

# "Et tu mi fili, Thomas?!"

Mátyás Brendel

Budapest University of Technology and Economics, Ph.D. program  
"History of Technology, Engineering and Science"

**Abstract:** In this paper I use or refer to a lot of metaphors, like "Brutus" for Kuhn, "Trojan Horse" for the Structure, "adopted sons" for Quine and Popper, "battlefield" for parts of philosophy. These are however only illustrations to possible theses or questions I investigate: "Did Kuhn refute logical positivism?", "Why was Carnap so tolerating so much Popper, Quine and Kuhn?", "Why did logical positivism go away?". I will argue that a complex view should be developed about these questions synthesising both Friedman's, Irzik's, Earman's, and Reisch's view on the one hand and Oliveira's on the other hand, who is criticising them. I try to sketch this complex view, which of course is not an attempt to a full story of logical positivism. Finally I describe Carnap's Principle of Tolerance as not merely a theory, but as a personal, ethical view, which he adopted in his behaviour with Kuhn, Popper, Quine and other rival philosophers.

## The "adopted sons" of Carnap

The first significant critique of logical positivism was Karl Popper's first book titled: „Logik der Forschung" („The Logic of Scientific Discovery") (Popper 1934), which was published, surprisingly, by the Vienna Circle itself, with the support of Carnap. Popper was regarded because of this - and because his view were really not so far from the views of logical empirism – as "the inner critic of the Vienna Circle". Carnap had a great personal impact on the young Popper as he writes it himself (Popper 2002, p. 342).

The main critiques of Popper were reflected by the Vienna Circle, and they partly modified their views. Carnap for example modified the thesis of verification to the thesis of confirmation, although this is not exactly the same as Popper's thesis of falsification. Popper's other critique about the protocol-sentence-debate helped Carnap to clarify his view, or even to strengthen his view against other members of the Vienna Circle. Carnap writes in his autobiography, which was included in "The Philosophy of Rudolf Carnap":

“These views [about protocol sentences, B.M] of his helped in clarifying and strengthening the physicalistic conception I had developed together with Neurath.” (Schilpp 1991, page 32)

Popper also criticized the concept of induction, which was a thesis of Carnap. Carnap had always defended his view. So there was also an acceptance and also an outstanding discussion between them. In summary, Popper and Carnap had a quite rational dispute until the end of Carnap's life.

Carnap repeatedly argued that the differences between him and Popper were not so great:

“Among philosophers in Vienna who did not belong to the Circle I found the contact with Karl Popper most stimulating, first by my reading of the manuscript of his book *Logik der Forschung*, and later in discussions with him. I remember with pleasure the talks I had with him and Feigl in the summer of 1932, in the Tyrolean Alps. His basic philosophical attitude was quite similar to that of the Circle. However, he had a tendency to overemphasize our differences. (...) Feigl and I tried in vain to effect a better mutual understanding and philosophical reconciliation.” (Schilpp 1991, p. 31)

Popper regarded himself much more as a killer of logical empiricism<sup>1</sup>, but he gives us a similar account later in this book about the same days in the Tyrolean Alps, and then he continues:

“I found in Carnap not only one of the most captivating persons I had ever met, but also a thinker utterly absorbed in, and devoted to, his problems, and eager to listen to criticism. (...) his intellectual courage in changing his mind, under the influence of criticism, even on points of fundamental importance to his philosophy.” (Schilpp 1991, p. 184).

Another milestone is Quine's paper „Two Dogmas of Empiricism” in 1951, which discusses first the distinction of analytic and synthetic, and then includes an early version of Quine's naturalistic, holistic epistemology. Nevertheless, Quine was also an advocate of Carnap, moreover, he was his student in Prague in 1934. Quine remained a friend of Carnap and later became an Honorary Consulting Editor of the Yearbook of the Institute Vienna Circle.

Carnap's and Quine's views diverged as time went by and they had a long discussion on this in their letters and publications. But this discussion was also a rational one with mutual respect. Richard Creath writes in “Dear Carnap, Dear Van, The Quine-Carnap Correspondence and Related Work”:

“Quine began his career as an enthusiastic supporter of Carnap, but over the years their path diverged. Quine became, in fact, Carnap's deepest, most persistent, yet most sympathetic critic. The disagreement emerged only very slowly, but those differences proved to be fundamental. Even the principals did not initially suspect how much so. But the story of Carnap and Quine is no tale of a friendship gone sour. Alongside and through the controversy there remained an abundant affection and respect on both sides. Quite possibly the conflict even deepened their friendship and vice versa. This is not always the case among philosophers...” (Creath 1990, p. 1.)

## **Carnap and the "Trojan Horse"<sup>2</sup>**

Following this, in the 50's and 60's logical empiricism was still present in the philosophy of science, despite the fact that Popper's school was coming more in focus. A common view is that the “Structure of Scientific Revolutions” (henceforth Structure) of Kuhn was the final cut to logical positivism. But even the case of Kuhn had a similar pattern. Although Kuhn was not a member of the Circle, nor a friend or student of Carnap or any other member, the book was still published in the International Encyclopaedia of Unified Science series, which was the creation of Neurath and where Carnap was an editor at that time.

According to the received view<sup>3</sup>, this book killed logical positivism, which is very surprising. We could imagine that Carnap would call out to Kuhn, like Caesar did to Brutus: "Et tu mi fili, Thomas?!". But nothing like this happened. The reasons are - among others - the personal qualities of Carnap.

---

<sup>1</sup> In this paper I use the terms "logical positivism", "logical empiricism" and "neo-positivism" as referring to the same philosophical school, without distinctions of possible differences or sub-schools.

<sup>2</sup> I read the term „Trojan horse” in Oliveira, 2002, but it was also used by Toulmin in S. Toulmin: “Cosmopolis: The Hidden Agenda of Modernity”, 1990 p. 84, and Borradori in G. Borradori: “The American Philosopher : Conversations with Quine, Davidson, Putnam, Nozick, Danto, Rorty, Cavell, MacIntyre, Kuhn”, University Of Chicago Press, 1994, p. 153.

<sup>3</sup> I will call the view that Kuhn did kill logical positivism as the received view in the following. Logical positivism is called itself as the received view by Suppe, 1974. I will not use the term "received view" to refer to logical positivism.

In 1991 George Reisch published a paper with the title „Did Kuhn Kill Logical Empiricism?“. We know from this paper that Carnap wrote to Kuhn two letters about his Structure with a positive attitude towards it, so Carnap did not regard Structure as dangerous to him. But Kuhn is regarded as the “murderer” of logical positivism, meaning that if Structure was not a killer knife, then it must have been at least a “Trojan Horse”, or maybe the metaphor of a bomb is better in some aspects. Maybe this bomb was not even intended to detonate so loudly and so devastatingly. The fact that Kuhn often had to defend himself against irrational interpretation of the Structure (Kuhn 2000b, p. 91, p. 126.) confirms this. Moreover, later Kuhn was returning to even less radical views (Bird 2004). All of these have to have some explanation, and it should be more complex than the above mentioned received view.

## Revisionists?

The first kind of explanation of this Kuhn-phenomenon was given by Reisch (Reisch 1991) himself. According to him the views of Kuhn and Carnap are much closer than believed by the received view. Reisch states that Kuhn's Structure is based on logical positivism. He shows that some of the philosophers in the Vienna Circle, like Neurath, for instance, emphasized the historical viewpoint, just like Kuhn. He also states that some of the most important theses of Kuhn, like incommensurability, the theory-ladenness of observations are already present at Carnap. In the opinion of Reisch Carnap's theory about internal-external distinction and linguistical framework corresponds to the paradigm-theory of Kuhn. Both are capable of explaining similar phenomena related to scientific revolutions. Kuhn's theory of normal science corresponds roughly to Carnap's “normal” theory of science.

The positive reception of Kuhn by Carnap is thus explained according to Reisch. This idea was continued by Earman, Irzik and Friedman, all of whom published similar ideas, with similar conclusions. Although Irzik had a small dispute with Earman on the question if the theory ladenness of observation was maintained by the post-Aufbau Carnap. Nevertheless all these philosophers give a detailed analysis of the parallels between Carnap, some other logical positivists and between Kuhn.

They emphasize different aspects, but the conclusions are similar. Naturally, these authors are not simply repeating the very same idea, but they have different goals with their analysis.

The goal of Reisch is to publish and explain Carnap's letters to Kuhn, and to introduce some kind of reconsideration of the relation of Kuhn and Carnap. He describes the views of Kuhn and Carnap to be similar, but not to be identical. Moreover, he re-opens and leaves open the question why logical empirism was over.

Earman (Earman 1993) could build on an already introduced problem and gives a more detailed analysis. His goal is to base the theory of Kuhn and Carnap to a Bayesian foundation. He also writes about the similarities of the theories of Kuhn and Carnap. In his introduction however, he emphasizes that he does not have any revisionist goals and he is not an advocate of neo-positivism.

Irzik (Irzik 1995, forthcoming, 1999) could already base his analysis on his predecessors, so it is again very detailed. He describes the views of Kuhn and Carnap to be similar and complementary.

Friedman (Friedman 1998, 2001, 2002a, 2002b) has a similar conclusion, he tries to show the neo-Kantian views in both Carnap's and Kuhn's philosophy.

Oliveira (Oliveira 2002) already criticizes the authors mentioned above in this chapter and calls them revisionists. He mentions the fact that Carnap did not refer to Kuhn in his writings. He explains the letters published by Reisch with Carnap's view of not regarding history of science as philosophy, and that he accepted Kuhn's book because he did not regard it as a competing view. According to Oliveira, Carnap may have regarded Kuhn's book simply as a

good history of science in the context of a sharp discovery-justification distinction, as sustained by logical positivists. I will mostly agree with this.

Let us examine the term "revisionist". "Revisionism" has at least two sense. The first one is "re-consideration". It is obvious that Reisch, Earman, Friedman and Irzik re-consider or re-evaluate logical positivism, as for example this is the title of one of Friedman's book and Irzik's paper respectively. On the other hand Earman's quote was protesting against revisionism. This seems to be a contradiction at the first glance. But it is not, since this is connected to another meaning of "revisionism".

Revisionism may also mean that these philosophers would like to restore, revive this philosophical school, which is obviously not the case. Earman denied revisionism only in this second sense. I called and will call these philosophers as "revisionists", since "re-considerists" or "re-evaluationists" do not sound good. To indicate that I mean the weaker sense of the word, I always put it in quotation marks.

It seems to be obvious that in a weaker sense some degree of re-examination is needed, and some work remains to the future, as for example Earman indicates it. Oliveira and the "revisionists" may disagree on the degree of revision needed. I will try to draw some conclusion on this.

## History and philosophy of science

If we consider the facts Oliveira presents, we can confirm him: Carnap did not refer to Kuhn in his writings, and Structure was categorized as history of science in the Encyclopaedia. We can even discover some more supporting evidence.

Neither did Kuhn refer to Carnap in Kuhn 1962, and only a single, non-critical reference can be found in Kuhn 1977, p. 259, note 29 (henceforth "Tension"). Kuhn refers once to Popper in Structure (p. 146), indicating that Popper's view were more in focus by that time than Carnap's. He also refers to Ernst Nagel's "Principle of Probability" (p.145); this being the only reference to a logical positivist philosopher.

Kuhn mentions first Carnap only in the "Afterwords" of 1993 (henceforth "Afterwords"), as a reflection to the mentioned paper of Earman, which is in the same volume. Let us take a quote from the Afterwords:

"Whatever role the problems encountered by positivism may have played in the background for The Structure of Scientific Revolutions, my knowledge of the literature that attempted to deal with those problems was decidedly sketchy when the book was written. In particular, I was almost totally innocent of the post-Aufbau Carnap, and discovering him has distressed me acutely." (Kuhn in Horwich 1993, p. 313).

Here, Kuhn himself admits that he could not possibly directly refute Carnap's philosophy, since he was not even familiar with the recent developments at that time. If Kuhn had refuted or intended to refute Carnap's philosophy, then he would had to know it properly. Let us also quote the case with Popper:

"Though I have read none of Sir Karl's work before the appearance in 1959 of the English translation of his Logik der Forschung (by which time my book was in draft), I had repeatedly heard a number of his ideas discussed. In particular, I had heard him discuss some of them when he was William James Lecturer at Harvard in the he spring of 1950." (Kuhn, 1977, 267, note 2).

Now, it seems that Kuhn's knowledge about Popper at the time of writing Structure could even be called sparse.

These facts do not amount to such a serious accusation, if we suppose that Kuhn's intention was not to refute logical positivism or Popper's rationalism, and he did not regard his views firstly as a refuting theory in philosophy of science.

This again underlines the thesis of Oliveira: Structure is not a refutation of Carnap, because it is mainly on a completely other topic: history of science, not philosophy of science. With Structure, Kuhn induced the historic turn, rather than a refutation of some philosophical view. Kuhn himself admits this:

"But it may be described more generally, as a study of the problems raised by the transition to what's sometimes called the historical and sometimes the (...) "soft" philosophy of science." (Kuhn 2000b, p. 90.)

"If he means that the generalizations which constitute received theories in sociology and psychology (and history?) are weak reeds from which to weave a philosophy of science, I could not agree more heartily." (Kuhn 2000b, p. 128.)

This second quote also has the consequence that a philosophical theory can not be simply and directly refuted by history of science.

There is a paper in Tension supporting the idea that Kuhn himself considered history of science and philosophy of science to be distinct:

"Few members of this will need to be told that, at least in the United States, the history and the philosophy of science are separate and distinct disciplines. Let me, from the very start develop reasons for insisting that they be kept that way." (Kuhn 1977, p. 4)

He describes the historian as telling stories and giving narratives, and the philosopher as follows:

"The philosopher, on the other hand, aims principally at explicit generalizations and at those, with universal scope. He is not a teller of stories, true or false." (Kuhn 1977, p. 4)

And later:

"By the same token, when speaking about philosophy of science, I have in mind neither those positions that shade over to applied logic, (...) Rather I am thinking of the central area that concerns itself with the scientific in general, asking, for example, about the structure of scientific theories, the status of theoretical entities, or the conditions under which scientists may properly claim to have produced sound knowledge." (Kuhn 1977, p. 12)

Taking this description, it is a good question if Structure is history or philosophy of science. For Kuhn it cannot possibly be both, since his opinion is that a scholar can not do both at the same time.

"Finally, and perhaps most important of all, limitations of space have drastically affected my treatment of the philosophical implications of this essay's historically oriented view of science. Clearly, there are such implications, and I have tried both to point out and to document the main ones. But in doing so I have usually refrained from detailed discussion of the various positions taken by contemporary philosophers on the corresponding issues. Where I have indicated skepticism, it has more often been directed to a philosophical attitude than to any one of its fully articulated expressions. As a result, some of those who know and

work within one of those articulated positions may feel that I have missed their point. I think they will be wrong, but this essay is not calculated to convince them. To attempt that would have required a far longer and very different sort of book." (Kuhn 2000a, p. x).

Taking this description of Kuhn, in the preface of *Structure*, the answer is clearer. *Structure* was intended to be a mostly historical book, with some potential philosophical consequences, which had not been articulated well enough in the book. It could not kill logical positivism, since it was "not calculated to convince" somebody about the philosophical consequences. It seems that the later works of Kuhn were also not suitable for this reason.

Carnap's opinion is a bit different about history and philosophy of science. For Kuhn, philosophy of science seems to be descriptive, for Carnap, it is mainly normative. For Carnap, philosophy of science is logical analysis (Carnap 1936, p. 42-43), (Carnap 1937, p. 331.), which was not proper philosophy for Kuhn.

"I opened by telling the class that my object was to show them the extra benefits of putting to work, within the historical or developmental approach to philosophy of science, some of the splendid analytic tools developed within the more static logical-empiricist tradition. I continue to think of my philosophical work as pursuing that goal." (Afterwords, pp. 312-313).

For Carnap the distinction of discovery and justification is important, for Kuhn it seems not so important. We have seen that Kuhn emphasizes the distinction of philosophy and history, but this distinction is based on a different degree of generalization. For Carnap the same distinction is based on the discovery-justification distinction.

So there is a shift between Carnap and Kuhn about this. Carnap classifies philosophy much closer to logic than Kuhn. There is no doubt that for Carnap, *Structure* was much more history and not philosophy. It is not important to decide, if Kuhn's or Carnap's view is "true", since there is no truth regarding this: it is a question of consensus. The important fact is only the shift in the topic in focus in Kuhn's and Carnap's work respectively: from logic to history, from the context of justification to the context of discovery.

Carnap was excited by *Structure*, but he did not refer to it. Another reason for this is that *Structure* had a non-formal, rhetorical, metaphorical style<sup>4</sup>, which was probably useless for Carnap. There was no logical model in it, which he could analyze.

On the other hand, there seems to be a small part in *Structure*, which is philosophical, for example the already mentioned pages 145 and 146. Here Kuhn describes two schools, the one of Popper and the other unnamed one represented only by "The Principle of Theory of Probability", by Nagel 1939. Kuhn makes some remarks on these schools in the way they try to solve the evaluation of scientific theories.

Nagel's book is about probability and the topic of confirmation of theories is just a chapter in the book, so it does not seem to be the best reference. This one chapter describes the attempts to create a theory of confirmation, and describes the unsolved problems. Nagel is also quite sceptical about the formal, numerical definability of degree of confirmation. Carnap's "Testability and Meaning" is referred to by Nagel, while Kuhn refers only explicitly to it later in "A Function for Thought Experiment" (1964), (Kuhn 1977, p. 259, note 29).

Moreover, Nagel's book appeared 23 years earlier than *Structure*, and Carnap's essay 26 years earlier, so they were really not the appropriate references to refute contemporary logical positivism. There are many writings of Carnap before *Structure*, which would be essential for an appropriate critique. Without the attempt of completeness, see for example: Carnap 1936, Carnap 1950, Carnap 1952, Carnap 1962b.

---

<sup>4</sup> For the significance of style see Wolters 2004.

Kuhn's remarks in *Structure* are not purely critical, the criticism that is present is very moderate. The critique is about the presupposition of natural observation language and about verification, which thesis was not held any more at this time by Carnap and not used at all by Nagel. The fact that Kuhn uses the word "verification" indicates his hiatus in preparation. Even the idea of comparing theories to each other instead of evaluating them solely, is not new for Carnap at the time of *Structure*.

In a summary about this question, we can conclude that in *Structure* Kuhn's did not seriously criticise logical positivism, it was not his intention and if it had been, the references and arguments would not have been sufficient for this.

Kuhn describes in several places some (probably several) kind of "received views" without exactly identifying them or giving references and quotes. In Horwich 1993, p. 312 Kuhn uses the term "received view", probably to describe positivism and logical positivism. He uses the term "positivist formulation", but without an exact reference.

In Kuhn, 2000b, pp. 106-108 logical positivism is named, without reference, then some kind of seventeenth century view is described, without referring, quoting or naming philosophers, then a view, which is called "classical philosophy of science" by Kuhn. The last one is probably logical empirism again. Now, this classical philosophy of science is described as "dimly aware of" the views Kuhn introduced, and Kuhn sates that nothing was really new of his views. All this is some kind of description of the philosophical background, on which Kuhn's "revolution" developed. So these are the criticized views Kuhn is supposed to refute.

Now, it seems that the adjective "dimly" is more suitable for Kuhn's argumentation, since it is not clear which views of which philosophers his new approach is opposing. As a reconstruction of his vague argument we could formulate it as follows: Kuhn is opposing some kind of 17. century empirism, and differing from logical positivism mainly in the fact that he lays more focus on some already known theses.

One reason for the vagueness of his argument is that - as we had seen – Kuhn's knowledge about contemporary logical positivism and Popper's view was superficial, which is a serious defect of Kuhn's work. The other reason may be that Kuhn did not want to refute any of these views directly.

To be sure, there are some theses, where Kuhn explicitly opposes Carnap, in later works also referring him, for example in *Afterwords*:

"Carnap emphasized untranslatability as I do. But, if I understand Carnap's position correctly, the cognitive importance of language change was for him merely pragmatic." (Kuhn in Horwich 1993, p. 314.)

This difference may be significant to distinguish Kuhn's philosophy from Carnap's, but it is not enough to state that Kuhn killed logical positivism, especially not with *Structure*. It may be rather stated that there is a direction in Carnap's philosophy, which is quite close to Kuhn's; there is however a difference, which is enough to regard Kuhn as a founder of a new kind of philosophy, without refuting the previous one. The difference is mainly of shifting the topic, style, attitude, and focus: Kuhn admits for a particular topic that "none of the things I've just said was quite new" (Kuhn 2000b, 108), and I think, this can be said in general as well. The difference is rather that for logical positivism Kuhn's theses were in another field, not significant for philosophy, they were however basically significant for Kuhn.

As a conclusion of this chapter I summarize my theses. (i) Both Carnap and Kuhn regarded philosophy of science and history of science to be distinct disciplines (with possible connections), and this is also my opinion. (ii) Carnap probably regarded *Structure* as mainly a book in history of science (Oliveira gives support for this), while Kuhn regarded it as a historical book in which he also sketched up some philosophical consequences. (iv) *Structure* contains also some philosophical theses, which are however partly not inconsistent with

Carnap's theses, but congenial with Carnap (as the "revisionists" points this out). (iv) For the remaining smaller part, where Kuhn may oppose Carnap, Kuhn admitted that he was not sufficiently prepared and did not come up with a convincing work to refute another philosophical view. So Kuhn did not directly kill logical positivism, especially not with Structure. (v) Kuhn rather and mainly induced the so called "historic turn" than refuted logical positivism.

## **Categorising Philosophical Schools**

There is an interesting remark in Oliveira's paper (Oliveira 2002, p. 9). Popper has been seen for a while as a logical positivist, Quine and Kuhn has never been seen as such. Both of them were supported by Carnap in a way. Quine was probably personally closest to Carnap, he regarded Carnap as a teacher. Quine was also an Honorary Consulting Editor of the Yearbook of the Institute Vienna Circle. There are similarities and differences between the views of Kuhn, Quine and Popper on the one hand and Carnap on the other hand. Finally, none of them are regarded logical positivists, except of Carnap. As Irzik indicates, there are even more, very significant similarities in the historical and social views of Kuhn and the views of Neurath, Frank and Zilsel (Irzik 1999, p. 56), who are however considered logical positivists despite quite significant differences with Carnap.

Now, what are the criteria for considering somebody a logical positivist? It seems that defining it with the membership of the Vienna Circle is too simple. It is also quite hard to find a thesis held by all of the logical positivists. It would also be hard to define a set of theses which would define logical positivism as a sufficient and required set of views.

Logical positivism, as any other philosophical school, is only a rough category of various philosophers. Since the category is not exactly defined, there may be differences inside the school and similarities with other schools. In philosophical debates we must not forget that it is only a rough category. That is why I find that such titles as "Did Kuhn kill logical positivism" (Reisch) or "Carnap and Kuhn: Arch Enemies or Close Allies?" (Irzik & Grünberg) are suggesting some over-simplification.

This is my answer to the Oliveira versus "revisionists" debate. I agree that there is much more similarity between Carnap and Kuhn than was earlier assumed. I also agree with the "revisionists" that Kuhn did not refute logical positivism. Nevertheless, I also agree with Oliveira that the reason is not that Kuhn was congenial with Carnap; but that Kuhn induced a historic turn, not a refutation. I further agree with Oliveira that there are significant differences between Kuhn and Carnap, which we must not forget.

In order to investigate such questions like the comparison of Carnap and Kuhn it is in my opinion worth to forget about philosophical schools and other over-simplified categories, like "enemy" or "ally". They will only bring endless and fruitless debates. One must not be afraid to formulate a complex picture with many shades.

In my view, Carnap and Kuhn were not allies nor enemies. If we stick to the martial metaphor, then they did not even "fight" on the same "battlefield" mostly. But the martial metaphor is not good at all, since philosophies do not always fight with each other, sometimes a competition is going on, without a direct fight with each other, sometimes a cooperation. There are moreover many projects going on, which are not in contradictions with each other, but sometimes fighting for resources, attention. Kuhn temporarily won this fight over logical positivism: he got huge attention.

## **The Principle of Tolerance as a practice in Carnap's life**

Returning to the question about that pattern of the "adopted sons" of Carnap. Carnap supported Popper, Kuhn and Quine among others and although he certainly did not see the



future, he must have seen the possibility that these young philosophers may become famous and may criticize him. On the other hand, he may have hoped to have a fruitful discussion with them. As a rationalist and tolerant philosopher, he must have accepted this. Carnap's Principle of Tolerance was not merely a theory, but a personal, ethical view, which he adopted in his behaviour with Kuhn, Popper, Quine and other rival philosophers.

There are many philosophers who maintain a rational and tolerant view, but there are many of them, where this tolerance does not appear in their personal live. In the case of Carnap the remarkable thing is that his tolerance was not merely a word, but also a practice.

To finish my paper with a last historical metaphor<sup>5</sup> we may say that Carnap's Achilles Heel was neither this tolerance, nor the criticism against his work. Carnap always managed to benefit from critiques, develop his theories. Carnap's armour was logic, but it was not a rigid armour: it was a chainmail of an exact structure, which could bend to critique and "reality". I hope that Carnap would forgive me the use of this metaphysical statement as a metaphor.

## Acknowledgement

I wish to thank for the Department of Philosophy and History of Science at Budapest University of Technology and Economics, where I am working on my PhD, especially for my supervisor Márta Fehér. I'd like to express my gratitude to Prof. José Carlos Pinto de Oliveira for his extensive comments and consultation. Finally, I thank to Géza Bohus for editing my manuscript

## References

A. Bird: „Kuhn, naturalism, and the positivist legacy”, *Studies in History and Philosophy of Science - Part A*, 35, 2, 337-356, 2004.

R. Carnap: "Truth and Confirmation", Adapted and reprinted in H. Feigl, W. Sellars (eds.), "Readings in Philosophical Analysis", Appleton-Century-Crofts Inc, 1949, (first publication 1936).

R. Carnap: „The logical syntax of language”, Open Court, 2002. (first edition 1937).

R. Carnap: "Logical Foundation of Probability", University of Chicago Press, 1962a, (first edition 1950)

R. Carnap: "The Continuum of Inductive Methods", University of Chicago Press, 1952.

R. Carnap: "Probability and Content Measure", Reprinted and revised in P. Feyerabend, G. Maxwell (eds.) "Mind, Matter, and Method", University of Minnesota Press, 1966, (first publication 1962b).

R. Creath (ed.): „Dear Carnap, Dear Van”, University California Press, 1990.

J. Earman: "Carnap, Kuhn, and the Philosophy of Scientific Methodology", in P. Horwich (ed.) "World Changes", pp. 9-36, MIT Press, Cambridge, Massachusetts, 1993.

M. Friedman: "Remarks on the History of Science and the History of Philosophy", in P. Horwich (ed.) "World Changes", pp. 37-54, MIT Press, Cambridge, Massachusetts, 1993.

M. Friedman: "On the Sociology of Scientific Knowledge and its Philosophical Agenda", *Stud. Hist. Phil. Sci.*, Vol. 29, No. 2, pp. 239-271, 1998.

---

<sup>5</sup> This metaphor with Carnap's Achilles Heel is related quite confusing with the metaphor of Structure as a Trojan Horse. Unfortunately the Greek-Roman metaphors I used as a complete system does not match to the other history of Carnap and Kuhn. Note that I only use these metaphors as illustrations, so this is not a fault in my argument.

- M. Friedman: "Reconsidering Logical Positivism", Cambridge University Press, Cambridge (United Kingdom), 1999.
- M. Friedman: "Dynamics of Reason", CSLI Publications, 2001.
- M. Friedman: "Kant, Kuhn and the Rationality of Science", *Philosophy of Science*, 69, 171-190, 2002.
- G. Irzik, T. Grünberg: "Carnap and Kuhn: Arch Enemies or Close Allies?", *Brit. J. Phil. Sci.*, 46, 285-307, 1995.
- G. Irzik: "A Re-Evaluation of Logical Empiricism", *Revista Patagonia de Filosofía*, Año 1, Vol. 1, pp. 49-68, 1999.
- G. Irzik: „Changing conceptions of Rationality from Logical Empiricism to Postpositivism”, in *Logical Empiricism*, University of Pittsburgh Press, 325-348, 2003.
- T. Kuhn: „The structure of scientific revolutions”, *International Encyklopedia of Unified Science*, vol 2, no. 2, University of Chicago Press, 2000a, (1962). [Structure]
- T. Kuhn: "The Essential Tension", The University of Chicago Press, 1977. [Tension]
- T. Kuhn: "Afterwords", in P. Horwich (ed.) "World Changes", pp. 311-342, MIT Press, Cambridge, Massachusetts, 1993. [Afterwords]
- T. Kuhn: "The Road Since Structure", The University of Chicago Press, 2000b.
- E. Nagel: "The Principles of the Theory of Probability", University of Chicago Press, Chicago, 1939.
- J. C. P. Oliveira: „Carnap, Kuhn and Revisionism: On the Publication of "Structure" in "Encyclopedia"”, forthcoming, <http://philsci-archive.pitt.edu/archive/00000708/>, 2002.
- J. C. P. Oliveira: “Carnap y el Revisionismo Histórico”. In : Vega, M., C. Maldonado y A. Marcos (eds.): *Racionalidad Científica y Racionalidad Humana*. Valladolid (España): Universidad de Valladolid. ISBN 84-8448-108-5, pp 151-164,2001.
- George Reisch: ”Did Kuhn kill logical empirism?”, *Philosophy of Science*, 58,264-277, 1991.
- P. A. Schilpp (Ed.): „The Philosophy of Rudolf Carnap”, Open Court, 1991. (first edition 1963.)
- F. Suppe (ed.): „The Structure of Scientific Theories”, Urbana, IL: University of Illinois Press, 1974.
- G. Wolters: "Styles in Philosophy: The case of Carnap", in S. Awodey, C. Klein (ed.) "Carnap Bought Home", Open Court, Chicago, 2004.