The Significance of the Hypothetical in the Natural Sciences

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From Axioms to Conventions and Hypotheses: The Foundations of Mechanics and the Roots of Carl Neumann's "Principles of the Galilean-Newtonian Theory"

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Abstract:

This paper is devoted to the rise of hypothetical thinking in the tradition of 19th century rational mechanics in general, and to the roots of Carl Neumann's paper on the "Principles of the Galilean-Newtonian Theory" within this tradition in particular. While Neumann's analysis of the law of inertia and Newton's concept of space is well known and accepted as an important step towards a better understanding of both, this historical background – which sheds light on Neumann's systematic arguments in different respects – has been widely neglected. It is shown that the rise of "pure mathematics" plays an important role for the rise of hypothetical thinking concerning the foundations of mechanics in general, and that this new understanding of mathematics is of utmost importance for Neumann's hypothetical-deductive concept of science.

1. Introduction

In 1866, the philosopher and psychologist Wilhelm Wundt published *Die physikalischen Axiome und ihre Beziehung zum Kausalprinzip* (Wundt 1866). This book is one of the latest manifests of what may be called "classical mathematical philosophy of nature" (CMN): It expresses the view that natural philosophy can be established on the basis of certain unshakable mathematical "axioms" of mechanics which deal with the movement of "ponderable" masses underlying certain forces and constraints. About four decades later, a second, revised edition of this work appeared under the title *Die Prinzipien der Naturlehre. Ein Kapitel aus einer Philosophie der Naturwissenschaften* (Wundt 1910). Wundt seized this opportunity in order to reflect critically on his former position, and to indicate a dramatic change with respect to the understanding and use of the concept "axiom," both in mechanics and in (pure) mathematics, in the two decades from 1866 onward: "What had been accepted as an axiom in former times was now labelled as "hypothesis," thereby expressing that also alternative systems of premises – perhaps deviating essentially from the established system – can be chosen, as long as they serve the purpose of linking the phenomena which have to be described" (Wundt 1910, 2).

In fact, the two decades which – according to Wundt – undermined the traditional "axiomatic view" and paved the way for a new "hypothetic view" in mechanics, include the first public debate about Bernhard Riemann's *Ueber die Hypothesen, welche der Geometrie zu Grunde liegen* (Riemann 1854 [1892], first publ. 1867) and – sometimes without a clear demarcation from Riemann's approach – on the Non-Euclidean Geometries of Gauss, Lobatschewskij and the Bolyais. They also include the rise of electrodynamics and thermodynamics as fields of mathematical physics in their own rights, and the rise of a broad discussion on the epistemological status and tasks of natural science, highlighted *inter alia* by the "description versus explanation-discussion" provoked by G. R. Kirchhoff's *Mechanik* from 1876 (cf. Kirchhoff 1876 [1897]). Wundt's historical explanation of the decline of CMN is restricted to three aspects: *(1)* the foundational debate in geometry, *(2)* the rise of the concept of energy, which undermined the traditional basis of mechanics and *(3)* the rise of electrodynamics and its radical "descendant," the electron theory of matter (Wundt 1910, 3). Today, a well-informed historian of science will add *at least* one more reason: *(4)* The rise of a new strand of phenomenalism within philosophy and the sciences, which is – with respect to the destruction of mechanical "axioms" – most obvious in the work of E. Mach.

As far as the criticism of Newton's theory of absolute space and the law of inertia is concerned, Mach had to accept – and frankly did accept (Mach 1872 [1909], 47) – one mathematician as his precursor who was obviously neither an adherent of phenomenalism, nor fitted well into Wundt's historical analysis of the decline of CMN: Carl Neumann, a son of the mathematical physicist Franz Ernst Neumann. Neumann the elder founded with Carl G. J. Jacobi the Königsberg seminar for mathematics and physics, which can be seen as the "nucleus" of German theoretical physics in the second half of the 19$^{\text{th}}$ century (see Olesko 1991). In 1869, Carl Neumann gave his lecture *On the Principles of the Galilean-Newtonian Theory* (Neumann 1869b) that – in sharp conflict with Wundt's position from 1866 – expressed emphatically a modern, even "Popper-like" hypothetical-deductive understanding of mathematical natural philosophy in general and especially a modern concept of mechanics (MMN: "Modern Mathematical Philosophy of Nature"), thus opening a vivid discussion on its foundations which lasted until Einstein and, in a way, made Einstein's revolution possible.

These cursory remarks point to the two main objectives of this paper: First and in general, I am interested in the way *how* – and the reason *why* – hypothetical thinking at first penetrated the discussion on the principles of classical mechanics in the course of the early 19th century. Wundt's analysis, elaborated and extended later by the history of science and the history of philosophy of science, concentrates on a relatively late period. Even a superficial glimpse reveals the development in question: The basic laws of mechanics, may they be formulated in a synthetic, Newtonian style or in the later, analytical manner, are at the beginning of the 19th century labelled as "axioms" (for example by J. L. Lagrange or J. Herschel), as "necessary truths" or "indubitable principles" (for example by P. S. de Laplace or W. Whewell) or – by a transcendental transformation of these properties – as "synthetic principles a priori" (by I. Kant, W. R. Hamilton and others). The set of basic laws used for the organization of theoretical mechanics was understood as a *unique* one, and each principle was dignified not only as general, but also as *certain* and *evident*, though the epistemological justifications of these features differed considerably among philosophers and scientists. In the second half of the nineteenth century, however, we meet with quite different notions for the *same* laws: After the "turning point" noticed by Wundt, they are labelled as "conventions" (by H. Poincaré, for example, though *not* for the first time), as mere "hypotheses" (by B. Riemann, C. Neumann, L. Boltzmann and others) or as provisional "descriptions" (see G. R. Kirchhoff or E. Mach, for example). This change of "second-order labels" is easily visible, but indicates a profound change of the understanding of rational mechanics as a both *mathematical* and *empirical* science that is less visible and the reasons of which are not completely understood until now. In short, this development can be described as removal of a traditionally mechanical Euclideanism – I am using this "Lakatosian" term deliberately as it is "epistemologically neutral"[1] – by a modern, "hypothetico-deductive" understanding of science

1 Euclideanism according to Lakatos expresses the view that the "ideal theory is a deductive system with an indubitable truth-injection at the top (a finite conjunction of axioms) – so that truth, flowing down from the top through the safe truth-preserving channels of valid inferences, inundates the whole system"; its basic aim "is to search for self-evident axioms – Euclidian methodology is puritanical, antispeculative" (Lakatos 1978, II, 28 and 29). Euclideanism in

for which the fallibility and revisability even of its *first* principles is decisive. Diachronic analysis of the writings of many philosopher-scientists show that the image of science in general, and mechanics in particular, underwent such a change during the last decades of the 19^{th} century – Hermann von Helmholtz is perhaps the most impressive and best investigated case study in this respect (see Schiemann 1997). The process in question is not a discontinuous, but a *gradual* one – it is a meta-theoretical *evolution* that, to a certain extent, first paved the way for the later scientific *revolution*. As I have described and examined this evolution with respect to mechanics in some detail elsewhere (Pulte 2005, chs. VI, VII), a short structural analysis of the early history in the second part of this paper will suffice here. This part is restricted to the *early* dissolution of mechanical Euclideanism and focuses on the role of a new understanding of mathematics in order to show that there were reasons within the traditional mechanics of "ponderable" masses independent of and prior to the empirical challenges (i. e. the integration of "new" areas of phenomena like those of electrodynamics and thermodynamics), independent of the rise of the debate about Non-Euclidean geometries and also independent of the rise of modern phenomenalism (or empirocriticism). In other words, the second part of this outline will end *before* these aspects became predominant in the discussion on the principles of mechanics. C. G. J. Jacobi will be the central figure of this part.

The second and more specific objective will be dealt with in the third part of this paper: The literature on Carl Neumann and on his lecture on the Galilean-Newtonian theory takes the new "MMN-position" presented there as something coming "out of the blue." I will try to show, however, that Neumann's turn can only be understood in the context of the earlier development or, to be more concrete, that it is strongly influenced by C. G. J. Jacobi and his new attitude towards mathematics and the mathematically formulated mechanical principles. This result seems to me of some importance for the history of philosophy of science, because it shows that the neo-humanist under-

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this sense is *epistemologically neutral* in so far as it is applicable both to traditional rationalism and empiricism: whether the axioms at the top are revealed by the 'light of reason' (see Descartes, for example) or 'deduced from phenomena' (Newton) is not relevant for the above definition of Euclideanism. Moreover, the dichotomy of traditional rationalism and empiricism conceals the common characteristic of infallibility.

standing of mathematics had a direct impact on the foundational discussion on space and the law of inertia in the later 19th century.

2. The Rise of Hypothetical Thinking on the Foundations of Mechanics in the First Half of the 19th Century

2.1 The Scientific and Metatheoretical Background

I. Newton in his *Principia* names his basic laws *axiomata sive leges motus*, thus formulating two different demands for them: they have to describe motion, and they have to organise the science of motion deductively. The latter demand becomes more and more important in the course of the 18th century, as different basic concepts and laws had to be integrated into rational mechanics. The long and complicated development in question is accompanied by a decline of empirical *and* metaphysical justifications of concepts and the laws combining them. This is true especially of the analytical tradition of mechanics, which becomes dominant from the middle of the 18th century onwards. Without doubt, a full understanding of this strand of mechanics can only be reached if the striving of the underlying mechanical Euclideanism for an axiomatic-deductive organization of the whole body of mechanical knowledge is taken into account. It has to be noticed, however, that in the course of this process an important meta-theoretical change takes place: The "first principles" of mechanics become *formal axioms* of science rather than material laws of nature. The principle of virtual velocities, later formulations of the principle of least action or Hamilton's principle clearly show the consequences of this change: The rise of these principles is accompanied by an increase of the deductive demands and, at the same time, a "semantic unloading" of their basic mathematical concepts like moment, action, *vis viva*, potential, or kinetic energy (cf. Pulte 2001, 62, 74–77).

Lagrange's analytical mechanics is most significant in this respect: On the one hand, it continues the efforts of Euler, d'Alembert and others to reach a deductive organization of mechanics, and brings these efforts to an end. On the other hand, however, it marks a *break* with the older tradition, thereby revealing the basic philosophical problems of mechanical Euclideanism: Lagrange wanted to base mechanics on certain and evident *mathematical* principles without any recourse to metaphysical or empirical justification: "Mechanics can be understood as a geometry with four dimensions," and the "analysis of mechanics as an extension of geometrical analysis" (Lagrange 1797 [1813], 337). This kind of mechanics is a logical *consequence* and, at the same time, a *dissolution* of Euclideanism in its older meaning: Axioms become formal principles of organization rather than principles with empirical content, and the whole system is held together by logical coherence rather than by "material" truth. In Lagrange's concept of mechanics, the higher calculus serves as the uniting element in the deductive chains. Insofar as order and unity become the main targets, and the calculus the main means, this mechanics is rightly called *analytical*, thus expressing both the ambitious methodological and the specific mathematical character of this science.

Lagrange shaped the image of analytical mechanics as a model science for more than half a century. His understanding of rational mechanics as a "self-sufficient" and formal mathematical science, however, inevitably leads to a smouldering conflict with the traditional meaning of axiom as a self-evident first proposition, which is neither provable nor in need of a proof. Lagrange wanted to start his mechanics with *one* principle, i. e. the principle of virtual velocities. In the first edition of his *Méchanique Analitique*, he introduced this general principle *verbatim* as a "kind of axiom" (Lagrange 1788, 12). In the second edition, however, he stuck to this title, but had to admit that his principle lacked one decisive characteristic of an *axiom* in the traditional meaning: It is "not sufficiently *evident* to be established as a primordial principle" (Lagrange 1853/55, vol. I, 23, 27). By two different so-called "demonstrations" he tried to *prove* his primordial principle by referring to simple mechanical processes or machines, thus trying to bring back *intuitive* truth to his "axiom."

Lagrange's formulation and his later demonstrations of the principle of virtual velocities posed a challenge for a number of mathematicians, such as Fourier, Laplace, de Prony, Gauss, Carnot, Poisson, Poinsot and Ampère. Their efforts to solve Lagrange's foundational problem show that the *Méchanique Analitique* indeed brought about a "crisis of principles" (Bailhache 1975, 7). All attempts to solve this crisis aimed at *better* demonstrations, giving the principle of virtual velocities a *more secure* foundation and making it *more evident* (cf. Pulte 1998, 158–161). Like Lagrange, the contemporary and following mathematicians applied their refined logical and mathematical methods in order to substantiate the principle of virtual velocities by geometrical and mechanical arguments. Their meta-theoretical position can aptly be described as "a sort of 'Rubber-Euclideanism'," because it "stretches the boundaries of self-evidence."[2] Despite this crisis, of which only the *avant-garde* of the contemporary scientific community was aware, analytical mechanics in the tradition of Lagrange was seen as a model science in influential philosophies of science, for example in A. Comte's *Cours de philosophie positive* (cf. Fraser 1990). Neither the "positivist" Comte nor empiricists like J. Herschel or J. S. Mill were critical about mechanical principles *qua* axioms, nor were semi-Kantians like W. Whewell or W. R. Hamilton. For different philosophical reasons – mainly for their empiricism or apriorism (in the Kantian, synthetic sense) with respect to mathematics – they kept to the traditional CMM-ideal (cf. Pulte 2005, Ch. VI.1).

2.2 C. G. J. Jacobi's Conventional Mechanics

In German speaking countries the image and understanding of mathematics in the early 19th century was strongly influenced by neo-humanism. This movement, then dominant in Germany, strongly emphasised that science and education (*Bildung*) are ends in themselves. Mathematics and the old languages in particular should be regarded as an expression of pure intellectual activity (see Jahnke 1990). Empiricist conceptions of mathematics like those of the French mathematical physics or British empiricism were sharply rejected, both with respect to their philosophical foundations and to their utilitarian consequences. Mathematics was understood as a "pure" and autonomous mental activity, governed only by the rules of logic and destined for the "honour of the human spirit" (cf. Knobloch et. al. 1995, 100–109, esp. 108). Mathematical truth therefore had to be independent from any external experience and also from mediating intuition in the sense of Kant. The neo-humanist ideal of pure mathematics brought about the problem of the *applicability* of mathematics to the empirical sciences in a new and fundamental form insofar as established answers to this problem (traditional metaphysical justifications, empiricist theories of abstraction, Kantian

2 Lakatos 1978, II, 7 and 9. Lakatos himself subsumes Lagrange and other mathematicians of the 18th century under this label. However, he also admits that the history of the decline of Euclideanism (including its degeneration into 'Rubber Euclideanism') in mechanics has still to be written. Pulte 2005 attempts to fulfil this *desideratum*.

synthetic a priori-approaches) lost their plausibility. In this context, the foundational problem of analytical mechanics described above was only *one* aspect – though one of eminent philosophical relevance – and the mathematician C. G. J. Jacobi was the first adherent of this new conception of pure mathematics who addressed this problem.

I will pass in silence over young Jacobi's Platonistic answer to the problem of applicability (cf. Knobloch et. al. 1995, 110–114), and turn immediately to his last *Vorlesungen über Analytische Mechanik* from 1847/48 (Jacobi 1847/48 [1996]; cf. Pulte 1994), the philosophically most interesting part of which was praised by Carl Neumann for the rigour of its criticism of the foundations of mechanics about two decades later (cf. part 3.1). Jacobi's rejection of Lagrange's mechanics is the first and most distinct expression of his criticism. As Jacobi's last lectures from 1847/48 were not published until 1996, his criticism was noticed only by some of his students (like B. Riemann) and other mathematicians (like C. Neumann). It was totally ignored in the histories of mathematics and physics, where Jacobi's contribution to mechanics – under the influence of his published *Vorlesungen über Dynamik* (from 1842/43, publ. 1866; see Jacobi 1884) – is unanimously subsumed into Lagrange's approach. During his time in Berlin, however, Jacobi came to a different estimation; his new attitude towards his old Lagrangian ideal is most lively expressed in a warning to his students at the beginning of his lectures directed against Lagrange's "Rubber Euclideanism," especially his attempts to give a demonstration of his "axiom" of virtual velocities.[3] I will omit the mathematical and physical details of Jacobi's criticism, but should point out the principle difference concerning their understanding of mathematics. He describes Lagrange's approach as follows (Jacobi 1847/48 [1996], 193–194):

> 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

3 "[Lagrange's] Analytical Mechanics is actually a book you have to be rather cautious about, as some of its content is of a more supernatural character than based on strict demonstration. You therefore have to be prudent about it, if you don't want to be deceived or come to the delusive belief that something is proved, which is [actually] not. There are only a few points, which do not imply major difficulties; I had students, who understood the *mécanique analytique* better than I did, but sometimes it is not a good sign, if you understand something" (Jacobi 1847/48 [1996], 26).

> 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.

Jacobi's reproach has two different aspects. First, he rejects Lagrange's purely analytical mechanics for its inability to describe the behaviour of real physical bodies. In this respect, he shares the view of those French mathematicians in the tradition of Laplace, who called for a "mécanique *physique*" instead of a "mécanique *analytique*." This point does not affect the foundations of mechanics itself. The *second* aspect, however, does affect such foundations, 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 Euclidean ideal. In so far as *this* ideal lacks "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 algebraic 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 1847/48 [1996], 93–96). But intuitive knowledge is no inferential knowledge in his sense; it is *not* based on unquestionable mathematical axioms and strict logical deduction. At this point Jacobi – the exponent of pure mathematics – dismisses Euclideanism as an ideal of *science:* 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. Indeed, as far as I am aware, Jacobi was the first representative of the analytical tradition who saw and drew this consequence.

Having described the origin and general features of Jacobi's *destructive* criticism of Lagrange's Euclideanism, I should shortly outline his *constructive* view of mechanical principles and the role of mathematics for them. 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, cannot be achieved. Moreover, the search for proper mechanical principles always leaves space for a choice between different alternatives. Jacobi, well educated in classical philology and very conscious of linguistic subtleties, consequently called first principles of mechanics "conventions," exactly 50 years before H. Poincaré did (Jacobi 1847/48 [1996], 3, 5):

> From the point of view of pure mathematics, these laws cannot be demonstrated; [they are] mere *conventions*, yet they are assumed to correspond to nature … Wherever mathematics is mixed up with anything, which is outside its field, you will however find attempts to demonstrate these merely conventional propositions a priori, and it will be your task to find out the false deduction in each case. …
>
> There are, properly speaking, no demonstrations of these propositions, they can only be made plausible; all existing demonstrations always presume more or less because mathematics cannot invent how the relations of systems of points depend on each other.

It is important here 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 is essential for Jacobi's "conventional turn." This is firstly because his idealistic background prevents him from believing that mechanical principles are grounded in experience. Secondly, he *shares* Lagrange's opinion that no metaphysical justification of such principles is possible. And finally, 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. It is in *mathematics* that the conventional character of these principles has to be located, because mathematics offers more possibilities than nature can realize.

For Jacobi, conventions are neither gained by experience (i. e., they are no inductive generalizations) nor are they a priori-principles (in Kant's sense). Comparable to Poincaré (cf. Pulte 2000), he comes to a "third-way-solution," which makes a choice between different alternatives possible and necessary. Jacobi, too, holds the opinion that this choice 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" (Jacobi 1847/48 [1996], 5). Of course, mechanical conventions, as principles, need to be empirically relevant.

They are assumed in order "to correspond to nature" in a way "that experiments show their correspondence" (Jacobi 1847/48 [1996], 3). Jacobi, however, is not explicit on the question of how conventions are to be handled in case of empirical anomalies. But as he sometimes remarks, that mechanical principles are not certain, but only "probable" (Jacobi 1847/48 [1996], 32–33), he obviously believes that experience is entitled to *falsify* principles. Poincaré, on the other hand, exempts mechanical conventions from empirical falsification.

It is important to note that Jacobi applies his concept not only to analytical principles, where it might be used in the trivial sense that different conventions can be used by merely formal, empirically meaningless operations, but also to Newton's synthetical "axioms," especially to the *law of inertia*. They, too, are first and above all *mathematical* propositions. Here, Jacobi comes close to the semantic aspect of conventions in Poincaré's "hidden definition-interpretation." As is well known, Poincaré regards the law of inertia as a fixation of the meaning of "force-free movement." Other *définitions déguisées* are possible and permissible, for example motions with changing velocity or circular motions. Jacobi's interpretation seems similar (Jacobi 1847/48 [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 [*setzen*] as justly any other movement as 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.

The circular argument presented here suggests that the law of inertia implies 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 external actions.

To sum up, one can say that Jacobi's "conventional" mechanics marks a sharp break with the older tradition of mechanical Euclideanism, and that his neo-humanist concept of pure mathematics is fundamental for this break. While possible mechanical principles are free inventions of mathematics, a methodologically reflected *decision* is necessary in order to come to empirical laws, which can, however, never gain the certainty of the propositions of pure mathematics. While the older tradition of mathematical physics keeps to an axiomatic-deductive ideal of mechanics (CMN), and strives for making its first principles safe and evident in its scientific practice, Jacobi rejects this ideal as futile and demands the acceptance of the revisability and fallibility of mechanical principles in scientific practice. In this sense, his conventional understanding of mechanics – which is not yet "conventionalistic" in the sense of Poincaré's doctrine elaborated half a century later – is an important early contribution to a modern, hypothetical understanding of mechanics (MMN).

2.3 A Note on the Reception of Jacobi's Lectures

One of the participants at Jacobi's *Vorlesungen über Analytische Mechanik*, delivered in Berlin in 1847/48, was Wilhelm Scheibner, whose notes served as the basis for the later publication (Jacobi 1847/48 [1996]). Scheibner went to Leipzig, where in 1853 he qualified as a university lecturer. The thesis he defended in his *disputatio* was: "The principles leading to the basic equations of mechanics are of a conventional nature, especially the principle of virtual velocities, and the principle named after d'Alembert cannot be demonstrated completely."[4] Other participants likewise passed Jacobi's ideas to colleagues and students (cf. Jacobi 1847/48 [1996], XLIX–LI).

The most eminent mathematician who attended the Berlin *Vorlesungen* was B. Riemann. After his return to Göttingen, he was busy working on the principles of natural philosophy and their epistemological and methodological implications. In this time he wrote the fragment *Neue mathematische Principien der Naturphilosophie* (Riemann 1853 [1892]). The title obviously alludes to Newton's *Principia*. The relation of natural philosophy and physical geometry cannot be discussed here (cf. Pulte 2005, 388–399). It should be noted, however, that the "New Principles of Natural Philosophy" *precede* his famous lecture *Ueber die Hypothesen, welche der Geometrie zu Grunde liegen* (Riemann 1854 [1892]). In his earlier fragment, Riemann picks up Jacobi's rejection of mechanical "axioms" and integrates this point of view into his own more empiricist framework, which likewise has no place for empirical laws which are distinguished by mathematical *certainty*. He does not, however, adopt Ja-

4 See the document 'Diss. Phil. Lip. 1840–1872' in the archives of Universität Halle ('Personalakte Wilhelm Scheibner'); cf. also Jacobi 1847/48 [1996], XLIX–L.

cobi's label "convention," but uses the more traditional notion "hypotheses" to articulate his position (Riemann 1853 [1892], 525):

> Newton's distinction between laws of motion or axioms and hypotheses seems to me untenable. The law of inertia is the hypothesis: If a material point were alone in the world and moved in space with a definite velocity, it would preserve this velocity constantly.

A hypothesis, according to Riemann, is anything which is "added to experience by our thinking" (Riemann 1892, 525). He refuses to accept a mathematical statement as an *axiom* of mechanics, if, as he thinks, it exceeds our experience and, in a certain sense, even contradicts it.[5]

It is important to note that his understanding of mathematics as the driving force of "hypotheticity" both in his mechanics and in physical geometry: Scientific experience needs mathematical conceptualisation, but, vice versa, mathematical concepts can only potentially be applied in present natural philosophy. Hence it follows a genuine *methodological* demarcation between both areas. Physics has to decide by measurement what concepts from the rich "supply" offered by mathematics are suitable for the representation of phenomena. As the mathematical principles can only be deductively checked by empirical facts, it is even possible to have different sets of principles, which are corroborated by the same facts (Riemann 1854 [1892], 273).

Without doubt, Riemann's fragment on "New Mathematical Principles of Natural Philosophy" presents mechanics as a hypothetical-de-

5 *Later* he makes a similar point concerning Euclidean axioms as a basis of physical geometry, when he says, that "we neither perceive whether and how far their connection is necessary, nor a priori, whether it is possible" (Riemann 1854 [1892], 273). When he doubts their *necessity*, he obviously has in mind *other* systems of physical geometry. When he doubts their very *possibility*, he does not only raise the question of logical consistency, but also the question of whether Euclid's axioms are true for physical space or not. A physical realisation of rectilinearity, according to Newton's first law, is a part of this problem, and it seems clear that this part was first understood as *problematic*.

It is a widespread misconception that geometry was understood as an 'independent' basis of mechanics and that, for this reason, hypotheticity of the principles of mechanics was a natural consequence of the hypotheticity of the principles of physical geometry. In Newton's *Principia*, we find an inverse 'foundational relation' between mechanics and geometry, and philosopher-scientists like von Helmholtz adopted this view: Geometry, as an empirical science, depends on mechanics (see Schiemann 1997, 219–234). This aspect is important to overcome the 'standard view' that the rise of MMN is a mere 'epiphenomenon' of the rise of Non-Euclidean geometries.

ductive science in its modern sense. It is as alien to Mechanical Euclideanism as Jacobi's "conventional mechanics." Hence, Riemann's approach is comparable to, but not identical with Jacobi's understanding of mathematics (cf. Pulte 2005, 375–388): It is basically the *autonomy* of mathematics from empirical constraints, which brings about the *hypothetical* in mathematical physics in his framework too. For the scientific community, however, this aspect of his work remained widely unknown, because his fragments on natural philosophy were not published before 1876.

C. Neumann, to whom I will now turn, learned from Jacobi's *Vorlesungen* through the notes taken by W. Scheibner. In 1869, when he gave his lecture *Ueber die Principien der Galilei-Newton'schen Theorie* (Neumann 1869b), he too did not know Riemann's fragments on natural philosophy, nor was he aware of Riemann's *Habilitationsvortrag* on geometry.[6] This is important in order to understand the actual roots and outlook of Neumann's inaugural lecture.

3. The Roots of Carl Neumann's Principles of the Galilean-Newtonian Theory

3.1 The Background until 1869

In order to understand the origin and scope of Carl Neumann's Leipzig "Principles," another inaugural lecture, given four years earlier in Tübingen and published as *Der gegenwärtige Standpunct der mathematischen Physik* (Neumann 1865), is extremely helpful: The first parts of both lectures are nearly identical (cf. Neumann 1865, 1–16 and Neumann 1868b, 1–11), thus making it easy to identify essentially *new* elements in the latter.

The Tübingen "Point of View" deals mainly with the mathematical theory of electricity and magnetism. Mechanics serves as an ideal of scientific theory formation: Its outstanding merit is to bring a great number of phenomena under a small number of "basic ideas" (*Grundvorstellungen*), and these are "inertia" and "attraction" (Neumann 1865, 13–

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6 Neumann refers to Riemann's *Hypothesen* in one of the footnotes to his lecture (Neumann 1869b, 31–32, n. 10). These footnotes, however, were added later (see, for example, Neumann 1869b, 24, n. 2). Cf. Pulte 2005, 402–412, for more details.

14). These basic ideas should not be understood as *explananda* in the sense that unknown phenomena are reduced to known phenomena, because the basic ideas themselves are "not more explicable" and even "totally incomprehensible" (Neumann 1865, 14, cf. 34–35). Mechanics forms a model science for other parts of physics for exactly this reason: A perfect science reduces a maximum of phenomena under a minimum of basic ideas, albeit that these ideas themselves may be epistemologically opaque (see Neumann 1865, 17). Neumann dismisses here the traditional CMN-claim for *evidence* of first principles, and anticipates some of Mach's and Kirchhoff's ideas of how scientific theories and phenomena are related. However, the basic ideas of mechanics (inertia, gravity) are neither understood as revisable, arbitrary or matter of choice, nor does Neumann discuss the validity of the mechanical principles related to these ideas (law of inertia, second law of motion, law of gravity) – the Tübingen "Point of View" does not include any critical discussion of the principles of mechanics at all. Finally, the contribution of *mathematics* to the character and status of mathematical physics plays no significant role in this lecture. These features have to be kept in mind with respect to the Leipzig "Principles" from 1869.

In 1868, Neumann moved from Tübingen to Leipzig, where he got the opportunity to study Jacobi's lecture from 1847/48 first hand *via* W. Scheibner (cf. Jacobi 1847/48 [1996], LII, n. 166). One year later, he published a paper on the principle of virtual velocities and discussed Jacobi's mathematical treatment affirmatively. Moreover, he was impressed by Jacobi's philosophical analysis of the principles of mechanics. In comparison to the Königsberg *Dynamik*, he states, Jacobi's Berlin lecture "distinguishes itself by a criticism of the *foundations of mechanics* which – in this rigour – has never been articulated in public until now" (Neumann 1869a, 257). From Neumann's marks and marginal notes in Scheibner's copy we know which of Jacobi's remarks he was most interested in; those discussed above (part 2.2) belong to them (see Jacobi 1847/48 [1996], LXII, 3–4). At the third of November 1869, when he was fully aware of Jacobi's point of view, Neumann gave his inaugural lecture in Leipzig. While former reconstructions of this lecture assumed it as a "given" starting point of the public debate on the foundations of mechanics (see, for example, DiSalle 1993), the background sketched here seems important to me for an understanding of the origin as of the content of this lecture. In what follows, I will leave aside Neumann's analysis of the law of inertia, absolute space and the "body alpha" (cf. Pulte 2005, 421–429), and confine myself to his meta-theoretical point of view.

3.2 "Hypotheticity" in Neumann's Principles of the Galilean-Newtonian Theory

Though Carl Neumann repeats large passages of his Tübingen lecture, his general *objective* in 1869 is quite different from that in 1865. He is now interested in the truth and certainty of the principles of mathematical physics in general, and of mechanics in particular. Mathematics itself becomes important for his argument, and he draws *now* for the first time a sharp demarcation between the principles put first at the deductive structures of physical theories and those of logic or pure mathematics. In full agreement with Jacobi, Neumann sharply defines where the parallel between theories of mathematical physics and pure mathematics ends, i. e. *before* the principles of the theories in question (Neumann 1869b, 12):[7]

> For if we wanted a physical theory that is not based on some incomprehensible and hypothetical fundamental notions, but rather one that proceeds from theorems that bear the stamp of *irrevocable certainty*, that offer in themselves the guarantee of an *unassailable truth*, then we would be forced to take refuge in the theorems of logic or mathematics. But it would prove impossible to deduce a physical theory from such purely formal theorems.

Mathematical physics can not be deduced from propositions of logic and pure mathematics, because these propositions are without empirical content. An empirical theory can profit from the truth and certainty of those propositions only *qua* "deductive certainty," not at the genuine level of principles. This *duality* of certism (with respect to logic and pure mathematics) and fallibilism (with respect to mathematical physics) is not present in Neumann's Tübingen lecture, and it can be traced back to his reception of Jacobi. But there is more to come with respect to the principles of mathematical physics (Neumann 1869b, 12–13):

> We have to admit that for those principles or hypotheses [of physics] – indeed *because* they are incomprehensible, *because* they are arbitrary – one cannot speak of correctness or incorrectness, of probability or improbability, at all. …

7 In the following quotations from Neumann's lecture the English translation from 1993 was used, but modified in several cases (cf. Neumann 1993).

> To be sure we are sometimes able to use the word *probable* as well as the word *true* as an *Epitheton ornans* [i.e. a decorating epithet]. But we shall wish to claim only that until today these principles have best been corroborated, not that they are established forever, and even to a lesser extent that they (like a theorem of logic or mathematics) offer in themselves the guarantee of unassailable stability, the guarantee of irrevocable truth.

Strictly speaking, principles that are starting points of a theory of physics will *never* correctly be called true or probable. Rather, they will always be regarded ... as something *arbitrary* and *incomprehensible*.

While Jacobi is willing to accept mechanical principles in the best case as "probably" true, Neumann's *dictum* "neither true nor probable" – reminiscent of A. Osiander's famous preface to Copernicus's *De revolutionibus* (Neumann 1869b, 12, 24–25) – goes further: As mathematical physics is strictly deductive, neither truth nor probability (in the sense of "degree of certainty") can be transferred to the principles at the top (cf. part 3.3). Therefore the first principles are not immune from empirical falsification. Neumann's attitude, that even these principles are at stake when a theory is tested, explicitly includes the basic principles of the Galilei-Newtonian *mechanics:* They, too, can be overthrown; they are "arbitrary" and "moveable," as he repeatedly says (Neumann 1869b, 13–15, for example). These characteristics of any principles are rooted in their *mathematical* character: The area of mathematics is "infinite," and therefore the "latitude for the arbitrary choice of principles is extraordinarily large" (Neumann 1869b, 32, 31, n. 10). This does not mean that Neumann asks for an arbitrary proliferation of principles without methodological guidance, but that any claim for their validity depends on empirical tests at the *end* of a deductive chain, and that the process of testing can never – even in the case of repeated corroboration – justify a dogmatic attitude towards the theory in question and the principles at its top (cf. Neumann 1869b, 23).

Like Riemann, Neumann does not adopt Jacobi's notion *convention*, but uses the traditional *hypothesis* for his characterisation of first principles of mathematical physics. (All these "principles" are, basically, "hypothesis"; Neumann 1869b, 12). Like Jacobi, however, he stresses the possibility of choosing quite different principles, thus indicating that different sets of principles and therefore different theories on one area of phenomena are possible (Neumann 1869b, 23). And like Riemann, he *explicitly* rejects not only evidence and certainty of first principles, but also one last *residuum* of traditional CMM: the *uniqueness* of first principles (and theories) of mathematical physics.

This is the reason why Neumann rejects *young* H. von Helmholtz's claim that principles of mathematical physics are to be understood as elements of "objective reality" (Neumann 1869b, 23). It is the "*essence of mathematical-physical theories,"* he says, to be "subjective constructions, originating from us," and "starting from arbitrarily chosen principles and developed in a rigorous mathematical manner," and determined to "supply us with the most accurate picture possible of phenomena" (Neumann 1869b, 22).

Within this philosophical framework, Neumann's discussion of absolute space and the Newtonian law of inertia as well as the introduction of his well-known "body Alpha" – topics not central for my outline itself – gain a definite methodological meaning: Neumann divides up the traditional law of inertia as an indubitable, dogmatic principle into three different principles (existence of Alpha, rectilinearity, uniformity), which together form the empirical content of this law. This decomposition and the explication of different empirical attributes by Neumann are paradigmatic for a modern understanding of mathematical philosophy of nature (MMN): explication and reflection of premises, criticism of hidden (metaphysical) assumptions, operational formulation of empirical tests and other characteristics of a *modern concept of science*[8] can be found in Neumann's Leipzig inaugural lecture. And the origin and meta-theoretical viewpoint of this hallmark of CMN cannot be understood without the rise of a new understanding of mathematics.

3.3 A Note on Neumann as a Precursor of Popper

K. R. Popper, in his article *A Note on Berkeley as a Precursor of Mach*, acknowledged Berkeley's modern, *quasi-Machian* critique of essentialism in general (Popper 1953). Mach frankly acknowledged at least Carl Neumann's priority with respect to the critique of Newton's absolute space and the law of inertia (Mach 1872 [1909], 47, n. 1). It seems, however, that Popper *nowhere* acknowledged Neumann's merits for the rise of a "critical" concept of science in his sense, including a strict fallibilism.

Admittedly, Neumann is not looking for an epistemological and methodological basis of his understanding of scientific theories. There

8 Cf. Diemer 1968, Diemer and König 1991, Schiemann 1998, Part A, esp. A. IV, and Pulte 2005, ch. II for a detailed analysis of the characteristics of 'classical' and 'modern' concepts of science.

is no criticism of induction, no discussion of a criterion of demarcation or an epistemological investigation into the "empirical basis" to be found in his lecture. However, the mathematical physicist Neumann and the philosopher of science Popper share some convictions and insights concerning scientific theories worth mentioning. Both thinkers are anti-essentialists and anti-instrumentalists (cf. Pulte 2005, 418–421). Both are anti-dogmatists and deductivists with respect to scientific theories and hold the view that they are, by and large, determined by their first principles. Both emphasize that the corroboration of principles can never demonstrate their truth or probability. And both understand theory-building as a *creative* process of inventing and testing principles and share the belief in scientific progress as an outcome of this process, as long as it is controlled by a methodological reflection. Popper certainly would have subscribed to the concluding sentence of Neumann's "Principles": "We must always be aware that these principles are something *arbitrary*, and therefore something *mutable*, in order to survey, wherever possible, what effect a *change* of these principles would have on the whole shape of a theory, and to be able to realise such a change at the right time, and (in a word) to be able to preserve the theory from *petrification*, from an *ossification* that can only be pernicious and an obstacle to the advancement of science" (Neumann 1869b, 23; Neumann's emphases).

4. Conclusion

I would like, with three short remarks, to sum up my outline of the structural development of the rise of hypothetical thinking with respect to the foundations of mechanics. Firstly, modern understanding of mechanics as a genuine *physical* science should not blind us to the fact that in the 18th and in early 19th century it was credited with the evidence and certainty of mathematics, being *de facto* regarded as epistemologically equivalent to Euclidean geometry by nearly all scientists and most philosophers of science. *Euclideanism* in Lakatos's sense was, indeed, the dominant image of rational mechanics as a science up to the middle of the 19th century (CMN). Secondly, I have stressed the "top down-perspective" of the working mathematical physicist, in order to show that the dissolution of mechanical Euclideanism and the rise of hypothetical thinking starts *here*, at the "top." And it *had* to start here, because a "bottom up" dissolution (by empirical falsifiers) could take place only after the existence of true "axioms" of mechanics became dubious. The modern understanding of mechanics (MMN), predominant in the last two decades of the 19^{th} century, is an outcome of *both* processes. Thirdly, I have underlined the importance of a new understanding of mathematics for the development in question. In the course of the 19^{th} century, a "shrinking-process" of mathematical evidence and certainty takes place, and not only physical geometry, but also mathematical physics is affected by this process. The concept of *pure mathematics*, isolating arithmetic, algebra and analysis as the remaining mathematical "paradise" of evidence and certainty from the larger area of the mathematical sciences, plays a crucial role in this process. My outline has stressed the position of the prominent mathematicians C. G. J. Jacobi, B. Riemann and C. Neumann, but minor figures like W. Scheibner, W. Schell, O. Rausenberger and others could be added.

While the application of mathematics in the sciences was, for a long time, understood as the best possible expulsion of the "demon named hypotheticity," the rise of modern mathematics and – in its succession – modern logic taught philosophy of science that this kind of "exorcism" will not work for the empirical sciences. Though W. V. O. Quines "Two Dogmas" promoted a new empiricism in the philosophy of mathematics, the older lesson was not lost. And today, there is hardly any scientist or philosopher of science who believes that hypotheticity of principles of empirical theories and, consequently, of empirical theories themselves, is a demon at all.

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