of B. A system of rigid bodies is called *conservative* when its full mechanical energy preserves a constant value in the course of its motion.

Mais revenons à nos moutons. The reason for all these considerations was Lagrange's *principe de conservation des force vives.* The term *force vive* means $2T$, and the expression *conservation des forces vives* is incorrect, since not the *force vive*, but the *full mechanical energy* is conserved in some mechanical processes. Anyway, let us make a brief comment on the situation we are confronted with.

Lagrange claims in the *Mèchanique Analitique* "d'exposer les principes qui servent de fondement à la Dynamique" and adduces, in the capacity of principle No 1 "le principe de conservation des forces vives". In other words, he claims that "le principe de conservation des forces vives" is sufficient for "de fondement à la Dynamique". Maybe it is, but "la Dynamique" which? Certainly not the dynamics as we understand it today. Surely, "la Dynamique" as it existed in Lagrange's head. But the dynamics of conservative mechanical systems is certainly too miserable for a scientific ideal.

Let us remember the restrictions we were compelled to impose on a dynamical problem in order that the kinetic energy theorem could provide a dynamical integral (50).

First, the postulate of ideal constraints (30).

Second, generalized active forces independent of the generalized velocities (44).

Third, existence of a potential function (48).

Even if we suppose that the analytical dynamics is such a freak of nature that its whole essence is run dry, being restricted to the conservative dynamical systems only — even in this case, we say, "le principe de conservation des forces vives" is insufficient in the capacity of a "fondement à la Dynamique". The dynamical integral (50) provides a single equation among the parameters (32) of the rigid body, its generalized velocities (33), and the time t. At the same time, the full solution of the dynamical problem requires $2l$ equations involving $2l$ arbitrary constants of integration: this solution consists in discovering (32) as completely determined functions

$$q_\lambda = q_\lambda(t;\ q_{10}, \ldots, q_{l0};\ \dot{q}_{10}, \ldots, \dot{q}_{l0}) \tag{53}$$

($\lambda = 1, \ldots, l$), where the initial values

$$q_{\lambda 0} = q_\lambda(0) \qquad (\lambda = 1, \ldots, l) \tag{54}$$

of the parameters (32) of B and the initial values

$$\dot{q}_{\lambda 0} = \dot{q}_\lambda(0) \qquad (\lambda = 1, \ldots, l) \tag{55}$$

of its generalized velocities (33) play the role of $2l$ arbitrary constants.

The range of action of "le principe de conservation des forces vives" is extremely narrow.

As we have already mentioned, the question that the kinetic energy theorem (14), and hence the kinetic energy integral (50), is undeducable save by the aid of Euler's dynamical axioms (24) is, for the time being, laid aside.

Apropos of the principle of *conservation du mouvement du centre de gravité* Lagrange writes:

"Le second principe est dû à Newton, qui, au commencement de ses *Principes mathématiques*, démontre que l'état de repos ou de mouvement du centre de gravité de plusieurs corps n'est point altéré par l'action réciproque de ces corps, quelle qu'elle soit; de sorte que le centre de gravité des corps qui agissent les unes sur les autres d'une manière quelconque, soit par des fils ou des leviers, ou des lois d'attraction, etc.; sans qu'il y ait aucune action ni aucun obstacle extérieur, est toujours en repos ou se meut uniformément en ligne droit. D'Alembert a donne depuis à ce principe une plus grande étendue, en faisant voir que, si chaque corps est solicité par une force accélératrice constante et qui agisse suivant des lignes paralléles, ou qui soit dirigée vers un point fixe et agisse en raison de la distance, le centre de gravité doit décrire la même courbe que si les corps étaient libres; a quoi l'on peut ajouter que le mouvement de ce centre est, en général, le même que si toutes les forces des corps, quelles qu'elles soient, y étaient appliquées, chacune suivant sa propre direction."

The first remark that this text of Lagrange's provokes concerns Newton's name. According to Lagrange. "Newton . . . démontre que l'état de repos ou de mouvement du centre de gravité de plusieurs corps n'est altéré par l'action réciproque de ces corps, quelle qu'elle soit". As a matter of fact, one finds in Newton's *Principia* [28] nothing of the kind.

The statement that in *Principia* the foundations of rational mechanics have been laid pertains to the Newtonian mythology. The reality is quite different:

"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.* It is not the function of the historian to guess what Newton might have done or could have done, not is what Mach could do with Newton's principles relevant; the cold fact is, the equations are not in Newton's book. As we shall see, 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 systems Newton has studied and for other systems nowadays thought of as governed by the "Newtonian" equations. To summarize: 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" [27, p. 92 – 93].

Clearly and simply, no dynamical problem concerning a single rigid body is solved in Newton's *Principia*. The whole of rigid body dynamics is a creation of Euler's hands. Euler was the first to realize that "while Newton had used the word "body" vaguely and in at least three different meanings ... the statements of Newton are generally correct only when applied to masses concentrated at isolated points; he [Euler] introduced the precise concept of *mass-point*, and his is the first treatise [29] devoted expressly and exclusively to it" [*ibid.*, p. 107].

In other words, citing Newton's name is connection with *le principe de conservation du mouvement du centre de gravité*, Lagrange allows a historical mistake to slip in. As regards the name of D'Alembert mentioned in the same connection, it is, inasmuch as rigid body dynamics is concerned, controversial to the highest degree. The criticism the following fragment from Truesdell's *Essays* contains is, by our opinion, not as categorical as it should be:

"At the age of twenty-four, D'Alembert published a book with a title indicative of what mechanics was in the Age of Reason: "Treatise on Dynamics, in which the laws of equilibrium and motion of bodies are reduced to the smallest possible number and are proved in a new way, and where is given a general principle for finding the motion of several bodies which react mutually in any way." This is the book to which we have several times referred in connection with the program of the age. In regard to practice, it is less successful. Contrary to the usual claims, neither did D'Alembert reduce dynamics to statics, nor did he, here or anywhere, propose either of the two forms of the laws of dynamics now usually called "D'Alembert's principle", these being due to Euler and Lagrange, respectively, at a later period" [*ibid.*, p. 112 – 113].

By our opinion, D'Alembert's *Traité* [30] does not contain the complete solution of a single dynamical problem concerning rigid bodies. For that reason we find it inadequate that Lagrange refers to D'Alembert in connection with *le principe de conservation du mouvement du centre de gravité*, as far as namely rigid bodies are concerned. Anyway, let us see somewhat closer what does, as a matter of fact, this principle state.

According to Lagrange's formulation, already cited, "l'état de repos ou de mouvement cu centre de gravité de plusieurs corps n'est point altéré par l'action réciproque de ces corps, quelle qu'elle soit". Three remarks are relevant in this connection.

First, Lagrange's *principe de conservation du mouvement du centre de gravité* is a non-constructive statement, while a mathematical axiom, or principle, or law, must contain in itself something constructive. (If a mathematical axiom, like Euclid's fifth postulate, for instance, states the impossibility of a mathematical phenomenon, its goal is to ensure the soleness of a mathematical object, the existence of which is already independently established.)

Second, Lagrange's second principle does not imply the laws of motion of a single rigid body, being either meaningless or trivial in the case of one only rigid body.

Third, the principle of conservation of the motion of the center of gravity of a system of several bodies is a *trivial corollary* from Euler's first dynamical axiom. Indeed, the definitions

(56) $$m = \int dm \quad ,$$

and

(57) $$\boldsymbol{r}_G = \frac{1}{m}\int \boldsymbol{r}\,dm$$

of the mass and the radius-vector

(58) $$\boldsymbol{r}_G = \boldsymbol{OG}$$

of the mass-center G respectively of any rigid body B imply

(59) $$m\boldsymbol{w}_G = \int \boldsymbol{w}\,dm$$

provided

(60) $$\boldsymbol{w}_G = \frac{d^2\boldsymbol{r}_G}{dt^2},$$

since by hypothesis

(61) $$\frac{d}{dt}(dm) = 0, \quad \frac{d}{dt}(m) = 0.$$

Now (59) and the first relations (23), (24) imply

(62) $$m\boldsymbol{w}_G = \boldsymbol{F}.$$

The equation (62) which is an immediate corollary from Euler's law of momentum, expresses an important fact, classical for rigid body analytical dynamics, namely that the mass-center G of any rigid body B is moving in the same manner, as would a mass-point, located at G, the mass of which is equal to that of B and which is acted on by the same forces that act on B.

Let now a system S of n rigid bodies B_ν ($\nu = 1, 2, \ldots, n > 1$) be given and let m_ν and G_ν be the mass and the mass-center of B_ν ($\nu = 1, \ldots, n$) respectively. Let

$$\boldsymbol{r}_\nu = OG_\nu \qquad (\nu = 1, \ldots, n). \tag{63}$$

Then the mass-center G of S is defined by

$$\boldsymbol{r}_G = \frac{m_1\boldsymbol{r}_1 + \cdots + m_n\boldsymbol{r}_n}{m_1 + \cdots + m_n}. \tag{64}$$

On the other hand, let $\boldsymbol{F}_\nu$ be the basis of the system of forces, acting on B_ν ($\nu = 1, \ldots, n$) respectively and let

$$\boldsymbol{w}_\nu = \frac{d^2\boldsymbol{r}_\nu}{dt^2} \qquad (\nu = 1, \ldots, n). \tag{65}$$

Then Euler's theorem (62) implies

$$m_\nu \boldsymbol{w}_\nu = \boldsymbol{F}_\nu \qquad (\nu = 1, \ldots, n) \tag{66}$$

and (66), (64) imply that G is moving according

$$\boldsymbol{w}_G = \frac{\boldsymbol{F}_1 + \cdots + \boldsymbol{F}_n}{m_1 + \cdots + m_n}. \tag{67}$$

Let us now suppose that the bodies of the system S are subjected to additional interactions. More precisely, let the rigid body B_ν ($\nu = 1, \ldots, n$) be acted on by the rigid body B_μ ($\mu = 1, \ldots, n$; $\mu \neq \nu$) by a system of forces with basis $\boldsymbol{F}_{\mu\nu}$. Then, according to Newton's law of action and reaction,

$$\boldsymbol{F}_{\mu\nu} + \boldsymbol{F}_{\nu\mu} = 0. \tag{68}$$

The rigid body B_ν ($\nu = 1, \ldots, n$) is moving according to the law

$$m_\nu \boldsymbol{w}_\nu = \boldsymbol{F}_\nu + \sum_{\mu=1}^{n} \boldsymbol{F}_{\mu\nu} \qquad (\nu = 1, \ldots, n), \tag{69}$$

provided

$$\boldsymbol{F}_{\nu\nu} = o \qquad (\nu = 1, \ldots, n). \tag{70}$$

Adding the equations (69) with (70) together and taking into consideration (68), one obtains

$$\sum_{\nu=1}^{n} m_\nu w_\nu = \sum_{\nu=1}^{n} F_\nu. \qquad (71)$$

The last relation, together with (64), implies again (67), therewith proving Lagrange's *principe de conservation du mouvement du centre de gravité.* Let us note that it is undeducable save by the aid of Euler's first dynamical law which does not occur among Lagrange's dynamical principles.

We do not know how this *principe de la Dynamique* has been used in rigid body dynamics of Lagrange's predecessors and contemporaries for solving special problems. For us, however, it is wholly useless in the capacity of a mathematical tool for such aims. By our opinion, today it represents nothing more than a curious mathematical fact. In any case, the claims that it could serve *de fondement à la Dynamique*, as Lagrange unambiguously states, are frankly ridiculous.

Apropos of the principle of *conservation des moments de rotation* or *principe des aires* Lagrange writes:

"Le troisième principe est beaucoup moins ancien que les deux précédents, et parait avoir été découvert en même temps par Euler, Daniel Bernoulli et d'Arcy, mais sous des formes différentes. Selon les deux premiers, ce principe consiste en ce que, dans le mouvement de plusieurs corps autour d'un centre fixe, la somme des produits de la masse de chaque corps par sa vitesse de circulation autour du centre et par sa distance au même centre est toujours independente de l'action mutielle que les corps peuvent exerces les uns sur les autres, et se conserve la même tant qu'il n'y a aucune action ni aucun obstacle extérieur. Daniel Bernoulli a donné ce principe dans le premier Volume des *Mémoires de l'Académie de Berlin*, qui a paru en 1746, et Euler l'a donné la même année dans le tome I[er] de ses *Opuscules;* et c'est aussi le même problème qui les y a conduit.... Le principe de d'Arcy, tel q'il l'a donné à l'Académie des Sciences, dans les *Mémoires* de 1747, qui n'ont paru qu-en 1752, est que la somme des produits de la masse de chaque corps par l'aire que son rayon vecteur décrit autour d'un centre fixe sur un meme plan de projection est toujours proportionelle au temps. On voit que ce principe est une généralisation du beau théorème de Newton sur les aires décrites en vertu de forces centripètes quelconques; et pour an apercevoir l'analogie ou plutôt l'identité avec celui d'Euler et de Daniel Bernoulli, il n'y a qu'à considérer que la vitesse de circulation est exprimée par l'éléments, multiplié par la distance au centre, donne l'élément de l'aire décrite autour de ce centre; d'où l'on voit que ce dernier principe n'est aurte chose que l'expression différentielle de celui de d'Arcy. Cet auteur a présenté ensuit son principe sous une autre forme qui le rapproche davantage du précédent, et qui consiste en ce que la somme des produits des masses par les vitesses et par les perpendiculaires tirées du centre sur les directions du corps est une quantité constante. Sous ce point de vue, il en a fait même une espèce de principe métaphysique qu'il appelle la *conservation de l'action*, pour l'opposer ou plutot pour le substiteur à celui de *la moindre quantité d'action*; comme si des dénominations vagues et arbitraires faisaient l'essence des loi de la nature et pouvaient, par quelque vertu secrète, ériger en causes finales de simples résultats des lois connues de la Mécanique. Quoiqu'il en soit, le principe don't il s'agit a lieu généralement pour tous les systèmes de corps qui agissent les uns sur les autres d'une façon quelconque, soit par des fils, des lignes inflexibles, des lois d'attraction, etc., et qui sont de plus sollicités par des forces quelconques dirigées à un centre fixe, soit que le système soit d'ailleurs entièrement libre, ou qu'il soit assujetti à ce mouvoir autour de ce même centre. La somme des produits des masses par les aires décrites autour de ce centre et projetées sur un plan quelconque est toujours proportionelle au temps."

The formulations of Lagrange's in connection with the *principe de conservation des moments de rotation* or *principe des aires* are considerably more indistinct than those concerning the first two principles: his explanations are obscure to such a degree that quite a lot of historical generosity is required for their acceptance.

In accordance with Lagrange's own words, the principle of conservation of the moments of rotation consist in the fact that, if several bodies are moving around a fixed center, the sum of the products of the masses of the bodies with their velocities of rotation around the said center and with their distances from the latter is always independent from the mutual interactions of the bodies, whatever they might be. It is, however, completely meaningless to speak about velocity of rotation of a body around a center and about a distance of a body from a center, unless the body in question is infinitesimal, i.e. a mass-point. In such a manner, a century after Newton and five years after Euler's death Lagrange is using a mechanical language, cast aside by Euler fifty years ago [29]. It is clear that, in Lagrange's formulation at least, the *principe des aires* belongs to mass-point dynamics and has, therefore, nothing to do with rigid body dynamics, no matter that Lagrange is speaking about *corps* and not about patricles.

The use of the term *centre* in Lagrange's formulation is a quite formal one: the role of this *centre* could play any other fixed point in space. This term is used only in the capacity of a system of reference and not as a source of forces (central forces) acting on the mass-points.

One should not disregard the fact that all three principles already cited in Lagrange's formulation concern systems of (finite or infinitesimal) bodies and not a single rigid body. This fact is by no means accidental: it is the result of a mechanical politics with far reaching sequels.

Indeed, as it has been emphasized in the first part [1] of this series, a characteristic feature of the Lagrangean dynamical tradition consists in the fact that anything done by the Lagrangeanists is accomplished for ***systems of a finite number of mass-points*** only, being afterwards quite unpardonably applied to rigid bodies in a completely illegal way (see the analysis on p. 18 – 20 of [1]). This is the quintessence of Lagrange's dynamical politics, his mechanical ***Weltanschauung***, his scientific credo.

A most remarkable scientific incident, if that is the word, will throw additional light on Lagrange's ideological positions in mechanics. In his article [31] he has written:

"Je considére les corps proposés comme l'assemblage d'une infinité de corpuscules ou points massifs unis ensemble de manière qu'il gardent toujours nécéssairement entre eux les mémes distances... J'aurai par les principes de mécanique, à cause que le système est supposé libre autour d'un point fixe, et qu'il n'est d'ailleurs sollicité par aucune force étrangère, j'aurai, dis-je, sur-le-champ ces trois équations..." (quoted by [32, p. 590]).

Thence Lagrange claims to have derived the theorem of moment of momentum. Euler, who refused to accept the picture of a rigid body as composed of infinitely many mass-points, wrote apropos of [31] in the introduction of his memoir [33] with ill-concealed if latent irony:

"But when I tried with greatest avidity to follow 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 analytical devices he used" (quoted by [27, p. 260], see also [34]).

A broad hint — blindness. This is written in 1775, and Euler has been stone-blind years ago. His blindness did not prevent him from printing many tens of articles every year. Moreover, namely this paper that "confession" is taken from contains the greatest mechanical discovery of Euler in all his scientific life — his dynamical axioms. In order to understand Euler's delicacy, one must bear in mind, that all his life through Euler has been extremely tolerant of his younger scientific colleague. The inverse theorem is not true.

Summing up, one should clearly state that Lagrange's *principe des aires* could by no means serve *de fondament à la Dynamique.*

Apropos the last *principe de la moindre quantite de l'action* Lagrange writes:

"Je viens enfin au quatrième principe, que j'appelle de la *moindre action*, par analogie avec celui que Maupertuis avait donné sous cette dénomination et que les écrits de plusieurs auteurs illustres ont rendu ensuit si fameux. Ce principe, envisagé analytiquement, consiste en ce que, dans le mouvement des corps qui agissent les unes sur les autres, la somme des produits des masses par les vitesses et par les espaces parcurus est un minimum. L'auteur en a deduit les lois de la réflexión et de la réfraction de la lumière, ainsi que celles du choc des corps, dans deux Mémoires lus, l'un à l'Académie des Sciences de Paris, en 1744, et l'autre, deux ans aprés, a celle de Berlin. Mais ces applications sont trop particulières pour servir à établir la vérité d'un principe général, elles ont d'ailleurs quelque chose de vague et d'arbitraire, qui ne peut que rendre incertaines les conséquences, qu'on en pourrait tirer pour l'exactitude même du principe. Aussi l'on aurait tort, ce me semble, de mettre ce principe, présenté ainsi, sur la même ligne que ceux que nous venons d'exposer. Mais il y a une autre manière de l'envisager, plus générale et plus rigoureuse, et qui mérite seule l'attention des géomètres. Euler en a donné la première idée à la fin de son Traité des isopérimètres, imprimé à Lausanne en 1744, en y faisant voir que, dans les trajectoites décrites par des forces centrales, l'intégrale de la vitesse multipliée par l'élément de la courbe fait toujours un maximum ou un minimum. Cette propriété, qu'Euler avait trouvée dans le mouvement des corps isolés, et qui paraissait bornée à ces corps, je l'ai étendue, par le moyen de la conservation des forces vives, au mouvement de tout système de corps qui agissent les unes sur les autres d'un manière quelconque; et il en est résulté ce nouveau principe général, que la somme des produits des masses par les intégrales des vitesses multipliées par des éléments des espaces parcourus est constammement un maximum ou un minimum. Tel est le principe auquel je donne ici, quoique improprement, le nom de *miondre action*, et que je regarde, non comme un principe métaphysique, mais comme un résultat simple et général des lois de la Mécanique. On peut voir dans le tome II des *Mémoires de Turin* l'usage que j'en ai fait pour résoudre plusieurs problèmes difficiles de Dynamique. Ce principe, combiné avec celui des forces vives et développé suivant les régles du calcul des variations, donne directement toutes les équations nécéssaires pour la solution de chaque problème."

(All fragments of *Méchanique Analitique* concerning the four dynamical principles of Lagrange are quoted after [35, t. I, p. 238, 257 – 262].)

This last principle of Lagrange's is obviously a variational one. As a such, its range of action is restricted to conservative dynamical systems only, and in this connection it falls under the criticism made apropos of the principe *de conservation des forces vives*, third point. Therefore, the last statement of Lagrange's, namely that this principle "donne directement toutes les équations nécéssaires pour la solution de chaque problème", belongs to the fairy-tales of the Lagragean tradition.

At that, let us underline that no variational principle of analytical dynamics is up to now proved save for mechanical systems consisting of a finite number of mass-points only. In order words, any application of such a principle to rigid bodies is mathematically illegal, at least till the contrary is proved.

In such a way it is established beyond doubt that Lagrange's claims in the *Méchanique Analitique*, namely that "cet Ouvrage ... réunira et presentera sous un même point de vue les différents principes trouves jusqu'ici pour faciliter des questions de Mécanique, en montrera la liaison et la dépendance mutuelle, et mettra à portée de juger de leur justesse et de leur étendue", is not true. Indeed, as already emphasized, the four principles discussed above purely and simply cannot serve "de fondement à la Dynamique". The fact that Lagrange included them in his treatise is not yet the biggest misfortune. Far worse is the circumstance that he overlooked the only dynamical principles on which the mathematical consolidation of the logical foundations of analytical mechanics is possible: the Eulerian laws of momentum and of kinetical moment.

Before we enter this topic, it is necessary to turn our attention to a phenomenon inherent to the early history of mechanics and focused on the existence of an immense number of mechanical principles. Come to that, what does in actuality a *dynamical principle* mean?

A categorical answer of this question is nowhere to be found in the mechanical literature. An attempt along these lines has been made at the beginning of this century by A. Voss in the *Encyclopedia of Mathematical Sciences* [36].

Voss begins his article *Die Prinzipien der rationellen Mechanik* with a brief periphrastic explanation of the various features of the notion:

"*Prinzipe und Prinzipien der Mechanik.* Der Ausdruck Prinzipe oder Prinzipien wird in der Mechanik in sehr verschiedener Weise angewendet. Unter *Prinzipien* versteht man in irgend einer Wissenschaft, hier speziell der *Mechanik*, *erstens* Aussagen, die nicht wieder auf andere demselben wissenschaftlichen Gebiete angehörige Behauptungen zurückgeführt, sondern als Ergebnisse anderer Resultate der Erkenntnis angesehen werden..., z.B. die *Axiome* oder *Postulate*..., und die teils logischer oder methodologischer, teils metaphysischer oder physikalischer, Art sein können; *zweitens, allgemeine aus den Grundvorstellungen der Mechanik gewonnene Sätze*, die, wenn auch in ihren einfacheren Fällen auf Grund früherer deduzierbar, doch in ihrem weitesten Umfange tatsächlich nicht mehr vollständig beweisbar erscheinen (z.B. das Prinzip der virtuellen Geschwindigkeiten, das *d'Alembert*'sche, resp. *Gauss*'sche Prinzip); *drittens*, ... allgemeine *rein mathematische Methoden für die Behandlung der Mechanischen Probleme*, die an sich zunächst völlig auf Grund von Prinzipien der zweiten Art erweisbar, zur rein deduktiven Behandlung ausgedehnter Teile der Mechanik ausreichend sind, in ihrer weitesten Ausdehnung allerdings wieder einen heuristischen Charakter erhalten (*Hamilton*'sches Prinzip, Prinzip der kleinsten Aktion); *endlich*, nach *C.G.J.Jacobi* ... *analytische Methoden*, Integralgleichungen der dynamischen Differentialgleichungen zu gewinnen... Die methodologische Stellung der Prinzipien zweiter und dritter Art zueinander wird sehr verschieden beurteilt. Auch wenn man beide, wie vielfach zu geschehen scheint, gleichsam als Beschwörungsformeln anseht, in denen ein lange fortgesetzter Prozess induktiver Erkenntnis seinen Ausduck gefunden hat, besteht doch ein sehr wesentlicher Unterschied in dem Grade der Abstraktion, der in beiden Fällen eintritt. — Die im Texte getroffene Unterscheidung von Prinzipien verschiedener Art kann überhaupt nur eine allgemeine sein; an welcher Stelle jede einzelne der mannigfaltigen als "Prinzipe" im Laufe der Zeit bezeichneten Aussagen einzuordnen ist, wird von den oft schwankenden Vorstellungen abhängen, die den Ausdruck Prinzip begleiten" [36, *erster Teilband*, S. 10 – 11].

Apropos of the fourth sort of mechanical principles Voss says:

"Wir rechnen dahin die analytische Verwendung des Satzes von der lebendigen Kraft, die Schwerpunktsintegrale, das Prinzip der Flächen, des letzten Multiplikators, *Hamilton*'sches Prinzip der varierenden Wirkung, das *Poisson-Jacobi*'sche Prinzip, die mannigfachen Transformations- und Äquivalenzprinzipe etc. etc." [*ibid.*, S. 10 – 11].

It is worth noting that Voss is fully aware of the contemporary unenviable state of the logical foundations of rational mechanics as well as of the hopelessness, in these strained circumstances, of any try at strict formulations:

"Die Erscheinung, dass die Resultate mathematischer Lehrgebäude von grundliegender Wichtigkeit oft eine lange Zeit hindurch ihrer strengen wissenschaftlichen Begründung vorausgeeilt sind, hat sich in weit höherem Grade bei der *Mechanik*, wie bei der Arithmetik oder Infinitesimalrechnung wiederholt. Man kann den Standpunkt, welchen die systematische Entwicklung der Mechanik in ihrer gegenwärtigen Gestalt einnimmt, etwa mit dem der Infinitesimalrechnung vor *Cauchy* vergleichen, auf den sich fast wörtlich die Bemerkungen von *Hertz* in seiner Einleitung zur Mechanik anwenden lassen ... über das bei der Exposition der Grundlagen der Mechanik häufig hervortretende Bestreben, über die Schwierigkeiten und Verlegenheiten in denselben möglichst bald hinaus und zu konkreten Beispielen zu kommen... Ein Blick auf den gegenwärtigen Zustand der Werke über Mechanik soweit sich dieselben nicht auf eine rein mathematische Behandlung, sondern auf die Entwicklung der eigentlich mechanischen Vorstellungen beziehen, dürfte indes zeigen, dass unter denselben, da wo es sich nicht um stereotype Wiederholung gewisser Wendungen handelt, die grössten Verschiedenheiten hinsichtlich des Prinzipien bestehen" [*ibid.*, S. 8 – 9].

In the second part *Die allgemeinen Prinzipien der rationellen Mechanik* of his article Voss discusses three classes of mechanical principles.

Apropos of the first class *philosophische Prinzipien* Voss writes:

"Von den philosophischen Prinzipien muss hier neben dem schon oben erwähnten *Kausalitätsprinzip* der *Satz vom zureichenden Grunde* hervorgehoben werden. Man schloss aus dem letzteren, dass der in Bewegung unabhängig von allen anderen Dingen vorgestellte materielle Punkt seine *Richtung* nicht ändern könne, während ein gleiches auch von der *Grösse* der Geschwindigkeit zu behaupten erst durch die *dialektische Unterscheidung* von Ursache und Wirkung möglich wird, welche die Ursache *ausserhalb* des Bewegten verlegt... Auch bei der Entwicklung des Kraftbegriffes, den Beweisen für das Parallelogramm der Kräfte, der Betrachtung der Fernwirkung zwischen zwei materiellen Punkten etc., spielt dieser Satz eine historische Rolle...

Dass man aus *bloss logischen* Prämissen keine Entscheidung über *reale* Verhältnisse treffen kann, wird gegenwärtig wohl nicht bezweifelt... Anders aber steht es, wenn der Satz vom zureichende Grunde in der Form eines logischen Schlusses auftritt, *dessen Prämissen vollständig als aus der Erfahrung bekannt vorausgesetzt werden*...

Von ganz wesentlichen Einflusse sind für die Entwicklung der Mechanik *teleologische* Prinzipien gewesen. Das Prinzip der kleinsten Wirkung ist geradezu von *Euler* ... aus einem solchen Gesichtspunkte abgeleitet; *Gauss*' Prinzip des kleinsten Zwanges, sowie gewisse Prinzipe der Elastizitätstheorie ... knüpfen ebenfalls an solche Vorstellungen an. — Die Frage, ob in der Natur wirklich Thatsachen vorliegen, welche den Gedanken beschäftigen, dass mit dem kleinsten Aufwande von Mitteln der grösste Effekt erreichbar werde, braucht man indessen hier nicht zu berühren. Bei Überlegungen dieser Art dürfte meistens ein sicheres *Maass* weder der aufgewandten Mitteln noch des erreichten Effektes zugrunde gelegt sein, sodass *die Behauptung einen klaren Sinn überhaupt nicht besitzt*. Was aber die Anwendung desselben in der *Mechanik* betrifft, so sind diese teleologischen Gesichtspunkte im *eigentlichen Sinne* schon um deswillen ganz unzutreffend ..., weil keineswegs — weder beim Prinzip der kleinsten Aktion, noch beim *Gauss*'schen Prinzip — die wirkliche Bewegung durch eine Minimumeigenschaft vor allen anderen ebenfalls *möglichen* ausgezeichnet ist, sondern nur gegenüber gewissen rein fingierten, im allgemeinen aber *unmöglichen* Bewegungen. Thatsächlich haben sich allerdings diese teleologischen Gesichtspunkte für den Aufbau der Wissenschaft als sehr förderlich erwiesen, und es erscheint in mehrfacher Beziehung von Interesse, die allgemeinen Gründe hierfür aufzusuchen...

Mach ... hat dagegen auf andere Prinzipien aufmerksam gemacht, welche aller

Naturauffassung zugrunde liegen sollen, die der *Ökonomie* und *Einfachheit.* Nach ihm ist es das Ziel aller Wissenschaft, das Gebiet der einzelnen Erfahrungen durch zusammenfassende Beschreibung derart zu ersetzen, dass durch den geringsten Aufwand an Gedankenarbeit dasseble übersehen werden kann. Das ist natürlich nur dadurch möglich, dass man die den einzelnen Erfahrungen zugrunde liegenden *Elemente* aufsucht und durch deren gesetzmässige Konstruktion eine Erklärung der Vorgänge liefert, für deren fortschreitende Ausbildung dann wieder rein formale Prinzipien, wie das der *Kontinuität* und *Stetigkeit*, — dem *Hankel*'schen... Prinzip der *Permanenz der formalen Gesetze* vergleichbar — vor allem die *Prinzipien der Analogie*..., d.h. die Übertragung gewisser Gedankenreihen, welche für ein Gebiet vollständig entwickelt sind, auf *neue* Gebiete, maassgebend werden" [*ibid.*, S. 18 – 20].

Apropos the second class mathematische Prinzipien Voss says:

"Insbesondere ergeben sich aus dem Prinzip der Einfachheit gewisse *allgemeine Gesichtspunkte rein mathematischer Art.* Solche liegen vor, wenn man den Bewegungsraum als *Euklidischen* mit seiner unendlichen Theilbarkeit..., die Koordinaten der Bahnen der Punkte als *stetige*, beliebig oft differenzierbare Funktionen der Zeit..., wenigstens insoweit von einzelnen singulären Stellen abgesehen wird, ansehen, wenn wir demgemäss von den Grenzwerten..., d.h. den Geschwindigkeiten und Beschleunigungen reden..., wenn wir voraussetzen, dass das Verhältniss von Masse zum Volum bei kontinuierlicher Raumerfühlung bei stets abnehmender Grösse des letzteren sich einem bestimmten, überdies wieder differenzierbaren Grenzwerte, der Dichtigkeit, nähere. Die Mathematik ist freilich gegenwärtig so weit entwickelt, dass auch bei Zugrundelegung des allgemeinen Begriffes der stetigen Funktion noch bestimmte Aussagen möglich sind, doch hat sich bisher kein Bedürfnis gezeigt, in die Mechanik diese von der anschaulichen Form der Bewegungsvorgänge weit abliegenden Abstraktionen aufzuheben... Man betrachtet es ferner als ein allgemeines Prinzip, dass eine durch Kräfte definierte Bewegung, durch ihren Anfangszustand vollkommen bestimmt sei...; ausreichend ist dafür, die Kräfte als eindeutige beliebig oft differenzierbare, insbesondere als reguläre Funktionen der Koordinaten und Geschwindigkeiten vorauszusetzen... Hierher gehört auch das Homogeneitätsprinzip. Die Begriffe der Mechanik erfordern die Festsetzung einer Reihe *fundamentaler Einheiten* (z.B. für Länge, Zeit, Masse in der reinen Kinetik), aus denen weitere Begriffe (wie z.B. Geschwindigkeit, Beschleunigung, Kraft etc.) abgeleitet werden. Es liegt nun in der Natur der Sache, dass bei vielen Betrachtungen *Beziehungen zwischen diesen Begriffen von der Wahl dieser Grundeinheiten unabhängig* sein müssen. Solche Gleichungen bleiben daher invariant, wenn die Fundamentaleinheiten der Maassbestimmung durch irgend welche andere unabhängig von einander ersetzt werden. In diesem Charakter der Invarianz besteht das *Prinzip der Homogeneität*...; durch dasseble wird denjenigen Gleichungen der Mechanik, welche zur Beschreibung von den gewählten Einheiten unabhängiger Vorgänge dienen sollen, ein *formeller Charakter* zugeschrieben, der sich zur Prüfung solcher Gasätze selbst nützlich erweist... In einer etwas andere Form kommt das Prinzip bei der Untersuchung dynamischer Verhältnisse als *Prinzip der Ähnlichkeit* (principe de similitude) zur Verwendung... Endlich sei noch an das *Su-*

perpositionsprinzip, als einer unmittelbaren Folgerung aus den Eigenschaften der Lösungen linearer homogener Differentiagleichungen, erinert" [*ibid.*, S. 20 – 23].

At last, apropos of the third class *mechanisch-physikalischen Prinzipien* Voss writes:

"In naher Beziehung stehen den mathematischen Prinzipien die *mechanisch-physikalischen*, insbesondere die *Kontinuitätshypothese*, die man über die *Materie*... die *bewegliche Substanz*, zugrunde legt. Es scheint nicht angebracht, hier zu erörten, ob die Mechanik ein Interesse daran hat, den Begriff der Materie neben dem für sie allein maassgebenden der *Masse* beizubehalten. Die Mechanik geht zunächst von dem Begriff des *materiellen Punktes*..., d.h. eines geometrischen, aber vermöge seiner Masse inserer Beobachtung zugängligen, Punktes, dann von dem System n solcher Punkte aus, und man wird geneigt sein, die Vorgänge bei einem beliebig grossem n, wenigstens soweit allgemeine Theoreme in Frage kommen, hiernach zu beurteilen. Es ist aber leicht zu sehen, dass man auf diesem Wege nicht unmittelbar zu der Vorstellung der Bewegung eines kontinuierlich mit Masse erfüllten Raumes kommt" [*ibid.*, S. 24 – 25].

While in the third part *Die Grundbegriffe der rationellen Mechanik* of Voss' article the basic notions of phoronomy, statics, and dynamics are introduced and discussed, the fourth part *Die speziellen Prinzipien der rationellen Mechanik* contains a formulation and analysis, together with detailed historical and bibliographical data, of the principle of virtual velocities, the principle of Fourier, the principle of D'Alembert and Lagrange's dynamical equations, the principle of Gauss, the principle of Hamilton, the principle of least action, the principle of living force, and the general principle of energy in physics.

Although written almost a century ago ("abgeschlossen im Juli 1901"), this article of Voss' is distinguished by its exactness, elaborateness, and thoroughness, which are scarcely found in the later literary sources on the subject. It penetrates into the historical roots of the idea of mechanical principles, its role in the development of mechanics, its many-sided aspects, and, after all, its prejudices if not superstitions. These are the important reasons inciting us to carry here such vast fragments from this article: after all, the encyclopedia [36] is nowadays not easy of access for the ordinary reader. Nevertheless, after studing even such a profound account on the principles of rational mechanics, one keeps on being at a loss when faced with the question: *what does namely a mechanical principle mean*?

No wonder Voss could not give an explicit answer of this question: the only way to such an answer leads through Hilbert's axiomatic scheme, and his *Grundlagen* [37] have been published only two years before Voss finished *Die Prinzipien der rationellen Mechanik*. If rational mechanics is nothing but mathematics, and mathematics of the first water at that, as Truesdell maintains [27, p. 335, 337], then mechanical principles should differ in no way from mathematical principles. In order words, a principle in mechanics should mean the same thing as a principle in mathematics. Hence, the answer of the question "what does a mechanical principle mean?" would be completely trivial if one knows the answer of the question "what does a mathematical principle mean?" What does it mean, indeed?

As a matter of fact, the term *principle* is in mathematics a historical remnant and its use today may be exculpated only by the language tradition. In reality, this is avoided in modern mathematical texts as an anachronism.

A *principle* in mathematics means a *theorem* or an *axiom*: *tertium non datur*. In other words, a mathematical principle is a mathematical statement which, as such, is either provable (or disprovable) or unprovable (or undisprovable): in the first case it is, purely and simply, a (true or untrue) mathematical theorem; in the second case it is, also purely and simply, a mathematical axiom — neither more, nor less. Classical examples for mathematical principles of the first and second kinds are the principle of Cavalieri and the principle of mathematical induction, respectively: nowadays the principle of Cavalieri is a simple theorem of integral calculus, and the principle of mathematical induction is an arithmetic axiom.

Naturally, the qualification of a mathematical statement as "provable" or "unprovable" is a relative, rather than an absolute, procedure: it depends upon the adopted axioms of the specific mathematical theory. In such a sense, a mathematical principle, qualified as a "theorem" in a given axiomatic system may turn out to be an "axiom" in another one.

This interpretation of the term *principle* is by no means something new: Hilbert proclaimed is more or less a century ago. Hilbert's name is mentioned here in no way accidentally: when the word *axiom* was used above, its meaning was in Hilbert's sense — not the old-fashioned, discredited, disreputed, though nevertheless frequently used by the physicists, in general, and by the mechanicians, in particular, namely "an obvious truth needing no demonstration". Dedekind used to say: *Was beweisbar ist, soll in der Wissenschaft nicht ohne Beweis geglaubt werden.* An axiom is a mathematical statement without a proof not because it does not need one, but for the simple reason that it cannot be proved (in the frames in a given system of other axioms). Hilbert was the first to see that an axiom must unconditionally include undefinable mathematical notions: if all the terms an axiom includes were defined preliminarily, then it would be a (true or untrue) theorem and no axiom at all. The indefinable terms the axioms of a special mathematical theory (arithmetic, geometry, topology, analytical mechanics, in the long run) include are, according to Hilbert's axiomatical principles, defined implicitly namely by this system of axioms.

All these things are truisms today for all educated mathematicians all over the world. Their repetition here may be excused only by the fact that, all the same, they have not yet become a part of the intellectual furniture of most contemporary mechanicians, of their scientific mental constitution. The uninitiated in the present state of affairs in rational mechanics may find it hard to believe; the cold fact, however, remains that ideologically most modern representatives of such a fundamental mathematical theory as analytical mechanics share the logical prejudices of their colleagues from the epoch before the French revolution.

Why should Lagrange call principles the laws de conservation des forces vives, de conservation du mouvement du centre de gravité, de conservation des moments de rotation, and de la moindre quantité de l'action, all these propositions being simple mechanical theorems? A taste for a pompous language? Putting on a consequential air? Or amenability to irrationality? The philosophers delight in chewing upon das Wesen der Dinge, over den Kern der Sache, on weltumfassende

Gedanken; the mathematicians are, however, men of quite another kind: they believe in cats that catch mice.

Similarly, why should one call "principles" those propositions Voss refers to, dividing them into three groups? As regards the *Kausalitätsprinzip*, the *Satz vom zureichende Grunde*, and the *teleologische Prinzipien*, mathematically they are hardly worthy of being mentioned. The same holds good for the *Kontinuitätshypothese*, for Mach's principles of *Ökonomie* and *Einfachheit*, for Hankel's *Prinzip der Permanenz der formalen Gesetze*, *die Prinzipien der* Analogie, the *Stetigkeitshypothese*, the principles of *Homogeneität*, of *Ähnlichkeit*, and the *Superpositionsprinzip* — all these must be regarded as general requirements that are satisfied in a most natural way in the process of technical development of the special mathematical theory under consideration. However, as regards the other mechanical "Prinzipien" Voss mentions — for instance, *das Prinzip der virtuellen Geschwindigkeiten, das d'Alembert'sche Prinzip, Gauss'sche Prinzip des kleinsten Zwanges, das Hamilton'sche Prinzip, das Prinzip der kleinsten Aktion*, etc. — all these statements are specific mechanical theorems to which the qualification "principle" is nowadays attached without the slightest motive or any leg to stand on.

The only two legitimate pretendents for the principles of rigid body analytical dynamics in the whole history of this science among all other impostors are Euler's two dynamical axioms. If one has not become conscious of this fact, then he does not know a B form a buffalo foot in rational mechanics. The essence of the laws of momentum and of kinetical moment is discussed at length in the articles [16, 17], so that in this connection the reader is referred to these sources. What we shall do here is to write down the analytical expressions of Euler's axioms (24) called the *Eulerian dynamical equations*: they will serve us for deepening our understanding of a rigid body dynamical problem.

Let $\bar{p} = \overline{\Omega P}$ for any point P of the rigid body B and let $\bar{p}_G = \overline{\Omega G}$. Then the identity

$$r = r_\Omega + \bar{p} \tag{72}$$

implies

$$\int r\,dm = \int r_\Omega\,dm + \int \bar{p}\,dm \tag{73}$$

or

$$r_G = r_\Omega + \frac{1}{m}\int \bar{p}\,dm \tag{74}$$

by virtue of (56), (57). Now (74) and the identity

$$r_G = r_\Omega + \bar{p}_G \tag{75}$$

imply

(76) $$\overline{\rho}_G = \frac{1}{m}\int \overline{\rho}\, dm.$$

If by definition

(77) $$L_\Gamma = \int \overline{\rho} \times (\overline{\omega} \times \overline{\rho})dm - m\overline{\rho}_G \times (\overline{\omega} \times \overline{\rho}_G)$$

and

(78) $$\boldsymbol{M}_G = \boldsymbol{M} + \boldsymbol{F} \times \boldsymbol{r}_G$$

denotes the moment with respect to G of all forces, both active and passive, acting on B, then Euler's second dynamical axiom takes the form

(79) $$\dot{L}_\Gamma - \boldsymbol{M}_G = \boldsymbol{O}.$$

Let by definition

(80) $$\overline{\rho} = \xi\overline{\xi}^{\circ} + \eta\overline{\eta}^{\circ} + \zeta\overline{\zeta}^{\circ},$$

(81) $$\overline{\rho}_G = \xi_G\overline{\xi}^{\circ} + \eta_G\overline{\eta}^{\circ} + \zeta_G\overline{\zeta}^{\circ},$$

(82) $$I_{\xi\eta} = \int (\xi^2 + \eta^2)dm, \quad I_{\eta\zeta} = \int (\eta^2 + \zeta^2)dm, \quad I_{\zeta\xi} = \int (\zeta^2 + \xi^2)dm,$$

(83) $$D_{\xi\eta} = \int \xi\eta\, dm, \quad D_{\eta\zeta} = \int \eta\zeta\, dm, \quad D_{\zeta\xi} = \int \zeta\xi\, dm,$$

(84) $$A = I_{\eta\zeta} - m(\eta_G^2 + \zeta_G^2), \; B = I_{\zeta\xi} - m(\zeta_G^2 + \xi_G^2), \; C = I_{\xi\eta} - m(\xi_G^2 + \eta_G^2),$$

(85) $$D = D_{\eta\zeta} - m\eta_G\zeta_G, \quad E = D_{\zeta\xi} - m\zeta_G\xi_G, \quad F = D_{\xi\eta} - m\xi_G\eta_G.$$

Besides, let $\boldsymbol{i}, \boldsymbol{j}, \boldsymbol{k}$ be the unit vectors of the axes Ox, Oy, Oz respectively of the inertial system of reference $Oxyz$ and let by definition

(86) $$\boldsymbol{r}_G = x_G\boldsymbol{i} + y_G\boldsymbol{j} + z_G\boldsymbol{k},$$

(87) $$\boldsymbol{F}_\mu = F_{\mu x}\boldsymbol{i} + F_{\mu y}\boldsymbol{j} + F_{\mu z}\boldsymbol{k} \qquad (\mu = 1, \ldots, m),$$

(88) $$\boldsymbol{R}_\nu = R_{\nu x}\boldsymbol{i} + R_{\nu y}\boldsymbol{j} + R_{\nu z}\boldsymbol{k} \qquad (\nu = 1, \ldots, n).$$

At last, let

$$\overline{\omega} = \omega_\xi \overline{\xi}^{\,\circ} + \omega_\eta \overline{\eta}^{\,\circ} + \omega_\zeta \overline{\zeta}^{\,\circ}, \tag{89}$$

$$M_G = M_{G\xi} \overline{\xi}^{\,\circ} + M_{G\eta} \overline{\eta}^{\,\circ} + M_{G\zeta} \overline{\zeta}^{\,\circ}. \tag{90}$$

Then Euler's first dynamical axiom is equivalent with the following system of differential equations

$$m\ddot{x}_G - \sum_{\mu=1}^{m} F_{\mu x} - \sum_{\nu=1}^{n} R_{\nu x} = 0, \tag{91}$$

$$m\ddot{y}_G - \sum_{\mu=1}^{m} F_{\mu y} - \sum_{\nu=1}^{n} R_{\nu y} = 0, \tag{92}$$

$$m\ddot{z}_G - \sum_{\mu=1}^{m} F_{\mu z} - \sum_{\nu=1}^{n} R_{\nu z} = 0, \tag{93}$$

and Euler's second dynamical axiom (79) is equivalent with the following system of differential equations

$$A\dot{\omega}_\xi - (B - C)\omega_\eta\omega_\zeta - D(\omega_\eta^2 - \omega_\zeta^2) - E(\dot{\omega}_\zeta + \omega_\xi\omega_\eta) \\ -F(\dot{\omega}_\eta - \omega_\zeta\omega_\xi) - M_{G\xi} = 0, \tag{94}$$

$$B\dot{\omega}_\eta - (C - A)\omega_\zeta\omega_\xi - E(\omega_\zeta^2 - \omega_\xi^2) - F(\dot{\omega}_\xi + \omega_\eta\omega_\zeta) \\ -D(\dot{\omega}_\zeta - \omega_\xi\omega_\eta) - M_{G\eta} = 0, \tag{95}$$

$$C\dot{\omega}_\zeta - (A - B)\omega_\xi\omega_\eta - F(\omega_\xi^2 - \omega_\eta^2) - D(\dot{\omega}_\eta + \omega_\zeta\omega_\xi) \\ -E(\dot{\omega}_\xi - \omega_\eta\omega_\zeta) - M_{G\zeta} = 0. \tag{96}$$

The differential equations (91) – (96) namely are called the Eulerian dynamical equations. In particular, the equations (91) – (93) are called the ***Eulerian dynamical equations for the motion of the mass-center of a rigid body***, and the equations (94) – (96) are called the ***Eulerian dynamical equations for the motion of the rigid body around its mass-center.***

Since the Eulerian dynamical equations are equivalent to the Eulerian dynamical axioms, in the sense that (91) – (96) are implied by (24) and, inversely, (24) are implied by (91) – (96), sometimes the Eulerian dynamical equations (91) – (93) and (94) – (96) are (incorrectly) called the ***first*** and the ***second Eulerian dynamical law*** respectively.

Let us now give a brief professional characterization of the Eulerian dynamical equations (91) – (96): what in them is given, and what is sought in a dynamical rigid body problem?

There are *given*: first, the *inertial moments* (82) and the *deviational moments* (83), as well as the *mass-center* (76) of the rigid body (or, if not explicitly given, they can be computed, the geometry of the rigid body B and its density being known by the conditions of the dynamical problem under consideration), hence the quantities (84), (85); and, second, the *active forces* (1) acting on B (i.e. their bases and moments).

There are sought, first, the independent parameters (32) of B as *functions* (53) of the time t and of the initial conditions (54), (55); and, second, the *reactions* (2) of the geometrical constraints, which take part in (91) – (96) by means of their *bases* (88) in view of

$$\boldsymbol{N}_\nu = \boldsymbol{r}_{m+\nu} \times \boldsymbol{R}_\nu \qquad (\nu = 1, \dots, n). \tag{97}$$

Let us note that, by virtue of (78), (15), (10) the relations

$$\boldsymbol{M}_G = \sum_{\nu=1}^{m+n} (\boldsymbol{r}_\nu \times \boldsymbol{F}_\nu + \boldsymbol{F}_\nu \times \boldsymbol{r}_G) \tag{98}$$

hold good: at that, the radius-vectors (6) are given functions of the parameters (32) of B and possibly of the time t.

Formulated in such a manner, the dynamical problem is, in the general case, mathematically indetermined: the number of the unknown functions (53), together with that of the unknown components $R_{\nu x}$, $R_{\nu y}$, $R_{\nu z}$ ($\nu = 1, \dots, n$) of the bases (88) of the reactions (2) is greater than the number of the Eulerian dynamical equations (91) – (96) which is 6 at most (5 in the case of a rectilinear rigid rod). From a mathematically formal point of view this indeterminateness is of the kind of the indefiniteness in a geometrical problem, say, for a triangle, the two only sides of which are given. The physical motivation of this indeterminateness is, however, much deeper.

For the time being we shall not enter into detail in this problem. It is closely connected with the definition of the notion of geometrical constraints imposed on rigid bodies and with the dynamical nature of such constraints. We shall confine us to the remark that the simplest way to make a dynamical problem mathematically completely determined passes through the hypothesis that the geometrical constraints imposed on the rigid body are *smooth*. By definition, a geometrical constraints is *smooth* if, and only if, the reaction it generates is normal to this constraint. Applied to the reactions (2) this hypothesis implies

$$\boldsymbol{R}_\nu d\boldsymbol{r}_{m+\nu} = 0 \qquad (\nu = 1, \dots, n). \tag{99}$$

Now (99) implies that in the case of smooth constraints the postulate (30) of ideal constraints is satisfied trivially: if a geometrical constraint is smooth, it is *eo ipso* ideal.

The hypothesis of smooth constraints is classical for analytical dynamics and it is traditionally accepted in the predominant majority of problems this science solves.

Now one can state completely categorically that the competence of the Eulerian dynamical equations (91) – (96) or, just the same, of the Eulerian dynamical axioms (24), is extended over any dynamical problem concerning rigid bodies, in the presence of any geometrical constraints imposed on them and under any sufficiently determined conditions concerning the mathematical nature of the forces of friction. Moreover, although in Euler's times the non-holonomic dynamics belonged to the distant future, nevertheless the Eulerian dynamical equations (91) – (96), and they only, are in the position to solve completely any non-holonomic dynamical rigid body problem.

In other words, the Eulerian dynamical equations (91) – (96) provide a possibility to give, for any (holonomic or non-holonomic) dynamical rigid body problem, a mathematically completely correct answers of the following three questions:

First, are the conditions of the problem consistent: *does the dynamical problem possess a solution or not* (or, in a mechanical aspect, is the motion, described in the conditions of the dynamical problem, possible or not)?

Second, *if a motion exists, then which is it?*

Third, *which are the reactions that generate this motion?*

There we are: here is what Euler has done for analytical dynamics. This is — using Lagrange's own words — what *ne laissera rien à désirer*. These are – again quoting Lagrange — *des formules générales, dont le simple développment donne toutes les équations nécéssaires [et suffisantes] pour la solution de chaque problème*. This is what Euler has published *thirteen years* before the *Méchanique Analitique* saw the light of day and what Lagrange passed over in silence in seven languages in his *Traité*. This is what is seldom found in the mechanical textbooks and is completely omitted in Pars' treatise [26] dealing with twentieth century's analytical dynamics and claiming "to give a compact, consistent, and reasonably complete account on the subject *as it now stands*" [p. VII, our italics]. Because the Eulerian dynamical equations are the nightmare of the Lagrangean dynamical tradition — a rope in hanged-man's house.

We come now to the sign of the cross of this tradition — to the notorious *Lagrangean dynamical equations*

$$\frac{d}{dt}\frac{\partial T}{\partial \dot{q}_\lambda} - \frac{\partial T}{\partial q_\lambda} - Q_\lambda = 0 \qquad (\lambda = 1, \ldots, l) \tag{100}$$

for whose validity the postulate (30) of ideal constraints is a *conditio sine qua non*: the equations (100), simply and purely, do not hold if this postulate is violated.

Almost any modern textbook on analytical dynamics inflames over these equations. Pars, for instance, writes:

"The whole of analytical dynamics is based upon, and is derived from, the theorem of Lagrange that I call the *fundamental equation*... The presentation of the subject ... involves the translation of the fundamental equation into a number of different forms ... six in all... The beautiful and powerful theorem contained in the equations (6.2.1) and (6.2.2) was established by Lagrange in 1760. It provides a simple and expeditious method of forming the equations of motion for any dynamical system... The equations have a central place in Lagrange's great work, the "Mécanique Analytique" [sic] ... published in 1788, one of the epoch-making books in the history of mathematics... The *Mécanique Analytique* is the primary source of the subject of analytical dynamics, and it is rightly regarded as one of the outstanding intellectual achievement of mankind" [*ibid.*, p. VII, 76].

A more sober appraisal is found in Truesdell's *Essays*:

"For systems of mass-points, in 1782 Lagrange had introduced "generalized coordinates" and had achieved the invariant formulation of analytical dynamics now called "Lagrange's equations", from which his treatise has gained its main fame.

Much has been said in praise of these equations which belong to the only part of mechanics of the eighteenth century that is reasonably well known today. Their importance for later work in analytical mechanics and in modern physics is clear. Less clear, perhaps, is that abstractness of formulation conceals the main conceptual problems of mechanics: The role of inertial frames and the concept of rigidity, essential to the classical idea of "observer", are hidden in the invariance of algebra.

It is true that Lagrange's equations have the same form in all descriptions, but it is not true that a system of differential equations in Lagrange's form necessarily belong to a dynamical system satisfying Euler's law in some frame, let alone an inertial frame. By obscuring the forces, Lagrange's equations conceal the invariance group of classical mechanics, which is immediately plain from Euler's equations. Moreover, Lagrange's equations do not reflect the space-time geometry of classical mechanics, the main property of which is the possibility of adding together vectors located at different points. Without this distant parallelism, we may speak of energy, but we cannot even form those other basic quantities of classical mechanics, momentum, center of mass, and moment of momentum. From Lagrange's equations, we cannot tell whether a system has momentum or not; Euler's equations show at once that it does, and this is borne out by the fact that the general integrals of linear momentum first appear in treatments based upon Euler's formulation. In any case, Lagrange's equations are relevant only to special kinds of mechanical systems and are far less general than Euler's laws or than Lagrange's principle of virtual work" [27, p. 133].

It is strange that such an illustrious author as Truesdell makes no mention of other ailments of Lagrange's dynamical equations that are as grave as inborn vices. We shall begin our exposition with the first of them, which is remediable in contrast to the other ones.

Let us remind Voss' remark, already cited: "Es ist aber leicht zu sehen, dass man auf diesem Wege nicht unmittelbar zu der Vorstellung der Bewegung eines kontinuierlich mit Masse erfüllten Raumes kommt." Now, it turns out, that this "leicht zu sehen" is inapplicable to the author of the dynamical treatise [26]. Indeed, all he accomplishes in his book does not a whit touch rigid bodies. The rigid bodies slip out of his fingers as water runs out a sieve. The "fundamental equation" he writes down, the "six forms" of it he derives, all dynamical theories he develops on all the pages of his composition [26] — all this is meaningless as far as rigid bodies are concerned. Because all Pars performs is dynamics of systems of a finite number of discrete mass-points: of one mass-point, of two mass-points, of three mass-points, of four mass-points, of ten mass-points, of hundred mass-points, of thousand mass-points, of million mass-points, of milliard mass-points, of milliard milliards mass-points, of n mass-points, at last, where n is any natural number. And that is it! No body: no sphere, no ellipsoid, no cube, no pyramid, no rod, no disc.. In two words: no continuum, only discrete mass-points, no matter how great their number may be.

Incredible dictu? Incredible. *Factum*? Fact.

Hic Rhodus, hic salta. *Hic* L.A.Pars' *A Treatise on Analytical Dynamics, hic* his "fundamental equation". If somebody states that it is meaningful, to say nothing of valid, for a single rigid body, then this somebody is not all there.

Things being so, what about the solemn promise of the author of [26] "to give a compact, consistent, and reasonably complete account on the subject as it now stands"? *Nominibus mollire licet mala.*

Let us play fair, however. As a matter of fact, promising to expose "the subject as it now stands" he is speaking the naked truth, by an irony of fate. In the traditional literary sources analytical dynamics stands now exactly as he presents it in [26]. It would be unjust, however, if we let him bear the brunt alone. He is not solitary. He only accomplishes what his teachers have taught him. What all his colleagues do. (And what all his pupils will do.) It is not the person — it is the system. It is not the professor — it is the profession. And it is not the analytical dynamics — it is the Lagrangean tradition in it. The Lagrangean tradition is as dangerous as a highway robber: it kills the intellectual faculties of the mechanicians, their individuality, their abilities to think independently, critically, and authentically.

In order not to let the author of [26] take the rap alone, let us invite for company some other authors, adherents of the same mechanical philosophy, say those of the monograph [38] on non-holonomic dynamics. Both books are almost coevals. Though quite different in contents, approaches, and style, far reaching analogues may, nevertheless, be observed.

The first of these similitudes is the attitude towards Euler's dynamical laws. It is true that chapter II of [38], entitled "Изучение движений неголономных систем на основе общих законов динамики. Классические задачи о качении твердого тела по поверхности", is dedicated to the solutions of some classical non-holonomic problems concerning rolling of rigid bodies on surfaces without sliding and slipping (inertial rolling of a dynamically symmetrical ball on a sphere; a heavy symmetrical ball rolling on an inclined plane; rolling of a heavy ball inside a sphere; rolling of a ball inside a sphere, which on its part is rolling on an inclined plane; rolling of a disk and a torus on a horizontal plane; rolling of a hollow sphere with an inside gyroscope on a horizontal plane; motion of Tchapligin's sled; rolling of a homogeneous ball on any surface under the action of a system of forces, the basis of which passes through the center of the ball, ect.). In the same time, however, it is true that the authors of [38] use, at least in their theoretical considerations, Newton's rather than Euler's laws of momentum and of kinetical moment; the first sentence of § 1, entitled "Общие законы динамики. Обобщение теоремы площадей", reads: "Рассмотрим систему, состоящую из N материальных точек с масами $m_i(i = 1, 2, \ldots, N)\ldots$", and all that follows afterwards is derived for such systems only, being in the sequel, nevertheless, applied namely to rigid bodies (just now listed above). On the other hand, in the "Введение" of this book one reads the following most queer statement:

"Зарождение динамики неголономных систем, по-видимому, следует отнести к тому времени, когда всеобъемляющий и блестящий аналитический формализм, созданный трудами Эйлера и Лагранжа, оказался, к всеобщему удивлению, неприменимым к очень простым механическим задачам о качении без проскальзывания твердого тела по плоскости" (стр. 7).

Such a statement is perfectly unacceptable by virtue of two reasons. First, Euler never created "analytical formalism", the contrary may be maintained only by him who is ignorant of the very essence of Euler's work in mechanics, as well as in analysis. Second, it is absolutely false that Euler's mechanical apparatus is inapplicable to the problems of non-holonomic dynamics: as we have already emphasized above, namely Euler's dynamical laws of momentum and of kinetical moments are the genuine and only tools for solving non-holonomic dynamical problems.

In any case, the authors both of [26] and [38] are unanimous in their attitude that, as far as non-holonomic dynamics is concerned, Euler is a *persona non grata*, although, it is true, the name itself is not mentioned explicitly: *homines notos nominare odiosum est.*

Chapter III of the monograph [38] entitled "Аналитическая динамика неголономных систем" deals with various non-holonomic versions of the Lagrangean holonomic dynamical equations: equations of motion of non-holonomic systems with Lagrangean multipliers (equations of Routh), equations of Tchapligin and Voronetz, equations of Volterra and Maggi, equations of motion in quasicoordinates, equations of Appell, etc. The role of a starting point of all the following exposition plays § 1 of this chapter, entitled "Принцип виртуальных перемещений и уравнения Даламбера–Лагранжа", where the principle of virtual displacements (as the principle of virtual work is called in [38]) and the dynamical equation of D'Alembert–Lagrange are deduced.

In order to understand the principle of virtual displacements one must understand the meaning of the term virtual displacement itself. Now the authors of [38] write:

"... виртуальным перемещением системы называется перемещение, которое система совершает при виртуальным варьировании её обобщенных координат. Под виртуальном варьированием при этом понимается бесконечно малое изменение координат, совместимое с наложенными на систему связями и совершаемое в фиксированный момент времени" (стр. 91).

We do not know the reaction of the readers of this article in connection with the above excerpt from [38]. As to the authors of this article they confess that their feelings would hardly be different, if they were confronted with a fragment from the

Koran. They think, at that, that they are scarcely the only people in this world for which such "definitions" sound enigmatically. And they have good grounds for such suspicions.

Indeed, what other reason, save complete lack of understanding of the essence of the (metaphysical?) notion of virtual displacement, could raise the well-known violent dispute concerning the commutativity or non-commutativity of the d-operation and δ-operation, a far away echo of which resounds in the following text from [38]:

"Какие же перестановочные соотношения правильны? До последнего времени даже в учебной литературе не было единой точки зрения в вопросе о перестановочности операций дифференцирования по времени $\frac{d}{dt}$ и виртуального варьирования δ (или, коротко говоря, операций d и δ) при наличии неинтегрируемых кинематических связей. Известны две точки зрения. Согласно одной (которой придерживаются, например, Вольтерра и Гамель), перестановочность операций d и δ имеет место для всех истинных координат $q_1, q_2, \dots, q_n$, независимо от того, является система голономной или неголономной. Согласно другой точки зрения (Суслов, Леви-Чивита и Амальди), переместимость операций d и δ имеет место только для голономных систем. В случае неголономных систем соотношение

$$d\delta q_i - \delta d q_i = 0 \tag{6.4}$$

принимается только для тех обобщенных координат, вариации которых (в соответствии с уравнениями неголономных связей) являются независимыми, а для остальных координат перестановочные соотношения выводятся, исходя из уравнений неголономных связей, и оказываются отличными от (6.4). Эта вторая точка зрения получила всеобщее признание, и ее приверженцы считали первую точку зрения ошибочной. Однако Гамель показал, что обвинение в ошибке не бесспорно, так как, если рассуждать иначе, то первая точка зрения также оказывается приемливой, а именно: противоречие можно снять, если вариации квазикоординат соответствующих связей не полагать равными нулю. Вместе с тем Гамель не дал обоснования своему рассуждению и на вопрос, почему именно так следует поступать, а не иначе, он не ответил. В последнее время в этом вопросе была внесена ясность в работе ..., где показано, что при надлежащем подходе к перестановочным соотношениям обе точки зрения верны и не противоречат, как это казалось, друг другу. Анализ вопрос показывает, что противоречие возникает из-за отсутствия определения исходных понятий, т.е. операций d и δ, содержащихся в перестановочных соотношениях" (стр. 139).

It is obvious that all these profound thoughts — to use Euler's expression apropos of Lagrange's mental speculations — are anything save mathematics. We shall not enter into the essence (if any) of the notion virtual displacement — this *deus ex machina* of the Lagrangean dynamical tradition. In any case we have not yet found an irreproachable definition of this term in all the mechanical literature we have had an opportunity to hold in our hands in the course of a not quite brief professional life, and we think it is very problematic whether there is any at all: otherwise the debates around (6.4) would be quite pointless. At the same time any diplomaed mechanician all the world over would feel deeply offended if one makes the slightest allusion that there is something to be desired in his understanding of the virtual displacements. *C'est la vie.* In result disputes about $d\delta - \delta d$, as if the problem $\boldsymbol{a} \times \boldsymbol{b} + \boldsymbol{b} \times \boldsymbol{a} = ?$ is a question of "единной точки зрения", of "всеобщее признание", of "приверженцы", ect., and so on, и так далее. As regards the above fragment from [38], one is puzzled. Mechanics? — Maybe, being printed in a mechanical monograph — Lagrangean mechanics, however. Mathematics? — Mathematics, my foot.

Things being as they are, let us give a look in [38] in order to see how the principle of virtual displacements is derived in this book. There we read:

"Как известно, движение материальной точки полностью определяется совокупностью сил, приложенных к точке. Для написания уравнений движения системы материальных точек мы должны ввести силы, действующие на каждую из точек, образующих рассматриваемую систему. В число этих сил будут входить и силы, возникающие из-за наличия связей между отдельными точками системы. Таким образом, уравнения движения системы оказываются записанными в виде

$$m_i \boldsymbol{w}_i = \boldsymbol{F}_i + \boldsymbol{R}_i \qquad (1.1),$$

где m_i — масса i-й точки, $\boldsymbol{w}_i$ — ее ускорение, $\boldsymbol{F}_i + \boldsymbol{R}_i$ — действующая на нее суммарная сила, причем $\boldsymbol{R}_i$ — результирующая сил связей, наложенных на i-ую точку. Поскольку мы интересуемся только движением системы, силы связей $\boldsymbol{R}_i$ оказываются лишь вспомагательными величинами, которые мы вынуждены ввести и которые затем следует исключить" (стр. 90 - 91).

Several objections simply obtrude themselves in connection with these formulations, especially as regards the reactions. It is true that "мы интересуемся движением системы", but it is not true that "мы интересуемся *только* движением системы". We are deeply concerned with two other extremely important problems:

First, *does* the mechanical system move at all?

Second, if it does, then *why* does it move?

The answer of the first question is equivalent to the answer of the question whether the system of equations (1.1) is *consistent* or not, in other words, does it possess a *solution* or not? This problem cannot be solved without the reactions $\boldsymbol{R}_i$, since they take part in (1.1) and predestinate the conclusion.

Second, if the system of equations (1.1) has a solution (i.e. if the mechanical system can move), then it moves under the actions not only of the active forces $\boldsymbol{F}_i$, but of the passive forces $\boldsymbol{R}_i$ as well. Hence, $\boldsymbol{R}_i$ represent one of the reasons for the motion of the mechanical system and, consequently, the half at least of the answer of the question *why* does the system move.

In both cases the role of the reactions of the constraints R_i is decisive. Therefore they by no means can be qualified as "лишь вспомагательными величинами". They are *unknown quantities* of the dynamical problem under consideration completely equal in rights with all the other unknown quantities of this problem, the *existence* of which we must establish in a mathematically perfectly irreproachable way, and which, if they exist, we must *determine* from these very equations (1.1) which the authors of [38], like any genuine Lagrangeanists, so vigorously endeavour to eliminate. After these comments the statement of the authors of [38] that the reactions of the constraints are something that "мы вынуждены ввести и которые затем следует исключить" sounds more than queer.

Another objection, not less categorical than that concerning the reactions, affects the relations of the mechanical objects mass-points and rigid bodies. The mechanical ideology of the authors of [38] apropos of these relations is manifested in the following considerations.

"Таким образом, вначале были установлены законы движения материальных точек.

Для изучения законов движения более сложных механических систем стали прибегать к мысленному разбиению такой системы на материальные точки. Такой подход привел к точечному представлению механической системы, т.е. механическую систему стали представлять в виде совокупности материальных точек, которые подчинены определенным связям.

Эта точка зрения проникла даже в гидромеханику в виде так называемого лагранжевого задания движения жидкости, но затем в значительной мере была вытеснена понятием сплошной среды и соответствующим ему эйлеровым способом задания движения жидкости" (стр. 90).

The primitivism of this speculative scheme comprehended by Euler more than two clear centuries ago has never put out of countenance the Lagrangeanists who persevere in it with a doggedness deserving a better destiny. This primitivism involuntarily reminds the historical dawn of integral calculus when Democrite considered the geometrical bodies as sums of an extremely large number of extremely small atoms – hence the denomination of the *method of indivisibles* of this approach by the Latinized *individuum* of the Greek 'άτομος. The development of these ideas should be very instructive for the Lagrangean mechanicians of the Twentieth Century. Archimedes computed the areas and volumes of many figures combining the principles of the level with the idea that a plane figure is composed of an infinite number of parallel rectilinear segments, and a geometrical body is composed of an infinite amount of parallel plane sections. Still in the antiquity, however, such ideas and methods came in for serious criticism. Archimedes, for instance, took the view that it is absolutely obligatory to redemonstrate by the *method of exhaustion* (to use a denomination introduced in the seventeenth century; the essence of the method consists in infinite succesive approximation of unknown quantities by known ones) of any result obtained by the method of indivisibles. Not lesser instructive should be the fact that ardent disputes around the structure of the continuum have arisen still in the mediaeval science, to be continued not less passionately in our times. In any case, no mathematicians of today would apply to the circle a theorem on those only grounds that it is proved for any regular polygon: the *точечные представления* of continual media are, in times long ago, not only completely compromised in mathematical science, but even once and for all exiled out of its paradise.

(This picture does not a whit lessen the heuristic importance of discrete approach to continua, especially in physical theories; in any case, the Archimedean model of mind should not be cast aside.)

We see, in the long run, that the authors of the monograph [38] are intending to derive the principle of virtual displacements for systems of a finite number of mass-points. Here is the formulation of the result of their efforts in this direction:

"Перейдем теперь к формулировке принципа виртуальных перемещений; необходимое и достаточное условие равновесия системы материальных точек с идеальными связями заключается в равенстве нулю виртуальной работы задаваемых сил, т.е.

$$\sum_{i=1}^{N} \boldsymbol{F}_i \delta \boldsymbol{r}_i = 0 \qquad (1.6).$$

Уравнение (1.6) не содержит реакций связей" (стр. 92).

This "result" is absolutely chimerical. Or, using mathematical rather than poetical language, it is completely false.

We shall disprove the sufficiency of the principle of virtual displacements (1.6) for the equilibrium of a system of mass-points using the very arguments of its authors. Their immediate text reads:

"Таким образом, в статике задача об исключении реакций идеальных связей...

$$\delta A = \sum_{i=1}^{N} \boldsymbol{R}_i \delta \boldsymbol{r}_i = 0 \qquad (1.2)$$

... решается следующим образом: прилагая к каждой точке соответствующую реакцию, осуществляющую наложенную связь, заменяют систему совокупностью "свободных" точек, условием равновесия для каждой из которых будет следующее:

$$\boldsymbol{F}_i + \boldsymbol{R}_i = \boldsymbol{O} \quad (i = 1, 2, \dots, N) \qquad (1.7).$$

Если теперь каждое из уравнений (1.7) умножить скалярно на $\delta \boldsymbol{r}_i$ и все выражения сложить, тогда получим уравнение

$$\sum_{i=1}^{N} \boldsymbol{F}_i \delta \boldsymbol{r}_i = -\sum_{i=1}^{N} \boldsymbol{R}_i \delta \boldsymbol{r}_i \qquad (1.8),$$

которое совпадает с (1.6), поскольку правая часть обращается в нуль согласно условию (1.2) идеальности связей" (стр. 92 – 93).

Let us display some mathematical leniency, accepting that in this manner it is proved that (1.7) and (1.2) imply (1.6) indeed. What of it? This proves that the principle of virtual displacement (1.6) is, in the presence of ideal constraints (1.2), a necessary condition for the equilibrium (1.7) of N mass-points. But the authors of [38] state that this same condition (1.6) is also sufficient for (1.7). Is it indeed?

By no means.

First, these same authors do not say in their book a single word more in connection with the proof of the principle of virtual displacement. They have done their work, washed their hands of, shut the door in the reader's face, and locked it out with seven padlocks. The sentence immediately following the just now cited text from [38] reads: "Возвращаясь к динамике, естественно решить задачу об исключении реакций связей сначала также для систем с идеальными связями" (стр. 93). *Acta est fabula.*

Second, there exist counter-examples. In other words, there exist mass-point problems, in which the would-be-sufficient condition (1.6) of the principle of virtual displacements is satisfied, but the mass-points are not in equlibrium.

Here is such a counter-example.

Let a mass-point P be given, on which no active force acts and which is constrained to move on a smooth sphere S. Then P is under the action of the reaction $\boldsymbol{R}$ of S. At that,

$$(101) \qquad \boldsymbol{R} \neq \boldsymbol{0},$$

since otherwise P would be moving alond a straight line and not on S. If $\boldsymbol{F}$ denotes the active force acting on P, then by condition

$$(102) \qquad \boldsymbol{F} = \boldsymbol{0}$$

and (101), (102) imply

$$(103) \qquad \boldsymbol{F} + \boldsymbol{R} \neq 0.$$

The relation (103) displays that P is not in equilibrium under the action of $\boldsymbol{F}$ and $\boldsymbol{R}$.

But it is in equlibrium according to the authors of [38]. Indeed, if O denotes the center of S and $\boldsymbol{r} = \boldsymbol{OP}$, then, on the one hand,

$$(104) \qquad \boldsymbol{F}\delta\boldsymbol{r} = 0$$

in view of (102); on the other hand,

$$(105) \qquad \boldsymbol{R}\delta\boldsymbol{r} = 0,$$

since S is smooth by condition. Now (105) displays that the postulate of ideal constraints is satisfied by $\boldsymbol{R}$, and (104) is, according to the authors of [38], a sufficient condition for the equilibrium of P. *Q.e.d.*

This counter-example has been constructed not later than November 1, 1967 and published in the article [39]; in other words, it and [38] are the same age. Nevertheless, up to now it has been set at naught by the mathematical and mechanical public. Never mind: *Nolo episcopari.*

Other counter-examples, displaying the insufficiency of the principle of virtual displacements for the equilibrium of rigid bodies, are given in the articles [9] and [24], while the principle itself is discussed in the article [13].

If we have paid so much attention to the principle of virtual displacements, the reason is that *abyssus abyssum invocat*, as the Bible preaches: the statical principle of virtual displacements has, thanks to the efforts of D'Alembert and Lagrange, in analytical dynamics a most crooked abortion. It is called the *principle* or the *equation of D'Alembert-Lagrange*, and it namely is what Pars in [26] calls the *fundamental equation*, insisting that "the whole of analytical dynamics is based upon, and is derived, from" it. Let us see what are the authors of [38] doing on this occasion:

"Рассматривая в дальнейшем системы с идеальными связями, заметим, что уравнения (1.1) по форме аналогичны уравнениям (1.7). На эту аналогию обратил внимание уже Даламбер, который сформулировал свой известный принцип, позволивший задачу о составлении уравнений динамики формально свести к составлению уравнений статики. Умножая теперь (1.1) на $\delta \boldsymbol{r}_i$ и складывая все выражения, мы получим уравнение, известное под именем уравнения Даламбера-Лагранжа

$$\sum_{i=1}^{N}(m_i w_i - \boldsymbol{F}_i)\delta \boldsymbol{r}_i = 0 \tag{1.9}$$

которое в динамике играет такую же большую роль, как и принцип виртуальных перемещений (1.6) в статике" (стр. 93).

In such a way, the authors of [38], on the one hand, and the author of [26], on the other hand, are in perfect harmony apropos of their appraisals of the notorious principle of D'Alembert-Lagrange. In this respect they do not differ from any Lagrangeanist whoever, whenever, and wherever all the world over. But — there is a "but", nevertheless...

But all these men are moving along a one-way street. Indeed, both treatises [26] and [38] are lacking in the slightest hint that the routine mathematical manipulations by the aid of which (1.9) is derived from (1.1) possess the necessary quality of reversibility. The text of [38] immediately following the last citation reads:

"Таким образом, мы пришли к тому, что для любого виртуального перемещения произвольной механической системы с идеальными связями имеет место равенство (1.9). В координатной форме уравнение Даламбера-Лагранжа (1.9) может быть представлено в виде

$$\sum_{j=1}^{3N}(m_j \ddot{x}_j - X_j)\delta x_j = 0 \tag{1.10}$$

Пусть $q_1, q_2, \ldots, q_n$ обобщенные координаты рассматриваемой системы..." (стр. 93).

Afterwards the authors of [38] derive, on the basis of (1.9), the Lagrangean dynamical equations (100).

In other words, the authors of [38] derive (1.9) from (1.1) in case of (1.2), leaving the question open whether, again in the case (1.2), the equations (1.1) are restorable on the basis of (1.9). But this question is of cardinal importance. It is identical with the question of the equivalence of the equations of motion of a system of N mass-points and of the D'Alembert-Lagrange's dynamical equation (1.9). And if they are not equivalent? What then?

Then there must exist dynamical problems for which the equations of motion (1.1) and the principle of D'Alembert-Lagrange (1.9) will lead to different solutions. Moreover, since Lagrange's dynamical equations (100) are derived by means of (1.9), then they also will, in some cases of dynamical problems at least, lead to solutions different from those obtained by means of (1.1). Since the equations (1.1) namely are authoritative as regards the motions of mass-point systems, as far as Newton's dynamics is accepted as authentic, this implies that both D'Alembert-Lagrange's principle (1.9) and the Lagrangean dynamical equations (100) are unreliable and untrustworthy in the capacity of dynamical laws.

This danger is not only a potential one, it is actual. Indeed, there exist counter-examples. There exist dynamical problems, non-holonomic as well as holonomic, involving rigid bodies, for which the solutions, obtained by the aid of the Eulerian dynamical axioms, on the one hand, and by the Lagrangean dynamical equations (or their non-holonomic versions in the non-holonomic case), on the other hand, are quite different — in other words, mutually exclusive. Such counter-examples may be swarmed in legions. The first of them were published in our articles [5 – 7, 21, 22, 24]. They were called dynamical "paradoxes" or *antinomies* and the mathematical mechanism ruling these mechanical phenomena is revealed in the articles in question. This is, however, a topic we shall treat later in detail. For the time being we shall still analyse some characteristic features of the treatise [38].

(The principle of D'Alembert-Lagrange has a most counter-productive effect in educational aspect. Indeed, let us imagine a young student making his first steps in science who sees for the first time the deduction of the simple corollary (1.9) from (1.1) and (1.6), pompously named the principle of D'Alembert-Lagrange; if he is observant, then he will not leave unnoticed the fact that the requirement (1.6) is a quite artificial one — a little short of a trick. Now, what will the student's feelings be? A simple something — the science! You perform a scalar multiplication of (1.1) with δr_i, add the obtained results together, take (1.6) into consideration and — without any intellectual effort — your name remains in mechanics forever! Is it strange that Lagrange has so many imitators? That many dozens, if not hundreds, of equations of motion of every sort and kind have been till now proposed on the mechanical stock-market — in any case more, than particular dynamical problems have been solved by their aid? That all this scientific politics led to such a devaluation of mathematical values, to such an inflation of mechanical works? Alas, indeed. And all this is an inevitable consequence of the letter and spirit of the Lagrangean dynamical tradition.)

All following considerations in chapter III of [38] are, directly or indirectly, based on D'Alembert-Lagrange's equation (1.9). Therefore, all kinds of non-holonomic dynamical equations of motion are valid only to mechanical systems consisting of a finite number of discrete mass-points and to nothing else — at least not, before the contrary is proved in an irreproachable manner. That is why all the applications to rigid bodies of these non-holonomic dynamical Lagrangean versions that are made in the monograph [38] are mathematically illegal — and no prevarications, no matter how ingenious they might be, can alter this fact. In reality, the authors of [38] have come to a dead-lock in the moment they have decided to expose in their book those non-holonomic equations of motion that have become most popular in this domain, following the originals of their inventors at that — in these originals is also nothing more than mechanical systems of a finite number of discrete mass-points.

There is a very interesting problem in connection with the equation (1.9) of D'Alembert–Lagrange on which we should like to fix the reader's attention.

There are many mechanical notions which are firstly defined for systems of a finite number of mass-points and afterwards these definitions are generalized for rigid bodies by a mere formal change of finite sums by integrals.

Let P_ν be mass-points with masses m_ν $(\nu = 1, \dots, n)$ respectively and let S be their system. Then by definition

$$m = \sum_{\nu=1}^{n} m_\nu \tag{106}$$

and

$$\boldsymbol{r}_G = \frac{1}{m}\sum_{\nu=1}^{n} m_\nu \boldsymbol{r}_\nu \tag{107}$$

are the *mass* and the radius-vector of the *mass-center* G of S respectively provided $\boldsymbol{r}_\nu = \boldsymbol{OP}_\nu$ $(\nu = 1, \dots, n)$ and $\boldsymbol{r}_G = OG$. Similarly, the quantities

$$\boldsymbol{K} = \sum_{\nu=1}^{n} m_\nu \boldsymbol{v}_\nu, \tag{108}$$

$$\boldsymbol{L} = \sum_{\nu=1}^{n} \boldsymbol{r}_\nu \times m_\nu \boldsymbol{v}_\nu, \tag{109}$$

$$T = \frac{1}{2}\sum_{\nu=1}^{n} m_\nu \boldsymbol{v}_\nu^2 \tag{110}$$

where $\boldsymbol{v}_\nu = \dot{\boldsymbol{r}}_\nu$ $(\nu = 1, \dots, n)$ are by definition the *momentum*, the *kinetical moment*, and the *kinetic energy* of S respectively. Now the analogy between the definitions (106) — (110), on the one hand, and (56), (57), (23), (11) respectively, on the other hand, is obvious. At that, the advisability of the second series of definitions is suggested by the first one.

Similarly, there are theorems of discrete analytical mechanics which have analogues in the continual one. For instance, *Newton's laws of momentum* and *of kinetical moment* for a single mass-point P with mass m, namely

$$\frac{d}{dt}(m\boldsymbol{v}) = \boldsymbol{F}, \qquad \frac{d}{dt}(\boldsymbol{r} \times m\boldsymbol{v}) = \boldsymbol{M}, \tag{111}$$

$\boldsymbol{F}$ and $\boldsymbol{M}$ denoting the basis and the moment (with respect to O) respectively of the system of forces acting on P, provided $\boldsymbol{r} = \boldsymbol{OP}$ and $\boldsymbol{v} = \dot{\boldsymbol{r}}$, applied to S, lead to the systems of equations

$$\frac{d}{dt}(m\boldsymbol{v}_\nu) = \boldsymbol{F}_\nu, \qquad \frac{d}{dt}(\boldsymbol{r}_\nu \times m_\nu \boldsymbol{v}_\nu) = \boldsymbol{M}_\nu \tag{112}$$

$(\nu = 1, \ldots, n)$, $\boldsymbol{F}_\nu$ and $\boldsymbol{M}_\nu$ $(\nu = 1, \ldots, n)$ denoting the same as $\boldsymbol{F}$ and $\boldsymbol{M}$ respectively, however, for P_ν $(\nu = 1, \ldots, n)$ instead for P.

Let by definition

$$\boldsymbol{F} = \sum_{\nu=1}^{n} \boldsymbol{F}_\nu, \qquad \boldsymbol{M} = \sum_{\nu=1}^{n} \boldsymbol{M}_\nu. \tag{113}$$

If we add (112) together and take into consideration (113) and (108), (109), then we obtain the equations (24) respectively. But as a proof of Euler's dynamical laws this approach is false, since, by its very essence, it holds good for systems of a finite number of mass-points only. At that, the proved validity of (24) for such systems does not a whit imply the validity of (24) for rigid bodies: for rigid bodies the laws (24) are beyond demonstration; they are dynamical axioms.

With this only exception, all other continual analogues of theorems of discrete mechanics, which are true, can be proved, as for instance the kinetic energy theorem (14), which is trivially true in the discrete case, had been generalized by us in the continual case.

Now let the following question be put: which continual variant does D'Alembert-Lagrange's principle (1.9) admit?

Strange though it may seem at first glance, the answer of this question is, negative: no variant. The equation (1.9) of D'Alembert-Lagrange, proposed for discrete mechanical systems, has no immediate analogue in rigid body dynamics.

The reason for this state of affairs is concealed in the lack of a strict mathematical definition of the notion of virtual displacement already mentioned above. The Lagrangeanists are reasoning in the following manner. Let (32) be mutually independent parameters of the discrete system S of mass-points introduced above. This means that all $\boldsymbol{r}_\nu$ $(\nu = 1, \ldots, n)$ are wholly determined functions

$$\boldsymbol{r}_\nu = \boldsymbol{r}_\nu(q_1, \ldots, q_l) \qquad (\nu = 1, \ldots, n) \tag{114}$$

of (32) whence

$$\delta \boldsymbol{r}_\nu = \sum_{\lambda=1}^{l} \frac{\partial \boldsymbol{r}_\nu}{\partial q_\lambda} \delta q_\lambda \qquad (\nu = 1, \ldots, n). \tag{115}$$

Now (115) and D'Alembert-Lagrange's principle imply

$$\sum_{\lambda=1}^{l}\sum_{\nu=1}^{n}(m_\nu \boldsymbol{w}_\nu - \boldsymbol{F}_\nu)\frac{\partial \boldsymbol{r}_\nu}{\partial q_\lambda}\delta q_\lambda = 0. \tag{116}$$

Let again by definition

$$Q_\lambda = \sum_{\nu=1}^{n}\boldsymbol{F}_\nu \frac{\partial \boldsymbol{r}_\nu}{\partial q_\lambda} \qquad (\lambda = 1, \dots, l) \tag{117}$$

be the generalized active forces acting on the system S. On the other hand, (114) imply

$$\boldsymbol{v}_\nu = \sum_{\lambda=1}^{l}\frac{\partial \boldsymbol{r}_\nu}{\partial q_\lambda}\dot q_\lambda \qquad (\nu = 1, \dots, n) \tag{118}$$

whence

$$\frac{\partial \boldsymbol{v}_\nu}{\partial \dot q_\lambda} = \frac{\partial \boldsymbol{r}_\nu}{\partial q_\lambda} \qquad (\lambda = 1, \dots, l; \quad \nu = 1, \dots, n). \tag{119}$$

Besides

$$\boldsymbol{w}_\nu \frac{\partial \boldsymbol{r}_\nu}{\partial q_\lambda} = \frac{d\boldsymbol{v}_\nu}{dt}\frac{\partial \boldsymbol{r}_\nu}{\partial q_\lambda} = \frac{d}{dt}\left(\boldsymbol{v}_\nu \frac{\partial \boldsymbol{r}_\nu}{\partial q_\lambda}\right) - \boldsymbol{v}_\nu \frac{d}{dt}\frac{\partial \boldsymbol{r}_\nu}{q_\lambda} \tag{120}$$

and

$$\frac{d}{dt}\frac{\partial \boldsymbol{r}_\nu}{\partial q_\lambda} = \sum_{\mu=1}^{l}\frac{\partial^2 \boldsymbol{r}_\nu}{\partial q_\mu \partial q_\lambda}\dot q_\mu \tag{121}$$

($\lambda = 1, \dots, l$; $\nu = 1, \dots, n$). At last, (118) imply

$$\frac{\partial \boldsymbol{v}_\nu}{\partial q_\lambda} = \sum_{\mu=1}^{l}\frac{\partial^2 \boldsymbol{r}_\nu}{\partial q_\lambda \partial q_\mu}\dot q_\mu \qquad (\lambda = 1, \dots, l; \quad \nu = 1, \dots, n) \tag{122}$$

and (121). (122) imply

$$\frac{d}{dt}\frac{\partial \boldsymbol{r}_\nu}{\partial q_\lambda} = \frac{\partial \boldsymbol{v}_\nu}{\partial q_\lambda} \qquad (\lambda = 1, \dots, l; \quad \nu = 1, \dots, n). \tag{123}$$

Now (120), (119), (123) imply

$$\boldsymbol{w}_\nu \frac{\partial \boldsymbol{r}_\nu}{\partial q_\lambda} = \frac{1}{2}\frac{d}{dt}\frac{\partial \boldsymbol{v}_\nu^2}{\partial \dot q_\lambda} - \frac{1}{2}\frac{\partial \boldsymbol{v}_\nu^2}{\partial q_\lambda} \tag{124}$$

($\lambda = 1, \dots, l$; $\nu = 1, \dots, n$) and (116), (117), (124), (110) imply

(125)
$$\sum_{\lambda=1}^{l} \left(\frac{d}{dt} \frac{\partial T}{\partial \dot{q}_\lambda} - \frac{\partial T}{\partial q_\lambda} - Q_\lambda \right) \delta q_\lambda = 0.$$

Obtaining (125), the Lagrangeanists maintain that, since δq_λ ($\lambda = 1, \dots, l$) are mutually independent, the relation (125) is possible if all coefficients of these variations are zeroes, i. e. if the Lagrangean dynamical equations (100) (for discrete dynamical systems!) hold good. This conclusion cannot be implied if $d\mathbf{r}_\nu$ and dq_λ stood instead of $\delta \mathbf{r}_\nu$ and δq_λ respectively ($\lambda = 1, \dots, l$; $\nu = 1, \dots, n$). Indeed, in this case

(126)
$$\sum_{\lambda=1}^{l} \left(\frac{d}{dt} \frac{\partial T}{\partial \dot{q}_\lambda} - \frac{\partial T}{\partial q_\lambda} - Q_\lambda \right) \dot{q}_\lambda = 0$$

would stay instead of (125), and (126) do not imply (100).

If one accepts (125) and hence (100) for an equivalent of D'Alembert-Lagrange's principle (116), then one could say that the Lagrangean dynamical equations for rigid bodies are a continual version of this principle.

Let us now make a brief recapitulation. The aims of this second part, *The Present*, of the series of articles entitled *Lagrange or Euler?* are to give an undisguised account on the contemporary state of analytical dynamics as it may be established by the popular literary sources. We have analysed in some details only two such sources, namely [26] and [38], but this is more than enough, since they are typical for almost all books on the subject published nowadays. We have not discussed results published in the articles [2 – 24], since though chronologically belonging to *The Present*, thematically they belong to *The Future*: a mathematical result belongs to a certain epoch, if it is popular in this epoch, and [2 – 24] are not popular, for the time being at least. (For instance, although printed in 1799, Wessel's work [40], containing for the first time in the mathematical literature the geometrical representation of the complex numbers, belongs to a period in the history of mathematics at least twenty years younger, being wholly unnoticed in the meantime, to be rediscovered a hundred years later [41].) The main results of [2 – 24] will be discussed in the third, final part of this series.

In the beginning if this article the questions have been put: what does the term *Lagrangean dynamical tradition* mean; which are its *characteristic features*; what does *Lagrangean mechanics* mean? It is high time to try and give, more or less satisfactory, answers to these questions.

We must fix our attention on the first of these questions. Its answer, if genuine, *eo ipso*, answers the second one. As regards the third, one may say: *Lagrangean mechanics* means that misshapen image of rational mechanics which the Lagrangeanists wear out in their minds.

The Lagrangean dynamical tradition is an unfortunate phenomenon in the history of rational mechanics, in general, and of analytical mechanics, in particular, conceived and borne in the womb of *Méchanique Analitique* in the very dawn of the French Revolution. In 1783 died Euler who:

"... was the domination theoretical physicist of the eighteenth century. While his work is underestimated in the usual, vague historical books and attributions, the short factual history in the old *Handbuch der Physik* lists twice as many specific discoveries for Euler as for any other one physicist, earlier or later, and even at that most of his work is omitted. When Euler was nineteen, still a student, he outlined a great treatise on mechanics in six parts, only three of which he lived to finish in the next fifty years, although his program was virtually achieved by the hundreds of papers he published in all branches of mechanics and was supplemented by treatises on naval science, ballistics, and astronomy... He put most of mechanics into its modern form; from his books and papers, if indirectly, we have all learned the subject, and his way of doing things is so clear and natural as to seem obvious... Three problems were critical and central for the development of a general mechanics. All three consisted in discovery of differential equations of motion for particular kinds of space-filling bodies:

1. A rigid body.
2. A perfect fluid.
3. An elastic bar.

All three of these problems were solved by Euler... With Euler's memoir in 1775, the whole program of rational mechanics becomes clear... In fact, this scheme remained general enough for at least 100 years" [27, p. 106, 167, 173].

Truesdell's last statement would remain true, if he wrote 1 000, or 1 000 000, or even eternity instead of 100 years. As a matter of fact, Euler's mechanical scheme is perpetual inasmuch as Euclid's geometrical scheme is. It is true that in this century new mechanical domains have been discovered that lie altogether outside Euler's range. These discoveries, however, concern Euler's mechanics to such a degree, to which the discovery of the A-bomb concerns Euclidean geometry: it pertains to entirely different phenomena, for which any juxtaposition is pointblank pointless. In Euler's days no thermodynamic and electromagnetic effects could be taken into consideration in mechanics. As regards the relativity, from a purely logical point of view its relation with Newton-Eulerian dynamics is just the same, as the relation between Euclidean and non-Euclidean geometries.

Under these circumstances, the *Méchanique Analitique* is not only a complete depreciation of the whole scientific inheritance of Euler in mechanics — moreover, it is a strong deprecation against it. As we have seen, Lagrange does not include Euler's dynamical axioms in his list of dynamical principles. The worst is yet to come: Lagrange's *Traité* wholly disregards these fundamental dynamical laws, setting against them Lagrange's dynamical equations. All his life through Lagrange has not understood the very essence of his own equations, overestimating them in the most repulsive way, and thousands of mechanicians today all the world over share this prejudice. As we shall see in details in the third part of this series, the Lagrangean dynamical equations are, purely and simply, linear combinations of the projections of the Eulerian dynamical axioms on appropriate axes.

Indeed, as it has been shown in the articles [10 – 12, 14, etc.] the following *fundamental identities of Lagrangean formalism* hold good:

$$\frac{d}{dt}\frac{\partial T}{\partial \dot{q}_\lambda} - \frac{\partial T}{\partial q_\lambda} - Q_\lambda - Q^*_\lambda = (\dot{\boldsymbol{K}} - \boldsymbol{F})\frac{\partial \boldsymbol{r}_G}{\partial q_\lambda} + (\dot{\boldsymbol{L}}_\Gamma - \boldsymbol{M}_G)\frac{\partial \bar{\boldsymbol{\omega}}}{\partial \dot{q}_\lambda} \tag{127}$$

($\lambda = 1, \ldots, l$), where by definition

$$Q^*_\lambda = \sum_{\nu=1}^{n} \boldsymbol{R}_\nu \frac{\partial \boldsymbol{r}_{m+\nu}}{\partial q_\lambda} \qquad (\lambda = 1, \ldots, l) \tag{128}$$

are the *generalized forces of the reactions of the constraints* imposed on the rigid body B. The equations (127) are, as a matter of fact, mathematical ***identities***. In other words, the relations (127) are obtained by the aid of identical transformations accomplished on the definitions of the mechanical quantities involved, namely (15) for $\boldsymbol{F}$ and $\boldsymbol{M}$, (57) and (9) for $\boldsymbol{r}_G$ and $\bar{\boldsymbol{\omega}}$ respectively, (78) for $\boldsymbol{M}_G$, (76) and (77) for $\bar{\boldsymbol{p}}_G$ and $\boldsymbol{L}_\Gamma$ respectively, (23) for $\boldsymbol{K}$, (11) for T, and (42) and (128) for Q_λ and Q^*_λ respectively. Now

$$Q^*_\lambda = 0 \qquad (\lambda = 1, \ldots, l) \tag{129}$$

in the case of ideal constraints (30). For such constraints, consequently, the fundamental identities (127) take the form

$$\frac{d}{dt}\frac{\partial T}{\partial \dot{q}_\lambda} - \frac{\partial T}{\partial q_\lambda} - Q_\lambda = (\dot{\boldsymbol{K}} - \boldsymbol{F})\frac{\partial \boldsymbol{r}_G}{\partial q_\lambda} + (\dot{\boldsymbol{L}}_\Gamma - \boldsymbol{M}_G)\frac{\partial \bar{\boldsymbol{\omega}}}{\partial \dot{q}_\lambda} \tag{130}$$

($\lambda = 1, \ldots, l$). Now (130) imply that the left-hand sides of the Lagrangean dynamical equations (100) are, neither more nor less, linear combinations of the projections of the left-hand sides of the Eulerian dynamical axioms (24) (the first of the latter) and (79) on axes parallel to the vectors $\frac{\partial \boldsymbol{r}_G}{\partial q_\lambda}$ and $\frac{\partial \bar{\boldsymbol{\omega}}}{\partial \dot{q}_\lambda}$ ($\lambda = 1, \ldots, l$) respectively. Now any child could judge what a value could be assigned to a mathematical result derivable from another, well-known, mathematical result, by a mere projection of the latter.

Let us note that the fundamental identities of Lagrangean formalism (130) at once prove, in case of ideal constraints, the validity of the Lagrangean dynamical equations for rigid bodies rather than for systems of a finite number of discrete mass-points only, on the basis of the Eulerian dynamical axioms. In such a manner this missing link in the dynamical literature is discovered and put on its right place. Let us remind that, at the beginning of this article, we have underlined that one of the ailments of the Lagrangean equations is curable. Up to now this disease consisted in the fact that these equations have been nowhere proved except for discrete mass-points. Now the fundamental identities (130) provide a medicine for this weakness.

Why are the Lagrangeanists so opposed against the Eulerian dynamical axioms or, just the same, against the Eulerian dynamical equations (91) – (96) as well? Very simply: they do not like the reactions these latter equations contain. And why do they not like these reactions? Very simply again: the mathematical problem the equations (91) – (96) solve is much more complicated than the mathematical problem the equations (100) solve. Working with (100) is out and out easier: you compute the kinetic energy T of the rigid body and the generalized active forces Q_λ ($\lambda = 1, \dots, l$), differentiate with respect to q_λ, $\dot{q}_\lambda$ ($\lambda = 1, \dots, l$) and to t, substitute the derivatives in (100) and that is the end of your troubles. A neat job. While, working with (91) – (96) you have to compute inertial and deviational moments (where a considerable amount of geometry is needed; here is the open sesame of Lagrange's faith that his "méthodes ... ne démandent ni constructions, ni raisonnements géométriques ou méchaniques") and moments of forces, to eliminate unknown reactions, etc. A painful task, is it not?

Do not, however, get mislead by a popular fallacy: do not take all this at its face value The problem *Lagrange or Euler?* is not a matter of taste — of liking or disliking reactions of constraints. There is more to it than that. This problem is much deeper. It is a matter of principle. The point is that working with the Eulerian dynamical equations (91) – (96) you obtain the genuine solution of the dynamical problem under consideration, and you obtain it by the sole possible way. While, working with the Lagrangean dynamical equations (100) you attack windmills.

By the aid of the Lagrangean dynamical equations it is impossible to solve even a single dynamical problem concerning constrained rigid bodies.

Does this statement seem improbable, incredible, unbelievable, inconceivable, unthinkable? Does it? At first glance, it does. Let us, however, make a little thinking.

As already twice emphasized, any dynamical problem, as any mathematical problem in general, as a matter of fact consists of two questions:

1. Does there exist a solution of the problem?
2. If a solution exists, then which is it?

Now, *the Lagrangean dynamical equations answer neither the first, nor the second of these questions* (with the only exception, when the rigid body is free).

In order to see that, let us look again at the fundamental identities (130). As a matter of fact, they deliver a method for the exclusion of the unknown reactions of the constraints. Let the rigid body be non-free, i.e. $l < 6$ ($l < 5$ in the case of a rigid rod). Then the Lagrangean dynamical equations (100) represent a system of l differential relations, while the number of the Eulerian dynamical equations (91) – (96) is $6 > l$: it is clear that, in this case, *the Eulerian dynamical equations are unrestorable from the Lagrangean ones.*

On the other hand, the reactions of the constraints are that namely mathematical factor which may make the Eulerian dynamical equations an inconsistent system of conditions. Let this be the case. Then the dynamical problem has no solution, while the Lagrangean dynamical equations manifest a completely determinate (although wholly illusionary) solution: the reactions are excluded in them and they cannot "see" the inconsistency.

(There is no need to compute the number of the Eulerian (6) and of the Lagrangean ($l < 6$) equations in order to conclude that the first are not restorable from the latter. It is enough to take into consideration that the Lagrangean equations do not contain the unknown reactions and that the Eulerian ones do: as plainly as anything.)

The impotence of the Lagrangean dynamical equations at the face of the problem of the existence of a solution is the clue to the riddle of most of the dynamical "paradoxes" we have mentioned above. Many of them are, indeed, dynamical problems with no solution. The Eulerian dynamical axioms catch this phenomenon, while the Lagrangean equations do not, supplying the problem with a completely definite solution. *Cum grano salis*, the Lagrangean dynamical equations are so powerful that they can describe non-existing motions.

Let us now suppose that the conditions of the dynamical problem are consistent, hence a solution exists. Now we state that this solution slips through Lagrange's fingers.

When existing, the solution of a dynamical problem for a rigid body consists in answering two questions:

1. How does the body move?
2. Why does the body move?

The first question is answered fairy well by the Lagrangean dynamical equations, while the second is not. Indeed, a constrained rigid body is moving under the action of both the active forces (1) and the passive forces (2). The first ones are given in the conditions of the dynamical problem under consideration, while the second ones are wholly unknown quantities that are to be determined in the process of problem solving. Naturally, the Lagrangean dynamical equations, where the reactions are deliberately excluded, are completely helpless at the face of this question.

In few words: *the Lagrangean dynamical equations describe the motion if any, and cannot determine the reactions of the constraints.*

The main characteristic feature of Lagrange's approach to analytical mechanics is its superficiality. Proposing a mathematically simple *Schablon* for problem solving, it does not stimulate the solver to penetrate its deeper aspects, the geometry of the rigid body and of the constraints imposed on it. At that, not only Lagrange with his notorious "On ne trouvera point de Figures dans cet Ouvrage", but together with him any of his adherents is boasting with this neglecting of geometry. So, for instance, Pars reproduced on p. 76 of his treatise [26] this statement of Lagrange's, commenting it in a most stilted manner.

In the meanwhile, no genuine mechanical problem can be solved without geometry, let analytical or differential, if not synthetical. The very definition of the fundamental for analytical mechanics rigid body concept is purely geometrical, as well as the notion of geometrical constraints imposed on such bodies. The Lagrangeanists, however, inspired by their magister's mechanical philosophy, pay little attention to such prejudices as strict mathematical definitions or rigorous mathematical demonstrations: the intuition of the initiated is for them much more trustworthy and reliable. Truesdell's ascertainment that in the *Méchanique Analitique* one could find "no explanation of concepts, no illustrations either by diagrams or by developed examples, and no attempt to justify any limit process by rigorous mathematics" [27, p. 173] remains valid for any mechanical exposition of any Lagrangeanist.

All these circumstances lead to such a situation in the mechanical literature with Lagrangean deviations that it hardly could be qualified otherwise save as chaotic. Along with the rigid body concept, this topsyturvydom concerns to the highest degree the notions of holonomic and non-holonomic constraints imposed on rigid bodies.

In order not to be unsubstantiated, let us again take a look at the repeatedly mentioned monograph [38] on non-holonomic dynamics, seeking to see the way its authors introduce these basic dynamical concepts.

The first sentences the book begins with read:

"При рассмотрении многих вопросов движения и равновесия механических систем возможна дискретная идеализация, т.е. идеализация, при которой механическое состояное рассматриваемой системы определяется конечным числом величин. В этом случае систему называют дискретной. К дискретным системам, например, относятся системы, состоящие из конечного числа материальных точек и абсолютно жестких тел.

Во многих случаях в силу устройства самой системы отдельные ее части не могут двигаться произвольным образом, их движения и положения как-то связаны между собой и подчинены ряду условий и ограничений. На систему, как принято говорить, наложены связи. Конкретный вид этих связей может быть весьма различный. Это может быть, например, шестереночное зацепление, соединение двух отдельных тел стержнем неизменной длины или что-либо другое. Для нас сейчас будут важны лишь те ограничения на геометрическое расположение и движение отдельных частей системы, которые влекут эти связи. Связи могут налагать ограничения на возможные геометрические расположения отдельных частей — такие связи называются геометрическими, и на кинематически возможные ее движения, т.е. на возможные значения скоростей ее отдельных частей — такие связи называются кинематическими. Ясно, что всякая геометрическая связь вместе с тем представляет собой и некоторую кинематическую связь; однако обратное, как оказывается, может и не иметь места, т.е. связь между возможными скоростями отдельных частей системы может не приводить к ограничениям на возможные их положения" (стр. 9).

This text implies that the book [38] is addressed to a widest auditorium — in any case, to readers also who have not the slightest idea of non-holonomic dynamics. It is clear also that it is not a mathematical text. In a sence, it is even a text that could be as well designed for students in physics. As a matter of fact its aim is to give a physical motivation of the mathematically formal definitions and demonstrations which, naturally, must follow. As such explanatory text it is beyond criticism: any author has his rights of methodological concessions.

The quoted fragment of [38] is followed immediately by an example. Forthwith after it one reads:

"На существование кинематических связей, не накладывающих никаких ограничений на возможные конфигурации механической системы, было обращено внимание сравнительно недавно. Еще Лагранж в своей знаменитой "Аналитической механике" не подозревал об их существовании. Это выразилось в том, что он считал возможным для любой механической системы с учетом условных уравнений, вытекающих из природы этой системы, выбирать независимые координаты с независимыми же вариациями. Этот просмотр обнаружился лишь значительное время спустя в связи с изучением различных случаев движения твердых тел, принужденных катиться без проскальзыванием по плоскости или по более сложной поверхности. Только в 1894 г. Герц ввел разделение связей и механических систем на голономные и неголономные.

Представим себе, что мы мысленно или фактически сняли с системы ряд имеющихся у нее связей и что после этого ее геометрическое положение определяется n величинами $q_1, q_2, \ldots, q_n$, называемыми обобщенными координатами. Тогда произвольному изменению этих обобщенных координат во времени соответствует некоторое движение освобожденной системы. Если теперь вновь наложить на систему снятые связи, то уже не всяким изменениям обобщенных координат $q_1, q_2, \ldots, q_n$ будет соответствовать некоторое движение системы. Изменения обобщенных координат и их значения должны теперь подчинятся ряду условий, нарушение которых означало бы нарушение наложенных связей. Эти условия могут, в частности, выражаться системой неравенств вида

$$f_\alpha(q_1, q_2, \ldots, q_n; \dot{q}_1, \dot{q}_2, \ldots, \dot{q}_n; t) \geqq 0 \tag{1.3}$$

($\alpha = 1, 2, \ldots, m$), и тогда соответствующие связи называются односторонными, неудерживающими или освобождающими, или уравнениями вида

$$f_\alpha(q_1, q_2, \ldots, q_n; \dot{q}_1, \dot{q}_2, \ldots, \dot{q}_n; t) = 0 \tag{1.4}$$

($\alpha = 1, 2, \ldots, m$), и тогда соответствующие им связи называются двусторонными, удерживающими или неосвобождающими. Удерживающие связи в свою очередь делят на геометрические и кинематические, зависящие и независящие от времени, соответственно тому, входит или не входит явно время в их уравнения. Связи называются геометрическими, если они выражаются уравнениями вида

$$f_\alpha(q_1, q_2, \ldots, q_n; t) = 0 \tag{1.5}$$

($\alpha = 1, 2, \ldots, m$), и кинематическими, если выражающие их уравнения имеют вид

$$f_\alpha(\dot{q}_1, \dot{q}_2, \ldots, \dot{q}_n; q_1, q_2, \ldots, q_n; t) = 0 \tag{1.6}$$

($\alpha = 1, 2, \ldots, m$). Это определение вполне согласуется с тем, что говорилось о геометрических и кинематических связях в самом начале этого параграфа.

Кинематические связи (1.6) могут быть интегрируемыми или неинтегрируемыми, соответственно тому, вполне интегрируема или нет соответствующая система дифференциальных уравнений (1.6). Интегрируемые кинематические связи — это по существу геометрические связи. Напротив, неинтегрируемые связи, вообще говоря, отличны от геометрических и к ним не сводятся...

Механическая система с неинтегрируемыми кинематическими связями, не сводящимися к геометрическим, называется неголономной системой. Неголономная система характеризуется тем, что для нее не существуют обобщенных координат, произвольным изменениям которых соответствовало бы движение системы, не нарушающее ее связей. Подчеркнем, что согласно этому определению наличие одной неинтегрируемой связи еще не означает неголономности системы, поскольку эта связь может оказаться интегрируемой в силу остальных уравнений связей" (стр. 10 - 12).

This fragment from [38] has been quoted here so pedantically since it, if one can put it like this, constitutes the theoretical basis of all the contents of the book. All that follows consists in routine manipulations based on these definitions of the authors. Therefore it is to the highest degree interesting to what extent this theoretical basis is reliable from a formal mathematically point of view.

The cold fact is, alas, that this reliability is equal to zero. There is not a bit of it. All these explanations are anything but mathematics. As a physical text, this exposition could yet be accepted. As a mathematical text its value is reduced to nought.

Those are no definitions at all. At the best, all these sentences are a try for an elucidation on an intuitive level. The formulas (1.3) - (1.6) are, from a mathematical point of view, completely meaningless. The authors of [38] are making fruitless efforts to give mathematical definitions of mathematically undefinable things.

As a matter of fact, the term constraint, geometrical as well as kinematical, is no mathematical notion, in the genuine sense of the word. Nor are the terms holonomic and non-holonomic constraint, holonomic and non-holonomic dynamical problem, holonomic and non-holonomic dynamics, at last. All these terms are, simply, only a *façon de parler*, abreviations, a concise — in other words, stenographic — manner to express telegraphically a complicated mathematical situation requiring many pages of formulas and symbols for its adequate formal mathematical description.

There we are. That is the size of it. Neither more, nor less.

As we have repeatedly underlined, one of the gravest deformity of Lagrange's mechanics, if there is such a thing, consists in its categorical refusal to construct a mathematically strict, logically irreproachable, definition of the fundamental for analytical dynamics notion of rigid body: such a definition requires plenty of geometry, and the Lagrangeanists are proud of having settled their accounts with geometry once for ever; instead they use the notion of a mechanical system which they do not define, so that it might mean anything and nothing in the same time. But *naturam expellas furca, tamen usque recurret*, as the saying is: *volens nolens*, a strict mathematical definition of the rigid body concept exists. And it is a definition that satisfies the most fastidious modern mathematical criteria for logical rigous. Now, it turns out that the intuitive idea of a geometrical constraint must be, and actually is, embedded in the fundament of this strict mathematical definition of the rigid body concept.

As to the special kinds of geometrical constraints the mechanical praxis has arrived at in the course of its time-honoured history, they are not defined by general unmeaning tittle-tattle, but are specified by means of painstaking numbering, as it is described in [23].

It turns out now that — as far as nothing more is hypothesized in the conditions of a dynamical rigid body problem — as a rule, this problem is mathematically indefinite one in the sense that its unknown quantities are more in number than the equations one has in his disposal for their determination.

Two, and two only, cases are familiar to the mechanicians, proposed by the centuries-old dynamical praxis at that, when the problem concerning the motion of a constrained rigid body under the action of given active forces and of unknown passive forces is mathematically a completely determined one — in other words, when the number of the unknown quantities in the dynamical problem is exactly equal to the number of the equations available for their determination.

The first one is the case of *smooth* geometrical constraints.

The second one is the case of *rolling without sliding and slipping*, or *in the presence of sliding and slipping of a completely determined kind*, of a surface on a surface, of a curve on a surface, of a surface on a curve, and of a curve on a curve.

Dynamical problems of the first kind are called *holonomic*. Dynamical problems of the second kind are called *non-holonomic*.

That part of analytical dynamics that is concerned with holonomic dynamical problems is called *holonomic dynamics*; that part of analytical dynamics that is concerned with non-holonomic dynamical problrems is called *non-holonomic dynamics*.

And that's that.

The conditions of rolling without sliding and slipping, or with sliding and slipping of a completely determined kind, are expressed mathematically by means of relations between the parameters of the rigid body, their velocities, and possibly the time. These namely relations the authors of [38] take cognisance of when they write down the equations (1.4) and (1.6). But these equations are corollaries, not definitions. To lay them in the groundwork of the edifice of non-holonomic dynamics is just the same as to state that the wind blows since the branches of the trees are swaying.

Some words more in connection with above citation from [38]. The authors write "мы мысленно или фактически сняли с системы ряд имеющихся у нее связей ... вновь наложить на систему снятые связи." If those are physical experiments with physical objects, then there is nothing to blame. Mathematically, however, these manipulations are wholly impermissible. According to a dynamical axioms, any constraint imposed on a rigid body generates a force (reaction of the constraint) acting on the body, the directrix of which passes through the point of contact of the body and the constraint. Now the authors of [38] do not know yet whether such a reaction exists at all: it exists if, and only if, the conditions of the dynamical problem under consideration are consistent — a question wholly ignored by the Lagrangeanists, as we have already seen above. In other words, these authors do not know at all whether their operations with "taking off" constraints and "putting them down again" are mathematically legitimate. Before they are perfectly sure that these operations are not illegal, they have no right to accomplish them in their minds.

Another important remark concerns again the "definition" (1.6) of [38]. It does not follow from the author's explanations that the case, when all the m equations (1.6) are non-holonomic, is excluded. In other words, the hypothesis, that all these m equations are non-holonomic, does not contradict the formulations of these authors. But this hypothesis is an error with immense consequences. Its immediate corollary is the existence of God.

In order to fix the ideas, let us, for the sake of brevity and simplicity, explain the essence of this criticism on the example of a single free mass-point with mass m and coordinates x, y, z with respect to an inertial Cartesian system of refernce $Oxyz$. Let the mass-point be subject to the action of no active force, and let the non-holonomic constraint

$$\dot{x} - e^{t}(1 + y) = 0 \tag{131}$$

be imposed on it. This formulation of the problem does not a whit contradict the formulations of the authors of [38]: $m = 1$ in (1.6), $n = 3$, $q_1 = x$, $q_2 = y$, $q_3 = z$,

$$f_1(\dot{q}_1, \dot{q}_2, \dot{q}_3; q_1, q_2, q_3; t) = \dot{q}_1 - e^{t}(1 + q_2). \tag{132}$$

If we suppose that the mass-point is under the action of no passive forces, then the kinetic energy theorem implies

$$\dot{x}^2 + \dot{y}^2 + \dot{z}^2 = C, \tag{133}$$

C denoting a constant of integration, and (131) contradicts (133). Hence the mass-point must be under the action of some passive force

$$\boldsymbol{R} = R_x \boldsymbol{i} + R_y \boldsymbol{j} + R_z \boldsymbol{k}, \tag{134}$$

the quantities R_x, R_y, R_z being unknown.

(Let us now stop for a while and think.

The first thing brought to our attention in this instant is the fact that, in their definition of non-holonomic constraints, the authors of [38] have not yet said a single word about the existence of forces of the kind (134). If one follows painstakingly through their exposition, then he will see that for the first time the term *reaction of a constraint* is used on p. 50:

"Освободим зассматриваемую систему от связей, которые заменим неизвестными, вообще говоря, силами реакции $\boldsymbol{R}_i$ этих связей."

Nota bene. First: on this third page of chapter II (p. 50) the authors speak of *reactions of constraints*, making no mention of the term *constraint* beforehand. Second: Immediately below *on p. 50* they *use* D'Alembert-Lagrange's equation which they *derive on p. 93*, as underlined above. Third: nowhere before they have said a word about the *possibility to substitute a reaction for a constraint.* Never mind.)

Let us take it for granted that our mass-point is acted on by the force (134). Then Newton's axiom of momentum implies

$$m\ddot{x} = R_x, \quad m\ddot{y} = R_y, \quad m\ddot{z} = R_z \tag{135}$$

and, together with (131), the first equation (135) implies

$$R_x = me^t(1 + y + \dot{y}). \tag{136}$$

As regards R_y and R_z, as well as the dependence of y and z of t, nothing reasonable can be said. In other words, we are faced with a mathematically completely indeterminate dynamical problem.

So far we have reasoning as Newtonians. Let us now think as Lagrangeans.

If the reaction (134) in non-ideal, then we, together with Lagrange, are helpless in front of the dynamical problem in question. But we do not like to be helpless. Let us, therefore, assume, that it is ideal, i. e.

$$\mathbf{R}d\mathbf{r} = 0. \tag{137}$$

The postulate of the ideal constraints (137) being satisfied, the kinetic energy theorem implies

$$dT = \mathbf{R}d\mathbf{r} = 0, \tag{138}$$

whence again (133) and again a contradiction with the non-holonomic constraint (131).

The existence of God is implied by non-ideal reactions (134). Indeed, in such a case nothing paradoxal is established in (135) and (136). Where did, however, the reaction (134) come from? From thin air? In our atheistic world this is impossible. But it exists according to p. 50 of the treatise [38]. Hence it is created by God. *Quod erat demonstrandum.*

If seriously, then any mechanician must obey as strictly as the ten commandments the following dynamical canon:

No non-holonomic constraint is possible in absence of holonomic ones.

Many other things can be said about the Lagrangean machanical tradition in holonomic and non-holonomic dynamics, but it is high time to bring our exposition to an end. Hoping that the reader has, though passing, obtained a rough idea of the present state of affairs in analytical mechanics where the influence of this tradition is still predominant, we shall conclude our exposition with a final critical remark on the application of the Lagrangean politics in non-holonomic dynamics.

While in the presence of geometrical constraints only imposed on rigids bodies the postulate of ideal constraints (30) is a question of a *definition*, i.e. it is included in the very conditions of the dynamical problem, in the presence of one at least non-holonomic constraint it is, on the contrary, a *question of proof.* In other words, the constructor of such a problem has yet lost the initiative on the question whether the constraints are ideal or non-ideal — the situation has slipped out of his hands. He is compelled to formulate the conditions of the dynamical problem, saying no word concerning the nature of the reactions of the constraints: are they ideal or not is a fact that must be established in the process of the problem solving.

Now the Lagrangeanists are faced with an impossible situation. Except for certain simple if important special non-holonomic problems (rolling of a rigid body on a surface *without* sliding and slipping), in the general case they simply can say nothing about the reactions. The reason for this paradoxal state of affairs is rooted in the very essence of their dynamical approach. Their pride consists in the expelling of the reactions out of their equations. But once the reactions expelled, the Lagrangeanists do not know whether they can apply their equations or not.

Euler's dynamical equations (91) - (96) solve this problem at once. The reactions take part in these equations. Now nothing remains, but to determine these reactions, to form the sum in the left-hand side of (30) and to establish whether it is zero or not. But why should a non-Lagrangeanist take all these pains and waste his time? He is not interested in the postulate of ideal constraints. The Eulerian dynamical equations (91) - (96) solve the non-holonomic problem in both cases, the Lagrangean in none: if the reactions are not ideal, they are inapplicable; while if they are ideal, the equations are applicable, but the problem solver is ignorant of the fact. Some dynamical "paradoxes" constructed on the basis of this situation are published in the article [23].

All that has been said up to now implies that the Eulerian dynamical axioms are the only possible way leading to the goal — a modern analytical dynamics. These axioms establish in analytical mechanics a dictatorial regime, but not more autocratic than, for instance, the principle of mathematical induction in arithmetic: they simply do not admit an alternative. As to the Lagrangean dynamical equations (100), they are, as we have seen by means of the fundamental identities (130) of Lagrangean formalism, simple corollaries from the Eulerian dynamical axioms — mere projections, as a matter of fact.

Analytical mechanics is beyond Lagrange's powers. The unconditionality of this ascertainment is not a whit diminished by the fact that the Lagrangean dynamical tradition keeps on as yet governing the minds of the mechanicians all over the world in this very moment: those are the agonies of a dying tendency, of a condemned *Weltanschauung.* If it is true that every Age has its Middle Ages, then analytical mechanics has not yet overlived them. Life is, however, stronger than anything. He, who sees the verity, should be calm, as regards Euler in mechanics: *Nolite flere, non est mortuus, sed dormit.*

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