

Jacobi's Criticism of Lagrange: The Changing Role of Mathematics in the Foundations of Classical Mechanics

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C. G. J. Jacobi in his *Lectures on Analytical Mechanics* (Berlin, 1847–1848) gives a detailed and critical discussion of Lagrange's mechanics. Lagrange's view that mechanics could be pursued as an axiomatic-deductive science forms the center of Jacobi's criticism and is rejected on mathematical and philosophical grounds. In this paper, Jacobi's arguments are presented and analyzed. It is shown that Jacobi's criticism is motivated by a changed evaluation of the role of mathematics in the empirical sciences. This change is interpreted as a process of dissolution of Euclideanism (in the sense of Lakatos) that dominated theoretical mechanics up to Jacobi as the leading ideal of science. © 1998 Academic Press

In seinen *Vorlesungen über analytische Mechanik* (Berlin, 1847–1848) setzt sich C. G. J. Jacobi ausführlich und kritisch mit Lagranges Mechanik auseinander. Im Mittelpunkt der Kritik steht dabei Lagranges Auffassung, Mechanik könne als eine axiomatisch-deduktive Wissenschaft betrieben werden. Diese Auffassung wird von Jacobi aus mathematischen und philosophischen Gründen zurückgewiesen. Im vorliegenden Aufsatz werden Jacobis Argumente dargestellt und analysiert. Es wird gezeigt, daß seine Kritik auf einer veränderten Beurteilung der Rolle der Mathematik in den empirischen Wissenschaften beruht. Diese Veränderung wird als Prozeß der Auflösung des Euklidianismus (im Sinne von Lakatos) als leitendem Wissenschaftsideal der theoretischen Mechanik bis hin zu Jacobi interpretiert. © 1998 Academic Press

Dans son *Cours de Mécanique analytique* (Berlin, 1847–1848), C. G. J. Jacobi présente une discussion détaillée et critique de la mécanique de Lagrange. La critique de Jacobi concerne avant tout la conception de la mécanique comme science axiomatique et déductive défendue par Lagrange. Il rejette cette conception pour des raisons mathématiques et philosophiques. Dans cet article seront présentés et analysés les différents arguments de Jacobi, démontrant que sa critique est fondée sur un changement d'appréciation de la fonction de la mathématique dans les sciences empiriques. La position nouvelle de Jacobi sera interprétée comme conséquence de l'élimination de l'Euclidianisme (dans le sens de Lakatos), le prototype scientifique de la mécanique rationnelle jusqu'à Jacobi. © 1998 Academic Press

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INTRODUCTION

[Lagrange's] *Analytical Mechanics* is actually a book you have to be rather cautious about, as some of its content is of a more supernatural character than based on strict demonstration. You therefore have to be prudent about it, if you don't want to be deceived or come to the delusive belief that something is proved, which is [actually] not. There are only a few points, which do not imply major difficulties; I had students, who understood the *mécanique analytique* better than I did, but sometimes it is not a good sign, if you understand something. [28, 29]

Carl Gustav Jacob Jacobi (1804–1851) gave this warning to his students in the *Vorlesungen über analytische Mechanik*, which he delivered in Berlin in 1847–1848. This series of lectures is not only his *last* major contribution to the subject, it has to be considered the most detailed and authentic source of his later views on the foundations of mechanics [58]. As it shows important differences from the well-known *Vorlesungen über Dynamik* from 1842 to 1843 [26], which has hitherto been regarded as the most comprehensive and reliable source in this respect, the recent publication of the *Analytische Mechanik* [28] might suggest a new image of Jacobi's role in the history and philosophy of mechanics.

Without doubt, this will prove correct about the relation of Jacobi's and Lagrange's mechanics. In light of Jacobi's older papers and lectures, his above-quoted criticism is quite remarkable: Hamilton and Jacobi were said to be the most successful mathematicians in the first half of the 19th century who developed mechanics along "Lagrangian lines." When Hamilton called Lagrange's *Mécanique analytique* "a kind of scientific poem" [24, 104], he implied that he himself added some new "stanzas" to the very same poem. More specifically, when Jacobi called Lagrange's famous textbook a successful attempt "to set up and transform" [26, 1] the differential equations of motion, he implied that Hamilton's and his own contributions should be regarded as a necessary and sufficient complement to this attempt, as they provided the means for integrating those equations. The Hamilton–Jacobi theory can be seen as a logical consequence of Lagrange's approach. Felix Klein was thus quite right when he said that "Jacobi's extension of mechanics is essential with respect to its analytical side" [30, 203] and that it had to be criticized for its lack of physical relevance [30, 206–207].

This view, however, can no longer be accepted, if Jacobi's *Analytische Mechanik* is taken into account. This paper aims to show, on the basis of this source, that the essence of "analytical mechanics" in the sense of Lagrange is undermined by Jacobi's criticism. In the first two parts, I will outline Lagrange's understanding of analytical mechanics and show that this understanding leads to serious foundational problems. Then I will sketch Jacobi's changing attitude toward mathematical physics, without which the *Analytische Mechanik* can hardly be understood. In the fourth and fifth part, I will present what can be called the hard core of Jacobi's criticism, i.e., his analysis of Lagrange's two so-called "demonstrations" of the principle of virtual velocities. In the sixth part, I will argue that Jacobi's criticism is substantially due to a changed attitude toward the role of mathematics in mechanics, and I will outline the philosophical relevance of this shift.

1. MECHANICAL EUCLIDEANISM AND MATHEMATICAL INSTRUMENTALISM: TRADITION AND MODERNISM IN LAGRANGE'S ANALYTICAL MECHANICS

Clifford Truesdell has aptly described Lagrange's *Mécanique analytique* as an attempt to organize 17th- and 18th-century rational mechanics using new mathematical techniques. Starting with the principle of virtual velocities, the whole of dynamics and statics is presented in a deductive manner, thus clarifying the logical relations

of its so-called “principles” without creating new mechanical concepts and without increasing its empirical content [62, 32–35]. This description is correct but rather incomplete. Due to Truesdell’s notorious fixation on mathematics *alone* and his lack of philosophical interest [62, 11], it takes into account neither important elements of *continuity* nor a genuinely *new* element in Lagrange’s mechanics.

There is a *continuity* in Lagrange’s attempt to present mechanics as an axiomatic science; he starts from a seemingly evident, general, and certain principle and develops it in a deductive manner with a minimum of further assumptions. As he put it, he “intended to reduce the theory of this science, and the art of resolving the problems which are related to it, to general formulas” [33, v]. This remark should not, however, be understood as an announcement of an abstract theory of mathematical physics in a modern sense, i.e., of an uninterpreted mathematical *model* that can be shown to be in partial agreement with physical reality. Rather, it should be viewed as an expression of his belief in an *intrinsic* mathematical structure of nature itself. His bold claim to make mechanics “a new branch” of analysis [33, vi] is rooted in his conviction that the calculus is appropriate for uncovering the essential laws of nature and their logical relations.

With the advantage of hindsight, we cannot but describe Lagrange’s program as an “overambitious exercise” [22, 325], but it is important to be aware of the fact that the decisive characteristics of his ideal of mechanics have a long history. Even his immediate predecessors in the analytical tradition of mechanics—Euler, d’Alembert, and Maupertuis—adhered to the same belief in a mathematically structured reality and in the capability of the human mind to condense this reality into a deductive symbolical structure with only a few first, indisputable principles. This belief is, above all, inherited from Cartesian philosophy with its ideas of “clarity” and “simplicity,” as several investigations of their philosophies of science have shown [16; 25; 55].

Lagrange’s image of science, however, is not specific to the rationalistic tradition. The ideal of a deductively structured mechanics with certain, general, and evident principles or “axioms” at the top can also be found in the more empirical Newtonian program and in early 19th-century French positivism [16]. D’Alembert is particularly interesting in this respect because he influenced Lagrange’s ideal of mechanics more than any other scientist or philosopher. He declared himself an empiricist, but his philosophical statements about mechanics show not even the slightest trace of a Humean skepticism concerning the capacities of mathematical physics to reveal the real laws of nature [25, 151–169].

I will use Lakatos’s term *Euclideanism* to label rational mechanics as it was pursued by d’Alembert, Lagrange, and most¹ of the other 18th- and early 19th-century mathematicians, physicists, and philosophers. This notion seems to me particularly appropriate for Lagrange for three reasons.

First, it expresses the view that the “ideal theory is a deductive system with an

¹ A remarkable, though not very influential exception is Lazare Carnot [7]; see Gillispie [18, 31–100] for a detailed analysis of his mechanics.

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” [40, 2: 28]. Its basic aim “is to search for self-evident axioms—Euclidian [!] methodology is puritanical, antispeculative” [40, 2: 29]. Lagrange’s analytical mechanics fulfills this definition very well. As he claimed that statics and dynamics can be based on *one* (and only one) “fundamental principle” [33, 8], and as he elaborated this *mononomism* more successfully than any of his predecessors, we can characterize the *Mécanique analytique* as the *most articulated* form of mechanical Euclideanism in 18th-century rational mechanics.

Second, Euclideanism is an epistemologically neutral label, i.e., it includes both *empirical* and *rationalistic* foundations of the science in question.² 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 18th-century mechanics, and in many textbooks they are inseparably interwoven. Lagrange’s position in this respect is by no means clear, either, though he sometimes shows a critical attitude toward metaphysical arguments for first laws which can be interpreted as a commitment to empiricism (e.g., [55, 187–189]). Lagrange is only one example (though a good one) to illustrate the more general historiographical thesis that the decisive philosophical feature of rational mechanics at this time cannot be understood in terms of the dichotomy “rationalism–empiricism.” This epistemological pattern is inappropriate to grasp the development of mechanics into a highly organized 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 characterizes mechanics at the time in question.

Third, Lakatos’s label makes explicit that Euclidean geometry served as the model-science for mechanics. It was *Lagrange* who in his *Théorie des fonctions analytiques* described “mechanics as a geometry with four dimensions” (including time as the fourth dimension) and the “analysis of mechanics as an extension of geometrical analysis” [37, 337]. These words clearly express his view that all relevant mechanical knowledge can be brought under an axiomatic-deductive structure and that it possesses the same distinctive characteristic as any mathematical knowledge: *infallibility*.

So far I have presented Lagrange’s *Mécanique analytique* in the tradition of

² 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” [40, 2: 6]. This holds true especially for mechanics. The traditional empirical–rationalistic dichotomy conceals the common basis of infallibility and is not very useful historiographically [40, 2: 70–103]. It should be mentioned that Lakatos himself subsumes Lagrange and other mathematicians of the 18th century under “Rubber-Euclidianism” [40, 2: 7, 9]. I shall come back to the (dis-)qualifier “Rubber” later.

18th-century rational mechanics. His famous and already mentioned claim, however, to give a *purely analytical* theory of mechanics, marks in a certain and unnoticed sense a break with this tradition. Lagrange's claim is generally understood as a rejection of all geometrical means, "that no figures are to be found in this book" [39, vi], as he himself emphasizes. This reading suggests itself and is correct, though we know today that it applies more to Lagrange's presentation and justification of mechanical propositions than to their invention or discovery [20, 679]. The tasks of "reducing" mechanics to the calculus and the calculus to a sound algebraical basis are, in his program, complementary in order to reach a secure foundation of the whole of mechanics [19, 7–10].

But another interpretation of Lagrange's claim can be added to this, and this seems to me of equal importance. Restricting mechanics exclusively to the methods of analysis implies dispensing not only with other *mathematical* methods (and even with "mechanical considerations" [39, vi]) but also with *extramathematical* methods. Indeed, Lagrange's *Mécanique analytique* is the first major textbook in the history of mechanics that 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's 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. This is in striking contrast not only to 17th-century foundations of mechanics such as that of Descartes, Leibniz, and Newton but also to the approaches of Lagrange's immediate predecessors, Euler, Maupertuis, or d'Alembert [55, 232–240]. In short, a century after Newton's *Principia*, Lagrange's textbook can be seen as an attempt to update the mathematical principles of natural philosophy while abandoning the traditional subjects of *philosophia naturalis*. In this *special* sense, the *Mécanique analytique* is also a striking example of mathematical instrumentalism.

2. RUBBER EUCLIDEANISM: THE BASIC DILEMMA OF LAGRANGE'S MECHANICS AND ITS RECEPTION

The combination of Lagrange's *new* instrumentalism (with respect to philosophy of nature) and *old* Euclideanism (with respect to philosophy of science) is the decisive philosophical characteristic of Lagrange's mechanics.³ On the one hand, it made the *Mécanique analytique* a model of how mathematics should be used in

³ I have already applied the characterization "mathematical instrumentalism" to Lagrange's mechanics in [55, 238]. In order to avoid misunderstandings, I need to emphasize that "instrumentalism" here refers rather to philosophy of nature (I₁) 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 such as 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). For example, Lagrange in his first papers introduced the principle of least action as a "universal key to all problems," which meant that he introduced potential and living

physics; it is its advanced mathematical *and* antimetaphysical style which made Lagrange's textbook attractive for working mathematicians as well as for *positivistic* philosophers such as Comte [16]. In my later discussion of Jacobi's criticism, I will refer to Lagrange's textbook in this special sense, i.e., as the widely accepted *best realization* of a purely mathematical Euclideanism in physics.

On the other hand, this combination bears a significant tension. Lagrange himself was partly aware of it, 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, the principle of virtual velocities. In order to achieve this aim, he had to formulate it in a fairly general and abstract manner, using his calculus of variations. In the first edition of his *Mécanique analytique*, he introduced this "very simple and very general" principle in statics as "a kind of axiom" [33, 12]. Lagrange appeased his tangible discomfort with the word "axiom" by extensive references to its successful use by great authorities of the past such as Galileo and Descartes [33, 8–12]. (The history of mechanics in Lagrange's textbook partly serves as a *substitute* for missing philosophical justification.) In the second edition, he stuck to the word "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" [39, 1: 23, 27].

Euclideanism demands evidence; instrumentalism tends to dissolve it. This is the basic *dilemma* of Lagrange's mechanics. It was probably brought to his attention in 1798 by Fourier [14; 9, 238], and his way out of it was the same as Fourier's. In two different so-called demonstrations from 1798 and 1813, he tried to *prove* his primordial principle by referring to simple mechanical processes or machines [35; 37].

As will be shown later, Jacobi's criticism from 1847 is primarily an analysis and a rejection of these attempts to mediate evidence by supposed demonstrations, but

force as basic concepts of mechanics and gave an "adequate statement of the laws of a fairly extensive branch of mechanics . . . without the use of an *a priori* concept of force" [62, 33] in Newton's sense. (The elimination of force meant nothing to him, however, despite the fact that there was an intense philosophical discussion about this concept in 18th-century science and philosophy.) Later, he exchanged the principle of least action for the principle of virtual velocities, because the latter had certain advantages for a mathematical-deductive organization of mechanics [15, 233–234; 55, 258]. He thereby reintroduced forces into his mathematical formalism without any discussion of this *conceptual* shift and without calling into question the intrinsic nature of mechanical laws. This is an illustration of instrumentalism (I_1). In contrast, instrumentalism with respect to philosophy of science (I_2) is characterized 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 a *real* content itself. In my opinion, a consistent instrumentalism (I_1), which is not supported by an adequate theory of representation (such as, for example, Heinrich Hertz's *Bildtheorie*), inevitably leads to (I_2). Therefore the distinction of (I_1) and (I_2) is *generally* unnecessary, but as Lagrange's view is *not* consistent in this respect, it has to be kept in mind.

it was by no means the only one. Lagrange's formulation and (or) demonstration of the principle of virtual velocities posed a challenge for a number of mathematicians 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 Poinot (1806, 1838, 1846). They aimed at an *extension* of Lagrange's principle, taking into account conditions of constraint given by inequalities (Fourier, Cournot, Gauss, Ostrogradsky), and (or) at its *better foundation*. The second aim was pursued either by demonstrations, which "reduced" the principle of virtual velocities to that of the lever, to that of the composition of forces, or to both (Fourier, de Prony, Laplace, Carnot, Ampère, Cournot, Poisson), or, finally, by a quasi-geometrical analysis of the notions "mechanical system," "force," and "equilibrium," leading to a general theorem which contains Lagrange's principle as "a simple corollary" (Poinot) [49, 233].

These attempts reveal a "crisis of principles" [2, 7] caused by the *Mécanique analytique*. As they are well documented in the literature, I shall not discuss any of them at this point. Jacobi's debt to the relevant papers will be dealt with later on.⁴ It must be stressed, however, that all these attempts aimed at *better* demonstrations, giving the principle of virtual velocities a *more secure* foundation and making it *more evident*. (Even Poinot, who held that this principle was unnecessary for a satisfactory deductive organization of mechanics, did not renounce the presentation of a "clear and rigorous" demonstration [49, 216].) Like Lagrange, they applied their refined logical and mathematical methods to mediate evidence to the principle of virtual velocities. Lakatos aptly described such a position as "a sort of 'rubber-Euclidianism'" because it "stretches the boundaries of self-evidence."⁵

At first glance, Gauss seems to present an exception to this all-dominating rubber-Euclideanism. In announcing his own principle of least constraint from 1829, he explicitly stated that Lagrange's formulation (i.e., the principle of virtual velocities for statics in combination with d'Alembert's principle) contains "in material respect" all other fundamental principles of statics and dynamics and that therefore "no new principle of the doctrine of movement and equilibrium is possible" [17,

⁴ Jacobi obviously did not know the whole literature about the different demonstrations of the principle of virtual velocities. Beside Lagrange, he also mentioned Fourier, Gauss, Ostrogradsky, and Poinot [14; 17; 45; 46; 47; 49], but not Ampère, Carnot, or Cournot. Lindt's detailed study [41] from 1904 is still an excellent presentation of the whole subject, but I refer also to [3, 95–115; 12, 361–369; 23, 1: 302–308] for general discussions and to [2] for Poinot and [9] for Fourier.

⁵ See his [40, 2: 7]. Lakatos's characterization is applicable even to those representatives of French mathematical physics who criticized Lagrange's mechanics for its lack of physical relevance. From Duhem's *L' évolution de la mécanique* [13] to Grattan-Guinness's *Convulsions* [23], it has been stressed justly that the approaches of Laplace, Poisson, and their school are *anti-Lagrangian* in so far as they revealed the shortcomings of Lagrange's approach relative to concrete physical objects. Poisson therefore called for a "physical mechanics" instead of Lagrange's "analytical mechanics" [53, 361], where physical mechanics should not deal with idealized mass points and rigid constraints but with real bodies underlying certain physical actions. But, again, this criticism does not aim at Lagrange's Euclideanism itself, but at a more flexible handling of mathematics at a *lower* level, that is, with regard to its application to special physical phenomena.

232]. On the other hand, he remarked that “in the way it is expressed, it does not exhibit immediately the credit of recommending itself as plausible” [17, 232–233]. Probably with these statements in mind, Jacobi referred to Gauss as someone who held the opinion that the principle of virtual velocities “should be regarded as a principle, which is not in need of a proof” [26, 15], and that introducing “something conventional” into mechanics should be allowed [28, 10]. In this respect, Ernst Mach’s interpretation of Gauss is also interesting. In his *Science of Mechanics*, he criticized Euclideanism as “the mania for demonstration” [42, 72; cf. 40, 2: 7]. With an odd reference to Jacobi’s first remark above, however, he excluded Gauss from this criticism, claiming that *he* had “the right view of the principle of virtual displacements”—obviously, because this view was in agreement with Mach’s philosophical position: “It is not possible to demonstrate mathematically, that nature has to be, as it is” [42, 68].

It is true that Gauss did *not* add another mathematical demonstration of the principle of virtual velocities to the literature. Nevertheless, both Mach’s and Jacobi’s interpretations of his view missed the point. Gauss neither understood first principles of mechanics as convenient tools for an economical description of phenomena (Mach), nor did he want to introduce conventional elements into mechanics (Jacobi). If we take Gauss’s thesis seriously, that *no* really new principle can replace that of Lagrange, his principle of least constraint should be understood as a different *formal* presentation of the old principle, which offers a “new advantageous point of view, from which either this or that problem can be solved more easily, or from which a special adequacy [*Angemessenheit*] is revealed” [17, 232]. The last point, itself, is revealing because at the end of his paper Gauss drew a parallel between the modification of movement according to his principle of least constraint “by nature” and the compensation of errors of measurement according to his methods of least squares “by the calculating mathematician” [17, 235]. Here, in this strongly anthropomorphic analogy, he found evidence and certainty which he missed in Lagrange’s original formulation. Gauss is no exception to rubber-Euclideanism, but he stretched the boundaries of evidence in a *different* direction. Euclideanism continued to dominate rational mechanics. Fourier’s pseudo-Aristotelian motto of the very first paper, which was devoted to a criticism of Lagrange’s principle, is characteristic of the *whole tradition*: “Geometrare est probare” [14, 20].

3. THE EDGE OF MECHANICAL EUCLIDEANISM: JACOBI’S VIEW OF MATHEMATICS AND HIS CHANGING ATTITUDE TO MATHEMATICAL PHYSICS

Jacobi was born in 1804 and started his university career around 1825, a dozen years after Lagrange’s death [32]. From the very beginning of his career, he cultivated the role of a pure mathematician rather than that of a physico-mathematician, which was typical of the then dominating French tradition [23]. His early attitude to science can be seen as a result of neohumanism, a broad philosophical and cultural movement with considerable impact on German mathematics. According to this *Weltanschauung*, science and scientific education had ends in themselves.

Mathematics especially should be regarded as an expression of pure intellectual creativity and as a means of developing it further, needing no other justification whatsoever. Applied or mixed mathematics, which was at the core of the French tradition, was often hardly tolerated and was seen rather as a *degradation* than as a legitimation of mathematics [31, 100–106].

In his early career, Jacobi was quite absorbed by this ideal of *pure [reine] mathematics*. Proper mathematics in his sense is a product of the “inherent dynamism of human spirit” [31, 112]; it neither depends on sense experience in any epistemological respect, nor is it in need of external verification. Therefore, he was explicitly hostile to French mathematical physics as it was practiced by Fourier, Laplace, Poisson, and others [31, 106–109 and 114]. Criticized himself by Fourier, who could see no practical use for Abel’s and Jacobi’s theory of elliptic functions, Jacobi gave his famous reply: “A philosopher like him should have known that the unique aim of science is the honor of human spirit” [5, 276].

Of course, Jacobi could not and did not ignore the success of mathematics in physics, especially in mechanics. His earliest attempt to explain this undeniable fact, given in his inaugural lecture at the University of Königsberg in 1832 [27; trans., 31, 111–114], is basically Platonistic. The mathematization of nature demands, as a necessary prerequisite, that “the concepts of our spirit be expressed in nature. If mathematics was not created by our spirit’s own accord [and] in accordance with the laws inculcated in nature, those mathematical ideas implanted in nature could not have been perceived” [31, 112–113]. He charged “the school of the famous Count of Laplace” with too strong an inclination toward physics, thereby leaving the “true and natural way” of Euler and Lagrange and “damaging not only pure mathematics but also its application even to physical problems” [31, 114]. To put it in terms of a paradox, according to the young Jacobi, applied mathematics at its best is pure mathematics. The more pure mathematics is developed “according to the eternal laws implanted in the human spirit” [31, 113], the more “mathematics implanted in nature” is revealed to the human spirit [31, 113].

In this context, Jacobi’s commitment to Lagrange’s approach [31, 114] is not astonishing because the *Mécanique analytique*—one of Jacobi’s favorite textbooks when he studied mathematics as an autodidact—seemed to be the best realization of this ideal. Though he does not mention mechanics explicitly, we may assume that he (also) had this model of mathematical physics in mind when he described the philosophical significance of applying mathematics: “The same eternal laws are valid in our spirit as in nature; this is the prerequisite, without which the world cannot be understood, without which no knowledge of the things of nature would be possible. . . . Let us consider nature, as far as it expresses mathematical laws” [31, 112]. It is not necessary to elaborate on the point that Euclideanism was no problem for him at this time but that it was at the core of his philosophy both of pure mathematics *and* of mathematical physics.

Jacobi’s mathematical investigations during the first half of his career show a strong orientation toward his ideal of pure mathematics. Until 1834, he published no papers worth mentioning on mathematical physics or astronomy [59, XXII].

Even when he started working on the theory of differential equations of motion around 1835, stimulated by Sir William Rowan Hamilton, he was not interested in possible physical or philosophical implications of the Hamilton–Jacobi theory or of mechanics in general. From his remarks in the *Vorlesungen über Dynamik* of 1842–1843 in Königsberg, we can conclude that at this time he shared Lagrange's rejection of the metaphysical foundations of mechanics and his mathematical instrumentalism in general [26, 43–44; 59, XLII–XLIV]. In short, mechanics at its best was for Jacobi analytical mechanics in the sense of Lagrange, and there is not the slightest trace of criticism with respect to the aim and structure of the *Mécanique analytique* to be found in his papers about mechanics published previously.

This is in striking contrast to the *Analytische Mechanik* of 1847–1848. Jacobi's view of the value and meaning of applied mathematics changed remarkably in the years before he gave these lectures, as has been shown elsewhere [58, 502–505; 31, 116–120]. In short, this process can be described as the socialization of a pure mathematician by a broader scientific community, leading to a *physicalization* and *historization* of Jacobi's interests.

While he adhered to pure mathematics as the standard of rigor and certainty, he arrived at a clearer distinction of mathematical and empirical knowledge and developed a more critical attitude toward the problem of *why* mathematics as a product of our mind should be applicable to natural reality. He gave up his quite naive Platonism sketched above and came to a more modern and modest point of view. His new criticism of Lagrange's mechanics is the most distinct expression of this change because it led to a dissolution of traditional mechanical Euclideanism. Carl Neumann, who studied the manuscript of the *Analytische Mechanik* in 1869, described Jacobi's lecture as “outstanding on account of its criticism of the *foundations of mechanics*, which in this keenness was probably never articulated openly until now.”⁶

4. MECHANICAL EUCLIDEANISM AND ITS PROBLEMS: (1) LAGRANGE'S PRINCIPLE OF VIRTUAL VELOCITIES AND JACOBI'S CRITICISM OF HIS FIRST DEMONSTRATION FOR STATICS

Lagrange's “general principle of virtual velocities,” as it is presented in the second part of the *Mécanique analytique* (1788), goes back to his prize-winning paper, *Recherches sur la libration de la Lune* (1764) [15, 219–232]. It joins together “the principle that M. d'Alembert has given in his *Traité de dynamique*” and the “principle of virtual velocities [that] has led us to a very simple analytical method for solving all the questions of statics,” thus providing “a similar method for the problems of dynamics” [33, 180–181].

When Jacobi started his Berlin lectures on *Analytische Mechanik* in 1847, he was

⁶ The complete original quotation is: “Während jene *Königsberger* Vorlesung fast ausschliesslich nur die Darlegung und Vervollkommnung der in der Mechanik anzuwendenden *analytischen Methoden* zu ihrem Gegenstand hat, zeichnet sich die genannte *Berliner* Vorlesung aus durch eine Kritik der *Fundamente der Mechanik*, wie sie in solcher Schärfe wohl bis zum heutigen Tag noch niemals zur öffentlichen Aussprache gelangt sein dürfte” [43, 257; Neumann's italics].

quite aware of the fact that the presumed certainty and generality of the whole system of Lagrange's *Méchanique analytique* was based upon the principle of virtual velocities. The main difference between these lectures and his former ones consists in the addition of a large introductory part devoted to the laws of rest and motion, especially to the principle of virtual velocities [58, 505–507]. Jacobi borrowed Lagrange's general principle from the first volume of the *second* edition of the *Méchanique analytique* (1811) [36, 1: 258; 39, 1: 274], interpreting it as a “symbolic presentation of the system of differential equations” of a free mechanical system [28, 6]. If each of the n mass-points m underlies a force with Cartesian coordinates X_i , Y_i , and Z_i , it assumes this form [28, 6]:

$$\sum_{i=1}^n \left\{ \left(X_i - m_i \frac{d^2 x_i}{dt^2} \right) \delta x_i + \left(Y_i - m_i \frac{d^2 y_i}{dt^2} \right) \delta y_i + \left(Z_i - m_i \frac{d^2 z_i}{dt^2} \right) \delta z_i \right\} = 0. \quad (1)$$

Jacobi made clear that (1) also applies to a system under constraints, which are given by m equalities containing the $3n$ coordinates of the mass points ($m < 3n$). In this case, the $3n$ increments δx_i , δy_i , δz_i are no longer “completely arbitrary, but are virtual variations,” i.e., variations compatible with these constraints [28, 8].

In the case of equilibrium, the “total moment of the moving forces” vanishes, and (1) implies that the remaining “total moment of the sollicitating forces” X_i , Y_i , and Z_i equals zero [28, 11]:

$$\sum_{i=1}^n \{ X_i \delta x_i + Y_i \delta y_i + Z_i \delta z_i \} = 0. \quad (2)$$

In the case of equilibrium, this sum must be equal to zero, and, vice versa, if this total moment is equal to zero, there must be equilibrium. Jacobi described this as the “fundamental principle of statics” or “the famous expression, which Lagrange gave for the principle of virtual velocities” [28, 11].

Lagrange's original formulation in the first part of his *Méchanique analytique* did not use Jacobi's Cartesian representation (2), but was based on the undivided forces P, Q, R, \dots , acting along lines p, q, r, \dots on single mass-points. A small disturbance of the system causes changes of the position of the mass points which are called the “variations” or, because they are taken for the “first moment” of movement, the “virtual velocities” of these points [39, 1:19, 23]. Let their projections on the lines p, q, r, \dots be dp, dq, dr, \dots , where opposite directions of P and dp are indicated by a minus sign. Then the total moment of forces or, in modern terms, the “virtual work” of the system is $Pdp + Qdq + Rdr + \dots$, and (2) assumes the Lagrangian form [39, 1: 25]

$$Pdp + Qdq + Rdr + \dots = 0, \quad (3)$$

which Jacobi, of course, interpreted also as a reformulation of (1) for the case of equilibrium. He acknowledged the “immense importance of this symbolic form” (1) and held the view that “the discovery of this importance belongs to the greatest inventions of the last century” [28, 6]. He accepted the great value of Lagrange's principle

for a deductive organization of mechanics, but he also wanted to make clear beyond any doubt that it has to be established as a principle in the literal sense: a *beginning* or “a proposition without demonstration” [28, 9]. No complete proof is possible [28, 9], and all attempts in this direction must be understood merely as a “reduction to simpler considerations, which are in the end *curae posteriores*” [28, 10]. He obviously wanted to spell out that “something conventional” [28, 10] had to be introduced into analytical mechanics at its very beginnings. (Jacobi’s motivation is analyzed in more detail in Section 6.) The most rewarding way to achieve this end was to elaborate on the attempts of Lagrange—the outstanding representative of analytical mechanics and the author of its most “important and authoritative” textbook [28, 29]—to make the principle of virtual velocities evident. Jacobi’s criticism applied to both of Lagrange’s demonstrations of (3), but only in the context of the second one did he discuss the implications of their shortcomings for the transition from statics to dynamics, i.e., for a derivation of the dynamic form (1) from the static form (3).

4.1. Lagrange’s First Proof

Lagrange’s first attempt was based on the so-called “principle of the pulleys.” He introduced, as a mere thought-instrument, a set of massless and frictionless pulleys, an inextensible cord, and a mass with a unit-weight. His leading idea was to represent forces P, Q, \dots , acting on different points of the mechanical system by pairs of these pulleys and suitable winding numbers m, n, \dots for the cord, provided that the tension of the cord is equal at all points.⁷ (See Fig. 1.)

Lagrange had to assume at this point that the proportions P, Q, \dots are rational, so that it is always possible to find such integers m, n, \dots , if these integers are chosen

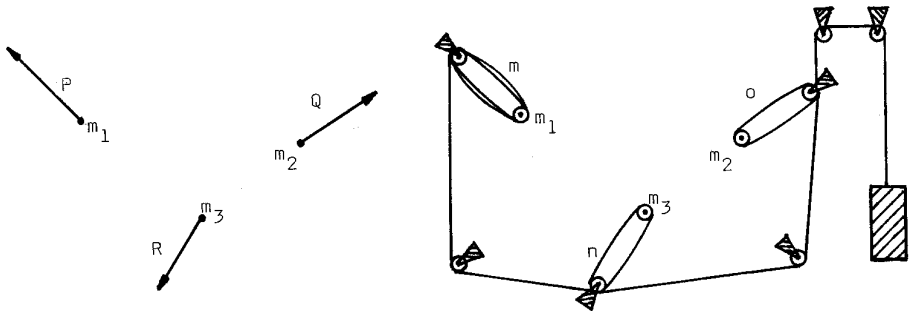


FIG. 1. Lagrange’s first proof.

⁷ See [35] for Lagrange’s original discussion or Part I, Sect. 18 of the new edition of the *Mécanique analytique* [39, 1: 23–25]. The following illustration, as well as the one given in the next section, is taken from Jacobi’s *Analytische Mechanik* [28, 26, 90]. Both were added by the editor and cannot be found in Jacobi’s original lectures (nor, of course, in Lagrange’s mechanics). The notations are adapted to those used in the text.

sufficiently large. For the sake of simplicity, we can then *identify* the measures of forces P, Q, \dots and the numbers of windings m, n, \dots . Taking this for granted, the expression for the total virtual moment can be expressed quite easily in geometrical terms:

$$Pdp + Qdq + \dots = mdp + ndq + \dots \quad (4)$$

If the system is subjected to a small displacement, mdp is the change of length of the cord for the first pair of pulleys, ndq for the second, \dots , and the sum on the right is the *total change* of length of the cord. If this sum is zero, (4) will supply the wanted “analytical expression of the principle of virtual velocities” [39, 1: 25].

Lagrange also took into account that the case of $Pdp + Qdq + \dots$ being *negative* might describe equilibrium “because it is impossible that the weight moves upward by itself” [39, 1: 25]. In this case, however, the displacements dp, dq, \dots can always be replaced by $-dp, -dq, \dots$, and (4) will be positive, demonstrating that “the opposite displacement, which is equally possible, will cause the weight to descend and destroy the equilibrium” [39, 1: 24].

But *when* can the total length of the cord $mdp + ndq + \dots$ be assumed to be zero? Lagrange held it to be “evident that in order to maintain the system \dots in equilibrium, it is necessary that the weight cannot descend as a result of any arbitrarily infinitesimal displacement of the system’s points; as weight always has the tendency to descend it will—if there is a displacement of the system inducing it to descend—necessarily do so and produce this displacement of the system” [39, 1: 24].

Jacobi, reading this remark of Lagrange to his students, could not keep calm when he came to the word “evident [offenbar]”: “This is certainly a bad word, wherever you find it, you can be sure, that there are serious difficulties; [using] it is an evil habit of mathematicians \dots ” [28, 29]. In other words, where Lagrange asserted evidence and mathematical exactitude, Jacobi ascertained darkness and logical incorrectness.

Two conclusions on which Lagrange’s reflections are based have to be distinguished in order to understand the structure of Jacobi’s argument clearly. Let arbitrary infinitesimal movements be applied to the mechanical system. Then these two cases are possible:

(L1a) If no movement causes the weight to descend, the system is in a state of equilibrium. In this case, (4) equals zero.

(L1b) If there is (at least) one movement that lets the weight descend, the system is not in equilibrium. The weight will necessarily sink down “by itself” and will produce a displacement of the system. In this case, (4) does not equal zero.

4.2. Jacobi’s First Refutation

According to Jacobi, both (L1a) and (L1b) are problematic from a logical and mathematical point of view. For the sake of brevity, I sum up his detailed discussion in four points [28, 29–39]:

(J1a) Conclusion (L1a) is *probably* right, if it is restricted to the case of stable equilibrium. (This restriction, however, is not admissible, as he argues in (J1d))

But even in this case, (L1a) cannot claim certainty because it is based on nothing but *empirical* evidence, especially because it implicitly follows the superficial observation that a weight can only move *downward* unless it is in a state of equilibrium.⁸ Already the simple pendulum shows, however, that the direction of a moving force is not necessarily the same as that of the movement itself. Jacobi concluded: “. . . you have to realize that these probable considerations are not more than probable, and must not be taken as a [mathematical] demonstration” [28, 32–33].

(J1b) Conclusion (L1b) is definitely wrong because it disregards unstable and median equilibrium. Again, it is sufficient to examine a simple pendulum, where the weight is placed perpendicularly above the point of suspension. This shows that a state of equilibrium is possible which is destroyed by all infinitesimal displacements not inserted in the direction of the connecting rod.⁹ Lagrange therefore was by no means entitled to claim that the weight will *necessarily* sink down because (L1b) is nothing but a sufficient condition for stable equilibrium. “It is very suspicious,” Jacobi said, “that Lagrange could use this form of deduction in spite of all the clear and well-known cases, where obviously the contrary proves to be true” [28, 31]—the more, I would add, as Lagrange clearly distinguished stable and unstable equilibrium in a later passage of his *Mécanique analytique* [39, 1:70–76]. Jacobi pointed out, however, that the failure of (L1b) “destroys the whole character of a demonstration” [28, 30].

(J1c) Even if one shares the opinion of Jacobi's Königsberg colleague and friend, Wilhelm Bessel, that unstable and median equilibria are irrelevant in physics because they are always and immediately destroyed by “forces whirring around” [28, 32], it would not be admissible to restrict a demonstration of the principle of virtual velocities to stable equilibrium: “. . . in the transition from statics to dynamics and in many other considerations it is not at all assumed that the equilibrium is stable, and if the propositions shall not be unnaturally restricted and lose their value, desisting from instantaneous equilibrium is not allowed” [28, 32]. (This point will be extensively discussed in the next part.)

(J1d) Jacobi finally stated that there is a “very important case, where Lagrange's consideration is not at all applicable, namely, if the conditions of the system are not represented by equalities, but by inequalities” [28, 35]. This point refers both to Lagrange's formulation and, of course, to his demonstration of the principle of virtual velocities. A more general formulation of this principle in the form

$$Pdp + Qdq + Rdr + \dots \leq 0 \quad (5)$$

⁸ René Dugas made the same point more than a century later in his work, *A History of Mechanics*: “We remark here, with Jouguet, that Lagrange's demonstration is based on physical facts—on certain principles of pulleys and strings” [12, 336]. The reference is to the second volume, *L'Organisation de la mécanique* (1909), of Émile Jouguet's *Lectures de mécanique* [29, 2: 179]. Jouguet had been Dugas's teacher.

⁹ Joseph Bertrand referred to this mistake some years later in an annotation to the third edition of the *Mécanique analytique* (1853): “On a objecté, avec raison, à cette assertion de Lagrange l'exemple d'un point pesant en équilibre au sommet le plus élevé d'une courbe . . .” [39, 1: 24]. Here, he probably had Dirichlet's paper “On the Stability of Equilibrium” [11] in mind which he mentioned later [39, 1: 71] and which he added to Lagrange's textbook [39, 1: 457–459]. It will be briefly discussed at the end of this section.

permits constraints expressed by inequalities (i.e., the case of a mass-point outside a sphere). A demonstration of (5) would have to abandon Lagrange's exclusion of negative total momentum described earlier because his argument that dp, dq, \dots could always be replaced by their opposites is no longer valid if constraints given by inequalities are taken into account. Fourier first extended Lagrange's axiom (3) to the more general form (5) and tried to base it upon the principles of the lever and the composition of forces.¹⁰ As Jacobi in this point explicitly followed Fourier's "Mémoire sur la statique" and subsequent papers by Gauss and Ostrogradsky [28, 38–39], there is no need to pursue his argument here. It is not important for a discussion of his *original* criticism.

Both (J1c) and (J1d) object to the lack of generality of Lagrange's axiom: (J1c) refers to the transition from (2) or (3) to the dynamic form (1), and (J1d) to the transition from (2) or (3) to the more general static form (5). But Jacobi's principal concern at this stage is *intension* rather than *extension*, as is revealed by (J1a) and (J1b). These objections aim at the vague meaning of equilibrium in Lagrange's first attempt. Missing here is not only a proper distinction between stable equilibrium and other forms of equilibrium but also a distinct conception of equilibrium altogether. According to Jacobi, equilibrium can only be assumed as a "mathematical fiction," as something that cannot be found in nature itself [28, 56, 58, 51]. Mixing up sense experience with mathematical reasoning in order to gain a proper understanding of this notion, as Lagrange did (J1a), is therefore not allowed. As a mathematical notion, it needs a proper *mathematical* definition. In Jacobi's words:

If we do not enter into all these probable considerations about ascent and descent because these are [only] appearances, we will say that equilibrium takes place if, for infinitesimally small displacements of the system of mass-points, the displacement of the weight is an infinitesimal magnitude of the 2nd order, so that the infinitesimal part of the 1st order vanishes. Whether this infinitesimal magnitude of the 2nd order is positive or negative is all the same, it can be positive for all displacements as well as negative, or rather positive for some and negative for some other [displacements], and equilibrium always takes place . . . [28, 35]

Jacobi did not reveal what inspired this weak definition of equilibrium. There can be little doubt, however, that it had its origin in a talk "On the Stability of Equilibrium" [11] given by his Berlin colleague and close friend, Johann Peter Gustav Lejeune Dirichlet, before the Prussian Academy of Science in January 1846. The lecture was published the same year, i.e., about one year before Jacobi's lectures

¹⁰ See his "Mémoire sur la statique" [14] from 1798. Fourier defines moments with the plus and minus signs reversed, and therefore presents (5) with \geq instead of \leq . Though Jacobi was interested in Fourier's extension (5) of Lagrange's principle, he did not analyze Fourier's elaborate attempts with sufficient "carefulness to present a clear demonstration of a principle, which serves as the foundation of mechanics" [14, 36]. The reason for this omission might well be that the core of Jacobi's criticism of Lagrange, namely, his inadequate notion of *equilibrium* (see below), is not called into question by Fourier's paper. Quite the opposite: Discussing the stability of equilibrium, Fourier refers to the "very elegant analysis of the famous author of the *Mécanique analytique*" [14, 47; cf. 39, 1: 69–76]. For profound discussions of Fourier's "Mémoire," see [2, 47–52; 9; 12, 361–366; 23, 1: 303–308; 41, 166–169].

on *Analytische Mechanik* were given. Without going into the details of Dirichlet's paper, this hypothesis suggests itself.

Dirichlet demonstrated *directly*, that the maximum of a function φ (in modern terms, the minimum of the potential energy Π , where $\varphi = -2\Pi$) characterizes the stable equilibrium of a mechanical system. Lagrange, on the other hand, gave an *indirect* demonstration of the same result based on a development of Π in a power series. He assumed that one was allowed to break off this development after terms of the second order, and that these terms could be presented as a sum of negative squares in case of a minimum of Π . Then he could show in the third section of his *Mécanique analytique* that a minimum of Π yields a stable equilibrium and a maximum an unstable equilibrium of the system [39, 1: 72–74].

Dirichlet criticized Lagrange's assumption as groundless for obvious reasons [11, 6], and Jacobi shared this criticism. In this context, it is important to note that Lagrange described his characterization of stable and unstable equilibrium as "a direct consequence of the demonstration of the principle of virtual velocities which we have given at the end of the first section" [39, 1: 74]. Jacobi obviously became suspicious of this consequence as a result of Dirichlet's talk¹¹ and then turned his attention to Lagrange's purported demonstration itself. Jacobi's definition of equilibrium, quoted above, takes into account Dirichlet's criticism and leaves no room for Lagrange's attempt at supplying evidence for the principle of virtual velocities by the principle of pulleys. For Jacobi, it can no longer count as a mathematical demonstration, but merely as an artificial "construction" [28, 24].

If I am right in attributing the roots of Jacobi's first criticism to Dirichlet, this manifest shift of mathematical rigor might well illustrate what Jacobi appreciated most in his friend's contributions to mathematics, i.e., his high standards of exactitude and accuracy. In a letter to Alexander von Humboldt from December 1846, Jacobi said of Dirichlet that "only he, not me, not Cauchy, not Gauss knows, what a completely rigorous mathematical demonstration is . . . we only know it from him. When Gauss says, that he *demonstrated* something, it seems to me very probable, when Cauchy says it, you can likewise bet on it or against it, when Dirichlet says it, it is *certain* [gewiss] . . ." [48, 99].

5. PRACTICAL EUCLIDEANISM AND ITS PROBLEMS: (2) THE TRANSITION FROM STATICS TO DYNAMICS AND JACOBI'S CRITICISM OF LAGRANGE'S SECOND DEMONSTRATION

5.1. Lagrange's Second Proof

Some months before he died, Lagrange attempted another demonstration of the principle of virtual velocities. This last proof of his Euclideanistic spirit can be found in the second edition of his *Théorie des fonctions analytiques* from 1813 [37, 350–357; 38, 377–385].

¹¹ And rightly so, as is confirmed anew by Joseph Bertrand's remark about Lagrange's argument in the third edition of the *Mécanique analytique* (1853). For the case of equilibrium, Lagrange holds it to be "evident" that the forces P , Q , R can be assumed to be constant. Bertrand correctly criticizes this step, which "on the contrary, completely changes the nature of the function" Π [39, 1: 75].

Again, Lagrange started with the principle of pulleys, as he called the application of his thought-instrument. This time, however, he did not use the pulley as a substitute for forces acting on different masses from *outside*, but to represent the *inner* connections or constraints between the masses. Technically, Lagrange's second construction is much more complex than the first, and so is Jacobi's second destruction. Now, the transition from statics to dynamics, which had hitherto been peripheral, becomes important. In order to point out just the leading ideas, I confine my analysis at this point to Lagrange's discussion of a mechanical system with two masses and one condition of constraint [38, 379–383], though all considerations can be extended to the general case with m constraints and n masses ($m < 3n$) [38, 383–385]. Lagrange's demonstration can be reconstructed in three steps, of which the second one is of special importance for Jacobi's discussion:

(L2a) Let there be two masses M and N with coordinates x_1, y_1, z_1 and x_2, y_2, z_2 at rest. If these masses underlie a time-independent mutual action or constraint, this constraint can be expressed by a function [38, 382]

$$u(x_1, y_1, z_1, x_2, y_2, z_2) = 0. \quad (6)$$

Taking M or N as fixed, (6) determines a certain two-dimensional surface for the remaining mass N or M , respectively. Lagrange wanted to show that the force of constraint can be assumed to be *perpendicular* to this surface, i.e., that the force on M and N can be expressed in this way:

$$\begin{aligned} \text{Force on } M: & \left(\lambda \frac{\partial u}{\partial x_1}, \lambda \frac{\partial u}{\partial y_1}, \lambda \frac{\partial u}{\partial z_1} \right); \\ \text{Force on } N: & \left(\lambda \frac{\partial u}{\partial x_2}, \lambda \frac{\partial u}{\partial y_2}, \lambda \frac{\partial u}{\partial z_2} \right). \end{aligned} \quad (7)$$

The Lagrange multiplier λ is an *indeterminate* coefficient at the moment.

We will see in (L2c) that Lagrange needed the special mathematical form (7) for his demonstration. Here, a more general aspect of (7) merits emphasis. Being able to express the forces of constraint in such a way by the constraint (6) itself, is of the utmost importance for his approach because there is a gap between his purely mathematical representation of rigid, geometrically conceived constraints, on the one hand, and physical actions given by force functions (by the law of gravity, for example), on the other hand. Equation (7) is meant to *bridge* this gap, in the sense that it provides quantitative forces corresponding to certain geometrical constraints, thus making the forces of constraint comparable with physical forces from outside. I will come back to this point below.

(L2b) To achieve the representation of the forces of constraints (7), Lagrange assumed that constraint (6) can be replaced by an ideal pulley, its inextensible cord being fixed at point M , wound m times around M and a fixed roll A , then going to another fixed roll B , wound n times around B and the second point N , and then

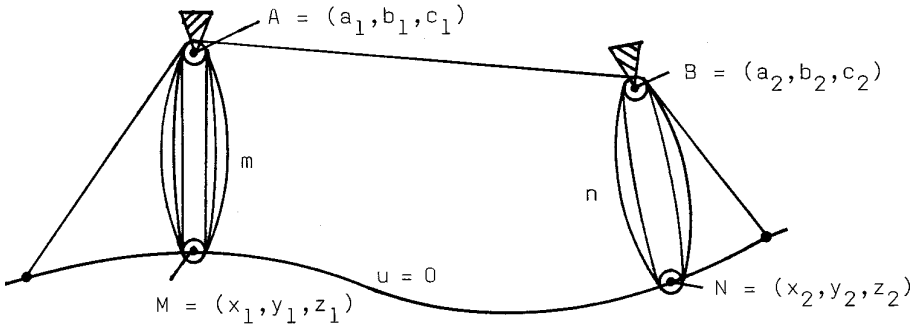


FIG. 2. Lagrange's second proof.

fixed at N . (See Fig. 2.) The inextensibility of the cord can be expressed by the equation [38, 382]

$$f = m \overline{AM} + n \overline{BN} - d = 0, \tag{8}$$

with a certain length-constant d . The function f takes the form

$$f = m \sqrt{(x_1 - a_1)^2 + (y_1 - b_1)^2 + (z_1 - c_1)^2} + n \sqrt{(x_2 - a_2)^2 + (y_2 - b_2)^2 + (z_2 - c_2)^2} - d \tag{9}$$

and has all the properties Lagrange needs for his aims. It is easy to see that the pulley forces can be expressed by the partial derivatives of f , as is assumed for u in (7). But is it legitimate to replace the constraint function u by the pulley function f ?

The function f contains nine arbitrary constants: the six coordinates of the points A and B , the length constant d , and the two winding-numbers m and n . Obviously, the special form of f imposes these two conditions on its derivatives:

$$\left(\frac{\partial f}{\partial x_1}\right)^2 + \left(\frac{\partial f}{\partial y_1}\right)^2 + \left(\frac{\partial f}{\partial z_1}\right)^2 = m^2; \quad \left(\frac{\partial f}{\partial x_2}\right)^2 + \left(\frac{\partial f}{\partial y_2}\right)^2 + \left(\frac{\partial f}{\partial z_2}\right)^2 = n^2. \tag{10}$$

So there are seven constants left. In order to substitute u for f , the corresponding surfaces must touch each other. This geometrical demand is fulfilled if the first partial derivatives of f and u are identical:

$$\begin{aligned} \frac{\partial f}{\partial x_1} &= \frac{\partial u}{\partial x_1}, & \frac{\partial f}{\partial y_1} &= \frac{\partial u}{\partial y_1}, & \frac{\partial f}{\partial z_1} &= \frac{\partial u}{\partial z_1}, \\ \frac{\partial f}{\partial x_2} &= \frac{\partial u}{\partial x_2}, & \frac{\partial f}{\partial y_2} &= \frac{\partial u}{\partial y_2}, & \frac{\partial f}{\partial z_2} &= \frac{\partial u}{\partial z_2}. \end{aligned} \tag{11}$$

These six ‘‘conditions of contact,’’ as Lagrange called them [38, 382], can always be fulfilled by adapting the seven various constants. It is therefore admissible to replace u by f . The pulley forces fulfill the orthogonality condition (7) due to the special form (9) of function f . Condition (7) is consequently valid for arbitrary functions u , given by (6). The coefficient λ in (7) is the remaining one of the nine

undetermined constants and can be interpreted physically as the tension of the pulley's rod.

(L2c) In the case of equilibrium, these forces of constraint are balanced by free forces from outside for *every single* mass-point. We can therefore replace each component of the free forces by the corresponding partial derivative of the function u according to (7). In order to get the total moment of the mechanical system needed in our expression of the principle of virtual velocities (2), we have to sum up the product of these partial derivatives and arbitrary displacements, expressed by $\delta x_i, \dots$. The result is

$$\sum_{i=1}^2 (X_i \delta x_i + Y_i \delta y_i + Z_i \delta z_i) = \sum_{i=1}^2 \lambda \left(\frac{\partial u}{\partial x_i} \delta x_i + \frac{\partial u}{\partial y_i} \delta y_i + \frac{\partial u}{\partial z_i} \delta z_i \right) = 0. \quad (12)$$

The last equality holds because the displacements $\delta x_i, \dots$ have to be compatible with the constraint (6), implying that the total differential of u must be zero ($du = 0$).

Equation (12) supplies the analytical expression of the principle of virtual velocities (2) for two masses and one constraint. It also indicates that the argument is valid for an arbitrary number of mass points n and a number of constraints m ($u_1 = 0, \dots, u_m = 0$) because, in this case, we can repeat the argument with m pulleys (with $du_1 = 0, \dots, du_m = 0$) and get the same result (2). Lagrange therefore contentedly closed his second attempt with the remark that “the principle of virtual velocities becomes a natural consequence of the formulas which express the forces by the equations of the conditions [of constraint]” [38, 1: 385].

5.2. Jacobi's Second Refutation

Richard Lindt, in his otherwise excellent historical analysis of the principle of virtual velocities from 1904, still described Lagrange's second demonstration as “exact and general” [41, 163]. Jacobi, however, would have rejected both attributes. His criticism referred mainly to the second step (L2b), that is, to the replacement of the constraint function u by the pulley function f . In order to show that Lagrange was wrong, he concentrated on what he regarded as the weakest point in the whole of Lagrange's mechanics:

The transition from statics to dynamics generally means a simplification of matters and indeed—reading the *mécanique analytique* makes you believe that the equations of motion follow from those of equilibrium. This, however, is not possible, if the laws are known only in respect to bodies at rest. It is a matter of certain probable principles, leading from the one to the other, and it is essential to know that these things have not been demonstrated in a mathematical sense but are merely assumed. [28, 59]

This argument again refers to Lagrange's claim to have given a *mathematical* demonstration of his fundamental principle. Jacobi rejected this claim on mathematical and metamathematical grounds. Here, I confine myself to his mathematical discussion, which considered the range of Lagrange's method of multipliers in the context of his second demonstration. As shown above (for $m = 1$), these multipliers

are introduced as undetermined coefficients, but they have a special mechanical meaning. They express the way the constraints affect the system, or, in more physical terms, they are proportional to the force exerted by a constraint. We can get rid of a constraint $u = 0$ on a mass with coordinates (x, y, z) , if we apply a force

$$\left(-\lambda \frac{\partial u}{\partial x}; \quad -\lambda \frac{\partial u}{\partial y}; \quad -\lambda \frac{\partial u}{\partial z} \right), \quad (13)$$

where the minus indicates that this force has the *opposite* direction to the force of constraint. In algebraic terms, this simply means that we subtract this force from both sides of the equation. It is, Jacobi says, the “essence of [Lagrange’s] analytical mechanics that simple algebraic operations correspond to mechanical considerations. This substitution of constraints by forces corresponds to bringing terms on the other side of the equations” [28, 54]. To accomplish this substitution, it is obviously necessary to determine the Lagrange multipliers of the system. Jacobi was mainly concerned with the problems of *how* this works if we proceed from statics to dynamics and *if* this procedure is compatible with Lagrange’s second demonstration. Following his extensive algebraic discussion [28, 54–92], I will now turn to the general case of a system with n points and m constraints ($m < 3n$) and first provide some well-known formulas.¹²

The independent constraints and their first two derivatives with respect to time, which are needed later, are given by [28, 41, 61, 84–85]

$$u_k = 0, \quad k = 1, \dots, m, \quad (14a)$$

$$\frac{du_k}{dt} = \sum_{i=1}^{3n} \frac{\partial u_k}{\partial x_i} \frac{dx_i}{dt} = \sum_{i=1}^{3n} \frac{\partial u_k}{\partial x_i} \dot{x}_i, \quad (14b)$$

$$\frac{d^2 u_k}{dt^2} = \sum_{i,j=1}^{3n} \frac{\partial^2 u_k}{\partial x_i \partial x_j} \dot{x}_i \dot{x}_j + \sum_{i=1}^{3n} \frac{\partial u_k}{\partial x_i} \ddot{x}_i. \quad (14c)$$

According to Lagrange, these constraints can be incorporated into the principle of virtual velocities (Eqs. (1) and (2)) by restricting the variations $\delta x_i, \dots$ according to (14a), but also by adding the corresponding variations, multiplied by the Lagrange

¹² It is important to note that the letters a, b, c in the following markings of formulas correspond to Jacobi’s three steps (J2a), (J2b), (J2c) described below. Identical numbers refer to corresponding formulas in the three different steps (that is why not all numbers appear in all steps). For the sake of brevity, I will no longer use Lagrange’s and Jacobi’s Cartesian representation, but a canonical form. The coordinates of the n mass-points are named x_1, \dots, x_{3n} , with masses m_1, \dots, m_{3n} (where $m_1 = m_2 = m_3$, etc.) and force components X_1, \dots, X_{3n} . The indices i and j always run from 1 to $3n$ and the indices k and l always run from 1 to m ($m < 3n$). In order to make the parallel between statics and dynamics more transparent, I will continue to use the dynamic principle of virtual velocities in the form (1), though Lagrange wrote the accelerating force and the external force as a sum [39, 1: 274] and though Jacobi inverted the order in the differences of (1) [28, 6]. A welcome consequence of my writing is that from (15a) onward, all terms belonging to the constraints appear on the *right* side of the equations. This does not change, of course, any of Jacobi’s arguments.

multipliers, to the total moment [39, 1: 79]. In the latter case, the static form (2) of the principle of virtual velocities becomes [28, 50]:

$$\sum_{i=1}^{3n} X_i \delta x_i = \sum_{l=1}^m \lambda_l \delta u_l. \quad (15a)$$

In the dynamic case, (1) changes to [28, 43] (for (15b), see below):

$$\sum_{i=1}^{3n} (X_i - m_i \ddot{x}_i) \delta x_i = \sum_{l=1}^m \lambda_l \delta u_l. \quad (15c)$$

Again, if equilibrium holds, (15a) supplies these $3n$ equations of rest [28, 54]:

$$X_i = \sum_{l=1}^m \lambda_l \frac{\partial u_l}{\partial x_i}. \quad (16a)$$

Mutatis mutandis, (15c) leads to these $3n$ equations of motion [28, 44] (for (16b), see below):

$$X_i - m_i \ddot{x}_i = \sum_{l=1}^m \lambda_l \frac{\partial u_l}{\partial x_i}. \quad (16c)$$

After these preliminaries, we turn to Jacobi's argument in some detail. As in Lagrange's discussion, it is convenient to isolate three steps:

(J2a) The case of *equilibrium*, i.e., the mechanical system subject to the given forces X_i from outside and m given constraints (14a), is at rest, and the position of the n mass-points has to be determined [28, 48]. This poses no problem for Lagrange's method of multipliers. There are $3n + m$ equations (16a) and (14a) to determine the m Lagrange multipliers λ_l and the $3n$ coordinates of the mass-points. If these $3n + m$ equations do not contradict each other, the problem is always solvable. After determining the λ_l , we can interpret the system as a free one by adding on both sides of (16a) the negative of the sum of all forces of constraint [28, 54–56]:

$$X_i - \sum_{l=1}^m \lambda_l \frac{\partial u_l}{\partial x_i} = 0. \quad (17a)$$

In particular, we find that the direction of the forces of constraint is always determined by the constraints themselves (i.e., orthogonal to the corresponding surface), while their intensities (magnitudes of λ_l) depend on the constraints *and* the external forces [28, 56].

How can this simple procedure be transferred to a *dynamic* system? This question was at the center of Jacobi's further discussion [28, 54–92]. Again, the principle of virtual velocities (15c) supplies $3n$ equations, but these are now the second-order *differential* equations of motion (16c). If we assume that the system (16c) is solvable and is indeed solved, a difficult problem remains—at least if we take Lagrange's basic idea seriously that constraints can always be replaced by suitable forces. We

have to determine the $6n$ arbitrary constants belonging to the solution of (16c) in accordance with the constraints (15). In Jacobi's words: "... when everything has been completely integrated, this problem has to be solved only to determine the constants" [28, 60]. In more physical terms, the initial positions and velocities of the mass-points must be chosen in such a manner that the movement itself conforms with the constraints for all later times.

(J2b) Jacobi now inserted an *intermezzo* that can be interpreted as a step to bridge the gap between (15a) and (15c) and their solutions (16a) and (16c). What happens if an instantaneous impulse of finite size, incompatible with the constraints, is exerted on the system at rest? The real movement of the mass-points must, of course, be modified according to the constraints.¹³ If a_i are these "material, mechanically given" impulses at $t = 0$ and if \dot{x}_i are the corresponding velocities actually adopted by the mass-points of the system, their relation is given (by analogy to (15c) and (16c)) by the equations [28, 61]

$$\sum_{i=1}^{3n} (X_i - m_i \dot{x}_i) \delta x_i = \sum_{l=1}^m \lambda_l \delta u_l, \tag{15b}$$

$$a_i - m_i \dot{x}_i = \sum_{l=1}^m \lambda_l \frac{\partial u_l}{\partial x_i}. \tag{16b}$$

Equation (16b) shows that a given impulse a_i is divided into the impulse $m_i \dot{x}_i$, which is actually adopted by the system, and the impulse on the righthand side, which is taken up by the constraints.

In order to determine the Lagrange multipliers λ_l and subsequently the real impulses, one can substitute for the velocities \dot{x}_i in (14b) by means of (16b). The result is [28, 63]

$$\sum_{i=1}^{3n} \frac{\partial u_k}{\partial x_i} \frac{1}{m_i} \left(a_i - \sum_{l=1}^m \frac{\partial u_l}{\partial x_i} \right) = \sum_{i=1}^{3n} \frac{1}{m_i} a_i \frac{\partial u_k}{\partial x_i} - \sum_{l=1}^m \lambda_l \frac{1}{m_i} \frac{\partial u_k}{\partial x_i} \frac{\partial u_l}{\partial x_i} = 0$$

or

$$b_k = \sum_{l=1}^m A_{kl} \lambda_l, \tag{17b}$$

where

$$b_k := \sum_{i=1}^{3n} \frac{1}{m_i} a_i \frac{\partial u_k}{\partial x_i}, \quad A_{kl} := \sum_{i=1}^{3n} \frac{1}{m_i} \frac{\partial u_k}{\partial x_i} \frac{\partial u_l}{\partial x_i}. \tag{18b}$$

¹³ Jacobi had already discussed this point in his *Dynamik* of 1842–1843 but did not use it in his discussion of Lagrange's second demonstration. See [26, 54–56].

Jacobi now showed in a detailed algebraical excursion,¹⁴ that the multipliers λ_l can always be determined by (17b) if and only if the m constraints are independent [28, 79]:

$$\det(A_{kl}) \neq 0. \quad (19b)$$

This time, the multipliers λ_l depend on the constraints due to the first partial derivatives in (14b) *and* on the initial impulses a_i . When the multipliers are found from (17b), the impulses belonging to the constraints (righthand side of (16b)) can be determined, too. By analogy to (17a), the mechanical system can be treated as a free one on subtracting these components [28, 81]:

$$a_i - m_i \ddot{x}_i - \sum_{l=1}^m \lambda_l \frac{\partial u_l}{\partial x_i} = 0. \quad (20b)$$

Lagrange's idea of eliminating the constraints of a mechanical system by means of his method of multipliers thus works for this *intermezzo*, too [28, 82]. The procedure described guarantees that the arbitrary constants in the system of differential equations (15c) can be determined appropriately, and this is a *conditio sine qua non* for mastering the dynamic problem itself.

(J2c) In the case of *dynamics* the mass-points underlie not only constraints (14a) but also continuously acting forces X_i according to (15c). It is by no means evident and must therefore be proven "that the really moving forces [$m_i \ddot{x}_i$ in (15c)] do not violate the constraints of the system" [28, 83]. The procedure is analogous to (J2b), with (15b) and (16b) replaced by (15c) and (16c). As the accelerations in (16c) have to be compatible with the constraints, we can replace the \ddot{x}_i in (14c) by means of (16c) and get [28, 85–86]:

$$\begin{aligned} & \sum_{i,j=1}^{3n} \frac{\partial^2 u_k}{\partial x_i \partial x_j} \dot{x}_i \dot{x}_j + \sum_{i=1}^{3n} \frac{\partial u_k}{\partial x_i} \left(X_i - \sum_{l=1}^m \lambda_l \frac{\partial u_l}{\partial x_i} \right) \\ &= \sum_{i,j=1}^{3n} \frac{\partial^2 u_k}{\partial x_i \partial x_j} \dot{x}_i \dot{x}_j + \sum_{i=1}^{3n} \frac{1}{m_i} X_i \frac{\partial u_k}{\partial x_i} - \sum_{l=1}^m \lambda_l \sum_{i=1}^{3n} \frac{1}{m_i} \frac{\partial u_k}{\partial x_i} \frac{\partial u_l}{\partial x_i} = 0 \end{aligned}$$

or

$$U_k + c_k = \sum_{l=1}^m A_{kl} \lambda_l. \quad (17c)$$

The A_{kl} are still the same as in (18b), but U_k and c_k are given by

¹⁴ This excursion is of some historical interest, too, as it largely relies on the theory of determinants from Cramer, Gauss, and Cauchy and, of course, on his own investigations of determinants. For all details, however, I have to refer to the commentaries on the passage in question (footnotes 97 to 126 in [28, 63–79]). Nevertheless it should be mentioned that Jacobi excused Lagrange's omission of such an investigation: "... there are several such cases in the *mécanique analytique* which Lagrange could not solve because of the unsatisfactory state of algebraical knowledge at that time" [28, 65].

$$U_k := \sum_{i,j=1}^{3n} \frac{\partial^2 u_k}{\partial x_i \partial x_j} \dot{x}_i \dot{x}_j, \quad c_k := \sum_{i=1}^{3n} \frac{1}{m_i} X_i \frac{\partial u_k}{\partial x_i}. \quad (18c)$$

Equation (17c) provides a system of equations to determine the multipliers λ_l as did (17b). Again, their determination is possible if the constraints are independent, i.e., if (19b) is fulfilled. The new function U_k , however, implies an important difference between (17b) and (17c) and changes, as Jacobi said, “the whole character” of the problem [28, 84].

U_k is a homogeneous function of second order in the velocities \dot{x}_i , and its coefficients are the second derivatives of the constraint functions (14a). Consequently, the multipliers λ_l will depend on these *second* derivatives and will become homogeneous functions of the velocities, too. But Lagrange, in his second demonstration of the principle of virtual velocities, only demanded *first*-order approximation when he replaced the constraint functions u_i with the pulley functions f_i . There were no conditions whatsoever imposed on their second derivatives:

From this results an objection to the transition from statics to dynamics. The principle of statics does not deal at all with points in motion and a particular inquiry, a particular principle has to be assumed, as to how the velocities are constituted and modified In the equations we just formed, the second differential equations of the constraints u appear, but in considerations which aim at a demonstration of these propositions, and of which Lagrange especially made use in his *théorie des fonctions*, the given constraints are substituted without further notice by a different system of constraints, which underlies only the one restriction, that the new constraints are identical both in the positions of the material points at a certain time and in the first differential equations [28, 86]

It is convenient to distinguish two levels in Jacobi's analysis: a technical one and a more fundamental one. At the technical level, he criticized Lagrange's replacement of constraint functions with pulley functions as unjustified because the contact conditions (11) are too weak for this purpose. On the fundamental level, however, he questioned Lagrange's very idea that a system under constraints can be treated as a free one using the method of multipliers. In the dynamic case (J2c), there is no equivalent to the formulas (17a) or (20b), unless we *introduce* such a formula by analogy as a “new principle” [28, 87–88] in the form of (15c), respectively (16c). The interpretation of these equations with determined multipliers λ_l makes it clear why this principle is really new and why “something conventional” [28, 10] has to be assumed in the transition from statics to dynamics. Equation (16c) means geometrically that the difference between the external forces X_i and the “really moving forces” $m_i \ddot{x}_i$ is *orthogonal* to the constraints, as the forces X_i were for resting mass-points in (16a): “This, however, is a principle, which is out of the question in statics . . .” [28, 87]. As was shown, the λ_l depend on the velocities of the moving bodies, and it is by no means evident that we should assume orthogonality for the dynamic case, too [28, 88].

Jacobi's criticism corroborates physical intuition. While quasi-geometrical constraints can legitimately be used for bodies at rest, which underlie certain forces, they involve difficulties for bodies in motion and reacting upon their constraints on account of their movement. Jacobi therefore summed up his criticism with the

remark that Lagrange, in his treatment of dynamic problems, mixed up two different kinds of mechanical conditions—physical forces acting from outside and mathematically given constraints—which are in reality “quite heterogeneous” [28, 87]. He does not, however, base his whole argument on physical intuition. Apparently, he wanted to see Lagrange’s conception of analytical mechanics hoist with its own petard, i.e., with nothing but *mathematical* reasoning.

6. DISMISSING MECHANICAL EUCLIDEANISM: THE CHANGING ROLE OF MATHEMATICS

Jacobi’s discussion of the principle of virtual velocities and his analysis of Lagrange’s two demonstrations in the *Analytische Mechanik* (1847–1848) is the most significant addition to the *Dynamik* (1842–1843). But *why* did he spend about one quarter of his last lectures on this point? I have discussed the historical background of his new critical attitude toward the foundations of mechanics in some detail elsewhere [58, 502–505; 59, XXXIX–XLVIII]. Here, I consider only one aspect of his development, but—with respect to his criticism of Lagrange—probably the most important one: his new attitude toward the role of mathematics in mechanics and physics in general. Both Jacobi’s admiration for and his reservations about the great project of a purely analytical mechanics are best expressed in these words to his students:

... you see here a purely mathematical operation as a perfect counterimage of things happening in nature, this is in a way always the task of applied mathematics. . . . Everything is reduced to mathematical operation This means the greatest possible simplification which can be achieved for a problem . . . , and it is, in fact, the most important idea stated in Lagrange’s analytical mechanics. This perfection, however, has also the disadvantage that you do not study the effects of forces any longer Nature is totally ignored and the constitution of bodies (whether their elements are inflexible, expansible, elastic, etc.) is replaced merely by the defined equation of constraint. Analytical mechanics here clearly lacks any justification; it even abandons the idea of justification in order to remain a pure mathematical science. [28, 193–194]

Jacobi’s reproach has two different aspects. First, he rejected Lagrange’s purely analytical mechanics for its inability to describe the behavior of real physical bodies. In this respect, he shared the view of those French mathematicians in the tradition of Laplace who called for a “*mécanique physique*” instead of a “*mécanique analytique*,”¹⁵ though he had criticized exactly this “school of the famous Earl of Laplace” [31, 114] 15 years earlier (recall Section 3). However remarkable this shift is, it only concerns low-level adaptations of mathematical techniques to certain physical demands. It does not affect the foundations of mechanics itself.

¹⁵ See [53, 361; 13, 37–46], also part 2 (esp. note 5) and part 3 above. Poisson’s distinction of “*mécanique physique*” and “*mécanique analytique*” from 1829 is foreshadowed in Poinsot’s *Sur la théorie générale de l’équilibre* from 1806 where he distinguishes “*mécanique rationnelle*” and “*mécanique physique*” [52, Appendix, p. 24]. It must be stressed, however, that Poinsot, Poisson, and other representatives of French mathematical physics at that time continued Lagrange’s mechanical Euclideanism (see Section 2 above). In other words, they differed from Jacobi not in the first, but in the *second*, and more important, aspect of his criticism of Lagrange discussed above.

The *second* aspect, however, does so because it concerns the status of first principles of mechanics. For Lagrange, the principle of virtual velocities was vital to an axiomatic-deductive organization of mechanics, and his two proofs were meant to save this Euclideanistic ideal. His whole program of reducing mathematics to analysis and analysis to algebra aimed at a secure foundation of mechanics by mathematics. In so far as *this* concept “lacks any justification” and “even abandons the idea of justification in order to remain a pure mathematical science,” as Jacobi said, it can rightly be described as “dogmatic” [19, 4].

Contrary to Lagrange, Jacobi used mathematics with respect to the foundations of mechanics not dogmatically but critically. He applied his analytical and algebraical tools systematically in order to show that mathematical demonstrations of first principles *cannot* be achieved. He neither said that all attempts in this direction are in vain nor found his forerunner's attempts equally bad. He regarded Lagrange's second attempt as more plausible than the first one [28, 92–93], and Poinot's demonstration [49] as better than Lagrange's second one, since it makes the principle of virtual velocities “intuitive [anschaulich]” [28, 93–96]. But intuitive knowledge is not inferential knowledge; it is *not* based on unquestionable axioms and strict logical and mathematical deduction. Jacobi, the representative of pure mathematics, dismissed Euclideanism as an ideal of any science that transcends the limits of pure mathematics. The formal similarity between the mathematical-deductive system of analytical mechanics and a system of pure mathematics (such as number theory, for example) must not lead to the erroneous belief that both theories meet the same epistemological standards, especially that the first principles of mechanics or axioms are as certain and evident as the axioms of pure mathematics. For this reason, Jacobi explicitly warned his students of the traditional view of mechanical laws at the beginning of his lecture:

From the point of view of pure mathematics, these laws cannot be demonstrated; [they are] mere conventions, yet they are assumed to correspond to nature Wherever mathematics is mixed up with anything, which is outside its field, you will however find attempts to demonstrate these merely conventional propositions *a priori*, and it will be your task to find out the false deduction in each case. . . .

There are, properly speaking, no demonstrations of these propositions, they can only be made plausible; all existing demonstrations always presume more or less because mathematics cannot invent [*sich aus den Fingern saugen*] how the relations of systems of points depend on each other. [28, 3, 5]

Jacobi's point of view is that of the *pure* mathematician, drawing a line between mathematics itself and “anything which is outside its field.” This marks a striking contrast to Lagrange's physico-mathematician's point of view and to the French tradition of mathematical physics in general. An adequate understanding of this difference cannot be reached without analyzing the role of mathematics in the diverging disciplinary contexts [31].

Jacobi proceeded from Lagrange's assumption that the conception of a mathematically and axiomatic-deductively organized mechanics stands and falls with the certainty of the principle of virtual velocities, and he wanted it to fall. He wanted

to make it definitely clear that Lagrange's "constructions" [28, 24, 89] must *not* be regarded as mathematical demonstrations and do not establish evidence and certainty of this principle. Jacobi therefore pointed out its shortcomings in great mathematical detail. To put it in a nutshell, he used mathematics in order to show that Euclideanism as an ideal of science is unattainable in mathematical physics and that "rubber Euclideanism" only conceals the basic difference between pure mathematics and the application of mathematics in the empirical sciences.¹⁶

Complementary to this negative and, in some respects, destructive discussion of Lagrange's mechanical Euclideanism, a positive and constructive view of the role of mathematics in mechanics can be found in Jacobi's last lectures. Mathematics offers a rich supply of *possible* first principles, and *neither* empirical evidence *nor* mathematical or other reasoning can determine any of them as true. Empirical confirmation is necessary, but can never provide certainty. First principles of mechanics, be they analytical or Newtonian, are not certain, but only *probably* true [28, 3, 5, 32, 59]. The certainty of such principles, the essential feature of mechanical Euclideanism, is replaced by fundamental *fallibility*. Moreover, the search for proper mechanical principles always leaves space for a *choice*. Due to the creative power of mathematics, there is a supply of possible principles, and a decision according to considerations of simplicity and plausibility is necessary. Jacobi consequently called first principles of mechanics "conventions" [28, 3, 5], exactly 50 years before Poincaré did. A detailed comparison with Poincaré's view, which would show both striking similarities and important differences, is beyond the scope of this paper [58, 514–515]. At least one central common point should be emphasized. For Poincaré *and* Jacobi, first laws of mechanics are not intrinsic laws of nature "deduced" from phenomena (Newton) nor are they principles imposed by reason (being synthetic *a priori* in the sense of Kant). According to Jacobi, however, they are not protected from empirical anomalies. They are creatures of mathematics, eligible and revisable according to empirical evidence and convenience. Mathematical instrumentalism, already practiced by Lagrange and propagated by the later tradition of analytical mechanics, inevitably leads to a dismissal of mechanical Euclideanism. Jacobi seems to be the first representative of analytical mechanics who drew this consequence.

7. CONCLUSION

Jacobi's *Analytische Mechanik* marks a turning point but no singularity in the history of the *Principia mathematica philosophiae naturalis* during the 19th century, as it had considerable influence on the scientific community. Bernhard Riemann,

¹⁶ As I have termed Lagrange's approach to mechanics "rubber Euclideanism" and Jacobi's criticism a dismissal of this position, it seems appropriate to end this part with Braithwaite, who probably inspired Lakatos's notion of Euclideanism [40, 2: 10–11, esp. n. 1]. There can be little doubt that Jacobi would have agreed with Braithwaite's remark: "The enormous influence of Euclid has been so good in inducing scientists to construct deductive systems as more than to counterbalance his bad influence in causing them to misunderstand what they were doing in constructing such systems; the good genius of mathematics and of unself-conscious science, Euclid has been the evil genius of philosophy of science—and indeed of metaphysics" [6, 353].

for example, attended Jacobi's lectures and rejected mechanical principles as axioms, as Jacobi did. Carl Neumann studied the *Analytische Mechanik* in great detail and developed mathematical techniques for movements under constraints along Jacobian lines. More importantly, he shared and articulated Jacobi's criticism of mechanical principles in his famous and influential inaugural lecture at Leipzig *On the Principles of the Galilei–Newtonian Theory* [44]. There is a line of mechanical non-Euclideanism starting with Jacobi that later led to serious doubts about the validity of so-called Newtonian mechanics [56; 59]. A development originating in analytical mechanics thus became important for classical mechanics in general. This tradition is quite independent of Ernst Mach's well-known criticism of absolute space and the law of inertia and *precedes* it. It is nevertheless widely neglected in the history of mathematics and physics. But just as the frequently drawn parallel between mechanics and geometry should be taken seriously, we should pay attention not only to the changes in the foundations of geometry but also to those of rational mechanics. Both areas deserve our attention. Neither is superfluous, if we really want to understand scientific change so frequently and carelessly called revolutionary.

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