

“Caught in the Middle”.

Philosophy of Science between the Historical Turn and Formal Philosophy as Illustrated by the Program of “Kuhn Sneedified”¹

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Abstract

This paper is concerned with the development of philosophy of science in the 1970s. The explanatory framework is the picture of two fundamental split-offs: the controversial establishment of history and sociology of science and of formal philosophy of science as independent disciplines, against the background of more traditional “conceptual” varieties of philosophy of science. We illustrate these developments, which finally led to somewhat “purified” versions of the respective accounts, by examining a case study, namely that of the structuralist school, which emerged in the 1970s as an attempt to reconcile historical and formal approaches in philosophy of science. We try to explain the failure of this initial program of “Kuhn Sneedified” and its transition to a more purified formalist version, based on the fact that the former attempt was caught somewhere amidst the purism of conceptual, historical, and formal accounts of philosophy of science.

Structuralism¹, in its mature form, as established around 1979, is a paradigm case of a “Continental European style” formal philosophy of science. But, in its initial form, it was neither a purified formalist account, nor even European at all. The main ideas of structuralism were invented by the US philosopher Joseph Sneed, a pupil of Patrick Suppes. Sneed’s 1971 book, *The Logical Structure of Mathematical Physics*, was a highly respected work even in the American scene. In spite of his unquestionable abilities as a logician and a formal philosopher of science, Sneed was never employed at any philosophy (of science) department at a major university in the US.² In Germany, on the other hand, Sneed’s work was quick to attain such prominence that, for some decades, there was hardly any work in German philosophy of science that could ignore the structuralist program. From the mid-1970s and the mid-1990s, a good deal of German philosophy of science was devoted to the structuralist program, and a number of specialists (most of them equipped with attractive positions at philosophy departments in Germany) worked, more or less exclusively, in the realm of Sneed formalism.³

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However, structuralism was not just a local phenomenon; it was formulated and appreciated, at least partly, in the international context.⁴ Although the most important publications in the field appeared between 1971 and the beginning of the 1990s, structuralism is still discussed and used in philosophy of science (inside and outside of Europe). At any rate, structuralism was never really a big issue in the international philosophy of science scene, but rather a specialized account of formal philosophy of science that covered a particular niche of the discussions in that vast field of research.

In spite of the purely formal character of the structuralist program in its mature form, the initial proposal made by (Stegmüller 1973) was by no means intended to be a mere contribution to *formal* philosophy of science. The latter book was not just a comment on Sneed but a comment on Sneed *and* Kuhn, a thoroughgoing discussion of both the structuralist framework *and* the philosophy of *Structure*, which culminates in the programmatic proposal of an account of philosophy of science that incorporates formal *and* informal *and* historical considerations (see, in particular, Stegmüller 1976, vii-xi). In a recent email, Joe Sneed wrote the following, with regard to (Sneed 1971/1979) and (Stegmüller 1973):

I think both Stegmüller and I regarded these discussions as primarily an attempt to show that a certain kind of formal description of empirical theories made it possible to understand Kuhn's account of the historical development of these theories in a way that made it appear a lot more 'rational' (i.e., compatible with the logical empiricist conception of science) than Kuhn's initial account might have suggested. We both assumed (perhaps naively) that Kuhn's account of history (perhaps purged of some rhetorical excesses) was essentially accurate. We were proposing an extension (reform?) in the methodology of formal philosophy of science to make it more adequate to deal with 'reality' as described by Kuhn. (Email to the author on 31 March 2012)

This initial approach by Stegmüller and Sneed is not identical to what was later called 'structuralism' particularly the formalist account of *Architectonic*. Therefore, as (Stegmüller 1973) contains at least as much Kuhn as it contains Sneed, we henceforth refer to it under the title "Kuhn Sneedified" and distinguish it from structuralism in its mature form, as developed by Sneed, Stegmüller, Balzer, Moulines, and others.⁵ In sharp contrast to the presentation of "Kuhn Sneedified" in (Stegmüller 1973), in (Stegmüller 1979), we read the following:

The structuralist approach should be looked at as *the striving for an extension of the Bourbaki program to science* rather than as an attempt to reconstruct the ideas of T.S. Kuhn. The fact that, with this approach, some aspects of Kuhn's philosophy of science can be substantiated, or at least made more plausible, should be considered as a side-effect only. (Stegmüller 1979, 1)

In its mature form, structuralism was quite successful (at least on the German scene), while "Kuhn Sneedified" failed after a short period of enthusiasm and some promising initial exchanges between Stegmüller and Kuhn. This must be regarded as puzzling insofar as "Kuhn Sneedified" was in fact much more ambitious and probably also philosophically more interesting than the mature form of structuralism. The aim of this paper is to explain these puzzling developments.

Note also that the main differences between "Kuhn Sneedified" and mature structuralism are not to be found in the formal details. With respect to formalism, the development of structuralism was an evolutionary one, and even the topics that structuralists discussed did not change drastically over time (e.g., even "mature" structuralists were interested in the dynamics of the sciences and in the formalization of certain Kuhnian notions). However, there was a dramatic change with regard to the personnel involved in the project and (as a consequence of this) the systematic locus where the project was situated. After some initial involvement on the part of Kuhn and even of Feyerabend, and at least some rudimentary attempts to do histo-

ry of science, there was no more involvement of people from the HPS side after 1979. Moreover, while structuralist formalism (at least in the case of Stegmüller) was initially intended as a sort of comment on Kuhn, and “Kuhn Sneedified” therefore belonged to the literature on Kuhn, after 1979 structuralism found its place as a purified contribution to the literature in formal philosophy.⁶

1. The encounter between Stegmüller and Kuhn

The chronology of Stegmüller’s movement toward Kuhn can be reconstructed as follows.⁷ In 1971, Stegmüller was still (more or less) ignorant of Kuhn’s work (he might have read only the Lakatos-Musgrave volume), and his attitude toward Kuhn was entirely negative.⁸ At that time, he started studying Sneed’s book, and immediately decided to publish some “lecture notes” on it.⁹ Around the middle of 1972, Stegmüller decided to write an extension to volume 2 of (Stegmüller 1969-1986), which should have dealt exclusively with Sneed (Kuhn was not yet mentioned in that context) (Dahms 2010, 316, n.198). It was only in the first half of 1973 that Stegmüller officially “outed” himself as a Kuhnian in letters to a number of friends and colleagues, including Yehoshua Bar-Hillel (03-26-1973), Herbert Feigl (03-30-1973), Paul Feyerabend (04-30-1973), Kurt Hübner (04-31-1973), and Carl-Gustav Hempel (06-29-1973). Stegmüller probably discovered the connection between Sneed and Kuhn somewhere between the middle and the end of 1972 and transformed his reception of Sneed very quickly into a reception of “Kuhn Sneedified”, in which Kuhn played the central role.¹⁰

The whole undertaking was a real “snapshot” and a sort of reluctant revolution. Reading Stegmüller’s correspondence from March to April of 1973, one gets the impression that Stegmüller himself was more or less convinced from the beginning that he would never be able to convince anyone to appreciate his new approach. In spite of this defensive attitude (concerning his ability to convince the scientific community of the accuracy of his account), Stegmüller was ready to defend Kuhn quite strongly. To his friend and colleague Kurt Hübner, he expressed his task as an attempt “to take Kuhn’s challenges seriously and to develop entirely new criteria of rationality”, something that “(apparently) no one tried to do before” (Stegmüller to Kurt Hübner, 04-31-1973 [my translation]). This formulation is remarkable because it shows that Stegmüller’s “paradigm shift” – from a well-behaved Carnapian to a defender of Kuhn’s “new criteria of rationality” – was understood as a radical one by Stegmüller himself.¹¹ On the other hand, Stegmüller expressed his fear of being “caught in the middle”:

However, I am afraid of becoming caught in the middle here [“mich zwischen alle Stühle zu setzen”]. Apart from one logical error in Kuhn, which admittedly is quite serious (and gets repeated over and over by him and Feyerabend in different varieties), I think that he is right in all the other points of the controversy, in particular concerning the question of immunity of a theory against ‘contradictory experiences’.

Yet I can hear Feigl saying: “Now this Stegmüller has also gone over to these obscurantists”. In the same way, however, I can imagine vividly what Feyerabend would say when is confronted with my manuscript: “That is the peak point of logical weirdness. Now these super-logical crackpots are beginning to use ‘the laughably inadequate methods of a logician’ as a means for the analysis of the *dynamics* of the sciences”. (Stegmüller to Yehoshua Bar-Hillel, 03-26-1973 [my translation])

In fact, Stegmüller also expressed his fear that his account would not be understood at all – “particularly in Europe”, reasoning that “there were *too many* seemingly completely heterogeneous things to be combined”, e.g. the new “non-statement view”, Ramsification, or the location of the notion of a paradigm in Wittgenstein and Kuhn. (Interestingly, Stegmüller did not mention history, sociology, or psychology of science here, as further examples for such

“seemingly completely heterogeneous things”). He concluded with a quite ambivalent vindication of his approach:

Without Sneed, all these thoughts may have not crossed my mind. On the other hand, I had been so horribly irritated by T.S. Kuhn [“hatte ich mich über T.S. Kuhn so wahnsinnig geärgert”] that it no longer appears to be surprising, once the point of the ‘dialectic flip’ [“dialektischer Umschlag”] had come up. (Stegmüller to Yehoshua Bar-Hillel 03-26-1973 [my translation])

No less defensively (and with some of the same formulations – “gone over to these obscurantists”, etc.), Stegmüller wrote to Feigl, another close friend and colleague (König-Porstner 2010), to Hempel, and even to Feyerabend. However, Stegmüller hesitated to write about his new ideas to Kuhn personally. He sent the book to him (after its appearance in late 1973), with only a short inscription and a brief autobiographical note, describing himself “as a Carnapian who was perhaps becoming a proto-Kuhnian” (Kuhn 2000, 318). Similarly, he sent his book to Sneed, but without any personal remarks (Stegmüller to Sneed, 12-05-1973, and Sneed to Stegmüller, 12-24-1973). The reason for this defensive approach was obviously not that Stegmüller had any personal reservations about either Kuhn or Sneed but, rather, a mixture of noble restraint and the uncertainty of someone who was never really comfortable in English. It is, therefore, all the more remarkable that Kuhn not only read Stegmüller’s book quite carefully (Kuhn 2000, 318), but also started correspondence with the latter with a rather enthusiastic letter in which he wrote, among other things:

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. [...]

Contrary to a popular impression, I am not an enemy of formalism. (Could anyone trained as a theoretical physicist really take such a position?) Rather, I’ve objected to the sort of formalism long applied to philosophy of science, and I’ve had no notion how to find a substitute for it. I think the answer you and Sneed provide may fill the gap I have long felt, but it is taking me forever to assimilate it. (Kuhn to Stegmüller, 08-14-1974)

It took Kuhn another couple of months to go through the whole manuscript:

Having finished your book, I retain all the very great admiration, pleasure, and excitement in it that I expressed in my first letter to you. Unlike the German readers described in your letter of 28 October [where S. reported that German readers (mis)understood his book as a (devastating) critique rather than an (affirmative) comment on Kuhn], I have not misunderstood the spirit of your Chapter IX. In addition, I can (and mean to) testify that you understand my views extraordinarily well. (Kuhn to Stegmüller, 01-20-1975)

This second letter was accompanied by a 30-page-long (!) “excerpt from a Letter of 20 January 1975 by T.S. Kuhn to W. Stegmüller” (Kuhn remarked that he had “never written a letter nearly so long as this one”). In this manuscript, Kuhn essentially agreed with most of Sneed’s and Stegmüller’s interpretations of his account, and he even admitted that some sort of a reduction between incommensurable theories might be possible. However, he formulated a criticism of one single (but crucial) point. Kuhn wrote:

I am extremely skeptical of your claims concerning this aspect of the relation between an older theory and its historical successors. In this respect, your discussion of reduction is the one part of your book that I find unpersuasive. [...] I am not confident that *any* pair of historical theories separated by a revolution will satisfy your reduction relation. (Kuhn to Stegmüller, 01-20-1975, p.22 of the “excerpt”)

This skepticism remained in the version of the “excerpt” that was presented by Kuhn at a symposium in the context of CLMPS 1975 (London, Ontario), as well as in the published version (Kuhn 1976). Kuhn disagreed with Sneed and Stegmüller exactly at that point where

their theory intended to fill a “rationality gap” in Kuhn’s account.¹² However, in spite of this one critical point, Kuhn’s reception remained entirely positive.

Interestingly, in a long reply to Kuhn’s “excerpt”, Stegmüller more or less completely ignored the more devastating aspects of Kuhn’s criticism (i.e. the passage from p.22f), and referred only casually to the newly invented tools for “approximations” between theories, as developed by Moulines around 1975 (which indeed may provide a good answer to a minor point of Kuhn’s criticism).¹³ Kuhn, on the other hand, sharpened his criticism in his paper for the symposium in London, Ontario. He even prepared the third version of his paper, which was:

“... intended only to somewhat sharpen our differences. You and Sneed know very well that they are, but some of those who listened to the symposium seem to have thought that I was inviting them to replace my work with the new formalism, something that none of us expects.” (Kuhn to Stegmüller, 09-03-1975)

At a personal level, the congress in London, Ontario seemed delightful to both Stegmüller and Kuhn (they switched to the familiar “Du”), and there are no signs that their estimation of each other might have changed later. Unfortunately, there is no documentation of the discussions in London, Ontario, where Kuhn, Stegmüller and Sneed all gave talks, which obviously were attended by a large and quite prominent audience.¹⁴ However, the correspondence between Kuhn and Stegmüller reveals (at least indirectly) that the discussion was unsatisfying for both Stegmüller and Kuhn.¹⁵ As Kuhn’s aforementioned remark suggests, some Kuhnians seem to have thought that Kuhn had become an “old style” formalist philosopher of science. Most of the people in the audience (Stegmüller referred to Mary Hesse, in particular) seem not to have understood what the relation between Sneed’s formalism and Kuhn’s account may be. Some others might have been instantly convinced that the whole approach could never work.¹⁶ At any rate, there seem to have been few reactions to the symposium in London, Ontario, most of them being of a negative nature. This conclusion is supported by the fact that, immediately after the symposium in London, Ontario, the correspondence between Kuhn and Stegmüller (in spite of their obvious personal estimation of each other) fell off to a minimal level: after the publication of their symposia contributions in *Erkenntnis*, there were only two more letters between them, one in 1978 and the other in 1986.¹⁷ “Kuhn Sneedified” was obviously done after the disappointment in London, Ontario.¹⁸ Even Paul Feyerabend’s essentially positive review of “Kuhn Sneedified” (Feyerabend 1977) failed to motivate Stegmüller to come back to that program again. On the contrary, in a somewhat paradoxical reaction to Feyerabend’s review, Stegmüller backpedaled even further and revised the whole setup of his account from “Kuhn Sneedified” to a purified formalist “structuralism” (Stegmüller 1979).

Thus, “Kuhn Sneedified” appears to have been an episode in the history of philosophy of science that can be more or less reduced to (1) one single book, namely (Stegmüller 1973); (2) the personal encounters between Stegmüller and Kuhn, in 1974 and 1975; (3) the 1975 symposium in London, Ontario and its published version in *Erkenntnis* (Kuhn 1976; Sneed 1976; Stegmüller 1976a); (4) Feyerabend’s review (Feyerabend 1977); and (5) the aforementioned presentations and articles by Stegmüller between 1973 and 1975. Neither the followers of Kuhn nor Stegmüller’s students and other structuralists seem to have found anything convincing in the conception of “Kuhn Sneedified”. There is virtually no discussion on that topic in the literature on Kuhn. And even in the structuralism literature, the topic of revolutionary science and incommensurability more or less faded out after 1979.¹⁹

2. Why did “Kuhn Sneedified” fail?

In order to identify the reasons for the rejection of “Kuhn Sneedified”, we consider the interactions between the advocates of “Kuhn Sneedified”/structuralism and other philosophical currents: (a) the Kuhn-Feyerabend community; (b) other varieties of “Continental European style” formal philosophy of science; (c) pure mathematical philosophy; (d) “old style” philosophy of science.

(a) *Interactions with the Kuhn-Feyerabend community*

Apart from Kuhn’s positive reactions and the singular review of Feyerabend, there were hardly any other reactions to “Kuhn Sneedified” in the context of the research communities around Kuhn and Feyerabend.²⁰ This is remarkable in view of the fact that “Kuhn Sneedified” played such a prominent role in Kuhn’s work that no Kuhnian could ever ignore it. (Kuhn 1976) is no doubt a classic of the Kuhn literature, but it is usually interpreted as a step in the development of Kuhn’s notion of incommensurability rather than a comment on Sneed. Meanwhile, there has been rapid growth in literature on the philosophy of *Structure* in general, and on the problem of incommensurability in particular. However, there has been virtually no consideration of formal aspects in the context of that literature, and “Kuhn Sneedified” is hardly ever mentioned at all.²¹

Obviously, there is a sociological phenomenon at work here that we may call *positive ignorance*, as it is not ignorance due to lack of awareness (i.e., *negative ignorance*) but ignorance in spite of awareness. A case in point is the negative attitude toward formal styles of philosophy that can be found quite frequently, possibly because it is very hard to argue against formal philosophical work on an informal level. Thus, in the absence of any real knock-down argument or substantive refutation of a given formal approach, a common reaction is to characterize that approach in very general terms as an *irrelevant* or *unnecessary formalization* or *over-formalization*. However, this sort of argumentative strategy is not generally formulated explicitly (ignorance cannot easily be cried out). Thus, the presence of positive ignorance generally has to be proven indirectly: we have to first prove that there was awareness of phenomenon X, and then also that there were no reactions to X in spite of that awareness. These two factors may be seen as good indications that the phenomenon of positive ignorance is at work. In the case of the Kuhn literature, these two factors are quite clear present. First, (Kuhn 1976) is widely acknowledged to be an important contribution; thus, everyone in the Kuhn community is aware of “Kuhn Sneedified”. Second, there is virtually no mention of “Kuhn Sneedified” in the whole Kuhn literature. (qed)²²

Actually, the only possible conclusion to be drawn concerning the relation between structuralism and the “historical turn” is that there was no interaction at all after the episode of “Kuhn Sneedified”, which was essentially just a personal matter for Kuhn and Stegmüller. For lack of any straightforward rational explanation, we conclude that the reason for this astonishing absence of interaction must be seen in the sociological phenomenon of *methodological purification* (occurring on both sides), which led to a situation in which the methods of the respective other side were no longer available as reasonable philosophical methods.

(b) *Interactions with other varieties of “Continental European style” formal philosophy of science*

In the 1970s (and 1980s), a number of more or less purely formal varieties of philosophy of science, which we call “Continental European style”, were established. It is remarkable that these varieties can be found mainly in Continental Europe and to a lesser degree in the US and GB. Besides structuralism, the research fields of truth-likeness (Kuipers 1987) and belief revision (Gärdenfors 1988) may be seen as other varieties of “Continental European

style” work of that kind. Recently, there have been a number of efforts to combine these formalisms – in the sense of obtaining structuralist belief revision, belief revisionist verisimilitude, and the like (Andreas 2011; Kuipers & Schurz 2011). However, there was also plenty of scope for disagreement initially, at least between structuralism and truth-likeness.²³ The structuralist conception is relativistic insofar as there is no specification of a truth value for theories, and structuralism does not specify any explicit normative criteria that theories have to fulfill.²⁴ Thus, at least in its orthodox form, structuralism is clearly incompatible with scientific realist claims, and it is probably even too weak for van Fraassen’s program of constructive empiricism. Truth-likeness, on the other hand, is a philosophical program that grew out of Popper’s conception of “verisimilitude” and its failure (Kuipers 1987, introduction). The whole enterprise of truth-likeness is an effort to find a version of formalism other than Popper’s inapplicable one, which nevertheless may save a certain notion of the degree of truth-likeness of a (partially false) scientific theory. In other words, truth-likeness is an attempt to overcome relativism and to prove some sort of scientific realism. Thus, in spite of methodologically being examples of the same tradition in philosophy, structuralism and truth-likeness are clearly situated on different sides of the spectrum of possible systematic philosophical positions. Indeed, the structuralist conception was criticized by supporters of truth-likeness such as Ilkka (Niiniluoto 1981) and Raimo (Tuomela 1978), more or less exactly along the lines just drawn out, namely from a realist standpoint. On the other hand, structuralist formalism was acceptable to truth-likeness theorists, such as Theo Kuipers, in its very abstract form à la (Stegmüller 1979), but hardly in the form of “Kuhn Sneedified” (Kuipers 1987, 79-99). To conclude, a comparison of structuralism and truth-likeness indicates that these two currents form methodologically similar but systematically incompatible philosophical schools (at least at the time of their advent in the 1970s).

(c) *Interactions with pure mathematical philosophy*

There is another variety of formal philosophy, which was also established, in its purified form, during the 1970s – namely, the subdiscipline that we may call “pure mathematical philosophy”. It mainly comprises that branch of mathematical logic which was established in the 1970s and 1980s under the title of “philosophical logic”.²⁵ There are a number of significant differences between pure mathematical philosophy and “Continental European style”. Pure mathematical philosophers are always primarily mathematicians or computer scientists (and sometimes also studied philosophy), whereas “Continental European style” formal philosophers of science are always primarily philosophers (and sometimes also studied mathematics or computer sciences). These two groups attend different conferences and mostly publish in different journals. Moreover, pure mathematical philosophy seems to have begun as a “purification” phenomenon that was at least partly intended as a way to overcome formal philosophical work of exactly the kind that is represented by “Continental European style” formal philosophy of science. In the programmatic article (van Benthem 1982), for example, we find some sharp and even polemical criticism of the formal philosophical tradition as exemplified by Carnap and Sneed. According to van Benthem, the latter tradition differs in “mentality” from the pure mathematical tradition, as “logicians [i.e., pure mathematical philosophers in our sense] want *theorems* where these philosophers [i.e., ‘Continental European style’ formal philosophers in our sense] often seem content with *definitions*.” (p. 433) However, van Benthem lacks any clear argument against the latter approach, although he does polemically reject it. Actually, there are two different approaches to formal philosophy: one that is more interested in formal frameworks and the ways in which they illustrate certain philosophical notions; one that prefers formal theorems. It is neither the case that the framework approach was historically replaced by the pure mathematical approach (numerous varieties of “Continental

European style” formal philosophy of science have been devoted to the framework approach until recent times) nor is it the case that the pure mathematical approach may have “refuted” the framework approach. At any rate, the question whether philosophy of science may provide theories of the domains it addresses or, rather, elucidations and illustrations at a background level is a very complex one. Moreover, the mere lack of “regulative theorems” may not be used as a knock-down argument against the framework approach. This sheds new light on the well-known argument against structuralism (and even against Carnap) that identifies it as a much too complicated and (in view of the absence of “regulative theorems”) useless formalism.

In sum, then, for our explanation of the failure of “Kuhn Sneedified”, it is important to note that (because of a fundamental conflict of “schools”) this program could not expect any support from the pure mathematical philosophy camp.

(d) Interactions with “old style” philosophy of science

Traditional philosophy of science, as developed mainly in the US after 1945, reacted to the purifying tendencies manifest in formal philosophy and in the “historical turn” with systematic immunization. This is clearly the case at least for the period relevant to our study, i.e., the 1970s.

First of all, it is indeed remarkable that, with a few exceptions, philosophy of science (as developed mainly in the US), did not pick up on any new developments in the field of formal philosophy – neither “Continental European style” nor pure mathematical style). By contrast, during the 1970s, the philosophy of science obviously restricted its concerns with formal methods even further, and began to view any and all formal treatments of philosophical problems with suspicion. Thus, it is quite obvious that the challenges from the formal philosophy scene were blocked by a certain strategy of purifying an “informal” style. As a consequence of this, formal philosophy of science in general gained a bad reputation within the camp of “old style” philosophy of science. This is essentially just another phenomenon of “positive ignorance”, that is, the most characteristic feature of the interactions between “old style” philosophy of science and formal philosophy of science is the complete lack of engagement. In other words, the anti-formalism of “old style” philosophy of science was of a rather implicit nature, and did not lead to programmatic formulations of any kind.²⁶

In the case of the “historical turn”, the immunization strategy of “old style” philosophy of science was much more obvious and significantly more offensive. Here, we find the development of a thorough *anti-relativist* attitude in the course of the 1970s, which not only rules out the newly developed currents of science studies but even the more traditional proposals of Kuhn and Feyerabend.²⁷

Significantly enough, “Kuhn Sneedified” turns out to be a development here that drifted in exactly the opposite direction – that is, toward a significantly *more relativist* and *more formalist* picture of the sciences, almost exactly at the same time when such attitudes fell into disfavor within the “old style” philosophy of science camp. Thus, as long as the anti-relativist and anti-formalist stance was widely shared in the philosophy of science scene, any sympathy for “Kuhn Sneedified” could hardly be expected. This was essentially true even for those heretical philosophers of the traditional scene who, like Bas van Fraassen, supported anti-realism.²⁸ For realists like Hilary Putnam and Frederick (Suppe 1989), of course, the structuralist program was never an option.²⁹

3. Conclusions

In several ways, Stegmüller's project of "Kuhn Sneedified" was indeed "caught in the middle", i.e. the fears that Stegmüller expressed to his colleagues around 1973 were not ungrounded. Actually, Stegmüller's project got into trouble on even more fronts than Stegmüller had ever expected. First, his expectations were essentially fulfilled that the tradition-minded philosophers of science would take his project as an attempt to justify Kuhn's "relativism", whereas the Kuhn- and Feyerabend-camp (with the exception of Kuhn and – at least partly – Feyerabend themselves) would regard it as simply another form of useless formalization. Additionally, some other troubles arose that Stegmüller did not (and could not) anticipate in 1973. In particular, people working on pure mathematical philosophy were put off both by "Kuhn Sneedified" and by the mature version of structuralism, since they in fact rejected the whole idea of logic as a mere "formal framework" for the sciences. Another variety of a "Continental European style" formal philosophy of science, such as truth-likeness, was also incompatible with "Kuhn Sneedified" (and even with mature structuralism) because it wanted to defend the viability of an anti-relativist picture of the sciences (more or less in the sense of Popper's critical rationalism). Finally, structuralism was also at odds with the upcoming current of scientific realism and other varieties of (anti-Kuhnian) anti-relativism in the philosophy of science. In a word, around 1975, there was de facto no current of philosophy of science at all that did not have reasons to reject "Kuhn Sneedified" (and for the most part also the mature version of structuralism). And it was not only the systematic but also the emotional climate in philosophy of science that was poised against the modest and conciliatory attitude of "Kuhn Sneedified" – at that time, polarization was the slogan, not reconciliation (cf. Feyerabend 1975; Bloor 1976).

As a consequence of these developments, the whole project of "Kuhn Sneedified" was forced into isolation. It did not disappear immediately, because the scientific climate in Germany, in sharp contrast to the broader international situation, was actually quite favorable for the structuralist project (Dahms 2010, 317-321). However, even on the German scene, "Kuhn Sneedified" could only survive in a significantly slimmed-down and purified version, which hardly left any further scope for interdisciplinary exchange (in the sense of exchange with other philosophical currents and disciplines).³⁰ These developments were the results of systematic incompatibilities that emerged from several processes of specialization and purification in analytic philosophy and philosophy of science in the 1970s.³¹ Whether or not these purifications were good things is a question that goes beyond the scope of the present paper.

However, it is certainly an aim of this work to motivate a fresh look on "Kuhn Sneedified", even from a systematic standpoint. If it is true that failure of "Kuhn Sneedified" was the result of some "external" historical mechanisms rather than a rock-solid rational refutation then it may appear to be worthwhile to reconsider the original program. For example, the question in what sense Kuhn's notion of scientific progress as developed in the last two sections of *Structure* may be understood as compatible with more "old style" logical empiricist conceptions of the sciences may be answerable only in the context of a more formal treatment of Kuhn such as "Kuhn Sneedified". But this is a topic for another paper.³²

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5. Notes

¹ Cf. (Sneed 1979 [1971]; Stegmüller 1973, 1975, 1976a, 1978, 1979; Kuhn 1976; Feyerabend 1977), and the standard account of (Balzer et al. 1987) (here: *Architectonic*). We also refer to some items in the Stegmüller Nachlass at the Brenner Archive in Innsbruck (see <http://www.uibk.ac.at/brenner-archiv/archiv/stegmueller.html>). Since this paper exclusively deals with the historical development of structuralism it is by no means necessary (though it would be certainly helpful) to be familiar with the structuralist formal-

ism, as presented in *Architectonic*. The paper should be accessible even for non-experts who are merely interested in the development of history of philosophy of science in the last decades, without being specifically interested in the structuralist movement. Apart from the structuralism literature, the following works of Thomas Kuhn are most relevant to the present discussion: (Kuhn 1962/1970) (here: *Structure*), (Kuhn 1976, 2000). Occasionally, we refer to information that was provided to the author through personal communication (either orally or by email) by the following contemporary witnesses: Wolfgang Balzer, Aant Elzinga, Walter Höring, Carlos Ulises Moulines, Hilary Putnam, and Joseph Sneed.

² The correspondence of Stegmüller with Sneed and with Suppes documented this sad episode, although it did not fully clarify why Sneed was never employed at Stanford or any other major university in the US. However, one obvious reason seems to be that Sneed's work was simply too formal for the American scene, although it was explicitly based on the so-called West Coast Approach of (McKinsey et al. 1953; Adams 1955). Whereas in the latter approaches formalism was used in a rather moderate way, Sneed and his followers decided to develop philosophy of science as an exclusively formal business. This extremely formalist attitude obviously was not appreciated even by Patrick Suppes and other representatives of the West Coast Approach. On the differences between the American and the European scene (and the more pronounced affinities to formal approaches in Europe) see our remarks, below, in section 2b.

³ In particular, the following German full professors of philosophy worked, more or less exclusively, in the realm of the structuralist program: Wolfgang Balzer (Munich), Ulrich Gähde (Hamburg), and Carlos Ulises Moulines (Munich). Those concerned mainly, but not exclusively, with structuralism were Thomas Barthelborth (Leipzig) and Werner Diederich (Hamburg). The following scholars were concerned with structuralism, at least for some time, though they had other important focal points in their work: Michael Heidelberger (Tübingen), Walter Höring (Tübingen), Andreas Kamlah (Osnabrück), Felix Mühlhölzer (Göttingen), Karl-Georg Niebergall (Berlin), Thomas Mormann (Munich and San Sebastian), and Wolfgang Stegmüller (Munich). Note also that structuralism is by no means identical with the "Stegmüller school". Of the 24 school members listed in (Dahms 2010a), only three were exclusively devoted to structuralism (Moulines, Balzer, and Gähde) and five (Kamlah, Mühlhölzer, Hinst, Heidelberger, and Rott) partly devoted. The other 16 scholars (among them, Eike von Savigny, Wolfgang Spohn, and Julian Nida-Rümelin) were never concerned with structuralism. Outside of Germany, structuralist work was done by many pupils of Moulines, mainly in Spain, Argentina, and Mexico.

⁴ Several publications of the structuralist school appeared in first-rate international journals such as *Erkenntnis*, *Synthese*, or *The British Journal for the Philosophy of Science*. Cf. the bibliography (Diederich et al. 1989, 1994). Also, structuralism elicited a number of reactions in the international context. See, for example, (Bogdan 1979, 207-212; Niiniluoto 1981; van Benthem 1982, 1983, forthcoming a; Giere 1985, 346, n.10, 1988, 285, n.1; van Fraassen 1989, 190ff; Suppe 1989; Da Costa and French 1990, 250f.; Humphreys 1994, II, 195-218 and 275-300; Hintikka 1998; Hartmann et al. 2008, 65f.). Among these, the receptions of Jaakko Hintikka, Nancy Cartwright, Ronald Giere, Newton Da Costa, and Stephen French are positive in nature, the receptions of Patrick Suppes, Frederick Suppe, Johan van Benthem, and Ilkka Niiniluoto are rather critical, and the reception of Bas van Fraassen is somewhat ambivalent.

⁵ The term "Kuhn Sneedified" was used by Paul Feyerabend (somewhat ironically) in (Feyerabend 1977). Note also that the term "structuralism" was introduced in (Stegmüller 1979) and was not used before by Sneed, Stegmüller, and their followers. Thus, the term "structuralism" must be seen as a label that exclusively denotes the post-Kuhnian stage of the development of this research program. It is only during the period of "Kuhn Sneedified" that we find any deeper concern for the history of science. The most important example of the work of that kind is (Heidelberger 1980). There was also a research project "Historical case-studies on theories of the development of science" (1974-1980), funded by the DFG and directed by Walter Höring, that was also, perhaps, intended (at least initially) as a means for the development of the historical branch of "Kuhn Sneedified" (personal communication from Walter Höring and Joseph Sneed).

⁶ Since this paper intends to explain the internal development of a certain research project – i.e., the development from "Kuhn Sneedified" to structuralism in its mature form – it is not its aim at all to discuss the viability of any of these versions of this research project. For a more systematic discussion of structuralism see (Damböck 2011).

⁷ For pragmatic reasons, our case-study exclusively deals with Stegmüller's interactions with Kuhn. However, as a reviewer of this article points out, it is important to note that Stegmüller and his followers were not the only philosophers of science in the German scene of the 1970s and 1980s who dealt with Kuhn. Whereas the Erlangen school of Paul Lorenzen and his followers were hardly interested in Kuhn, Erhard Scheibe not only showed interest in Kuhn and Sneed but also developed another variety of structuralism, which is not identical with the Sneed-Stegmüller approach. Moreover, there is even a third structuralist conception in Germany, which was developed by Günther Ludwig. For an overview and a comparison of these three varieties of "structuralism" see (Schmidt 2008).

⁸ In (Stegmüller 1971), there was a reference to (Lakatos & Musgrave 1970) but not to *Structure*. (Stegmüller 1969) and (Stegmüller 1970) made no mention of Kuhn at all. Moreover, (Stegmüller 1971, 16) mentioned Kuhn in an entirely negative tone, speaking of a conception that “*certainly cannot be an approach to philosophy of science*”. For a more detailed treatment of Stegmüller’s movement toward Kuhn and Sneed, see (Dahms 2010, 310-332).

⁹ See the excerpt from a letter to Mayer-Knaupp from Springer Verlag, dated 07-10-1971, in (Dahms 2010, 316). In a personal communication, Carlos Ulises Moulines informed me that Sneed’s book and the writings of Kuhn and Lakatos had been studied in the context of a DFG project on “theoretical concepts”. It seems likely that Stegmüller obtained information on these philosophers from work in this project. It is also remarkable that Stegmüller and his students started to study Sneed’s book immediately after its appearance. The reason for this rapid reception was probably that Walter Höring, who was Stegmüller’s assistant at the time and who had been in close contact with Sneed since 1963, had commented on Sneed’s manuscript (personal communication from Sneed and Höring).

¹⁰ In a letter to Yehoshua Bar-Hillel, dated 02-19-1973, Stegmüller still wrote exclusively about Sneed, and about how he felt concerned “a little bit with the dynamics of theories and with what I call the ‘non-statement view of theories’”. There was no mention of Kuhn yet.

¹¹ (Dahms 2010, 315) also suggests that this radical movement in Stegmüller’s thought after 1970 may have had something to do with the death of Carnap, which may have led to a theoretical re-orientation in the pupils of Carnap, such as Hempel and Stegmüller. This inference is supported by the fact that even Hempel, a close friend of Stegmüller for decades, welcomed Sneed’s book (see, for example, Bar-Hillel’s letter to Stegmüller, dated 02-28-1973) and accepted many of Kuhn’s ideas, more or less at the same time when Stegmüller also converted to Kuhn.

¹² See (Stegmüller 1976, 215), where these “rationality gaps” are singled out in the following way: “Two points in Kuhn’s conception remain unexplained. First, how it is that the older, dislodged theory had been *successful*. Second, how we come to speak of *progress* in the case of scientific upheavals.” Both points, according to Stegmüller, are to be explained by means of the structuralist notion of reduction.

¹³ Stegmüller to Kuhn, 06-09-1975: The minor point that approximation may cover is the fact that some of the intended applications of an old theory may just approximately apply in the context of a new theory (e.g., Newtonian physics in the context of relativistic mechanics). For more information on approximation, as invented by Moulines, see (Balzer et al. 1987, ch. VII) and the references cited therein.

¹⁴ Stegmüller initially planned to publish some notes on these discussions in *Erkenntnis* (Stegmüller to Kuhn, 10-15-1975.). Unfortunately, they were never published, and we could find no traces of such a project in Stegmüller’s Nachlass.

¹⁵ This impression was supported by Moulines in a personal communication. Sneed told me that he had been somewhat awed by the large audience, but could not recall any sharp disagreements in the discussions after the talk. Aant Elzinga, who was in the audience, remembers that the whole event was disappointing insofar as there was almost no reaction (information gained in personal communication with Aant Elzinga). The latter impression seems to be the most likely reason for the disappointment of Stegmüller and Kuhn. The philosophy of science community did not react with hostility to their approach but, rather, ignored the whole thing.

¹⁶ Hilary Putnam, who seems to have missed this event (this, at least, is the information Putnam gave me in an email dated 26 March 2012), might have found here a combination of two of his most loathed concepts, namely the “relativism” of Kuhn’s “historical turn” and the “useless formalism” of a “semantic” philosophy of science in the Carnap tradition. Unfortunately, there is no documentation of Putnam’s reaction to “Kuhn Sneedified”. At any rate, in the aforementioned email, Putnam emphatically underlined the impression that the latter account appeared to be totally at odds with his own philosophical convictions.

¹⁷ The occasions for these exchanges were the publications of (Stegmüller 1979) and (Stegmüller 1986).

¹⁸ Besides (Stegmüller 1973), there are three more papers that represent “Kuhn Sneedified” in its full-fledged form – namely, (Stegmüller 1975, 1976a, 1978). It is important to note that the latter two papers were contributions to the conferences that took place in 1975 – namely CLMPS in London, Ontario (August 1975) and a workshop “on topical problems in philosophy and history of science” in Kronberg, near Frankfurt (July 1975). Cf. the proceedings (Butts and Hintikka 1977; Radnitzky and Andersson 1979).

¹⁹ In *Architectonic*, for example, only normal science is discussed extensively, whereas revolutionary science and incommensurability issues are somewhat hidden in the chapter on “reduction”. A more detailed discussion of the incommensurability issue is provided by (Moulines 2000). Recently, the situation obviously started changing. For example, at a structuralism workshop in Munich, in 2012, there were four presentations exclusively concerning the problem of incommensurability. See <http://posmunich.wordpress.com/>.

²⁰ We mainly point to those research communities here, who are interested in Kuhn and Feyerabend from a more philosophical point of view (as part of the HOPOS community). Examples for the literature in question are mentioned in the following note.

²¹ The only real exception is (Dilworth 2008). In (Hoyningen-Huene 1993), Sneed and Stegmüller are mentioned, but “Kuhn Sneedified” is not discussed with any detail. In (Bird 2000; Sankey 1993; Soler et al. 2008; Hoyningen-Huene and Sankey 2010), “Kuhn Sneedified” is not even mentioned. In (Horwich 1993), Sneed and Stegmüller are only mentioned by Kuhn himself. In (Nickles 2003), Sneed and Stegmüller are mentioned just once in the contribution by Richard E. Grandy (p.251) as further examples for criticisms of the “received view” of philosophy of science.

²² Ignorance may hardly be identified as a one-sided phenomenon. Indeed, the structuralists also ignored the Kuhn- and Feyerabend-community. Whenever we find a discussion of problems of *Structure* in the classical structuralism literature, these problems are discussed without any reference to the respective Kuhn literature, except for the references to *Structure* and, of course, to (Kuhn 1976) and (Feyerabend 1977).

²³ Belief revision was developed a couple of years later than the other two formalisms. Thus, there could not have been any discussion of it during the relevant period in the 1970s. Apart from this, there can be little doubt that the overall philosophical attitude of belief revision is quite similar to that of structuralism, as both accounts are “relativist” conceptions, which do not take into account the question of the truth of a theory at all.

²⁴ The only correspondence-like aspect we find in structuralist formalism is a formulation of empirical adequacy. Cf. the “empirical claim” of a theory in (Balzer et al. 1987, 91).

²⁵ The representative handbook of philosophical logic is (Gabbay and Guentner 2001); the subject appeared in its first edition between 1983 and 1989. The main journal of that field is the *Journal of Philosophical Logic*, which first appeared in 1972.

²⁶ There were always some more specialized contexts that left scope for more formal work, even in the US. Philosophers, such as Patrick Suppes, Wesley Salmon, Clark Glymour, and John Earman, worked in these more formalist niches, even in the 1970s. An introductory textbook by (Salmon et al. 1992) covers this more formalist perspective of philosophy of science in the US. However, even these philosophers used formal methods in a way significantly more informal than that of “Continental European style” formal philosophers of science, and none of them ever developed an entirely formal framework that may be compared to “Continental European style” conceptions in terms of formal effort. Thus, it is not surprising at all that even philosophers, such as Suppes, Glymour, and Earman, identified structuralism as an “over-formalization”. Moreover, the mainstream conceptions of philosophy of science in the US were always almost entirely non-formal. See, for example, the standard anthologies, (Feigl and Brodbeck 1953) and (Boyd et al. 1991), both of which barely cover formal approaches of any kind. For example, there is no systematic treatment of logic and other fields of mathematics at all. One reason for this lack of affinity with formal methods may be the strong pragmatist tradition in the US, while another reason may be the institutional dynamics that led to the establishment of philosophy of science in the context of the respective centers in Pittsburgh and Minnesota. On the latter aspect, cf. (Stadler 2010a).

²⁷ Note also that this anti-relativist attitude was adopted even by anti-realists such as Bas van Fraassen and Larry Laudan. For example, (Laudan 1977) developed an anti-realist picture of the dynamic of the sciences, which nevertheless is also anti-relativist (and anti-Kuhnian).

²⁸ For van Fraassen, the structuralist option was definitely *too relativistic* because his constructive empiricism established some sort of a “partial realism”. The latter implied that scientific theories had to be true to the phenomena (empirically adequate). However, structuralism does not even imply such a minimal commitment. Cf. (van Fraassen 1989, 190ff). On the other hand, “heretical” philosophers, such as Bas van Fraassen, Ronald Giere, or Nancy Cartwright, certainly showed a more positive attitude toward structuralism. Cf. the references provided in footnote 4, p.3 above. That is to say, these philosophers appreciated structuralism for its formal merits, say, as a purist example of a formal framework account in the logical empiricist tradition. At the same time, however, they did not find their philosophical arguments supported by structuralism in any substantial way.

²⁹ Note also that, since the structuralist program was an approach to *general* philosophy of science, it should not be considered surprising that philosophers of science interested in the *special sciences* were not particularly interested. However, structuralists did make at least some effort to get involved in some topics bearing upon the special sciences, such as philosophy of psychology and philosophy of economy (for an overview, see Stegmüller 1986). At any rate, these efforts remained somewhat isolated and hardly received any attention outside the structuralism camp.

³⁰ The explanation why the later version of “structuralism” as mainly developed by Sneed, Balzer, and Moulines was not successful in the US seems to be much simpler than the reason for the lack of success of “Kuhn Sneedified”: the later version of “structuralism” is a typical example of “Continental European style” formal philosophy of science, whose manifestations never were taken seriously in the US because they were considered to be too formal (cf. section 2b).

³¹ As a reviewer of this paper suggests, one may count these purifications as examples for the later Kuhnian model of evolutionary speciation of scientific disciplines. At any rate, it is not the aim of this paper to provide an explanation for the phenomenon of purification as exemplified here.

³² See (Damböck submitted).