

Thomas Kuhn and the Structuralist View of Scientific Theories. The Formal and the Historical Perspective of Theory Change

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The main basis of this talk is the scientific correspondence between Thomas Kuhn and Wolfgang Stegmüller from the Stegmüller Nachlass at the Brenner Archive in Innsbruck. This correspondence provides new insights into the philosophical positions of both Stegmüller and Kuhn. In particular it turns out from the correspondence that Kuhn's reaction to Stegmüller's formal "Sneedification of Kuhn" was entirely positive and that Kuhn took Stegmüller's formal account quite seriously, as a means for a further clarification of his philosophical positions. This fact seems to support the following more general hypothesis about the way how Kuhn treated the philosophical material that he considered. Kuhn had no general preference at all, for or against a particular current in philosophy. He was open for any kind of method or tool that allowed him to clarify his account of the sciences. As a consequence of this, he found *naturalistic* clarifications for his account, he found clarifications in the *history of philosophy* (especially, in Neo-Kantianism) and he also found clarifications in *logic* (namely, the Sneed-Stegmüller conception). Because of his impartiality concerning philosophical methods and positions he had no problem to use all these tools and methods together. At any rate, the present paper cannot proof this general thesis about Kuhn's treatment of philosophical currents. It merely provides an account of his treatment of the structuralist school and tries to illustrate that this account may fit properly into the overall picture just suggested.

Kuhn's groundbreaking work *The Structure of Scientific Revolutions* (henceforth: *Structure*) deeply challenged the "received view" of scientific theories, as it was developed by logical empiricists in the first half of the last century. Crucial notions like „paradigm“, „normal science“ and „revolution“ cannot be explained in the received formal frameworks of logical empiricism. Therefore, Kuhn's account is incompatible as a whole with logical empiricism in its classical form. But does this also imply that his account is necessarily incompatible with *every* kind of a *formal* account of the sciences?

This question may be answered affirmative, on the basis of the following picture. The idea may be that Kuhn's account in *Structure* is a "descriptive" one whereas the formal frameworks that the logical empiricists developed are „normative“. Kuhn, according to this view, (only) *describes* the sciences as they actually are, therefore his (exclusive) method is (and has to be) history of science. The logical empiricists, on the other hand, tell us how the sciences should be, therefore their (exclusive) method is (and has to be) logic.

(Fortunately) this picture is completely false (in a number of ways). Firstly, Kuhn's account in *Structure* is no more (or less) descriptive than any other account in philosophy of science. In a letter to Wolfgang Stegmüller Kuhn wrote on this topic:

Structure and my related articles [...] must classify not simply as reconstruction (like all history) but as *rational reconstruction* (and therefore not as history at all). They are, that is, always concerned with the general, historical matter being introduced only as evidence, not for its own sake, in a manner long traditional in philosophy of science. *Kuhn to Stegmüller, 20 January 1975, p.3*

Secondly, *Structure* also has a *normative* perspective. Though it is true that *Structure* does not formulate normative instructions to the scientists from a philosophical meta-standpoint, it tries to formulate instructions to the scientists from the standpoint of the sciences in themselves. I quote from the just mentioned letter to Stegmüller again:

I am not [...] recommending changes in current professional norms but explaining why existing ones work and why abandoning them might result in the disappearance of science. [...] "If you want to get scientific results, you should behave the way scientists (as here described) normally do". *Kuhn to Stegmüller, 20 January 1975, p.4*

Thirdly, it turns out that although Kuhn's primary method was history of science he never claimed that logic cannot be useful for the reconstruction of the dynamic of scientific theories. Again, in his letters to Stegmüller he said:

Contrary to a popular impression, I am not an enemy of formalism. (Could anyone trained as a theoretical physicist really take such a position?) *Kuhn to Stegmüller, 14 August 1974*

Formalism is not itself rational reconstruction but a sometimes useful tool towards that end. *Kuhn to Stegmüller, 20 January 1975, p.4*

In other words, for Kuhn formalism *and* history both may function as useful tools for philosophy of science.

On the basis of these preliminary considerations on Kuhn we now give a short outline of the structuralist view as developed by Joseph Sneed, Wolfgang Stegmüller, and others. In *The Logical Structure of Mathematical Physics* (1971) Sneed presented the (later so-called) *non-statement view* as a means for the rational reconstruction of the dynamics of scientific theories, pretty much in the sense of Kuhn's *Structure*. Stegmüller picked up this idea and worked it out with much more detail, in a number of writings between 1973 and 1979.

A crucial point of this framework in the form Stegmüller adopted it from Sneed is that it only has an expression for *empirical adequacy* while there is no primitive notion of *truth* defined in this framework at all. For Stegmüller, scientific progress has to be explained exclusively on the empirical level, without any access to the absolute truth value of a scientific theory. In other words, Stegmüller shares Kuhn's form of „relativism“ and he rejects at the same time the (Popperian) idea of scientific progress as inevitably linked to an absolute notion of “verisimilitude”. On the formal level, this becomes possible simply because the constitutive objects of a scientific theory are not statements but structures here. A structure in itself never can be “true”, it only can have the property of being related to other structures in one or another way. Thus, it becomes a question of the contingent layout of a structuralist framework whether and to what extent it may include notions like “truth” and “empirical adequacy”. Sneed, in his original proposal, seems to have preferred a less relativistic version of the framework, but Stegmüller left no doubt that what he wants to express with this framework is a defense of (almost) exactly that form of relativism that was developed in the *Structure*. In a letter to Kuhn he said:

The best defense against this kind of accusation [namely, the accusation of relativism – C.D.] is to defend this variety of ‘relativism’. *Stegmüller to Kuhn, 9 June 1975*

However, it should also be noted here that Stegmüller's way towards this structuralist defense of Kuhn's ideas was anything but straightforward. As late as 1971 Stegmüller wrote a polemical essay (“Das Problem der Induktion: Humes Herausforderung und moderne Antworten”) where he sharply criticized both the extremely normative accounts of the Popperians and the Erlangen school *and* the (seemingly) relativistic and purely descriptive account of Kuhn. It was just on the basis of Sneed's ingenious formal reconstruction of Kuhn that Stegmüller suddenly converted and became an ardent advocate of Kuhn's relativistic account of the sciences, which he now took as the only proper alternative to the excesses of a dogmatically normative philosophy of science. In other words, for Stegmüller Kuhn's

philosophy was entirely acceptable (after 1973), but only in the context of a *formal* reconstruction that he himself called *Carnapissime*.

In spite of his sudden enthusiasm, Stegmüller hesitated to send his crucial work from 1973 *The Structure and Dynamics of Theories* to Kuhn. To a number of correspondence partners (among others Yehoshua Bar-Hillel, Herbert Feigl, Paul Feyerabend and Carl Gustav Hempel) he expressed his fears that his highly formal account may be accepted neither by the traditional philosophers of science nor by Kuhn and by the historians of science. Finally, he sent his manuscript to Kuhn, just with a short dedication, but without a cover letter. Astonishingly enough, Kuhn's reaction was quite enthusiastic. It was Kuhn who started the correspondence with Stegmüller. In his first letter he wrote, for example:

For ten years I had been waiting for someone to pick up my (and others') incomplete enterprise and show how to carry it further. From an early stage of my involvement with your book, it has seemed to me likely that you are the man, and I have been correspondingly deeply moved. *Kuhn to Stegmüller, 14 August 1974*

This reaction of Kuhn was not just a flash in the pan. A couple of month later Kuhn sent a letter of no less than 30 pages to Stegmüller, containing detailed comments on his book (some quotations from this letter I already presented above). An abbreviated version of this letter becomes the contribution of Kuhn to a symposium on "the Sneed formalism" that Sneed, Stegmüller, and Kuhn delivered at the CLMPS congress in London/Ontario in 1975. At the end of this short but intensive intellectual exchange Kuhn came to the conclusion:

I think there is now virtually nothing that we disagree about. *Kuhn to Stegmüller, 19 June 1975*

Although this deep agreement between Stegmüller and Kuhn seems to remain unchanged in later times it is obvious that the liaison between Kuhn and Stegmüller was an episode in the development of philosophy of science, without any lasting effects. In particular, structuralism clearly had failed as an attempt to a rapprochement between traditional philosophy of science and the more historically and sociologically orientated projects. Its further development was almost exclusively that of a special branch of "formal philosophy" and not as a means for a "Sneedification of Kuhn". Structuralism became just another formal approach (and as such it was more or less successful, indeed). It failed, however, as an attempt towards a philosophy of science that includes *both* formal *and* historical methods. Stegmüller's fears, as he expressed them in a letter to Jehoshua Bar-Hillel appeared to be not ungrounded, after all:

I am afraid, however, to become caught in the middle. Apart from one logical failure in Kuhn [...] I think that he is right in all the other points of the controversy. [...] I hear Feigl say: "Now this Stegmüller has also gone over to these obscurantists". In the same way, however, I can imagine vividly what Feyerabend would say when he faces my manuscript: "That is the peak point of logical weirdness. Now these super-logical crackpots begin to use 'the laughable inadequate methods of a logician' as a means for the analysis of the *dynamics* of the sciences". Stegmüller to Jehoshua Bar-Hillel, 26 March 1973

[Hier, nur zum Vergleich das Deutsche Original, bitte nicht vorlesen!

"Ich fürchte allerdings, mich zwischen alle Stühle zu setzen. Abgesehen von einem logischen Fehler bei Kuhn [...] glaube ich, daß er in allen übrigen Punkten in dieser Kontroverse recht hat. [...] Ich höre schon Feigl sagen: 'Jetzt ist der Stegmüller auch noch zu diesen Obskurantisten übergelaufen.' Ebenso aber kann ich mir ziemlich lebhaft vorstellen, was Feyerabend sagen würde, wenn er mein Manuskript zu Gesicht bekäme, etwa: 'Das ist doch der Gipfelpunkt an logischer Verrücktheit. Jetzt fangen diese superlogischen Spinner auch noch an, 'the laughable inadequate methods of a logician' zu benutzen, um die *Wissenschaftsdynamik* zu analysieren.'"]

Although Stegmüller's fears concerning Feyerabend were ungrounded (the latter wrote a long review of Stegmüller's book for the *British Journal of Philosophy of Science*, which was overall positive) it is obvious that the structuralists completely failed to establish their method with any closer interdisciplinary contact to the more historically and sociologically oriented philosophers of science. Consequently, the project of a rapprochement between these two "camps" in philosophy of science had failed as a whole.

What I want to suggest here, however, is that the reason for the failure of this project is not at all a systematic one (whenever there was a systematic discussion between the opponents there was broadest agreement) but more or less exclusively a *sociological* one, very roughly on the following line. Formal philosophy, and the different branches of a more historically and sociologically oriented philosophy of science (from the Kuhn-Hanson-Toulmin tradition to the Edingurgh school and the strong program in sociology of science) were newly initiated as independent fields of research in the 60s and the 70s of the last century. At this time these research fields primarily needed to defend themselves as independent fields. In such a climate of the establishment of new disciplines it is no surprise at all that interdisciplinary cooperations between the new fields and between new fields and the old ones become extremely difficult, if not impossible.

Today, however, both formal philosophy and the more historically and sociologically oriented branches of philosophy of science are quite well established as independent fields of research. As a consequence of this, it may turn out that there are no social and financial issues

anymore that prevent us from the respective interdisciplinary contacts. On this basis it may be fruitful to reconsider Stegmüller's original proposals from 1973 as an interdisciplinary approach that allows us to combine formal and empirical methods in philosophy of science. The correspondence between Kuhn and Stegmüller may provide a good starting point for this project.