

I. C h o b a n o v

Si licet parva componere magnis

*Now that history of science has become a recognized university discipline, supported by chairs, meetings, journals, societies, boards, committees, and subsidy by the taxpayers - in a word, professionalized ... - most who promote the independence of this new field are unable to solve a calculus problem, let alone follow a paper of hydrodynamics a hundred years old. Science, accordingly, is cut down to the size of its professed historians, and a competent scientist who dares raise his voice in the history of science is dismissed as an intruding amateur ... If a page from the middle of [Principia] were copied on a typewriter and handed to a Professor of the History of Science who has just detailed Newton's magnificent discoveries to his class, he might well fail to recognize it coming from the pen of his hero ... such is the universal ignorance in the true history of mechanics.
Clifford Truesdell: Essays in the History of Mechanics*

The immediate cause for this paper of mine is article [1], published some years ago. Much to my regrets, it has escaped my attention about that time. Meanwhile, I have been asked by a Bulgarian mathematical journal to translate this article [2]. This has been the occasion to take a first-hand look at [1]. It aroused my curiosity in some way.

As its title implies, article [1] is concerned with some personal relations between Leonhard Euler and Jean Le Rond D'Alembert. I must confess that it contains some points of view entirely new for me.

According to its author, "D'Alembert ... often provoked Euler's unfriendly reactions. A spirit of rivalry naturally arose between the two

giants ... In his private correspondence also D'Alembert often challenged Euler's results. All this did not win him the sympathy of Euler or of others ... Euler felt that D'Alembert not only raised objections to the work of others, but was very touchy when he himself was criticized."

One of the reasons for these controversies the author of [1] sees in the rather different scientific *natures* of his heroes: "Euler's attitude to the physical world was not always very realistic. The world, he believed, ought to be as his mathematics revealed it to be. If it turned to be different, he was sometimes inclined to suspect the observational findings than his mathematics ... he applied it profusely on all problems, and sometimes arrived at conclusions that were not exactly reflections of the physical world."

Moreover, "Euler ... was not always very concerned about the "metaphysical foundations" of mathematics as his rival D'Alembert was. It is true that D'Alembert was sometimes annoyed by the fact that his criticisms were not always taken seriously by the master mathematician of the century. And he also felt that Euler often exploited mathematical formalisms without inquiring into their metaphysical foundations."

Besides, "there are numerous instances where Euler's own investigations followed D'Alembert's initiation of the subject ... Euler often simply ignored in his writings what others have done ... he was little inclined to recognize the contributions of lesser minds to the fields in which he worked ... in many of Euler's papers one finds a glaring absence of names of legitimate predecessors, or sometimes these names are mentioned as an afterthought."

Meanwhile, "Daniel Bernoulli, another outstanding genius of the age, was also involved in some of the controversies and misunderstandings between Euler and D'Alembert".

At that, "the animosity between Euler and D'Alembert was for the most part unidirectional ... With all that, [D'Alembert] had only the greatest respect for Euler the mathematician. On the other hand, Euler's treatment of D'Alembert ranged from genuine appreciation to sarcastic compliment and downright scorn."

And so on, and so forth, et cetera.

Confiteor: when I for the first time was faced with these texts from [1], I could not help being reminded of Turgenev's words *deus loci communis*. This starting impression became later a certitude: those are arbitrary allegations. Thereby, I by no means take a side in an argument whatever: I only state that - even if true - they are uncorroborated. They are only said, and that's all there is to it.

The leitmotif of [1] is the author's finding that "historians of science have not been very sympathetic to D'Alembert", and the contents of the article consists in this theme and variations: "Even if profound insights and germinal ideas were sometimes credited to D'Alembert, he is usually referred to as not only creatively inferior to Euler, but even unworthy of the fame and recognition that he received...The comments of eminent physicists and mathematicians have generally been very favourable

to D'Alembert and his work, but those of historians have not always been so."

The author of [1] finds this situation unjust. Himself a "Professor of Physics and Humanities", he gives in his article the utterance of his profound indignation at this unfairness, stating that "the only expression of contempt came from those who were involved with D'Alembert in controversies over priority and /or other personal matters".

But the "historian of science" who certainly makes his blood boil, is Truesdell, "one of D'Alembert's pitiless critics ... the dean of all D'Alembert's despisers ... This eminent scholar and historian, having edited parts of Euler's complete works, seems to have inherited his idol's antipathies for D'Alembert. Indeed, they are so magnified in his writings that it is doubtful if even Euler would concur with him on all points".

Exempli gratia, he quotes the following statements that Truesdell has taken the liberty to write:

"It is difficult to account for the high reputation gained by D'Alembert despite the contempt he earned from almost all of the few who in his own day were competent to judge the mathematical researches in the subjects he studied ... in attempting to connect physical experience with mathematics, he heaped folly on folly ... one searches for the little solid matter as a sparrow pecks out a few nutritious seeds from a dungheap - a task not altogether savory" [3].

It is much to my regret that I have to present here such vast excerpts from [1]; there are, however, certain reasons for doing so.

To begin with, they are symptomatic and instructive with a view to the attitude of the author of the article towards the *dramatis personae* in his narrative and to the thesis he advocates. Naturally enough, I myself could never perform his *plaidoirie* more successful than he himself.

In the second place, there are some points in these fragments I shall refer to in the sequel.

Last, not least, in this respect I am only following his own steps. As a matter of fact, the exposition of [1] is based on the principles *relata refero* and *inductio per enumerationem simplicem*: the content of the article consists mainly of quotations amalgamated by commentaries. It lists and cites appraisals, assessments, estimates, evaluations, judgements, opinions, etceteras on D'Alembert's works and deeds, due to many persons, his contemporaries or ours (Todhunter, Cajory, Ravetz, Truesdell, Condorcet, Lagrange, Merz, Mach, Euler, Daniel Bernoulli, Maupertuis, Voltaire, Lambert, Laplace, Gauss, Sylvester, Cauchy, Bertrand, Cantor, and Comte, all these being listed in the order they appear on the stage of [1]), but it gives no evidence of having ever ventured "to profane the shrine" of its hero - the treatise [4] in the first place - "by opening its covers to peer inside", if I may be permitted to use the words of this same devil we just talked about [5, p.

138]. In this respect the approach of [1] to the theme announced in its title is very much alike Lord Dorwin's "scientific method", so picturesquely described by Isaac Asimov in his *Foundation Trilogy* [6, pp. 58-60, 65].

Of course, any quotation reproduced in [1] is pedantically correct.

It is true, for instance, that "Lagrange's *Mécanique Analytique* gave him much credit for his contributions to rational mechanics. Indeed, this was the starting point of the universal recognition according to the so-called D'Alembert's principle in subsequent years." It is also true that "when Gauss formulated his principle of mechanics he started by referring to D'Alembert's principle, stating how it reduces dynamics to statics."

Quite true is also that "August Comte confessed that he had constructed his sociological system by taking inspiration from D'Alembert's principle".

All this is true. I mean, it is true that they all *said such things*. There is a motto of Marischal College, Aberdeen. It reads: *They say, what they say, let them say*.

But is it true *what they say*? Are the *things themselves true*? Does *their saying so* make them true? What matters more: *who* said something or *what* did he say?

In §10 of *Section première. Sur les différents principes de dynamique* of *Second partie. La dynamique* of [7] Lagrange has written verbatim: "Le *Traité de Dynamique* de D'Alembert, qui parut en 1743, mit fin à ces espèces de déficit, en offrant une méthode directe et générale pour résoudre, ou du moins pour mettre en équations tous les problèmes de Dynamique que l'on peut imaginer. Cette méthode réduit toutes les lois du mouvement des corps à celles de leurs équilibre et ramène ainsi la Dynamique à la Statique ... il était réservé à d'Alembert d'envisager ce principe d'une manière générale et de lui donner toute la simplicité et la fécondité dont il pouvait être susceptible... Tel est le principe que d'Alembert a donné dans son *Traité de Dynamique* et dont il a fait un heureux usage dans plusieurs problèmes..."

Now is all that true? Is it true that D'Alembert's *Traité* put an end to all the defiances that the naissance of an entirely new science - rigid body dynamics - bid to all mathematicians since Newton? Is it true that it offered indeed a direct and general method for the solution of all the problems of dynamics one may imagine, even merely reducing them to mathematical equations? Is not Lagrange's dynamical imagination a deplorably poor one? Is there no problem of rigid body dynamics slipping like a snake through the fingers of "le principe de d'Alembert" and bringing, in such a way, all those superlative claims of Lagrange to naught? Moreover, is it possible at all to erect the whole edifice of rigid body dynamics on the base of a single dynamical principle? At last, *terribile dictu*: is there at all any dynamical problem concerning rigid bodies that could be solved by the aid of "le principe de d'Alembert"? Do not all these broad advertisements of the high qualities of D'Alembert's

merchandises belong to the folklore of the Lagrangean dynamical tradition?

Is it true that it is possible to reduce all the laws of motion of the rigid bodies to those of their equilibria, bringing in such a manner dynamics under the sway of statics? If yes, then what a fool the ancient Syracusan *mechanicus* has been not to see such a simple thing? Gauss believed it possible: does however, the faith of a man, let even Gauss, turn a credo into a truth? Euler's *Letters à une Princesse Allmende* swarm with what Truesdell called "folly on folly" and they are now of no but historical interest; the author of [1] cites delightedly Lagrange's equitable remark apropos those *Lettres* that "for the sake of his honour he should not have published" such things. Has not, however, the idea crossed his mind that the same remark, with equal reason at that, may be applied to Gauss' paper [8] as well?

Is it true that a sociological system can be inspired by a mechanical principle, let even dynamical? To what extent can at all a philosopher understand the meaning of such a principle? Or penetrate the contents of a mathematical *Traité*? Philosophers not infrequently philosophize on philosophy of mathematics: what is likely mathematicians to say about the mathematics of philosophers? Is not Comte's intellectual confession merely an old man's coquetry, a reverence of courtesy in front of Dame Mathematics? Paying homage to mathematical fashion on the part of non-mathematicians in order to give additional authority and a polish to the otherwise inevitably vague mental speculations is a phenomenon not infrequently observed and taken at its face value only by non-mathematicians; upon mathematicians' faces it calls forth only condescending smiles. Are Comte's words not only floral tributes to the mathematical vogue, comparable to the proverbial projects *Principia Medicinae Theoreticae Mathematica* of Cheyne and *Theologiae Christianae Principia Mathematica* of Craig immediately after the publication of Newton's much talked about *Principia* [9]?

If I may be permitted to introduce myself, then I would lay a special emphasis upon the following three circumstances.

First, I am a mathematician by training and a mechanician by choice.

Second, I have never been "involved with D'Alembert in contraversies over priority and/or other personal matters".

And third, I don't mind searching "for the little solid matter, as a sparrow pecks out a few nutritious seeds from a dungheap".

Under the label "mechanician" I imply that I have worked almost all my professional life (fourty years, as a matter of fact) mostly in analitical mechanics. That is why, not only are the names of Euler, D'Alembert, and Lagrange not alian to me, but we all are in some way *eiusdem farinae*. Therefore, *salva venia* I say that I have put all the above questions by no means platonesquely, neither jocularly, nor defiantly. I mean what I say. The danger that the genuine answers to all

those interrogations will turn out to be quite antipodal to the corresponding persuasions of Lagrange, Gauss, and Comte is in no way a potential one: it is as actual as death itself.

History of mathematics proposes many cases of scientific if not entirely impersonal rivalries. Classical instances are the controversies Leibniz - Newton, Jacob - Johann as well as Johann - Daniel Bernoulli, Klein - Poincare, Cantor - Kronecker, Hilbert - Brouwer, etc. Besides purely scientific, all those clashes involve knotty personal, psychological, as well as ethical problems.

Now I declare that the rivalry topic is of little if any at all interest to me. Neither are the moral and philosophical ones. I am neither priest, nor parson; neither rabbi, nor mufti. There are some nuances, though. That is why I shall spend some words in this connection, once for ever settling all accounts with those themes.

The author of [1] is undisguisedly aiming at a scientific rehabilitation of D'Alembert and at a redemption of his physical and mathematical reputation. This is a praiseworthy endeavour. What is not so praiseworthy is that he is standing upon a rather slippery ground. Instead of a sound professional analysis of his own about certain D'Alembert's scientific performances and achievements, he simply quotes and sites another's appraisals and assessments; at the same time he has chosen a spectacular line of conduct - a strong wine for young heads by reason of its sensationalism: he lists unfavourable characteristics of Euler's, namely his "neglect of others", his carelessness in mathematical rigour, his disregard for the "metaphysical foundations" of mathematics, his "obstinacy", his philosophical zeal, delusions, and flaws, his religious zest and fanaticism. (One is under the impression that the only misdeed against D'Alembert Euler has not been charged with in [1] is the former's illicit birth.) All that may be so. One must, however, not forget that one's enemy's defeat is not necessarily one's own victory - that Euler's demerits are by no means D'Alembert's merits.

Being a knowing blade, I am not anxious about myself's peace of mind. But most of the readers of *Intelligencer* have never before been, nor shall be afterwards, in a professional contact with the heroes of [1]. Now I am asking myself, what impression will they be left with as regards to the image of Euler, on one hand, and D'Alembert, on the other, gained through the reading of the article in question?

For these images are twisted *ad nec plus ultra*.

There is another point too. One cannot but be in full agree with the conclusion that "no matter what historians say and how they judge [D'Alembert], neither the world of science, nor the history of ideas can ignore the thoughts and contributions of this great and prolific savant of the 18th century". Neither can one raise serious objections against the final chord of [1]: "The clashes and controversies between him and Euler are merely symbolic of the perennial conflicts at the intellectual and other levels of opposing forces that constantly guide history and propel the world." At the same time, reading that "in this year of the

200th death anniversary of D'Alembert it will be of some interest to look a little more closely into D'Alembert's standing vis-à-vis Euler, and to see how far some of the damaging remarks against D'Alembert, both by his contemporaries and by later historians, are justified", one is really at a loss whether the price of such a salvation of D'Alembert's scientific reputation at the expense of Euler's scientific and purely human name is not too high?

I am not quite sure what the real meaning of the word "rivalry" is and what exactly significance does the author of [1] put into this term. At any rate, if one accepts it in the usual, ordinary and common sense - as a contest, competition, emulation, race, et cetera - then the mere idea that such a thing could exist between Euler and D'Alembert is as fantastically ridiculous, as for instance the picture of Charly Chaplin as a match of Joe Lewis on a boxing ring, or socialistic emulation versus capitalistic competition. It is not only that at least two persons are needed for any rivalry: rivalry is simply impossible but *inter pares*. Is such the case here?

Absit invidia verbo, but hitting the bull's eye one may state that - compared to Euler - D'Alembert belongs to *dii minorum gentium*. That is why the "vis-à-vis" in the above excerpt from [1] reminds the paraphrase *si tanta licet componere magnis* of a Vergilius' versus, as well as the Latin saw *quod licet Jovi, non licet bovi*.

As a formula of the Roman civil law reads, *impossibilia nulla est obligatio*. It is not D'Alembert's fault that he can not be a rival to Euler. Nobody can. Vis-à-vis Euler is a backbreaking task. Like Archimedes and Chomo-lungma, Euler is beyond rivalry.

The "D'Alembert-Euler Rivalry" is a tale of a tube, fabricated by a popular sketch, bearing all the characteristic features of a scientific folklore and paying tribute to the superstitions of a traditional faith, of scientific legends, mathematical myths and mechanical sages. It even multiplies them, creating new ones - those of would be rivalries.

It is melodramatic to depict Euler's unidirectional personal antipathies on the background of D'Alembert's "greatest respect and admiration". Let it be noted *entre parenthèses* that anybody having even the slightest idea of the aggressive and offhand way D'Alembert stated his stands would by no means ever believe there were many things in this *mundus hic quam optimus* he had "respect and admiration" for. Maybe it is only a *façon de parler* in the main - a matter of temperament, character, an outward expression of inward disposition, an exhaust valve of inside tension. May be. However, *quod non est pauculum dicere* at that: *quasi tulerit Gracchos de seditione querentis*?

Euler is the least eligible person in the world in the capacity of D'Alembert's scapegoat, and this choice of the author of [1] kills his hero with kindness. It reminds of a man who is jumping into a river in order not to get wet with the rain. In fact, things are presented

entirely topsyturvy in [1]. As Juvenalis noted, *dat veniam corvis, vexat censura columbas*: it is Euler, not D'Alembert, whose mechanical and physical "work is undervalued in the usual vague historical books and attributions" [5, p.106]. This is, however, a topic we shall not enter into details here.

Est modus in rebus. Article [1] is written in a breach of the admonitions *versate diu, quid ferre recusent, quid valeant umeri* and *sumite materiam vestris, qui scribitis, eaquam viribus* of Horatius. There is *plus sonat, quam valet* in it, by Seneca's words, *sunt verba et voces, praetereque nihil*, by Horatius'.

And yet, there is nothing completely useless in this God's Earth: *malum nullum est sine aliquo bono*, as Plinius Maior used to say; *nulum sine auctoramento malum est*, as Seneca preached.

If article [1] has not been published, then the present paper certainly would not be written. Now isn't *that* quite something?

This *cum grano salis*, of course. If in sober earnest, then I should say that my acquaintance with [1] really and truly provoked me, catalyzing the execution of a long-standing intention. I have known a lot about D'Alembert a long time ago, but I could not force myself to introduce a system of this knowledge of mine. All I came to know was peripheral, if that's the word: those were things I had learned accidentally, by chance. Now, thinking over [1] I at once realized that there is a dozen of questions crying for answers. I list some of them right below, without an order or system - just as they came into my mind:

1. Why - in spite of my though amateur interest in history of mechanics - have I never come to know D'Alembert more closely?
2. Why - in spite of all the fame of the work - have I never come to read the *Traité de Dynamique* with the due attention, satisfying my curiosity with a happy-go-lucky come-and-go looking through its pages?
3. How much, and what kind of, mechanics can be found in this dynamical treatise?
4. Did D'Alembert redeem his promise given in the title of his *Traité*?
5. Where are the mathematical theorems D'Alembert ever proved?
6. Where is his genuine place in the real history of mechanics?
7. What do I know about the development of mechanics between *Principia* and *Traité de Dynamique*?
8. Does there exist a serious *investigatio causarum* for the *barbam-video-sed-philosophum-non-video* attitude to D'Alembert on the part of some authors?
9. Is Truesdell right, or is he wrong, stating the things that roused the indignation of the author of article [1]?
10. What do I know about the professional contacts between Euler and D'Alembert?
11. Did Lagrange indeed take something fecund out of D'Alembert's treatise, or is his *révérence* to it only a scientific pose?
12. What does Twentieth Century Dynamics say concerning "le Principe

de d'Alembert"?

13. (Not a few) etceteras?

14. In other words, I have been faced with the necessity of a thorough-going stock-taking of my knowledge about D'Alembert's mathematical-mechanical performances and attainments.

Naturally, nobody should cherish any illusion that one could answer all those and many other similar questions in a few single pages of a paper like this. One cannot do it not only because it is technically impossible. I, on my part, cannot succeed since I do not know the answers: like Cicero, *nec me pudet, ut istos, fateri nescire quod nesciam*. I not only doubt there would be a man alive today who could - I certainly know there is not such a man. Any other viewpoint is unprofessional, incompetent, and ignorant - in a word, light-minded. All those questions are rather hard nuts to crack. Any of them requires a long-drawn-out, versatile, and profound preparation and research to be answered. Some of them are most likely to be left unanswered for a long time ahead. At any rate, I have the intention of doing my best in this direction, and I sincerely hope that *Theoretical and Applied Mechanics* will kindly give me a chance to make the readers of the journal acquainted with some of the results of my efforts along these lines.

It is high time to bring my exposition to an end - at least by virtue of the editorial time-limit. I have tried to expose two entirely different points of view on the subject, and if I have failed, then I should have only myself to blame. Nevertheless, some final words are utterly important.

Ich sieze D'Alembert und Lagrange, aber ich duze Euler. Therefore I must reject in the most emphatic manner the accusation in [1] that "Euler ... was not always very concerned about the "metaphysical foundations" of mathematics". My stand is based, on one hand, on the *ipsissima verba* of the author of the article that "Euler produced formulas and theorems in ... lavish abundance and with ... easy success"; and on the other hand, on Lord Kelvin's most remarkable finding that *mathematics is the only good metaphysics*.

I am not sure also to what extent the epithets "pitiless critic" and "dean of D'Alembert's dispisers", ascribed to Truesdell in [1], are justifiable by the cold facts. In the synopsis of his booklet [10] this "eminent scholar and historian", as Raman qualifies him, is described as belonging "to no philosophic or scientific school; he considers himself an outsider and a sceptic regarding science, philosophy and society. Various reviewers have praised his use of a literary idiom in scientific writings". I have got the impression that this prolific, perspicacious, and uncompromising erudite, whose pen is exotic, eccentric, and even extravagant, and whose head is non traditional, non-orthodoxal, and sometimes even heretical, does not enjoy the sympathies of many historians of science and especially of inventors of scientific

fairy-tales. How could he, indeed? That's where the shoe pinches:

"Not only the public press but also the précis of paedagogues has taught us to gulp superlatives as daily narcotics. The professor who forgets to class, rate and rank his subject as the first and finest something will fail to find it mentioned in the students' terminal examinations. The busy modern calls for culture in predigested quintessence pills, packed in abridged paperbacks, explained by folksy prefaces in pellet-paragraphs of sugared baby-talk, lullabies to smugness. Suggestion to a general audience that historical facts must precede if not replace historical enthusiasm may expect only oblivion, the palm of dullness" [5, p.3].

All this sounds rather familiar. Oh yes, of course! Hear:

Mundus vult decipi, ergo decipiatur.

Vulgus vult stupeo, ergo stupeat.

In my opinion Truesdell is apt to overrate D'Alembert rather than underrate him. In his Essays, published almost twenty years before [1] and by no means unknown to the author of this article, D'Alembert's name is mentioned on at least 40 pages. Therein important priorities in rational mechanics are credited to D'Alembert.

So, for instance, speaking of the ideology in the *Traité de Dynamique*, Truesdell writes:

"The clearest impression, of course in part a summary of practice, was given by D'Alembert in 1743:

1. Rational mechanics, like geometry, must be based upon axioms which are obviously true.

2. Further truth in mechanics follows by mathematical proof.

He aimed ... to envisage in the most abstract and simple manner what can be the particular object of this science; to suppose nothing, to admit nothing in this object, beyond the properties which the science being treated supposes there" [5, p.94].

Vide infra: "Let it not be thought, however, that the embracing and elegant generality of mechanics came from philosophical speculations. Mechanics is a science of experience; for the theorist, physical experience is balanced against the experience of earlier theories for the phenomena. The history of rational mechanics is neither experimental, nor philosophical; it is *mathematical*; it is a history of *special problems*, concrete examples for the solution of which *new principles and methods* had to be created. But the solution of a special problem was never left to stand alone; since there was only one true mechanics, the special case served not as an end in itself but as the *guide to the right conception*. The order and design of the structure were as valuable as the soundness of its members. In 1743 D'Alembert wrote:

Up to the present, ... more concern has been given to enlarging the building than to illuminating the entrance, to raising it higher than to giving the proper strength to its foundations.

In this work I have set myself the double object of pushing back the bounds of the science of mechanics and of smoothing the way to it, and my

principal aim has been to achieve in some measure the one of these objects by the other; that is, not only to derive the principles of mechanics from the clearest ideas but also to apply them to new uses; to show all at once the uselessness of several principles used heretofore in mechanics and the advantage which may be drawn ... from others; in a word, to extend the principles by reducing them" [5, p. 96].

What I am asking now is: can such a reviewer of D'Alembert's mechanical philosophy be qualified as his "merciless critic and despiser"? It is quite a different problem whether D'Alembert succeeded in putting his ideas into flesh and blood: as Tibullus says, *est nobis voluisse satis*; as Ovidius says, *ut desint vires, tamen est laudanda voluntas*; as Propertius says, *in magnis et voluisse sat est*.

Naturally, *si non-non*. Truesdell knows quite well the dictum *reddite quae sunt Caesaris Caesari et quae sunt Dei Deo*:

"Contrary to the usual claims, neither did D'Alembert reduce dynamics to statics, nor did he, here [in the *Traité de Dynamique*] or anywhere, propose either of the two forms of the law of dynamics now usually called D'Alembert's principle" [5, p. 113].

This is a point permitting no shilly-shallyings, no interpretations, and no qualifications - a place for *hic Rhodus, hic salta*: if one claims to disprove Truesdell's statement, then one must point at the work of D'Alembert and at the page of this work where any of the two forms of "D'Alembert's principle" has been printed in black and white. Can Raman do that? Not only he - nobody can.

Dynamics cannot be reduced to statics. The superstitious belief that it can belongs to the most damaging dogmata of D'Alembert-Lagrange's dynamical religion. It cannot since equilibrium is invariant with respect to the systems of reference and motion is not.

A rigid body is, by definition, in equilibrium under the action of a given system of forces if, and only if, this system of forces is equivalent to zero (i. e. its basis and its moment are zeroes), and this latter condition does not depend on systems of reference - it is invariant with respect to an arbitrary replacement of one system of reference by another. On the contrary - this is a *toto caelo* difference between statics and dynamics - the motion of a rigid body depends on the system of reference chosen: the equations of motion are true for a certain class of systems of reference (*inertial systems*), while they are entirely wrong for all other systems of reference (*non inertial systems*). God Almighty and Satan himself cannot alter this *Status Quo*, which pertains to one of the greatest discoveries of that Magnificat of Human Sublimity - Newtonian-Eulerian Dynamics.

For the lack of room I cannot reproduce here all mechanical discoveries credited to D'Alembert by Truesdell in his *Essays*, and it is really a pity since I would do it with the greatest pleasure. There is a passage in [5], however, that I can pass by no means over in silence.

Truesdell, namely, gives a *currente calamo* sketch of Lagrange's mechanical scientific image where D'Alembert's influence upon him is looked upon in the most infavourable manner:

"1. There was a little new in the *Mécanique 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 in mechanics derive from his earliest period, when he was studying Euler's papers and *had not yet fallen under the personal influence of D'Alembert*" (pp. 246-248, my italics).

I am stressing on these things since it is Lagrange namely who created D'Alembert's fame in analytical mechanics through his *Mécanique Analitique*, giving him the credit for his "principe de dynamique". *Sit venia verbo*, but it is a deep inner conviction of mine that, as regards to analytical dynamics at least, not many persons would ever mention today D'Alembert's name at all if there were not Lagrange's *Mécanique Analitique*.

I shall drive now the last nail in the lid of my narrative. It may be entitled *D'Alembert-Lagrange's proof for the existence of God*. As it is well known, in contrast to Newton and Euler, whose religious bigotry is fabulous, D'Alembert and Lagrange were manifested atheists. Now I shall display that they were religious atheists.

There is such a passage in *Essays*: "At the end of the 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 *Mécanique Analitique* ... the presentation of mechanics [in it] is strictly algebraic, with no explanation of concepts, no illustration either by diagrams or by developed examples, and no attempt to justify any limit process by rigorous mathematics", (pp. 134, 173). Being entirely concordant with these inferences, I shall supplement them with two important addenda:

First, the birth-date of Lagrangean formalism is not 1788, but 1743, and its birth-place is not *Mécanique Analitique*, but *Traité de Dynamique*.

Second, one of the most typical features of this formalism is its complete disregard of existence problems.

Although D'Alembert's place in the written history of mechanics is still rather indeterminate, one may definitely state that he pertains to the prehistory of rigid body dynamics. Throughout the *Traité de Dynamique* his term "corps" must be perceived as meaning "mass-point", in order that his statements make sense or be true, if at all. To all appearances, the idea that a rigid body may be regarded as an "ensemble de corpuscules" is his invention that he bequeathed to his spiritual heir Lagrange. In his

article [11] the latter wrote: "I consider the proposed body as an assembly of corpuscles or mass-points joined together in such a way as always to conserve their mutual distances." The idea that such a *monstrum horrendum, informe, ingens* may not exist at all did not flash through his mind. Neither did it come in D'Alembert's head. *Incredibile dictu*, nobody put this question in the course of two clear centuries.

The importance of the existence of a mathematical object with prescribed properties is too plain *Banalität* to be discussed here. Its logical negation - the non-existence - is the keystone of a most important mathematical method, namely the *reductio ad absurdum*. The existence problem has been the *lapis offensionis et petra scandali* for cardinal mathematical theories. The whole of Euclidean geometry is based on the hypothesis that there exists a triangle the sum of whose angles equals two right ones. Now what about a system of mass-points with constant mutual distances? Does such a thing exist?

D'Alembert and Lagrange say *Yes* as uproariously as if one is hearing the *vox Stentorae*.

If we believe them, then we unavoidably arrive at the conclusion that there exists at least one God. Or an Old Harry. Let me show that.

First of all, let me note that - since Lagrange says nothing about the number of the corpuscles - his hypothesis, if true, must hold for two mass-points too. For the sake of brevity - and with this end in view only - I shall consider this most simple case namely.

And so, having a complete confidence in D'Alembert and Lagrange, I suppose that P_1 and P_2 are two mass-points with masses m_1 and m_2 and with radius-vectors r_1 and r_2 respectively, the mutual distance between which remains the same in the course of the time t . In other words,

$$(1) \quad \frac{d}{dt} \left(r_1 - r_2 \right)^2 = 0 \quad (\forall t \in R),$$

R denoting the field of all real numbers. If v_1 and v_2 denote the velocities of P_1 and P_2 respectively, then (1) is equivalent to

$$(2) \quad \left(r_1 - r_2 \right) \left(v_1 - v_2 \right) = 0 \quad (\forall t \in R).$$

Old man Newton, in whom D'Alembert and Lagrange trust, preaches that the mechanical behaviour of mass-points is predetermined by the forces acting on them, only by those forces, and by nothing save those forces. Moreover, he sermonizes that if F_1 and F_2 are the forces acting on P_1 and P_2 respectively, then

$$(3) \quad \frac{d}{dt} \left(m_\nu v_\nu \right) = F_\nu \quad (\nu = 1, 2; \forall t \in R) ,$$

provided all derivatives are taken with respect to an inertial system of reference. Hence, according to Newton's dynamical credo, condition (2) is equivalent to the hypothesis that there exist wholly determined forces F_1 and F_2 , acting on P_1 and P_2 respectively according to the laws (3) and such that (2) is realized at any moment of time t .

Accordingly, (2) must hold good in the initial moment $t=0$. Let by definition

$$(4) \quad r_{\nu 0} = r_\nu(0), \quad v_{\nu 0} = v_\nu(0) \quad (\nu = 1, 2)$$

be the initial conditions for the motion of P_1 and P_2 . Then (2) implies

$$(5) \quad (r_{10} - r_{20})(v_{10} - v_{20}) = 0 .$$

But the conclusion (5) is a complete idiocy or, using the decorous euphemistic expression, absurdity. Indeed, (5) is a restriction imposed on the initial conditions (4) of our dynamical problem concerning two free mass-points. This restriction is implied by the hypothesis that there exist such forces F_1 and F_2 , acting on P_1 and P_2 respectively, that the distance $P_1 P_2$ remains constant for any moment of the time t . In other words, the forces F_1 and F_2 impose a restriction on the initial conditions of our dynamical problem. However, a fundamental dynamical principle reads: the initial conditions of a dynamical problem are absolutely independent of the forces. This is a particular case of a more general principle of the theory of differential equations according to which the initial conditions are independent of the particular kind of equations.

In this way, we arrive at an absurdity. It is due to the hypothesis that there exist such forces F_1 and F_2 that et cetera. In other words, such forces do not exist.

But the points P_1 and P_2 remain, by Lagrange's mechanical ideas, equidistant. Since this cannot be realized by forces, the only possibility that remains is that this miracle is due to providetial intervention. *Quod erat demonstrandum.*

Some desperate sceptics may say that there is another possibility, too - that Lagrange's mechanical ideas are not worth a bean. This possibility is not to be neglected, either.

Lo and behold! There is one of the things Truesdell had in mind when he wrote that "in attempting to connect physical experience with

mathematics [D'Alembert] heaped folly on folly". There are many other things of this kind, too. Apropos of Lagrange's article [11] Euler says something similar, though by means of considerably milder expressions. With a concealed, even if in no way lesser, irony he writes: "But when I tried with greatest avidity to follow in detail his extremely profound thoughts, truly I could not get myself to go through all these 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". Blindness! Euler wrote this in the introduction of his article [12] worthy of the exclamation *cedite Romani scriptores, cedite Grai, nescioquid majus nascitur Iliade*: in it Euler succeeded at last, after tremendous efforts in the course of half a century, to formulate the two basic laws of rational mechanics - the dynamical axioms of momentum and of moment of momentum. This is, however, another theme I shall not enter into here.

The pious fraud *de mortuis aut bene, aut nihil* of Diogenes Laertius has been, still by his contemporaries, rejected and replaced by the more realistic *Losungen de mortuis aut bene, aut male* and *de mortuis veritas*. I confess that if I am faced with a strict requirement for a categorical appraisals of D'Alembert's role in the history of mechanics, then I shall get driven in a tight corner. This is a problem with many unknowns yet. Doubtless, he was one of the pioneers. Any human dead breeds problems: if there is *vae victis*, there is *vae victoribus* too. D'Alembert could, anyhow, with a great deal of justification, claim: *feci quod potui, faciant meliora potentes*.

Euler did.

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