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Rational Mechanics in the Eighteenth Century. On Structural Developments of a Mathematical Science*

Zusammenfassung: Rationale Mechanik im 18. Jahrhundert. Zur strukturellen Entwicklung einer mathematischen Wissenschaft. Die Rolle der Mathematik in den Wissenschaften sowie in der Wissenschaftsphilosophie des 18. Jahrhunderts kann kaum hoch genug veranschlagt werden. In neueren wissenschaftsphilosophischen Darstellungen wird diese Rolle jedoch häufig in einer anachronistischen Weise beschrieben und analysiert, indem jüngere Auffassungen über formale Mathematik einerseits und empirische Wissenschaft andererseits in die Vergangenheit projiziert werden: Man mag vom heutigen Standpunkt aus tatsächlich versucht sein zu sagen, dass Philosophen des 17. und vor allem des 18. Jahrhunderts die Bedeutung der Mathematik für die ‚Repräsentation' physischer Phänomene oder als ‚Instrument' der deduktiven Erklärung und Voraussage erkannten. Solche Modernismen gehen jedoch am eigentlichen Punkt vorbei, nämlich der Tatsache, dass es für die mechanische Philosophie per se eine ‚mathematische Natur der Natur' gibt. Hinzu kommt, dass sich das Verständnis von dieser ‚mathematischen Natur' im Verlauf des 18. Jahrhunderts aus verschiedenen (mathematischen, philosophischen und anderen) Gründen dramatisch veränderte. Dieser Sachverhalt wurde in früheren philosophischen Analysen der fraglichen Entwicklung kaum wahrgenommen. Heutige Wissenschaftsphilosophie aber sollte der Wissenschaftsgeschichte eine historisch genauere Analyse anbieten, ohne darüber ihren – von Historikern durchaus nicht immer geschätzten – Auftrag aufzugeben, grundlegende Konzepte und Methoden der fraglichen Wissenschaft, die für ihr Verständnis von Bedeutung sind, offenzulegen. Der folgende Beitrag gibt eine ‚strukturelle Skizze' über die Entwicklung der rationalen Mechanik von Newton bis Lagrange. Er versucht dabei dem Sachverhalt Rechnung

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zu tragen, dass rationale Mechanik im 18. Jahrhundert primär als eine mathematische Wissenschaft verstanden wurde. Ausgehend von diesem mathematischen Status versucht er auch, gute Gründe für den grundlegenden Wandel ihrer Wissenschaftsauffassung im fraglichen Zeitraum zu liefern.

Summary: Rational Mechanics in the Eighteenth Century. On Structural Developments of a Mathematical Science. The role of mathematics in eighteenth-century science and of eighteenth-century philosophy of science can hardly be overestimated. However, philosophy of science frequently described and analysed this role in an anachronistic manner by projecting modern points of view about (formal) mathematics and (empirical) science to the past: 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. But such modernisms are missing the central point, i.e. the 'mathematical nature of nature' according to mechanical philosophy. Moreover, the understanding of this mathematical nature changed dramatically in the course of the eighteenth century for various (i.e. mathematical, philosophical and other) reasons – a fact hardly appreciated by former philosophical analysis. Philosophy of science today should offer a more accurate analysis to history of science without giving up its task – not always appreciated by historians – to uncover the basic concepts and methods which seem relevant for the understanding of science in question. This paper gives a 'structural account' on the development of rational mechanics from Newton to Lagrange that tries to give justice to the fact that rational mechanics in the eighteenth century was primarily understood as a mathematical science and that – starting from this understanding – also tries to give good reasons for the fundamental change of the concept of science that took place during this period.

Keywords: rational mechanics, mathematical science, eighteenth century, Newton, Lagrange

Schlüsselwörter: rationale Mechanik, mathematische Wissenschaft, 18. Jahrhundert, Newton, Lagrange

1. Introduction

Historians and philosophers of science largely agree about the fundamental importance of mathematics for the shaping of science in the later seventeenth and in the eighteenth century. But there is less agreement about the questions what this role exactly was and why it worked as a model of science for philosophers and scientists both in the tradition of rationalism and empiricism. The modern view tends to stress mathematics as a 'tool' of representation of physical phenomena and as a device for their deductive explanation and (or) prediction from general laws.

According to this picture, the rise of mathematical physics is a kind of 'epi-phenomenon' of the rise of the new experimental sciences, and the genuine mathematical part of physics is understood as a methodologically directed, constructive enterprise that somehow depends on experimental and observational data. This picture, however, does not do justice to the essential mathematical character of nature according to mechanical philosophy, which forms the basis of all advanced research programs of rational mechanics in the eighteenth century.

Application of mathematics in mechanical philosophy is a very colourful and heterogeneous business, reaching from the quantitative measurement of 'primary qualities' to the deductive organisation of whole theories. In the context of mechanical philosophy even the very idea of 'applying' mathematics to natural objects is misleading to a certain extent, as far as basic quantifications are involved.[1]

This paper, however, will not focus on mathematics at this basic level, but on the meaning of mathematics for rational mechanics in the context of theory-building: It will try to outline some developments and features of rational mechanics as a mathematical science, which seem to me important in order to understand why and how the very concept of science changed in the century from Isaac Newton's *Principia* (1687) to Joseph-Louis Lagrange's *Mécanique Analytique* (1788). A 'structural sketch' like this, which is meant to characterize a whole century of spirited development, will probably provoke the criticism of historians of science, who are tempted to 'falsify' general statements by presenting (at least) one exception to any general claim. But both history and philosophy of science are in need of 'synthetic' attempts from time to time: No historical knowledge without general claims, no progress of historical knowledge without such claims and the provocation of counter-examples which, of course, do exist. Exceptions, however, do not 'falsify' in historical theory, but prove the rule. It seems more interesting to analyse, for example, why Christiaan Huygens – one of the most eminent mathematicians and natural philosophers of the eighteenth century – does not fit well in the picture I am going to draw in this article than not to see the wood for the trees.

In order to avoid excess length of this paper, illustrating examples are widely omitted.[2] I will start with some general remarks on mathematics and mechanics under the premise of mechanism, which lead to an explication of the central question of this paper. Subsequently, I will take a short look at Newton's *Principia* (1687) as one – and the most important – starting point of the development in question. The fourth and largest part of this paper attempts to give a characterisation of the development of rational mechanics in the course of the eighteenth century. Finally, I deal with Lagrange's *Mécanique Analytique* (1788) and contrast it with earlier foundations of rational mechanics.

2. Mechanism, mathematics, and rational mechanics

Under mechanism, the primary aim of natural philosophy was the determination of the motion of material particles under different physical conditions. Motion itself being regarded as a genuine mathematical concept, natural philosophy had to be not only an experimental, but also a mathematical science. If the original idea of motion is taken seriously, the attribute 'mathematical' does not mean 'mathematics applied to science' but rather 'science, having essentially to do with mathematical entities', i.e. with space, time, quantifiable matter, and its changing space-time-relations.

For this reason 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 deals with natural motions and aims at uncovering their primary laws. Newton made this intention quite clear by the full title of his *Principia*. It was, however, the label 'mechanica rationalis' from the preface of his masterpiece that became prominent later:

The ancients considered mechanics in a twofold respect; as rational, which proceeds accurately by demonstration, and practical. […] rational mechanics 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 […].[3]

"Consider philosophy rather than arts": Newton's use of 'mechanica rationalis' underlines his foundational claims with respect to the mathematical principles of natural motion. It is important to note that at this early stage – and also in the course of the eighteenth century – rational mechanics never was considered as an artificial or purely formal 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. This does still hold for rational mechanics in its advanced analytical form which was mainly developed by Leonhard Euler, Jean Baptiste d'Alembert and Lagrange – though with the latter a turning point will be reached, as I am going to show. Within the frame of rational mechanics, mathematical symbols and even the most abstract mathematical formulas refer to matter and motion as essential elements of physical reality. In short, mathematics in the context of rational mechanics is 'semantically laden'.

A second characteristic 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 Imre Lakatos' sense.[4] It is understood as the best example of an axiomatic-deductively established mathematical science with empirical content next to geometry. I will here confine myself to three of the most important features of this ideal: Its first principles are not only true, but certainly true, i.e. they are infallible with respect to empirical 'anomalies'. Secondly, Euclideanism is epistemologically neutral, i.e. it includes both empirical and rationalistic foundations of the science in question.[5] First principles can be revealed by 'the light of reason' (as in René Descartes, for example) or they can be 'deduced from phenomena' (as in Newton, for example). Thirdly, Euclideanism is anti-pluralistic. There is one (and only one) true mathematical science of nature, and it is defined by their mathematical principles or axioms.

Newton, in his *Principia*, uses a noteworthy phrase for his first mathematical principles, which makes explicit its twofold task with respect to mechanism and to Euclideanism: "axiomata sive leges motus".[6] As 'leges motus', these principles act as empirical laws of nature which govern the behaviour of (possibly all) material bodies; they explain phenomena. As 'axiomata', they act as first principles of the theory of mechanics. They govern the known special laws and examples of mechanics; they organise the whole body of mechanical knowledge in a deductive manner.

From a philosophical point of view it is, however, by no means evident that primary laws of nature are also 'prime candidates' for axioms of a deductively organized theory. Nor is it clear whether such a 'metatheoretical coincidence' should be possible at all: Characteristics of natural laws, in this historical context, are truth, empirical generality, explanatory power with respect to the phenomena, and a certain intuitivity and evidence with respect to the underlying scientific metaphysics. In some systems, like that of Descartes or of Gottfried Wilhelm Leibniz, they are even regarded to be epistemologically necessary. On the other hand, characteristics of first principles or 'axioms' are truth, evidence, generality and deductive power with respect to the other laws of mechanics. Moreover, they are thought to be neither provable by other propositions nor – due to their evidence – to be in need of such a proof. It has to be kept in mind that the traditional meaning of 'axiom' is at work here.

Obviously, the two sets of conditions have features in common, but they do not coincide. This is an important point of my argument: Laws have to explain nature, axioms have to organize theories. It is by no means clear that both demands are granted by the same principles, and it will be shown that – in the course of the eighteenth century – it becomes increasingly difficult to satisfy both demands by the same principles. This tension creates a fundamental problem for mechanical Euclideanism at the end of the eighteenth century.

A third feature, brought about by divergent scientific metaphysics under the premise of mechanism, is of equal importance: Albeit the fact that Euclideanism as leading ideal of science is 'anti-pluralistic', the practice of rational mechanics in the first half of the eighteenth century is pluralistic in character: It is shaped by at least three competing programs, each being Euclideanistic in the sense described above, and driven by different metaphysical commitments about the 'nature' of matter, space, time, and force. One has to distinguish between 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's dynamics, based on laws of impact and the conservation of 'vis viva',[7] and backed by a quite complex metaphysics of primitive and derivative forces.[8]

Without any doubt, Newton's *Principia* was most successful in empirical respect. It was, however, neither unique in its intention, nor was it complete or faultless in its execution, nor was it understood as 'revolutionary' by the first generation of its readers, as far as the principles of mechanics are in question.[9] That Newton laid down principles which are sufficient to solve all problems of mechanics is still a popular belief, but this belief is simply not true. As far as the foundations of rational mechanics are at stake, the great 'Newtonian revolution' did never take place. Rejecting Thomas Kuhn's image of rational mechanics, but picking up his terminology, one might say: With regard to the foundations of rational mechanics, the first half of the eighteenth century was not 'normal', because the seventeenth century was not 'revolutionary'. Rather, this period shows features of a 'revolution in permanence'. During such a period, however, formal elements of science gain a peculiar quality and relevance: While a 'conceptual discourse' across the boundaries of actual scientific metaphysics is hardly possible, mathematics becomes even more important as a means of scientific exchange. This does not mean to share the somehow naive view that mathematics works as a kind of 'meta-language', capable of solving even fundamental dissent about the foundations of rational mechanics. It means instead that mathematics played a key role in making accessible the results – the empirically testable outcomes – 'at the bottom' of one research program of mechanics to the other programs. It also means that mathematics was indispensable for integrating those parts which seemed valuable and that it was even the only means in order to formulate overarching principles from which all the accepted laws of mechanics – whether they emerged from the 'own' research program or not – could be derived. In short, scientific metaphysics tends towards a separation, mathematics tends towards an integration of different programs. And at the end of the eighteenth century, we have one system, which represents nearly all the accepted 'mathematical principles of natural philosophy', i.e. Lagrange's *Mécanique Analytique*. How could this integration happen? What was the price that had to be paid for this integration? And how did it change the understanding of rational mechanics as both mathematical and empirical science? In general, I will argue that there was a growing tension between the different demands which rational mechanics should fulfil and that this tension preludes a dissolution of Euclideanism as an overarching ideal for rational mechanics at the end of the eighteenth century.

3. The case of Newton's *Principia* (1687)

To what extent does Newton's program fit to the characteristics of mechanical Euclideanism? 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, corollas etc. – makes this quite clear. On the other hand, it is, of course, easy to see that the definitions and '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[10] further propositions (laws of forces, for example), concrete examples etc. and then uses the 'axiomata' in order to derive conclusions which are empirically testable.

But it has to be stressed that the empirical verification (or falsification) only aims at the hypothesis introduced and not at the 'axiomata'. Classical empiricism, as represented by Newton, does not 'automatically' imply fallibility of laws or even of first principles.

This thesis is supported by Newton's methodological reflection on his 'axioms': It is interesting to note that Newton is pretty cautious with statements about the axiomatic status of his laws of motion. His whole methodology seems to contain but one positive instruction of what to do when an inductive generalisation ("Conclusion") is in conflict 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.[11]

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 generalisation ("Conclusion") are undisputable true. But Newton's consequence – the restriction of the range of applicability by enumeration of 'exceptions' – bears a special problem in the case of his axioms: According to his empiricist methodology, they can work as axioms only if they are most general, or even of unrestricted generality. On the other hand, they should allow restrictions, if we take his methodology seriously. But what Newton really does, in contrast to his methodology, is to immunize his axioms not only from falsification, but also from restriction: "As in Geometry [...] so in experimental Philosophy" 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 falsifying (or better: 'restricting') phenomenon he declares that "there is no such phaenomenon in all nature".[12]

Obviously there is an antagonism between Newton's empiricist methodology and his actual attitude towards the axiomata: He claims that they are most general results of induction, and therefore can be understood as universal laws of nature. But he actually introduces a set of ingeniously chosen mathematical principles which function as axioms for the deductive structure of the *Principia*. In fact, asserted truth is 'injected' into rational mechanics not from the bottom, but from the top, and its flow down to the level of phenomena cannot be turned round by conflicting observations. For Newton, the material truth of axioms, inundating the whole system of propositions, stems from mathematics itself. His 'mathematical realism'[13] is at the core of what may be described as a 'semi-empirical' variant of mechanical Euclideanism: His ontology of 'absolute, true and mathematical space' and 'absolute, true and mathematical time' is indispensible for his foundation of mechanics, though not backed by his empiricist methodology.

It seems worth mentioning that Newton and the Newtonians of the first generation were as certistic as their rationalist rivals: Their aim was, according to Colin MacLaurin, "to proceed with perfect security, and to put an end for ever to disputes".[14] Newton's letters and the context of his famous 'hypothesis non fingo' make obvious that his version of certism is directed against the rationalist versions of Descartes and Leibniz, claiming that the axioms of 'Experimental Philosophy' are well-grounded, while those of 'speculative philosophy' are mere metaphysical fictions.[15] One question – which is dealt with in the next part – is how Newton's Euclideanism bears upon the understanding of rational mechanics in the context of these 'alien' Euclideanisms, being part of the rationalist tradition.

4. Characteristics of rational mechanics in the eighteenth century

It has often been claimed that Newtonian mechanics made its way on the continent despite all philosophical resistances because it was empirically extremely successful, especially in the field of celestial mechanics. This argument is certainly relevant for Newton's gravitational theory, 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 working philosopher-scientists in this period (like Euler or d'Alembert, for example). Secondly, it is applicable only in favour of the Newtonian program, but not in the case of its drawbacks. How to explain, for example, that Pierre L.M. De 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?[16] How to explain the general tendency towards general principles without causal claims like the principle of least action or of virtual velocities in the later eighteenth century? And how to explain the rise of conservational laws in rational mechanics, which have no foundation in Newton's scientific metaphysics? Questions like these cannot be answered in a satisfying manner by referring to Newton's 'empirical success', nor by the mere claim that a general epistemological switch from 'rationalism to empiricism' took place. We need to regard the practice of mathematical physics under the premise of diverging forms of Euclideanism in order to understand these features.

4.1 Inflation of principles

What characterizes rational mechanics above all in the second third of the eighteenth century is an inflation of mathematical principles: Numerous so-called principles of statics which had to be integrated into a general science of mechanics, the principle of the conservation of momentum 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, and so on – not to mention all the 'principles' of continuum and fluid mechanics which had to be coordinated to the rational mechanics of mass-points.[17] These laws were brought about by the three different programs of rational mechanics, they grew out of the study of special problems and idealised physical situation, which gained their relevance for a mathematical theory of nature by the respective scientific metaphysics. They were confirmed by applications to different problems, and often gained their status as 'principles' by this applicability to different classes of phenomena alone. They were not 'deduced' from higher principles (like the principle of sufficient reason in Leibniz's mechanics, for example), nor were they 'deduced' from phenomena (in the sense of Newton's inductivism), but they revealed their relevance by their (possibly limited) deductive and explanatory power. In brief, their status as a 'principle' was neither due to metaphysical nor to empirical foundation, but due to the deductively proceeding practice of mathematical physics alone.

But Euclideanism – the dominating ideal of science[18] – could not 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 and competing scientific programs, unity is the aim of Euclideanism.[19] If a possibly 'basic' principle of one program turns out to be of (probably limited) deductive power for a different one, it has to be integrated in the deductive structure of the latter program, thereby 'explaining' its applicability. Both the "mania of demonstration"[20] in this period, rightly stated by Ernst Mach, and the fact that it was sometimes unclear that "something must be assumed"[21], rightly stated by Clifford A. Truesdell, illustrate the efforts made in order to reach systematic order and, at the same time, the fact that the bindings toward the actual scientific metaphysics worked loose.

4.2 An illustration: conservation laws in Euler

The 'removal' of mathematical principles, which deductively organize mechanics, and scientific metaphysics, in which originally evidence and 'necessity' of first principles were rooted, should be illustrated by at least one concrete example, which is taken from Euler's contribution to rational mechanics. Euler is probably the most prolific mathematician of the eighteenth century; his contributions to the development of rational mechanics can hardly be overestimated.[22] With respect to the development of new concepts and principles of rational mechanics he is the most important figure in the middle of the century. My illustration deals with the relation of his scientific metaphysics, which is strongly influenced by his Cartesian matter-theory, and his mathematical formalism of rational mechanics, which by various historians of science was and is described as a 'Newtonian' one.

Contrary to the historical wisdom of Peter G. Tait and others, it goes without saying today that conservation laws as 'principles' have no place in Newton's program. It is less well known that they were also alien to Euler's scientific metaphysics. Euler was very suspicious that 'vis viva' or 'momentum' (i.e. impulse, to use the modern word) – if they were understood as basic concepts of mechanics and, at the same time, as magnitudes which are conserved in all mechanical processes – are introduced as 'essential forces' or 'active principles' into rational mechanics, which contradicts his Cartesian theory of 'passive' matter.[23] But Johann I. Bernoulli and other 'Leibnizians' convinced him that the concept of 'vis viva' is of considerable relevance in order 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 general impulse as a derived concept, i.e. as time integral of Newtonian force.[24] Problems of conservation of 'vis viva' or impulse were thus transformed into problems of Newtonian mechanics and, in a way, of formal mathematics: When does an 'integrable' force function exist? It depends on the answer of this question, in which (special) case the famous vis viva-controversy can be decided in favour of Leibniz or not. This problem, 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 ("logomachies").[25] Conservation of 'vis viva', an axiom of Leibniz's mechanics, and conservation of impulse, in nuce an axiom of Descartes' mechanics, are no longer axioms or principles in Euler's program, but they become derived laws, which can be used in special mechanical cases in order to explain certain classes of physical phenomena. A 'Newtonian', external and directive force, however, becomes necessarily a primitive concept of Euler's mathematical formalism, though he did never accept 'essential' forces in his theory of matter, i.e. at an ontological level. This tension between formalism and scientific metaphysics results from Euler's striving for a deductive organisation of principles of mechanics which belong to different research programs. Later, he tried to dissolve this tension by a 'matter-theoretical' interpretation of his principle of least action.[26]

4.3 Metatheoretical 'decline of the centre of gravity'

The illustrative example above highlights the importance of a 'Newtonian' conceptual framework for Euler's program, though Euler did not accept important parts of

Newton's scientific metaphysics: The relation of metaphysical foundation and mathematical formulation of mechanics becomes ambiguous. This, however, cannot be pursued further in this paper – nor the immediate empirical success which Euler gained with this framework.

Here, only the consequences of a deductive organisation of the whole body of mechanical knowledge is emphasized: It is not sufficient to have 'certain and evident' axioms. It must also be shown that the mechanical knowledge accepted as true (for empirical reasons) falls under these axioms. To use a 'quasi-Lakatosian' picture: It is not sufficient to introduce 'truth from the top' by intuitive and indubitable mathematical axioms; it is important to be able to 'lead truth down to the bottom'. This is a characteristic feature of Euclideanism in an advanced stage of rational mechanics: It focuses not so much on how to come to evident and certain axioms at the top, but on the deductive structure of the growing body of mechanical knowledge. Alwin Diemer used in a more general context the metaphor of the "decline of the centre of gravity"[27] in order to illustrate such a 'structural' development within mature classical science that tends to become modern science.

In the course of the eighteenth century, a lot of mathematical and conceptual work has been done in order to build 'truth-preserving channels' for the deductive structure of rational mechanics.[28] The calculus of variation is an excellent example of a mathematical device which serves this purpose (and which, by the way, was developed for this reason). The outcome of this process was, as becomes visible already in Euler's Oeuvre, a hierarchical organized system, including elements of the different programs of rational mechanics, but 'crowned' by Euler's transformation of Newton's three laws of motion. But the 'decline of the centre of gravity' is already present here: What counts is the truth of the whole body of mechanical knowledge, which is rather 'represented' now by its axioms in a formal way than 'condensed' in these axioms in a material way. This means that the traditional, Euclidean meaning of 'axiom' is changing in the process of conserving Euclideanism as an ideal for the whole body of mechanical knowledge.

4.4 Decline of philosophical foundations

From this important shift results a growing independence of formalised mathematical physics from philosophical foundations of its principles, regardless whether these foundations are 'empirical' or 'rational' in character. It is the deductive power of principles rather than their empirical or rational basis, their formal axiomatic status rather than their status as a law of nature, their formal truth rather than their material truth, which become important. To make another loan of the former picture: If the deductive channels are already filled with truth, and the truth flow down to the phenomena can be guaranteed, the source of truth becomes less important. Euclideanism continues to be the ideal of science, but it becomes a syntactical rather than a semantical concept of science.

The 'syntactical turn' of rational mechanics in the course of the later eighteenth century is reflected by two main features. There is a conspicuous decline of methodological and metaphysical discussions and – at the same time – a rise of efforts to come to a deductive organization by appropriate mathematical techniques. The great controversies about the nature of space and time, the status of gravitation, the existence of conservation entities belong to the first half of the century rather than to the second half, while ‘technical’ discussions about the calculus of variations, potential theory and differential equations were more prominent in the second half than in the first. Rational mechanics transforms from a mathematical philosophy of nature, which is still in need for philosophical justification, to a self-sufficient mathematical representation of mechanical knowledge. Metaphysical regresses to the principle of sufficient reason, to the principle of identity, to the (alleged) ‘essence’ of matter or to teleology become increasingly unpopular in the second half of the eighteenth century. Most working mathematicians simply refer to empirical success to justify the respectively used principles of mechanics.[29]

4.5 Analytical principles of mechanics

This general development is perhaps best illustrated by the ‘analytical mechanics’,[30] to which Euler, d’Alembert and Lagrange contributed substantially. The rise of analytical principles like those of least action or of virtual velocities cannot be understood by the Mach-Kuhnian pattern of rational mechanics as ‘normal science’ in the tradition of Newton’s *Principia*. 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. The principle of least action, for example, was understood as an alternative to Newton’s foundation of mechanics by Maupertuis as well as by Euler. Only later, with Lagrange, it was interpreted as a merely formal alternative to a ‘Newtonian’ axiomatization of mechanics, i.e. as a part of ‘normal Newtonian science’ in Kuhn’s sense.[31]

The rise of analytical principles is accompanied by a semantical unloading of their basic mathematical concepts at the ‘top’ of mechanics, as might be illustrated by the concept of action in Euler and Maupertuis or the concept of virtual displacement in d’Alembert and Lagrange. There is a close relation between the process of ‘semantical unloading’ and the changing function 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. The applicability to a large number of heterogeneous problems was unique in the history of mechanics. This led to the view that principles like those of least action can work as an organising principle of the whole of mechanics, i.e. as principles from which a great variety of special laws of motion and rest can be deduced.[32] 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 typical of the development of analytical mechanics in general: Its rise in the second half of the century highlights the striving of Euclideanism for an axiomatic-deductive organisation of science. But it has to be noticed that in the course of this process an important change takes place in so far as principles become formal axioms which deductively organise a whole theory rather than laws of nature which are meant to describe and explain concrete phenomena. Lagrange’s mechanics is most significant in this respect.

5. The case of Lagrange's *Mécanique Analytique* (1788)

In a certain sense, the *Mécanique Analytique* is the high point and, at the same time, the final point of the development sketched so far. In a different way, however, it also marks a break with the older tradition, thereby revealing the basic philosophical problems of mechanical Euclideanism. Lagrange's approach can be described as a 'Newtonian' one with respect to its underlying scientific metaphysics.[33] His genuine philosophy of science in a narrow sense, however, was strongly influenced by d'Alembert and Euler. As both his predecessors, he wanted to base mechanics on certain, evident and perhaps even 'necessary' principles. His commitment to mechanics as an essentially mathematical enterprise in the traditional Euclidean sense is perhaps expressed best in this famous phrase from the introduction of his *Théorie des fonctions analytiques*: "Mechanics can be understood as a geometry with four dimensions, and the analysis of mechanics can be understood as an extension of geometrical analysis".[34] Geometry continues to be the formal ideal of science, though its original methods and subjects – figures and extended magnitudes in space – are removed from rational mechanics. With respect to his *Mécanique Analytique*, Lagrange proudly proclaimed that "no figures are to be found in this work".[35] Geometrical methods and spatial intuition are eliminated in the representation and demonstration of mechanical propositions at the cost of the analytical approach.

Two additional aspects of the foundational change that takes place with Lagrange have to be mentioned. First, the absence of nearly all kind of methodology as well as of explicit metaphysical foundation. The *Mécanique Analytique* was the first major textbook in the history of mechanics which refrained from any kind of explicit philosophical reflection. In short, a century after Newton's *Principia*, Lagrange gave an 'update' of the mathematical principles of natural philosophy, while he abandoned the traditional subjects of philosophia naturalis. His bold claim to make mechanics "a new branch"[36] of analysis implied not only a rejection of geometrical method. It also implied a rejection of explicit philosophical foundations in the broadest sense: Lagrange was no longer interested in the conceptual foundations of his mechanics; he even changed the basic concept of his mechanics for reasons of 'formal economy'. The basic mathematical concepts at the top of the deductive structure of his mechanics are unloaded of meaning. Lagrange's analytical mechanics therefore can no longer be understood as an Euclideanistic enterprise in the traditional sense, because this implies a certain commitment to the 'material truth' of its first principles.

This development seems, in a way, inevitable given the conditions of 'global' Euclideanism and successfully competing research programs. I already stressed that the different scientific metaphysics underlying each program tends to separate these programs, and that mathematics work against it and aim at an integration of the different approaches. In Lagrange, this integrating power of mathematics gained a special quality: While each program aims at building up deductive structures (filled with different contents), global Euclideanism tends towards building up a unique 'superstructure' (left with the problem what its content is). Lagrange was confronted with an abundance of mechanical laws which were totally different in semantical respect and which emerged from different research programs. He had good reasons to accept these laws as valid, because they had turned out to be useful in order to describe (and deductively 'explain') certain classes of mechanical problems. His Euclideanism now operated on the level of these laws which are already expressed in algebraical or analytical form. The higher calculus served as the uniting element in its deductive chains. In so far, as order and unity became the main targets and the calculus the main means, his mechanics is rightly called analytical. While the 'sliding of the centre of gravity' continues, axioms are left as formal means of deduction. The whole system of Lagrange's analytical mechanics is hold together by logical coherence rather than by material truth.

The second aspect that should be stressed is that Lagrange's *Mécanique Analytique* had a long-term effect on the status of mechanical principles or 'axioms'. This was brought about by a significant tension of which Lagrange himself was partly aware, and some of his successors were even more so: His conjunction of old Euclideanism and new mathematical instrumentalism suggested that the first principles of mechanics can be established without recourse to any kind of geometrical and physical intuition or philosophical foundation. This led to a conflict with the traditional meaning of an axiom as a self-evident first proposition, which is neither provable nor in need of a proof. Lagrange started his major work with one principle, i.e. the principle of virtual velocities. In the first edition of his *Mécanique Analytique*, he introduced it as "a kind of axiom".[37] Later, 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 seemed to him that it is "not sufficiently evident to be established as a primordial principle".[38]

This is the basic dilemma of Lagrange's mechanics: Euclideanism demands evidence, instrumentalism dissolves evidence of first principles. The dilemma became even more obvious when Lagrange later tried to demonstrate his formal axiom. His attempts were followed by a large number of other 'demonstrations', which all aimed at a mediation of lost intuition and evidence. There was a manifest "crisis of principles"[39], and this crisis was a result of the unsolved tension in Lagrange's mechanics. This degeneration of the old Euclidean ideal might be called 'Rubber-Euclideanism'– a label used by Lakatos in a different context – because it "stretches the boundaries of self-evidence".[40] Rational mechanics in the course of the nineteenth century reveals a continuing decline of this ideal, and Lagrange's mechanics was the unintended turning point of this development. Despite this decline, Euclideanism remained an attractive ideal within theoretical mechanics for nearly one century, as was shown elsewhere.

6. Conclusion

I outlined some developments of eighteenth century's mathematical philosophy of nature from a 'unified' point of view that regards 'mechanical Euclideanism' as the ideal and dominant concept of science, which is rooted in mechanical philosophy and in a 'classical' understanding of science that can be traced back at least to Aristotle's *Analytica posteriora*. According to this ideal, rational mechanics is mathematics both with respect to its content and its form. Kant and later Kantians transformed the philosophical foundation of rational mechanics, but sustained basically the same ideal.

Today, Euclideanism seems no longer defendable as a position in the philosophy of the empirical sciences. This is mainly an outcome of the 'revolutions' which took place in early twentieth-century physics – and of the philosophical lessons drawn from these scientific achievements by Karl R. Popper and others. It should be kept in mind, however, that even in the nineteenth century so-called 'Newtonian' mechanics – which in fact was much more than the mechanics in the tradition of Newton – was considered to rest on unshakable, indubitable true axioms. The decline of this mechanical Euclideanism, which starts unwillingly with Lagrange, is a long-term 'evolutionary' process which had to happen before these revolutions could take place: 'No Einstein without the (meta-theoretical) evolution from Lagrange to Mach and Neumann', one might say. This development is not only – and presumably not in the first instance – brought about by empirical problems of classical mechanics, but rather by a change of what 'proper' mathematics means. Lagrange's 'analytical purism' can be understood as an early starting point of this development, which is strongly determined by the rise of so-called 'pure' mathematics in the early nineteenth century. This led to an increase of rigor and to a 'shrinkage' of those areas where mathematics can gain evidence and certainty. Comparable to Euclidean geometry, rational mechanics was strongly involved in this process, and lost its 'Euclidean dignity' forever.

1 'Application' refers to a relation or a process of adaptation between two distinguishable entities. As far as mechanics, in the view of mechanical philosophy, has to do with magnitudes (like extension, time, motion) from its very beginnings, this concept does not seem appropriate here. Rational mechanics is not established by the 'application' of mathematics, but it is mathematics from the very beginning (see part 2).

2 Cf. Helmut Pulte, *Axiomatik und Empirie. Eine wissenschaftstheoriegeschichtliche Untersuchung zur Mathematischen Naturphilosophie von Newton bis Neumann*, Darmstadt: Wissenschaftliche Buchgesellschaft 2005, here chapter III and IV, for more details.

3 Isaac Newton, *Mathematical Principles of Natural Philosophy and His System of the World.* Translated into English by A. Motte in 1729. Revised, and supplied with an historical and explanatory appendix by F. Cajori, Berkeley: University of California Press 1934, p. XVII–XVIII. Alan Gabbey has shown that the term 'mechanica rationalis' – taken over by Newton from Pappus – was used in Goclenius' *Lexicon philosophicum Graecum* and therefore "was in (probably common) use during the first decade of the seventeenth century, at the latest". Alan Gabbey, Newton's Mathematical Principles of Natural Philosophy: a Treatise of 'Mechanics'?, in: Peter M. Harman, Alan E. Shapiro (eds.), *The Investigation of Difficult Things*, Cambridge: Cambridge University Press 1992, p. 305–322, here p. 309. It is worth mentioning that Newton's reference to Pappus is an example of a certain 'back to the ancients-leaning' in Newton which is (inter alia) motivated by his striving for mathematical certainty in natural philosophy along traditional geometrical (Euclidean) lines. For further details see the excellent study of Niccolò Guicciardini, *Isaac Newton on Mathematical Certainty and Method*, Cambridge/London: The MIT Press 2009, here especially part IV.

4 Cf. Imre Lakatos, *Philosophical Papers*, ed. by John Worrall and Gregory Currie, 2 vols., Cambridge: Cambridge University Press 1978, especially vol. 2, p. 28–29.

5 Lakatos makes clear that the dichotomy 'Euclidian-Empiricist' (or later: 'Euclidian-Quasi-empirical') applies to 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"; see Lakatos, *Philosophical Papers* (see note 4), vol. 2, p. 6. This holds true especially for mechanics. The traditional dichotomy 'empiricism-rationalism' conceals the common basis of infallibility and is not very useful in a historiographical respect.

6 *Isaac Newton's Philosophiae naturalis principia mathematica. The Third Edition (1726) with Variant Readings*, assembled by Alexandre Koyré, I. Bernard Cohen, with the assistance of Anne Whitman, 2 vols., Cambridge, Massachusetts: Harvard University Press 1972, p. 39 and 54.

7 For a more detailed discussion of these research programs and the historiographical problems involved see Helmut Pulte, *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: Franz Steiner 1989, p. 6–22.

8 A thorough and attentive, though widely ignored study of Leibniz's concept of force is given by Hans Stammel, *Der Kraftbegriff in Leibniz' Physik*, PhD thesis, University of Mannheim 1982.

9 Concerning the foundations of mechanics, Newton's principles ('axiomata sive leges motus') and his law of gravitation must be sharply distinguished. What was soon understood as 'revolutionary' (in the sense of an obvious and irreversible break with the older tradition of mechanism) was his celestial mechanics, i.e. the application of the law of gravity, in combination with his three principles, to the motion of the moon and the planets. In the last five decades, however, historical research has revised the traditional picture – drawn by nineteenth-century philosophers and scientists like Ernst Mach, William Thomson or Peter G. Tait and perpetuated by Thomas S. Kuhn and others – that Newton's *Principia* was 'revolutionary' with respect to the principles of mechanics. Quite contrary, it has been shown that neither Newton's three laws of motion were entirely new, nor that they were understood as such by his contemporaries and his immediate successors. For a good overview see Henk J.M. Bos, Mathematics and Rational Mechanics, in: George S. Rousseau, Roy Porter (eds.), *The Ferment of Knowledge: Studies in the Historiography of Eighteenth-Century Science*, Cambridge: Cambridge University Press 1980, p. 327–355.

10 This is one reason why Newton's mechanics was frequently presented as a model of the 'hypothetical-deductive' concept of science in the modern sense. This view, however, ignores the fact that Newton regards the 'axiomata' by no means as hypothetical. See, as an example of the hypothetical-deductive interpretation, Ralph M. Blake, Isaac Newton and the Hypothetico-Deductive Method, in: Ralph M. Blake, Edward H. Madden (eds.), *Theories of Scientific Method: Renaissance Through the Nineteenth Century*, Seattle/London: University of Washington Press 1966, p. 119–143.

11 Isaac Newton, *Opticks: Or a Treatise of the Reflections, Refractions, Inflections Colours of Light. Based on the 4th edition, London, 1730. With a Foreword by Albert Einstein*, ed. by I. Bernard Cohen, New York: Dover Publications 1952 (repr. 1979), p. 404 (Qu. 31). As is well known, this passage paraphrases Newton's fourth rule of reasoning in philosophy of the *Principia.* Cf. Newton, *Mathematical Principles* (see note 3), p. 400.

12 Newton to Cotes from March 28th, 1713; see Isaac Newton, *The Correspondence*, eds. Herbert Westren Turnbull, Jonathan French Scott, Alfred Rupert Hall and Laura Tilling, 7 vols., Cambridge: Royal Society 1959–1977, vol. 5, p. 396–397.

13 This label is rightly applied to Newton by Max Jammer, *Concepts of Space. The History of Theories of Space in Physics*, Cambridge, Massachusetts: Harvard University Press 1954, chapter 4.

14 Colin MacLaurin, *An Account of Sir Isaac Newton's Philosophical Discoveries*, London: Millar 1748 (repr. Hildesheim/New York: Olms 1971), p. 8. For a more detailed discussion of Newton's understanding of axioms see Pulte, *Axiomatik und Empirie* (see note 2), chapter III.

15 For more information about this context see Pulte, *Axiomatik und Empirie* (see note 2), p. 126–131.

16 See Pulte, *Das Prinzip der kleinsten Wirkung* (see note 7), p. 81–103.

17 Good overviews can be found in René Dugas, *A History of Mechanics.* Translated by J.R. Maddox, New York: Dover Publications 1988; Ernst Mach, *Die Mechanik in ihrer Entwicklung, historisch-kritisch dargestellt*, Leipzig: Brockhaus 91933 (repr.: Darmstadt 1982); Clifford A. Truesdell, A Program Toward Rediscovering the Rational Mechanics of the Age of Reason, *Archive for History of Exact Sciences* 1 (1960), 1–36; Aurel Voss, Die Prinzipien der rationellen Mechanik, *Encyclopädie der mathematischen Wissenschaften mit Einschluß ihrer Anwendungen*, vol. 4 (1901), p. 1–121.

18 I already mentioned at the beginning that this ideal does not determine the image of science of all philosophers and scientists in question here, i.e. 'dominating' does not mean 'unique'. There are also areas like natural history where this ideal at least was not relevant for scientific practice. I would like to stress, however, that it was dominant (though not exceptionless) as a leading ideal of science and of special relevance for the mechanistic tradition that shaped what was labelled 'classical mechanics' from the late nineteenth century onwards (and which should not be reduced to 'Newtonian mechanics'). Moreover, I would like to stress that I am using the term 'Euclideanism' in this paper as a systematic one in order to structure a complex historical process. It refers to no special historical

usage, whether this fits to my 'Lakatosian' coining or not. Finally, I also underline that 'Euclideanism' as it is used here also characterizes science in the important tradition of classical empiricism (see note 5). For further information about this aspect, especially for my analysis of Francis Bacon's concept of science in this context, see Pulte, *Axiomatik und Empirie* (see note 2), chapter II, see especially p. 33–39 and p. 66–75.

19 As d'Alembert put it in the full title of his well-known textbook from 1743: "Traité de Dynamique, dans lequel les loix de l'equilibre & du Mouvement des Corps sont réduites au plus petit nombre possible, & démontrées d'une maniére nouvelle, & où l'on donne un Principe général pour trouver le Mouvement de plusieurs Corps qui agissent les uns sur les autres, d'une maniére quelconque."

20 Mach, *Mechanik* (see note 17), p. 72.

21 Truesdell, Program (see note 17), p. 10.

22 Most noteworthy in this respect are his two large textbooks: Leonhard Euler, *Mechanica sive motus scientia analytice exposita*, 2 vols., Petersburg: Academiae Scientiarum 1736; Leonhard Euler, *Theoria motus corporum solidorum seu rigidorum ex primis nostrae cognitionis principiis stabilita et ad omnes qui in huiusmodi corpora cadere possunt accommodate*, Rostock/Greifswald: Röse 1765.

23 See, for example Leonhard Euler's *Anleitung zur Naturlehre, worin die Gründe zur Erklärung aller in der Natur sich ereignenden Begebenheiten und Veränderungen festgesetzt werden* [written around 1755, published posthumously], in: Leonhardi Euleri *Opera omnia* (3)1, Leipzig: Teubner 1926, p. 16–178, especially p. 16–19.

24 See, first of all, Leonhard Euler, De la force de percussion et de sa véritable mesure, *Histoire de l'Académie Royale des Sciences et Belles-lettres de Berlin* 1 (1746), 21–35; the same, in: Leonhardi Euleri *Opera omnia* (2)8, Leipzig: Teubner 1965, p. 27–53 and Euler, *Anleitung zur Naturlehre* (see note 23), p. 62–63 and 79–82.

25 Euler, De la force (see note 24), p. 34.

26 See Pulte, *Prinzip* (see note 7), p. 150–181 for a detailed reconstruction of this process.

27 See Alwin Diemer, Die Begründung des Wissenschaftscharakters der Wissenschaften im 19. Jahrhundert – Die Wissenschaftstheorie zwischen klassischer und moderner Wissenschaftskonzeption, in: the same (ed.), *Beiträge zur Entwicklung der Wissenschaftstheorie im 19. Jahrhundert*, Meisenheim a. G.: Anton Hain 1968, p. 3–62, here p. 31: "*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' […]" [Hervorhebung im Original].

28 Euler's work is unique in this respect. I think that neither the rise of the variational calculus nor of potential theory nor of a great deal of the theory of ordinary and partial differential equations can be understood without regarding this object. I cannot, however, elaborate on this point here.

29 For examples and a more detailed analysis of this change see Pulte, *Axiomatik und Empirie* (see note 2), p. 156–168.

30 It is important to note the general use of this technical term: Not all mechanics which make use of the 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 that of least action) or differential variational principles (like that of virtual velocities). Using such principles, analytical mechanics is 'calculating' from its very beginnings. Cf. Mach, *Mechanik* (see note 17), p. 445.

31 See Pulte, *Prinzip* (see note 7), especially p. 193–205 and 252–268.

32 This becomes directly clear, for example, from the titles of some of Maupertuis' and Euler's essays on the principle of least action: See Pierre L.M. de Maupertuis, Accord des différentes Loix de la Nature qui avoient jusqu'ici paru incompatibles, *Mémoires de l'Académie Royale des Sciences de Paris* (1748), 417 [*Oeuvres* 4, p. 3]; the same, 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 (1758), 267 [*Oeuvres* 4, p. 31]; Leonhard Euler, 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 (1753), 169 [*Opera omnia* (2)5, p. 152].

33 Lagrange's commitment to 'directive' forces and to atomism as well as his theory of matter in general support this thesis. See Pulte, *Prinzip* (see note 7), p. 232–240.

34 Joseph-Louis Lagrange, *Théorie des fonctions analytiques.* Nouvelle édition, revue et augmentée par l'auteur, Paris: Courcier 1813, p. 337.

35 Joseph-Louis Lagrange, *Mécanique Analytique*, Paris: Desaint 1788, p. vi.
36 Lagrange, *Mécanique Analytique* (see note 35), p. vi.
37 Lagrange, *Mécanique Analytique* (see note 35), p. 12.
38 Joseph-Louis Lagrange, *Mécanique analytique.* Revue, corrigée et annotée par Joseph Bertrand. 3rd edition, 2 vols., Paris: Mallet-Bachelier 1853 [Oeuvres 11, 12], vol. 1, p. 27, cf. also p. 23.
39 Patrice Bailhache, Introduction et commentaire, in: Louis Poinsot, *La Théorie générale de l'équilibre et du mouvement des systèmes*, Paris: L'Histoire des Sciences: Textes et Études 1975, p. 1–199, here p. 2.
40 Lakatos, *Philosophical Papers* (see note 4), vol. 2, p. 7.

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