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

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[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 criti-

cised for its lack of physical relevance (Klein [1925], 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
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.

**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

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*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 naive 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 for this proposition. In his first “demonstration” of its truth, Lagrange introduced a set of pulleys to represent the forces (Lagrange [1788], [1811]; 23–26). This set is to be understood as a mere thought-instrument, with massless and frictionless pulleys, an inex- tensible 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.

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:

\[ Pdp + Qdq + \ldots = 0 \]

\[ \text{In modern terminology, if the system is in a state of equilibrium, then the virtual work must be zero.} \]

\[ \text{Evident truth can hardly be claimed} \]

\[ \ldots \text{this is a bad word; wherever you find it, you can be sure that there are serious difficulties; [using] it is an evil habit of mathematicians, so old that I found it recently in the} \]
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 equilibrium:

\[ Pdq + Qdp + \ldots = 0 \]

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

\[ Pdq + Qdp + \ldots \neq 0 \]

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).

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**THÉORIE DES FONCTIONS ANALYTIQUES,**

**CONTENANT**

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

PARIS,

M. V. COURCIG, Imprimeur-Libraire pour les Mathématiques,  quai des Augustins, n° 57. 1815.

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 imp-
pulses of the mass points depend on the
first partial derivatives of the con-
straints with respect to the coordinates
(Jacobi [1996], 59–64, 78–82).
Jacobi then discussed the initial
state of the same system under a con-
tinuously acting force. Now, an analo-
gous procedure has to be performed to
determine the Lagrangian multipliers
and to show that the real accelerations
(forces) are compatible with the con-
straints, taking compatibility of the ini-
tial 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 sec-
dond 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 parti-
cular inquiry, a particular prin-
ципе has to be premised, how the
velocities are constituted and modi-
ified....

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 undergo certain physi-
cal 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. There-
fore, Jacobi ([1996], 87) asks for a new
principle, “according to which both
conditions of movement can be com-
pared and determined in their mutual
interactions.” But such a principle cer-
tainly transcends Lagrange's very con-
ception of analytical mechanics, as
Jacobi ([1996], 193–194) sharply points
out in a more general discussion of
Lagrange's approach:

Everything is reduced to mathema-
tical 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 igno-
red, and the constitution of bod-
ies..... 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 mathema-
tical 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 demoli-
tion job? I believe that Jacobi system-
atically applied his analytical and alge-
braic tools in order to show that
mathematical demonstrations of me-
chanical principles cannot be achieved.
He does not say that all attempts of
demonstration are in vain, or that one
of his forerunners is as bad as
another (Jacobi [1996], 93–96). He ac-
cepts that such attempts can lead to
new insights in the principles of me-
chanics. But Jacobi insists that
Lagrange's conception of a mathe-
matical 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 reg-
arded as a purely mathematical sci-
ence, and its mathematically formu-
lated principles should not be regarded
as intrinsic laws of nature. Rather,
mathematics offers a rich supply of
possible principles, and neither empiri-
cal 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 sim-
plicity and plausibility. It is thus Jacobi
who calls these first principles of me-
chanics "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 con-
ventions, yet they are assumed to
correspond to nature.... Wherever
mathematics is mixed up with any-
thing outside its field, you will how-
ever find attempts to demonstrate
these merely conventional proposi-
tions 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 be-
tween mathematics itself and "anything
outside its field," as he says. Mathema-
tical 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 pro-
positions of mathematics itself. It is to
make this distinction unmistakably
clear that he points out the shortcom-
ings of Lagrange's so-called demon-
strations.

Jacobi's criticism is not restricted
to the principle of virtual velocities, nor
to the principles of analytical mechan-
ics 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: not exactly! 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 given 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 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.

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