

Between Enthusiasm and Disenchantment: Ernest Nagel and Logical Empiricism

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1. Introduction. The relation between logical empiricism and American pragmatism is one of the more difficult problems in history of philosophy.¹ This relation cannot be described as a point-like event, but rather as a process that evolved for various decades. Since some time a variety of contradicting narratives about this difficult and complicated issue is on the market.

In this paper I don't want to tell another global story about this issue. Rather, I'd like to take a more local perspective and concentrate on the details that concern the vicissitudes of a philosopher who played an important role in the encounter of Logical empiricism and American pragmatism, namely, the American philosopher Ernest Nagel (1901 – 1985). Although Nagel was one of the most influential American philosophers of science in the middle of the 20th century, he has been unduly ignored in recent debates on the relation between logical empiricism and pragmatism that without doubt is one the crucial episodes of the history of 20th century analytic philosophy.

In this paper, I want to explore some aspects of Nagel's changing attitude towards the „new“ logical-empiricist philosophy arriving from Europe at the shores of the New World in the 1930s. Like many other scientifically-minded American philosophers in

¹ For the sake of simplicity let us assume in the following that there were only two parties of the encounter – the logical empiricists and the American pragmatists. Actually, this is a simplified description of the situation: Reality was more complicated. Science-oriented philosophy in US comprised more than pragmatism, there was an important philosophical current called (Columbia) naturalism, contextualistic naturalism, realism or similarly, closely related to but different from genuine pragmatism (cf. JEWETT (2011, 2012), Kuklick (2001)).

the beginning Nagel welcomed the logical empiricists whole-heartedly.² He was one of the younger American philosophers who had visited the protagonists of European logical empiricism in the early 1930s before they had to emigrate to the US or elsewhere. Moreover, together with Morris and others, he served as a host of the European emigrants, when they had to build up a new life in America. As I want to show, this early enthusiasm did not last. At the end of his philosophical career in the late 1970s, Nagel's early positive attitude towards logical empiricism shown in the 1930s had been replaced by a much more reserved one, to put it mildly. In other words, instead of a global narrative about the general relation between two multi-faceted philosophical currents I propose to pursue a kind of longitudinal analysis of one individual philosopher who played an important role in the encounter of the two movements.

Logical empiricists were present on the American scene, in one way or other, for approximately 50 years, say, from the 1930s to the 1980s.³ Nagel's career as a professional philosopher comprised roughly the same period.

The organization of the paper is as follows. To set the stage, in the next section I briefly recall some of the main contemporary narratives that are told to describe the complex relationship between logical empiricists and American pragmatists in the second third of the 20th century. All these narratives are of a global character. They all paint the relation between the two philosophical currents with a broad brush, hardly taking into account the vicissitudes of individual philosophers. As I want to show they all have difficulties to deal adequately with the case of Nagel. Indeed, he turns out to be an

² Certainly not all, however. For instance, according to Jewitt, J.H. Randall, one of the protagonists of Columbia naturalism, still in 1948 described Carnap as „a Prus-sian systematizer“ who was „relatively insulated from the main currents of American experience and thought“ (Jewitt (2011, 91)).

³ Herbert Feigl came to America in 1931, Carnap passed away in 1970. Feigl and Hempel, often characterized as the „last logical empiricists“, lived until 1988 and 1997, respectively.

interesting challenge for all existing narratives of the relation between logical empiricism and American pragmatism.

In the third section, I'll deal with Nagel's early work that exhibits an unmitigated enthusiasm towards the „new logical empiricist philosophy“. In particular, Nagel's contribution *Charles S. Peirce: Pioneer of Empiricism* (Nagel 1940) to the *5th International Congress for the Unity of Science* in Harvard is treated.

The topic of section 4 is Nagel's growing dissatisfaction with Carnap's version of logical empiricist philosophy in the following decades. This dissatisfaction was clearly expressed in Nagel's criticism of Carnap's inductive logic (Nagel 1963) and more generally in his last book *Teleology Revisited and Other Essays on History and Philosophy of Science* (Nagel 1979).⁴ There he criticized very harshly Carnap's philosophy of science in general as ahistoric and outdated.

One of the distinctive features of Nagel's philosophy of science is the emphasis that he put on the role of history of science for philosophy of science. A compelling evidence for this attitude are his works on the history of geometry and algebra that up to the present day are considered as valuable contribution to the history of ideas. This aspect of Nagel's philosophy of science is treated in section 5. Finally, in section 6 we briefly discuss the question who is the audience of philosophy of science. One may say that Carnap and Nagel represented opposed possibilities of how the profession of a philosopher of science may be understood: Carnap as a „conceptual engineer“ was engaged in the task of inventing the conceptual tools for a better understanding science as a complex endeavor in itself, while Nagel was to be considered more as a „public intellectual“ engaged in the project of realizing a more rational and enlightened society are discussed.

⁴ This book contains Nagel's *Dewey Lectures* that he had delivered at Columbia University in 1977 and may be considered as a kind of *summa* of Nagel's philosophy and history of science.

2. The Encounter of Logical Empiricism and American Pragmatism: A Potpourri of Narratives. The encounter of European logical empiricism and American pragmatism is a complex and multi-faceted event of 20th century history of philosophy. Perhaps the simplest narrative of this event is Rorty's version of a (temporal) replacement of home-spun American pragmatism by logical empiricism. It goes like this:

Along about 1945, American philosophers were ... *bored* with Dewey, and thus with pragmatism. They were sick of being told that pragmatism was the philosophy of American democracy, that Dewey was the great American intellectual figure of their century, and the like. They wanted something new ... What showed up ... was logical empiricism, an early version of what we now call „analytic philosophy“.

The incursion of this kind of philosophy was ... a mixed blessing. ... [I]t represented a temporarily fruitful confusion of a *very* good idea (that language was a more fruitful topic for philosophical reflection than experience) with a couple of rather bad ones (that there was something worth preserving in empiricism; ...). Rorty (1995, 70)

Fortunately, according to Rorty, the eclipse of pragmatism did not last for long. With Rorty himself actively engaged in the project, the eclipse was overcome in the next decades. At least, this is the story that Rorty wanted to make his audience believe:

The narrative I have tried to construct in my books tells how the bad ideas gradually, in the course of the 1950s and 1960s, got filtered out and thus made it possible for pragmatism to get a new lease on life by undergoing linguistification. (Rorty (1995, 70))

A different version of this eclipse narrative is offered by Soames. He skips the second half of Rorty's story, i.e., the alleged resuscitation of classical pragmatism in the form of (Rortyan) *neo*-pragmatism the heroes of which were Dewey, Wittgenstein, and Heidegger does not exist for Soames. In his narrative, even Dewey does not occur (to say nothing about Wittgenstein and Heidegger). According to him, the only contribution

that American pragmatism had to offer to the new analytical wave was the logical achievements of Peirce and C.I. Lewis. As minor figures in the transition from the pre-analytic to the analytic period in American philosophy M. Cohen and Nagel are briefly mentioned (Soames (2008, 451/452)).

Not all people agree with the stories told by Rorty and Soames. Misak, for instance, in her book *The American Pragmatists* (Misak 2013), completely discards the replacement or eclipse narrative. According to her, the logical empiricist invaders were assimilated by American pragmatism in such a way that they hardly left any trace:

One thing, however, should be clear from my account of the fortunes of pragmatism. Those who would argue that pragmatism was bullied into the backwaters by the logical empiricists ... have their intellectual history wrong. Not only were there strong connections between pragmatism and logical empiricism, but the logical empiricists drifted closer and closer to their pragmatist cousins until the views were almost indistinguishable. (Misak (2013, 254)).

These largely incompatible narratives may leave the reader somewhat perplexed, since even the simple question „At the end of the day, who replaced whom?“ does not find an unanimous answer. Even less satisfying is what this potpourri of narratives has to offer for the task of determining Nagel’s position in this changing conceptual landscape. As usual, Rorty’s overall general pastiche is of no use for finding out any detail. Soames mentions Cohen’s and Nagel’s early contributions to a (broadly understood) logical empiricist philosophy of science but ignores Nagel’s later criticism of Carnapian logical empiricism. Misak’s assimilation narrative entails that Nagel should have recognized the later Carnap as a fellow pragmatist. However, as will be shown in the next section, this was not the case.

Rorty, Soames, and Misak offer competing global narratives about the question how the evolving relation of logical empiricism and American pragmatism may be

understood. The following two proposals of Richardson and Uebel are of a somewhat different kind. These authors deal with some more specific aspects of the encounter of the two movements that mainly concern a small group of protagonists. Nevertheless, also for them Nagel's case is a challenge.

Richardson's focus is on Carnap (cf. Richardson (2008)). According to him, the emigration to America and the contact with American pragmatists led Carnap to adopt a kind of *sui generis* pragmatism. As Richardson's rightly points out, Carnapian pragmatism has to be distinguished from „genuine“ American pragmatism. That Nagel did not recognize later Carnap's philosophy as a pragmatism may be read as an indirect confirmation that „Carnapian pragmatism“ was not identical (and perhaps not even compatible) with „genuine“ or „ordinary“ American pragmatism.⁵ Moreover, if Richardson is right (as I think he is), Carnap did not drift “closer and closer to his pragmatist cousins”, and, as will be shown in section 4, Nagel did not see him as someone who drifted towards his “pragmatist cousins”. Other European empiricists such as Frank, Neurath, or Hempel indeed may have drifted. But they do not represent the entire logical-empiricist spectrum.

Still another proposal to understanding the evolution of logical empiricism in America has been put forward by Uebel. His thesis is

that we can ascribe to [the members of the left wing of the Vienna Circle] the conception of a [...] „bipartite metatheory“, a conception of philosophy of science as comprising both formal-logical and empirical investigations. ... This conception constitutes the „unified science“ alternative to Moritz Schlick's Wittgensteinian conception as meaning determination (Uebel (2012, 117)).

⁵ Whether it is expedient to characterize later Carnap's philosophy as a kind of pragmatism is, of course, another question. This question, however, will not be discussed in this paper.

The bipartite metatheory seems to offer an elegant and ecumenical („tolerant“) way of doing philosophy of science everybody should be content with. Indeed, Carnap may be characterized as a partisan of such a theory. In a letter to R.S. Cohen he described the task of such a bipartite metatheory as follows:

For a total (not only logical) theory and analysis of knowledge and science, it is certainly very important to take into account also activities, including (1) the practical behavior of scientists in their research work (this may include pragmatics but goes far beyond it), and (2) the ways in which science is of help in all fields of practical life. I have myself not made any investigations of these kinds; but this does not mean that I regard them as less important. (R.S. Cohen (1963, 150), quoting from a letter of Carnap written to R.S.C. dated 12. August 1954)

The addressee Cohen of this letter was less than fully convinced by Carnap’s answer and objected:

But what is the status of a purely logical analysis of knowledge in a total theory of scientific knowledge, once pure syntactic and pure (formal) semantic reconstructions are left behind? (ibid.)

Carnap seems to have assumed that the logical and the non-logical ingredients of a “total theory” of knowledge and science can be juxtaposed and put together in such a way that they form a conceptual whole. How the purely logical and the non-logical parts of the total theory fit together, how they interacted with each other (if at all), was not a problem for him. Not all of his fellow philosophers were able to conceive the issue in such a relaxed manner. For instance, Nagel was unable to see the conceptual unity allegedly underlying the two components of the bipartite metatheory. He came to the conclusion that Carnap’s version of philosophy of science was a deadlock while he considered philosophers such as Frank, Hempel and others as kindred spirits.

More generally, Nagel is a problem for all existing narratives that aim to explicate the complex relation of logical empiricism and pragmatism: Nagel is a problem for Misak's drifting thesis, since he would have vigorously denied that Carnap had drifted toward genuine pragmatism. Nagel is a challenge for Rorty's version of the eclipse narrative since till the end of his life he stuck to the thesis that science is the basis for a humane and liberal civilization (Nagel (1979, 10)). Very probably, Nagel would not have subscribed to Richardson's thesis that the later Carnap's philosophy of science can be characterized as a kind of pragmatism. Of course, one may contend that Nagel simply got it wrong. But perhaps this is a solution of the problem that is a bit too simple to be right.

3. Early Enthusiasm. In 1939 the *Fifth International Congress for the Unity of Science* took place in Harvard/Massachusetts. Nagel was one of the congress participants. He took the opportunity to remind of the centennial anniversary 1839 of Peirce, presenting him as a „pioneer of empiricism“ who had anticipated many of the insights that contemporary American pragmatism and the empiricism of the Vienna Circle had obtained independently from each other. For him, this kind of convergence (that often occurred in science) was to be considered as evidence that some measure of truth had been attained:

It is therefore a happy sign that so many of the central ideas of the present movement have been independently on both sides of the Atlantic. One is not minimizing the contributions of the Vienna Circle in pointing out that many of its recent views have been taken for granted for some time by American colleagues, largely because the latter have come to intellectual maturity under the influence of Peirce (Nagel 1940 (69/70)).

...

Were [Peirce] still among us he would surely have endorsed the happy marriage of the cultivation of logic and the empirical temper which distinguishes this movement, and he would joined hands with us in furthering the quest for and the understanding of progressively more adequate tools of inquiry. (ibid., 80)

Presenting Peirce as the founding father of empiricism required, of course, a considerable amount of philosophical surgery on the body of Peirce's philosophy. Peirce's highly metaphysical idealist systems had to be extirpated and excluded from consideration. The young Nagel had no qualms to do just this. Even more, according to him, the Vienna Circle's logical empiricism was an expedient tool for improving the central notion of Peirce's pragmatism:

„Peirce's own formulation of the pragmatic maxim leaves much to be desired in the way of explicitness and clarity; and more recent formulations, such as those by Professor Carnap and others, have the same general intent but superior precision. (Nagel (1940, 73)).

Regrettably, Nagel never pointed out more precisely where Carnap had achieved such a remarkable deed, and Carnap never boasted to have done it. Thus, it remained unclear exactly where Carnap had proposed an improved version of the Peirce's pragmatic maxim, to say nothing about the issue whether he had faithfully followed such a maxim in his philosophical work.

Although Nagel's thesis that Carnap had the merit of having improved Peirce's pragmatic maxim may sound a bit overstated, to put it mildly, he was not the only one who claimed a profound affinity between logical empiricism and American pragmatism. Much later Carnap's fellow empiricist Philip Frank confessed that he had had a similar lightning recognition long ago even before the two had arrived in America:

When I read [Carnap's *Aufbau*] it reminded me strongly of William James's pragmatic requirement, that the meaning of any statement is given by its „cash value,“, that is, by what it means for human behavior. I wrote immediately to Carnap, „What you advocate is pragmatism. This was as astonishing to him as it had been to me. We noticed that our group ... had reached conclusions by which we could find kindred spirits beyond the Atlantic in the United States. (Frank (1949, 33)

In recent years, many different quite sophisticated interpretations of the *Aufbau* have been proffered, none of them, however, confirms Frank's reading of Carnap's *opus magnum*. Be this as it may, there can be no doubt that in the 1930s members of both groups were strongly engaged in the project of bringing together the two philosophical movements. Even Carnap may have appeared as someone who had a keen interest in the project that logical empiricism and American pragmatism joined forces. In *Testability and Meaning* (Carnap (1936/37) he jettisoned unnecessary philosophical ballast that hindered a closer alliance with pragmatism: First, he pointed out that methodological solipsism should not be considered as the only possible one, and not even as the best interpretation of the *Aufbau*. *A fortiori*, in no way logical empiricist philosophy of science should be considered as being committed to methodological solipsism in general. Second, he abandoned the overly strong and unrealistic concept of (complete) verification replacing it by (gradual) confirmation. *Testability and Meaning* is often taken as evidence for the emergence of a „new“ flexible Carnapian logical empiricism that subscribed to some essential philosophical theses of American pragmatism. A closer look reveals that this apparent assimilation was far from complete.

Carnap complied with the pragmatist doctrine only in one point, namely, that absolute verification of synthetic assertions was impossible and one had to be content always with a more or less complete confirmation. Other essential ingredients of his doctrine remained intact after his contact with American pragmatism.

Back to Nagel and his approximation to the new logical empiricist wave. In 1939 he published one of the early successful contributions to Neurath's *International Encyclopedia of Unified Science*, namely, the monograph *The Principles of Probability* (Nagel 1939). *The Principles of Probability* became a well-recognized standard work and could be considered as an example of a successful collaboration of logical empiricism and pragmatism in the context of Neurath's *Encyclopedia of Unified Science*. Nagel intended to take Peirce's pragmatism as a framework for a genuinely pragmatist approach to probability. In the course of time it turned out that Nagel's conception was essentially different from that of Carnap and Reichenbach.

Nagel's *Principles of Probability* intended to satisfy all requirements that a good unifying item of the *International Encyclopedia of Unified Science* should satisfy. Nagel based his considerations on the founding fathers of American Pragmatism Peirce and Dewey, but also mentioned Carnap's then quite recently published *Testability and Meaning*, Frank's *Das Kausalgesetz und seine Grenzen*. Moreover, he brought Morris' trisection of syntax, semantics, and pragmatics into play characterizing *Principles* as a contribution relevant for the semantics and pragmatics of the concept of probability, and not for its syntax.

Thus, at least on the surface, *The International Encyclopedia of Unified Science* presented itself as a model of collaboration between logical empiricism and American pragmatism. In the official press announcement of the *International Encyclopedia* that was published in the volumes of all monographs of the first volume *Foundations of the Unity of Science* the editors pointed out that the collaborators of the Encyclopedia might have different points of view, but that all agreed in considering the unity of science as the ideal aim of their efforts:

They agree that any form of speculation other than that recognized in science, has to be eliminated they stress the importance of logical analysis in various

fields, and in taking into account the historical development of scientific concepts and regulative principles. Such collaborators include, for instance, persons stemming from the Vienna Circle, from the Berlin group of scientific philosophers, from the Polish school of logicians, from the group centering around Scientia and the Centre de synthèse, as well as representatives of American pragmatists, the English analytical school, French conventionalism, ... (Foundations of the Unity of Science)

This description of a peaceful and harmonious collaboration of the various groups was – of course – a highly idealized picture of what really happened. Behind the curtain the activity of the protagonists often could not be described as a very harmonious one (cf. Reisch (2006), Dahms (1999)). In particular, there were heavy quarrels who should be the author for the piece on probability and induction. Originally, Reichenbach was designed as author for this item, but put for several reasons, this was not realized (see Dahms (1999) for details). The clash between Nagel's and Carnap's conceptions of probability broke out only much later in the 1960s, since in the 1940s Carnap's ideas on probability and induction were still in an embryonic stadium. Hence Nagel's empiricist-pragmatist piece seems to have pleased everybody (with the possible exception of Reichenbach).

In sum, the idyllic picture of a fruitful collaboration of European logical empiricism and American pragmatism that the *Encyclopedia* project might have offered to a superficial observer in the 1940s, was not to last long. With respect to the issue of probability and induction, it would turn out as an illusion only a few years later.

4. Alienation and Disenchantment. When exactly Nagel's disenchantment with orthodox logical empiricism began is hard to say. Although in his contribution to the *Fifth International Congress for the Unity of Science* 1939 in Harvard he described American pragmatism as being in full harmony with logical empiricism, even at that

time Nagel was not a dyed-in-the-wool logical empiricist. This is shown by his work on the philosophy of mathematics around the same time when the logical empiricists arrived in the New world (Nagel (1935, 1939) In this work he ascribed to history of science a much more prominent role for philosophy of science than orthodox logical empiricists would have been prepared to swallow. This issue will be treated in more detail in the next section.

In any case, a profound alienation between the philosophical outlooks of Carnap and Nagel is clearly documented in Nagel's contribution to the Schilpp volume dedicated to Carnap (Schilpp 1963) and Carnap's rejoinder to it. Nagel's criticism of Carnap's inductive logic is to be considered as much more than a disagreement between fellow philosophers who consider each other as belonging to the same movement.⁶ Moreover, Nagel's „most ungracious essay“ (Nagel (1963, 825)) cannot be dismissed as an insulated, perhaps only temporal disagreement concerning some technical details. Nagel republished it in his last book, apparently considering it as an important piece of his philosophical legacy.⁷ Nagel rejected Carnap's conception of probability for fundamental reasons. According to him, Carnap's account of probability and induction had not much to do with the way how these concepts were used in common-day life and the sciences. As he put it:

... if the major criticisms advanced in [this essay] hold water, it shows that despite the remarkable constructive power and ingenuity Carnap has brought to the reconstruction of inductive logic, he has not resolved the

⁶ Schilpp (1963) was published with a large delay. Hence it may seem plausible to assume that Nagel's contribution is the result of work that can be traced back well into the mid-1950s.

⁷ Nagel's alienation and disappointment with orthodox logical empiricism as evidenced by Nagel (1979) has been ignored in the secondary literature: Limbeck-Lilienau (2012) and Misak (2008, 2013) don't mention Nagel (1979) at all. This holds true, of course, for Uebel (2007), since this paper only deals with the early years of the relation between the Vienna Circle and American pragmatism.

outstanding issues in the philosophy of induction, and his general approach to the problems is not a promising one. (Nagel (1963, 825))

Carnap's answer to Nagel's politely formulated, but radical critique was unmistakable:

I am sorry that my overall reaction to the essay by my dear old friend Ernest Nagel could not be more positive. My convictions on the possibility and the nature of inductive logic, acquired in many years' work and vindicated by constant reexamination, can only be shaken by strong arguments (Carnap (1963, (995))).

In plain English, then, for Carnap, Nagel had failed to bring forward „strong arguments“ against his inductive logic.⁸

Nagel was not impressed by harsh Carnap's criticism. On the contrary, he flatly ignored it and stuck to it till the end of his life. Almost ten years after Carnap's death he re-published his verdict put forward in Schilpp (1963) without any change or any further explanation. Thus, it may be considered as his last word on Carnap's inductive logic, that some, among them Carnap himself, considered as the flagship of his philosophy of science. Nagel was not just anybody in the logical-empiricist's community. After all, he was the author of *The Principles of Probability Theory* (Nagel (1939), i.e., the logical Empiricism's official monograph on probability and induction in *The International Encyclopedia of Unified Science*.

At the end of his philosophical career, Nagel's critique of Carnapian logical empiricism was no longer confined to Carnap's inductive logic. Its target had been widened and

⁸ Nagel was not the only contributor who was rebuked by Carnap in this way: According to Carnap, Popper had – once again - completely misunderstood him, Putnam's claim that Carnapian inductive logic was impossible was simply dismissed.

Carnap did not disagree, however, with Kemeny, who put forward the thesis that „the problem of induction ... [was] certainly the central issue in any philosophy of science“ and ended with the hymnical conclusion [that] „we must class Carnap's contribution to the problem of induction among the greatest achievements of modern Philosophy (sic).“ (Kemeny (1963, 711, 737))

become Carnap's philosophy of science in general. In the introduction of Nagel (1979) he singled out Carnap's version of logical empirist philosophy of science as obsolete in its entirety. Compared with the classical pragmatisms of Peirce and Dewey on the one hand, and other versions of logical empiricism such as Frank's and von Mises', Carnap's orthodox logical empiricism came off worst. Pointing out that the philosophy of science of the logical empiricists Frank and von Mises philosophy was not formalist and ahistoric, Nagel blamed Carnapian philosophy of science as being responsible for the recent rise of a new orientation in philosophy of science that was skeptical of the efficacy of scientific method for attaining genuine knowledge:

Much of the animus of the "new orientation" in the philosophy of the science is directed against the alleged ahistorical character of the "orthodox approach"; against the latter's supposed claim that the observational evidence for a scientific theory can be assessed by using the rules of a formal calculus; These characterizations of the "old philosophy of science" are conceivably true of *some* philosopher in this category (for example, Rudolf Carnap). These characterizations are a caricature of *most* of the older generation of writers on the subject (for example C.S. Peirce, Josiah Royce, John Dewey, M.R. Cohen, or P.W. Bridgman); and they are not even true of some of Carnap's fellow logical positivists (such as Philipp Frank or Richard von Mises). Unlike Carnap, none of these thinkers subscribed to an ahistorical evaluation of the evidence for a scientific theory; and none of them identified the rationality of science with the use of exclusively formal canons for assessing claims to knowledge. It is misleading to ascribe to all representatives of the "orthodox approach" to the philosophy of science the beliefs that are idiosyncratic of what at best is a relatively small subset of that group of thinkers (Nagel (1979, 3)).

This global criticism is clearly an extrapolation of Nagel rejection of Carnap's project of inductive logic. Nagel did not subscribe to a kind of Uebel's "bipartite metatheory" according to which logical empiricists such as Frank and von Mises on the one side and

Carnap on the other side worked on the same project of a comprehensive logical empiricist philosophy of science.⁹ He did not recognize the allegedly possible division of labor between those who dealt with the logical aspects of science and those who concentrated on the empirical aspects. At the end of the day, Nagel considered Carnap's logical way as misguided and fruitless.

While in his replies to Nagel, Putnam, and Popper in Schilpp (1963) Carnap sharply rejected the criticisms that these authors had put forward against his logic of induction, his replies to Morris, Cohen, and Frank are formulated in more reconciliatory tone. With respect to pragmatism, in rather vague terms he even expressed his gratitude to American pragmatism in general:

The influence of the pragmatist ideas has been very fruitful for the development of my conceptions. It did not derive so much from the works of the founders of pragmatism (whose formulations I could often not easily accept, e.g., Peirce's metaphysics and Dewey's discussions of logical and epistemological problems), but from later representatives such as C.I. Lewis, Charles Morris, Ernest Nagel, and Sidney Hook, whose formulations seemed clearer and closer to those customary in science. Carnap (1963, 861)

Thereby Carnap placed himself elegantly on the side of rationality and science: Although classical pragmatists such as Peirce and Dewey scientifically left something to be desired, he generously admitted that things had improved with the younger generation of pragmatists. This sounds better than it really is: Actually it is hard to find any reference to Lewis in Carnap's work – with the exception of *Testability and Meaning* (Carnap 1936). The same is true for Morris, Nagel and Hook.¹⁰

⁹ Perhaps surprisingly, in Nagel (1979) Neurath and the *International Encyclopedia of Unified Science* is not mentioned even once. Hempel's account of functional explanation in biology is discussed in detail in the last section of the book.

¹⁰ For instance, the above quotation above is the only one where Hook is mentioned by Carnap in Schilpp (1963).

In any case, the alleged „fruitful influence of pragmatist ideas on [Carnap’s] conceptions“ was not acknowledged by later Nagel who did not recognize Carnap as a fellow pragmatist. At the end of his philosophical career he considered him as a representative of an obsolete formalist philosophy of science. On the other hand, the later Nagel was explicitly sympathized with Frank’s Austro-American version of logical empiricism. That is to say, he did not subscribe to an approach that much later Uebel has baptized as a „bipartite metatheory“ of philosophy of science to which the members of the left wing of the Vienna Circle subscribed. According to Uebel, Carnap’s formal philosophy and science and the more empirically oriented psychological, sociological, and historical works of his Vienna circle fellow philosophers of science were to be considered as two components of a comprehensive „bipartite metatheory“ of philosophy of science. Whatever the virtues of the bipartite metatheory may be, Carnap as one of its early practitioners, has not convinced the later Nagel.

5. A Role for History of Science in Philosophy of Science. Since some time it is a common place that good philosophy of science cannot be pursued done without taking a solid account of history of science.¹¹ For Nagel, the thesis that philosophy of science has to take into account history of science would hardly have been exciting news. Not only that he chose for his *John Dewey Essays* the title *Teleology Revisited and Other Essays in the Philosophy and History of Science*. Throughout his career as a philosopher of science issues of history of science played an important role for him. This holds in particular for the history of mathematical sciences that he pursued since his beginnings

¹¹ For a discussion of the various ways how philosophy and history of science may collaborate in order to provide a better understanding of their common subject, see the introduction of the Festschrift for Michael Friedman *Discourse on a New Method – Reinvigorating the Marriage of History and Philosophy of Science* edited by Domski and Dickson (2007).

of a professional philosopher, see his *“Impossible Numbers”*: *A Chapter in the History of Modern Logic* (Nagel (1935 (1979))) and *The Formation of Modern Conceptions of Formal Logic in the Development of Geometry* (Nagel (1939 (1979))).

Nagel’s contributions to the philosophy and history of mathematics are discussed until today (cf. for example, Kitcher (2012), Stump (2015), or Blanchette (2017)). The fact that Nagel republished these early pieces in his last book after more than forty years is evidence that he considered them as permanently relevant pieces of his philosophical work.¹²

This is confirmed by the fact that they are discussed in the literature of philosophy and history of mathematics until today. Let us briefly discuss *„Impossible Numbers“* and compare it with a piece of Carnap’s written a bit earlier and also dealing with the issue of *„impossible numbers“*, although of a different kind. Today, the *„impossible numbers“* Nagel dealt with are just the familiar *„complex numbers“*. They have the form $(a + ib)$ and appear as roots of polynomials $p(x) := a_n x^n + a_{n-1} x^{n-1} + \dots + a_1 x + a_0$. From the perspective of modern mathematics, the numbers $(a + ib)$ (in particular their *„imaginary parts“* ib) are no more mysterious than ordinary *„real“* numbers a . Thus, some conceptual effort is required to understand why in the evolution of mathematics these numbers (and others as well) have been considered as epistemologically and ontologically dubious for such a long time.

Traditionally, “number” is conceived as being an answer to the questions “How many?” and, in cases of extensive measure, “How much?” Evidently, complex numbers cannot be considered as reasonable answers to these traditional questions. Consequently, they

¹² This is confirmed by the following late comment of Nagel to be found in Nagel (1979): [These papers] are the sole fruits of a long since abandoned plan to write a comprehensive history of changes during the 19th century in methodological ideas employed in various branches of inquiry – in the natural, psychological, and social sciences, but also in a number of humanistic disciplines (such as history, legal scholarship, and hermeneutics. Nagel (1979, 318).

are “impossible numbers”. Already negative numbers are difficult to be conceived as answers to questions concerning quantity and extension. On the other hand, “impossible numbers” were undeniable useful. Thus, if mathematics was to be the science of quantity, then complex numbers, not being quantities in any intelligible sense, were to be considered as “impossible numbers”, indeed. Thus, it cost mathematicians and philosophers alike a lot of conceptual effort to understand that mathematics should not be understood as the science of quantity in the traditional sense.

In the following only *Impossible Numbers* will be treated in some detail. It should be noted, however, that in *The Formation of Modern Conceptions of Formal Logic in the Development of Geometry* Nagel argued for the very same general thesis, namely, that for a more profound understanding of modern science, in particular for the understanding of modern logic and mathematics and their role in the ongoing development of modern science, the history of the sciences is essential. This attitude stands in strong contrast to that of Carnap who around the same time discussed another kind of “impossible numbers” but in a quite different attitude than Nagel. Thus, Nagel’s philosophy of mathematics was interested in a history of mathematical ideas or concepts that went beyond a mere presentation of their logical relations. More precisely, Nagel argued that some

central doctrines of contemporary logic will become illuminated and made more persuasive by examining the developments in which they terminate. In particular, a consideration of the procedures of mathematics within the historical settings in they operate may provide materials for a just appraisal of the limitations of traditional conceptions of mathematics and logic, as well as of the more recent views that have replaced them. Nagel (1979, 196)

In Carnap’s logicist philosophy of mathematics one does not find anything even remotely similar. From a Carnapian perspective there was just one logically correct formulation of a concept, and perhaps some (outdated) logically flawed precursors. An

instance of this attitude is provided by Carnap's remarks on infinitesimals as a kind of "impossible numbers" vividly discussed by many mathematicians and philosophers of mathematics since the end of the 19th century. Carnap gave them short shrift:

The inventors of the infinitesimal calculus (Leibniz and Newton) ... could not say ... what actually is to be understood by the "derivative" of a function. They could ... not give a precise definition of the concept "derivative". However, their formulations for this definition used such expressions as "infinitesimally small magnitude" ... turn out to be pseudoconcepts (empty words). It took more than an century before an unobjectionable definition of the general concept of a limit and thus of a derivative was given. Only then all those mathematical results which had long since been used in mathematics were given their actual meaning. (Carnap (1928), 307/308)

For Carnap, history of science does not teach anything to philosophy of science. In contrast, for Nagel, we may learn a lot about science from history of a science. This holds, in particular, of mathematics. For instance, from the history of mathematics we can learn that "[t]he proper and exclusive subject matter of mathematics is not quantity" as many philosophers have claimed (Nagel (1935, 167)) This thesis is elaborated in considerable detail in his papers "*Impossible Numbers*": *A chapter in the History of Modern Logic* (Nagel 1935) and *The Formation of Modern Conceptions of Formal Logic in the Development of Geometry* (Nagel 1939). The logical empiricists' official "identification" of mathematics and logic, or, more precisely, the "derivation" of the former from the latter is quite useless in this endeavor. Thus, Nagel's work in philosophy and history of mathematics goes in a quite different direction than Carnap's logicist approach.

6. Philosophy of Science – Who is the Audience?¹³ The difference between logical empiricist and pragmatist philosophy of science, as exemplified by Carnap and Nagel,

¹³ The second half of this section's title is borrowed from Kitcher (2019).

should not be conceived as solely a matter of different personal styles. Rather, it points to a profound difference of how to understand the role of the philosophy of science.

Nagel's account was based on the pragmatist assumption that the task of philosophy of science is to clarify the broad significance of science for human life. This significance goes beyond the practical control over nature which science yields. Science is more than a set of practically useful technologies. For Nagel, science made the world intelligible. It satisfies the human craving to know and to understand as Aristotle said.¹⁴ The task of philosophy of science was to contribute to a "scientific culture" as the Neo-kantian Cassirer would have said. A Carnapian philosopher of science might not have militated explicitly against this aim, but he would have preferred to describe the philosopher's task more theoretically as kind of conceptual engineering directed to the improvement of the conceptual apparatus of science.

Nagel was an Aristotelian naturalist, who considered the desire for knowledge as an ingredient of human nature. Patrick Suppes, who was Nagel's student and later became one of the most influential philosophers of science of the second half of the 20th century wrote in the biographical memoir of his teacher:

[What] is most important to emphasize about [Nagel's] more than forty years' association with Columbia University is the central role he played in the intellectual life of Columbia, and more generally, of New York City. To many of students he was the outstanding spokesman of what philosophy could offer in terms of the analysis of the scientific method, as it is practiced in many different sciences, and in the relation between science and perennial problems of philosophy such as those of causality and determinism. ...

... Throughout his career Nagel tried to combine the best elements of Cohens's philosophical realism and Dewey's radical instrumentalism. ... It is fair to say that the range of his scientific interests and knowledge exceeded

¹⁴ Indeed, as Dewulf recently argued convincingly, Nagel's philosophy of science may be characterized as a kind of Aristotelian philosophy of science (cf. Dewulf (2018, 156 – 157)).

that of any other philosopher of science of his generation in the United States.
(Suppes (1994, 258 - 259)).¹⁵

Nagel's role as a philosopher of science, as described by Suppes, is quite different from the Carnapian model of the philosopher as a linguistic engineer engaged in the construction of conceptual apparatuses that work in an optimal way for some purpose or other. Without denying the existence of far-reaching differences between their philosophical convictions, philosophers like Nagel, Lewis, Dewey, and even Rorty, should be grouped in another class of philosophers as the conceptual engineer Carnap, who resembles more a player of Hesse's *glass bead game* than a pragmatist committed to the actual world.¹⁶ For Carnap, praxis remained a matter of private commitment, so to speak. Nagel practiced a quite different idea about the profession of a philosopher of science.

Natural, although somewhat embarrassing questions for many philosophers of science are „What is philosophy of science good for?“, „Who is the audience of philosophy of science?“, or „To whom philosophy of science is addressed to?“ (cf. Kitcher (2019)).

Kitcher proposed three possible answers:

Philosophers, scientists, and interested citizens within and beyond the academy. I argue that our discipline is potentially relevant to all three, but I particularly press the claims of the interested citizens.

Obviously, for a Carnapian conceptual engineer the educated citizen is not the first addressee. Scientific engineering aims to improve the conceptual apparatus of the

¹⁵ Further information about Nagel's role as a leading figure in the philosophical and cultural life of New York can be found in Jewitt (2012) and Dewulf (2018).

¹⁶ This is not to deny that Carnap personally showed much social commitment, but this commitment was not founded in his theoretical convictions as a professional philosopher of science. Expressed in a somewhat unfriendly way the „boundless ocean of possibilities“ that he had evoked emphatically in *The Logical Syntax of Language* (Carnap 1937) may have some features in common with Hesse's *Glass Bead Game* (1943(2002)).

sciences. Whether his kind of logical philosophy of science actually has been relevant to scientists, can be doubted as well. Be this as it may, Nagel's way of doing philosophy of science came rather close to Kitcher's comprehensive idea of the discipline.

7. Concluding Remarks. In the 1930s Nagel's role in the encounter of logical empiricism and American pragmatism can be accurately described as a committed mediator and bridge builder between the two movements. In later years, this description is no longer true. Nagel's negative assessment of later Carnap's philosophy (first, his account of probability and inductive logic, later his approach in its entirety) shows that for Nagel the much evoked convergence between logical empiricism and American pragmatism has not taken place.¹⁷

Nagel's rejection of Carnap's logical approach cannot be misunderstood as the reaction of a philosopher who was simply unable to understand formal arguments. Rather, Nagel rejected Carnap's philosophy as an ultimately non-pragmatist way of doing philosophy that did not take into account (or even explicitly rejected) the fundamental connection of knowledge, action and valuation that the pragmatist Lewis expressed in a concise way as follows:

Knowledge, action, and evaluation are essentially connected. The primary and pervasive significance of knowledge lies in its guidance of action; knowing is for the sake of doing. And action, obviously, is rooted in evaluation. For a being which did not assign comparative values, deliberate action would be pointless; and for one which did not know, it would be impossible. Conversely, only an active being could have knowledge, and only such a being could assign values to anything beyond his own feelings.
(Lewis (1946, 5))

¹⁷ This fact does not exclude that there may have been logical empiricists, who converged or „drifted“ to pragmatism, e.g., Frank and Hempel (cf. Mormann (2017), Wolters 2001).

Despite paying lip-service to the contrary, hard-boiled logical empiricists such as Carnap never acknowledged this connection with no ifs and buts. This attitude rendered impossible a close relationship with Carnapian logicist empiricism and American genuine pragmatism.

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