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ORDER OF NATURE AND ORDERS OF SCIENCE

On the Mathematical Philosophy of Nature and its Changing Concepts of Science from Newton and Euler to Lagrange and Kant

> [...] to derive two or three general Principles of Motion from Phaenomena, and afterwards to tell us how the Properties of all corporeal Things follow from those manifest Principles, would be a very great step in Philosophy, though the Causes of those Principles were not yet discover'd.
>
> (Isaac Newton, *Opticks*, Qu. 31)
>
> Les principes de la Mécanique sont déja si solidement établis, qu'on auroit grand tort, si l'on vouloit encore douter de leur vérité. Quand même on ne seroit pas en état de les démontrer par les principes généraux de la Métaphysique, le merveilleux accord de toutes les conclusions qu'on en tire par le moyen du calcul, avec tous les mouvemens des corps [...] seroit suffisant pour mettre leur vérité hors de doute.
>
> (Leonhard Euler, *Réflexions sur l'espace et le tems*, § 1)
>
> Je me suis proposé de réduire la théorie de [Méchanique], & l'art de résoudre les problêmes qui s'y rapportent, à des formules générales, dont le simple développement donne toutes les équations nécessaires pour la solution de chaque problême.
>
> (Joseph Louis Lagrange, *Méchanique Analitique*, Avertissement)
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> So konnten also jene mathematische Physiker metaphysischer Prinzipien gar nicht entbehren [...]. Darüber aber bloß empirische Grundsätze gelten zu lassen, hielten sie mit Recht der apodiktischen Gewißheit, die sie ihren Naturgesetzen geben wollten, gar nicht gemäß, daher sie solche lieber postulierten, ohne nach ihren Quellen a priori zu forschen.
>
> (Immanuel Kant, *Metaphysische Anfangsgründe der Naturwissenschaft*, Vorrede)

1. PRELIMINARIES: THREE POINTS OF DEPARTURE AND ONE AIM

The role of mathematics in eighteenth-century science and philosophy of science can hardly be overestimated, though it was and is frequently misunderstood. From today's point of view, one might be tempted to say that philosophers and scientists in the seventeenth and even more in the eighteenth century became aware of the importance of mathematics as a means of 'representing' physical phenomena or as an 'instrument' of deductive explanation and prediction. According to this view, the rise of mathematical physics is a peripheral aspect of the new experimental sciences, and the mathematical part of physics is a methodologically directed, constructive enterprise that is somehow 'parasitical' with respect to experimental and

W. Lefèvre (ed.), Between Leibniz, Newton, and Kant, 61-92

observational data. But such modernising outcomes of logical empiricism are missing the central point, i.e., the 'mathematical nature of nature' according to mechanical philosophy. I will start with some general considerations about mathematics under the premise of mechanism before coming to the aim of my paper.

1.1. 'Semantical ladenness' of mathematics

On the premise of mechanism, the primary aim of natural philosophy was the determination of the *motion* of material particles under different physical conditions and the science of motion was the 'hard core' of natural philosophy. Motion itself being regarded as a genuine mathematical concept, natural philosophy had to be not only an experimental, but also a *mathematical* science. Taking this idea seriously, the attribute 'mathematical' should be understood not as 'mathematics applied to science' but rather as 'science, having essentially to do with mathematical entities'.

This is the reason why the new science of motion should be called *mathematical philosophy of nature* rather than *mechanics*. The traditional meaning of mechanics as an art which is directed *against* the 'nature' of bodies obscures the fact that the 'new' mechanics dealt with *natural* motions and aimed at the uncovering of their primary laws. While Newton made this intention quite clear when he chose the title *Philosophiae naturalis principia mathematica* for his chief work, it was the name *mechanica rationalis*[1], used by him in the preface in order to underline his *foundational* claims, that became prominent in the eighteenth century – perhaps for the sake of brevity, and for this reason only I will use it throughout this paper. It is important to note, however, that in the course of the eighteenth century, rational mechanics – even in the abstract, 'analytical' form that can be found in the works of Euler, d'Alembert and Lagrange – never became a 'purely' mathematical exercise without physical meaning: its concepts and primary laws were located in natural reality, and (therefore) its deductive consequences were expected to be empirically meaningful. Hence mechanics *between Leibniz, Newton and Kant* should not be understood as 'applied' mathematics in the modern sense (a syntactic structure to be 'filled' with semantic content by empirical data and rules of correspondence), but as the most important part of *mathesis mixta* in the traditional sense, i.e., as a part of mathematics that is *eo ipso* a part of natural philosophy, because it was the science of the (mathematical) laws of (natural) motion. Within the frame of rational mechanics, mathematical symbols and even the most abstract mathematical formulas are, so to speak, 'semantically laden'.

1.2. Euclideanism

A second common feature of mathematical philosophy of nature *between Leibniz, Newton and Kant* is of equal importance with respect to the role of mathematics: Rational mechanics follows the ideal of Euclidean geometry, or, to be more precise, its concept of science is best described as 'Euclideanism' (in Lakatos' sense). I will confine myself in this introduction to its most important feature: its first principles are not only true, but certainly true, i.e., infallible with respect to empirical 'anomalies'. This means, first and above all, that rational mechanics should not be understood as a hypothetical-deductive, but rather as an *axiomatic*-deductive science. In other words: If the hypothetical-deductive method is "at the core of modern science [neuzeitliche Wissenschaft]" (Böhme, *Alternativen der Wissenschaft*, 84), as is sometimes claimed, rational mechanics from Newton to Kant is *not modern*, and if it is defined as 'modern' [neuzeitlich], which is probably desirable for a science that was widely regarded as a prototype by both scientists and philosophers of science in the course of the eighteenth and nineteenth centuries, this characterisation cannot be true. The 'historical stability' of classical mechanics from Newton to Einstein is not only due to its empirical success, but also to its Euclideanistic leanings, and the decline of 'mechanical Euclideanism' was a necessary historical premise for the replacement of classical mechanics at the beginning of the twentieth century.

Newton, in his *Principia*, used a noteworthy phrase which makes these two sides of mathematics in natural philosophy visible: *axiomata sive leges motus*. As *leges motus*, his well-known mathematical propositions act as primary laws of nature which govern the behaviour of (possibly all) material *bodies*. As *axiomata* they act as first principles of the *theory* of mechanics, they govern the known laws and examples (especially from Kepler's celestial and Galilei's terrestrial mechanics) in order to gain a deductive organisation of the whole body of mechanical knowledge.

It is, however, by no means evident that primary laws of nature are 'prime candidates' for axioms of a deductively organised theory, nor is it clear whether such a 'metatheoretical coincidence' is possible at all: From natural laws the philosopher-scientist expects truth, empirical generality, explanatory power (mechanical explanation of possibly all phenomena of nature), a certain plausibility and intuitiveness with respect to his scientific metaphysics and (perhaps) necessity. From first principles or 'axioms' of a theory he expects, above all, truth, deductive power (entailment of all the other laws of a theory); moreover they are thought to be neither provable by other propositions nor – due to their evidence – to be in need of such a proof.

These demands correspond to each other, but they do not coincide. Why should they be granted by the *same* principles? Why should the basic laws of nature be identical with the axioms of a mathematical theory of nature? Kant, in his *Critique of Judgement* and elsewhere, discusses the possibility that this may not be the case: Though basic laws exist, their deductive power might be insufficient in order to build up a coherent order of science. Despite universal lawfulness, nature might, so to speak, refuse logical order. In this case man would come only to an 'aggregate' of regularities, i.e., to a number of diverging empirical laws, but not to order and unity.

This is a central point of my discussion: Laws have to explain nature, axioms have to organise theories. But a 'congruence' of the *order of nature* and the *orders of science* is increasingly difficult to guarantee when science produces a growing body of knowledge. Traditional mechanical Euclideanism is at stake here.

1.3. Orders of science

The plural 'orders' refers to a third point which should be mentioned at the outset: At the beginning of the eighteenth century, there were indeed *fundamentally* different attempts to gain a coherent system of 'mathematical principles of natural philosophy': At the least, Descartes' 'geometrical' mechanics, based on his laws of impact, Newton's mechanics of forces, based on his three laws and the law of gravitation, and Leibniz' dynamics, based on laws of impact and the conservation of *vis via*, should be sharply separated.

With respect to its empirical bearing Newton's *Principia* was obviously the most successful attempt, but it was neither unique in its intention, nor was it faultless or complete in its execution, nor was it understood as 'revolutionary' by the first generation of its readers, as far as the *principles* of mechanics[2] are concerned. That Newton laid down principles which are sufficient to solve all problems of mechanics is a legend which was invented by so-called 'Newtonians' of the first generation, spread by Lagrange, Montucla and others until it became a 'canon law' of history of science with Mach's *Mechanics*.[3] In recent times, Thomas Kuhn was its most prominent advocate[4], but this did not improve the 'law': it is simply false. It was mainly Clifford Truesdell's enormous contribution to the history of rational mechanics which made obvious that it was during the eighteenth century rather than the seventeenth century that classical mechanics, as it is known today, took shape. Therefore, 'Classical mechanics' and 'Newtonian mechanics' (understood as mechanics laid down by *Newton*) are by no means synonymous. As far as the *foundations* of rational mechanics are at stake, the great 'Newtonian revolution' did not take place.

Today, we see better than some decades ago that rational mechanics in the eighteenth century emerged from *different* sources and grew into a coherent system not before the end of the eighteenth century. Descartes, Newton, Leibniz, Huygens, Euler, d'Alembert, Lagrange and others contributed to the conceptual and mathematical framework that is known today as 'classical mechanics': The three first mentioned tried to establish *fundamentally* different sciences of mechanics, driven by *different* systems of 'scientific metaphysics'[5] and therefore based on different basic concepts and different 'first' laws of motion. I have elsewhere proposed that the development of rational mechanics in the first half of the eighteenth century could be essentially interpreted as a competition of these three great research programs of Descartes, Newton and Leibniz. If there is some truth in this conjecture – and a detailed analysis of the numerous controversies about the 'nature' of space and time, the conservation of *vis viva* and the concept of Newtonian force (esp. gravitation) might show that it is – the Mach-Kuhnian picture of eighteenth century rational mechanics as a 'normal' and 'formal' elaboration of the Newtonian paradigm cannot be upheld. To put it in the nutshell of Kuhnian terminology: with regard to the foundations of rational mechanics the eighteenth century was not 'normal', because the seventeenth century was not 'revolutionary' (Pulte, *Prinzip*, esp. 18). At least the first half of the *siècle du lumière* is characterised rather by the competition among fundamentally different endeavours to clear up the conceptual and formal framework of rational mechanics, and its outcome is by no means 'Newtonianism' in its original meaning.

During a period of 'revolution in permanence', however, so-called 'formal' elements of science gain a peculiar quality: While a 'conceptual discourse' across the boundaries of actual scientific metaphysics was hardly possible and almost futile (as is best illustrated by the famous Leibniz-Clarke correspondence), the language of mathematics became even more important for a small (and in a way isolated) scientific community that promoted rational mechanics (as is best illustrated by the continental reception of Newton's *Principia*). This is not to share the somehow *naive* view that mathematics in the age of reason worked as a kind of 'meta-language', capable of solving even *philosophical* problems of rational mechanics and, as it were, 'replacing' the Babylonian confusion of the different tongues of metaphysics – a view obviously shared by Lagrange.[8] It means, however, that mathematics played a key role in making accessible the results of one research program of mechanics to the others, that it was indispensable in integrating those parts which seemed valuable and that it was the only means of formulating 'towering' principles (like those of least action and virtual displacements) from which all the accepted laws of mechanics, whether or not they emerge from the 'native' research program, could be derived.

Scientific metaphysics tends towards a separation, mathematics tends towards an integration of different programs. At the end of the eighteenth century, we have one (and *only* one) system which represents *all* of the accepted 'mathematical principles of natural philosophy': Lagrange's *Méchanique Analitique*. But how could this integration happen? And what was its price, i.e., did it hold what mathematical philosophy of nature, a century earlier, promised? These questions address the central point of my paper, the *change* of concepts of science within rational mechanics and the reasons for this change.

1.4. Understanding the Change of Concepts of Science

'Semantical ladenness' and 'integrative potential' of mathematics as well as 'global' Euclideanism (i.e., Euclideanism of all programs of rational mechanics) are three points of departure of my survey. Its aim is a better understanding of the metatheoretical change of rational mechanics which took place in the course of the eighteenth century and is most obvious if we compare Newton's *Principia* (1687) and Lagrange's *Méchanique Analitique* (1788). Jürgen Mittelstraß has described the difference between both works as a replacement "of a 'Euclidean' [synthetical] construction of physics by an 'analytical' construction,"[9] and he has criticised this development as part of a methodological 'degeneration' that started with Newton. I have criticised the shortcomings of this view elsewhere (Pulte, *Mathematische Naturphilosophie*, ch. III.6). Here, I will try to *explain* the development from Newton to Lagrange by showing that both approaches are in the same 'Euclidean line' (though my definition of 'Euclidean' is different), the latter fulfilling, however, a different function: no decline of method, but rather a change of theoretical demands.

In general, I will argue that there is a growing tension between the *order of nature* and the *orders of science* that led to a dissolution of Euclideanism, beginning at the end of the eighteenth century and becoming most obvious in a crisis of meaning of so-called 'axioms' or 'principles' of mechanics. (This development, promoted by the rise of analytical mechanics, opened the way for conventionalism and instrumentalism in mechanics over the course of the following century, starting with Jacobi, Riemann and Carl Neumann and continued by Mach, Hertz, Poincaré, Duhem and others.) As I am aiming at a *structural outline* of these developments, examples, hopefully representative and illuminating, are reduced to a minimum.

2. MECHANICAL EUCLIDEANISM: THE CASE OF NEWTON'S *PRINCIPIA*

2.1. Mechanical Euclideanism

Lakatos' metatheoretical concept of 'Euclideanism' seems to me for several reasons an appropriate label for rational mechanics as pursued by most[8] of the eighteenth- and early nineteenth-century mathematicians, physicists, and philosophers: First, Euclideanism means that the "ideal theory is a deductive system with an indubitable truth-injection at the top (a finite conjunction of axioms) – so that truth, flowing down from the top through the safe truth-preserving channels of valid inferences, inundates the whole system." And its basic aim "is to search for self-evident axioms – Euclidian [sic!] methodology is puritanical, anti-speculative." (Lakatos, *Philosophical Papers*, II 28 and 29).

Secondly, Lakatos' concept is epistemologically neutral, i.e., Euclideanism includes both *empirical* and *rationalistic* foundations of the science in question.[9] For mechanics this means that it includes theories whose first principles are allegedly revealed by 'the light of reason' (Descartes) as well as theories whose first principles are allegedly 'deduced from phenomena' (Newton). Both kinds of justifications can be found in eighteenth-century mechanics, and in many textbooks they are inseparably interwoven. D'Alembert, Euler and Lagrange could well illustrate the (more general) thesis that the decisive philosophical feature of rational mechanics at this time cannot be understood in terms of the traditional dichotomy 'rationalism / empiricism'. This epistemological pattern is hardly suitable for grasping the development of mechanics into a highly organised body of knowledge. Largely independent of epistemological fixations, it is probably the search for certain, evident and general first principles and for suitable procedures of deductive inference with the overall aim of arriving at (possibly all) valid 'special' laws, which characterises mechanics at the time in question: different epistemological justifications, but equal metatheoretical fixations.

Thirdly, Lakatos' concept, used as a label, makes explicit that Euclidean geometry served as the model science for mechanics. This does not only mean that all relevant mechanical knowledge can be brought under an axiomatic-deductive structure, but also that it possesses the same distinctive characteristic as any other mathematical knowledge: *infallibility*.

In applying 'Euclideanism' in Lakatos' sense to rational mechanics from Newton to Lagrange,[10] I would like to add some metatheoretical features which are not to be found in Lakatos' work, but seem to be in line with his understanding of Euclideanism as the dominating 'classical' concept of science: First, I regard it as a general characteristic of mechanical Euclideanism that its principles are true *in isolation*. This does not necessarily mean that they are logically independent from other principles, but rather expresses the fact that *holism* (in the sense of Duhem and Quine) is alien to mechanical Euclideanism: The set of principles at the top is not interpreted as *one* logical conjunction, which (as a whole) is true, but as an aggregate of individual principles which are true and therefore applicable to the same physical system without 'interfering'. That is, for example, the reason why two principles like the law of inertia and the law of gravitation, though they seem to contradict each other in a certain sense[11], can coexist in Newton's *Principia* as axioms.

A second addition refers to the *metatheoretical* status of each element of a theory: This status is immutable, i.e., it cannot change with the context of application. The law of inertia, for example, being understood as a synthetic *axiom*, cannot 'degenerate' into an analytic *definition* (of being free of external forces).[12]

Thirdly, the set of principles of mechanics is understood not only as necessary and sufficient to deduce all accepted special laws (thereby altogether *making* them into proper laws) and to explain all phenomena in question. It is also understood as *unique* in the sense that no second, fundamentally different set is possible. The order of science is a *unique* representation of the order of nature. There is one (and only one) *true* mathematical science of nature, and it is defined by their mathematical principles or axioms.

The fourth and last addition is of limited range in the temporal aspect: I claim that the *early* programs of classical mathematical philosophy of nature (Descartes', Newton's, Leibniz' program) have in common that the irreducible, basic concepts of mechanics, as they appear in its principles, bear ontological burdens, i.e., to these concepts is ascribed a *fundamentum in re* in their actual scientific metaphysics: To space, time and mass, indispensable for any kind of mechanics, are added the concept of *vis viva* in Leibniz' program, rooted in his ontology of primary and derivative forces,[13] and the concept of (external and directive) force in Newton's program.[14] As no concept enters the level of mathematical principles which is *not* ontologically relevant in itself or 'derived' from ontological principles (Leibniz), we can characterize the early programs as different types of *mathematical realism*[15] or (partly) even of *mathematical essentialism*: Their first principles reflect the causal relations of nature.

2.2. Axiomatic Structure and Empiristic Methodology

In how far does Newton's program fit to these characteristics? It has often been stressed that the *Principia* 'follows' the standard of Euclid's *Elements*: The formal structure of the *Principia*, distinguishing definitions, axioms, propositions, corollars etc., makes this quite clear. It is, of course, easy to see that the definitions and so-called *axiomata sive leges motus* do not 'contain' the lower-level propositions of the deductive structure in the sense of Euclid's geometry. Newton frequently introduces *hypothetically*[16] further propositions (for example laws of forces), concrete examples etc. and than uses the *axiomata* in order to derive conclusions which are empirically testable.

But it has to be kept in mind that the empirical verification (or falsification) aims only at the hypothesis introduced, not at the *axiomata*. Are they hypothetical in the broadest (modern) meaning, i.e., propositions not yet acknowledged to be true and therefore regarded as revisable in the light of new experience? This is part of the question of whether Newton's mechanics is Euclideanistic in Lakatos' sense.

Euclideanism in Lakatos' sense obviously means *more* than a formal analogy to Euclid's *Elements*. It implies, first and above all, truth and infallibility of the first principles (or axioms) of the science in question. This ascription is perhaps easy to accept in the case of Descartes, but might be somehow provocative in the case of Newton.[17] The reason seems obvious: Newton claimed that he 'deduced' all of his laws, the axioms or laws of motion included, from phenomena. His discussion of the method of 'analysis and synthesis' and his methodologically articulated empiricism in general are most often understood as an implicit rejection of all Euclideanistic leanings, as they are ('clearly' and 'distinctively') to be found in Descartes' philosophy of science: a hallmark not of an axiomatic-deductive, but of a hypothetical-deductive science.

I do not agree with such a view, the outcome of the efforts of logical empiricism to make Newton its patron saint. Classical empiricism, as represented by Newton, does *not* 'automatically' imply fallibility of laws or even of first principles: Hertz' famous dictum, "that which is derived from experience can again be annulled by experience" (Hertz, *Principles*, 9) is *not* a part of this doctrine. Quite on the contrary, its basic attitude can be described like this: 'That which is derived from experience (by careful, gradual induction) can never be annulled by (further) experience'. Without going into the details of Newton's allegedly 'empirical' foundation of his axioms[18] and without discussing the vast literature on his philosophy of science, I would like to focus on the status of Newton's so-called 'axioms'.

Interestingly enough, Newton is pretty cautious with statements about the 'axiomatic' status of his laws of motion, the difference between axioms and 'lower level'-laws, the possibility of excluding all 'hypothetical' elements from law statements in general, and from the laws of motion in particular. The reason is that his empiricism yields no epistemological criteria of demarcation between axioms, laws and hypothesis though he obviously wants to distinguish axioms and 'usual' laws as well as laws and hypothesis. The whole methodology, as it is laid down in the *Regulae philosophandi* of the *Principia*, in the *Queries* of the *Opticks* and elsewhere, contains but one *positive* instruction of what to do when an inductive generalisation ("conclusion") conflicts with experience: "[...] if no Exception occur from Phaenomena, the Conclusion may be pronounced generally. But if at any time afterwards any Exception shall occur from Experiments, it may then begin to be pronounced with such Exceptions as occur." (Newton, *Opticks*, 404). Conflicting observations or experiments cannot falsify general conclusions, but only restrict their range of application. Falsification is even *excluded*, because according to Newton's empiricism both the conflicting phenomenon ("Exception") and the inductive conclusion are indisputably *true*.

But Newton's solution – restriction of the range of applicability by the enumeration of 'exceptions' – bears a problem in the case of axioms: According to his empiristic methodology, they can work *as axioms* for the (one and only) reason that they are *most general*, or even of unrestricted generality. On the other hand they *should* be open for restriction, if we take his methodology seriously. But what Newton really does, in contrast to his methodology, is to 'immunise' his axioms not only from falsification, but also from restriction. "As in Geometry [...] so in experimental Philosophy," he says, hypotheses and "first Principles or Axioms" have to be sharply separated: "These Principles are deduced from Phaenomena & made general by Induction: wch is the highest evidence that a Proposition can have in this philosophy [...]"; with respect to a possible falsifier (more appropriate: 'restrictor') he argues that "there is no such phaenomenon in all nature."[19]

Newton sometimes parallels his laws of motion with the axioms of geometry in order to underline the certainty he ascribes to these laws. "Hypothetical philosophy," as proposed by "[Des]Cartes, Leibnitz & some others" is contrasted with his own "experimental philosophy," which starts from "the three Laws of motion [which] are proposed as general Principles of Philosophy tho founded upon Phaenomena by no better Argument then that of Induction without exception of any one Phaenomenon" (Newton, *Correspondence*, V 398f.). He also reveals essentialistic leanings when he compares the knowledge of these principles with knowledge of the impenetrability of bodies (ibid. 399) – a property understood as most general and belonging to "the foundation of all philosophy" (Newton, *Mathematical Principles*, 399).

Newton obviously saw that his Euclideanism could not be founded on his empiristic methodology, though methodology was necessary to 'disguise' a certain essentialism with respect to first laws which *cannot* be established empirically. He therefore uses several other arguments in order to underpin the assumed certainty of principles – for example physico-theological arguments, especially with respect to his law of gravitation[20]. Furthermore, the laws of motion and their corollaries are summed up by the comment "Hactenus principia tradidi a mathematicis recepta & experientia multiplici confirmata,"[21] a phrase that might appeal to the *koinai ennoiai* or *communes animi conceptiones* at the time of Euclid's *Elements* (Szabo, *Geschichte*, 378-389), i.e., to principles which are neither demonstrated nor in need of demonstration, though accepted as true by all mathematicians. In a different context, Newton even describes a violation of the first law as an event which would disturb "the whole frame of nature, & in the general opinion of mankind is as remote from the nature of matter as [...] [penetrability]" (Newton, *Correspondence*, V 399). 'The general opinion of mankind': Remember that for Galileo, a generation earlier, Newton's first law was less than evident – it was unknown to him in the 'linear' form presented by Newton.

2.3. Newton's Euclideanism

These references are meant to throw some light on an antagonism between Newton's empiristic methodology and his actual attitude towards his *axiomata*: He *claims* that they are most general results of induction, and therefore can be understood as *laws of nature*. But he actually introduces a set of ingeniously chosen mathematical principles which function as *axioms* of the deductive structure of the *Principia*: Truth is 'injected' from the top, and its flow down to the level of phenomena cannot be turned round by conflicting observations. They work *de facto* as synthetic propositions *a priori*. Kant's interpretation of the *Principia* was closer to the historical truth than later, 'modernising' attempts.

The certainty of principles Newton supposed is only included in his methodology *ex negativo*, i.e., only to the extent that notable exceptions from valid inductions are not counted as falsifiers, but as 'restrictors'. But axioms are obviously exempted from restriction *without* methodological justification: greatest generality and certainty coincide in his philosophy of science. This coincidence is not (and cannot be) explained by his methodology, but is rather rooted in his ontology: The material truth of axioms, inundating the whole system of propositions, stems from *mathematics* itself. Newton holds the view that geometry is not a science that can be separated or abstracted from mechanics, but a science which shows how to apply *mechanically constructed* entities to physical reality.[22] This application poses no problem in itself: As they stem from nature, they are applicable to it. Rectilinearity, for example, *is* 'natural'; Euclidean geometry and simple algebraic relations (proportionality, for example) *are* empirically relevant. This may serve as an illustration of the thesis that mathematics – in Newton's case as in classical mathematical philosophy of nature in general – is 'semantically laden'.

Without going into the details of Newton's philosophy of mathematics, it seems clear that his *mathematical realism*[23] is at the core of what I described as his mechanical Euclideanism. This is the reason why the ontology of 'absolute, true and mathematical space' and 'absolute, true and mathematical time' is indispensable for *his* attempt to found rational mechanics. With respect to these entities, his axioms function as *synthetic* principles *a priori*. The traditional, rationalistic-minded Euclideanism demanded *metaphysical* support for these principles (as *causes are equal to its effects*). Newton rejects such support, though he cannot renounce metaphysics in his attempt to provide mechanics a 'secure' foundation: Methodological inductivism is not sufficient to reach this end.

3. NEWTONIAN AND ANALYTICAL PERSPECTIVES: EULER'S PROGRAM OF RATIONAL MECHANICS

It was Clifford Truesdell's huge contribution to eighteenth-century rational mechanics which has shown that Euler is its towering figure. Especially with respect to the development of its principles, his *oeuvre* is unique: We owe to him a sound formulation of the principle of least action (1744), the general formulation of 'Newton's' second law (1750), the law of conservation of moment of momentum (1755), the differential equations of an ideal liquid (1755), the general equations for the rotation of rigid bodies (1760) and numerous other achievements. Truesdell made Euler's immense work accessible to the history of science, thereby changing our understanding of its development in the course of the eighteenth century dramatically.[24]

Nevertheless, Truesdell's presentation of Euler's rational mechanics is one-sided and, in a way, misleading: According to him, "the history of rational mechanics is neither experimental nor philosophical, it is *mathematical*," (Truesdell, *Program*, 11) and consequently he presents Euler's contribution by and large as a *mathematical* one.

But Euler has more to offer. As Ernst Cassirer remarked, he is "the true and classical witness of the spirit of mathematical philosophy of nature," and the philosopher-scientist who "most completely represents the scientific consciousness in the middle of the eighteenth century."[25] While Euler's work by and large can support Truesdell's claim that rational mechanics was not experimental, it is by no means suitable to show that it was not philosophical. Quite the contrary: Euler's rational mechanics is *both* mathematical and philosophical in its character, and I claim that *both* parts are indispensable in understanding the coherence and continuity of his program.

The reason, however, why I chose Euler's mechanics as the 'fulcrum' between Newton and Lagrange is not so much its broad scope, nor its mere success in uncovering mechanical principles. The main reasons are rather that his program can serve, first, as a prototype of Euclideanism in the middle of the eighteenth century. Euler frequently states that rational mechanics has to start with a few necessary principles and that all changes in nature have to be explained by these principles in a deductive manner. His Euclideanism can be further described as *essentialism* in Popper's sense, because it proceeds from the idea that "all laws of nature can be deduced necessarily from one analytical principle (the essential definition of 'body')." (Popper, *Logik der Forschung*, 385). It was Euler's main concern to base mathematical mechanics on a theory of matter in which primary forces – regarded as incompatible with inertia – have no place. As I tried to show earlier, his scientific metaphysics and his philosophy of science were strongly influenced by Descartes (Pulte, *Prinzip*, esp. 110-121). The Cartesian ideal of a rational mechanics on an equal footing with geometry is always present in Euler's works, as it is in d'Alembert's.[26] Euler's *Mechanica* (1736), d'Alembert's *Traité* (1743) and Euler's *Theoria motus* (1765) are the three major textbooks in the second third of the eighteenth century, and their most important common feature is *Euclideanism*.

Notwithstanding its Euclideanism, Euler's program is, secondly, successful in integrating the results of other programs, namely Newton's and Leibniz', though Euler rejects the Newtonian mechanics of forces as well as Leibniz' dynamics on philosophical grounds. There is a 'peaceful coexistence' of diverging elements of different programs to be found in his work. In particular, we find both an elaboration of a 'Newtonian' axiomatisation (a label which will need some qualification) and the beginnings of an 'analytical' axiomatisation of mechanics (principle of least action, conservation of *vis viva*) in his work.

I am interested in how this integration worked and to what extent it changed the character of mechanical Euclideanism in the middle of the eighteenth century. For the sake of brevity, I will concentrate on four points which seem to me illuminating in these two respects.

3.1. 'Synthetical' Beginnings of Analytical Mechanics

Lagrange, in his *Méchanique Analitique*, called Euler's *Mechanica* (1736) the first book "in which analysis was applied to the science of motion."[27] Euler himself remarked that predecessors like Hermann and Newton treated mechanics "in the way the ancients did, by synthetic geometrical demonstrations," while he preferred the "smooth and uniform method" of analysis (Euler, *Mechanik*, I 3). As far as the use of higher calculus[28] is concerned, the *Mechanica* was indeed the starting point of 'analytical' mechanics.

But from a metatheoretical point of view, Euler's first mechanics is a traditional, *synthetic* one: It begins along 'Newtonian lines' with a discussion and definition of basic concepts like space, place, time, motion, rest, mass (*via* inertia) force, and proceeds with the laws of motion, which are, contrary to Newton, 'demonstrated' and therefore "not only true, but necessarily true."[29] Nearly the whole first chapter (§§ 1-82), parts of the second (§§ 99-117) and smaller parts of the following chapters are devoted entirely to the conceptual foundations of mechanics and problems of measurement. The same could be shown for Euler's second major work on mechanics, his *Theoria motus* (1765) and for numerous smaller articles. It has been asserted that the appearance of analytical mechanics *eo ipso* marked a 'methodological turn' and even a fundamental change in the 'concept of science' to the extent that analytical mechanics disregarded conceptual and methodological foundations and made experimental data its methodological starting point.[30] This thesis, however, does not withstand detailed historical examination.

3.2. 'Newtonian' Axiomatisation without Newtonian Ontology

Aside from all novelties with respect to content, there is also a new *metatheoretical* element in Euler's program, though not the one rejected above. I would like to illustrate this new element with just one example:

Euler's axiomatisation of mechanics is, by and large, a Newtonian one. He accepts Newton's first and third laws as starting points of his *mathematical* theory and tries to 'demonstrate' their *a priori* status, and he was the first who established the general form of Newton's second law in his *Découverte d'un nouveau principe de mécanique* (1750).[31] At this time he believed that it would "include all the laws of mechanics" and could serve as the "unique fundament" of the whole of mechanics (including the movement of continua, percussion and all processes which were presumed to be based on action at a distance).[32]

Euler did not, however, accept Newtonian, 'directive' forces as primary ontological entities, neither in his *Mechanica* nor later. There is a discrepancy between his ontology on the one hand and the basic concepts[33] of his mathematical theory on the other. This seems to contradict the essentialism I ascribed to him, and has indeed provoked interpretations of his program as being 'instrumentalistic'. But as was shown elsewhere, Euler never accepted this 'gap' between the mathematical part of his mechanics and his scientific metaphysics as final. He always looked for an explanation of forces by 'matter and motion' and found such an explanation in the impenetrability of matter, determining forces by the principle of least action and thereby basing his mathematical, 'Newtonian' mechanics on a 'quasi-Cartesian' theory of matter.[34]

Though Euler's solution is 'conservative', in so far as it sticks to traditional essentialism, the fact remains that *mathematical* axiomatisation and *ontological* foundation differ: Force is a central and irreducible concept of his mathematical mechanics, but alien to his concept of matter. This marks a difference between Euler's program and 'earlier' programs of mathematical philosophy of nature, as characterised above.

The case of Euler shows, as other cases (like d'Alembert and Maupertuis, for example) would show likewise, a *growing tension* between the mathematical treatment of rational mechanics and its foundation in scientific metaphysics. But what was its root?

3.3. *'Inflation of Principles' and Metatheoretical 'Sliding of the Center of Gravity'*

It has often been claimed that Newtonian mechanics made its way on the continent, despite all philosophical resistance, because it was empirically *successful*, especially in celestial mechanics. Applied to Euler, this might serve as a convenient explanation of how he dealt with forces: He was too much of a *mathematician* to dispense with the fruitful Newtonian mechanics of forces, and too much of a (Cartesian) philosopher to recognise forces as primary entities.

This argument should not be rejected indiscriminately, but it is of limited range with respect to the foundations of mechanics: First, it presupposes a prevalence of 'empirical success' over 'rational foundation', which seems problematic for the working philosopher-scientists in this period, especially for Euler. Secondly, it is applicable only *in favour* of the Newtonian program. But how to explain, for example, that Maupertuis – first an ardent disciple of Newton's philosophy and opponent of Leibniz and Descartes – rejected Newtonian forces in his later career and tried to replace Newton's laws by his 'non-causal' or 'descriptive' principle of least action, thereby making Leibniz' concept of *action* the primary concept of his mathematical mechanics? How to explain the *general tendency towards general principles* without causal claims (least action, virtual velocities etc.)?

It seems to me that questions like these cannot be answered satisfactorily by 'empirical success', nor by a general epistemological switch from 'rationalism to empiricism'. We need to consider the *practice of mathematical physics* (under the premise of diverging forms of Euclideanism) in order to understand these features.

What characterises rational mechanics above all in the second third of the eighteenth century is an *inflation of principles*: Numerous principles of statics which had to be integrated into a general science of mechanics, the principle of the conservation of momentum (or impulse, in modern terminology) for impact, the principle of *vis viva* conservation for (elastic) impact and central force problems, the three so-called Newtonian principles, the principle of moment of momentum, Maupertuis' *loi du repos* and the general principle of least action, d'Alembert's principle and the principle of virtual velocities, d'Arcy's principle, Koenig's principle etc. – not to mention the numerous principles of continuum and fluid mechanics which had to be integrated into the rational mechanics of mass points.

All of these principles grew out of the study of special problems and idealised physical situations, whose relevance for a mathematical theory of nature was determined by the current scientific metaphysics. They were confirmed by applications to different problems, and often gained their status as 'principles' by this restricted applicability alone. They were not 'deduced' from higher principles, nor 'deduced' from phenomena (in Newton's sense), but revealed their relevance by their (possibly limited) explanatory power. In a word: Their status as a 'principle' was not due to metaphysical or empirical foundation, but to the *deductively proceeding practice* of mathematical physics alone.

But Euclideanism cannot tolerate a plurality of principles, especially when they grew out of 'alien' scientific metaphysics. It strives for a small number of axioms, from which lower-level principles must be deduced: Plurality of principles is a result of different scientific metaphysics, unity is the aim of Euclideanism.[35] So if a (possibly 'basic') principle of one program turns out to be of (probably limited) deductive power for a different program, it has to be integrated in the deductive structure of the latter program, thereby 'explaining' its applicability. The 'mania of demonstration' (Mach, *Mechanik*, 72) and the fact that it was sometimes unclear that 'something must be *assumed*' (Truesdell, *Program*, 10) illustrate the efforts made in order to reach systematic order and, at the same time, that the ties to current scientific metaphysics were loose.

To give a concrete example: Conservation laws have no place in Newton's program. They were alien to Euler's scientific metaphysics, too: Euler was suspicious that *vis viva* and *impulse* (to use the modern word), introduced as basic concepts of mechanics, would mean introducing 'indestructable' entities – essential forces (or active principles), which are not allowed by his theory of passive matter. Johann Bernoulli and other 'Leibnizians' convinced him, however, that the concept of *vis viva* is of considerable interest to understand the different cases of elastic impact, and it also became important for his own investigation of central force problems. Euler therefore introduced *vis viva* as a *derived* concept, i.e., as the line integral of (Newtonian) force, and he also introduced impulse as a *derived* concept, i.e., as the time integral of (Newtonian) force.[36] Problems of conservation of *vis viva* and impulse were thereby transformed into problems of Newtonian mechanics and, in a way, to a problem of *mathematics*: When does an integrable force function exist? It depends on the answer to this question, in which (special) case the famous *vis viva* controversy can be decided in favour of Leibniz or not. The problem of force conservation, which was at the bottom of one of the most tedious disputes between the different programs of mechanics in the eighteenth century, thus became, as Euler said, a mere dispute about words ("logomachie") (Euler, *De la force*, 34). Conservation of *vis viva*, an 'axiom' of Leibniz' mechanics, and conservation of impulse, (*in nuce*) an axiom of Descartes' mechanics, are no longer axioms or 'principles' in Euler's program, but *derived laws*, which still can be used, however, in order to explain special physical phenomena.

This example, too, refers to the importance of the 'Newtonian' conceptual framework for Euler's program. But Euler's Newtonian leanings on the level of mathematical presentation are in this context not the main point of my argument, and it is neither the immediate empirical success of this framework (i.e., the deductive explanation of phenomena) nor the idea that certainty and evidence of basic axioms must be assured by proper 'demonstrations', based on scientific metaphysics.

Here, my main point is rather the deductive organisation of mechanics *itself*: It is not sufficient to *have* 'certain and evident' axioms, it must be shown that the whole mechanical knowledge accepted as true falls *under* these axioms. To use Lakatos' metaphor: It is not sufficient to introduce 'truth from the top' by indubitable axioms, it is also essential to be able to lead truth down to the bottom by building 'truth-preserving channels' (Lakatos, *Philosophical Papers*, II 28). This is a characteristic feature of Euclideanism in a developed (or advanced) stage: it focusses no longer on how to come to evident and certain axioms, but on the deductive structure of the growing body of knowledge. Alwin Diemer, who seems to have been the first German philosopher of science who tried to find criteria of demarcation between 'classical' and 'modern' science, used the metaphor of the "decline of the centre of gravity" to illustrate such a 'structural' development within classical science.[17]

In the course of the eighteenth century, a lot of mathematical and conceptual work was done in order to build 'truth-preserving channels' for the deductive structure of rational mechanics. Its outcome was, as already becomes visible in Euler's huge *oeuvre*, a hierarchically organised system, including elements of the different programs, but 'crowned' by Euler's transformation of Newton's three laws of motion.

But remember the 'decline of the centre of gravity': What counts here is the truth of the *whole* body of mechanical knowledge, which is – according to the Euclidean concept of science – 'represented' by its axioms in a formal way rather than 'condensed' in these axioms in a material way.

From this shift results a growing independence of mathematical physics from the philosophical foundations of its principles, be these foundations 'empirical' or 'rational': It is the deductive power of principles rather than their empirical contents, their axiomatic status rather than their status as 'laws of nature', their formal truth rather than their material truth, which become important. To borrow again from Lakatos' picture: If the deductive channels *are* filled with truth, and the truth flow down to the phenomena can be guaranteed, the source of truth becomes less important. Euclideanism continues to shape the concept of science, but it becomes a *syntactical rather than a semantical* concept of science.

This development of rational mechanics in the course of the eighteenth century is reflected by two main features: a decline of metaphysical discussions and a rise of deductive organisation by appropriate mathematical techniques. The great controversies about the 'nature' of space and time, about the status of gravitation, about the existence of entities which are conserved in all nature, belong to the first half of the century rather than to the second, while 'technical' discussions about the calculus of variations, potential theory, differential equations and perturbation theory were prominent in the second half rather than in the first.

Euler himself saw this shift fairly early, and used it as an argument to restrict the impact of traditional metaphysics on science: In his famous *Réflexions sur l'espace et le tems* (1748) he explicitly stated that it is mistaken to think that mechanics or mathematical physics in general receive true foundations from metaphysics, but that, *vice versa*, metaphysics has to model its basic ideas in such a way that its conclusions agree with the "indisputable" principles of mechanics (Euler, *Réflexions*, esp. 376f.; Cassirer, *Erkenntnisproblem*, 475-479). Mathematical physics does not (and cannot) dispense with philosophical foundations, but it can (and must) determine *what* has to be founded. Not autonomy of science from philosophy, but a certain 'equilibrium' of science and philosophy is his object – a model which resembles Hilbert's distinction of mathematics and philosophy of mathematics ('metamathematics').

3.4. Analytical Principles of Mechanics

The development sketched above is perhaps best illustrated by analytical mechanics[78], to which Euler contributed substantially, too. The rise of analytical principles like the principle of least action or the principle of virtual velocities cannot be understood by the Mach-Kuhnian pattern of rational mechanics as 'normal science' in the tradition of Newton's *Principia* (see part 1.3). These principles originated from concrete problems, and their development was driven, *at first*, by other programs and (partly) by substantial philosophical difficulties of the Newtonian program – and not by 'formal' demands.

The principle of least action, for example, was understood as an *alternative* to Newton's foundation of mechanics by Maupertuis as well as Euler. Both underlined its descriptive and, so to speak, 'phenomenological' character in contrast to its explanatory function in terms of a Newtonian mechanics of forces. While the concepts of causality and force ran into a crisis, it was meant to provide a new foundation of mechanics, which had *not* to make use of these problematic concepts. Only *later*, with Lagrange, it became a merely *formal* alternative to a 'Newtonian' axiomatisation of mechanics,[79] i.e., a part of 'normal Newtonian science' in Kuhn's sense.[80]

I am mentioning this 'context of discovery' because it is part of the development described above (see part 3.3.) and might best illustrate some of its implications. Again, I will use mainly the principle of least action for illustration.

First, the concept of *action* used in this principle is no longer a concept which is determinated as 'basic' by actually scientific metaphysics: Maupertuis picked it up from Leibniz, but it had no genuine meaning in his *own* mechanics. Being forced to give a 'higher' justification of his principle (a demand of traditional mechanical Euclideanism), action became a measure of 'divine force' – a retrogression to occasionalism which was not rooted in Maupertuis' genuine scientific metaphysics. Euler, too, was initially worried about the fact that action could not be justified by clear philosophical arguments. It made its way into his mechanics not because it was rooted in his scientific metaphysics, but because it turned out to be useful. It can generally be said that concepts like action, *effort* and potential energy, in the case of the least action principle (and Maupertuis' *lois du repos*), or virtual work (virtual displacement), in the case of d'Alembert's principle and the principle of virtual velocities, do not have the same semantic relevance as the basic concepts in the earlier programs of mechanical Euclideanism (like force, velocity, *vis viva*, etc.). The rise of analytical principles is accompanied by a 'semantical unloading' of their basic mathematical concepts.

This process is, secondly, parallel to the changing role of analytical principles. They started from special problems, but soon turned out to be applicable to a wide range of phenomena and even to derive a number of more special laws of motion and other laws. Maupertuis and Euler[81] extended the principle of least action to optics (derivation of the law of reflection and refraction), to the statics of point masses and continua (derivation of Maupertuis' *lois du repos*, the principle of the lever, special forms of 'Dirichlet's principle'), to the mechanics of impact (conservation of impulse and, in the case of elastic collision, of *vis viva*) and to central force problems (derivation of Kepler's laws and special forms of the equations of motion). This applicability to a wide range of 'heterogeneous' problems was *unique* in the history of mechanics, and it led Euler and Maupertuis to the view that the principle of least action can work as an organising principle of the *whole* of mechanics, i.e., a principle from which a great variety of special laws of motion and rest can be deduced.[42] While metaphysical discussions were prominent in the early career of the principle of least action, its later development was determined by the extension and analysis of its integrative and deductive power.

This seems to me exemplary for analytical mechanics in general: The rise of analytical mechanics in the second half of the century highlights the striving of Euclideanism for an axiomatic-deductive organisation of science. But it has to be noted that in the course of this process an important change takes place in so far as *principles become formal axioms of science rather than laws of nature*. Lagrange's mechanics is most significant in this respect.

4. THE EDGE OF CERTAINTY: LAGRANGE'S ANALYTICAL MECHANICS

In a way, Lagrange's mechanics *completes* the development sketched above though, in a different way, it marks a *break* with the older tradition, thereby revealing the basic philosophical problems of mechanical Euclideanism. In short, Lagrange's approach can be described as *Newtonian* with respect to the philosophy of nature, leading to an ideal of mechanics which tries to explain all phenomena by central forces acting between discrete particles. His philosophy of science, however, was strongly influenced by d'Alembert and Euler. As both his predecessors, he wanted to base mechanics on certain and evident principles: "Mechanics can be understood as a geometry with four dimensions, and the analysis of mechanics can be understood as an extension of geometrical analysis." (Lagrange, *Théorie des fonctions* (2nd ed.), 337).

Geometry continues to be the ideal of mechanics, though the shape of Euclideanism changes considerably. In trying to illustrate this change, I will confine myself to three major points.

4.1.Changing Principles and Concepts

Before I come to what I regard to be the main features of Lagrange's concept of science, I would like to refer to a remarkable, though widely neglected development in Lagrange's foundations of mechanics: For reasons to be discussed later (see part 4.2) Lagrange started his mechanics with *analytical* principles. In his early career, he had chosen Euler's *principle of least action* as "the universal key to all problems, both of statics and dynamics."[43] In his first paper on analytical mechanics, he not only derived from it different 'integrals of movement', but also the 'Newtonian' (or rather 'Newton-Eulerian', see part 3.2) differential equations of motion for all conservative forces (Lagrange, *Application*, 369). This was a remarkable achievement within eighteenth-century rational mechanics, because Lagrange's paper was the first work "in which an adequate statement of the laws of a fairly extensive branch of mechanics was gotten without the use of an *a priori* concept of force" (Truesdell, *Program*, 33). This *could have been* the starting point of a new conceptual foundation of mechanics which actually was not elaborated until the last decades of the nineteenth century by Helmholtz, Hertz and others. But actually it *was no* starting point, because Lagrange did not even take notice of it – at least he discussed it nowhere. In later papers as well as in his *Méchanique Analitique* he rather replaced the principle of least action with his 'variational' form of the *principle of virtual velocities*, thereby *reintroducing* forces as basic concepts of his mechanics. If the reconstruction given elsewhere (Pulte, *Prinzip*, 252-258) is right, this switch was due primarily to the fact that the latter principle turned out to be more useful in deductive respects: 'Deductivity' wins over conceptual foundation – or at least makes conceptual foundation a problem which no longer requires discussion. This is one of the features of Lagrange's approach to which I will come now.

4.2. No Geometry, no Methodology, no (explicit) Scientific Metaphysics: The New Meaning of 'Analytical'

From the beginning, Lagrange's main aim was a coherent deductive system of the laws of rest and motion. Both the history of mechanics and its dominating concept of science (i.e., Euclideanism) make this aim plausible: When Lagrange started his scientific career in the fifties, he was confronted with a totally different state of mechanics than Euler was twenty years earlier. As already mentioned, the mechanics existing then presented a great number of generally accepted laws and so-called principles, including Newton's laws of motion (in Euler's form), d'Alembert's principle and the principle of least action. Lagrange's Euclideanism could (and had to) operate beyond the level of special examples (as Euler's), but on the level of more or less general propositions. These propositions were actually presented in an algebraic or even analytic fashion, in which geometry possibly served as a means of illustration,[44] but no longer had important foundational or inferential tasks. This is the reason why Lagrange focussed on analytical principles as 'candidates' for axioms of his system (see part 4.1) and disregarded synthetical or geometrical means. That "no figures are to be found in this work," (Lagrange, *Méchanique Analitique*, vi) as he later proudly remarked, is an outcome of the state of affairs of the mechanics of his time *and* of his Euclideanistic striving for a *unique* order of science – and not of personal preference, as was sometimes presumed. Nor does this mean, in and of itself, a fundamental change in the concept of science. In modern terms, geometry remained important for Lagrange in the context of discovery (Grattan-Guinness, *Recent Researches*, 679), but had to be eliminated from the context of presentation and justification. No principles other than the 'analytical' could actually do the job of deductive organisation. So much for Lagrange's neglect of geometry.

More serious with respect to a possible change in the concept of science is the absence of nearly any kind of methodology or explicit metaphysical foundation of mechanics (Pulte, *Jacobi's Criticism*, esp. 158.). Lagrange's *Méchanique Analitique* (1788) is the first major textbook in the history of mechanics which I know of which abandons any kind of explicit philosophical reflection. It says nothing about how space, time, mass, force (in Newton's sense) or *vis viva* (in Leibniz' sense) are to be established as basic concepts of mechanics, nor about how a deductive mathematical theory on that basis is possible. Neither are the metaphysical premises of his mechanics made explicit, nor is there any epistemological justification given for the presumed infallible character of the basic principles of mechanics. Lagrange's silence about foundational issues is in striking contrast not only to seventeenth-century programs of mechanics such as those of Descartes, Leibniz and Newton, but also to the approaches of Lagrange's immediate predecessors in the analytical tradition, i.e., Maupertuis, d'Alembert and Euler. In short, a century after Newton's *Principia*, Lagrange gives an 'update' of the mathematical principles of natural philosophy, while abandoning traditional subjects of *philosophia naturalis*. His bold claim to make mechanics "a new branch" of analysis (Lagrange, *Méchanique Analitique*, vi) by 'reducing' it to calculus and reducing the calculus to a sound algebraical basis in order to achieve a secure foundation of the whole of mechanics (Grabiner, *The Calculus as Algebra*, 7-10) can and should be understood not only as a rejection of geometrical means, but also as a *rejection of explicit philosophical foundations* in the broadest sense. This is the most important metatheoretical novelty of Lagrange's program. Insofar as Lagrange is not interested in the conceptual foundations of his mechanics, and even changes his basic concept for reasons of 'formal economy' (see part 4.1), his mechanics can no longer be understood as a Euclideanistic enterprise (in the 'traditional' sense, see part 1.2 and 2.1), but rather as an example of *mathematical instrumentalism*.[45]

Lagrange obviously shares, by and large, the scientific metaphysics underlying the Newtonian program (Pulte, *Prinzip*, 230-240), but this fact is not reflected in his 'purely mathematical' mechanics. Central forces, free mass-points, absolute space, absolute time and intuitive natural laws on the one hand, mathematical concepts of potential and kinetic energy, masses under ideal constraints, a 'structural' (with respect to the invariance properties of variational principles) rather than Euclidean space, time as a mere 'fourth coordinate' and abstract variational principles on the other hand: Mathematics serves as a formal frame, but is 'unloaded' of meaning.

We saw that in Euler's program, too, basic assumptions about nature and mathematical presentation deviated (no primary ontological forces here, but basic 'mathematical' forces there, for example). But this deviation could be cleared up by a reconstruction of his philosophical thinking: it was *explainable* by his own scientific metaphysics. In Lagrange's case, however, we are left with the simple *fact* that 'order of nature' and 'order of science' differ. An explanation cannot be found in his scientific metaphysics (because there is no explicit scientific metaphysics to be found in his work), but has to be sought in the *wider* historical context.

It is my thesis that Lagrange's mechanics is a logical *consequence* and, at the same time, a *dissolution* of Euclideanism in its original meaning: While the 'sliding of the centre of gravity' (in Diemer's sense, see part 3.3) continues, axioms become formal principles rather than principles with regard to content; the whole system is held together by logical coherence rather than by material truth. This is what happened in eighteenth-century rational mechanics, this is what later happened in geometry.

I claim that this development is, as far as the mathematical sciences are concerned, somehow *inevitable* under the conditions of 'global' Euclideanism and successfully competing research programs: Each program tends towards building up deductive structures (filled with different contents), global Euclideanism tends towards building up a unique 'superstructure' (left with the problem of what its content is). To put it more precisely: Lagrange was confronted with an abundance of different laws (Newton's laws, conservation laws, variational laws etc.) which emerged from different programs, and he had good reasons to accept them as valid, because they had turned out to be appropriate to describe (and deductively 'explain') different classes of mechanical problems. Lagrange's Euclideanism now operates on the level of these laws which are already expressed in algebraical or analytical form. It aims at a hierarchy of laws, starting with most general ones and ending with special ones and single problems. Higher calculus serves as the uniting element in the deductive chains. Insofar as order and unity become the main targets and the calculus the main means, this mechanics is rightly called *analytical*.

To sum up: Lagrange's main concern is a *deductive organisation* of the different laws, not the discovery of new ones.[46] Along with the aim of 'reducing' all mechanical problems to general equations, this is the main object of his program: "The various principles presently available will be assembled and presented from a single point of view in order to facilitate the solution of the problems of mechanics. Moreover, it will also show their interdependence and mutual dependence and will permit the evaluation of their validity and scope." (Lagrange, *Analytical mechanics*, 7).[47]

4.3. Loss of Evidence: 'Rubber Euclideanism'

Regardless of his 'mathematical instrumentalism' (see part 4.2), Lagrange stuck to the idea that mechanics can be built up from evident and certain axioms. The combination of new instrumentalism (with respect to philosophy of nature) and old Euclideanism (with respect to philosophy of science) seems to me the decisive characteristic of his mechanics as well as the weakest point of his approach.[48]

This combination bears a significant tension of which Lagrange himself was partly aware, and some of his successors in the French tradition of mathematical physics were even more so: the conjunction of Euclideanism and instrumentalism suggests that the 'deductive chain' can be started by first principles without recourse to any kind of geometrical and physical intuition or metaphysical arguments. This leads inevitably to a conflict with the traditional meaning of 'axiom' as a self-evident first proposition which is neither provable nor in need of a proof. Lagrange wanted to start with one principle, i.e., the principle of virtual velocities. In the first edition of his *Méchanique analitique*, he introduced this "very simple and very general" principle in statics as "a kind of axiom" (Lagrange, *Méchanique Analitique*, 12). He appeased his tangible discomfort with the title 'axiom' by extensive references to its successful use by great authorities of the past like Galileo and Descartes.[49] In the second edition, he stuck to the title 'axiom', but had to admit that his principle lacks one decisive characteristic of an axiom in the traditional meaning:

It is "not sufficiently evident to be established as a primordial principle" (Lagrange, *Mécanique Analytique* (2nd ed.), I 23 and 27).

Euclideanism demands evidence; instrumentalism tends to dissolve it. This is the basic dilemma of Lagrange's mechanics.[50] In two different so-called 'demonstrations' he tried to prove his primordial principle by referring to simple mechanical processes or machines (Lagrange, *Sur le principe des vitesses* (2nd ed.), 350-357), thus trying to bring back intuitive truth to his axiom. Lagrange's formulation and/or demonstration of the principle of virtual velocities posed a challenge for a number of mathematicians.[51] There was a "crisis of principles," (Bailhache, *Introduction et commentaire*, 2) and it was caused by the *Méchanique Analitique*. All attempts to solve it, however, aimed at better demonstrations, giving the principle of virtual velocities a more secure foundation and making it more evident. Like Lagrange, they applied their refined logical and mathematical methods to mediate evidence to the principle of virtual velocities. Lakatos, in a different context, aptly described such a position as "a sort of 'rubber-Euclideanism'" because it "stretches the boundaries of self-evidence." (Lakatos, *Philosophical Papers*, II 7).

This episode can be interpreted as a 'metatheoretical turning point' with respect to 'practised' mathematical physics: Some decades later, mechanical Euclideanism became suspicious, a development which opened the way to other concepts of science. But this story is certainly 'beyond Leibniz, Newton and Kant'.[52]

5. KANT AND EIGHTEENTH-CENTURY RATIONAL MECHANICS: TWO PROJECTIONS

Notwithstanding the development of 'practiced' Euclideanism as outlined above, the ideal of a theory of mechanics on equal footing with geometry continued to attract scientists and philosophers until the twentieth century. As is well known, Kant's philosophy of science was (and possibly is) the most important bastion of this ideal: Though 'revolutionary' in its philosophical approach, it was 'conservative' in its objective to found the theory of mechanics upon certain and apodictic principles. The preface of Kant's *Metaphysical Foundations of Natural Science* is perhaps the best articulated representation of a *classical* concept of science as distinct from a *modern* one[53] to be found in the whole history of philosophy of science. Apodictical certainty, *a priori* principles, systematic order and the necessity of a metaphysics of nature *as well as* mathematics in order to establish such a science are the main features of this concept. Kant borrowed it from the mathematical physics of his time, and he aimed at a philosophical foundation which the scientists themselves were unable to provide.[54] Newton's mechanics was his main object, but his attempt to gain a sufficient foundation also relied on Leibniz, Euler and other philosopher-scientists of the Age of Enlightenment.

It may appear daring or even misleading to discuss Kant's foundational attempt in the context of this paper: Kant next to Lagrange? This might be offensive to any philosopher (and perhaps to some mathematicians as well). Though I do not share such 'isolating' views, I will *not* discuss *any* details of Kant's mathematical philosophy of nature in this article.[55] I will rather confine myself to some observations on *how* Kant perceived contemporary rational mechanics and tried to save its Euclideanism by new means.

5.1. The 'Synthetical' Projection: Metaphysical Foundations

The *Metaphysical Foundations* are *synthetic* in at least three different (though not independent) senses: in a new or epistemological Kantian sense ('pure' synthesis of *a priori* concepts; *S1*), in a traditional or methodological, especially Newtonian sense ('proved' explanation of phenomena and special laws by deduction from principles; *S2*) and, close to this, in a traditional mathematical sense (relying on Euclidean geometry; *S3*). It was the primacy of *geometrical* construction (*S3*) which prevented Kant from considering approaches to mechanics which are basically different from Newton's as possibly relevant for his foundation of natural philosophy: Though he could have learned from the analytical tradition (see part 4.1) that mechanics *was* actually established on different conceptual bases (*S2*), and therefore might have come to the tempting philosophical problem of whether this *can* be done, i.e., whether a metaphysical foundation (*S1*) for such attempts can be provided, Kant restricted the *Metaphysical Foundations*, by and large, to a modified Newtonian mechanics: (*S3*) obviously was too evident for Kant – or was this restriction really brought about by (*S1*), as a 'true' Kantian would argue?

Though Kant did not want to pursue empirical science, he wanted to show *how* (on the basis of synthetic *a priori* principles) an empirically successful *and* mathematical science of mechanics is possible and *that* (as a science) it forms a system, i.e., "an interrelation of reason and consequence." (Kant, *Metaphysische Anfangsgründe*, A V).

The preface of the *Metaphysical Foundations* is promising, and many interpreters did not go beyond it. Kant's accomplishment of his plan in the following parts (*Phoronomy*, *Dynamics*, *Mechanics*, *Phenomenology*) is, however, less encouraging. Where does he 'demonstrate', for example, the assumption made in the addition to the second definition of his *Dynamics* "that all movement which one body [eine Materie] can impress to another must be regarded as applied along the straight line between both points"? (*ibid*., A 35). Other 'lacunae' could easily be added. The main reason for Kant's limited success with respect to 'contents', however, is the fact that he *nowhere* deduces Newton's second law of motion or an equivalent law of motion, and, it seems to me that from the conceptual frame chosen in the *Metaphysical Foundations*, no successful empirical science can arise *without* such a law. Kant, however, does not even mention it in his book.[36] By and large it must be said that only the first part (*Phoronomy*) was sufficiently developed by him.

If we try to sum up the achievements of the *Metaphysical Foundations* not from an 'internal' point of view,[37] but placing it in the wider context of contemporary science and philosophy of science, the result might look like this: They offered *synthesis* (along *S3*), they promised empirical relevance (*S2*) on the basis of its synthetic *a priori* frame (*S1*) and they gained only beginnings of the systematic order, which (according to Kant) is characteristic to 'proper science' and which, in a formal, *mathematical* (though not *synthetical*, i.e., geometrical) manner, largely was achieved by mathematical physics *itself* (see part 4).

5.2. The 'Analytical' Projection: Critique of Judgement

Though analytical mechanics played no role in Kant's 'critical' foundations of natural philosophy, he did not completely ignore it. He paid special attention to Maupertuis's principle of least action, because it seemed to include not only "the most general laws, by which matter actually acts," but also a plentitude of special laws for quite heterogenous areas of phenomena, thereby giving "unity to the infinite manifold of the universe, and order to blind necessity." (Kant, *Beweisgrund*, A 63). In his pre-critical period he shared Maupertuis' physico-theological interpretation of this principle and made it a part of his argument that "a necessary order of nature derives the harmonious arrangements of matter from the necessary laws of interaction constituting the very essence of matter itself," as Michael Friedman aptly put it (Friedman, *Kant*, 13). In Lakatos' above-quoted metaphor: Using the physico-theological argument, Kant injects truth at the top (principle of least action) and claims that it can be spread over the whole system of science (plentitude of special laws of nature) by deductive inference, thus making these special laws (which start as mere inductive generalisations – 'empirische Regeln') not only true, but *necessarily* true, and their connections *necessary*, too. His ideal of a systematic order of science, which in a way should be *isomorphous* to the order of nature, is thus guaranteed.

This thought evolved into one of the central problems of Kant's philosophy of science during his 'critical period', when the physico-theological argument was no longer acceptable. While the old 'solution' was given up, the *problem* remained: We possess, as a *matter of fact*, a number of special laws ["besondere Gesetze"] – empirical rules, which are obviously true, but which have not yet been proved to be necessary. They form a mere 'aggregate' of possible laws, but no system: Nature reveals regularities, even possible laws, but no order;[illegible] it might be structured in a such way that we are left in a "labyrinth of a manifold of possible [and] special laws of nature" (Kant, *Erste Fassung*, 19) forever: No order of nature, no order of science.

This problem belongs to the *Critique of Judgement*, because it is *reflecting* judgement which has to subsume special laws under (possibly existing) more general laws or principles. Kant introduced the 'transcendental principle of judgement' in order to save his ideal of order and unity: We must *assume* that nature forms a well-conceived system for our mind. In our context, i.e., in the context of *mathematical* order of nature, this premise can be labelled Kant's *subjective formal teleology* of nature.

This kind of teleology is explicitly introduced by Kant as a regulative, *not* as a constitutive principle. He himself, however, 'transcends' this distinction when he tries to show that subjective formal teleology implies the *necessity* of the special laws of nature (i.e., their very lawfulness). This attempt has no sound basis in his 'critical' philosophy of science, and it cannot yield the necessity of special laws and thereby the existence of a scientific *system* Kant was looking for.[illegible] It is as a relapse to the pre-critical period, an appeal to the both *discoursive and intuitive* divine understanding which man can never reach. Thus the ideal of mechanical Euclideanism – an 'order of science' in agreement with the 'order of nature' –

continues to exist in Kant's philosophy of science as a regulative idea of reason, but cannot constitute a scientific system in an *objective* sense.

6. CONCLUSION

I tried to describe and explain some developments of eighteenth-century mathematical philosophy of nature from a unified point of view, regarding *mechanical Euclideanism* as its dominant concept of science. Regardless of epistemological fixations, this concept of science shaped mechanics up to the end of the century. It strives, above all, for certain (infallible) and evident principles, but also for unity and completeness: It aims at an *axiomatic*-deductive structure of the *whole* 'order of science'. From Euclid's *Elements* to Newton's *Principia* two thousand years passed without overcoming this ideal and its dogmatic implications. So, if an all-out hypothetical-deductivism is regarded as the essential feature of *modern* science, rational mechanics in the age of reason was *not modern*. If, on the other hand, we want to integrate this key discipline into modern science – certainly desirable according to the common understanding of modernity – we have to look for reasons for metatheoretical change which ultimately led to a *consequent* fallibilism (i.e., a fallibilism with respect to *principles*) within mechanics.

In this paper, I tried to locate one important reason for this change within 'mechanical Euclideanism' itself: in order to save the ideal of order and unity, rational mechanics in the course of the eighteenth century had to rely increasingly on abstract mathematical tools and techniques, thereby 'unloading' its axioms of that meaning and intuition which was initially (relative to the scientific metaphysics in question) their characteristic. This process ended in Lagrange's mechanics: The *Méchanique Analitique* makes use of 'first' principles only as *formal* axioms with great deductive power which can no longer be understood as laws of *nature* in the original sense. This is what, in the end, caused a 'crisis of principles' and promoted phenomenalistic, conventionalistic and instrumentalistic concepts of mechanics in the course of the nineteenth century.

Kant, on the other hand, tried to 'synthesise' mechanical knowledge in some principles, which are, under the premises of his system, certain and evident, but he made by no means clear how the whole body of accepted knowledge could be based on these principles. In his philosophy of mechanics, the unique 'order of science' remained an 'projected' ideal and nothing more.

Though there were more reasons to believe in the ideal of Euclideanism – the very possibility of certain foundations and an axiomatic-deductive order of science – in the eighteenth century than today, this metatheoretical concept cannot be 'refuted' – not, of course, by history, and not by philosophical arguments either. It must be kept in mind, however, that it is "the *Programme of Trivialization of Knowledge*." This holds true for mathematics at the turn towards the last century and shortly beyond, where all 'reductions' to logic failed, this holds even more true for mathematical physics in the eighteenth century. Nevertheless I regard Euclidianism as a *historiographically* useful category, especially with respect to the history of mathematical physics. Metatheoretical concepts like this are necessary if we want to understand fundamental changes in the sciences and their philosophy. But they have to be supplemented by guiding historical questions and filled with 'factual' history in order to uncover *reasons* for historical change: Perhaps no unique *history of reason* will be possible, but there is certainly more *reason in history* than some of our 'postmodern' contemporaries would imagine.

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NOTES

1 "[...] rational mechanics [mechanica rationalis] will be the science of motions resulting from any forces whatsoever, and of the forces required to produce any motions, accurately proposed and demonstrated. This part of mechanics, as far as it extended to the five powers which relate to manual arts, was cultivated by the ancients, who considered gravity (it not being a manual power) no otherwise than in moving weights by those powers. But I consider philosophy rather than arts and write not concerning manual but natural powers, and consider chiefly those things which relate to gravity, levity, elastic force, the resistance of fluids, and the like forces, whether attractive or impulsive; and therefore I offer this work as the mathematical principles of philosophy, for the whole burden of philosophy seems to consist in this—from the phenomena of motions to investigate the forces of nature, and then from these forces to demonstrate the other phenomena [...]" (Newton, *Mathematical Principles*, XVII-XVIII). In one of his early papers on the history of rational mechanics, Clifford Truesdell asked for any precursor of Newton using the term 'rational mechanics', and I. Bernard Cohen later put forward the same question. Alan Gabbey has shown that it was used in Goclenius's *Lexicon philosophicum Graecum* and therefore "was in (probably common) use during the first decade of the seventeenth century, at the latest" (Gabbey, 309, n. 13). His argument, that Newton's *Principia* "was and was not a treatise on mechanics" (ibid., 308) seems to be in line with my understanding of 'mathematical philosophy of nature'; see Pulte, *Mathematische Naturphilosophie*.

2 Concerning the foundations of mechanics, Newton's principles (*axiomata sive leges motus*) and his law of gravitation should be distinguished. What was soon understood as 'revolutionary' (in the sense of an obvious and irreversible break with the past) was his celestial mechanics, i.e., the application of his three laws *and* the law of gravitation to the motion of the moon and the planets. In the last decades, however, more and more publications have shown that Newton's three laws of motion were neither entirely new, nor understood as new by his contemporaries and his immediate successors; for an overview see Bos, *Mathematics and Rational Mechanics*.

3 "Die Newtonschen Prinzipien sind genügend, um ohne Hinzuziehung eines neuen Prinzips jeden praktisch vorkommenden mechanischen Fall [...] zu durchschauen. Wenn sich hierbei Schwierigkeiten ergeben, so sind dieselben immer nur mathematischer (formeller) und keineswegs mehr prinzipieller Natur" (Mach, *Mechanik*, 272). Mechanics after Newton is characterised by Mach as a deductive, formal and mathematical development on the basis of his principles (ibid., 179).

4 "The Principia [...] did not always prove an easy work to apply, partly because it retained some of the clumsiness inevitable in a first venture and partly because so much of its meaning was only implicit in its applications. For many terrestrial applications, in any case, an apparently unrelated set of Continental techniques seemed vastly more powerful. Therefore, from Euler and Lagrange in the eighteenth century to Hamilton, Jacobi and Hertz in the nineteenth, many of Europe's most brilliant mathematical physicists repeatedly endeavoured to reformulate mechanical theory in an equivalent but logically and aesthetically more satisfying form. They wished, that is, to exhibit the explicit and implicit lessons of the *Principia* and of Continental mechanics in a logically more coherent version, one that would be at once more uniform and less equivocal in its application to the newly elaborated problems of mechanics. Similar reformulations of a paradigm have occurred repeatedly in all of the sciences, but most of them have produced more substantial changes in the paradigm than the

reformulations of the *Principia* cited above." (Kuhn, *Structure*, 33). Kuhn's marginal note ("*Principia* and of Continental mechanics") reveals the main problem which he failed to address *because* of the 'Machian shaping' of his history of mechanics.

[5] In this paper, I will use the term 'scientific metaphysics' for all assumptions which define the 'hard core' of a scientific research program in the sense of Lakatos. They belong to metaphysics, in so far as they are immune from empirical falsification, and they are scientific, in so far as they determine the problems, basic concepts and acceptable explanations of the science in question. Elkana, inspired by Lakatos, defines scientific metaphysics as "those untestable hypotheses which deal with the structure of the physical world and which direct scientists in their research" (Elkana, *Euler and Kant*, 278). The scientific metaphysics of mechanics shapes the understanding of matter and motion. It has genuinely to do with the concepts of space, time, mass and (eventually) with the concept of force and (or) energy and their mutual relations.

[6] See Lagrange's letter to d'Alembert of January 27, 1778 (Lagrange, *Oeuvres*, XIII 336).

[7] See Mittelstraß, *Neuzeit*, esp. 302. 'Euclidean' and 'synthetical' are obviously used as synonyms; see Mittelstraß, *Möglichkeit*, 119 and 236 note 19. The case study 'analytical mechanics' is also picked up in Mittelstraß, *Rationale Rekonstruktionen*.

[8] A remarkable, though not very influential exception is Lazare Carnot; see his *Principes*. A detailed analysis of Carnot's work can be found in Gillispie, *Lazare Carnot Savant*.

[9] Lakatos makes clear that the dichotomy 'Euclidian / Empiricist' (or later: 'Euclidian / Quasi-empirical') applies for *whole theories*, while single propositions are traditionally qualified as '*a priori* / *a posteriori*' or 'analytic / synthetic': "[...] epistemologists were slow to notice the emergence of highly organized knowledge, and the decisive role played by the specific patterns of this organization" (ibid., 6) This holds true especially for mechanics. The traditional empirical / rationalistic dichotomy conceals the common basis of infallibility and is not very useful historiographically (ibid., 70-103).

[10] In case of Lagrange, the term "Rubber Euclideanism" (ibid., 7, 9) would be more appropriate: see part 4.3 of this paper.

[11] For a detailed discussion see Hanson, *Newton's First Law*.

[12] Ronald N. Giere exemplifies in ch. 3 of his *Explaining Science* that this *classical* demand of 'metatheoretical invariance' is not accepted by modern philosophy of science.

[13] The concept of force in Leibniz's physics is analysed in some detail by Stammel, *Kraftbegriff*.

[14] "Force was an entity ontologically existent in the universe" (Westfall, *Force*, 87).

[15] It has to be kept in mind that *mathematical realism* in my sense only implies the ontological relevance of all concepts which are actually used in mathematical principles. This does not mean, however, that all ontologically relevant concepts enter these principles. The concept of impenetrability, for example, is ontologically relevant for all important programs of the time in question, but does not (and cannot) play a role in its mathematical formulation, because it has no quantitative meaning. It is a concept which later disappears from the textbooks of mechanics, though it is still present in some textbooks of general physics in the first decades of the nineteenth century.

[16] This is probably the reason why it was frequently presented as a model of the 'hypothetical-deductive' concept of science (see, for example, Blake, *Isaac Newton*).

[17] Therefore, I can and will restrict my attention to Newton in this context: I take it for granted that mechanics in the tradition of Cartesian or Leibnizian rationalism is accepted as 'Euclideanistic' in the sense described above.

[18] Remember, for example, the instances given as 'empirical' support of his first law, according to which "every body continues in its state of rest, or of uniform motion in a right line, unless it is compelled to change that state by forces impressed upon it": We find projectiles, "so as far as they are not retarded by the resistance of the air," a rotating top which "does not cease its rotation," and "even the greater bodies of the planets and comets" (Newton, *Mathematical Principles*, 13). There is obviously no *observation* which shows the uniformity and rectilinearity of 'natural' motion.

[19] Newton to Cotes on March 28, 1713 (Newton, *Correspondence*, V 396-397). Newton's statement was provoked by an example which was used by Cotes in order to explicate his foundations of mechanics: "[...] 'till this Objection be cleared I would not undertake to answer any one who should assert You do *Hypothesim fingere* [...]" (ibid., 392). Though Cotes' thought experiment is untenable (and therefore is not discussed here), it should be noted that Newton's rejection relies on the undubitable truth and generality of his *axiomata*.

See, for example, his famous letter to Bentley of December 10, 1692 (Newton, *Correspondence*, III 233). The law of gravitation does not, of course, belong to his axioms in a strict sense. But its certainty is vital for Newton in order to show that his celestial mechanics (presented in Book III of the *Principia*) can be based on a set of certain principles, i.e., his three laws of motion and the law of gravitation.

Newton, *Principia*, 64. Wolfer's German translation ("von den Mathematikern *angenommen*"; Newton, *Mathematische Prinzipien der Naturlehre*, 39) promotes 'conventionalistic' misinterpretations, while the brand-new translation by Volkmar Schüller ("von den Mathematikern *allgemein anerkannt*."; Newton, *Mathematische Prinzipien der Physik*, 40) does justice to the original meaning.

"[...] for the description of right lines and circles, upon which geometry is founded, belongs to mechanics. Geometry does not teach us how to draw these lines, but requires them to be drawn, for it requires that the learner should first be taught to describe these accurately before he enters upon geometry, then it shows how by these operations problems may be solved. To describe right lines and circles are problems, but not geometrical problems. The solution of these problems is required from mechanics, and by geometry the use of them, when so solved, is shown; and it is the glory of geometry that from those few principles, brought from without, it is able to produce so many things" (Newton, *Mathematical Principles*, XVII).

A label already applied to him by Jammer, *Problem des Raumes*, 110; see Burtt, *Metaphysical Foundations* and Strong, *Newton's Mathematical Way*, for similar judgements.

See part 1.3 for the 'non-Kuhnian' implications of this change.

Cassirer, *Erkenntnisproblem*, 472. According to Cassirer the second description was given by a historian of mathematics of his time, but he agrees with this judgement, especially "with respect to the methodological manner of the interpretation and treatment" of scientific problems.

See Hankins, *Jean d'Alembert*, for a detailed discussion of d'Alembert's Cartesian leanings.

Lagrange, *Mécanique Analytique* (2nd ed.), I 243. This passage is not included in the first edition.

To be more precise: the 'explicit' use. It is well known that Newton made use of calculus, but later 'translated' his results in a geometric language in order to facilitate the reception of his *Principia*.

See Euler, *Mechanik*, I 49. I cannot discuss his various 'demonstrations' in this paper.

See Mittelstraß, *Neuzeit*, esp. 301-302; see endnote 7 above, and Pulte, *Mathematische Naturphilosophie*, ch. III.6.

The relevance of Euler's *Découverte* is underlined by Truesdell, *Program* and Pulte, *Prinzip*, esp. 151.

Euler, *Découverte*, 88-89. Later he discovered that the principle of moment of momentum has to be added as a separate 'axiom'.

'Basic concepts' in *this* context always means 'concepts which are used in the actual axiomatisation of mechanics'.

For all details see Pulte, *Prinzip*, 150-181, esp. 176.

As d'Alembert put it in the title of his great textbook: "Traité de Dynamique, dans lequel les loix de l'equilibre & du Mouvement des Corps sont réduites au plus petit nombre possible, & démontrées d'une manière nouvelle, & où l'on donne un Principe général pour trouver le Mouvement de plusieurs Corps qui agissent les uns sur les autres, d'une manière quelconque."

See, for example, Euler's *De la force* and his *Anleitung*.

"Wenn von der *geistigen* oder *logischen Evidenz* als einem Wissenschaftskriterium [klassischer Wissenschaft] gesprochen wird, so soll ein relativ neutraler und umfassender Begriff verwendet werden, da damit ein Komplex mit vielerlei Nuancen gemeint ist. Genau genommen gilt das Wort nur für die frühe Wissenschaft, später tritt mehr und mehr die Idee der *logischen Struktur*, schließlich der logischen Ordnung als *System* an seine Stelle. Im einzelnen ist dazu folgendes zu sagen: Daß Wissen im Sinne des später so verstandenen wissenschaftlichen Wissens keine unmittelbare Evidenz in sich trägt, keine unmittelbare Wahrheit in sich birgt, ist eine implizite Voraussetzung der Theorie—eigentlich bis zur Gegenwart. So stand wie über der episteme der nous, so über [der] scientia als der 'mittelbaren' der intellectus bzw. die intelligentia als die eigentliche unmittelbare Einsicht der letzten Wahrheiten, der Axiome. Durch die Evidenz der Ableitung, der 'De-duktion'—('Apagoge')—wird dann die Sicherheit und Gewißheit der anderen Sätze garantiert. *Die wissenschaftliche Gewißheit und insofern die Wissenschaftlichkeit liegt also nicht so sehr in der ursprünglichen Schau als der gesicherten, d.h. systematischen Ableitung*. Dies wird zunächst unmittelbar gesagt und sinngemäß versucht, die entsprechenden Syllogismusstrukturen als die entsprechenden Wege zu entwickeln. In zunehmendem Maße verlagert sich dann später der Schwerpunkt: er rutscht gewissermaßen 'abwärts' [...]." (Diemer, *Begründung*, 30-31).

38 Note my general use of this technical term: Not all mechanics which uses calculus is called 'analytical' (Euler's *Mechanica*, for example, is not 'analytical' in this sense), but only mechanics in so far as it makes use of *principles*, which are based on analytical *principles*, i.e., integral variational principles (like the principle of least action) or differential variational principles (like the principle of virtual velocities).

39 See Pulte, *Prinzip*, 230-261, for a more detailled discussion of this development.

40 See endnote 4 above; see also part 4 for more details.

41 Here I do not distinguish between Maupertuis' and Euler's formulations, though they do differ in various details. It is noteworthy that Euler and Maupertuis always stressed that they discovered and elaborated the *same* principle.

42 This emerges even from the titles of some of Maupertuis' and Euler's essays on the principle of least action. See, for example, Maupertuis, *Accord de différentes Loix de la Nature qui avoient jusqu'ici paru incompatibles* and *Les Lois du Mouvement et du Repos déduites d'un Principe Metaphysique* and Euler, *Harmonie entre les principes générales de repos et de mouvement de M. de Maupertuis*.

43 Letter to Euler of May 19, 1756 (Lagrange, *Oeuvres*, Vol. 14, 391-392).

44 Euler, for example, frequently makes use of geometrical figures, even if he deals with 'analytical mechanics' (in the narrow sense defined in endnote 38).

45 In order to avoid misunderstandings, I must emphasise that 'instrumentalism' *here* refers to philosophy of nature (I1) rather than to philosophy of science (I2): Lagrange did not base his mathematical formulation of mechanics on an analysis of the fundamental concepts of philosophy of nature like matter, force, space and time, as did Descartes, Newton, Leibniz, d'Alembert, or Euler. Instead, he chose the basic concepts and laws of his theory in a mathematically convenient manner. This is what I call instrumentalism (I1) and which is best illustrated by Lagrange's switch described in part 4.1. In contrast, instrumentalism with respect to philosophy of science (I2) is characterised by the view that the whole theory of mechanics or at least one of its principles is only a tool to describe and predict phenomena without having any *real* content itself. Lagrange certainly would not have accepted being called an instrumentalist in this second sense (see part 4.3, but also part 4.1). He could not, however, have refuted such an ascription: A consistent instrumentalism (I1), which is *not* supported by an adequate theory of representation *inevitably* leads to (I2). Therefore the distinction is *generally* unnecessary, but as Lagrange's view is not consistent in this respect, it has to be kept in mind.

46 That is the main reason why Lagrange's mechanics was sterile in some respects: It contains no truly new principles, nor new concepts of mechanics, as Truesdell and others have justly remarked.

47 The *Méchanique Analitique* "réunira & présentera sous un même point de vue, le différens Principes trouvés jusqu'ici pour faciliter la solution des questions de Méchanique, en montrera la liaison & la dépendance mutuelle, & mettra à portée de juger de leur justesse & de leur étendue." (Lagrange, *Méchanique Analitique*, v)

48 Regardless of its philosophical shortcomings, the *Méchanique Analitique* became for some time a model of how mathematics should be used in physics. It is its advanced mathematical and anti-metaphysical style which made his textbook attractive for working mathematicians like Fourier as well as for positivistic philosophers like Comte (see Fraser, *Lagrange's Analytical Mechanics*). It was widely accepted as the *best realisation* of a 'purely mathematical' Euclideanism in physics.

49 ibid, 8-12. Lagrange uses the history of mechanics partly as a substitute for missing philosophical justification.

50 It was probably brought to his attention by Fourier's *Mémoire* from 1798.

51 From Fourier (1798), de Prony (1798), Laplace (1799), L. Carnot (1803) and Ampère (1806) to Cournot (1829), Gauss (1829), Poisson (1833), Ostrogradsky (1835, 1838), and Poinsot (1806, 1838, 1846). They aimed at an *extension* of Lagrange's principle, taking into account conditions of constraints given by inequalities (Fourier, Cournot, Gauss, Ostrogradsky) and/or at its *better foundation*.

52 In the passage above I reported upon the interpretation of Pulte, *Jacobi's Criticism*, 158-160; see 160-181 for the subsequent development of analytical mechanics, especially with respect to C.G.J. Jacobi.

53 See Diemer, *Begründung*, as well as Diemer and König, *Was ist Wissenschaft?*.

Kant, *Metaphysische Anfangsgründe*, A XIII; see also the mottos of this paper.

See Pulte, *Mathematische Naturphilosophie*, Ch. III.7 for Kant's contribution in the context of the general decline of 'mechanical Euclidianism'.

This gap was—curiously enough—often ignored (see, for example, Gloy, *Die Kantische Theorie*, Schäfer, *Kants Metaphysik*) or belittled. Eric Watkins shows in his The *Laws of Motion* in some detail that Kant's omission was by no means unique in eighteenth-century German attempts to justify the laws of motion. Euler and others, however, tried to give a justification of the second law, and Kant's strong orientation towards the introductory part of Newton's *Principia* also urges toward further explanation. Some notes in his *Opus postumum* suggest that Kant (later) might have regarded the relation between force and motion as a matter of *empirical* investigation. This would mean, however, a serious drawback for his claim to provide a foundation of mathematical philosophy of nature including dynamics, mechanics and phenomenology (in his terms).

See for example Gloy, *Die Kantische Theorie*, Plaass, *Kants Theorie* and Schäfer, *Kants Metaphysik*.

In a certain sense this was, restricted to the area of mechanics, Lagrange's problem, too: His attempt to organise mechanics by means of analytical principles starts with the *fact* that there are a number of accepted laws, but that no order and unity can be found among these laws. This may serve as a second reason for calling this projection 'analytical'.

For a criticism of Kant's approach see Pulte, *Physikotheologie*, esp. 320-327. I have tried to show that Kant's adherent J. F. Fries gave a more satisfying *methodological* solution of this problem (ibid., 327-341).

As Lakatos aptly states: "An Euclidean never has to admit defeat: his programme is irrefutable. One can never refute the pure existential statement that there exists a set of trivial first principles from which all truth follows. Thus science may be haunted for ever by the Euclidean programme as a regulative principle, 'influential metaphysics'" (Lakatos, *Philosophical Papers*, II 6).

Ibid, 5.

REFERENCES

References to publications in academic periodicals usually bear two dates (1748/1750, for example). The first refers to the year when the contribution was read (in the Berlin Academy, for example), the second to the year when the volume in question (of the Berlin *Histoires*, for example) was published. Abbreviations to collected works etc. are added in square brackets (as [AA] in case of Kant's *Gesammelte Schriften*, for example). Quotations and references taken from these editions are indicated by square brackets at the end of the contribution in question (as [AA 2, 63-163] for Kant's *Der einzig mögliche Beweisgrund*).

Alembert, Jean le Rond d'. *Traité de Dynamique*. Paris: David, 1743.

Bailhache, Patrice. "Introduction et commentaire." Louis Poinsot, *La Théorie générale de l'équilibre et du mouvement des systèmes*, ed. P. Bailhache, 1-199. Paris: Vrin, 1975.

Blake, Ralph M. "Isaac Newton and the Hypothetico-Deductive Method." *Theories of Scientific Method: Renaissance through the Nineteenth Century*, ed. E. H. Madden, 119-143. Seattle; London: University of Washington Press, 1966.

Böhme, Gernot. *Alternativen der Wissenschaft*. Frankfurt a. M.: Suhrkamp, 1984.

Bos, Henk J.M. "Mathematics and Rational Mechanics." *The Ferment of Knowledge: Studies in the Historiography of Eighteenth-Century Science*, ed. G.S. Rousseau and R. Porter, 327-355. Cambridge: University Press, 1980.

Burtt, Edwin A. *The Metaphysical Foundations of Modern Physical Science*. London: Routledge & Kegan Paul, 1932.

Carnot, Lazare N.M. *Principes fondamentaux de l'équilibre et du mouvement*. Paris: Deterville, 1803.

Cassirer, Ernst. *Das Erkenntnisproblem in der Philosophie und Wissenschaft der neueren Zeit*, vol. II. 3rd ed. (1922). Darmstadt: Wissenschaftliche Buchgesellschaft, 1974.

Diemer, Alwin. "Die Begründung des Wissenschaftscharakters der Wissenschaften im 19. Jahrhundert – Die Wissenschaftstheorie zwischen klassischer und moderner Wissenschaftskonzeption." *Beiträge zur Entwicklung der Wissenschaftstheorie im 19. Jahrhundert*, ed. A. Diemer, 3-62. Meisenheim a.G.: A. Hain, 1968.

Diemer, Alwin and Gert König. "Was ist Wissenschaft?" *Technik und Wissenschaft*, ed. A. Hermann and Ch. Schönbeck, 3-28. Düsseldorf: VDI Verlag, 1991.

Dugas, René. *A History of Mechanics* (1955). New York: Dover, 1988.

Duhem, Pierre. *The Evolution of Mechanics*. Alphen an den Rijn; Germantown: Sijthoff & Nordhoff, 1980.

Elkana, Yehuda. "Scientific and Metaphysical Problems: Euler and Kant." *Boston Studies in the Philosophy of Science* 14 (1974): 277-305.

Euler, Leonhard. *Opera omnia sub speciis societatis scientiarium naturalium helveticae*. 73 vols. [in progress], Leipzig; Berlin: Teubner and (from 1952) Zürich; Basel: Birkhäuser, 1911-1986.

Euler, Leonhard. *Mechanica sive motus scientia analytice exposita*. 2 vols., Petersburg: Academiae Scientiarum, 1736 [Opera omnia (2)1 and 2].

Euler, Leonhard. *Methodus inveniendi lineas curvas maximi minimive proprietate gaudentes sive solutio problematis isoperimetrici latissimo sens accepti*. Lausanne; Genf: Bousquet, 1744 [Opera omnia (1)24].

Euler, Leonhard. "De la force de percussion et de sa véritable mesure." *Histoire de l'Académie Royale des Sciences et Belles-lettres de Berlin* 1 (1744/1746): 21-35 [Opera omnia (2)8: 27-53].

Euler, Leonhard. *Gedancken von den Elementen der Cörper, in welchem das Lehr-Gebäude von den einfachen Dingen und Monaden geprüfet, und das wahre Wesen der Cörper entdecket wird*. Berlin: Haude und Spener, 1746 [Opera omnia (3)2: 349-366].

Euler, Leonhard. "Réflexions sur l'espace et le tems." *Histoire de l'Académie Royale des Sciences et Belles-lettres de Berlin* 4 (1748/1750): 324-333 [Opera omnia (3)2: 376-383].

Euler, Leonhard. "Découverte d'un nouveau principe de mécanique." *Histoire de l'Académie Royale des Sciences et Belles-lettres de Berlin* 6 (1750/1752): 185-217 [Opera omnia (2)5: 81-108].

Euler, Leonhard. "Harmonie entre les principes générales de repos et de mouvement de M. de Maupertuis." *Histoire de l'Académie Royale des Sciences et Belles-lettres de Berlin* 7 (1751/1753): 169-198 [Opera omnia (2)5: 152-176].

Euler, Leonhard. "Anleitung zur Naturlehre, worin die Gründe zur Erklärung aller in der Natur sich ereignenden Begebenheiten und Veränderungen festgesetzt werden" (posthumously ed.). *Opera omnia* (3)1, 16-178.

Euler, Leonhard. *Theoria motus corporum solidorum seu rigidorum ex primis nostrae cognitionis principiis stabilita et ad omnes qui in huiusmodi corpora cadere possunt accommodata*. Rostock/Greifswald: Röse, 1765 [Opera omnia (2)3 and 4].

Euler, Leonhard. *Mechanik oder analytische Darstellung der Wissenschaft von der Bewegung*. 2 Vols., Greifswald: Koch's Verlagshandlung, 1848 and 1850.

Euler, Leonhard. *Mechanik. Dritter Theil: Theorie der Bewegung fester und starrer Körper*. Greifswald: Koch's Verlagshandlung, 1853.

Fourier, Jean B. "Mémoire sur la statique, contenant la démonstration du principe des vitesse virtuelles, et la théorie des momens." *Journal de l'Ecole Polytechnique* (1)2 Cah. 5 (1798): 20-60.

Fraser, Craig. "J.L. Lagrange's Early Contributions to the Principles and Methods of Mechanics." *Archive for History of Exact Sciences* 28 (1983): 197-241.

Fraser, Craig. "Lagrange's Analytical Mechanics, Its Cartesian Origins and Reception in Comte's Positive Philosophy." *Studies in the History and Philosophy of Science* 21 (1990): 243-256.

Friedman, Michael. *Kant and the Exact Sciences*. Cambridge MA; London: Harvard University Press, 1992.

Gabbey, Alan. "Newton's Mathematical Principles of Natural Philosophy: a Treatise of 'Mechanics'?" *The Investigation of Difficult Things*, ed. P. M. Harman and A. E. Shapiro, 305-322. Cambridge: University Press, 1992.

Giere, Ronald N. *Explaining Science. A Cognitive Approach*. Chicago; London: The University of Chicago Press, 1988.

Gillispie, Charles C. *Lazare Carnot Savant*. Princeton NJ: University Press, 1971.

Gloy, Karen. *Die Kantische Theorie der Naturwissenschaft*. Berlin: De Gruyter, 1976.

Grabiner, Judith V. *The Calculus as Algebra: J.-L. Lagrange, 1736-1813*. New York; London: Garland Publishing, 1990.

Grattan-Guinness, Ivor. "Recent Researches in French Mathematical Physics of the Early Nineteenth Century." *Annals of Science* 38 (1981): 663-690.

Grattan-Guinness, Ivor. *Convolutions in French Mathematics. 1800-1840.* 3 vols., Basel; Boston; Stuttgart: Birkhäuser, 1990.

Hankins, Thomas L. *Jean d'Alembert. Science and the Enlightenment.* Oxford: Clarendon Press, 1970.

Hanson, Norwood R. "Newton's First Law: A Philosopher's Door into Natural Philosophy." *Beyond the Edge of Certainty*, ed. R. G. Colodny, 6-28. Englewood Cliffs, NJ: Prentice-Hall, 1965.

Hertz, Heinrich. *The Principles of Mechanics. Presented in a New Form*, ed. by R. S. Cohen. New York: Dover, 1956.

Jammer, Max. *Das Problem des Raumes. Die Entwicklung der Raumtheorien.* 2nd ed., Darmstadt: Wissenschaftliche Buchgesellschaft, 1980.

Kant, Immanuel. *Gesammelte Schriften*, ed. by (Königlich) Preußische Akademie der Wissenschaften and (Deutsche) Akademie der Wissenschaften (der DDR). 29 vols., Berlin; Leipzig: Akademie-Verlag, 1910-1983 [AA].

Kant, Immanuel. *Werke in zehn Bänden*, ed. by W. Weischedel. Darmstadt: Wissenschaftliche Buchgesellschaft, 1983 [Werke].

Kant, Immanuel. *Der einzig mögliche Beweisgrund zu einer Demonstration des Daseins Gottes.* Königsberg: Kanter, 1763 [AA 2, 63-163].

Kant, Immanuel. *Metaphysische Anfangsgründe der Naturwissenschaft.* Riga: Hartknoch, 1786 [AA 4, 465-565].

Kant, Immanuel. *Kritik der reinen Vernunft.* 2nd ed., Riga: Hartknoch, 1787 [AA 3].

Kant, Immanuel. "Erste Fassung der Einleitung in die Kritik der Urteilskraft." *Werke* 8, 171-232.

Kant, Immanuel. *Kritik der Urtheilskraft.* Riga: Lagarde und Friederich, 1790 [AA 5, 165-485].

Kuhn, Thomas L. *The Structure of Scientific Revolutions.* 3rd ed., Chicago: The University of Chicago Press, 1996.

Lagrange, Joseph-Louis. *Oeuvres*, ed. J.A. Serret and G. Darboux. 14 vols., Paris: Gauthier-Villars, 1867-1892.

Lagrange, Joseph-Louis. "Application de la méthode exposée dans le mémoire précédent à la solution de différens Problèmes de Dynamique." *Miscellanea Taurinensa* 2 (1760/1762): 196-268 [Oeuvres 1: 365-468].

Lagrange, Joseph-Louis. *Méchanique Analitique.* Paris: Desaint, 1788.

Lagrange, Joseph-Louis. "Sur le principe des vitesses virtuelles." *Journal de l'École polytechnique* (1)2 Cah. 5 (1798): 115-118 [Oeuvres 3: 315-321].

Lagrange, Joseph-Louis. *Théorie des fonctions analytiques.* Nouvelle édition, revue et augmentée par l'auteur. Paris: Bachelier, 1813.

Lagrange, Joseph-Louis. *Mécanique Analytique*, revue, corrigée et annotée par J. Bertrand. 3rd ed., Paris: Courcier, 1853 and 1855 [Oeuvres 11 and 12].

Lagrange, Joseph-Louis. *Analytical mechanics*, transl. A. Boissonnade and V.N. Vagliente. Dordrecht; Boston; London: Kluwer Academic Publishers, 1997.

Lakatos, Imre. *Philosophical Papers*, ed. J. Worrall and G. Currie. 2 vols., Cambridge: University Press, 1978.

Mach, Ernst. *Die Mechanik in ihrer Entwicklung. historisch-kritisch dargestellt.* 9th ed. (1933). Darmstadt: Wissenschaftliche Buchgesellschaft, 1982.

MacLaurin, Colin. *An Account of Sir Isaac Newton's Philosophical Discoveries* (1748). Hildesheim; New York: Olms, 1971.

Maupertuis, Pierre L. M. de. *Oeuvres.* 4 vols. (Lyon 1768), Hildesheim; New York: Olms, 1974.

Maupertuis, Pierre L.M. de. "Accord des différentes Loix de la Nature qui avoient jusqu'ici paru incompatibles." *Mémoires de l'Académie Royale des Sciences de Paris* (1744/1748): 417-426 [with changes in: Oeuvres 4: 3-28].

Maupertuis, Pierre L.M. de. "Les Lois du Mouvement et du Repos déduites d'un Principe Métaphysique." *Histoire de l'Académie Royale des Sciences et Belles-lettres de Berlin* 2 (1746/1748): 267-294 [with changes in: Oeuvres 4, 31-42].

Mittelstraß, Jürgen. *Neuzeit und Aufklärung. Studien zur Entstehung der neuzeitlichen Wissenschaft und Philosophie.* Berlin/New York: De Gruyter, 1970.

Mittelstraß, Jürgen. *Die Möglichkeit von Wissenschaft.* Frankfurt a. M.: Suhrkamp, 1974.

Mittelstraß, Jürgen (1981). "Rationale Rekonstruktion der Wissenschaftsgeschichte." *Wissenschaftstheorie und Wissenschaftsforschung*, ed. P. Janich, 89-148. München: Aspekte Verlag.

Newton, Isaac. *The Correspondence*, ed. by H.W. Turnbull et. al. 7 vols., Cambridge: University Press, 1959-1977.

Newton, Isaac. *Mathematische Prinzipien der Naturlehre* (1872). Darmstadt: Wissenschaftliche Buchgesellschaft, 1963.

Newton, Isaac. *Mathematical Principles of Natural Philosophy and His System of the World* (1729), ed. F. Cajori. 2 vols. (1934) Berkeley; Los Angeles; London: University of California Press, 1982.

Newton, Isaac. *Opticks: Or a Treatise of the Reflections, Refractions, Inflections Colours of Light*. 4th ed. (1730), ed. by I. B. Cohen. New York: Dover, 1952.

Newton, Isaac. *Philosophiae naturalis principia mathematica*. 3rd ed. (1726), ed. by A. Koyré and I.B. Cohen, 2 vols., Cambridge MA: Harvard University Press, 1972.

Newton, Isaac. *Die Mathematischen Prinzipien der Physik*, ed. by V. Schüller. Berlin; New York: De Gruyter, 1999.

Plaass, Peter. *Kants Theorie der Naturwissenschaft*. Göttingen: Vandenhoeck & Ruprecht, 1965.

Popper, Karl R. *Logik der Forschung*. 7th ed., Tübingen: Mohr (Siebeck), 1982.

Pulte, Helmut. *Das Prinzip der kleinsten Wirkung und die Kraftkonzeptionen der rationalen Mechanik. Eine Untersuchung zur Grundlegungsproblematik bei L. Euler, P. L. M. de Maupertuis und J. L. Lagrange*. Stuttgart: Steiner, 1989.

Pulte, Helmut. "Jacobi's Criticism of Lagrange: The Changing Role of Mathematics in the Foundations of Classical Mechanics." *Historia Mathematica* 25 (1998): 154-184.

Pulte, Helmut. "Von der Physikotheologie zur Methodologie. Eine wissenschaftstheoriegeschichtliche Analyse der Transformation von nomothetischer Teleologie und Systemdenken bei Kant und Fries." *Jakob Friedrich Fries. Philosoph, Naturwissenschaftler und Mathematiker*, ed. W. Hogrebe and K. Hermann, 301-351. Frankfurt a.M.: Peter Lang, 1999.

Pulte, Helmut. *Mathematische Naturphilosophie im Übergang. Eine wissenschaftstheoriegeschichtliche Untersuchung zum Verhältnis von Axiomatik und Empirie von Newton bis Neumann* [forthcoming].

Schäfer, Lothar. *Kants Metaphysik der Natur*. Berlin: De Gruyter, 1966.

Stammel, Hans. *Der Kraftbegriff in Leibniz' Physik*. Mannheim: Diss. Univ., 1980.

Strong, Edward W. "Newton's Mathematical Way." *Journal of the History of Ideas* 12 (1951): 90-110.

Szabó, István. *Geschichte der mechanischen Prinzipien und ihrer wichtigsten Anwendungen*. Basel; Boston; Stuttgart: Birkhäuser, 1979.

Truesdell, Clifford A. "A Program toward Rediscovering the Rational Mechanics of the Age of Reason." *Archive for History of Exact Sciences* 1 (1960): 1-36.

Truesdell, Clifford A. "The Rational Mechanics of Flexible or Elastic Bodies: 1638-1780". Introduction to *Leonhardi Euleri opera omnia* Vol. X et XI seriei secundae. Turici: Orell Füssli, 1960.

Truesdell, Clifford A. *Essays in the History of Mechanics*. Berlin; Heidelberg; New York: Springer, 1968.

Voss, Aurel. "Die Prinzipien der rationellen Mechanik." *Encyclopädie der mathematischen Wissenschaften mit Einschluß ihrer Anwendungen*, vol. 4(1), 1-121. Leipzig: Teubner, 1901.

Watkins, Eric. "The Laws of Motion from Newton to Kant." *Perspectives on Science* 5 (1997): 311-348.

Watkins, Eric. "Kant's Justification of the Laws of Mechanics." Studies in the *History and Philosophy of Science* 29 (1998): 539-560.

Westfall, Richard S. *Force in Newton's Physics. The Science of Dynamics in the Seventeenth Century*. London: MacDonald and New York: Elsevier, 1971.