That which you inherit from your
fathers you must earn in order to
possess. (Goethe, *Faust* I)

Appropriating Kuhn's Philosophical Legacy - Three Attempts: Logical Empiricism, Structuralism, and Neokantianism

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Abstract: In this paper we discuss three examples of the appropriation of Kuhn's ideas in philosophy of science. First we deal with classical logical empiricism. Perhaps somewhat surprisingly, the arch-logical empiricist Carnap considered Kuhn's socio-historical account as a useful complementation, and not as a threat of the philosophy of science of logical empiricism. As a second example we consider the attempt of the so-called structuralist philosophy of science to provide a "rational reconstruction" of Kuhn's approach. Finally, we will deal with Friedman's proposal to apply Kuhn's ideas to the formulation of a modernized, historically enlightened Kantian approach that is based on the concept of a non-apodictic constitutive and historically moving a priori.

Key words: Kuhn, Carnap, Kant, logical empiricism, (neo)kantianism, structuralism, history of science.

1. *Introduction.*

Thomas S. Kuhn was one of the most important intellectual figures of the second half of the 20th century. His opus magnum *The Structure of Scientific Revolutions* turned concepts such as "paradigm", "incommensurability", and "scientific revolution" into household words. Although *Structure* is not particularly difficult to read, it has proved a difficult and immensely controversial book to interpret. The aim of our contribution is not to add another interpretation to the many existing ones, at least not directly. Rather, we want to discuss three interesting appropriations of Kuhn's ideas that have been

brought forward in philosophy of science, and discuss why they failed, or at least, what are the difficulties they have been confronted with.

The outline of our paper is as follows: in the next section we will deal with the early reception of Kuhn's ideas by the late logical empiricism of the 1960s and 1970s. General wisdom has it that Kuhn's work provided the last nail in the coffin of the already moribund logical positivist philosophy of science. Actually, things were more complicated. As Reisch, Grünberg and Irzig, and other scholars have shown that the relation between classical logical empiricism and historicist approaches such as Kuhn's was more complex than the simplistic story according to which Kuhn (or Feyerabend or Popper or somebody else) "killed" Logical positivism (cf. Creath 1995). Indeed, Carnap "very much liked" *Structure* and played an active role in publishing it as the penultimate volume of the series *Foundations of the Unity of Science*. This series was the introductory part of *The Encyclopedia of Unified Science* that might be considered as the official platform of the logical empiricist movement.

The works of Hempel, Frank, and Neurath provide other evidence that the relation between logical empiricist philosophy of science and Kuhn's historicist approach cannot be described simply as a neat and clear-cut opposition. Hempel, for instance, fully in line with Kuhn, insisted that an adequate methodological theory must be informed by a close study of the history, sociology, and psychology of actual scientific research behavior. That Neurath subscribed to a picture of science, in which the role of science in politics and society played a pre-eminent role is hardly new. Similarly, as will be explained in some more detail later, Frank was a partisan of an engaged philosophy of science that emphasized the importance of the social and political aspects of the scientific entreprise. Thus, the real difficulty with the logical empiricist theory of science was not that it had no place for the non-logical aspects of science but that it did not manage to bring the logical and the extra-logical aspects of science together in a way that appealed to the general philosophical public. How the logical and the historical aspects of science studies matched in a logical-empiricist framework, however, was never sufficiently clarified. The logical empiricists failed to convince the general reader that their approach did justice to the logical *and* the social and historical aspects of science. As we want to show in the following, this shortcoming became clearly visible in the confrontation of late logical empiricism and Kuhn's "revolutionary" historical philosophy of science that took place in the 1960s and 1970s.

As our second example we will consider in section 3 a curious attempt to come to terms with the challenge which Kuhn's ideas presented to traditional philosophy of science, to wit, the attempt of Stegmüller and his school to reconstruct the basic concepts of Kuhn's conceptual apparatus in a purely formal manner. Thereby they hoped to enlarge the conceptual arsenal of formal philosophy, and aiming at a "combined theory of the dynamics of theories" that was formally precise *and* socio-historically founded. Somewhat surprisingly, Kuhn endorsed this attempt of formalizing his ideas – at least for some time.

As our third example we will consider in section 4 Friedman's attempt to apply Kuhnian ideas to formulate an historically enlightened modernized Kantian philosophy of science that is based on the concept of a non-apodictic constitutive moving a priori. As has been pointed out by several authors, there are Kantian and neo-Kantian features in Kuhn's thought. Late in his career, Kuhn himself cryptically characterized his approach "as a sort of post-Darwinian Kantianism" (Kuhn 1991, 12). On the other hand, scholars of history of philosophy of science have been engaged in excavating traces of a Neo-kantian legacy in Carnap's philosophy of science. Hence, it might not be too surprising that Carnap, Kuhn, and Kant have more in common than meets the eye.

The appropriations of Kuhn's ideas by late logical empiricism and structuralism, are historically naïve in the sense that they took the event "Kuhn" as something that just had happened and to which they had to react in some way or other: late logical empiricism bet on a peaceful coexistence between the received logically oriented philosophy of science, while the structuralists attempted to provide a "rational reconstruction" of Kuhn's socio-historical approach.

As time went on and *Structure* became a classic of the "new" historically oriented philosophy of science, the "Kuhnian revolution" itself became the study object of history of philosophy of science. In this vein, Coffa embedded the accounts of Kuhn and Carnap into a "semantic" tradition whose roots he traced back to Kant:

> Wittgenstein's domain of showing, his later grammar, Carnap's syntax, Sellars's categorial frameworks, and Kuhn's paradigms are some well-known members of the continuing series of attempts to find the right way of looking at that peculiar kind of knowledge that seems necessary and not vacuous, yet at the same time does not state any factual claim. Poincaré may have officially started the search when he observed that geometric axioms present themselves "in disguise", pretending to be claims but really being something else; … by calling them definitions, he clearly intended to assign to them a role in the determination of meaning. (Coffa 1991, 138).

Going further in this direction, Friedman argued that Kuhn's historicism might be used for the formulation of a modernized (neo)kantian philosophy of science based on the concept of a historically moving non-apodictic constitutive a priori (Friedman 2003).

In his beginnings, Kuhn himself was not very much interested in elucidating his place in history of philosophy. Although he was prepared to acknowledge some precursors such as Ludwik Fleck or Alexandre Koyré, he did not care much to conceive himself as member of some philosophical tradition. Only later he came to acknowledge the positive role of philosophy for the growing insight into the importance of historical considerations in philosophy of science:

> The concept of historical reconstruction that underlies *The Structure of Scientific Revolutions* ... is by no means original: I owe it primarily to Alexandre Koyré; its ultimate sources lie in Neokantian philosophy (Kuhn, 1987, 361).

Although Kuhn mentions Lange and Cassirer as Neokantian sources of his work it remains unclear, what he really understood by "Neokantianism" since he subsumed under this heading authors such as Brunschvicg and Meyerson who can be characterized as Neokantians only by stretching this category considerably (cf. Friedman 2003, 29). As we will argue in some detail later, calling Kuhn a Neokantian is rather ambiguous. Be this as it may, Kuhn and Neokantians such as Cassirer shared the conviction that history played a crucial role for a philosophical understanding of science. Hence, Kuhn was not the first to have pleaded for such a role for history of science, but certainly his plea has made the most impression in the second half of 20th century.

In section 5 we deal with Kuhn's legacy in the history of philosophy of science in the last decades of the 20th century. Recently, Hardcastle and Richardson gave Kuhn's appeal for a role for history a new twist to overcome the present stagnation of philosophy of science. According to them, philosophy of science, after more than forty years after *Structure* was published, has reached a phase of stagnation and degeneration. In order to overcome this impass they propose to adapt Kuhn's famous principle to philosophy of science in the following way:

> History, if viewed as a repository for more than anecdote or chronology, could produce a decisive transformation in the image of philosophy of science by which we now are possessed. (Hardcastle and Richardson 2003, vii, Kuhn 1962, 1).

In other words, they propose to tap the sources of history of philosophy of science to overcome the deadlock in which they see philosophy of science presently trapped. More

precisely, they propose to revisit the social and political commitment of the original logical empiricism of the Vienna Circle hoping that this might lead to a rejuvenation of contemporary post-Kuhnian philosophy of science.

In this way, Kuhn's legacy may influence in an indirect way not only our picture of science but also that of philosophy of science, and more generally, that of philosophy as a whole. Of course, it is no news at all, that the history of philosophy is an important ingredient for philosophy itself, but it is somewhat ironic that philosophy of science, as the allegedly on scientific part of philosophy that is left after all non-scientific metaphysical influences have been eliminated, turned out to depend on its history as well.

Before we start with the real thing let us note that not everybody considered Kuhn's work as something worth of being appropriated in some way or other. For some, Kuhn's philosophy of science is a real threat for occidental science and rationality as a whole. A particular wild example of this stance is provided by Scheffler's early criticism of *Structure*:

> The current attacks (of Kuhn, A.I. and T.M.) challenge the very opposition between science and speculative idealism, from which scientifically minded philosophies have sprung. The attacks threaten further the underlying moral motivation of these philosophies, their upholding of the ideal of responsibility in the sphere of belief as against willfullness, authoritarianism, and inertia. The issues are fundamental, indeed more fundamental than is generally realized, precisely because a powerful moral vision has implicitly been called into question. (Scheffler 1967, 7 – 8).

Over the years the number of such fervent anti-Kuhnian eruptions has somewhat diminished, but still today there are some who assess Kuhn's "achievements" in a quite similar way as Scheffler did; see for instance Fuller's anti-Kuhnian treatises *Thomas Kuhn. A Philosophical History for Our Time* (Fuller 2000) and *Kuhn vs. Popper. The Struggle for the Soul of Science* (Fuller 2004) where Kuhn, among other niceties, is accused of jeopardizing rational choice in science, crippling critical discourse in and about science, advocating "Big science" against "citizen science" etc. In a less shrill voice this is echoed by neo-Popperians such as Gattei who rehearses Scheffler's polemics from some forty years ago accusing Kuhn that his philosophy

> drastically impoverishes the reasons, aim, and scope of philosophical critique. Confrontation is often banned, criticism discouraged. Philosophical and scientific values of truth and rationality are replaced with commitment to the dominant tradition and consensus within a community of experts. (Gattei 2008, 135).

In contrast, in the present paper we take a broadly positive attitude towards Kuhn's work in that we believe that his philosophical ideas should be considered as fruitful for philosophy of science and related disciplines. Kuhn has the undisputable merit of havingvigorously reminded philosophy once again of the indispensable role of history for understanding science. In this endeavour he was not the first and perhaps his philosophical understanding of history was not the most sophisticated one, but the impact of his ideas on 20th century philosophy of science was certainly greater than that of anybody else.

2. *Logical Empiricism.*

One might think that the logical empiricists of the 1960s and 1970s did not find much in Kuhn's work that deserved to be appropriated in some way or other. Rather, they would regard Kuhn as the "arch-enemy" of any kind of logical and rational epistemology and philosophy of science. A good example for this sometimes almost hysterical reaction to Kuhn as a dangerous "irrationalist" is Scheffler's already mentioned contention that Kuhn's picture of science was that of an irresponsible idealist that intended to level out any difference between rational science and arbitrary speculation. This reaction was not, however, that of the leading figures of late logical positivism or empiricism such as Carnap, Hempel or Feigl - quite the contrary. They were at pains to see the relation between the logical-empiricist and Kuhn's socio-historically oriented perspective on science as a relation of peaceful co-existence and mutually fruitful division of labour and collaboration.

In the beginning, everything worked smoothly between Kuhn and the logical-empiricist establishment, in particular with Carnap. As Reisch has pointed out some years ago, *Structure* was originally commissioned as a monograph for the series *Foundations of the Unity of Science* (cf. Reisch 1991). This was a collection issued to introduce the famous *International Encyclopedia of Unified Science*, originally the brain-child of Neurath and, after his death in 1945, edited by Carnap and Morris. On the occasion of publishing *Structure* Carnap as editor wrote some letters to Kuhn, from which we quote the relevant passages:

> Thank you very much for sending me your manuscripts. … I believe that the planned monograph will be a valuable contribution to the *Encyclopedia*. I am

> myself very much interested in the problems, which you intend to deal with, even though my knowledge of the history of science is rather fragmentary. Among many other items I liked your emphasis on the new conceptual frameworks which are proposed in revolutions in science, and, on their basis, the posing of new questions, not only answers to old problems. …
> I am convinced that your ideas will be very stimulating for all those who are interested in the nature of scientific theories and especially the causes and forms of their changes. (after Reisch 1991, 266, 267).

There is no evidence that Carnap was not serious in his assessment. On the contrary: in private communication with Morris he characterized *Structure* "as a really fine piece of work". Carnap did not understand *Structure* as the death-knell for logical empiricism. Whether he was right or wrong in this is a matter of dispute, but certainly he did not predict the impact that *Structure* was to have on the intellectual landscape of late 20th century philosophy of science and beyond.

Indeed, it is not difficult to see why Carnap might have considered Kuhn's approach as congenial to his own. Due to the work of Reisch, Grünberg and Irzik, Earman and many others one may even set up a kind of dictionary between Carnap and Kuhn. Carnap's linguistic frameworks correspond to Kuhn's paradigms, and Kuhn's "normal science" which he described as puzzle-solving in a given paradigmatic framework has its counterpart in Carnap's concept of answering question or proving theorems inside a given linguistic framework. Finally, the replacement of one linguistic or ontological framework by another one corresponds to a scientific revolution in which one paradigm is substituted by another one. To put it in a nutshell, Kuhn's approach based on the notion of paradigm may be considered as an informal version of Carnap's, or, to put it the other way round, Carnap's linguistic frameworks may be seen a logicized counterparts of Kuhn's paradigms.

These analogies evidence that Carnap's positive reaction toward *Structure* and its publication in the *Foundations* of *Encyclopedia of Unified Science* was founded in some general assumptions of his philosophical perspective. For him, Kuhn's socio-historical perspective on science *complemented* the (better known) logical approach of logical empiricism. Already in 1934, when the logical empiricist movement had reached its peak in Vienna, Carnap had recognised as perfectly legitimate, "in addition to the logic of science ... also the empirical investigation of scientific activity, such as historical, sociological, and, above all, psychological inquiries." (Carnap 1934 (1937), 279). As Carnap pointed out, both kinds of investigations may be grouped together under the

heading “theory of science”. Some years later, in the *Encyclopedia*, we find the following extension of this brief remark:

> The task of science may be approached from various angles. ... We may, for instance, think of an investigation of scientific *activity*. ... [The] investigations of scientific activity may be called history, psychology, sociology, and methodology of science. The subject matter of such studies is science as a body of actions carried out by certain persons under certain circumstances. Theory of science in this sense will be dealt with at various other places in the *Encylopedia;* it is certainly an essential part of the foundation of science. (Carnap 1938, 42 – 43).

Thus, strictly speaking, it is not correct to accuse the logical empiricists as being obsessed by an overly narrow, purely logical perspective on science. Of course, Carnap, for his part, worked only on the logic of science, since

> unfortunately a division of labor is necessary, and therefore I am compelled to leave the detailed work in this direction to philosophically interested sociologists and sociologically trained philosophers. (Carnap 1963, 868).

In other words, contrary to general wisdom, Carnap’s philosophy of science cannot be characterized bluntly as one that admitted only the logical way as the legitimate way of doing theory of science; for him, theory of science had much greater spectrum, comprising the various kinds of empirical science studies as well. Whether Carnap’s pluralist conception of a comprehensive theory of science was actually viable, is, of course, a quite different question. Recently Thomas Uebel dubbed Carnap’s envisaged bipartite approach, which, according to him, was also favoured by Frank and Neurath, the “bipartite metatheory” of the Vienna Circle’s logical empiricism. The bipartite metatheory was designed to be the successor discipline of traditional metaphysically contaminated epistemology and philosophy of science. This „bipartite metatheory“ was said to have two components:

(1) a logical component in the sense of a Carnapian logic of science;

(2) an empirical part roughly in the sense of Neurath’s „behavioristics of scholars“ or Frank’s „pragmatics of science“.

Uebel argues that this bipartite theory should be considered as the common legacy of the three great figures of the Vienna Circle, to wit, Carnap, Neurath, and Frank. Indeed, it is not difficult to find in Neurath’s work a strong commitment to the social, political, and historical aspects of science (see for instance Nemeth 2007). In a similar vein, one can find in Frank’s *Modern Science and Its Philosophy* (Frank 1949) an explicit call

upon logical empiricists to engage in the task of formulating a comprehensive philosophy of science that takes into account logical *and* historical aspects of science:

> We have to complement our logico-empirical analysis – where "empirical" implies individual experiences – by "historical analysis", which is empirical not for the individual but for the human race. The history of science is the workshop of the philosophy of science. We have to teach the student all the relevant principles that have been set up in the course of history. … In this way the logico-empirical analysis gains life and color and becomes a living link between science and the evolution of the human race. (Frank 1949, 278).

Despite this and similar announcements, after WWII neither Carnap nor Frank made greater efforts to make visible the allegedly comprehensive nature of logical empiricist of philosophy of science to a general philosophical public. Scientists, historians or philosophers, who did not belong to the logical empiricism's inner circle, simply were not aware of the existence of the „bipartite metatheory" that was to comprise logic *and* pragmatic of science. To put it bluntly, people who were engaged in coming to terms with Carnap's logical and mathematical subtleties in inductive logic, say, did not interpret them as only one half of the story that logical empiricism had to tell about science. This situation for logical empiricism was aggravated by the fact that after the war, due to his death in 1945, Neurath's workdisappeared from the radar of the general public almost completely, and, for several reasons not to be discussed here, Frank's star was fading since the late 1950s (cf. Reisch 2005). Thus the mere coexistence of a strong logical component and a much lesser impressive component that dealt with non-logical aspects of science together with the abstract thesis that logic of science and pragmatics of science did complement each other, hardly justified the thesis that there was a logico-empiricist *theory* that comprised the logic and pragmatics as two parts. After all, a theory should have a certain amount of unity, which the bipartite metatheory clearly was lacking. The two halves of the envisaged bipartite theory neither shared a common methodology nor a common object.

Carnap might have been aware of this problematic situation. He might have hoped to use Kuhn's work as a substitute for the underdeveloped non-logical half of the bipartite theory that he himself could not take care of, and whose realization had been neglected also by his fellow empiricists. Evidently, *Structure* had more appeal to a general public than the obscure work of an elderly European professor of physics such as Frank, who, after all, was not an expert in matters of history of science. From this point of view, Carnap's positive attitude to Kuhn's work may be interpreted neither as mere politeness

nor as a blunt misunderstanding but as a clever move to mobilize some fresh forces from outside for a relaunch of a rejuvenated comprehensive logico-empiricist philosophy of science. If this manoeuvre had worked, it would have resulted in a peaceful and harmonious collaboration based on a neat division of labour between the two branches of a comprehensive theory of science, to wit, philosophy of science alias logic of science, and the ensemble of empirical studies of science such as history of science, sociology of science, psychology of science and possibly other disciplines.

This project turned out to be a complete failure: Instead of leading to a balanced comprehensive logico-empiricist theory of science, the traditional logic-oriented philosophy of science soon was considered as obsolete, and for the general public logical empiricism was dead as a dodo.

In the following decades, *The Structure of Scientific Revolutions* was interpreted in innumerable ways, but certainly never as belonging to the *Foundations* part of the *International Encyclopedia of Unified Science*. Although Kuhn's *opus magnum* became the greatest bestseller in philosophy of science ever, it was never seen as an integral part of the logico-empiricist *Encyclopedia.* In particular, it never threw any light on the other parts of the *Foundations* which remained in the darkness. Indeed, it seems fair to say that most other contributions to the *Foundations* left no trace at all - neither inside nor outside the camp of logical empiricists.

This fact should cause a certain discontent among the partisans of an alleged "alliance" between Kuhn and Carnap. The philosophical masses did not consider the historical approach of *Structure* as compatible with, to say nothing of being an integral part of, logical empiricist philosophy of science. Even Carnap himself, in his later writings after the publication of *Structure* in 1962, never mentioned *Structure* – not even in his semi-popular non-technical *Philosophical Foundations of Physics* (Carnap 1966) where mentioning *Structure* would have been quite appropriate - if one considers *Structure* as relevant for philosophy at all. Apparently, Carnap did not. For him, *Structure* remained a "nice piece" of history of science.

This fact has been seldom noticed in the growing literature on the alleged similarity or alliance between the views of Carnap and Kuhn. An exception is the recent paper of Pinto de Oliveira (Pinto de Oliveira 2007). He considers as the cause of this omission Carnap's strict separation between history of science and philosophy of science:

> Carnap certainly considered *Structure* as a (nice) work in the *history of science* … for which, as such, the *Encyclopedia* had already reserved space in advance. This would explain, at the same time, the warm reception of Kuhn's book in the letters and Carnap's total neglect of it in his works in *philosophy of science.* (Pinto de Oliveira 2007, 155).

According to Pinto de Oliveira, this somewhat schizophrenic reaction to *Structure* can be explained by the fact that Carnap strictly distinguished between history of science and logic of science due to his adherence to the strict distinction between the context of discovery and the context of justification. In other words, for him, *logic of science* and *pragmatics of science* are two different worlds that do not influence each other. The "bipartite metatheory" was nothing but a formal iuxtaposition of two unrelated components.

Frank's problems with the bipartite metatheory point in the opposite direction. In his work there is virtually no trace of the other component of the bipartite metatheory, to wit, logic of science in Carnap's sense, except some laudatory remarks that do no argumentative work at all. For a general public, in search of a comprehensive picture of science, this was not a very appealing standpoint: in Carnap's work there was little or nothing that could be interpreted as being even remotely related with the historical, sociological, or psychological aspects of science; in Neurath's and Frank's works, on the other hand, the reader found something to quench his thirst for historical, sociological and political aspects of science, information about historical, but not much dealing with the role of logic in science. In Frank's *Modern Science and its Philosophy* (Frank 1949b) which one may consider as his philosophical *summa*, there is hardly any trace of a sophisticated logical perspective on science. The standard explanation for this omission is that books like Frank's were written for a general public, and logic was not an appropiate topic for such an audience. Even if one admits that pursuing the bipartite strategy had made sense under certain circumstances, in the 1960s it had, as we shall see in more detail in section 5, fatal consequences for logical empiricism.

3. *Structuralism.*

As Scheffler's furious assault showed not all philosophers of science shared Carnap's conciliatory stance toward the new historical currents in philosophy of science. A case in question was the Austrian-German philosopher Wolfgang Stegmüller and his school. Stegmüller construed the event "Kuhn" as a dramatic shock for the community of

philosophers of science. According to him, Kuhn's idea of science filled most philosophers of science with "speechless horror" (Stegmüller 1974, 167). But, according to Stegmüller, the situation was not completely hopeless. Sneed's novel "structuralist conception of theories" was the appropriate framework to overcome the apparent schism between logical or formal accounts of science and socio-historical ones such as Kuhn's. According to Stegmüller, Kuhn was right in almost all points, nevertheless one needed not fear that science fall prey to irrationalism. Rather, among the many virtues of the structuralist metatheory - inaugurated by Sneed and further developed by the Stegmüller school – was the capacity to provide philosophy of science with the conceptual means for a "rational reconstruction" of Kuhn's socio-historical approach. In other words, the structuralist philosophy of science offered a comprehensive synthesis of the formal *and* the socio-historical perspectives on science. For some time, Kuhn himself was impressed by the structuralist appropriation of his ideas. In 1976 he confidently asserted:

> First, though still at an early stage of its development, the new formalism makes important new territory accessible to analytic philosophy of science. Second, though sketched with a pen I can still scarcely hold, preliminary charts of the new terrain display remarkable resemblance to a map I had previously sketched from scattered travellers' reports brought back by itinerant historians of science.
> …..
> If only simpler and more palatable ways of representing the essentials of Sneed's position can be found, philosophers, practitioners, and historians of science may, for the first time in years, find fruitful channels for interdisciplinary communications. (Kuhn 1976, 179, 181).

Later, Kuhn's enthusiasm for the new formal apparatus faded. In any case, his hope that the structuralist jargon might become something like a *lingua franca* for scientists, philosophers and historians in the trading zone between science, philosophy, and history, never came true . .

The basic conceptual tools for the structuralists's appropriation of Kuhn were their novel set-theoretical or model-theoretical concepts of "theory", "theory-nets", "holding a theory", "the empirical claim of a theory" and various "intertheoretical relations" (cf. Stegmüller (1979), Balzer, Moulines, Sneed (1987), Balzer and Moulines (1996)) that, according to Stegmüller, could be employed "to make rationally intelligible those historico-sociological aspects of scientific change that Kuhn had placed into the foreground". To give the reader a taste of what the structuralist "rational reconstruction" of Kuhn was all about let us briefly recall its most important concepts. For reasons of space and time,

we have to assume some basic knowledge of the structuralist vocabulary. Thus we assume to be known what is to be understood by a structuralist "theory element" $T = \langle K, I \rangle = \langle\langle M_p, M_{pp}, M, C\rangle, I\rangle$ (see for instance Stegmüller (1979, D4) or elsewhere). A theory-element is the smallest meaningful unit of the structuralist philosophy of science, intuitively, it may be considered as a kind of mini-theory consisting of a "theoretical core" K and a "domain of intended applications" I. Theory-elements are used to construe more complex structures such as theory-nets. After these preparatory remarks we can embark on the task of rationally reconstructing Kuhn via the following series of definitions:

(3.1) *Definition*. X is a *pragmatically enriched theory-element* iff there exist T, SC, h, and F such that

1) $X = \langle T, SC, h, F\rangle$;
2) T is a theory-element;
3) SC is a scientific community;
4) h is an historical interval;
5) $F \subseteq I$.♦

Remark. The concepts SC of a scientific community and h of a historical interval are taken as primitive. Apparently SC is to be understood as a set of scientists, and h a temporal interval in the usual sense. This may sound rather trivial, but at least it allows us to express assertions such as "SC_1 is a part of the community of SC_2", or "the historical interval h_1 comprises the historical interval h_2" in the set-theoretical jargon of structuralism. As has been mentioned already above, "real theories" are more complex structures than theory-elements. The following definition is to make this idea precise.

(3.2) *Definition*. $X = \langle |\mathrm{N}|, \leq \rangle$ is a *pragmatically enriched theory-net* (p.e net) iff

1) $|\mathrm{N}|$ is a finite set of pragmatically enriched theory-elements;
2) $\forall \langle T, SC, h, F\rangle, \langle T', SC', h', F'\rangle \in |\mathrm{N}|$ ($SC = SC'$ and $h = h'$)
3) $\leq \subseteq |\mathrm{N}| \times |\mathrm{N}|$;
4) $\forall \langle T, SC, h, F\rangle \leq \langle T', SC', h', F'\rangle \Leftrightarrow T$ is a specialization of T'.

The elements of F are those intended applications that are acknowledged by all members of SC, they are called *firm* applications.♦

Remark. A "specialization" is a so-called "intertheoretic relation" between theory-elements. A theory-element $T' = \langle K', I' \rangle$ is a specialization of $T = \langle K, I \rangle$ iff the components of T' are subsets of the corresponding components of T. The intuitive idea is that T' is more special than T, or, that T is more general than T'.

(3.3) *Definition*. Let N, N^* be two different theory-nets. N is supplantable incommensurably by N^* iff there is a relation $s \subseteq M_p^* \times M_p$ and $s_{pp} \subseteq M_{pp}^* \times M_{pp}$ and a non-empty set I_a such that:

(1) s is not effectively calculable.

(2) s_{pp} is a function, is effectively calculable and $s_{pp}(M^*_{pp}) = M_{pp}$.

(3) There do not exist n non-empty sets $M_1^*, \ldots, M_n^* \subseteq M_0^*$ such that $s(M_1^*), \ldots, s(M_n^*) \subseteq M_0$.

(4) (i) $I_a \subseteq I_0 \cap r_{pp}(I_0^*)$.

(ii) for each $y \in I_a$ ($y \in r(M_0^*)$ and $y \notin r(M_0)$).♦

Remark. Considering N and N^* as rational reconstructions of theories T and T^* in an informal sense, the intention of (3.3) is to give a formally precise explication Kuhn's notion of incommensurability: there is a "translation" s_{pp} between the data (applications) of T and T^*, but this translation cannot be extended to a translation between the full conceptual kernels of T and T^*.

(3.4) *Definition*. Let H be a non-empty finite set and $< \subseteq H \times H$. Then

(1) $\langle H, < \rangle$ is an historical order iff for all $x, y, z \in H$: $\neg x < x$; $x < y$ and $y < z$ $\rightarrow x < y$; $x < z$ or $y < x$.

(2) If $\langle H, < \rangle$ is an historical order then

$(2)_1$ $\min(H)$:= the unique $h \in H$ such that for all $y \in H (h < y$ or $h = y)$.

$(2)_2$ for $h \in H$, $h \neq \min(H)$, $h - 1$:= the unique $h' \in H$ such that $h' < h$ and $\neg \exists x \in H\ (h' < x$ and $x < h)$

(3) If X is a p.e. net then $h(X)$:= the unique h such that

$\forall$ <T, SC, h', F> $\in$ |N|(h' = h)

(4) If X is a p.e. net then

$(4)_1$ $SC(X)$:= the unique SC such that $\forall$<T, SC, h', F> $\in$ |N|(SC' = SC);

$(4)_2$ $F(X)$:= $\cup${F; $\forall T$, SC, h(<T, SC, h, F> $\in$ |N|)};

$(4)_3$ $A(X)$:= $\cup${I-F; $\forall K$, SC, h(<<K, I>, SC, h, F> $\in$ |N|)};

$(4)_4$ If **N** is a set of p.e. nets then H(**N**) := {h(X); $X \in$ **N**}.♦

(3.5) *Definition*. E is a theory-evolution iff there exist N, < such that

(1) E = (**N**, <);

(2) **N** is a finite net of p.e. nets;

(3) <**H**(**N**), < > is an historical order;

(4) $\forall X \in$ **N** $\forall$<T, SC, h, F> $\in$ |N|) [$h \neq \min(H(\mathbf{N})) \rightarrow \exists X' \in$ **N** (h(X') = h -1, $\exists$ <T', SC', h', F' > $\in$ |N'|, whereby T is a specialization of T'].♦

Remark. The concept of a theory-evolution may be characterized as a third-order concept in the hierarchy of structuralist concepts: on the first level there are theory-elements, on the second nets of theory-elements, and on the third nets of nets of theory-elements. The concept of theory-evolution intends to capture the development of theories over time.

(3.6) *Definition*. If E = <N, < > is a theory-evolution then X is a paradigm for E iff there exist K_0, I_0, such that

1) X = < K_0, I_0> is a theory-element;

2) $\forall Y \in$ **N** $\forall$<K, I, SC, h, F> $\in$ |N| such that

$(2)_1$ K is a core specialization of K_0;

$(2)_2$ $\forall x \in I \exists y \in I_0$ ($y \subseteq x$ and SC acknowledges y to be a paradigm subset of x.

NB: |N| is the first member of Y.♦

And at long last we finally arrive at the crowning achievement of the structuralist "rational reconstruction" of the key concept of Kuhn's philosophical legacy, to wit, a "Kuhnian theory-evolution":

(3.7) *Definition*. *E* is a Kuhnian theory-evolution iff

1) *E* is a theory-evolution;

2) $\exists < K_0, I_0 >$ ($< K_0, I_0 >$ is a paradigm for *E*). ♦

If we understand the structuralists's intentions correctly they interpret Kuhn as contending that the history of science may be described in terms of "Kuhnian theory-evolutions". A "set-theoretical reconstruction" or "rational reconstruction" of Kuhn's thesis is accomplished by definition (3.7). This is the structuralists's appropriation of Kuhn's legacy.

So what, the innocent reader may ask. Do we get a better philosophical understanding of what is conceptual change in the sense of Kuhn? Do we gain new insights in the conceptual subtleties of the various versions of the concept of incommensurability so many philosophers of science have struggled with? The structuralist philosopher of science Werner Diederich, after having presented a battery of similar definitions in his contribution to the anthology *Structuralist Theory of Science. Focal Issues, New Results* (Balzer and Moulines 1996) admitted:

> A suspicious reader might have gotten the impression that all these pragmatic and diachronic notions are somewhat sterile. In fact there are only very few theorems which have been derived from these definitions. However, the main spin-off should not be seen in an elaborated metatheory of the development of science, but in concrete reconstructions, which become possible by applying the structuralistically sharpened tools to actual scientific episodes. Such reconstructions have successfully been carried out by Moulines for classical particle mechanics and simple equilibrium thermodynamics. (Diederich 1996, 80).

We doubt that the application of the "structuralistically sharpened tools to actual scientific episodes" have persuaded many readers. Rather, we are inclined to invoke Aristotle's observation in *Nicomachian Ethics*:

> It is the mark of an educated man to look for precision in each class of things just as so far as the nature of the subject admits; it is evidently foolish to accept probable reasoning from a mathematician and to demand from a rhetorician scientific proofs. (Aristotle 1940, *Nicomachian Ethics*, Book I.3).

Others may object that falling back on Aristotle's wisdom betrays too strong an adherence to informal common-sensical categories. A modern mind should trust in the wonders of modern scientific reconceptualizations. After all, the achievements of modern science go well beyond anything that Aristotle could have imagined even in his boldest dreams. Hence, in order to assess the chances of the structuralist reconstruction

of Kuhn's historicist philosophy of science it may be more appropriate to rely on more scientific criteria than Aristotelian common sense. Taking into account the issue we are dealing with, it might be expedient to rely on some recommendations of Carnap himself:

> The acceptance or rejection of abstract linguistic forms … will be finally decided by their efficiency as instruments, the ratio of the results achieved to the amount and complexity of the efforts required. … Let us learn from the lessons of history. Let us grant to those who work in any special field of investigation the freedom to use any form of expression which seems useful to them; the work in the field will sooner or later lead to the elimination of those forms which have no useful function. (Carnap 1950, 221).

At the end of the day, for Carnap, as well as for Kuhn, a language form, a theory, or a paradigm, was an instrument for solving problems. In his reply to Strawson Carnap quite clearly expressed this instrumentalist stance as follows:

> A natural language is like a crude, primitive pocket-knife, very useful for a hundred different purposes. But for certain special purposes, special tools are more efficient, e.g., chisels, cutting-machines, and finally the microtome. ... Would anyone criticize the bacteriologist for using a microtome, and assert that he is evading the problem of correctly using a pocket-knife?
>
> The choice of a method for the solution of a given philosophical problem should be decided in each case by practical considerations. (Carnap 1963, 938, 939).

For the sake of the argument let us grant Carnap this „pragmatist" conception of philosophical problems and let us agree with him, against Strawson, that there is no reason why for the solution of philosophical problems one has to rely on the resources of ordinary language. Even then someone who tried to use a microtome for slicing bread would be considered as a bit strange, to put it mildly.

4. *Neokantianism.*

In this section we want to discuss Michael Friedman's attempt to apply some of the ideas of *Structure* and related work ("the best current historiography of science" according to Friedman) to the task of formulating a comprehensive account of philosophy of science which describes the dynamics of (scientific) reason in an exact manner and, at the same time, does justice to its social and historical complexities. We propose to call Friedman's approach "Neokantianism". This may not be the most accurate name,

clearly "Neoneokantianism" would be more appropriate, but certainly this would be a rather ugly name, so we stick to to "Neokantianism".

Friedman has presented his approach in a variety of publications, in particular in his 1999 Kant Lectures at Stanford University *Dynamics of Reason* (Friedman 2001), but also in papers such as (Friedman 2002, 2003) and elsewhere. In this paper we cannot offer a comprehensive survey of his "Neokantian" approach, rather, we will concentrate on that part of Friedman's Neokantianism where the appropriation of Kuhnian ideas plays an important role.

The starting point of Friedman's appropriation of Kuhnian ideas is the thesis that Kuhn's "paradigm" and Carnap's "linguistic framework" share important similarities (see section 2). Going beyond this general thesis Friedman contends that these similarities are best understood from a Kantian perspective in that both Kuhn's paradigm and Carnap's linguistic framework may be seen as contemporary versions of what in Kantian terms was called a constitutive a priori of scientific knowledge. In contrast to Kant's a priori, however, the modern versions of the a priori are not apodictic and fixed but may change over time. In Kuhnian terms, a change of the a priori is just a scientific revolution, Carnap, in less dramatic terms, calls it a change of language or a change of a linguistic framework. Thus Kuhn's central distinction between revolutionary science and normal science has a close parallel in Carnap's account. Just as for Kuhn, considerations that concern paradigmatic changes do not proceed in accordance with generally agreed upon rules as in normal science, for Carnap questions concerning which linguistic framework to adopt are external questions that cannot be answered by using the rules of standard logic but that require less than absolutely compelling "pragmatic" considerations about which Carnap had not much to say.

Friedman proposes to use the Kuhn/Carnap distinction between two essentially different phases or periods in the ongoing process of the scientific enterprise as an argument against some current conceptions of how science and philosophy are related. First, he argues against Quine's holism according to which philosophy is to be conceived as being on the same level as science itself: philosophy is just science concerned with very general issues; secondly, Friedman argues against Carnap's proposal to conceive philosophy as a special science, namely *Wissenschaftslogik* dealing with the logical structure of the language of science (cf. Friedman 2001, 19). According to Friedman Kuhn's description of the history of the empirical sciences as a series of longer periods

of "normal science" evidences that philosophy should neither be conceived as a branch of cognitive psychology, as Quine contended, nor as a branch of logic or mathematics, as Carnap maintained. The main reason that philosophy is not an "ordinary" science in Kuhn's sense is that its history does not exhibit the Kuhnian pattern of paradigmatic normal periods interrupted by short revolutionary periods. The historical evolution of philosophy (and other humanities as well) lacks such relatively quiet paradigmatic periods:

> In philosophy the most we ever achieve is temporary consensus on which figures or doctrines set the philosophical agenda – for the moment, that is, and within a relatively circumscribed setting. (Friedman 2001, 20).

This should, however, not be considered as a defect. Rather, the difference between science and philosophy is essential for the evolution of our scientific culture as a whole:

> Science, if it is to continue to progress through revolutions, […] needs a source of new ideas, alternative programs, and expanded possibilities that is not itself scientific in the same sense – that does not as do the sciences themselves, operate within a generally agree upon framework of taken for granted rules. For what is needed here is precisely the creation and stimulation of new frameworks or paradigms … capable of motivating and sustaining the revolutionary transition to a new first-level or scientific paradigm. (ibid., 23).

In other words, Friedman reserves a place for philosophy in the unending struggle for coping with the incommensurability of subsequent paradigms. The task of philosophy is to provide conceptual tools for sustaining the communication between partisans of different paradigms, even if this communication does not meet strict standards of scientific reasoning. Long ago, already Russell had proposed a similar conception of philosophy as a kind of an adventurous complement to science:

> Just as there are families in America who from the time of the Pilgrim Fathers onward had always migrated westward, toward the backwoods, because they did not like civilized life, so the philosopher has an adventurous disposition and likes to dwell in the region where there are still uncertainties. (Russell 1918(1994), 281).

As Nozick pointed out, philosophers too often have been concerned with the task of arguing that this and that must be a certain way, and they made it their business to close off possibilities (cf. Nozick 2002, 8). The new way of doing philosophy, as proposed by Russell, Nozick, and Friedman, emphasizes, on the contrary, that philosophy should open new conceptual possibilities that may be scientifically not accessible. Thereby,

philosophy and science make fundamentally distinct, yet mutually complementary contributions to the total ongoing dialectic of human knowledge. In line with these complementary roles of philosophy and science Friedman proposes a stratified system of knowledge that can be analyzed into three main strata or levels (cf. Friedman (2001, 45- 46):

(1) First level: Concepts and principles of empirical natural science such as the Newtonian law of universal gravitation or Einstein's equations for the gravitational field.
(2) Second level: Constitutive a priori principles such as the basic principles of geometry and mechanics. Kuhn calls these relativized a priori principles paradigms.
(3) Third level: Philosophical meta-paradigms or meta-frameworks, that serve as sources for suggestions, guidance, and orientation when an old paradigm seems to be in need to be replaced by a new one.

As Friedman points out, his approach bears close similarity with Cassirer's Neokantian approach presented 100 years ago in his *Substance and Function* (Cassirer 1910(1953)) (cf. Friedman 2001, 65ff). We cannot go into the details of the affinity of Cassirer and Friedman here, but we'd like to direct the reader's attention to the fact that there is a fundamental ambiguity in the term „neokantianism". On the one hand, it is used for any philosophical current that has appropriated some more a less important part of Kant's philosophy. On the other hand, „neokantianism" may be understood in a narrower sense, referring to the Neokantianism that flourished in Germany at the end of the 19th and at the beginnings of the 20th century, in particular to the Southwest school of Windelband and Rickert, and the Marburg School whose most important member for analytical philosophy seems to be Cassirer. The point is that the Marburg school, and in particular Cassirer subscribed to a „genetic epistemology" that conceived the evolution of scientific knowledge in much more continuous way (approved by Friedman) than Kuhn's conception with its emphasis on scientific revolution would admit.
Friedman wants to have both worlds: he wants to incorporate Kuhn's neokantianism that subscribes to the paradigm as a modern form of a relativized a priori, and he wants to incorporate Cassirer's neokantianism that endows a genetic epistemology of the Marburg brand of German Neokantianism. These two versions of neokantianism stand

in a certain opposition to each other which is most clearly evidenced by the following quotes:

> No single astronomical system, the Copernican as little as the Ptolemaic, can be taken as the expression of the „true" order, but only the whole of these systems as they unfold continuously according to a definite connection. (Cassirer 1910(1953), 322).

On the other hand, one has Kuhn's famous (or notorious) „world-change"-announcements (cf. Horwich 1993, Hacking 1993):

> The very ease and rapidity with which astronomers saw new things when looking at old objects with old instruments may make us wish to say that, after Copernicus, astronomers lived in a different world. (Kuhn 1962, 117)
> ...
> At the very least, as a result of discovering oxygen, Lavoisier saw nature differently. And in the absence of some recourse to that hypothetical fixed nature that he „saw differently", the principle of economy will urge us to say that after discovering oxygen Lavoisier worked in a different world. (ibid., 118).

In our opinion the „world change" declarations clearly show that Kuhn's rupturist model of the historical evolution of science is quite different from the rather continuous one that Cassirer endorsed, even if the later Kuhn mitigated some of his earlier more radical claims. Friedman attempts to play down these differences.

5. *Kuhn as Problem for History of Philosophy of Science.*

Conceiving history as an essential ingredient for a philosophical understanding of science points toward a thorough naturalization of philosophy of science. Philosophy of science no longer seeks to comprehend science starting from a basis of some pre-given a priori principles, rather it attempts to understand science from the ongoing historical evolution of scientific knowledge itself. This process of naturalization comprises, of course, not only the historical dimension, but also the sociological, the psychological and other ones. Thereby new empirical disciplines pop up such as history of science, psychology of science, sociology of science, and so on. On the other hand, traditionally the emphasis on the importance of history of philosophy was a stronghold of continental philosophy, while the interest of analytical philosophy for history was less fully developed. Due to the influence of Kuhn's work, this state of affairs has changed. This is evidenced, for instance, by the meanwhile well-developed history of the logical

empiricism of the Vienna Circle and similar currents that are usually pursued from an analytical perspective.

Even if there is a rather general consensus among philosophers that history of philosophy should play a certain role for philosophy, it is far from clear what precisely this role will be. For instance, some contend that we have to know the history of philosophy (of science) so as not to avoid important alternatives to contemporary proposals. In the case of philosophy in general, some authors want to go further, claiming that philosophy is essentially an historicist endeavor. How far this attitude may be applicable also to history of philosophy of science, remains to be investigated. In line with a general naturalistic outlook on science and philosophy, some years ago Hardcastle and Richardson spoke of a „historicist turn" in philosophy of science that might help to overcome the crisis that, according to them, plagues contemporary philosophy of science. With „historicist turn"

> We refer to a more recent development in which philosophers have begun to recover the problems, solutions and motivations of earlier projects in the philosophy of science, paying attention to how the historical figures engaged in these projects understood them. ... Adapting what is perhaps the most famous sentence in the philosophy of science of the second half of the twentieth century, we can assert that the history of the philosophy of science is coming to be viewed as more than a repository for anecdote and chronology, and can, if we allow it, produce a decisive transformation in the *philosophy of science* we now possess. (Hardcastle and Richardson 2003, vii).

To be explicit, the „most famous sentence in the philosophy of science of the second half of the 20th century" let us recall Kuhn's famous dictum once again:

> History, if viewed as a repository for more than anecdote or chronology, could produce a decisive transformation in the image of science by which we now are possessed. (Kuhn 1962, 1).

Hardcastle and Richardson's history-oriented way of doing philosophy of science may be interpreted as a kind of second-order appropriation of Kuhn's philosophical legacy, not to science, but to philosophy of science. Moreover, they claim that philosophy of science urgently needs this new means. According to them, contemporary philosophy of science is entangled in a deep conceptual, almost existential crisis, and history of philosophy of science might help overcome it. Indeed, they invite us to tap the spiritual sources of a quite specific period of the history of philosophy of science:

> [I]t may well be time to return to the social spirit of philosophy of science of the 1930. Perhaps that is our best philosophical venture in a world of anxious social and technological Maybes. (Hardcastle and Richardson 2003, xxvi).

More generally, the historical way of doing philosophy of science helps us recover conceptual resources that we lost from sight. Not everything „logical positivist" or everything „transcendental idealist" belongs in the dustbin of history. We may use history of philosophy of science as a source for exploring hitherto undeveloped or underestimated conceptual possibilities. Friedman's neokantianism is a good example. The historical way of doing philosophy of science might help overcome not only the wide-spread *amnesia* concerning matters historical but also the illusion that philosophy and its history can be neatly separated from each other.

A negative example of this attittude is the logical empiricism of the 1960s that strictly separated philosophy and history of philosophy. We already dealt with Carnap and the other adherents of the „bipartite metatheory". Another, even blunter example is provided by Reichenbach who in *The Rise of Scientific Philosophy* (Reichenbach 1951) drew a sharp line between „scientific" philosophy and history of philosophy:

> The comparison between the old and the new philosophy is a matter for the historian and will be of interest to all those who were brought up in the old philosophy and wish to understand the new one. Those who work in the new philosophy do not look back; their work would not profit from historical considerations. They are as unhistorical as Plato was, or Kant, because like those masters of a past period of philosophy they are only interested in the subject they are working on, not in its relations to previous times.
> ...
> Scientific philosophy attempts to get away from historicism and to arrive by logical analysis at conclusions as precise, as elaborate, and as reliable as the results of the science of our time. (Reichenbach 1951, 325).

Exactly this simplistic separation of the two fields hindered logical empiricism to respond adequately to Kuhn's challenge of finding a „role for history" in philosophy of science. Although Carnap's proposed division of labour was less blunt than Reichenbach's open dismissal of history for „scientific philosophy", the general public seems not to have clearly distinguished between the two accounts.

Kuhn's proposal of „a role for history" emphatically was not meant as one for a peaceful coexistence on the basis of a mutually accepted separation of two different worlds. It was a great error of the logical empiricists that they thought that Kuhn's account did not importantly disrupt their own (cf. Richardson 2007, 368). In some sense, this error is repeated by those „revisionists" who overestimate the similarities

between Carnap's and Kuhn's accounts conceiving Kuhn's paradigms more or less simply as informal versions of Carnap's linguistic frameworks, or vice versa (cf. Earman (1993, 32), Richardson (2007, 356)). Other scholars in the revisionist camp who are eager to diminish the difference between Kuhn and logical empiricism rely on a different strategy: They emphasize similarities between Kuhn and the „not-so-logical" logical empiricists such as Frank, Neurath or Zilsel. The most interesting case is Frank, since there was a direct relation between him and Kuhn. As Nemeth reports, Richard Butts, relying on Adolf Grünbaum as his witness, asserts that Kuhn got the essential idea of *Structure* from lectures of Frank coming (probably not quite seriously) to the astonishing conclusion:

> One of the fathers of scientific philosophy (i.e. Frank, (A.I. and T.M)) actually wrote Kuhn's *The Structure of Scientific Revolutions*. (Nemeth 2007, Footnote 11, 298).

These attempts of coming to terms with Kuhn, either by pointing at some similarities between Kuhn and Carnap, or, alternatively, by similarities between him and Frank, Neurath, or Zilsel, are less than compelling. The fact that none of the later logical empiricists was able to offer a convincing comprehensive account of the „true" logical empiricism to the general public, must be considered as a sign of profound conceptual weakness and exhaustion. A philosophical movement can hardly afford that there is a profound gap between its public image and its allegedly quite different true essence known only to some selected few. As Richardson points out, exactly this happened to logical empiricism in the 1950s and 1960s:

> Logical empiricism looked from the outside like something rather different from how it looked on the inside, and at least some philosophers of science as well as most, if not all, historians of science were well on the outside of logical empiricism. (Richardson 2007, 364).

In sum, the rise of the historicist account of philosophy of science and the corresponding decline of logical empiricism after the publication of *The Structure of Scientific Revolutions* keeps on being a difficult problem for history of philosophy of science and therefore for philosophy of science as well. We are still far away from a reasonably complete understanding of Kuhn's role in the history of philosophy of science of the 20th century, to say nothing about an unanimous assessment of his achievements.

6. *Concluding Remarks.*

Even if there is no unanimously agreed upon assessment on Kuhn's legacy for philosophy of science, as the very minimum one may say that Kuhn played a pre-eminent role in the endeavor of reminding philosophers of science (once again) of the indispensable role of history for understanding scientific rationality. As the many different attempts of appropriating Kuhn's ideas show, it is far from clear, however, how this role for history is to be conceived. It may well be the case that this problem has no unique solution, and certainly the three proposals that have been discussed in this paper, will not be the last word on this issue.

Carnap's proposal of a clear-cut and neat division of labour between a logic of science on the one hand, and empirical disciplines dealing with science such as history, sociology, and psychology, was not much more than a formal trick to eschew the real problem. Some forty years ago when *Structure* appeared on the scene it clearly failed. Whether the project of a logical empiricist „bipartite metatheory" that has been recently proposed (cf. Uebel 2009) will score better in the future, remains to be seen.

The structuralists's attempt of appropriating Kuhn's philosophical ideas failed in a quite different vein. In contrast to the logical empiricists who plead for a peaceful coexistence and division of labour between logical and non-logical perspectives of science, their structuralists successors, trusting in the allegedly much greater expressive power and flexibility of their set-theoretical apparatus optimistically opted for a full-fledged „rational reconstruction" of Kuhn's novel conceptual framework. As everybody knows, a rational reconstruction is nothing to which the predicates „true" or „false" are applicable. Rather, a rational reconstruction is assessed to be useful or not useful. Up to now, the verdict of philosophers, scientists, and historians of science about the usefulness of the structuralist reconstruction of Kuhn's ideas has been not very positive. The structuralist reconstruction is considered as a superficial formalization of Kuhn's informal concepts. No intelligibility is gained by translating concepts of ordinary language into a set-theoretical formalism. Even if one admits that everything can be translated into a set-theoretical jargon, it is hard to see why it should be translated. Again, this is not a final verdict. But more than thirty years have passed since Stegmüller first put forward his rational reconstruction of Kuhn, and apparently not much progress has been made.

What about the Neokantian attempt? It has certainly more philosophical appeal than the arid set-theoretical constructions of the structuralists and is more sophisticated than the

naive proposal of a neat division of labour that the later logical empiricists brought forward. Nevertheless, it cannot be considered as a fully satisfying response. Although it clearly was a clever move to reformulate the problem of incommensurability, which has bewitched generations of Kuhn scholars, as the general task of philosophy to take care of *communicative rationality* (Habermas) or *reasonableness* (Toulmin 2001) this shift can be considered only as a first step, since up to now these concepts are far from well understood. Take for instance, Habermas's characterization of communicative rationality that Friedman takes as his starting point:

> The concept of communicative rationality carries connotations that ultimately trace back to the central experience of the non-coercively uniting, consensus creating power of argumentative speech, in which different participants overcome their initially subjective points of view, and, thanks to the commonality of reasonably motivated convictions, assure themselves simultaneously of the unity of the objective world and the intersubjectivity of their context of life. (Habermas 1981/1984, vol. I, 8, Friedman 2001, 54).

Even on a charitable interpretation this description lacks clarity. Indeed, in his considerations Friedman relies on a quite different concept of communicative rationality as a means for maintaining a minimum of conceptual continuity during a change of paradigm:

> The mathematical exact sciences still serve as the very best exemplars we have of universal communicative rationality in spite of, and even because of, their profoundly revolutionary character. (Friedman 2001, 117, 118).

We admit that these words are truly heart-warming for all friends of the Enlightenment project. But one may well doubt whether they are in line with Habermas's and Kuhn's original ideas. Indeed, it was a characteristic feature of Kuhn's philosophy of science that for it the mathematical character of the exact sciences was of no importance at all. One could read *Structure* without becoming aware that modern physics, in contrast to Aristotle's, say, is a highly mathematized science. On the other hand, for Kant and the Neokantians mathematics was the backbone of scientificity. Hence, for Kant and the Neokantians it was quite natural to look for some kind of „continuity" between two „incommensurable" paradigms in the underlying mathematics, but not for Kuhn. Friedman's notion of communicative rationality, closely related to mathematization, is obviously quite different from Habermas's. In sum, for the time being, Friedman's Neokantian philosophy of science can hardly claim to be a fully elaborated account free of any internal conceptual problems. But this does not exclude that it may be the most promising appropriation of Kuhn's philosophical legacy available today.

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