Beyond the Edge of Certainty: Reflections on the Rise of Physical Conventionalism

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Abstract. Until today, conventionalism is mainly regarded from the point of view of geometry, both in historical as in philosophical perspective. This paper, which corresponds in a certain way with earlier studies of J. Giedymin, aims at a broader interpretation. The importance of the so-called 'physics of principles', rooted in the tradition of analytical mechanics, for Poincaré's conventionalism is emphasized. It is argued that important elements of physical conventionalism can already be found in this tradition that underwent a fundamental change in the course of the 19th century.

Résumé. Jusqu'à présent, le conventionnalisme a toujours été considéré à point de vue de la géométrie, que ce soit dans une perspective historique ou philosophique. Cet article, qui reprend dans une certaine mesure les travaux antérieurs de J. Giedymin, vise une interprétation plus large. L'importance de la 'physique des principes', qui s'inscrit dans une tradition de la mécanique analytique est mise en valeur par le conventionnalisme de Poincaré. J'y défends la thèse selon laquelle d'importants éléments du conventionnalisme physique peuvent déjà être trouvés dans cette tradition qui a subi un changement fondamental au cours du 19ème siècle.

Zusammenfassung. Der Konventionalismus wird auch heute noch in aller Regel — sowohl in philosophischer als auch historischer Perspektive — vom Standpunkt der Geometrie aus betrachtet. Dieser Beitrag, der in mancher Hinsicht an frühere Untersuchungen J. Giedymins anknüpft, zielt auf eine breitere Interpretation ab. Die Bedeutung der sogenannten 'Physik der Prinzipien', die ihre Wurzeln in der Tradition der analytischen Mechanik hat, für Poincaré's Konventionalismus wird unterstrichen. Dabei wird die These vertreten, daß

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wichtige Elemente eines physikalischen Konventionalismus bereits in dieser Tradition, die im Verlaufe des 19.Jahrhunderts einem grundlegenden Wandel unterlag, aufzuweisen sind.

1. Introduction

The assessment of conventionalism within the history of philosophy of science is still dominated by two dogmas: firstly, that Poincaré is its founder and most outstanding representative; secondly, that conventionalism is a mere philosophical by-product of the discovery of non-Euclidean geometries. Both dogmas are supported by the historical fact that Poincaré first developed his philosophical framework, especially his concept of 'convention', in the field of geometry (1887) and later 'applied' it to physics (1897); the second dogma seems to gain further evidence from the philosophical argument that geometrical conventionalism forms the 'basis' of physical conventionalism or, even stronger, that physical conventionalism is a necessary consequence of geometrical conventionalism and nothing more.

To my mind, however, both dogmas should be rejected on historical and philosophical grounds. Firstly, Poincaré is rightly considered as the most important representative of conventionalism, but the assumption of a single *founder* makes no sense: Conventionalism, as far as it is a reaction to traditional empiricism, rationalism and Kant's critical idealism, emerges from mathematical and scientific practice and has no unique 'starting point'. In order to support this thesis, I will discuss the main philosophical features of Poincaré's physical conventionalism and bring forward new historical evidence for elements of this type of conventionalism in earlier 19th century mathematical physics: C. G. J. Jacobi's views on the foundations of mechanics, unknown until his *Vorlesungen über analytische Mechanik* (1847/48) were published recently, show some striking similarities to Poincaré's position. Jacobi even makes use of the term 'convention' for the principles of mechanics half a century earlier than Poincaré. This example (among others) seems to me appropriate to make clear, secondly, that important features of physical conventionalism could and did indeed emerge independently from geometrical conventionalism.

I will start with a 'minimal description' of Poincaré's physical conventionalism (Part 2) and continue with an outline of 'Euclideanism' as historical predecessor and counterpart of conventionalism. This position was most prominently represented by Lagrange (Part 3). Then I will present Jacobi's attitude to the foundations of mathematical physics, which emerged from his rejection of Lagrange's approach (Part 4). Finally, I will compare his views, especially the meaning of 'convention' in his lectures, with Poincaré's physical conventionalism (Part 5) and draw some historiographical conclusions (Part 6).

2. A Minimal Description of Poincaré's Physical Conventionalism

Poincaré's conventionalism is, first and foremost, a *theoretical* one: Conventional elements are not already located in singular statements about phenomena, but in the 'higher level propositions' of a physical theory. There is a reality outside, which lends 'factual' content to physical theories, but there are different possibilities of building theories with the same content. Consequently, we cannot bind ourselves to a theory without making a decision. As physical theories are defined by their mathematical 'cores', this decision or choice is between different (sets of) *principles* of mathematical physics.

At this point *mechanics*, as the mathematically most advanced empirical science at the end of the 19th century, becomes important. What is the relation of mechanics and physics in general according to Poincaré's philosophy of science? Though Poincaré tentatively seperates mechanics and physics [1], he does nowhere draw a clear line of demarcation

1. Cf. Ch. X.4 of *La valeur de la science* [Poincaré 1906, p.182-184]. Here, Poincaré argues that principles are more important in geometry than in mechanics and more important in mechanics than in physics: Their 'deductive power', so to speak, decreases from geometry to mechanics to physics. This is a gradual, but no principal difference. In his introduction to *La science et l'hypothèse* [Poincaré 1914, XVI-XVII], he seems in need for a sharp demarcation when he explicates the differences of "hypothesis" in the different areas [ibid., XIV]. But after discussing geometry, he continues: "In mechanics, we are led to analogous conclusions and we see that the principles of this science, though founded on experiments, take part in the conventional character of geometrical postulates" [ibid., XVI]. We have both 'analogy' of mechanical and geometrical conventions and 'participation' of mechanical principles in the conventionality of geometry. 'Participation', however, can be understood by no means as a kind of 'logical inclusion'. The fixation of time, for example, is an element of mechanical (and thereby physical) conventionality, but does not affect geometrical considerations. "Time has to be defined in such a way that the equations of mechanics are made as simple as possible" [Poincaré 1906, 33]. This holds for other nongeometrical concepts of mechanics (like force and energy), too; cf. *(P5)* below.

We can add a similar (though, with respect to Poincaré's hierarchy of sciences, *inverse*) point to Poincaré's attempt to demarcate mechanics from physics: In the introduction of *La science et l'hypothèse* (see above) he continues: "Up to here [to mechanics] nominalism triumphs, but we now arrive at the physical sciences properly speaking. Here the scene changes: we meet with hypothesis of another kind, and we recognize their great fertility. No doubt at first sight our theories appear fragile, and the history of science shows us how ephemeral they are; but they do not entirely between both areas. On the contrary: his own programme of physics can be characterized as a traditional *mechanical* one, trying to base the whole of physics on a few mechanical principles. When he explains the so-called "physics of principles", most of the *physical* principles brought forward are rooted in classical *mechanics* [Poincaré 1906, 133]; they were extended and generalized in order to make mechanics the *unique* foundation of physics. Poincaré's talking about 'physics of principles' essentially means mechanical principles [2]. He also argues *for* mechanics as an experimental science and *against* mechanics as a deductively organized and merely mathematical science [3]. For him, the principles of mechanics are based on experiments as any other principles of physics [Poincaré 1914, 107, 138-139], though they are *not* determined by experiments and therefore in need of conventional fixations. As any 'complete' mechanical explanation of physical phenomena can be modified to a number of empirically equivalent alternatives, such an explanation needs additional conventional fixations [4]. From mechanical principles as conventions *and* conventional elements inherent in any scientific explanation arises a theoretical flexibility that makes *unifying* explanation possible [5]. This line

perish, and from each of them something remains. It is this something that it is necessary to try to discover, because it is this, and this alone, that is the true reality" [Poincaré 1914, XVI-XVII; translation from Friedman 1996, 335]. Though Friedman is right in his *description* that Poincaré considers genuinely physical disciplines like optics and electrodynamics "to be non-conventional" [ibid., 335], it has to be kept in mind that any theoretical *explanation* of physics that strives for unity *must* be conventional, because, according to Poincaré, it has to be based on mechanical concepts and is therefore in need of mechanical conventions.

2. Poincaré's dictum "... not mechanism is our true and only aim, but unity" [Poincaré 1914, 177] does *not* contradict this thesis for two reasons: Firstly, he sticks to mechanism as the *best available* means to arrive at unity, underlining the *possibility* of mechanical explanations for all phenomena [ibid., 177-178]. Secondly, according to Poincaré's 'argument of embedding', which demands new theories to preserve the structural elements of the old ones. No new unifying theory of physics is thinkable that does not grant the principles of classical mechanics a central place.

3. See his 'Duhemian' characterization of British mechanics as an experimental science and French mechanics as a deductive science at the beginning of Ch. VI of *Science et l'Hypothese* [Poincaré 1914, 91]. Poincaré supports the British point of view [ibid., 139-140].

4. Though on different grounds: "If a phenomenon permits a complete mechanical explanation, it permits an infinite number of different mechanical explanations, which accounts as well for all the details revealed by experience" [ibid., 222].

5. The proper answer to Poincaré's question, whether *nature is flexible enough* for unique mechanical explanation is, according to my interpretation, that *mechanical explanation is flexible enough* for nature. I think Psillos' analysis is (at least) misleading in respect to the relation of mechanism and unification [Psillos 1996, 183-184]. He argues that according to Poincaré "*unity* rather than *mechanism* is what science must aim for" [ibid., 184; cf. 188 and footnote 2, above]. This interpretation seems to me correct only in so far as Poincaré is prepared to renounce 'mechanism'

of argument against a philosophically relevant distinction of mechanics and physics in Poincaré's work could be prolonged. To put it in a nutshell: Clear and general criteria of demarcation between mechanics and physics (whether of ontological, epistemological or methodological kind) cannot be found in Poincaré's *œuvre*. Due to his 'mechanical conservatism' [6], *physically relevant* principles are primarily the principles of classical mechanics and his physical conventionalism deals first and foremost with *these* principles. For this reason a description of Poincaré's physical conventionalism can and should focus on his understanding of mechanical principles, including Newton's laws, the principle of conservation of energy and the principle of least action [7]. In *La valeur de la science,* Poincaré describes these "general principles of mechanics" as "the results of strongly generalised experiences; it seems that they owe an extraordinary high degree of certainty exactly to this generalisation" [Poincaré 1906, 133]. They are historical endpoints of the process of theory-building that always has to start with empirical facts [ibid., 134-135]. Their 'conventional' status is best explicated in *La science et l'hypothèse*, where Poincaré mainly discusses Newton's three laws of motion. A characterization of his physical conventionalism has chiefly to answer the question what it means to grant physical *principles* such a status. I think that the following five criteria, meant as a *minimal* description, do justice to Poincaré's ideas: [8]

(P1) *'Third way-epistemology'*: Principles as conventions are neither mere inductive generalisations nor are they synthetic a priori propositions imposed by reason. In epistemological respect, they form

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as the explanation of physical phenomena by concrete 'matter and motion–models'. 'Mechanism', however, as the theoretical attitude that all physical phenomena are determined by a few mathematical principles, which make use only of the 'mechanical' concepts of space, time, mass and *energy*, is at the core of his programme (cf. fn. 2).

6. [Gillies 1994] shows that Poincaré's 'conservatism' contrasts remarkably with his later scientific investigations, especially in *Sur la dynamique de l'électron* (1905).

7. I do not discuss the role of geometrical conventions in the formulation of mechanical principles, because this point does not affect my argument, nor is it necessary to deal with other types of conventions (like mere linguistic or metric ones). Central for Poincaré's physical conventionalism are the different mechanical principles, *they* are the "really interesting conventions" [Diederich 1974, S. 52].

8. See esp. [Poincaré 1914, 91-141] and [Poincaré 1906, 155-158 and 170-180]. An excellent analysis of Poincaré's understanding of mechanical principles as *définitions deguisées* (cf. *(P5)* above) can be found in [Diederich 1974, 50-61]. In this context, Diederich also elaborates on Poincaré's distinction of *faits brut* and *faits scientifique* that cannot be discussed here.

a third kind of propositions: their validity and certainty can neither be shown by experience nor by reason, but is a matter of convention.

(P2) *Pragmatic dimension*: As *conventions*, these principles are determined by a pragmatic decision. This decision or choice, however, is *not* arbitrary, but has to observe certain empirical and theoretical demands: it is guided by experience and considerations of *simplicity* and *convenience.*

(P3) *Empirical relevance*: As *principles*, these conventions are necessary for a unifying (i.e. mechanical) explanation of physical phenomena. Their deductive consequences should cover all the known physical facts and (possibly) predict new phenomena.

(P4) *Immunity*: Conventions are immune against empirical falsification. It is always possible to adhere to a convention which is in conflict with observation or experiment by changing other elements of the theory. The possibility of adherence in case of empirical anomalies is what distinguishes conventional principles and 'lower level' laws with empirical content.

(P5) *Semantic relevance*: Conventions, as *hidden definitions*, determine the basic concepts of the theory in question. Therefore, the choice between different possible conventions means a choice between different sets of 'basic' concepts for the description of phenomena.

It is important to note that Poincaré's physical conventionalism, as has been characterised above, does *not* depend on his geometrical conventionalism: *(P1) – (P5)* remain important if we dwell on a fixed theory of geometry (say Euclid's), and talk about different principles and theories on the 'basis' of this one theory (say Hertz's three pictures of mechanics; cf.[Poincaré 1897]). We therefore need not exceed *classical* mechanics in order to understand the main point of Poincaré's physical conventionalism: even if geometry is fixed (by convention), mathematics remains rich enough to allow different conceptual and formal representations of nature (and consequently needs further conventions). Physical conventionalism necessarily transcends geometrical conventionalism. This becomes clear if we have a closer look at criterion *(P5)*, which especially demands further explanation:

Different conventional principles define different theories with different sets of concepts. As any empirical law can be divided into a principle, which implicitly *defines* the concepts in use and is therefore 'isolated' from empirical verification or falsification, and in a remaining lawlike part, which can take its concepts for granted and is empirically testable [Poincaré 1906, 179-182], the principles of a theory fix its conceptual frame. As endpoints of the process of theory building they are, so to speak, 'petrifications' of those concepts that turned out to be most useful for understanding phenomena.

But what about empirically equivalent and conceptual different theories, i.e. theories with different principles but the same sets of deductive consequences, which can be confronted with observation and experiment? Poincaré is far from interpreting their basic concepts as *incommensurable* (in Thomas Kuhn's or Paul Feyerabend's sense, for example). He rather regards these theories as different linguistic expressions of the *same* content, though he does not claim that the meaning of a single concept can be conserved in a simple 'one to one translation'. What he does defend is that the 'essential' parts of reality are reflected by the *relations* of (crude) facts, relations which *must* be mirrored by the abstract mathematical structure of the different theories and which are recognizable, metaphorically speaking, by the 'isomorphisms' that map one theory to another [Poincaré 1914, 161-163]: A "deeper reality" is expressed in the structure of the most general principles of mathematical physics, and the very fact that the single phenomena of nature follow from these principles is "a truth that will remain the same in eternity" [ibid., 163]. This is the most important difference between Poincaré's conventionalism and mere *instrumentalism* and the best reason to use the label "structural realism" in order to characterize the epistemology underlying his philosophy of science [Zahar 1996]. To illustrate this by an analogy: structural realism holds the view that the essential features of outside reality may be hidden in a plurality of its perspective representations (conceptual schemes and corresponding laws), but become obvious if their relations are uncovered, thereby synthesizing an adequate picture of reality. Conventions can determine perspective representations, but, in the end, not the 'real' picture.

If Poincaré's articulation of physical conventionalism, especially his third-way epistemology *(P1)*, is regarded as an important contribution to philosophy of science, its *historical roots* are of considerable philosophical interest: their investigation can show *why* traditional empiricism as well as Kantian apriorism lost their evidence for many philosophers and scientists, especially *why* there was a growing conviction that the problem of the 'mathematical nature of nature' had to be tackled in a new way. Without going into the details of the historiography of conventionalism, it seems to me appropriate to distinguish three different

views concerning the rise of physical conventionalism.
According to the *first* view, Poincaré's physical conventionalism is a mere *appendix* to his geometrical conventionalism, both in historical and in philosophical respect [9]. The *second* view stresses the importance of philosophical changes for the scientific image of mathematical physics, especially the rise of positivism and phenomenalism [10]. Jerzy Giedymin has extended and, in a way, *deepened* both views, but he has also added a *third* one. Though it focusses on mathematical physics, as the second one (from which it cannot be sharply demarcated), it should be regarded as a separate one, because it deals more with philosophical implications of the *practice* of mathematical physics than with explicit philosophical reflections about it. This *third* view stresses the importance of the 'physics of principles', to use Poincaré's phrase.

The physics of principles was worked out mainly in the analytical tradition of mechanics, above all by Euler, Lagrange, Poisson, Hamilton and Jacobi. Armed with the calculus of variations, potential theory and the theory of differential equations, they created different types of mechanical theories which are based on different basic concepts, introduced by different mathematical principles as, for example, Maupertuis' principle of least action or Hamilton's extremal principle. Giedymin interprets them as prototypes of physical conventions in Poincaré's sense [Giedymin 1982, 42-89]. He argues that especially Hamilton's dynamics was of utmost importance both for Poincaré's own investigations into mathematical physics as for his philosophy of science, though he has to admit that it is not possible to demonstrate a direct influence [11].

Giedymin's reference to the analytical tradition is important and should be pursued — as I would like to do now, though I deviate from his approach in at least three different respects: Firstly, I will concen-

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9. According to this view, Poincaré was brought to geometrical conventionalism by the development of non-Euclidean geometries, and conventionalism in (physical) geometry is regarded as the only substantial part of empirically relevant conventionalism. This seems to be Adolf Grünbaum's view [Grünbaum 1973], though he does not discuss Poincaré's philosophy of science in general; cf. [Giedymin 1982, 9-10].

10. Susan Wright, for example, has underlined the importance not only of Maxwell's theory of mechanical model-building, but also of the philosophical influences of Mach, Kirchhoff and Hertz on Poincaré's thought [Wright 1975, 251-260].

11. Cf. [Giedymin 1982, 65, 72]. As far as Hamilton's philosophy is concerned, however, this lack of evidence is hardly astonishing, because Hamilton was influenced by Kant not only in his philosophy of arithmetic, but also in his philosophy of mechanics [Hankins 1980, 172-180], and his synthetic a priori view of mechanical principles is not compatible with Poincaré's physical conventionalism. If we nevertheless focus not on this philosophical background, but on Hamilton's mathematical physics itself, there are good reasons for interpreting it as a possible 'point of departure' for later physical conventionalism.

trate not on Hamilton, but on Hamilton's 'German twin' [12] Carl Gustav Jacobi. Secondly, I do not share the opinion that all inquiries of early physical conventionalism should end with Poincaré (or Duhem, perhaps). In other words: It is an unnecessary restriction to use Poincaré's *œuvre*, so to speak, as a 'point of intersection' of earlier developments which might be important in this context. A critical and genetic historiography has to assess these developments in their own rights, thereby avoiding 'teleological' constructions which end in Poincaré's philosophy of science. Thirdly, it seems to me necessary to take into account the changing role of *mathematics* in the analytical tradition of mechanics — an aspect of utmost importance for its image of science, but widely neglected in the history of science and philosophy of science. The latter aspect leads to Lagrange as the major representative of analytical mechanics *before* Hamilton and Jacobi.

3. Lagrange's *Méchanique Analitique* (1788) and its Basic Dilemma

Lagrange's *Mechanique Analitique* can be described as an outstanding example of *Euclideanism* in the sense of Lakatos [Pulte 1998, 155-158]. According to Lakatos, the ideal of Euclideanism "is a deductive system with an indubitable truth-injection at the top... — so that truth, flowing down from the top... inundates the whole system". Therefore the main aim of Euclideanism is "to search for self-evident axioms — Euclidean methodology is puritanical, antispeculative" [Lakatos 1978, 28]. Euclideanism first and foremost strives for axioms in the traditional sense, i.e. they are expected to be evident, certain, infallible and, of course, true.

Lagrange's analytical mechanics fulfills this definition very well. It is a great attempt to build up the whole of mechanics on one "fundamental principle", i.e. the principle of virtual velocities [Lagrange 1788, 10]. As he claimed that statics and dynamics can be based on this principle alone, and as he elaborated this *mono-nomism* more successfully than any of his predecessors, we can characterize the *Méchanique analitique* as the *most articulated* form of 'mechanical Euclideanism' in 18th-century's rational mechanics.

Euclideanism, as Lakatos and I use the term, is an epistemologically

12. Their personalities, attitudes of mind and scientific interests show striking similarities — a 'parallelism' that I can't examine in this paper. See the excellent biography [Hankins 1980] for Hamilton; [Koenigsberger 1904], though unsatisfactory in different aspects, for Jacobi; cf. also [Pulte 1996, XXIII] for the impact of Hamilton's early works on Jacobi's mechanics.

neutral label: it includes both rationalistic and empiristic foundations of the science in question. Whether its principles are revealed by 'the light of reason' (Descartes) or 'deduced from phenomena' (Newton) does not matter. Both justifications can be found in 18th century mechanics, and in many textbooks they are inseparably interwoven. Lagrange's immediate predecessors in the analytical tradition — Euler, d'Alembert and Maupertuis — were mechanical Euclideanists. They all believed in a mathematically structured reality and in the capability of the human mind to condense this reality into a deductively symbolical structure with only a few first axioms at the top.

But there is also an entirely *new* ["entiérement neuf"] element in the *Méchanique Analitique*: Lagrange's famous claim "to reduce the theory of this science, and the art of resolving the problems which are related to it, to general formulas", thereby making mechanics "a new branch of analysis" [Lagrange 1788, v-vi], should *not only* be understood as a rejection of all geometrical means: restricting mechanics exclusively to the methods of analysis implies dispensing not only with other *mathematical* methods, but also with *extra-mathematical* methods. The *Méchanique analitique* is the first major textbook in the history of mechanics that I know of which abandons explicit philosophical reflections and justifications. It says nothing about how space, time, mass, force 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 premisses of his mechanics made explicit, nor is there any epistemological justification given for the presumed certainty of the basic principles of mechanics. This is in striking contrast not only to Descartes', Leibniz', and Newton's foundations of mechanics in the 17th century, but also to 18th century rational mechanics [Pulte 1989]. 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 *Méchanique analitique* is not only a hallmark of (traditional) mathematical Euclideanism, but also a striking example of (new) mathematical *instrumentalism.*

The combination of new instrumentalism (with respect to philosophy of nature) and *old* Euclideanism (with respect to philosophy of science) bears a significant tension of which Lagrange was only partly aware: it suggests that the 'deductive chain' can be started by first principles without recourse to any kind of geometrical and physical intuiton 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. To be more concrete: 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. In the first edition of his *Méchanique analitique*, he introduced this "very simple and very general" principle in statics as "a kind of axiom" ("une espèce d'axiome de Méchanique") [Lagrange 1788, 12]. In the second edition, he adhered to the title axiom, but had to admit that his principle lacked one decisive characteristic of an *axiom* in the traditional meaning, because it was "not sufficiently *evident* to be established as a primordial principle" [Lagrange 1853/55 I, 23, 27].

Euclideanism demands evidence; instrumentalism tends to dissolve evidence. This is the basic *dilemma* of Lagrange's mechanics. 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 [Pulte 1998, 163-166, 169-172] — symptom of an obvious "crisis of principles" [Bailhache 1975, 7] caused by the *Méchanique analitique* and important for a number of attempts in the early nineteenth century to regain lost evidence and certainty of the foundations of mechanics. All these attempts aimed at *better* demonstrations, giving the principle of virtual velocities a *more secure* foundation and making it *more evident.* Lakatos, in a different context, aptly described such a position as "a sort of 'rubber-Euclideanism'" because it "stretches the boundaries of self-evidence" [Lakatos 1978, 7]. Euclideanism in general, including this 'degenerated' form, had to be called in question before new images of the science of mechanics, like conventionalism and phenomenalism, could arise.

4. Jacobi's *Vorlesungen über analytische Mechanik* and Its Way Out of Lagrange's Dilemma

It is not necessary to repeat, in this context, how important French mathematical physics was for the German scene in the early nineteenth century: Gauß, Bessel, Dirichlet, Franz E. Neumann and Jacobi are only the most prominent examples for the recovering of German mathematics and science by this intellectual 'blood transfusion'. Instead, I would like to point out that mathematics in Germany played a very different role in the context of science and philosophy, and Jacobi is perhaps the very best figure to illustrate this difference.

He was born in 1804 and started his university career around 1825. His early attitude to mathematics can be seen as a result of the then dominating neo-humanism. According to this 'Weltanschauung', science and scientific education have ends in themselves. Especially mathematics should be regarded as an expression of pure intellectual creativity and as a means to develop it further, needing no other justification whatsoever. Application of mathematics in the natural sciences or other areas was often less than tolerated, it was seen as a *degradation* of mathematics.

Young Jacobi was quite absorbed by this ideal of pure mathematics ("reine Mathematik"). Proper mathematics, as he understood it, had nothing to do with sensational experience. On the contrary, it was "a self movement of the human spirit" ("Eigenbewegung des menschlichen Geistes"), as he said in his Koenigsberg inaugural lecture from 1832 [Jacobi 1902, 112]. Jacobi was therefore explicitly hostile to contemporary French mathematical physics. Being himself criticized by Fourier, who couldn't see any practical use in Abel's and Jacobi's theory of elliptic functions, he gave the famous reply: "Le but unique de la science, c'est l'honneur de l'esprit humain" [Borchardt 1875, 276].

Jacobi's attitude to the role of mathematics in natural philosophy in this time can be described as basically *platonistic*. Mathematization of nature demands, as a necessary prerequisite, that "the concepts of our spirit are expressed in nature. If mathematics was not created by our spirit's own accord [and] in accordance with the laws [incorporated] in nature, those mathematical ideas implanted in nature could not have been perceived" [Jacobi 1902, 112-113]. To put it in terms of a paradox, according to young Jacobi, applied mathematics at its best is pure mathematics. Jacobi saw this *a priori conception* of mathematical physics realized at its best in Lagrange's analytical mechanics. There is *not* the slightest trace of criticism of Lagrange to be found in his work before 1845.

Later, in the last six or seven years of his life, Jacobi was more confronted with 'real-world'-problems of mechanics, astronomy and physics in general. He learned moreover that different formulations of mechanics (Lagrange's and Hamilton's were the most prominent) were possible and investigated their relations, *and* he became increasingly engaged in problems of the history of mechanics [Pulte 1994, 502-505]. These experiences revealed to him that quite different forms of mathematical representations of nature exist. While he adhered to his ideal of pure mathematics, he became more aware of the problem, *why* mathematics as a product of our mind should be applicable to natural reality. He gave up the quite naive platonism, which he had propagated in his earlier career, and came to a more modern and modest point of view.

Jacobi's 'new' criticism of Lagrange's mechanics is the most distinct expression of this change, and the most important one with regard to the foundations of mechanics. This criticism is totally ignored in the histories of mathematics and physics, where Jacobi is still to be found among the adherents of Lagrange's approach. Jacobi's new attitude to his old ideal is most lively expressed in this warning to his students:

> [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 *mecanique analytique* better than I did, but sometimes it is not a good sign, if you understand something [Jacobi 1996, 29].

These words aim mainly at Lagrange's 'Rubber Euclideanism', especially his attempts to 'demonstrate' his principle of virtual velocities. I cannot go into the mathematical and physical details of this discussion [Pulte 1998], but just want to sketch Jacobi's train of thought.

Lagrange's 'proofs' are based on certain physical concepts like stable and unstable equilibrium, the replacement of mechanical systems by pulleys and rods, the replacement of quasi-geometrical constraints by physical forces, and so on.

Now, Jacobi makes extensive use of his analytical and algebraic talents in order to show that all these presuppositions *exceed* Lagrange's conception of analytical mechanics, i.e. of a theory that is based on the calculus and thereby, according to Lagrange's approach, on algebra.

Take, for example, Lagrange's transition from statics to dynamics. Lagrange makes use of d'Alemberts principle in order to subsume statics *and* dynamics under his extended version of the principle of virtual velocities. Jacobi shows, however, that this transition is not supported by Lagrange's proofs of the principle of virtual velocities [Jacobi 1996, 59-93; cf. Pulte 1998, 172-178]. He summarizes:

> 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 1996, 59].

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 underly certain physical forces (as gravity, for example), on the other hand, a mass point is fixed on idealized, rigid curves or surfaces. Conditions of the second kind, that is, forces of constraint, can be replaced according to Lagrange's proofs in the case of rest, but not in the case of motion. Jacobi therefore asks for a new principle "according to which both disgruent conditions of movement can be compared and determined in their mutual interactions" [ibid., 87]. But such a principle would certainly transcend Lagrange's very conception of analytical mechanics, as Jacobi 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 [ibid., 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*', though he had criticized exactly this school fifteen years earlier. However remarkable this shift is, it only concerns low-level adoptions of mathematical techniques to certain physical demands. It does not affect the foundations of mechanics itself.

The *second* aspect, however, does, because it concerns the *status of first principles of mechanics.* For Lagrange, the principle of virtual velocities was vital to gain an axiomatic-deductive organization of mechanics, and his two proofs were meant to save this Euclideanistic ideal. In so far as *this* ideal lacks (as Jacobi says) 'any justification' and even 'abandons the idea of justification', it can rightly be described as "dogmatic" [Grabiner 1990, 4]. Jacobi, on the other hand, applies his analytical and algebraical tools critically in order to show that mathematical demonstrations of first principles *cannot* be achieved. At best they can make mechanical principles "intuitive" ("anschaulich") [Jacobi 1996, 93-96]. But in mechanics, intuitive knowledge is no inferential knowledge; it is *not* based on unquestionable axioms and strict logical and mathematical deduction. Jacobi, the representative of pure mathematics, dismisses 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 (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 or axioms of mechanics are as certain and evident as those of pure mathematics. Mathematical instrumentalism, practiced by Lagrange and propagated by the later tradition of analytical mechanics, inevitably leads to a dismissal of mechanical Euclideanism. To my knowledge, Jacobi was the first representative of the analytical tradition who saw this consequence. His philosophy of mechanics lies 'beyond the edge of certainty' [13].

5. 'Conventional' and 'Conventionalistic' Principles: A Short Comparison of Jacobi's and Poincaré's Views

Having described the origin and general features of Jacobi's *destructive* criticism of Lagrange's Euclidanism, his *constructive* view of mechanical principles and the role of mathematics should be dealt with. According to Jacobi, 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, whether analytical or Newtonian, are not certain, but only *probably* true. Certainty of such principles, a feature of mechanical Euclideanism, is replaced by fallibility. Moreover, the search for proper mechanical principles always leaves space for a *choice*. Jacobi, well educated in classical philology and very conscious of linguistic subtleties, consequently called first principles of mechanics "conventions", exactly 50 years before Poincaré did:

> 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

13. I borrowed this title from [Colodny 1965] — a book where, strange enough, 'certainty' is only a topic of minor importance; cf. also part 5 of this paper.

> invent how the relations of systems of points depend on each other [Jacobi 1996, 3, 5].

It is important to take note of Jacobi's 'point of view of pure mathematics': He draws a line between mathematics itself and 'anything, which is outside its field': Mathematical notions and propositions on the one hand and physical concepts and laws on the other hand must be sharply separated.

This marks a striking contrast to Lagrange's 'physico-mathematician's' point of view and explains Jacobi's 'conventional turn'. Firstly, his idealistic background prevents him from believing that mechanical principles are grounded in experience. Secondly, he *shares* Lagrange's opinion that no metaphysical demonstration of such principles is possible. Thirdly, he *rejects* Lagrange's view that mathematics itself can prove these principles as certain and evident. Mathematics, however, can offer different principles of describing physical reality in an economical way. The *creativity* and *autonomy* of mathematics are substantial in order to understand, why a decision between different mechanical principles is possible and necessary. It is here, in mathematics, where these principles *as conventions* have to be located.

To avoid misunderstandings it must be emphasized that Jacobi did *not* give an extensive discussion of his understanding of conventions, comparable with Poincaré's introduction to *La science et l'hypothése*, for example [14]. The fact, however, that he uses this notion repeatedly and that his view had an impact on other mathematicians like Riemann and Carl Neumann, whose philosophies of science contain 'conventional elements' [15], seems to justify a short comparison with Poincaré's view. For this purpose, I go back to the 'minimal description' of Poincaré's

14. As already mentioned, Poincaré did not coin the term 'conventionalism'. But in so far as he elaborated a philosophical framework that centres around the concept of convention, it is appropriate to attribute this label to him. Jacobi, however, did not work out his views in detail. That is why I call Poincaré's view of mechanical principles 'conventionalistic' and Jacobi's view 'conventional'; cf. also [Pulte 1994].

15. Bernhard Riemann attended Jacobi's *Analytische Mechanik* from 1847/48 and rejected mechanical principles as axioms (in the traditional meaning) soon afterwards (and *before* he discussed the 'Hypothesis which lie at the basis of geometry' in 1854). Carl Neumann studied the *Analytische Mechanik* in great detail. He shared Jacobi's criticism of mechanical principles and made it popular in his famous and influential inaugural lecture at Leipzig *On the Principles of the Galilei-Newtonian Theory* (1869). There is a line of mechanical 'non-Euclideanism' (cf. part 4) starting with Jacobi, which led to serious doubts about the validity of so-called Newtonian mechanics. This tradition is 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 science and philosophy. Cf. [Pulte 1996] for some further details.

physical conventionalism *(P1-P5)*. There are some striking similarities as well as differences to be noticed:

(J1) *'Third way epistemology'*: The most important 'common denominator' of Poincaré and Jacobi is the idea that mathematics opens a space for constructive developments that is not restricted or predicted by experience or reason in semantical respects. Therefore, both mathematicians come to a 'third way solution' with respect to mechanical principles: Its first laws are neither empirical laws, 'deduced from phenomena' (Newton), nor are they synthetic a priori principles, imposed by reason (Kant). While his idealistic background forbids Jacobi to take the 'Newtonian' alternative seriously, he explicitly states that it is not possible "to demonstrate these merely conventional propositions *a priori*" [Jacobi 1996, 5].

(J2) *Pragmatic dimension*: Jacobi, as well as Poincaré, holds the opinion that a choice between different alternatives has to be made that is not arbitrary, but restricted by considerations of simplicity and convenience: "... again a convention in form of a general principle will take place. One can demand that the form of these principles is as simple and plausible as possible" [ibid., 5].

(J3) *Empirical relevance*: Of course, mechanical conventions, as principles, need to be empirically relevant for Jacobi, too. They are assumed in order "to correspond to nature", in a way "that experiments show their correspondence" [ibid., 3].

(J4) *Immunity*: Jacobi is not explicit on the question of how conventions are to be handled in case of empirical anomalies. As he sometimes remarks, however, that mechanical principles are not certain, but only "probable" [ibid., 32-33], he obviously believes that experience is entitled to *falsify* principles. Where Poincaré argues that Hertz's dictum 'what is derived from experience can be annihilated by experience' does not take into account the conventional nature of principles (*making* these principles infallible), Jacobi anticipates Hertz's position. Mechanics cannot achieve the infallibility of pure mathematics and becomes, even on the level of first principles, a *fallible* science. This is the most important difference with Lagrange's Euclideanism, but also with Poincaré's conventionalism.

(J5) *Semantic relevance*: In Jacobi's mechanics we find at least one example that points in the direction of Poincaré's 'hidden definition-interpretation' of conventions, i.e. Newton's first law or the law of

inertia. Poincaré argues rightly that this principle is neither derivable from experiments nor an a priori proposition. He regards it obviously as a fixation of the meaning of 'force-free movement'. Other *définitions déguisées* are possible and permissible, for example motions with changing amount of velocity or circular motions. They would lead to other differential equations of motions and different laws for the forces which are exerted in case of a disturbance [Poincaré 1914, 93-99]. Jacobi's interpretation is very similar:

> 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 ["setzen"] as justly any other movement as law of inertia of a body, if you only add that external action is responsible if it does not 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 [Jacobi 1996, 3-4].

Jacobi's circular argument implies that the law of inertia is, indeed, a convenient definition: it determines the meaning of 'being free of external forces'. We are entitled to choose other movements, for example circular movement, if we can trace back any deviation from circular movement to an external action. Newton's first law is meaningful only if it is combined with different laws expressing these external actions — with the law of gravitation, for example. This is what Poincaré tries to make clear in *La science et l'hypothèse* and what is in perfect agreement with Jacobi's point of view.

I would like to finish my comparison with a note on 'structural realism', the central epistemological characteristic of Poincaré's position. Jacobi uses the term 'convention' several times in the context of the general laws of motion. He does not use it, however, in the context of conservation laws and invariance considerations of mechanics: Poisson's famous theorem about the generation of canonical invariants, for example, a theorem extended by Jacobi himself, is called "fundamental theorem of dynamics" [ibid., 132-141, 283-296] — by no means a sign of a 'conventional' interpretation of this theorem. His reduction of conservation laws to symmetry properties, later successfully pursued by Emmy Noether, does not operate on the level of 'conventional' mechanical principles, but expresses 'higher order-relations' of mechanical principles. Jacobi seems to interpret these relations, being "without analogy" and expressed in a theorem that "was set up thirty years ago, but nevertheless unknown" [ibid., 289], as basic mathematical insights into physical reality. Structural realism is a mark of his position, too. In other words: If the "theory of invariants as foundation of conventionalism" [Mette 1986] is an important feature of Poincaré's philosophy of science, we may find Jacobi at Poincaré's side. As Jacobi, however, gives no explicit comments on the epistemological status of these 'higher' principles, this conjecture is open to further investigations.

6. Conclusion

Modern understanding of mechanics as a genuinly *physical* science should not blind us to the fact that in the 18th and first half of the nineteenth century it was credited with the evidence and certainty of a 'pure' mathematical science, being *de facto* regarded as epistemologically equivalent to geometry by most scientists and many philosophers of science. Both sciences were generally regarded as axiomatic-deductively organized bodies of knowledge, infallible in their applications to physical phenomena. *Euclideanism* was the dominant image of rational mechanics as a science, and this image had to be called in question *before* new images (like phenomenalism and conventionalism) could take shape. Of course, there were dramatic changes in the foundations of geometry with considerable impact on the philosophy of science. But this should not lead to a one-eyed 'geocentrism': We have to realize *comparable* changes in mathematical physics, especially in rational mechanics, which are in no way 'reducible' to the former ones. Both, geometry *and* mathematical physics, deserve our attention, if we *really* want to understand scientific and philosophical changes so frequently and carelessly called 'revolutionary'.

In the course of nineteenth century, a 'shrinking-process' of mathematical evidence and certainty takes place, and geometry as well as mathematical physics are affected by this process. The concept of pure mathematics, isolating arithmetic as the remaining mathematical 'paradise' of evidence and certainty from the larger area of mathematical sciences, plays a crucial role in this process (Gauss, Jacobi and Poincaré are the best figures to illustrate this). Jacobi draws the inevitable consequence for mechanics as a mathematical *and* empirical science: He rejects evidence *and* certainty of its principles, thereby anticipating elements of physical conventionalism in the sense of Poincaré, as I tried to show. Poincaré's rescue for the mathematical 'waste lands' of geometry and mathematical physics is, in a way, a more conservative one: though evidence is lost, we can *make* principles "certainly true" *by convention* [Poincaré 1906, 180]. But this certainty 'by decision' is entirely different from certainty 'by evidence', as it was defended by traditional mechanical Euclideanism. So far, both Jacobi and Poincaré are 'certainly *beyond the edge of certainty*'.

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