Chapter 3
J. F. Fries' Philosophy of Science, the New Friesian School and the Berlin Group: On Divergent Scientific Philosophies, Difficult Relations and Missed Opportunities

Helmut Pulte

Vor dem Irren aber, so glauben wir,
schützt einzig und allein das Nichtdenken.

*(*Walter Dubislav, *1922)*

3.1 Fries' Development of Kant's Philosophy of Science

Fries never shied from admitting his indebtedness to Kant's approach and explicitly subordinated his own thought to the core elements of that framework. Specifically, "Kant's distinction of analytic and synthetic judgements, the fundamental question of how synthetic judgements a priori are possible, the discovery of the transcendental guideline and the system of categories and ideas, the discovery of pure intuition, and finally the implementation of the doctrines in his critiques" (Fries 1967–2011, vol. 29, 808).

If one aims at characterising Fries' own philosophical work—especially with regard to the New Friesian School and the Berlin Group—one is well advised to distinguish between two of its key facets: The first aspect, although inventive, is highly contested with respect to its philosophical method. However, it is without serious implications for the general understanding and estimation of science. The second aspect while having been widely neglected during Fries' lifetime, is also

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H. Pulte (✉)
Institute of Philosophy, University of Bochum, Bochum, Germany
e-mail: Helmut.Pulte@rub.de

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highly inventive. What is more, it is quite progressive as regards the philosophy of science and mathematics.

The first aspect of Fries' work regards his anthropological criticism of reason (Fries 1828–1831). Herein, he aimed to dispel what he called Kant's 'transcendental prejudice,' i.e. the view that even our *a priori* knowledge is in need of proof (which Kant tried to provide via a 'transcendental deduction' concerning the categories). According to Fries, we can justify the basic judgements of our cognition neither by transcendental or logical deductions, nor by demonstrations based on pure intuition. Instead, we have to make them explicit via a reflective introspection of reason. In order to achieve this 'demonstration' (*Aufweisung*), he suggested a regressive method of analysis of inner experience via reason, which is said to lead to (and at the same time, make aware) our basic judgements. Somewhat misleadingly, he called this procedure 'deduction,' and demarcated it from both the proof via first principles in propositional form as well as from demonstrations by intuition.

Since demonstrations are psychological procedures of introspection, Fries was often criticised for defending psychologism, in the sense of a reduction of philosophical judgements to empirical psychology. Kuno Fischer, for instance, famously phrased it like this: "Whatever is *a priori* can never be recognized *a posteriori*" (Fischer 1862, 99). But, in fact, Fries aimed at a psychological method of demonstration, not at empirical justification of a priori knowledge. As such, it is quite misleading to label him a psychologist (Sachs–Hombach 1999).

Fries' theory of justification by proof, demonstration, and deduction became pivotal for the science-orientated New Friesian School,[1] though it had no direct consequences for foundational issues of the 'exact' sciences. For, his psychological demonstrations did not develop any modification as regards the synthetic principles that are a priori of mathematics and the theory of motion. Moreover, he considered both Euclidian geometry and Newtonian mechanics to be sufficiently substantiated by these principles, though he gave them a methodological meaning that offered some opportunities for the later development of physics.[2]

Now I would like to explore the second aspect of Fries' philosophy, mentioned above, which often seems remarkably modern and is to be found 'below' the indicated level of a priori foundation. One might describe this project as further developing Kant's philosophy of science in a methodological and empirical direction. Such thoughts are less prominent in his major philosophical works than in his *Mathematische Naturphilosophie* (Fries 1822), in his books on logic (e.g. Fries 1837) and in several of his textbooks on the natural sciences. Here, Fries took significant steps to develop Kantian theory, out of a desire to reconcile it with the sciences of his times.

[1] See esp. Dubislav (1926a, 1929), Eggeling (1904), Grelling (1907), Kastil (1918), Nelson (1904, 1962).

[2] This is an important aspect with respect to special relativity to which I will come back later (see Sect. 3.4).

Fries was a philosopher with an excellent knowledge of mathematics and the natural sciences,[3] and he knew very well that Kant's *First Critique* and his *Metaphysical Foundations of Natural Science* only provided a philosophical foundation for a small area of mathematics and 'science proper.' For example, Kant never seriously undertook the philosophical analysis or justification of calculus, of formal algebra, of the theory of probability, or of analytical mechanics. Indeed, as is well known, he even relinquished the idea that chemistry could acquire the status of a proper science.

Fries' strategy was to extend Kant's approach to these 'new' sciences in two different manners. On the one hand, he developed a methodology of the empirical sciences that cast Kant's synthetic principles as a priori heuristic guidelines (*Maximen*) of empirical investigation, in areas where their constitutive character was by no means obvious. Here, he could tie in with Kant's analogies of experience of the first *Critique* and in the *Critique of Judgement*. On the other hand, he 'stretched' Kant's idea of science as a deductive *system* by disentangling the concepts of 'system' and 'theory.' For, while he held that there is only *one* system of scientific knowledge that stands as a regulative ideal, in Kant's sense, Fries thought that different empirical theories (sciences) governed by different 'local' principles are possible. In his 'philosophy of mathematics'—a term seemingly introduced by Fries[4]—he likewise extended the area of 'proper knowledge' gained by reason from the construction of concepts: He broadened Kant's understanding of mathematical knowledge by introducing 'productive imagination' (*productive Einbildungskraft*) as a foundational instance. Consequently, he asserted that syntax, i.e. the theory of pure laws of arrangement, should be considered as part of mathematics on equal footing with arithmetic (Fries 1822, 64–65; cf. Bernays 1933, 109).

Both these facets of Fries' new architecture of the philosophy of mathematics were representative of the actual mathematical developments of his time, which were coined not so much by geometry or (synthetic) mechanics as by formal arithmetic, algebra and 'analytical' mathematical physics. This is all the more important as Fries not only aimed to supply a broader foundation for 'pure' mathematics. He also thought that such a general foundation (i.e. beyond Euclidean geometry and elementary arithmetic) could stand as a source for fruitful hypothesis-building in the realm of the empirical sciences.

In what follows I will elucidate a number of the specific achievements of Fries' philosophy of science and mathematics. However, as these accomplishments are

[3] Besides the favorable statements on his abilities by the mathematician Carl Friedrich Gauß, the theoretical physicist Wilhelm Weber and others (cf. König and Geldsetzer 1979) one can appeal also to the naturalist Alexander von Humboldt: "Fries, in his mathematical–philosophical orientation, is a beneficence for Germany" (Henke 1937, 256).

[4] For a detailed historical report see König and Geldsetzer (1979, 45), and Pulte (1999a, 74–76).

described and analysed elsewhere in some detail,[5] I shall confine myself to those results and consequences for the 'exact sciences' which I consider relevant for the work of the Neo-Friesian School, and for their relationship to the Berlin Group:

(i) On the basis of an objective conception of probability, Fries offered the first philosophical analysis that sets out the legitimate area of application for probability statements (Fries 1842; see Fischer 2004). Via E. F. Apelt, J. von Kries and others, this approach gained some influence on the later discussion on probability (Grelling 1910; Reichenbach 1916, 1932). Experts of that time saw in Fries "the most consistent moulder" of objective probability (Sterzinger 1911, 52).

(ii) According to Fries, an indispensable task of any philosophy of mathematics is what has come to be described as 'critical mathematics.' This endeavor became an integral part of Hilbert's program of meta-mathematics: a philosophical justification (*Deduktion*) of the first mathematical principles or axioms. Without any doubt, this part of Fries' program—perpetuated by L. Nelson, G. Hessenberg, O. Meyerhof and others—was the most important one with respect to acceptance in the philosophical–mathematical community. Its influence on Hilbert's axiomatics—irrespective of manifest divergences—is obvious and well documented (Peckhaus 1990, 1999). Within the New Friesian School this topic probably allowed the most direct and intense recourse to Fries' original approach (see esp. Hessenberg 1904, 1907; Nelson 1905b, 1906, 1927; Grelling and Nelson 1908; Bernays 1930).

(iii) In his theory of rational induction, Fries relinquishes Kant's ideal of a *system* of experience in favour of a multiplicity of theories. A system continues to exist as a *synthetic a priori* foundation for mechanics. However, a multitude of theories is possible within this system, whose heuristic maxims may have a *constitutive* function (see Pulte 1999b). The theory of electricity or magnetism, for example, may have its own maxims that can gain constitutive relevance. This means that those maxims are—as candidates for general laws of nature—related to the mechanical laws of motion only in a weak sense of compatibility. As such, separate scientific theories serve as theoretical backgrounds for the

[5]See Pulte (1999a, 2005a (esp. Ch. IV), and 2006). For Fries' conception of 'theory' and 'system' as well as for foundational aspects of his methodology, the *Grundriß der Logik* (Fries 1827) is most important. His philosophy of mathematics and the more applied aspects of his methodology can be found in his *Mathematische Naturphilosophie nach philosophischer Methode bearbeitet* (Fries 1822). A general estimation of his achievements in both respect is given by the excellent introduction of the Editors (König and Geldsetzer 1979). For Fries' philosophy of pure mathematics see also Schubring (1999) and Herrmann (2000, Ch. 3). His contribution to the theory of probability is analyzed in Fischer (2004). The heuristic dimension of Fries' concept of probability is meticulously analyzed in van Zantwijk (2009, esp. Ch. 5). Some philosophical implications of his perception and interpretation of analytical mechanics are investigated in Pulte (2005b). A more general evaluation of Fries' philosophy of science and the broader 'aprioristic tradition' is intended in Herrmann (2012). A comprehensive analysis of German philosophies of nature in the early nineteenth century, including Fries' approach, is Bonsiepen (1997).

acquisitions of further experience: Observation always depends on 'guiding maxims.' While this theory of rational induction played an important role in the first Friesian School (see esp. Apelt 1854), it was of minor importance for the New Friesian School.

(iv) (Limited) Fallibilism and Conventionalism: 'Below' the level of synthetic principles a priori, empirical laws can basically be revised by new experiences. New hypotheses, however, must not contradict any *a priori* principles and are to be formulated in such a way that they can be "refuted for certain by experience" (Fries 1822, 21). In addition to this 'Popperian' element, Fries also introduces a conventional element at the same level. Specifically, he holds that for a fixed sets of phenomena, several empirically equivalent explanatory laws are possible. Between those, neither experience nor reason can decide, but only considerations of simplicity and convenience. Moreover, conflicting observation *never* challenges a single law, but *all* theoretical assumptions on which the deductive explanation of this observation is based (cf. Pulte 1999b for a more detailed discussion).

(v) Theory of space and motion: Regardless of the 'modern' elements of philosophy of science, described above, Fries was a 'Kantian conservative' as regards Euclidean geometry. Other geometries deserving of this name, i. e. axiomatized theories of pure space, were out of his ken. As such, he attempted to prove Euclid's parallel axiom in order to solve the ongoing public discussion about it in favour of a 'unique' Euclidean geometry (Herrmann 2000, 132–136 and 222–232). This 'Euclidean fixation' had a lasting impact on the New Friesian School, especially on Nelson (see his 1905b, 1906, 1927), which will be discussed later. However, Fries was quite aware that using Euclidean geometry to elaborate a theory of motion is problematic. Specifically, he noted that the distinction of a straight line as the trajectory of an inertial motion is in need of merely conventional fixations (Fries 1822, 413–418). Moreover, motion in general is basically relative: "We always have to talk about *relative* spaces, which are movable und which we may find moving, without ever coming to an absolute space as, so to speak, a fixed basic form of the world" (Fries 1822, 422). In order to deal with this *problem of relativity*, we have to *postulate* certain rules, under which the construction of motion is possible (Fries 1822, 423–424). His follower, E. F. Apelt, maintains likewise that "there is no absolute space [. . .] for assessments, in experience we have to take space as *comparative* (relative)" (Apelt 1910, 554–555). As far as I can see, these considerations on space remained unnoticed in the New Friesian School, and played no role for the Berlin Group either. They are, however, interesting for their discussion about the theory of relativity to which I will come back later (see Sect. 3.4).

To sum up, Fries' achievements are considerable, but only certain aspects of his philosophy of mathematics, (i) and (ii), have received attention, while interesting aspects of his philosophy of science, (iii)–(v), remained largely unnoticed. As such, it makes sense to take a look at the reception of his philosophy from a more general point of view in order to yield a better understanding of these findings, before we discuss their implications for the relationship of the New Friesian School and the Berlin Group in more detail.

3.2 Fries Reception and Deflation: Historiographical Remarks with Regard to Berlin

While Fries' efforts to reconcile philosophy, mathematics and the sciences received positive feedback with his contemporaries, the later reception of his work was less favourable. First of all, mainly because of a politically motivated interdiction to teach, Fries himself failed to set up a philosophical school. What is more, his most eminent disciple, E. F. Apelt (1812–1859), suffered an untimely death. Therefore the (first) 'Friesian school,' spearheaded by that latter scholar, was a philosophical flash in the pan. In addition, the reception of Fries' work within academic philosophy suffered from the dominance of German Idealism (especially Hegel and his adherents), to which his philosophy was opposed. Later, Neo-Kantianism and its imperative of going straight 'Back to Kant' led to a disregard of post-Kantian developments, even if they stood in close relation to his work. For these reasons and others, mainly rooted in the problematic German historiography of philosophy and the sciences (see Pulte 1999a), Fries' attempt to bring philosophy and science together was poorly received in the later nineteenth and early twentieth century, outside of the New Friesian School. Given this background, it is hardly surprising that direct references by the Berlin Group to the work of *Fries* are—apart from Dubislav and Grelling—rare exceptions. But, even beyond those considerations, the height of that alliance (1927–1933) was a century removed from the publication of Fries' most relevant contributions to the philosophy of science, and its disinterestedness in (or even hostility to) historical research (cf. Hentschel 1991, 34) made such a reach back in time out of the question.

Reichenbach's early leanings towards Kant's a-priorism are well known, and his perspective of the post-Kantian development is quite similar to that of many Neo-Kantians. Namely, that it is a period of philosophical degeneration and misunderstanding of science. This attitude is still visible in his late book on *The Rise of Scientific Philosophy* (more a book of historical fairytales than a serious historical investigation). Therein, Fichte, Schelling, Hegel and others are disqualified as "as if philosophers" (Reichenbach 1969, 142) with no affiliation to science. Whereas, Fries is not even mentioned.

On Reichenbach's approach, the legitimate follower of Kant is not the 'Kantianism' of academic philosophy, but philosophy following a "method of analysing science" (Reichenbach 1920, 71) that is applied to the latest achievements of science. As he states, "(o)ne should proceed with the history of philosophy, which attired herself in systems until Kant, not with the pseudo-systems of epigones, but with a new philosophy which originated from the *science* of the nineteenth century and has been further developed in the twentieth century."[6] Thus, he simply did not consider Fries a congenial philosopher with closely related aims and interests. Rather, it seems that he referred to Fries only once, albeit positively. In his *Elements of Symbolic Logic* Reichenbach stated that with respect to Fries' *New Critique of Pure Reason*: "(t)he fact that a proposition stating that a formula is logically necessary is in itself an empirical statement seems to have been first pointed out by J. F. Fries [. . .]" (Reichenbach 1947, 188). Also, in his dissertation on probability, he did refer at least to the objectivistic concept of probability of E. F. Apelt, J. von Kries and K. Grelling, who again referred to Fries (Reichenbach 1916, 215–223).

The consultation of the works of other members of the Berlin Group like Carl Gustav Hempel, Alexander Herzberg, Wolfgang Köhler or Kurt Lewin yields an equally disillusioning picture. At least Richard von Mises, in his *Kleines Lehrbuch des Positivismus*, allowed Fries an earnest endeavour of advancing Kant's theory "in a scientific sense." However, he surprisingly asserts that Fries "tried to constitute the Apriori psychologically by some sort of analysis of feelings of evidence—which is very close to our viewpoint" (von Mises 1939, 391). This is startling, since von Mises was not even too close to his own viewpoint in this systematic misjudgement.

As already mentioned, Dubislav and Grelling had a different attitude towards Fries. Grelling left the New Friesian School in 1922, after an argument with Nelson about Einstein's theory of relativity (Peckhaus 1990, 148; cf. Sect. 3.4). Whereas Dubislav had probably come into contact with Fries' and Nelson's philosophy during his studies of mathematics (inter alia with Hilbert) in Göttingen from 1914 onwards. Given their exposure, Dubislav (see sep. his 1926a, b, 1929) and Grelling (see esp. 1906, 1907, 1910) published on Fries and Nelson. Indeed, Grelling even published *with* the latter, on the topic of logic (e.g. Grelling and Nelson 1908).

Both Dubislav and Nelson later belonged to the "founding generation" (Rescher 2006, 282) of the Berlin Group, and were quite active members (Danneberg and Schernus 1994; Hoffmann 1994; Peckhaus 1994). Otto Neurath's short description of the Berlin Group mentions that Reichenbach, Dubislav and Grelling "focused primarily on logical and physical problems as starting points of epistemological critique (toeholds in Kantianism and Friesianism, influence of Cassirer and Nelson)" (Neurath 1930, 390; cf. Hentschel 1991, 30). Grelling, Dubislav and (the early) Reichenbach, from 1927 onwards, counter-balanced to a certain extent the strong positivistic leanings of the group, emanating from the Mach-orientated subgroup around Joseph Petzoldt. However, Reichenbach's subsequent departure from Kant's a priori was already terminated when he got into closer contacts to Dubislav and

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[6]Reichenbach (1969, 142). For him, scientific philosophy after Kant is simply a kind of 'Science as Philosophy.' Herbert Schnädelbach describes under this heading the changing relation of both areas after 1831 in a quite adequate manner: "Philosophy deserts to science to a degree that threatens its identity." (Schnädelbach 1991, 113) This strategy of defense, which can be detected in different philosophical movements of the nineteenth century, develops in Reichenbach's systematic turn of this historical development to the only legitimate form of philosophy at all. Ironically enough, Fries called for a philosophy that itself is "rigorous science (*strenge Wissenschaft*)" (Fries 1828–1831, vol. 3, 169).

Grelling. Thus, their philosophical influence on him—in terms of the mediating 'Friesian elements' described in the first section—was obviously very limited.

As such, apart from Grelling and Dubislav, the relationship between the Berlin Group and the work of Fries is mainly a history of *missed chances* (cf. also Sect. 3.3). For, while Reichenbach is more or less right in maintaining that one should not forget that the history of philosophy is "history and not philosophy" (Reichenbach 1969, 364); equally correct is the idea that a serious study of the history of philosophy can lead to interesting, maybe continuative or—to complete the augmentation in Reichenbach's sense—even original 'scientific philosophy.' But Reichenbach obviously stuck to the assumption—hardly justifiable by logic or experience—that even the most basic and seminal ideas of this philosophy depend on *present* scientific research: "He who contributes to the new philosophy does not look back, because his work would not profit from historical considerations" (Reichenbach 1969, 364).

3.3 Divergent Scientific Philosophies: The New Friesian School and the Berlin Group

It is clear that Fries' work largely failed to attract the attention of the Berlin Group, but what was the relation of its members to the New Friesian School, and what are their distinctive features? Freely adapted from Viktor Kraft, one might say that neither the Berlin Group nor the New Friesian School are 'unambiguous units' (cf. Haller 1993, 61). That is, they were not philosophically homogenous groups that can be characterized and differentiated via some rare common convictions. However, both groups were manifestations of a discontent with the academic philosophy of their time. In addition, both groups were concerned with a close collaboration of the different sciences and philosophy. In both groups one encountered, not only philosophers, but also mathematicians, natural scientists and other academics. However, this is where the similarities end. A closer look at the New Friesian School reveals substantive differences:

Nelson founded this school in 1903, when he was still a student in Göttingen. The founding members from philosophy, mathematics and other disciplines (Blencke 1978) were committed to the basic philosophical theorems of the Kant–Friesian philosophy as they were passed over by the (first) Friesian School around E. F. Apelt. From the beginning, Nelson laid claim to the philosophical and organizational leadership of the new school (Franke 1991, 66–71). Indeed, by 1904 he had already launched the *Abhandlungen der Fries'schen Schule, Neue Folge* as the mouthpiece of the new foundation. Co-edited by L. Nelson, G. Hessenberg and G. Kaiser, the *Abhandlungen* appeared with interruptions from 1904 to 1936. From the beginning it was meant to spread and develop the 'true' Kantian philosophy in the tradition of Fries and Apelt and to counter-balance the strong influence of Neo-Kantianism in the German philosophical journals of that time. In 1913, Nelson backed up the New Friesian School—a more or less informal group without institutional setting—with the *Jakob Friedrich Fries Gesellschaft*. It organized conferences and gained influential members like D. Hilbert (Peckhaus 1990, 152–154). The programmatic statements of the *Abhandlungen* and the discussions about the aims of the *Gesellschaft* allow for a rather precise appreciation of the New Friesian School and a demarcation of the Berlin Group.

In order to see how this is so, it is helpful to begin by disposing of a (possible) misunderstanding: To begin with the disposal of a (possible) misunderstanding: The attitude towards the history of philosophy seems *prima facie* quite similar and does not mark a criterion of demarcation. The commitment of the New Friesian School to Kant, Fries and Apelt[7] should be understood as a *systematic* one, not as an appeal to extensive historical research. Nelson starts his first contribution to the *Abhandlungen* with the motto: "There are scholars who hold the opinion that the history of philosophy (both old and new) itself is philosophy; these Prolegomena are not written for them" (Nelson 1904, 1). Whereas the Berlin Group states in its appeal from 1927 that it feels compelled to an empirical philosophy "on [the] basis of the experiences of the single sciences" (Hentschel 1991, 25), the systematic primacy of this earlier school of 'scientific philosophy' is the critical method in the line of Fries, especially the idea of an empirical-psychological self-introspection of human reason in order to uncover apriori-knowledge without transcendental deduction.

It has to be stressed, however, that even Nelson and other members of his school did not analyse and exhaust Fries' contributions to the philosophy of science with the accurateness it deserves: They strongly focused on his 'new' *Vernunftkritik* (cf. Sect. 3.1) and extensively analysed its epistemological implications. As such, they appreciated and developed his philosophy of (pure) mathematics (e. g. Hessenberg 1904, 1907; Nelson 1905b, 1906, 1927), and they also discussed his theory of rational induction and deduction in some detail (e.g. Nelson 1904, 1905a). However, neither Nelson nor other members of the group broached the issue of the conventionalist and fallibilist elements in Fries' philosophy of science, nor did they fully grasp his theory of space (cf. Sect. 3.1, (iii)–(v)). Because these innovative aspects of Fries' philosophy of the empirical sciences were not really reflected in the New Friesian School, their general attitude regarding the foundation of physics remained radically *conservative*, as I will subsequently show. It is this conservatism that I consider to be the main obstacle for a fruitful relation of the Berlin Group to the philosophy of the empirical sciences.

Nelson's dogmatism, which has no intellectual roots in Fries' philosophy, reveals the ambivalent role the empirical sciences played within the New Friesian School. Typifying this issue is the fact that he issued two prefaces within the first issue of the *Abhandlungen*. He begins with the manifesto of the First Friesian School, dating

[7]This commitment becomes most obvious from the Editor's foreword of the first issue of the *Abhandlungen* and is accompanied by a strong rejection to any other forms of Kantianism, which are charged of abandoning Kant's true critical method, being unscientific and obscurantism. They are philosophical sects which the history of philosophy will overcome as present science overcame "Patricius, Robert Fludd and Jakob Böhme. *Kant*, *Fries* and *Apelt*, however, will continue to stay next to *Keppler*, *Galilei* and *Newton*" (Hessenberg et al. 1904, xii).

from 1847, on which he then elaborates the second preface without uncovering any time-boundedness (cf. Pulte 2005a, Ch. V and VI) of this nearly 60 year old document. A few sentences from the 'old new' preface will highlight the fundamental relationship between scientific philosophy and empirical sciences to which Nelson recommitted the New Friesian School from the beginning[8]:

(a) Any philosophy which is in accordance with the exact sciences can be true, any one which is conflict with them must necessarily be wrong. [. . .]
(b) All knowledge of nature is inductive, it does not stem from philosophical concepts, but from experimentation and observation. [. . .]
(c) Induction alone would not lead to any fixed results, if it were not aided by philosophy of nature. Such philosophy of nature is and can be only the one whose mathematical principles have been developed by Neuton [sic!] and whose metaphysical basis has been clarified by Kant. Such mathematical philosophy of nature forms the background of all inductions and regulates their processes. [. . .] It is therefore nothing more than a delusion to believe that the inductive sciences exist independently of philosophy.

If we take these statements at face value—and the comments of the school on 'mathematical philosophy of nature' provide no reason to do otherwise—it is clear that the relation between scientific philosophy and empirical science is marked by a strong, almost necessary mutual dependence, which becomes obvious from the three points made above. *First*, scientific philosophy must not clash with the 'exact sciences'—if she does, it is to her disadvantage. So far, this is in line with the empiricist program of the Berlin Group. However, Nelson states very clearly at this early point—not yet occupied with their program, but with Positivism and Neokantianism—that according to this criterion, only the philosophy of Kant and Fries will remain due to its "*scientific method*" (Hessenberg et al. 1904, viii). *Second*, All empirical sciences are in need of observation and experimentation, and all their proper knowledge depends on rational induction. *Third,* the Kant-Friesian philosophy solely identifies the principles of Newton as the most general principles of rational induction. Accordingly, the inductive sciences, if they are to be considered as scientific, are dependent on the Kant-Friesian metaphysics of nature for justification.

This, of course, is a decisive point of demarcation between Nelson's view—the 'official doctrine' of the New Friesian School, with respect to the foundation of the empirical sciences—and the later position of their Berlin Group, mainly fixed by Reichenbach in his analysis of space and time in the succession of Einstein's theories of relativity. Nelson never revised his position from 1904—the year before the special theory of relativity emerged—in his later career. Rather, he integrated the 'double link' between scientific philosophy and a supposedly infallible science described above by means of Fries' theory of non-intuitive immediate knowledge

[8]Hessenberg et al. (1904, iv–vi); numbers added by me. The heading of the foreword is: "Vorwort der alten Folge, zugleich Vorwort der neuen Folge." See also Apelt et al. (1847, 3–5).

in a *certistic* theory of scientific knowledge. The synthetic principles of the natural sciences are to be justified by a synthetic a priori principle of rational induction. While, that principle is itself rooted in immediate a priori knowledge. Karl Popper—obviously not aware of Dubislav's relevant analysis of the foundational problem in Fries' philosophy (Dubislav 1926a)—perceptively criticised Nelson's circular reasoning in his early work *Die beiden Grundprobleme der Erkenntnistheorie* (Popper 1994, 110–114). I will not discuss the philosophical ambiguity of Popper's criticism,[9] but confine myself to what might be regarded as its 'moral' with respect to the foundations of the empirical sciences from a Friesian point of view. Specifically, it is untenable to establish the ultimate philosophical foundation of a *unique* system of knowledge by a fixed set of synthetic principles a priori—be they determined by a transcendental deduction or by empirical introspection. However, it does make sense to strive for the uncovering of first synthetic principles, by Fries' method of 'regressive abstraction,' on the basis of present scientific knowledge as a whole. The principles gained by this method are not 'absolute' but 'relative' a priori. That is, they can change in the course of the successive development of our scientific knowledge. As such, they act as heuristic directives for the application of the basic (or constitutive) concepts involved. I claim that such a 'liberalisation' follows the genuine intellectual tradition of Fries' philosophy of science, which aims indeed at a dynamical synthesis of Kantian apriorism and scientific development (cf. Sect. 3.1). Therefore, it is not by accident that philosophers from the Neo-Friesian tradition like Paul Bernays (1953, 125–131) or Stephan Körner (1979, 6–13; cf. 1970, 1984) later veered in this direction. As regards the mathematical philosophy of nature (or mechanics), this broadening fits even better with Fries' original approach, as the pure intuition of space and time does not amount to immediate knowledge in his sense (Bernays 1953, 119) and as his construction of motion does not rely on Newton's absolute space, but on relative spaces (cf. Sect. 3.1, (v)).

Reichenbach's early *Relativitätstheorie und Erkenntnis a priori* is certainly affine to this broadened Friesianism (cf. Reichenbach 1920, 1–5, 46–58), as well as—to some extent—the early discussion of the theories of relativity in the Berlin Group. However, the New Friesian School did not indicate in its *official* statements up to 1927 (the year when Nelson died and the Berlin Group was founded) any sympathy for such a course of liberalisation. Quite contrary, Nelson unflinchingly adhered to his *certism*, as regards his mathematical philosophy of nature, after the emergence of special relativity and, as far I can see, nearly until his death (cf. Sect. 3.4). In 1908 he opposed Ernst Mach's view on mechanics, as follows:

[9]On the one hand, Popper's charge of either circularity or infinite regress—in the context of his well-known trilemma of justification—falls short of the Friesian claim to achieve an demonstration (*Aufweisung*) bei introspection and *not* by a quasi-logical justification of a priori knowledge. On the other hand, the Friesians have to admit that this demonstration serves for a certain kind of justification—Nelson's claims above do make this quite obvious. However, contrary to the logical structure of Popper's criticism this justification does not aim at the truth of special propositions a priori, but at the *whole* of the transcendental perception (cf. Fries 1828–1831, vol. 2, 99–100). See Sachs–Hombach (1999) for a closer examination of Popper's criticism and why it does not do justice to Fries' method of demonstration.

"As the principles of mechanics do not stem from experience, it is only consequent when those who want to proceed empirically are converting the fundamental laws of mechanics into arbitrary assumptions, because the practicability of which is a matter of larger or lesser convenience only. However, with these [laws] they abandon any objective criteria of scientific truth and return to a pre-Galilean level of science." (Nelson 1908, 298) At the core of his adherence to (what he regards as) a 'Newtonian' foundation of the empirical sciences is his advocacy of metaphysics as an integral part of science itself. On this understanding, it is the task of true scientific philosophy to unveil this metaphysics and its fundamental role, in order to keep, so to speak, 'science itself scientific.' As Nelson writes, "(h)e who wants to eliminate metaphysics from science hands science over to a metaphysics *outside* of science—as without metaphysics no judgements are possible at all,—i.e. he unwittingly and unconsciously pays science over to mysticism. This should be considered in due time by those who regard the matter of science and enlightenment with passion" (Nelson 1908, 299). Popper's later warning addressed to Wittgenstein and the Vienna Circle sounds similar, though *he* insisted on a demarcation of metaphysics and science: "Positivistic radicalism annihilates metaphysics and along with it science" (Popper 1982, 11). And indeed, though the Berlin Group did not accentuate its anti-metaphysical bias as strongly as the Vienna Circle, Nelson's conception of scientific philosophy is quite different at this point. That is, scientific philosophy, for him, is not only about logical and methodological analysis of existent science, but also about its ineradicable metaphysics and its legitimate fundamental claims. Quite contrary, the "method of analysing science (*wissenschaftsanalytische Methode*)" of the Berlin Group was meant "to oppose consciously all claims of a philosophy which affirms an autonomous right of reason and which would like to establish a priori valid propositions which are not subject to scientific criticism" (Anonymous 1930, 72). Here we find developed the basic point of demarcation between the two scientific philosophies, the New Friesian School and the Berlin Group developed. All affinities in the areas of logic and the philosophy of (pure) mathematics notwithstanding, they had basically incompatible ideas about how the foundations of the empirical sciences should look like. This divergence takes a concrete shape and becomes most virulent with the rise of Einstein's theories of relativity—even more so as Reichenbach from 1920 to 1929 was their "busiest and most persistent defender against the most varied forms of contradictions and attacks" (Hentschel 1990, 178).

3.4 Relativity and Geometry in the New Friesian School

Basically, Nelson's adherence to Newton's mathematical principles of natural philosophy constitutes an *a priori* fixation on the space/time structure of classical mechanics. It is therefore hardly surprising that the New Friesian School's examinations of the special theory of relativity (SRT) in the *Abhandlungen* are rare and rather critical. Indeed, the general theory is ever only mentioned once, in an article of the *Abhandlungen* published after Nelson's death (Bernays 1933).

Otto Berg's paper "Das Relativitätsprinzip in der Elektrodynamik" from 1912 and Paul Bernay's paper "Über die Bedenklichkeiten der neueren Relativitätstheorie" from 1911 (published in revised form in 1918) deal with Einstein's SRT in a competent, fair and critical manner. Both accept the empirical findings and consider the technical apparatus of the special theory in some detail (Berg 1912, 336–375; Bernays 1918, 463–474). Both, however, are sceptical about to what extent the principles of Einstein's new theory really solve the fundamental problems of classical mechanics, or whether they are even mandatory in order to do so. The new concept of simultaneity poses special problems for both (Berg 1912, 376–378; Bernays 1918, 475–478). Additionally, they refer independently of each other to Walther Ritz's emission theory of light as a possible alternative with respect to Einstein's principle of the constancy of light velocity in vacuum (Berg 1912, 379; Bernays 1918, 479–481), in order to show that SRT is not a necessary consequence of the relevant empirical findings. Most importantly, both explicitly reject that philosophy has to admit basically new intuitions of space and time. Berg maintains that Einstein's principle of relativity exceeds empirical evidence and is, therefore, "a proposition that still can be confirmed or rejected. [. . .] The view that one has to adhere to the principle of relativity in any case cannot be derived from experience, but corresponds to a metaphysical need the warrant of which we would not like to discuss here" (Berg 1912, 382). Here, the strong suspicion becomes obvious that SRT entails 'bad metaphysics' disguised as empirical science. Bernays underlines the matter of principle in an even stronger manner and questions the legitimacy of using physical research to casting doubt on the "a priori given (*das a priori Gegebene)*" properties of space and time by a combination of experiment and new theory-building (Bernays 1918, 475). With respect to simultaneity, he develops an argument that is reminiscent of Kant's third analogy of experience (cf. Pulte 2010, 243–244), later picked up by Nelson (1962, 684–687). It concludes: "These disquisitions should be sufficient to show that, for the pure philosophical standpoint, the view that the theory of relativity entails new insights about the relation of space and time depends on a mere delusion" (Bernays 1918, 478). Though Bernays considered SRT to have significant explanatory power, especially with respect to electrodynamics, he thought that its basic principle had to be rejected, "because for the decision about the acceptability of a theory its explanatory value can only be regarded after its apriori (i. e. basically methodological) admissibility is guaranteed." As such, his basic message to the Friesian philosophers is that they need not be worried about Einstein's SRT: "The main result of these considerations is that there is no sufficient reason to doubt the hitherto existing conceptions of time and space" (Bernays 1918, 482). Other statements of the *Abhandlungen* at that time (1905–1918) are more or less mere echoes of Nelson's conservatism in this regard.[10]

Independent of the discussion on Einstein's theories, though systematically linked to his new physics, was the attitude of the New Friesian School towards non-Euclidean geometries. Nelson picked up the ongoing fundamental debate in geometry in 1905 and linked it—as did Hessenberg in the year before (Hessenberg 1904)—to Hilbert's axiomatics, in order to engross his program for the revival of Fries' critical mathematics (cf. Peckhaus 1990, 158–168). Hilbert's criteria for axiomatic systems (consistency, independence, completeness) are utilised for the 'critical project.' Nelson pursues his aim to demonstrate the superiority of a Kant-Friesian approach to geometry over other (i. e. empiricist and logicist) approaches, by and large turning the tables on his opponents. For, Kant's thesis that the axioms of mathematics have non-logical origin and that their validity does not depend on experience is *best proven* by the possibility of consistent, non-Euclidian geometries (cf. Nelson 1905b, 388 and 392). While the axioms of both Euclidean geometry and of non-Euclidean geometries are consistent, *only* the axioms of Euclidean geometry are additionally rooted in the pure intuition of space and, therefore, are apriori *and* synthetic. The consistency of axiomatic systems is neither sufficient for the truth of their axioms, nor for the existence of the matters they are meant to represent. Therefore, the main difference between the Euclidian geometry and its rivals is epistemological in nature. That is, the axioms of the former have a privileged origin in pure intuition, whereas the axioms of the latter do not.

This argumentation, in favour of a 'two-tier geometry,' was widely accepted among the remaining members of the New Friesian School approximately until Nelson's death. As such, it backed their rejection of Einstein's physics temporarily. However, after the general theory of relativity proved to be of remarkable success, such lines of argumentation became difficult to defend. In short, the employing of a non-Euclidian, i.e., epistemic inferior geometry would lead to such spectacular empirical successes was something the Neo-Friesians could hardly cope with. For the group's conservatism, as regards the discussion of space and time, became a problematic confinement for those members (or friends) of the Friesian School best versed in the 'exact sciences,' i. e. for Dubislav, Grelling and Bernays.

The year 1920 marked a turning point in the development of the group, due to the publication of Reichenbach's book *Relativitätstheorie und Erkenntnis a priori*. For, that work was dedicated to explaining how Kant's a priori might be conserved, even in the light of the theories of Einstein (Kamlah 1979, 475–477; cf. also Sect. 3.5).

[10] Kurt Grelling, for example, in 1907 did not doubt the philosophical justification of Newtonian mechanics (Grelling 1907, 169–171), but later changed his view. Alfred Kastil's presentation of Fries' theory of knowledge is equally 'conservative' with respect to the theory of space and time (Kastil 1918), as is Kowalewsky's analysis of Kant's treatment of the antinomies of pure reason (Kowalewsky 1918). Other references from the *Abhandlungen* might be added, though most of them are marginal as regards space and time. In general, mathematical philosophy of nature played no important role in this organ of the New Friesian School, and to a certain extent the later volumes reflect Nelson's turn to ethics and political philosophy (cf. Franke 1991).

As such, Reichenbach's approach was very appealing to several Neo-Friesians. The old demand of the Friesian tradition to disparage a philosophy which contradicts science (cf. quotation 11, point [1.] above) had to be taken seriously in the light of Einstein's challenge, and the need for a 'Kantian' philosophy that met both the old demand and the new challenge became pressing. At a meeting of the *Jacob Fries Gesellschaft*, Grelling gave a talk on the "Theory of Relativity and Critical Philosophy" in which he sided with Reichenbach; the minutes reveal that he was blamed because of his sharp antithesis to critical philosophy.[11] He ultimately fell out with Nelson and joined the Berlin Group. Furthermore, Dubislav, Bernays and others were impressed by Reichenbach's new analysis. Although he did not belong to Nelson's circle, Dubislav was a sagacious and, on principle, also a favourable critic of Fries' theory of justification. With regard to the general theory of relativity, he admonished not so much Fries (who could not have known about such a theory) but the Neo-Friesians Nelson and Hessenberg for holding a philosophy of geometry which "stands in complete contrast to the methodological proceeding of the modern physicist."[12] He also reproached the Kant-Friesian philosophy of mathematics for misusing pure intuition as an "asylum for sluggish reason" (Dubislav 1926a, 73). Anyone familiar with Kant's understanding of *ignava ratio* knows what a serious offence against the Kantian ideal of scientific philosophy Dubislav charged the most important representatives of the New Fries School with.

As is well known, such criticism of the 'renegades' from the Neo-Friesian camp accords quite well with the attitudes of the later Berlin Group towards 'critical' theories of space and time. Although this group did not formally constitute itself until 1927, its 'predecessor,' the *Gesellschaft für positivistische Philosophie*, was a forum where Einstein's SRT was discussed affirmatively and defended against philosophical criticism (Hentschel 1991) from 1912 onwards. Its leading figure, Joseph Petzoldt, belonged next to Reichenbach as amongst the most active supporters of Einstein; with both forming a philosophical stronghold around

[11] Minutes of the meeting of the *Fries-Gesellschaft* from August 15 and 16, 1921 (Nachlass Nelson, Bll. 243–253). I did not see these minutes and refer for further details to Peckhaus (1990, 148, n. 437). Peckhaus makes quite clear that Grelling later dissociated himself from Fries' philosophy, especially from its theorem that mathematics and 'science proper' is based on synthetic principles a priori.

[12] Dubislav (1926a, 72; cf. 71). Dubislav's sharp rejection of an alleged epistemological superiority of the Euclidean geometry deserves to be quoted more extensive, because it reveals the role of physics quite exact: "He who claims that Euclidean geometry would not only rest on consistent principles, but be also a mathematical discipline that can raise a claim to truth par excellence (*Wahrheit schlechthin*), which accordant to its truth character (*Wahrheitscharakter*) be alone applicable to real objects with success, stands in complete contrast to the methodological procedure of the modern physicist, because he [the physicist] does not appeal to pure intuition and does not dogmatically distinguish with its help one special geometry, but he takes that geometry as a basis of his geometry, which serves best to derive time, position and type of future events from present empirical knowledge. These will, when they actually take place, corroborate the suitability of the geometry in question. This means that he is prepared in principle to apply under all consistent geometries a different one in case that this allows for a more exact prediction" (Dubislav 1926a, 72–73). For his discussion of the theory of relativity, see also Dubislav 1933, 144–150.

Einstein's physics (Hentschel 1990). Reichenbach's later conventionalist answer to the problem of how geometry and physics are to be coordinated, emerging from his early Kantianism from 1920 onwards, gained broad support in logical empiricism and beyond (e.g. Grünbaum 1973; Friedman 1983).

I would like to close this section with a note on Nelson. In the second half of his short academic career he was more interested in 'practical' philosophy in a broad sense than in philosophy of science. Due to internal disputes (Franke 1991, 143–150) and the developments sketched above, the *Gesellschaft* lost a couple of experts in the philosophy of science. As a consequence, the activities of the New Friesian School in the field of *scientific philosophy* decreased dramatically after 1921. However, there are at least some short published statements which evince what Nelson's philosophical position was after this defeat. Significant, in this respect, are his posthumously published Göttingen lectures from 1919 to 1926 on *Fortschritte und Rückschritte der Metaphysik* (cf. Kraft 1962, 728). Within a defense of Fries' *hylologische Weltansicht* as a philosophically well-founded form of mechanism, he casually admits that classical mechanics is—mainly due to Einstein's theory of relativity—in a critical stage of its development and might perhaps collapse (Nelson 1962, 682–684). However, he warns against the "empirical dogma" and criticises any attempt to "draw premature metaphysical conclusions" from the present unclear states of physics because science is not entitled to do so. In fact, his advice is: "In view of this situation it seems not only justified [. . .] but it is the only position compatible with critical natural philosophy (*kritische Naturphilosophie*) to abstain from such metaphysical claims and to limit oneself to the conventionalist point of view which demands from physical theories only that they have a heuristic meaning."[13] This 'conventionalist-heuristic retreat' is vague and expectant, and obviously has no consequences for Nelson's discipleship to Kant's and Fries' foundation of classical mechanics. My hunch is that Nelson did not go further (and, in a way, *could* not go further) because of the epistemological consequences a full acceptance of Reichenbach's interpretation—even at its early, 'Kantian' stage from 1920—would have had. I will now give the main reason for this interpretation.

[13]Nelson (1962, 684). In the following, the 'heuristic meaning' of this intermediate physical theory is linked, again, to Fries' heuristic interpretation of mechanical principles. Nelson here also discusses the concept of simultaneity with regard to Kant's postulates of empirical thought in general in the Transcendental Analytic in order to show that the modern physicist is "in complete agreement with critical metaphysics" (Nelson 1962, 684–685). Already in 1921 he stated against Oswald Spengler that Einstein's theory should not to be understood as a symptom of decline of physics; with a reference to Hilbert he positively appraises its axiomatic form (Nelson 1921, 520–521).

3.5 Reichenbach in 1920 and Nelson: The Basic Epistemological Difference in a Nutshell

In his book *Relativitätstheorie und Erkenntnis a priori* (1920), Reichenbach focused on the question of what kind of a philosophy of space and time could do justice to both theories of relativity. As further conditions, he sought a position that would do without the synthetic aprioris of Kant and without drifting into an epistemologically untenable empirical conception of space and time. Reichenbach's answer consists mainly of an introduction of and a strict differentiation between two kinds of principles: the *axioms of coordination (Zuordnung)* and the *axioms of connection (Verknüpfung)*. The first ones are the principles of physical geometry which are not empirical, i. e. not subject to confirmation or refutation by certain observations or experiments. The second group, however, consists of empirical laws which can only be gained by observation and experiment. This split corresponds to the two different meanings Reichenbach finds in Kant's synthetic a priori judgements: the first being 'apodictically valid or valid for all times,' the second being '*constitutive* of the objects of experience'[14] (Reichenbach 1920, 46–58). For his part, Reichenbach adheres *only to the second* meaning; that the axioms of coordination are *a priori* in the Kantian sense of being constitutive for experience. Accordingly, he thought that we must define axioms of coordination before we can gain empirical knowledge by connecting sense data to points in space–time. This means that we cannot discuss the truth of any proposition that refers to experience without a priori fixation of coordinating principles.

However, Reichenbach dismisses the far-reaching first meaning of a priori, i. e. being necessary and immutable, which was also prominent in the writings of Kant, Fries, and Nelson. While the axioms of coordination do not depend on concrete experience, they do depend on the empirical state of knowledge of their time. Seen against this background, Einstein had good reasons to introduce *other* coordinating principles than Newton and Maxwell. The axioms of coordination can generally be revised according to new evidence, despite their being constitutive for experience. In short, they are not *apodictic* in the Kantian sense (Reichenbach 1920, 53).

With this 'bisection' of Kant's a priori, Reichenbach got rid of the problems that arose from the synthetic a priori in Kant's theory of space and time. He thus realized the remarkable 'gain' of aligning this position with the theories of relativity. However, from a Neo-Kantian (as well as a Neo-Friesian) perspective this gain is offset by a serious 'loss'. Specifically, because of the bisection of the *a priori*, our scientific knowledge can never be demonstrated to be *certain*, i. e. any change of the axioms of coordination will change the conceptual framework of physics and therefore its objects. To put it in 'Reichenbach's nutshell': "Here our view

[14]Reichenbach (1920, 46–58); cf. Klein (2000) for Reichenbach's physical geometry in the context of conventionalism and realism. Moritz Schlick pursued a similar approach and corresponded with Reichenbach on physical geometry. For this discussion as well as the changing concept of science at this stage of logical empiricism see Seck (2008).

differs from that of Kant: While for Kant only the determination of the individual concept is an infinite task, the perspective here is that our concepts of the object of science in general, of reality and its determinability, can only be a matter of subsequent specification" (Reichenbach 1920, 84). Even our best established scientific knowledge consists 'only' in a connection of perceptual experience and conceptual relations. In Nelson's terminology, this means that no comprehensive synthetic principle of 'rational induction' does exist, and all law-like propositions, based on experience, are only statements of probability and thus are, in principle, fallible. This consequence was later summed up by Reichenbach in these words: "There is no certainty at all remaining—all that we know can be maintained with probability only. There is no Archimedian point of absolute certainty left to which to attach our knowledge of the world [. . .]."[15] Ironically enough, this 'Popper-like' statement is directed against the 'absolutism' of positivism, even though it also fits quite well with regard to the work of Nelson who criticised positivism for "destroying not only itself, but also true science" insofar as it intended to eliminate metaphysics (Nelson 1914, 206; cf. 1908) as a warrantor of epistemic certainty. Another irony of the rise of fallibilism, at that time, is that Popper's fallibilism is rooted in his critical analysis of Fries' and Nelson's theory of justification (Popper 1994). By contrast, Nelson and his school considered the epistemological conclusion Reichenbach drew from the theory of relativity unacceptable. During Nelson's lifetime, no one who wanted to remain a member of the New Friesian School crossed this watershed.[16] For them, the acceptance or rejection of Reichenbach's interpretation of Einstein's *Relativitätstheorie und Erkenntnis a priori* was not a mere philosophical subtlety, but a matter of philosophical identity. I think that neither Nelson nor one of the remaining members of his school really faced Einstein's challenge for this reason.

3.6 Epilogue: 'Fries, Who Will Save You from the Friesians?'

After having defended Einstein repeatedly against some orthodox and undeviating Neo-Kantians, Reichenbach wrote despairingly to A. Berliner in April 1921: "Kant, who will save you from the Kantians?" (Hentschel 1990, 507) Along the same line, one might ask: Why did nobody save Fries from the orthodoxy of the Nelson school?

As in history more generally, the course of philosophy and science does not directly correspond to the merits of its protagonists. Indeed, although Fries'

[15] Reichenbach (1938, 192); cf. Reichenbach (1969, 272) about the method of "trial and error" as the only method left for prediction. For Reichenbach's epistemology in the broader context of empiricism see Poser (1998).

[16] This is a conjecture which a more extensive study of the sources would have to corroborate. It is certainly correct for the papers in the *Abhandlungen* of the New Friesian School, though due to the lack of experts in the school from 1920 onwards the subject is widely neglected. Bernays (1933) gives a positive estimation of Einstein's physics (cf. Sect. 3.6), but appeared 6 years after Nelson's death.

philosophy of mathematics eventually gained the appreciation it deserves, via the efforts of Nelson, Hessenberg, Dubislav, Grelling, Bernays and others, in this paper I have tried to show that his original philosophy of empirical science had even more to offer than the membership of the New Friesian School realised. For, instead of developing this philosophy further, according to Fries' constant demand for a close interaction of scientific achievements and philosophical reflection, this group meant only to conserve its mathematical philosophy of nature. They did so by trying to shelter it from theories of relativity, the philosophical impact of which were obviously underestimated. This general attitude was exactly *not* what the Friesian and New Friesian School demanded from scientific philosophy, namely: "Any philosophy that is in accordance with the exact sciences can be true, any one that is in conflict with them is wrong with necessity." Thus, Nelson, in spite of his great achievements with regard to the spreading of Fries' philosophy in general, certainly did not save Fries' mathematical philosophy of nature. In fact, he did Fries—in *this* important respect—a regrettable disservice. For, adherents like Dubislav or Grelling, who might have done better, changed sides, while others like Hessenberg or Rüstow confined their activities to perhaps less controversial subjects like the philosophy of (pure) mathematics, logic or social philosophy. Paul Bernays in 1928—a year after Nelson's death—tried to make good for the omissions of the New Friesian School when he set out the "Basic thoughts of Fries' philosophy in its relation to the current state of science." In light of the immense success of both theories of relativity he demanded a revision of the assumption of Kant and Fries that "geometry and physics are within the frame of our intuitive ideas (*anschauliche Vorstellungen*) of space and time, this being a condition of the possibility of scientific knowledge" (Bernays 1933, 107). Although belated, Bernays indicated that the genuine conception of Fries' philosophy of science actually did offer fruitful connections to the modern development of physics. But his calling for such a revision was not published before 1933 and came far too late to influence the ongoing discussion of relativity.

Fries' mathematical philosophy of nature, combining Kantianism and an early predisposition to conventionalist and fallibilist reasoning and alluding to a relational theory of motion, could have been a stimulating source and a point of systematic orientation for Reichenbach. However, he, too, failed to grasp this opportunity to bridge the gap between Kantian philosophy and scientific development. While Nelson did so because he erroneously believed that scientific philosophy demands the persistence of an orthodox Kantian metaphysics of nature, Reichenbach's conception of scientific philosophy excluded any instruction from *history* to the philosophy of science. Here our story exhibits another (third) irony. For, while Reichenbach refused to pay any attention to history in his scientific philosophy, the idea of a 'relativesed apriori' he introduced in his *Relativitätstheorie und Erkenntnis a priori* gained considerable attention and sympathy in present discussion exactly due to the *historical turn* of philosophy of science, and their confirmation of the *historical relevance* of constitutive principles, and their change in the succession of Thomas Kuhn's *Structure of Revolution.*[17]

Reichenbach could not willingly 'save Fries from the Friesians' because he decided—to Fries', and perhaps also to his, disadvantage—not to study the history of philosophy after Kant. However, his *Relativitätstheorie und Erkenntnis a priori* is a remarkable and fruitful approach, for that time, to bridge the gap between Kantian philosophy and the empirical sciences, by ranging "between transcendental method and the method of analyzing science" (Hecht 1994). In this double sense it is a synthesis in the spirit of Fries' mathematical philosophy of nature. Therefore, the question 'Who saved Fries from the Friesians?' is perhaps best answered by saying: Reichenbach, although unwittingly.

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[17]Cf. Kuhn (1996, esp. 78–80, 144–159). The intimate relation between the present discussion of the 'relativesed apriori' and the history of science is demonstrated in Friedman (2001); for Reichenbach's role see esp. 72–82. For a topical analysis of the subject with strong references to the tradition of pragmatism see Anacker (2012).

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