After 150 Years: News from Jacobi about Lagrange's Analytical Mechanics

Helmut Pulte

[Lagrange's] Analytical Mechanics *is a book you have to be rather cautious about, as some of its content is more supernatural 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 entail 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.*

Carl Gustav Jacob Jacobi ([1996], 29) made this remark in his last lectures on analytical mechanics, which he delivered in Berlin in 1847/48, about three years before his death. The contrast with the earlier and much better known *Lectures on Dynamics* of 1842/43 makes his criticism of Lagrange seem quite astonishing. Indeed, Hamilton and Jacobi are always 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" he implied that he himself added some new stanzas to the same poem. More specifically, when Jacobi ([1884], 1) called Lagrange's textbook a successful attempt to "write down and transform" the differential equations of motion, he implied that his and Hamilton's contributions should be regarded as a necessary and sufficient complement, showing how to solve these equations. In this respect, Felix Klein was quite right when he said, "Jacobi's extension of mechanics is essential with respect to its analytical side," but it has to be criticised for its lack of physical relevance (Klein [1926], 203, 206–207).

Nevertheless, if we take Jacobi's last lectures on analytical mechanics into account, this view is no longer tenable. These lectures have just been published, and I hope that they will lead to a change in Jacobi's place in the history of mechanics. Jacobi's criticism of Lagrange is the most explicit expression of what can be described as a shift from a physico-mathematician's view of mechanics to a "pure" mathematician's view. This shift has serious implications for the later philosophical understanding of what mechanical principles and the theory of mechanics are.

Lagrange and the Tradition of "Mechanical Euclideanism"

Like his predecessors Euler and d'Alembert, Lagrange attempted to give mechanics as an axiomatic science, starting from (seemingly) evident, general, and certain principles, and developing it in a deductive manner with a minimum of further assumptions. This abstract theory was presented as an expression of the intrinsic mathematical structure of nature itself. I will use Lakatos's term "Euclideanism" for Lagrange's concept of science, thus making explicit that Euclidean geometry was the model for this kind of presentation of mechanics (Lakatos [1978], 28–29). On this view, mechanical knowledge of the world has the same status as any mathematical knowledge: it is infallible.

It is well known that, in his *Mécanique Analytique*, Lagrange eschewed geometry, though we know today that this applies more to his presentation and justification of mechanical propositions than to their invention or discovery. More importantly, it seems to me that, in restricting mechanics to the methods of analysis alone, Lagrange claimed not only to dispense with other

Column Editor's address:
Faculty of Mathematics, The Open University, Milton Keynes, MK7 6AA, England

mathematical methods, but also to dispense with extra-mathematical methods. Indeed, Lagrange's *Mécanique Analytique* is the first major textbook in the history of mechanics without any kind of explicit philosophical discussion. The metaphysical assumptions of his mechanics are not made explicit, nor is there any epistemological justification given for the presumed infallible character of the basic principles. This is in striking contrast to Lagrange's immediate predecessors Euler, Maupertuis, and d'Alembert (Pulte [1989], 232–240).

This kind of "mechanical Euclideanism" contains a significant tension. Lagrange himself was partly aware of it, and his successors in the French tradition of mathematical physics were even more so. Lagrange not only adhered to the old Euclidean ideal of building up mechanics from evident, certain, and general first principles, he actually wanted to start with one (and only one) principle, the principle of virtual velocities. In order to achieve this aim, he formulated this principle in a very general and abstract manner, using his calculus of variations. In the first edition of his *Mécanique Analytique*, he introduced this principle as "a kind of axiom" (Lagrange [1788], 12). But later he had to admit that this principle lacked one decisive traditional characteristic of an axiom: It is "not sufficiently evident to be established as a primordial principle" (Lagrange [1811], 23). His way out of this dilemma was to clarify his principle by referring to simple mechanical processes or machines. Later critics, from Fourier and de Prony to Poinsot and Ostrogradsky, supported him in this (Lindt [1904]). All these critics aimed at better proofs, giving the principle of virtual velocities a more secure foundation and making it more evident. They were not suspicious about his Euclideanism, they just tried to realise it *better*. As we shall see, Jacobi's deepest criticism was to doubt the validity of any such attempt.

Carl Gustav Jacob Jacobi (1804–1851) (from *Meyers Enzyklopädisches Lexikon*)

Jacobi's Changing Attitude toward Mathematical Physics

Jacobi was born in 1804 and started his university career around 1825. His early attitude toward mathematics derived from the neo-humanism then dominant in Germany, which made science and scientific education ends in themselves. Mathematics in particular should be regarded as an expression of pure intellectual creativity, needing no other justification, and the application of mathematics to the natural sciences could even be seen as a degradation of mathematics (Knobloch/Pieper/Pulte [1995]).

In his early career, Jacobi was quite absorbed by this ideal of pure mathematics. He was explicitly hostile to French mathematical physics as it was successfully practised by Fourier, Laplace, Poisson, and others. On being

Where Jacobi gave his last lectures on *Analytische Mechanik*: Königliche Friedrich Wilhelms-Universität, Berlin (about 1840)

criticised by Fourier, who could see no practical use in Abel's and Jacobi's theory of elliptic functions, Jacobi gave the famous reply: "A philosopher like him should have known that the unique aim of science is the honour of the human spirit" (Borchardt [1875], 276).

He kept to his ideal of pure mathematics in his practical mathematical investigations. Even when he started working on the theory of the differential equations of motion around 1835, stimulated by W.R. Hamilton's investigations, he was not interested at all in the possible physical implications of this theory. Mechanics at its best was for Jacobi analytical mechanics in the sense of Lagrange. There is not the slightest trace of criticism of the foundations of Lagrange's mechanics to be found in his work before 1845.

Later, in the last six or seven years of his life, Jacobi was more and more confronted with problems of mechanics, astronomy, and physics in general, which deal with the concrete behaviour of physical objects. While he adhered to his ideal of pure mathematics, he became more aware of the problem of how mathematics as a product of our mind can be applicable to natural reality. He gave up the naïve Platonism which he had propagated in his earlier career, and came to a more modern and modest point of view. His criticism of Lagrange's mechanics is the most distinct expression of this change, but it is totally ignored in the histories of mechanics—it is not just Felix Klein who sticks to this picture of Jacobi!

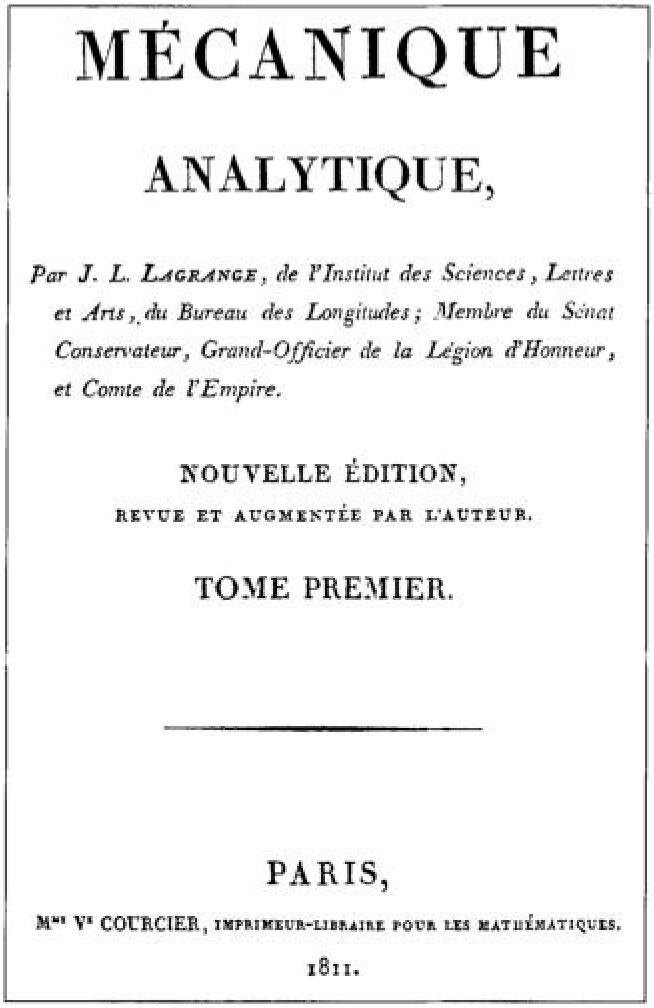
Jacobi's Criticism of Lagrange's Mechanics

Jacobi devoted about one quarter of his lectures to Lagrange's two so-called demonstrations of the principle of virtual velocities. In Lagrange's first attempt, which referred only to statics, he considered a system of connected masses. The single masses experience central forces P, Q, and so on. A small impact to the system leads to virtual displacements of the mass points (these displacements are called virtual, i.e., possible, because they must be compatible with existing connections). If the projection of the first displacement in the direction of the first force P is dp, and so on, then Lagrange's "axiom" of mechanics says that the system is in a state of equilibrium if the sum $Pdp + Qdq + \ldots$ vanishes:

$$Pdp + Qdq + \ldots = 0$$

In modern terminology, if the system is in a state of equilibrium, then the virtual work must be zero.

Evident truth can hardly be claimed for this proposition. In his first "demonstration" of its truth, Lagrange introduced a set of pulleys to represent the forces (Lagrange [1798]; [1811]; 23–26). This set is to be understood as a mere thought-instrument, with massless and frictionless pulleys, an inextensible cord, and a unit weight. The quantity $Pdp + Qdq + \ldots$ is then easily expressed geometrically as the total change of length of the cord. If this sum is zero, the weight obviously can't go up or down when a displacement is applied to the system.


MÉCANIQUE

ANALYTIQUE,

Par J. L. Lagrange, de l'Institut des Sciences, Lettres et Arts, du Bureau des Longitudes; Membre du Sénat Conservateur, Grand-Officier de la Légion d'Honneur, et Comte de l'Empire.

NOUVELLE ÉDITION,

REVUE ET AUGMENTÉE PAR L'AUTEUR.

TOME PREMIER.

PARIS,

Mme Ve COURCIER, IMPRIMEUR-LIBRAIRE POUR LES MATHÉMATIQUES.

1811.

Where Lagrange's first "demonstration" was reprinted: *Mécanique Analytique*, Vol. 1 (2nd ed., 1811)

Lagrange ([1811], 24) said:

> Now it is evident that as a necessary condition to maintain the system—being subjected to various pulling forces—in equilibrium, the weight cannot descend as a result of any infinitesimal displacement of the system's points—whatever the nature of this movement may be. As weight always has the tendency to descend, it will—if there is a displacement of the system inducing it to descend—actually and consequently do so and produce this displacement of the system.

In the state of equilibrium, Lagrange argued, an infinitesimal displacement of the system can not result in a descent of the weight, and a descent of the weight implies that there is no equilibrium. This idea, he claims, is "expressed analytically in the principle of virtual velocities."

In his Berlin lecture from 1847/48, Jacobi ([1996], 29) quoted Lagrange's consideration. When he came to the word "evident," he couldn't restrain himself from commenting:

> . . . this is a bad word; wherever you find it, you can be sure that there are serious difficulties; [using] it is an evil habit of mathematicians, so old that I found it recently in the

work of Diophantus, who applied it to a proposition which is very difficult to demonstrate even with modern analysis.

Where Lagrange asserts evidence and mathematical exactitude, Jacobi finds darkness and logical incorrectness. In outline, his criticism ran as follows. Lagrange's inference is based on two conclusions, or on a dichotomy of two cases:

If an arbitrary infinitesimal movement is applied to the mechanical system . . .

(A) . . . the weight does not descend—state of equilibium:

$$Pdp + Qdq + \dots = 0$$

(B) . . . the weight descends—no equilibrium:

$$Pdp + Qdq + \dots \neq 0.$$

Jacobi's criticism can be summed up in these points:

(1) Conclusion (A) is probably correct for stable equilibrium.

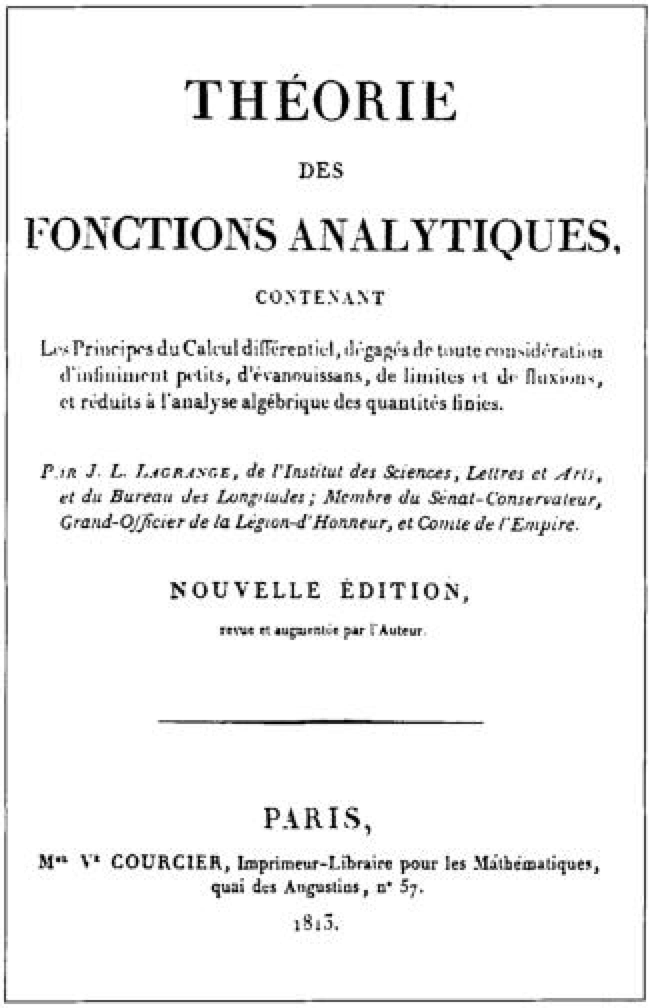
(2) Conclusion (B) is definitely wrong, because it doesn't take into account states of equilibrium which are not stable.

(3) The argument cannot be restricted to stable equilibrium, because this restriction is not maintained when the principle of virtual velocities is extended from statics to dynamics.

(4) (A) is merely based on experience and therefore not certain: ". . . you have to be aware that these probable considerations are not more than probable, and must not be taken as a [mathematical] demonstration" (Jacobi [1996], 32–33).

Lagrange's "construction", as Jacobi repeatedly calls it, therefore can never be accepted as a mathematical proof. His fourth point, in particular, makes clear that Lagrange mixed up mathematical reasoning with empirical knowledge, which cannot provide certainty and generality, but can only lead to probable truth for a restricted number of cases. This is the core of Jacobi's criticism, confronting mathematical physics with the strict standards he attributes to pure mathematics only.

Lagrange himself was not very happy with his first attempt for various reasons. Shortly before he died, he gave a new proof, in the second edition of his *Théorie des fonctions analytiques* (1813).


THÉORIE

DES

FONCTIONS ANALYTIQUES,

CONTENANT

Les Principes du Calcul différentiel, dégagés de toute considération d'infiniment petits, d'évanouissans, de limites et de fluxions, et réduits à l'analyse algébrique des quantités finies.

PAR J. L. LAGRANGE, de l'Institut des Sciences, Lettres et Arts, et du Bureau des Longitudes; Membre du Sénat-Conservateur, Grand-Officier de la Légion-d'Honneur, et Comte de l'Empire.

NOUVELLE ÉDITION,

revue et augmentée par l'Auteur.

PARIS,

Mme Ve COURCIER, Imprimeur-Libraire pour les Mathématiques, quai des Augustins, n° 57.

1813.

Where Lagrange gave his second "demonstration": *Théorie des Fonctions Analytiques* (2nd ed., 1813)

This time Lagrange ([1813], 379–385) used pulleys as a substitute for the inner connections or constraints between the masses. Lagrange sought to express the forces of constraint by an equation of constraint ($F = 0$). This was of the utmost importance for him, because there is a gap between his purely mathematical representation of rigid, geometrical constraints on the one hand and physical actions given by force functions (for example, by the law of gravity) on the other hand. These expressions are meant to bridge this gap. They should represent physical forces corresponding to certain geometrical constraints, and make them comparable with forces such as gravity.

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 was hitherto out of focus, becomes important. Jacobi attacked the very substitution of a constraint F by a 'pulley-function' f. This substitution, he said, is by no means evident. Jacobi ([1996], 59) wrote:

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.

Jacobi started with the following consideration: What happens if an instantaneous impulse of finite magnitude is exerted on the system at rest? Of course, the real movement of the mass points must be modified according to the constraints. To determine the Lagrange multipliers and subsequently the real impulses, one has to make use not only of the equations of constraint, but also of their first total derivatives in time. Jacobi showed in a lengthy algebraic development that this problem can be solved if the equations of constraint are independent. It is obvious that the real momentary impulses of the mass points depend on the first partial derivatives of the constraints with respect to the coordinates (Jacobi [1996], 59–64, 78–82).

Jacobi then discussed the initial state of the same system under a continuously acting force. Now, an analogous procedure has to be performed to determine the Lagrangian multipliers and to show that the real accelerations (forces) are compatible with the constraints, taking compatibility of the initial values of the velocities as given by the first step. Without going into the details of these calculations, it is clear that the second total derivative of the constraints with respect to time has to be used. Consequently, in this case the Lagrange multiplier will depend not only on the given forces, but also on the velocities of the particles and on the first and second partial derivatives of the equations of constraint (Jacobi [1996], 83–86). But in Lagrange's second demonstration, the substitution of these constraints by pulleys depends only on first-order approximation of the corresponding surfaces. There are no conditions imposed on the second derivatives of the pulley function f whatsoever. To quote Jacobi ([1996], 86) again:

> From this results an objection to the transition from statics to dynamics. The principle of statics doesn't deal with points in motion, and a particular inquiry, a particular principle has to be premised, how the velocities are constituted and modified . . .

According to Jacobi, Lagrange mixes up two kinds of mechanical conditions, which are in reality "quite heterogeneous," as he says: on the one hand, a mass can underlie certain physical forces (as gravity, for example); on the other hand, a mass point is fixed on idealised, rigid curves or surfaces. Conditions of the second kind, that is, forces of constraint, can be replaced by Lagrange's pulley in the case of rest, but not in the case of motion. Therefore, Jacobi ([1996], 87) asks for a new principle, "according to which both conditions of movement can be compared and determined in their mutual interactions." But such a principle certainly transcends Lagrange's very conception of analytical mechanics, as Jacobi ([1996], 193–194) sharply points out in a more general discussion of Lagrange's approach:

> 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 don't study the effects of the forces any longer. . . . Nature is totally ignored, and the constitution of bodies . . . 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.

Mechanical Principles and Mathematics in Jacobi's View

Why was it so important for Jacobi that he spent about 8 hours and more than 40 pages of his lectures on this demolition job? I believe that Jacobi systematically applied his analytical and algebraic tools in order to show that mathematical demonstrations of mechanical principles cannot be achieved. He does not say that all attempts of demonstration are in vain, or that one attempt of his forerunners is as bad as another (Jacobi [1996], 93–96). He accepts that such attempts can lead to new insights in the principles of mechanics. But Jacobi insists that Lagrange's conception of a mathematical mechanics stands and falls with the certainty of the principle of virtual velocities, and he wants it to fall. He wants to make clear beyond any doubt that Lagrange's "constructions" must not be regarded as mathematical demonstrations of the certainty of first principles, and that these principles are not to be taken as inevitable laws of nature. One might find this intention quite destructive, but Jacobi thought it unavoidable and positive.

This brings me to Jacobi's own views about mechanics, its principles, and the role of mathematics, which are quite different from Lagrange's. According to Jacobi, mechanics should not be regarded as a purely mathematical science, and its mathematically formulated principles should not be regarded as intrinsic laws of nature. Rather, mathematics offers a rich supply of possible principles, and neither empirical evidence nor mathematical or other reasoning can determine which of them is true. The search for proper mechanical principles always leaves room for a choice, which can be made according to considerations of simplicity and plausibility. It is thus Jacobi who calls these first principles of mechanics "conventions," exactly 50 years before Poincaré did. I quote Jacobi ([1996], 3):

> 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 outside its field, you will however find attempts to demonstrate these merely conventional propositions a priori, and it will be your task to find the false inference in each case.

Obviously, Jacobi here is still the pure mathematician, drawing a line between mathematics itself and "anything outside its field," as he says. Mathematical notions and propositions on the one hand and physical concepts and laws on the other hand are to be sharply separated. This is in striking contrast to Lagrange's physico-mathematician's point of view. According to Jacobi, we cannot expect generality, certainty, and evidence from statements about physical objects, but only from propositions of mathematics itself. It is to make this distinction unmistakably clear that he points out the shortcomings of Lagrange's so-called demonstrations.

Jacobi's criticism is not restricted to the principle of virtual velocities, nor to the principles of analytical mechanics in general. He also applies it to

Newton's principles. Let me quote some remarks about the law of inertia (Jacobi [1996], 3–4):

> From the point of view of pure mathematics it is a circular argument to say that rectilinear motion is the proper one, [and that] consequently all others require external action: because you could define as justly any other movement as the law of inertia of a body, if you only add that external action is responsible if it doesn't move accordingly. If we can physically demonstrate external action in any case where the body deviates, we are entitled to call the law of inertia, which is now at the basis [of our argument], a law of nature.

As is well known, Poincaré calls the principle of inertia a "disguised definition" to make explicit that it defines what a force-free movement should be. Again, we see a similar view in Jacobi, but half a century earlier. Can the basic laws of mechanics be understood as empirical generalisations or as synthetic principles *a priori*? Jacobi and Poincaré are not prepared to accept this classical dichotomy, their common answer is: neither nor! Both hold the opinion that experience or *a priori* reasoning cannot lead us to first principles but that these principles are fixed by convention. I think it is justified to say that in Jacobi's last lectures on analytical mechanics we can find at least sketches of what became known as conventionalism after the turn of the century (Pulte [1994]).

The important difference, however, is that Poincaré holds the opinion that we can always stick to the chosen conventions, that they always can be maintained as absolutely valid. Jacobi is not explicit on this point, but he obviously believes that empirical evidence is capable of falsifying principles. From time to time he remarks that they are not certain, but only "probably valid." Lagrange's Euclideanism is no longer a model of science for him. As far as I know, Jacobi is the first in the analytical tradition of mechanics who says farewell to Euclideanism and adopts some form of fallibilism.

Concluding Remarks

Just as we should take the frequently drawn parallel between rational mechanics and geometry seriously, we should pay attention not only to the changes in the foundations of geometry but also to those in mechanics. There is a line of mechanical non-euclideanism from Jacobi onwards, which later led to serious doubts about the validity of Newtonian mechanics. This tradition is quite independent of Ernst Mach's well-known criticism of absolute space, and precedes it. Nevertheless, it is widely neglected in the history of mathematics and physics.

Let me here just refer to Bernhard Riemann and Carl Neumann. Riemann was one of the students who attended Jacobi's lectures, and he picked up Jacobi's view of the principles of mechanics, before he came to geometry. (More precisely: Riemann's critical attitude towards axiomatic foundations starts with mechanics, and not with geometry.)

Carl Neumann studied Jacobi's *Analytical Mechanics* in great detail some months before he gave his famous inaugural lecture *On the principles of the Galilei-Newtonian theory*, which is remarkable in its logical analysis of the law of inertia and the concept of absolute space. This lecture marks the starting point of a broad and intensive discussion about the validity of Newtonian mechanics that lasted until Einstein.

Therefore, Neumann's words ([1870], 22) not only reflect Jacobi's point of view, they are an appropriate end to this paper:

> . . . it is also not absolutely impossible that the Galilei-Newtonian theory will some day be replaced by another theory, by another picture.

Acknowledgments

An extended and mathematically more detailed version of this paper will soon appear. It was worked out during my stay at the Department of History and Philosophy of Science, University of Cambridge, as a fellow of the Alexander von Humboldt Foundation. I am also grateful to Jeremy Gray for his comments and his help in preparing the final version of this article.

REFERENCES

Borchardt, W., Ed. [1875] Correspondance mathématique entre Legendre et Jacobi. *J. Reine Angew. Math.* 80, 205–279.

Jacobi, C.G.J. [1884] *Vorlesungen über Dynamik. Königsberg 1842/43.* Ed. A. Clebsch. 2nd ed., Berlin, *Gesammelte Werke, Supplementband.*

Jacobi, C.G.J. [1996] *Vorlesungen über analytische Mechanik. Berlin 1847/48.* Ed. H. Pulte. Braunschweig/Wiesbaden 1996.

AUTHOR

HELMUT PULTE
Ruhr-Universität Bochum
Fakultät für Philosophie, Padagogik
und Publizistik
D-44780 Bochum
Germany

Helmut Pulte, born in 1956 and educated at the Ruhr-Universität Bochum, is working on history of mathematics and physics and history of philosophy of science from the 17th to the late 19th century. Last year, he published C.G.J. Jacobi's lectures on *Analytical Mechanics* from 1847/48. He recently spent a year at the University of Cambridge, where he traced the relations of Hermann von Helmholtz and a number of British scientists. At present, he is preparing a book about the philosophical change in theoretical mechanics through the 19th century (from Kant to Hertz). He teaches history and philosophy of science at the University of Bochum. When time permits, he enjoys music, cinema, and playing football with his two children.

Klein, F. [1926] *Vorlesungen über die Entwicklung der Mathematik im 19. Jahrhundert*, Vol. I. Berlin.

Knobloch, E./Pieper, H./Pulte, H. [1995] ". . . das Wesen der reinen Mathematik verherrlichen". Reine Mathematik und mathematische Naturphilosophie bei C.G.J. Jacobi. *Math. Semesterber.* 42, 99–132.

Lagrange, J.L. [1788] *Méchanique Analytique*. Paris.

Lagrange, J.L. [1798] Sur le principe des vitesses virtuelles. *J. Ecole Pol.* (1)2, Cah. 5, 115–118. *Oeuvres* III, 317–321.

Lagrange,J.L. [1811] *Mécanique Analytique, Vol. 1.* Nouvelle edition. Paris. *Oeuvres XI.*

Lagrange, J.L. [1813] *Théorie des fonctions analytiques.* Nouvelle édition. Paris. *Oeuvres IX.*

Lakatos, I. [1978] *Mathematics, science and epistemology (Philosophical Papers, Vol. 2).* Ed. J. Worrall/G. Currie. Cambridge.

Lindt, R. [1904] Das Prinzip der virtuellen Verrückungen. *Abh. zur Gesch. Math. Wiss.* 18, 145–196.

Neumann, C. [1870] *Ueber die Principien der Galilei-Newton'schen Theorie.* Leipzig.

Pulte, H. [1989] *Das Prinzip der kleinsten Wirkung und die Kraftkonzeptionen der rationalen Mechanik.* Stuttgart.

Pulte, H. [1994] C.G.J. Jacobis Vermächtnis einer 'konventionalen' analytischen Mechanik. *Ann. Sci.* 51, 498–519.

Ruhr-Universität Bochum
Fakultät für Philosophie, Pädagogik und Publizistik
D-44780 Bochum
Germany