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Richard Creath *Editor*

Rudolf Carnap and the Legacy of Logical Empiricism

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RUDOLF CARNAP AND THE LEGACY OF LOGICAL EMPIRICISM

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Rudolf Carnap and the Legacy of Logical Empiricism

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Editorial

Rudolf Carnap was one of the most important philosophers of the twentieth century, and he came to worldwide fame during his time in Vienna, 1926–31, as a core member of the Vienna Circle. So the eight papers in this volume, all on Carnap and all given at a conference in Vienna, represent a homecoming of sorts. But the papers do not confine themselves to Carnap alone or to his time in Vienna. They explore his history and motivation, his interaction with other philosophers in both Europe and America, various aspects of his work on logic and philosophy of science, and even his political engagement.

The conference at which these papers were presented, "Rudolf Carnap and the Legacy of Logical Empiricism", was held on the 28th and 29th of June, 2010 at the University of Vienna, under the auspices of the Institute Vienna Circle. The meeting was organized jointly by Friedrich Stadler for the Institute and by Richard Zach on behalf of the *Collected Works of Rudolf Carnap*, forthcoming from the Open Court Publishing Company.

Carnap wanted philosophy to be scientific, that is, a progressive, cooperative venture. But as Michael Friedman shows the ideal of a scientific philosophy appeared long before Carnap and continues to inspire many philosophers even today. Logic was central to Carnap's vision of a scientific philosophy, and Georg Schiemer explores one of Carnap's large-scale logic projects, the *Untersuchungen zur allgemeinen Axiomatik*. This work was set aside in the 1930s and published only well after Carnap's death. Schiemer is able to show that the work is subtle and gives important clues to Carnap's thinking in logic. Philosophy of science was also central to Carnap's vision of philosophy. Matthias Neuber explores the scientific realism defended by Hans Reichenbach and Eino Kaila and Carnap's resistance to it. Richard Creath examines the related issue of Carnap's proposal for endowing theoretical terms with meaning by tying theories to their Ramsey sentences. Christian Damböck examines another empirical tradition, "German" empiricism, especially as exemplified by Wilhelm Dilthey, and assesses its influence on Carnap's view in the *Aufbau*.

Christoph Limbeck-Lilienau and Thomas Mormann, in their separate papers, explore the relations between Carnap and American pragmatism. They approach this topic differently, but each shows that this relation is both complex and illuminating. Finally, Thomas Uebel examines the political dimension of the Vienna Circle and particularly of Carnap within it. Uebel shows that logic and philosophy of science were from the beginning conceived of as part of a larger project that was indeed political in the broadest sense. Taken together, the papers show the rich variety of Carnap's concerns and his place within important intellectual traditions that shape our thinking even today.

Thanks go to the organizers of the conference, Friedrich Stadler and Richard Zach, to the host institutions, the Institute Vienna Circle and the University of Vienna, to the conference participants, and to the production team at the Institute.

Richard Creath
Tempe, Arizona, July 7, 2011

Michael Friedman

Scientific Philosophy from Helmholtz to Carnap and Quine

The concept of a "scientific philosophy" (*wissenschaftliche Philosophie*) first developed in the mid nineteenth century, as a reaction against what was viewed as the excessively speculative and metaphysical character of post-Kantian German idealism. One of the primary intellectual models of this movement was a celebrated address by Hermann von Helmholtz, "Über das Sehen des Menschen," delivered at the dedication of a monument to Kant at Königsberg in 1855. Helmholtz begins by asking, on behalf of the audience, why a natural scientist like himself is speaking in honor of a philosopher. This question only arises, he says, because of the current deplorable climate of enmity and mutual distrust between the two fields – a climate which is due, in Helmholtz's opinion, to the entirely speculative system of *Naturphilosophie* that Schelling and Hegel have erected wholly independent of, and even in open hostility towards, the actual positive results of the natural sciences. What Helmholtz is now recommending, however, is a return to the close cooperation between the two fields exemplified in the work of Kant, who himself made significant contributions to natural science (in his nebular hypothesis put forward in 1755), and, in general, "stood in relation to the natural sciences together with the natural scientists on precisely the same fundamental principles."[1] And it was this recommendation that was enthusiastically embraced within the emerging "back to Kant!" movement, where it led to the idea that all metaphysics should be replaced by the new discipline of "epistemology" or "theory of knowledge" (*Erkenntnistheorie*), so that philosophy itself would now become "scientific."

What is the nature of this new scientific philosophy that is now being explicitly opposed to *Naturphilosophie* in particular and post-Kantian idealism in general? What relation is philosophy now supposed to bear to the sciences, and what does it mean for philosophy to become scientific in this way? What exactly is being recommended when we are told that philosophy should stand "in relation to the natural sciences together with the natural scientists on precisely the same fundamental principles"?

For Helmholtz himself this means that philosophy – that is, epistemology or the theory of knowledge – should work in cooperation with the latest psychophysiological research in inquiring into the nature of the representations of our senses, and the relationship between these representations and the actual world to which they correspond. And it is for this reason that the body of his 1855 address

1 See Helmholtz (1865/1903, vol. I, p. 88).

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is occupied almost exclusively with reporting on some of his own work in the psycho-physiology of vision. As he makes clear in his most mature presentation of his epistemology in "The Facts in Perception" of 1878, it is Helmholtz's view that philosophy considers the relationship between our representations and the external world from the mental or psychological side, while natural science considers it from the physical or physiological side:

> The fundamental problem, which that time placed at the beginning of all science, was that of the theory of knowledge: "What is truth in our intuition and thought? In what sense do our representations correspond to actuality?" Philosophy and natural science encounter this problem from two opposite sides; it is a common task of both. The first, which considers the mental side, seeks to separate out from our knowledge and representation what originates from the influences of the physical world, in order purely to establish what belongs to the mind's own activity. Natural science, by contrast, seeks to separate off what is definition, designation, form of representation, and hypothesis, in order purely to retain what belongs to the world of actuality, whose laws it seeks.[2]

In both cases, however, our inquiry rests on the latest empirical findings of psychological and physiological research, and so, in the end, philosophy, for Helmholtz, is itself an empirical natural science – a branch of empirical psychology. In this way, Helmholtz anticipates the conception, popular in some circles today, that philosophy should become absorbed into cognitive psychology.

In 1921, the centenary year of his birth, Helmholtz's scientific achievements, in energetics, physiological psychology, the foundations of geometry, electrodynamics, and epistemology, were celebrated in a variety of memorial addresses, journal issues, and monographs. One especially important address, entitled "Helmholtz als Erkenntnistheoritiker," was presented by Moritz Schlick at the University of Berlin. Schlick had earlier earned a doctorate in theoretical physics under Max Planck at Berlin, but soon thereafter decided to pursue a career in philosophy instead. Schlick then became the leading philosophical proponent and expositor of Einstein's new theory of relativity with the publication of his extremely influential monograph, *Space and Time in Contemporary Physics*, which went through four editions between 1917 and 1922. In 1922, largely on the strength of his work on the philosophical significance of the theory of relativity, which had been enthusiastically endorsed by Einstein himself, Schlick was named in 1922 to the Chair for the Philosophy of the Inductive Sciences previously occupied by the scientists Ernst Mach and Ludwig Boltzmann at the University of Vienna, where he became the leader and guiding spirit of what we now know as the Vienna Circle of logical positivists. We might say, in this sense, that Schlick was the very first *professional* scientific philosopher.

Rudolf Carnap, ten years younger than Schlick, joined the Vienna Circle in 1926. *Der logische Aufbau der Welt*, Carnap's first major philosophical book

2 See Hertz and Schlick (1921/1977, p. 111/pp. 117-118).

(published in 1928) originated in his Habilitation under Schlick at the University of Vienna. Carnap, like Schlick, had earlier focussed in his graduate education on theoretical physics, which he studied under Max Wien at the University of Jena, where he also studied mathematics and, of course, the new mathematical logic under Gottlob Frege.

In his *Logical Syntax of Language* of 1934, however, Carnap broke with Schlick's earlier conception of philosophy as the foundational core of all the special sciences – and also with Schlick's later conception, formulated within the Circle under the influence of Wittgenstein's *Tractatus*, that "philosophy is not a doctrine but an activity."[3] For, if Wittgenstein is right, it now appeared that, if the scientific philosophers of the Vienna Circle truly wanted to avoid metaphysics, they would also have to give up the idea that philosophy could be a science in *any* sense. Carnap responded to this situation by urging that we should extend Hilbert's method of metamathematics from pure logic to the whole of philosophy. In particular, scientific philosophy should now become *Wissenschaftslogik* – the meta-logical investigation of the logical structure of the total language of science. Philosophy, in this way, becomes a branch of applied mathematical logic:

> The alleged peculiarly philosophical point of view, from which the objects of science are supposed to be considered, is abolished, just as the alleged peculiarly philosophical stratum of objects was already previously eliminated. Aside from the questions of the individual special sciences, the only questions that remain as genuinely scientific questions are those of the logical analysis of science – its sentences, concepts, theories, etc. We will call this complex of questions *Wissenschaftslogik* … . Taking the place of the inextricable tangle of problems that is known as philosophy is *Wissenschaftslogik*. Whether, on the basis of this conception, the designation "philosophy" or "scientific philosophy" should be applied to this remainder, is a question of expedience, which is not to be decided here.[4]

Yet it is a delicate matter to discern precisely how Carnapian *Wissenschaftslogik* serves to articulate the logical structure of the empirical sciences, on the one side, and to contribute to the definitive overcoming of traditional metaphysics, on the other.

Carnap writes in his "Intellectual Autobiography" (1963) that, even before his arrival at the Vienna Circle, he was very much struck by the difference between "controversies in traditional metaphysics" and "discussions in empirical science." In particular, he was "depressed by [metaphysical] disputations in which the opponents talked at cross purposes" and in which "there seemed hardly any chance of mutual understanding, let alone agreement."[5] When asked by his friends which philosophical position he himself subscribed to ("realism vs. idealism, nominalism vs. Platonism, materialism vs. spiritualism, and so on") he was unable to answer:

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3 See Wittgenstein (1922, § 4.112).

4 See Carnap (1934/37, § 72). Carnap sharply differentiates himself from Wittgenstein's doctrine of the inexpressibility of logical syntax in the following section.

5 See Carnap (1963, pp. 44-45).

"I could only say that my general way of thinking was closer to that of physicists and those philosophers who are in contact with scientific work."[6]

Carnap's early experience in physics and its foundations taught him that empirical testing, in this science, is facilitated and enabled by the prior choice of an appropriate mathematical framework: the theory of Riemannian manifolds in general relativity, Hilbert space in quantum mechanics, and so on. In *Foundations of Logic and Mathematics* (1939), Carnap formulates the point more abstractly and generally by the idea that any scientific theory (in physics, in particular) can be represented by an axiomatic formal system consisting of a logico-mathematical part and a properly empirical part, and that the empirical application of such a formal system principally involves experimental measurement procedures in which quantitatively formulated empirical laws yield testable statements about particular numerically specified outcomes via intervening logico-mathematical theorems. Because of the central role of numerical terms (including terms for real numbers) in these procedures, the logico-mathematical part of the system is most appropriately formulated as a higher-order system – as opposed to an elementary or first-order system – containing a sufficient amount of arithmetic and analysis. This is Carnap's most fundamental reason for consistently taking higher-order logic (or, equivalently, set theory) as part of the formal or inferential framework within which empirical testing proceeds – and it is this that most clearly and sharply distinguishes Carnap's conception of logic and mathematics from that developed later, in reaction to Carnap, by Quine.

I shall return to the Quinean alternative shortly. But I first want to observe that Carnap, in *Logical Syntax*, had already decisively rejected the traditional logicist view that mathematics can be reduced to logic in some antecedently specified sense. The new conception of analyticity he formulates embraces logico-mathematical propositions as a whole, and his preferred axiomatic treatment, formulated in what he calls Language II, simply takes all of classical mathematics (systematized in higher-order logic or set theory) for granted, with no attempt to reduce it to something simpler or (supposedly) more secure. At this point, however, Carnap was here directly confronted by the so-called "foundations crisis": with what looks like a characteristically unresolvable philosophical controversy affecting logic and mathematics themselves – involving a metaphysical dispute about the "true nature" of mathematical entities and an epistemological dispute about our cognitive access to them. Carnap's mature doctrine of the analyticity of logic and mathematics is intended to dissolve this controversy by means of the principle of tolerance: the choice between the three classical foundational "schools" of logicism, intuitionism, and formalism is merely practical or pragmatic and has nothing to do with any such ontological or epistemological worries.

Carnap's use of the analytic/synthetic distinction, in this context, has nothing to do with any epistemological program for explaining how logical and mathematical

6 See Carnap (1963, pp. 17-18).

certainty is possible by appealing to truth-by-convention or truth-in-virtue-of-meaning. Rather, according to precisely the principle of tolerance, the point of viewing the propositions of logic and mathematics as analytic lies in our *freedom to choose* which system of logico-mathematical rules best serves the formal deductive needs of empirical science. Classical mathematics, for example, is much easier to apply, especially in physics, than intuitionist mathematics, while the latter, being logically weaker, is less likely to result in contradiction. The choice between the two is therefore purely practical or pragmatic, and it should thus be sharply separated from all (apparently) theoretical disputes concerning what mathematical entities "really are" (independently existing "Platonic" objects or mental constructions, for example) or which such entities "really exist" (only natural numbers, for example, or also real numbers, that is, sets of natural numbers).[7] Carnap's aim, once again, is definitely to dissolve such disputes and to replace them, instead, with the more rigorous and fruitful discipline of *Wissenschaftslogik* – the logic of science.

It becomes clear in later works from the 1930s, especially *Foundations of Logic and Mathematics* (1939), that the point of the logic of science, in turn, is not so much to describe the nature of science or scientific method as it has been practiced so far, but to facilitate a new kind of interaction between philosophy and empirical science, which, in Carnap's eyes, promises to be particularly fruitful for both. Armed with the new methods of metamathematics, the philosopher – that is, the logician of science – can participate, together with the scientists themselves, in the development and clarification of formal inferential frameworks for articulating empirical theories and testing them by experimental methods. Unlike the empirical scientist, however, the logician of science, as such, is not concerned with actually testing such theories. Moreover, unlike the applied mathematician (for example, the statistician), who also develops formal inferential methods for use in the empirical sciences, the logician of science has an explicitly philosophical (or rather *anti*-philosophical) interest in developing a systematic approach for defusing unresolvable metaphysical controversies which, in Carnap's view, constitute an ever-present obstacle to progress in both the sciences and philosophy.

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7 For an especially clear statement of this view, see Carnap (1939, § 20): "Concerning mathematics as a pure calculus there are no sharp controversies. These arise as soon as mathematics is dealt with as a system of 'knowledge'; in our terminology, as an interpreted system. Now, if we regard interpreted mathematics as an instrument of deduction within the field of empirical knowledge rather than as a system of information, then many of the controversial problems are recognized as being questions not of truth but of technical expedience. The question is: Which form of the mathematical system is technically most suitable for the purpose mentioned? Which one provides the greatest safety? If we compare, e.g., the systems of classical mathematics and of intuitionistic mathematics, we find that the first is much simpler and technically more efficient, while the second is more safe from surprising occurrences, e.g., contradictions."

Just as Carnap was a student of Schlick's, Quine was a student of Carnap's. Quine reports that his initial contact with Carnap in Prague, in 1931, "was my first experience of sustained intellectual engagement with anyone of an older generation, let alone a great man[; i]t was my first really considerable experience of being intellectually fired by a living teacher rather than by a dead book." More generally: "Carnap was my greatest teacher. I got to him in Prague 38 years ago, just a few months after I had finished my formal studies and received my Ph.D. I was very much his disciple for six years. In later years his views went on evolving and so did mine, in divergent ways. But even where we disagreed he was still setting the theme; the line of my thought was largely determined by problems that I felt his position presented." Accordingly, Quine characterizes Carnap as "a towering figure … as the dominant figure in philosophy from the 1930's onward, as Russell had been in the decades before." Carnap's *Logical Syntax of Language* (1934), the first draft of which Carnap was completing while Quine visited him in Prague, was, in the thirties, "the main inspiration of young scientific philosophers … the definitive work at the center, from which the waves of tracts and popularizations issued in ever widening circles."[8] No wonder, then, that the dedication page of Quine's own masterwork, *Word and Object* (1960), reads: "To Rudolf Carnap, Teacher and Friend."

Section 56 of *Word and Object* contains Quine's most expansive diagnosis (in that work) of the difference between Carnap's and his own conceptions of philosophy. "Carnap has long held," Quine begins, "that the questions of philosophy, when real at all, are questions of language … [h]e holds that the philosophical questions of what there is are questions of how we may most conveniently fashion our 'linguistic framework,' and not, as in the case of the wombat or the unicorn, questions about extralinguistic reality … [h]e holds that those philosophical questions are only apparently about sorts of objects, and are really pragmatic questions of language policy." "But why," Quine continues, "should this be true of the philosophical questions and not of theoretical questions more generally? … After all, theoretical sentences in general [including those in mathematics and empirical science] are defensible only pragmatically; we can but assess the structural merits of the theory which embraces them along with sentences directly conditioned to multifarious stimulations."[9] Quine here echoes the "more thorough pragmatism" characteristic of his distinctive radically holistic version of empiricism presented in § 6 of "Two Dogmas of Empiricism" (1951): "Each man is given a scientific heritage plus a continuing barrage of sensory stimulation; and the considerations which guide him in warping his scientific heritage to fit his continuing sensory promptings are, where rational, pragmatic."[10] On this basis, in particular, there can be no Carnapian sharp boundary between scientific and philosophical questions. As Quine explains in the introductory paragraph of "Two Dogmas": "One effect of abandoning [the two dogmas] is … a blurring of the supposed boundary

8 See Quine (1971/1990, pp. 463-465).

9 See Quine (1960, p. 271).

10 See Quine (1951/1953, p. 46).

between speculative metaphysics and natural science. Another effect is a shift toward pragmatism."[11]

When discussing the empiricist project of the *Aufbau* in "Two Dogmas," Quine emphasizes the richness of Carnap's preferred logico-mathematical framework, which, as we observed, contains whatever parts of higher-order logic or set theory that are necessary for mathematically articulating and experimentally testing the empirical sciences. Quine, characteristically, remarks: "Empiricists there are who would boggle at such prodigality."[12] And, as I have argued in detail elsewhere, there appears to be very little doubt that Quine, from the very beginning, was one of these empiricists.[13]

Quine reports that he had already "felt a nominalist discontent with classes" as early as 1932-33.[14] In the famous discussions at Harvard during 1940-41, involving Carnap, Quine, and Alfred Tarski, Quine joined Tarski in advocating the even stricter nominalistic position that full (infinitary) first-order arithmetic is not truly understandable [*verständlich*] in the way in which concrete (perceptible) physical objects are. These discussions, for Quine, culminated in "Steps Toward a Constructive Nominalism," published in 1947 with Nelson Goodman (who also participated in some of the 1940-41 discussions). By reading this paper in the context of Quine's lectures on David Hume's philosophy during the summer session at Harvard in 1946,[15] one can make a strong case that the standards of *Verständlichkeit* motivating both Goodman and Quine are precisely those of traditional British Empiricism – according to which only terms referring to our immediate sense impressions are paradigmatically meaningful and only here can an epistemic "bed-rock of certainty" possibly be found.[16]

Beginning with "On What There Is" (1948), however, Quine breaks decisively with the nominalistic attitude towards mathematics represented in his joint paper with Goodman.[17] Quine articulates, instead, a radically holistic version of empiricism according to which all elements of our conceptual scheme – ordinary physical objects, theoretical entities in physics, *and* mathematical objects (numbers, sets, and so on) – are to be viewed as postulated entities ("myths" or "posits") in our overall empirical theory of the world, the goal of which is to simplify and systematize, as much as possible, the totality of our basic sensory evidence.[18] The

11 See Quine (1951/1953, p. 20).

12 See Quine (1951/1953, p. 39).

13 See Friedman (2006).

14 See Quine (1986, p. 14).

15 See Quine (2003).

16 See my article cited in note 12 above.

17 See Quine (1986, p. 26) for the relationship between his earlier "constructive nominalism" defended in the joint paper with Goodman and the new position of "On What There Is."

18 See Quine (1948/1953, pp. 16-17): "Our acceptance of an ontology is, I think, similar in principle to our acceptance of a scientific theory, say a system of physics: we adopt,

postulation of mathematical objects – including those of higher set theory – is empirically justified in precisely the same sense, and for the same reason, as is the postulation of theoretical entities in physics. The distinctive, radically holistic, version of empiricism characteristic of Quine's mature epistemology therefore arises as a response to the failure of his early nominalism, but it is motivated, in the end, by the very same empiricist qualms about the apparently great distance of "abstract entities" from immediate sense experience.[19]

By contrast, there is simply no room for such qualms in Carnap's version of empiricism. Carnap's experience in mathematics and physics taught him that the empirical evidence justifying the use of various theoretical entities in modern physics *presupposes* the prior choice of an abstract mathematical framework (for quantum mechanics, general relativity, and so on) – including whatever portion of higher-order logic or set theory is necessary for establishing the formal inferential links between physical theoretical assertions and the results of experimental measurement procedures. To suppose that there can be empirical evidence, in this sense, for the mathematical framework itself is therefore non-sensical. The question of which logico-mathematical framework to adopt cannot be a properly theoretical question at all (an "internal" question in Carnap's later terminology), but is rather purely practical or pragmatic (an "external" question).[20] And it is in precisely this way, as we have seen, that Carnap hopes permanently to safeguard the enterprises of axiomatic investigation and empirical testing from metaphysical contamination that could impede scientific progress.

Quine also takes himself to be a representative of scientific philosophy. He rejects mentalism, modality, and other undesirable philosophical accretions as unscientific; and he does so on the basis of a strict physicalism according to which "theory in physics is the ultimate parameter."[21] But Quine's version of scientific

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at least insofar as we are reasonable, the simplest conceptual scheme into which the disordered fragments of raw experience can be fitted and arranged. Our ontology is determined once we have fixed upon the over-all conceptual scheme which is to accommodate science in the broadest sense; and the considerations which determine a reasonable construction of any part of that conceptual scheme, for example, the biological or the physical part, are not different in kind from the considerations which determine a reasonable construction of the whole."

19 See Quine (1948/1953, p. 18): "A platonist ontology [of classes] is, from the point of view of a strictly physicalistic conceptual scheme, as much a myth as that physicalistic conceptual scheme is for phenomenalism. This higher myth is a good and useful one, in turn, in so far as it simplifies our account of physics. Since mathematics is an integral part of this higher myth, the utility of this myth for physical science is evident enough. In speaking of it nevertheless as a myth, I echo that philosophy of mathematics I alluded to earlier under the name of formalism [which, as Quine makes clear in note 9 on p. 15, includes the nominalism earlier developed by Goodman and himself]. But an attitude of formalism may with equal justice be adopted toward the physical conceptual scheme, in turn, by the pure aesthete or phenomenalist."

20 See Carnap (1950/1956), to which I shall return below.

21 See Quine (1969, p. 303).

philosophy is fundamentally different from Carnap's. The enterprise of Carnapian *Wissenschaftslogik* is intended to operate in close proximity to the empirical sciences themselves. Like applied mathematicians (e.g., statisticians), logicians of science investigate mathematical inferential frameworks for testing empirical theories; they do this at the more abstract level of formal logic and set theory, however, because it is here that Carnap finds a secure systematic method for warding off metaphysical contamination. Quine's version of scientific philosophy, by contrast, operates at a quite considerable distance from empirical scientific work. It involves nothing more nor less than the articulation of a grand naturalistic world view embracing logic, ontology, epistemology, philosophy of science, and more. In the opening words of "Two Dogmas," Quine's new picture of the philosophical enterprise is thus explicitly intended to effect "a blurring of the supposed boundary between speculative metaphysics and natural science" (compare note 10 above). And in this enterprise, in sharp contrast to Carnap's, Quine does his very best to accommodate prior philosophical commitments of a traditionally nominalist and empiricist sort, which, from Carnap's point of view, are merely unscientific – that is, *pre*-scientific – prejudices.[22]

Quine thereby diverges fundamentally from the entire earlier tradition of scientific philosophy from Helmholtz through Schlick to Carnap. Beginning with Helmholtz, the goal of scientific philosophy was definitively to reject the metaphysical tradition of post-Kantian German idealism in favor of a return to the more scientific approach of Kant himself – who, according to Helmholtz, "stood in relation to the natural sciences together with the natural scientists on precisely the same fundamental principles" (compare note 1 above). Quine, in rejecting any kind of Kantian (or neo-Kantian) commitment to mathematics as a prior inferential framework for the articulation and testing of empirical scientific theories, is, at the same time, breaking with the Kantian, neo-Kantian, logical empiricist,

22 See Carnap's eloquent protest at the very end of "Empiricism, Semantics, and Ontology" (1950/1956, p. 221): "The acceptance or rejection of abstract linguistic forms, just as the acceptance or rejection of any other linguistic forms in any branch of science, will finally be decided by their efficiency as instruments, the ratio of the results achieved to the amount and complexity of the efforts required. To decree dogmatic prohibitions of certain linguistic forms instead of testing them by their success or failure in practical use, is worse than futile; it is positively harmful because it may obstruct scientific progress. The history of science shows examples of such prohibitions based on prejudices deriving from religious, mythological, or other irrational sources, which slowed up the developments for shorter or longer periods of time. Let us learn from the lessons of history. Let us grant to those who work in any special field of investigation the freedom to use any form of expression which seems useful to them; the work in the field will sooner or later lead to the elimination of those forms which have no useful function. *Let us be cautious in making assertions and critical in examining them, but tolerant in permitting linguistic forms*." Several pages earlier (pp. 218-219), Carnap discusses "nominalism" in arithmetic – which (as I have argued in the article cited in note 12 above) clearly alludes to the Harvard discussions with Tarski and Quine (and Goodman) concerning such nominalism in 1940-41.

and especially Carnapian *anti-metaphysical* conceptions of scientific philosophy as well.

This fundamental divergence between Quine and the earlier tradition is reflected (somewhat ironically) in the circumstance that Quine's distinctively holistic version of empiricism is motivated more by problems in the foundations of logic and mathematics than by any real engagement with the empirical sciences themselves. Whereas Helmholtz was a physicist and psycho-physiologist, and both Schlick and Carnap began their intellectual careers in physics, Quine, from beginning to end, was much more concerned with logical, ontological, and epistemological questions concerning the nature of mathematics than with the state of contemporaneous empirical science. Indeed, even Quine's naturalized epistemology, viewed as a branch of empirical (behavioristic) psychology, proceeds largely independently of the empirical psychology of his time.[23] It has been noted more than once that Quine can be illuminatingly viewed as playing Hegel to Carnap's Kant.[24] What we are now in a position to appreciate is that Quine thereby turns Helmholtz's original approach to scientific philosophy – including Helmholtz's own attempt, based on his ground-breaking contributions to the psycho-physiology of the senses, "to separate out from our knowledge and representation what originates from the influences of the physical world, in order purely to establish what belongs to the mind's own activity" (compare note 2 above) – almost completely on its head.

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23 This is especially clear in Quine (1974).

24 See, e.g., Schuldenfrei (1972).

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CARNAP'S *Untersuchungen*: LOGICISM, FORMAL AXIOMATICS, AND METATHEORY

INTRODUCTION

This paper discusses Carnap's attempts in the late 1920s to provide a formal reconstruction of modern axiomatics.[1] One interpretive theme addressed in recent scholarly literature concerns Carnap's underlying logicism in his philosophy of mathematics from that time, more specifically, his attempt to "reconcile" the logicist approach of reducing mathematics to logic with the formal axiomatic method. For instance, Awodey & Carus (2007) characterize Carnap's manuscript *Untersuchungen zur allgemeinen Axiomatik* from 1928 as a "large-scale project to reconcile axiomatic definitions with logicism, and transform implicit into explicit definitions." (ibid., 29) It is argued that Carnap's central idea was to balance a *Fregean* (or *Russellian*) foundational stance with the modern model-theoretic viewpoint introduced in Hilbert's *Grundlagen der Geometrie* (see (Reck 2004)). It was also shown in recent literature that Carnap's attempt to provide a logicist reconstruction of axiomatics is limited in several ways.[2] No closer attention, however, has so far been dedicated to some of the details of his proposed reconciliation.

The aim in this paper is to give a closer analysis of Carnap's theory of general axiomatics in *Untersuchungen*, specifically of the impact of a tacit logicist assumption underlying his semantics for axiom systems. The central notion to be investigated in this respect is that of a *logical interpretation* (or a *logical model*). Carnap mentions the term several times in his writings from the time without specifying its exact meaning. His understanding of the logical interpretability of an axiom system will be examined in comparison with similar accounts in the work of Alfred Tarski and Friedrich Bachmann. The subsequent investigation is guided by the following interpretive questions:

1. In what sense is Carnap's work on general axiomatics in *Untersuchungen* entangled with classical logicism?

1 Carnap's early contributions to axiomatics comprise two published articles, (Carnap 1930) and (Carnap & Bachmann 1936), his logic manual *Abriss der Logistik* (Carnap 1929), and the posthumously published typescript *Untersuchungen zur allgemeinen Axiomatik* (Carnap 2000).

2 Compare in particular (Awodey & Carus 2001), (Reck 2004), (Reck 2007), and (Bonk & Mosterin 2000).

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2. What is the specific role of the *logical interpretability* of axiom systems in his theory?

3. Given Carnap's logicist background, does his formal reconstruction meet the semantic innovations of modern formal axiomatics?

The discussion of these points will start with a brief presentation of Carnap's theory of general axiomatics and his definition of formal models (Section 2). Following this, the second question concerning his specific ties with logicism will be discussed in Section 3. In Section 4, it will be argued that Carnap's attempt to reconstruct the model theory of axiomatic theories is based on a specific logicistic premise, namely a definability condition for formal models. The details of this assumption will be discussed in Section 5 following the comparison of two similar accounts by Tarski (Section 4.1) and Bachmann (Section 4.2). Finally, in Section 6, the third question will be addressed. As will be shown, Carnap's logicist premise has strong limiting effects for his attempt to express the semantic metatheory of formal axiomatics.

CARNAP'S *General Axiomatics* IN 1928

Carnap's theory of general axiomatics is presented in *Untersuchungen* (Carnap 2000) and, in published form, in the second part of *Abriss der Logistik* (Carnap 1929, 70-72). The basic idea expressed there is that axiomatics is a kind of "applied logistic," i.e. an application of a simplified version of Russell's logical type theory (henceforth STT). Carnap's formal reconstruction is based on the distinction between two possible conceptions of axiomatics, viz. "contentual" ("*inhaltliche*") and formal axiomatics. In the former, the "primitive symbols" ("*Grundzeichen*") of a theory have a fixed meaning. Axioms and consequences of the theory make substantive claims about the logical relations of these interpreted terms. In the latter, the primitives are implicitly defined by the theory and do not have a fixed interpretation. This difference from contentual axiomatics is specified in the following passage in *Untersuchungen*:

> One can also understand the primitive concepts as unspecified elements and as relations of an unspecified domain where it is only stipulated that they relate to each other as specified in the axioms. In case that elements and relations are found in different domains that satisfy these formal specifications, then the axiom system can be applied to each one of these domains; in each of these domains also the consequences of the axiom system hold under the respective interpretation. (Carnap 2000, 88)[3]

3 "Oder aber man fasst die Grundbegriffe auf als unbestimmte Gegenstände und Beziehungen eines unbestimmten Gebietes, von denen nur festgelegt wird, dass sie sich so zueinander verhalten, wie es in den Axiomen bestimmt wird. Finden sich auf verschiedenen Gebieten Gegenstände und Beziehungen, die diese formalen Bestimmungen erfüllen, so kann das Axiomensystem auf jedes dieser Gebiete bezogen

Carnap adds that this is the "method commonly used by the mathematician," most notably in Hilbert's *Grundlagen der Geometrie* (Hilbert 1899). Now, as is generally known, Hilbert's book is paradigmatic for its systematic use of the formal axiomatic method for the presentation of a mathematical theory, in his case Euclidian geometry. Moreover, it also proposes a new understanding of the semantics of axiomatic theories. Without going into further historical detail, one can pin down at least three related semantic innovations in *Grundlagen*:

(i) the schematic conception of the primitive mathematical terms (i.e. of 'point', 'line', 'between', etc.) of the theory, and thus their reinterpretability;

(ii) the introduction of analytic models (Hilbert's "*system of things*") for the interpretation of a theory;

(iii) the systematic use of model construction and variation to prove metatheoretic results about the theory (such as its consistency, the independence of individual axioms, as well as different completeness properties).[4]

Points (i) to (iii) can in fact be viewed as adequacy conditions for any attempt of a logical reconstruction of modern axiomatics. In Carnap's theory from 1928, it is obvious from the passage cited above that condition (i), i.e. the reinterpretability of the primitive terms, is clearly met. Formally, this is done by the symbolization of the primitive symbols as free (higher-order) class and relation variables of STT. In consequence, axioms are expressed by propositional functions that range over the primitive terms. The axiom system is also presented as a propositional function (expressing the conjunction of its axioms). Systems with more than one primitive term of the form $f(P, Q, R, ...)$ are abbreviated by a 'model variable' ('*Modellvariable*') M of the next higher type-level (Carnap 2000, 88):[5]

$$f(R_n, S_n, T_n) \sim f(M_{n+1})$$

werden; auf jedem dieser Gebiete gelten dann auch in entsprechender Deutung die Folgerungen des Axiomensystems." (Carnap 2000, 88)

4 See (Hallett 2008) for a fuller discussion of Hilbert's methodological innovations: "The basic technique which Hilbert adopted for this investigation is that of modelling, more strictly, of translating the theory to be investigated into another mathematical theory. For this, it is essential (...) that the primitive concepts are not tied to their usual fixed meanings; they must be free for reinterpretation." (ibid., 211)

5 This treatment of axiomatic systems as propositional functions was first introduced in (Carnap 1929, 70-72). It should be noted that this convention of symbolizing axiomatic theories was common practice at that time. Carnap's logical presentation of primitive terms and axioms in STT was closely inspired by Frege's attempted reconstruction of Hilbertain axiomatics. A similar treatment of axiomatic primitives as variables is present in the works of Russell, the American postulate theorists, and Tarski. In fact, the same convention was also suggested by Hilbert himself three decades after his original controversy with Frege in (Hilbert & Bernays 1934, 7). See (Mancosu, 2006, 212-216) for a broader historical survey of this "widespread conception".

Carnap also makes a serious attempt to come to terms with Hilbert's semantic innovations (ii) and (iii), viz. the introduction and systematic use of formal models of a theory. Briefly, axiom systems are conceived by him as "theory-schemata" that allow different empirical and mathematical interpretations. The term "formal model" is reserved for the second type of interpretation. Carnap gives a detailed discussion of the notion in Section 2.3 of *Untersuchungen* (ibid., 95). In (Carnap 1930), the following shortened definition is given:

> If fR is satisfied by the constant R_1, where R_1 is an abbreviation of a system of relations $P_1, Q_1, \ldots$; R_1 is called a "model" of f. A model $f(R_1)$ is a system of concepts of the basic system, generally a system of numbers (number classes, relations and so forth). (ibid., 303)[6]

Carnap's definition has to be considered as an early attempt to capture in a logically precise way the notion of models in axiomatics. Whereas different informal version of the concept had been used in the 1920s and before, it is mainly here as well as in Tarski's work from the same time that the first formal explications of it were given.[7] This said, it is obvious that Carnap's definition of a formal model is – in several ways – untypical compared to the modern notion. In recent scholarly work, it was characterized as "something like model theory" (Awodey & Carus 2001, 145) or as an "early heuristic form of the modern concept" of models (Bonk & Mosterin 2000, 38) without, however, giving further qualification of where precisely Carnap's conception differs from the modern understanding. Some aspects are clearly anachronistic from today's perspective: first, note that models are devised for theories formulated not in first-order but in higher-order logic, particularly in STT. Moreover, Carnap is using in *Untersuchungen* a pure logical language with an empty signature, whereas, in the modern account, models are usually constructed for theories expressed in languages with a non-empty signature. This difference is evident in Carnap's conceptual architecture of models. Briefly, a model is nowadays understood as a tuple $\langle D, I\rangle$ where D designates the (non-empty) universe and I the interpretation function that assigns elements of D (subsets of D, relations on D, and functions from D^n to D) to the non-logical symbols of the language. In Carnap's version, there exists no interpretation function I, but only valuations to the variables R, S, T, i.e. to the primitive terms of the theory. A model M_1 is thus conceived as an n-tuple of relations of a specified type (or more precisely, as a tuple of relation constants) that are assigned to

6 "Wird fR durch die Konstante R_1 befriedigt, wobei R_1 Abkürzung für ein System von Relationen $P_1, Q_1, \ldots$ ist, so heißt R_1 "Modell" von f. Ein Modell ist ein System von Begriffen der Grunddisziplin, meist ein System von Zahlen (Zahlklassen, Relationen u.dergl.)." (ibid., 303)

7 See (Badesa 2004) for an account of the early model theory in the work of Löwenheim and Skolem. See (Mancosu 2010) for an overview survey of recent work on Tarski's early notion of a model. See (Jané 2006) for a more general discussion of the semantic notions 'system of things', 'application', and 'representation' in the work of the Italian 'Peanists' as well as the American postulate theorists Veblen, Young and Huntington.

the primitive symbols and that "satisfy" the axiom system f. Given this, we now turn to the specific logicist assumptions implicit in Carnap's early semantics for axiomatic theories.

A LOGICIST RECONSTRUCTION OF AXIOMATICS?

It was indicated in the Introduction that Carnap's theory can be considered as a 'logicist' reconstruction of formal axiomatics. This has to be refined. More than his logicist predecessors, particularly Frege, Carnap was aware of key methodological innovations of modern axiomatics. His use of 'applied logicistic' in the formalization of the mathematical primitives of a theory and its models was clearly intended to meet these desiderata. Moreover, neither in *Untersuchungen* nor in *Abriss* was the relation to logicism addressed in any way. There is one exception: at one point in (Carnap 2000), in introducing the logical "basic discipline" for his study of axiomatics, Carnap mentions Russell's project of the logical reduction of mathematics in *Principia Mathematica* only to stress that his own work on axiomatics is "independent of the mentioned (Russellian) conception." (ibid., 62) This suggests that there is no closer conceptual connection between Carnap's work on axiomatics and his more general logicist leanings in the philosophy of mathematics at that time. However, the situation is more intricate. As is well known, the logicist reduction of mathematics to logic depends crucially on the formal notion of *interpretability*. A mathematical theory T_1 (expressed in formal language L_1) is interpretable in a logical theory T_2 (expressed in the logical language L_2) in the following way: (1) the mathematical terms of T_1 are defined in L_2, i.e. for every primitive or defined concept P of T_1 a formula ϕ of L_2 is given that explicitly defines it; (2) the quantifiers of L_2 are relativized to the quantifiers of L_1. Given conditions (1) and (2), all axioms and theorems of T_1 can be expressed as sentences of L_2 that are deducible from the axioms of T_2. In the case of the classical logicist reduction, T_2 is a logical theory like Russell's theory of types and L_2 a pure (higher-order) logical language.[8]

How does this method relate to Carnap's logical reconstruction of formal axiomatics?[9] Evidently, in both cases the notion of a logical translation plays a central role. In Carnap's general axiomatics, this is the translation of a mathematical axiomatic theory into a fully interpreted type-theoretic system, his "basic discipline" ("*Grunddisziplin*"). The first step of this was outlined above: primitive terms are expressed as variables, the axioms and sentences of the theory as propositional functions of STT. There is a second step in Carnap's translation that has not been sufficiently considered so far: not only the theory but also its interpretations, viz. the formal models have to be expressed in pure logic. Thus, a full

8 For a more detailed definition see e.g. (Burgess 2005, 50-51).

9 Compare in particular (Reck 2004, 172-175) on this point. For a more general account of Carnap's evolving conceptions of logicism throughout his intellectual career see (Bohnert 1975).

logical reconstruction of an axiom system also includes the logical interpretation of its models. Carnap does not discuss this additional condition in *Untersuchungen* or in *Abriss*. It is mentioned explicitly, though, in his contribution to the Königsberg discussion on the foundations of mathematics in 1930. Here, in the attempt to reconcile the three foundational schools, he makes the following well-known remark on the "logical analysis of the formalistic system:"

> 1. For every mathematical sign one or more interpretations are found, and in fact *purely logical interpretations*.
> 2. If the axiom system is consistent, then upon replacing each mathematical sign by its logical interpretation (or *one of its various interpretations*), every mathematical formula becomes a tautology.
> 3. If the axiom system is complete (...), then the interpretation is unique; every sign has exactly one interpretation, and with that the formalist construction is transformed into a logicist one. ((Hahn et al. 1931, 143-144) quoted from: (Awodey & Carus 2001, 153, emphasis added))

Carnap's understanding of a 'logicist' reconstruction of the formalistic, i.e. the formal axiomatic method is clearly outlined here. Point 1 states the assumption that the primitive terms of an axiom system can be translated in logical, i.e. logistically defined terms. Point 2 states that the resulting sentences are truths of the background logic. This logical interpretability of mathematical theories indicates a novel conception of logicism that is closely related to but not identical with the classical logical reductionism of Frege and Russell. There, the common method was to present logical (and explicit) definitions the mathematical primitives of a theory first and then, in a second step, to show that each axiom of the theory in question can be deduced from the logical axioms of the background logic. In Carnap's general axiomatics, this method is somewhat reversed. Carnap starts with the formal specification of a particular axiom system and then, in a subsequent step, considers the logical construction of several possible models for this system. Nonetheless, it is clearly this account of "providing a logical interpretation" of theory (in combination with the condition (3) in the above passage) that connects Carnap's work on axiomatics with the logicist program. To show that an axiom system has a *single* logical interpretation that makes it "tautological" (i.e. logically true) is but a variant of the central claim of logicism, namely that the theory in question can be reduced to genuine logic. Given this general picture, one interpretive issue concerning Carnap's approach needs further clarification: what precisely does he understand by a "purely logical interpretation?" And: how is this notion related to his conception of formal models?

THE (LOGICAL) DEFINABILITY OF MODELS

One of the first commentators of *Untersuchungen*, the mathematician Hasso Härlen, already took notice of a logicist assumption in Carnap's treatment of the models

of an axiom system. In commenting on a draft of the manuscript in a letter dated from January 1, 1928, he remarks that:

> The existence of instances of application [is here] equivalent to the existence of models. This claim is hypothetical; I would like to name it the *hypothesis of logicism*: that no structure can be described axiomatically if it is not already describable in logic. (RC 081-01-34, emphasis added)[10]

And, in emphasizing the difference between Carnap's account and Hilbert's use of analytic models in (Hilbert 1899) he adds:

> Hilbert mapped geometry to a mathematical model, not to a formal one in your sense. Basically, this amounts to the same, but Hilbert's mapping does not differ principally from those of Lobatschefsky, Klein, and Beltrami. (RC 081-01-34)[11]

The conceptual difference between Hilbert's "mathematical models" and Carnap's "formal models" mentioned here is crucial. In Härlen's view, using the latter as presentations of the former is based on an additional assumption, a "hypothesis of logicism," namely that the interpretations of a mathematical theory can be "captured logically."

It is important to note that the logical definability of models mentioned here can be understood in two ways. In the first and weaker sense, the assumption simply requires that for every (consistent) mathematical axiom system, at least one logical model can be constructed. In the second, stronger sense, it requires that all models can be translated into logical models. Both versions of the definability assumption have been discussed in the recent literature on Carnap's general axiomatics, sometimes without sufficiently stressing their difference. For instance, in his discussion of *Untersuchungen*, Reck explicitly attributes the stronger assumption to Carnap: "Carnap assumed that every model of a higher-order theory is definable." (Reck 2007, 195) In discussing Carnap's proof of the central theorem in the manuscript, viz. the so-called "*Gabelbarkeitssatz*," he notes in another paper that:[12]

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10 "Vorhandensein von Anwendungsfällen [ist hier] gleichbedeutend mit Vorhandensein von Modellen. Diese Behauptung ist hypothetisch; ich möchte sie die Hypothese des Logizismus nennen: dass keine Struktur axiomatisch erfassbar ist, die nicht schon logisch erfassbar ist." (RC 081-01-34)

11 "Hilbert hat die Geometrie auf ein mathematisches Modell abgebildet, nicht auf ein formales in ihrem Sinn. Im Endeffekt ist das ja das gleiche, aber dem Sinn nach ist Hilberts Abbildg. nicht prinzipiell von denen Lobatschefskys, Kleins, Beltramis verschieden." (RC 081-01-34)

12 The theorem states the equivalence of three notions of completeness discussed by Carnap, i.e. categoricity ('monomorphism'), syntactic completeness ('deducibility') and semantic completeness ('non-forkability') (Carnap 2000, 133-139). Carnap's proof of the theorem was shown to be flawed for several reasons (see (Awodey & Carus 2001) and (Goldfarb 2004)). Nonetheless – taking into account Carnap's definability assumption for models – the equivalence between semantic completeness and categoricity does

Carnap implicitly assumes that, for any model M, "being isomorphic to M" is expressible in the simple theory of types. This assumes that any model M is definable in simple type theory, which is not true as became clear after Carnap's work. (Reck 2004, 170)

In contrast, in discussing the same proof, (Awodey & Reck 2002) describe Carnap's tacit assumption in the weaker sense: "In particular, he in effect assumed that any consistent theory has a model that is definable within simple type theory, which is false." (ibid., 35)

It is important to clearly distinguish between the two versions – weak and strong definability – given the fact that they have different implications for the formalization of the metatheory of axiom systems. We will return to the limiting effects of the definability constraints on models in the last Section. In this and the next Section, the question will be addressed which of the two versions Carnap actually assumed in *Untersuchungen*. Not surprisingly, it is difficult to give a conclusive response to this given the fact that the logicist assumptions were (mostly) left implicit in Carnap's writings on axiomatics from the 1920s. Nonetheless, one can get a clearer picture of his account by comparing it with similar accounts of a 'logicized' axiomatics by two of Carnap's contemporaries, Alfred Tarski and Friedrich Bachmann.

Tarski's "effective interpretability in logic"

Carnap's "Gabelbarkeitssatz" is usually discussed with reference to (Tarski & Lindenbaum 1935) where the theorem was stated correctly for the first time. Here, in contrast to *Untersuchungen*, the additional assumption concerning the logical definability of models is explicitly mentioned.[13] The main result of the paper is the formulation of a theorem that connects the logical definability of concepts to an invariance condition under permutations of the "universal domain V" of a simple type-theoretic language. The theorem states that all notions definable "by purely logical means", viz. in terms of STT, are invariant under all permutations of V and vice versa (ibid., 385).[14] Applied to mathematical theories, this invariance result leads to a second theorem concerning the definability of mathematical relations. Tarski and Lindenbaum paraphrase it in this way for their example of a theory of Euclidian geometry:

in fact hold for theories with a logical interpretation. See (Awodey & Carus 2001) for a detailed discussion of Carnap's proof and a more recent positive partial result by Dana Scott.

13 Compare, in particular, (Bonk & Mosterin 2000, 41-43), (Coffa 1991, 282), and (Awodey & Carus 2001, 60).

14 It should be noted that a similar invariance-based approach to logically definable notions is also presented in (Carnap 2000). Compare a passage on "structural properties" of relations where this is made explicit: "The structural properties are in a way the properties invariant under isomorphic transformations. They are of particular importance for axiomatics." (ibid., 74)

Intuitively speaking, every relation between objects (...) which can be expressed in terms of logic and geometry is invariant with respect to every one-one mapping of space onto itself in which the relation a is preserved, i.e. with respect to every similarity transformation (and not only with respect to every isometrical transformation) (ibid., 388)

Note that in the case of mathematical 'deductive theories', the relevant notion of definability is not definability within a pure logical language (like STT), but definability within a formal language with a non-empty signature, i.e. with non-logical terminology (say STT*). For the presented axiomatic system of geometry, Lindenbaum and Tarski then point out the limits of logical definability compared to geometric definability (in this extended logical language): briefly, there are geometrical relations that are definable in STT*, but not in STT (ibid., 389).[15] This has direct consequences for their modified treatment of the *Gabelbarkeitssatz*: the equivalence of semantic completeness (here "non-ramificability") and categoricity is conceived as a direct "application of [Theorem] 1," i.e. the first invariance theorem stated above. For this reason, an additional condition concerning the logical definability of the primitive terms of the axiomatic theory is mentioned. Their Theorem 10 states: "Every non-ramifiable axiom system which is effectively interpretable in logic is categorical." (ibid., 391) The "effective interpretability in logic" of a theory is specified in the following way:

[Theorem 10] (...) proves to hold under a supplementary assumption. We define an axiom system '$\alpha(a, b, c, ...)$' to be *effectively interpretable in logic* if there is in logic a sentential function '$\tau(x, y, z, ...)$' such that the following three logical formulas are logically provable:

$$(x, y, z, ...) : \tau(x, y, z, ...). \supset .\alpha(x, y, z, ...);$$
$$(\exists x, y, z, ...).\tau(x, y, z, ...);$$
$$(x', x'', y', y'', z', z'') : \tau(x', y', z', ...).\tau(x'', y'', z'', ...).$$
$$\supset .x' = x''.y' = y''.z' = z''.$$

(ibid., 390-391)

This convention to formalize axiom systems in STT closely resembles Carnap's approach. In Tarski's case, an interpreted theory is expressed by a sentential function $\alpha(a, b, c, ...)$ where $a, b, c, ...$ are logical constants that name "extralogical" primitive terms. A formal, disinterpreted system is then symbolized as $\tau(x, y, z, ...)$, i.e. as a sentential function of pure STT (i.e. without non-logical constants). The "effective interpretability" of axiomatic theories in logic can thus be understood as the translation of an interpreted theory of the form $\alpha(a, b, c, ...)$ expressed in STT* (i.e. STT with the signature $\{a, b, c, ...\}$) into a formal theory

15 Tarski and Lindenbaum note for the case of geometry: "The distinction between logic and geometry comes to light, however, in the discussion of three termed relations; for, as is well known, there are infinitely many three- (and many-termed) relations which are definable in Euclidian geometry." (ibid., 389)

of the form $\tau(x, y, z, ...)$ expressed in pure STT. This can be effected in two ways: in the above passage, the three conditions are supposed to fix that (i) the axiom system α is a consequence of the logical theory τ; (ii) τ is consistent, and (iii) τ holds of a unique sequence of values for $x, y, z, ...$ A second way to characterize the logical interpretability of a theory is also outlined in the article. It is based on an insightful reformulation of conditions (i) and (ii) of the above passage:

> (...) the axiom system '$\alpha(a, b, c, ...)$' is effectively interpretable in logic if and only if there are logical constants 'a'', 'b'', 'c'', ... (undefined or defined) such that the sentence '$\alpha(a', b', c', ...)$' obtained by substituting these logical constants for the primitive terms in the axiom system discussed is logically provable. (ibid., 391)

In this translation, the primitives of an interpreted theory expressed by the non-logical terminology $a, b, c, ...$ are substituted by the logical constants $a', b', c', ...$ of STT. The axiom system is thus effectively interpretable in logic if the system $\alpha(a', b', c', ...)$ can be deduced from the axioms of the logical background theory. This conception of a logical interpretation is similar to Carnap's account of logical models. Models are in both cases understood as tuples of logical or logically definable relations $\langle a', b', c', ...\rangle$ that are substituted for the primitive terms of the formalized axiom system. The translation involved here presupposes the construction of a particular model that consists solely of logical constants. Conceived in this way, the "effective interpretability" of an axiom system then simply states that there is at least one model of the theory that is definable in pure STT.

Bachmann's "logical constitution of a model"

A comparable account of a logical definability assumption can be found in a relatively unknown work of the mathematician Friedrich Bachmann, a later collaborator of Carnap. Bachmann's topic of his inaugural dissertation (Bachmann 1934), the formal presentation of different axiom systems of arithmetic and the study of their logical relations shares various points of contact with both (Tarski & Lindenbaum 1935) and (Carnap 2000). Unlike Carnap, however, Bachmann is explicit in his aim to provide a logicist presentation of axiomatic theories.[16] In the Introduction, he outlines this task in the following way:

16 In his Introduction, Bachmann speaks of an "allgemeine Untersuchungen zur Axiomatik der Arithmetik." There is no indication, despite the strong similarity to Carnap's *Untersuchungen* that Bachmann was aware of Carnap's 1928 typescript when working on his dissertation. Nonetheless, in Bachmann's bibliography, explicit reference is made to (Carnap 1929). Moreover, there exists an extensive discussion of the treatment of extremal axioms in (Carnap 1930, 46-47). This topic was also the subject of the later collaboration between the two (Carnap & Bachmann 1936).

The subject of this work is the problem of the deducibility of arithmetic from logic or – to be more precise – the problem of the deducibility of an interpretation of arithmetic from logic. (Bachmann 1934, Introduction)[17]

He mentions two "subtasks" for the "solution of the problem:"

1) The logical characterization of the models of arithmetic (the presentation of an axiom system of arithmetic).

2) The logical constitution of a model of arithmetic (the presentation of a system of constants and the proof that this system of constants holds in an axiom system of arithmetic. (ibid., Introduction)[18]

For Bachmann, these two steps constitute a method for a logicist reconstruction of axiomatic theories of arithmetic. The close ties to classical logicism are evident in the specifics of these two tasks. He discusses task (1) for different axiomatizations of "elementary arithmetic" (ibid., 27-52). As in Tarski and Carnap's accounts, theories are expressed as "sentential forms" ("*Aussageformen*") and the disinterpreted primitive terms as variables. Models are also conceived in essentially the same way as in (Carnap 2000). A model for the system of elementary arithmetic $f(x\ R\ \alpha)$ is specified in this way: "Every triple of values that, if substituted for $x\ R\ \alpha$, transforms the axioms into true sentences is called a model of arithmetic." (ibid., 1)[19]

Given these striking similarities, two points of difference to Carnap's account are worth mentioning here. The first concerns Bachmann's first subtask of the "logical constitution of a model," i.e. the "presentation of a system of constants" as a model for the theory of arithmetic. Unlike Carnap, Bachmann is explicit about the type of constants admissible for this task. He states that the terms constituting a model have to name mathematical concepts (like 'Zero', 'Successor', and 'Natural Number') that are logically definable. This is spelled out as an explicit condition for his task of showing the "deducibility of an interpretation" of arithmetic. What is missing (and probably deliberatively left unmentioned) in Carnap's treatment is the specific discussion of the "presentation" ("Aufweis") of a logically constituted model (ibid., 9). For Bachmann, in the case of elementary arithmetic, this task consists in the presentation of a sequence $\langle 0, \mathrm{Succ}, NN\rangle$, where each concept can be given an *explicit* reductive definition (in the underlying higher-order logic). Thus, the first task of step (2) is to present at least one purely logical model just as

17 "Der Gegenstand dieser Arbeit ist das Problem der Ableitbarkeit der Arithmetik aus der Logik oder – wie ich mich genauer ausdrücken möchte – das Problem der Ableitbarkeit einer Interpretation der Arithmetik aus der Logik." (Bachmann 1934, Introduction)

18 "1) einer logischen Charakterisierung der Modelle der Arithmetik (Angabe eines Axiomensystems der Arithmetik); 2) der logischen Konstituierung eines Modells der Arithmetik (Angabe eines Konstantensystems und Beweis, dass dieses Konstantensystem einem Axiomsystem der Arithmetik genügt)." (ibid.., Introduction)

19 "Jedes Wertetripel, dass in $x\ R\ \alpha$ eingesetzt die Axiome in wahre Aussagen überführt, heißt ein Modell der Arithmetik." (ibid.., 1)

in (Tarski & Lindenbaum 1935). A second difference to Carnap's *Untersuchungen* concerns Bachmann's second task of step (2), i.e. the "proof that this system of constants holds in an axiom system of arithmetic" (ibid., Introduction). Following the presentation of a logical model (as a sequence of logically defined constants) for the axiom system in question, Bachmann holds that one has "then [to] show of its definientia that they form a model of arithmetic" (ibid., 9). This amounts to the proof that the logically defined notions '0', 'Succ', and 'NN' "satisfy" the conditions specified in the axiom system (ibid., 18-26). Recall that in Carnap's definition of formal models, the relevant notions of 'truth' and 'satisfaction' were left informal, at least in 1928. Nonetheless, it is obvious that they were considered to be semantic in nature. In Bachmann's case, the proof that a model of the form $\langle 0, \text{Succ}, NN\rangle$ "satisfies" the axiom system is given "by exclusive use of the means of proof of logic" (ibid., 8). It is thus understood in purely syntactic terms, i.e. in terms of its logical derivability. Put differently, an interpretation holds in an axiom system if the sentence gained by the substitution of the variables in $f(x\ R\ \alpha)$ by adequate constants is deducible in the logical background system. Note that also in (Tarski & Lindenbaum 1935), the adequacy of an "effective interpretation" of an axiom system in logic is *not* expressed via a semantic notions of truth but in terms of the "logical provability" in STT.[20] This point clearly underlines the logicist background in these accounts. However, the logicism involved there is clearly not identical to Frege and Russell's original programs. Instead of showing the deducibility of arithmetic from logic, Bachmann is quite explicit that what he aims at is to show the deducibility of "certain interpretations" of arithmetic from logic (Bachmann 1934, 8). Given his two-step approach, it is important to note that the "logical constitution" of an interpretation of arithmetic means for him, just as in Tarski's case, the construction of a single logical model that is then used to show that the formula $\exists x \exists R \exists \alpha AS(x\ R\ \alpha)$ is deducible in the background logic. Here again, the definability condition seems to be understood in the weak form.

Nevertheless, there exists a crucial difference between Tarski and Bachmann that concerns the general motivation of their formal reconstructions of axiomatics. For Tarski, the "effective interpretation in logic" is conceived as one among alternative ways to formalize mathematical theories. Given the convention of expressing axiom systems as propositional functions with several variables, models can be presented either as sequences of non-logical concepts (of STT*) or as sequences of "defined or undefined" logical expressions of pure STT.[21] In contrast, the exclusive aim in (Bachmann 1934) is to devise a reconstruction of axiomatics close in spirit to classical logicist reductionism. In this understanding, the stronger definability assumption concerning models turns out to be relevant. Consider again Bachmann's second task of showing the "deducibility of an interpretation of arithmetic from logic," i.e. the presentation of a single "logically constituted model"

20 See also (Coffa 1991, 282-283) for a discussion of this point.

21 See (Gomez-Torrente 2009) for a broader survey of Tarski's "logical pluralism" in the formalization of mathematical theories in the 1930s.

that holds in the theory. Given his logicist motivation, any model of elementary arithmetic that can be subject to this second task *has* to be logical in the sense specified above. The stronger definability assumption is a natural consequence of this. It is also explicitly mentioned in Bachmann's book. In his discussion of the "proof that the system of constants holds for an axiom system," he gives an explanation why the models must be logical in his reconstruction:

> For the second task to succeed it is necessary that the three constants are constructed from the primitive signs of logic and bound variables, *in short: that they are logical constants*. A sentence with nonlogical constants is only deducible from logic if it is transformed into a universally valid sentential form in case the constants are substituted by variables. Since the sentential form A-ASI(x, R, α) is not universally valid, this approach is not possible. (ibid., 8-9, emphasis added)[22]

The arithmetical theory A-AS I(x, R, α) is not universally valid since one can construct "non-models" that fail to satisfy it (ibid., 5-6). Consequently, Bachmann argues, any model constructed for the theory has to consist exclusively of logical constants in order to allow the proof of the validity the interpreted system. Thus, in his specific presentation of axiomatics, all models relevant for consideration have to be "logically constituted" in this sense.

CARNAP'S LOGICAL MODELS

How do Tarski's "effective interpretability in logic" and Bachmann's "logical constitution of a model" relate to Carnap's definability assumption for models? In the latter's remarks on the "logical analysis of the formalistic system" in Königsberg, the intention was obviously to provide a logicized version of formal axiomatics close in spirit to both Tarski's and Bachmann's accounts. Moreover, his talk of "one or more (...) in fact purely logical interpretations" of the mathematical primitives suggests that Carnap too proposed a weak version of logical definability in 1930.[23] However, it is obvious from (Bachmann 1934) that if the logicistic reconstruction is considered as the exclusive method to treat axiomatic theories, it effectively implies the stronger assumption, namely that all relevant models of a given theory are logically definable. Turning to *Untersuchungen* now, it was noted above that Carnap's primary aim here was not to provide reconstruction in

22 "Damit die Lösung der zweiten Aufgabe gelingt, ist es notwendig, dass die drei Konstanten aus den Grundzeichen der Logik und gebundenen Variablen aufgebaut, kurz: dass sie logische Konstanten sind. Eine Aussage über ausserlogische Konstanten ist nämlich nur dann aus der Logik ableitbar, wenn sie bei Ersetzung der Konstanten durch Variable in eine allgemeingültige Aussageform übergeht. Da nun die Aussageform A-AS I(x, R, α) nicht allgemeingültig ist, ist dieser Weg nicht gangbar." (ibid., 8-9, emphasis added)

23 A similar account of the "logical interpretation" (in contrast to the "descriptive interpretation") of an axiomatic theory in STT can still be found in (Carnap 1939).

Bachmann's terms. Carnap stresses in the introduction of the manuscript that the principle objective is to provide formal explications, i.e. "*precise definitions for the concepts*" used in axiomatics and *not* a Fregean or Russelian reduction (Carnap 2000, 59). In this sense, Carnap's account in 1928 is closer in spirit to Tarski's discussion of deductive theories. Nonetheless, his theory differs from the latter's pluralist account with respect to the specific logical framework. Tarski considered different kinds of formal languages in his work on the "methodology of deductive sciences:" pure logical languages (like STT) as well as mathematical languages (including non-logical terminology). As a consequence of this, different kinds of model construction are possible. The logical interpretability of an axiom system outlined above is one possible case here. Another, clearly non-logicist convention to express the semantics of a theory is the introduction of (a sequence of) *non-logical*, mathematical constants $\langle a, b, c, ...\rangle$ that are assigned to the variables of an axiom system $\alpha(x, y, z, ...)$. This approach is ruled out in Carnap's framework given his exclusive focus on the definability of models in pure type-theoretic logic. Thus, the fundamental difference to Tarski's account is that, for Carnap, all possible interpretations of an axiom system are to be expressible in his "basic system." This tacit condition of the *logical interpretability* in STT is evident in Carnap's substitutional conception of models. Consider the following passage in *Untersuchungen* where he further specifies the notion:

(...) in short, we speak of "models" of an axiom system und thereby mean logical constants, i.e. "*systems of concepts of the basic discipline*" (these are mostly systems of numbers). (ibid., 94)[24]

This point is also highlighted in his distinction between "formal models" and "realizations," i.e. empirical interpretations of an axiom system:

As the values of the primitive relations of an axiom system both logical as well as nonlogical constants can occur; the axiom system can either be applied to concepts of the basic discipline or also to real concepts ("Realbegriffe") (concepts of a nonlogical, empirical state of affairs). (ibid., 93)[25]

In this conception, models are exclusively composed of the logical terms of the "basic discipline". The same account can also be identified in a later, thematically related paper, (Carnap & Bachmann 1936). In specifying the notion of an isomorphism correlation between "n-place models (i.e., sequences with n members)" the authors hold that such a correlation is defined "(...) over the field of this relation, i.e., over the totality of constants of the basic, assumed language which

24 "(...) wir sprechen kurz von "Modellen" eines Axiomensystems und meinen damit logische Konstanten, also "*Systeme von Begriffen der Grunddisziplin*" (und zwar sind dies meist Systeme von Zahlen)." (ibid., 94)

25 "Als Werte einer Grundrelation eines Axiomensystems können sowohl logische als auch nicht-logische Konstanten auftreten; das Axiomensystem kann angewendet werden auf Begriffe der Grunddisziplin und auch auf Realbegriffe (Begriffe eines nichtlogischen, empirischen Sachverhalts.)" (ibid., 93)

can appear as elements of models (...)" (ibid., 74). Here again, the underlying language is a pure and higher-order logic. This substitutional conception of models is still present in the first English edition of *Logical Syntax of Language* from 1937. A small section not included in the original German version is devoted to the "axiomatic method" (Carnap 2002, Section 71e). A definition of models and model domains is given here that is comparable to the ones in 1928 and in 1936:

> In the first method, the domain of the interpretations of a certain primitive symbol is the domain of the substitution-values of the variable. If, as is usual, it is a case of primitive variable within a system of types, then the same relations of types must hold between the symbols of the model as hold between the corresponding primitive variables. (ibid., Section 71e)

Each of the passages cited here describes the same picture: formal models are conceived as n-tuples of relational terms with a fixed meaning in the interpreted background logic. The assignments to the "primitive variables" of an axiomatic theory are not understood extensionally in terms of objects, sets, or relations, but substitutionally in terms of substitution-values of STT.

This substitutional conception of models imposes strong conditions on the scope of logical expressions in Carnap's logical background theory. The expressibility condition for models effectively presupposes that there is a range of logical constants in STT comprehensive enough to express all classes, relations, etc. used in models. Typically, constant expressions can be added in two ways to a formal language. They can be stipulated at the outset with the specification of the primitive symbols of the language. They can also be introduced via definitions based on the given vocabulary. For example, if a certain formula of the language specifies a certain class of (or relation on) the intended domain of individuals, a new constant can be added to the language to name this set (or relation). A similar approach can be found in Carnap's specification of his "basic discipline" in *Untersuchungen*. The main difference to a modern conception lies in the fact that the constant expressions are all logical here. Thus, the language does not contain non-logical expressions (i.e. "external signs" ("Fremdzeichen") denoting possible "external concepts" of an axiom system) (Carnap 2000, 89-90).[26] In setting up his background theory, Carnap gives the following classification of the possible logical terms:

1. proper logical constants ($\forall, \exists, \rightarrow, \neg, ...$);
2. terms of "absolute arithmetic" (successor, number, 0, 1, 2, ...);
3. terms of "absolute set theory" (class, $\in$);
4. terms of relation theory (ibid., 59-60).[27]

26 "The present investigation discusses, as is usual in mathematics, only axiom systems without external concepts." (ibid., 90)

27 Compare Carnap: "By "logical constants" we understand all signs of the basic discipline, i.e. the signs of logic in the closer sense (...) as well as the arithmetical signs

Combined, they constitute the syntactic resources of his "basic discipline" from which the models of a theory are constructed. Two points should be noted here. First, the notion of logical concepts is obviously conceived here in a much more general sense than in the modern account. The logic includes not only the proper logical terminology of (1) but in addition the genuinely mathematical terms from arithmetic, set theory, and most importantly, relation theory. Compare Carnap's following remark on this generalized account of logic:

> The schools disagreeing on the question of the proper foundation of mathematics will come to agree that in the basic discipline the usual arithmetical, set-theoretical and also logical concepts have to be included; we want to designate the concepts of these fields (...) with the unifying expression "logical concepts." (ibid., 62)[28]

Given this generalized "scope of logic," the question arises how the expressions of (2) to (4) are related to the proper logical constants of (1). Carnap is more or less silent in *Untersuchungen* on how the terms of set theory, relation theory, and arithmetic enter into the "basic discipline." He does mention that these concepts could be logically constructed from proper logical concepts by reductive definitions as in the work of Frege and Russell (ibid., 61-62). He leaves undecided, however, whether they should be conceived as having such a "logistic meaning." In fact, in a side remark, Carnap is quite explicit that he is not interested in the logicist project in *Untersuchungen*:

> For the following investigation of axiomatic problems it is irrelevant whether the set-theoretical and arithmetical concepts are presented as independent primitive concepts or whether they are deduced from those concepts of logic. (ibid., 62)[29]

Thus, he deliberately leaves open the question whether primitive mathematical notions like '0', '1', '2', '+', '<', '>', '$\in$', etc. should be treated as undefined primitive logical constants or as constants defined from the core logical vocabulary of STT.[30] However, Carnap's indifference concerning this question in *Untersuchungen* is doubtful on closer inspection. The generalized account of logical concepts

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(e.g.: 1, 2, + etc.)." (ibid., 89) At other places of *Untersuchungen*, also the signs of set theory and relation theory are mentioned.

28 "Die in der Frage der Begründung der Mathematik auseinandergehenden Richtungen werden darüber einig sein, dass in der Grunddiziplin die üblichen arithmetischen, die üblichen mengentheoretischen und außerdem noch logische Begriffe vorkommen müssen; die Begriffe dieser Gebiete wollen wir (...) mit der zusammenfassenden Bezeichnung "logische Begriffe" nennen." (ibid., 62)

29 "Für die folgenden Untersuchungen axiomatischer Probleme ist es indessen nicht von Belang, ob die mengentheoretischen und die arithmetischen Begriffe als selbstständige Grundbegriffe aufgestellt sind oder aus denen der Logik abgeleitet werden." (ibid., 62)

30 This marks a crucial difference to the decidedly logicistic account in (Bachmann 1934). Recall that, for Bachmann, the relevant logical constants used in a model of axiomatic arithmetic are particularly those mathematical terms that can be given a logicist definition in pure logic (Bachmann 1934, 8-9).

is closely conceptually tied to a logicist approach. The whole idea of classifying genuinely mathematical terms (like 'number', 'successor', 'point', 'betweeness' etc.) as logical is based on the fact that they can be defined in proper logic. This point is explicitly stated in later work by Carnap, e.g. in (Carnap 1939), where the following description of the expanded class of logical signs is given:

> Further, all those signs are regarded as logical which are definable by those mentioned [GS: those of (1)]; (...), all signs of the system of [Principia Mathematica] by Whitehead and Russell and of nearly all other systems of symbolic logic, all signs of mathematics (including arithmetic, analysis of real numbers, infinitesimal calculus, but not geometry) with the meaning they have when applied in science (...). A defined sign is descriptive if its definiens contains a descriptive sign, otherwise logical. (ibid., 57-58)

It is precisely this condition for logical constancy that also seems to underlie Carnap's conception of the "basic discipline" in *Untersuchungen*: a sign is logical if it is a logical primitive or if it can be defined in terms of primitive logical expressions.[31] Irrespective of this, with Carnap's substitutional conception of models and his generalized scope of logic in mind, one can gets a more refined understanding of his tacit definability assumption in *Untersuchungen*: All models of a given axiom system are expressible in the "basic discipline" in the sense that all constitutive parts (sets, relations, and individuals) of a model are named by constant expressions of STT. These constants are (i) either conventionally fixed to be part of the basic vocabulary or (ii) introduced via explicative definitions in STT.

A RESTRICTED METATHEORY

Carnap's project of general axiomatics in *Untersuchungen* is based on the following tacit logicist assumption: mathematical models such as the analytic models in (Hilbert 1899) are to be reconstructed as logical models. Given this, to what extent does his theory capture the semantic innovations of modern axiomatics? To anticipate, his logicist premise concerning the logical expressibility of all models appears to have strong, limiting consequences for his reconstruction

31 A comparable conception of the scope of logic can be identified in the work of Carnap's contemporary Tarski from the 1930s. See (Goméz-Torrente 1996) on his conception of logical notions: "(...) Tarski reserves his most inclusive use of the word 'logic' for a system of logic based on the theory of types (...), such a "logic" is a system, therefore, in which arithmetical constants can be defined in terms of logical constants, and arithmetic developed as logic." (ibid., 134) Compare also (Goméz-Torrente 2002) for a more general presentation of this view: "(...) all primitive symbols denoting notions in that language (...) are (logical constants)^{T}; also, if the definition were applicable to defined symbols, all these symbols would be (logical constants)^{T}. Such results agree well with (...) traditional practice (for example the practice of the logicists, but others as well) according to which the constants of the language of the theory of types are logical constants." (ibid., 16)

of the metatheory of axiomatic systems. It effectively restricts the class of possible models of a theory to those expressible in his background logical system, the "*Grunddisziplin*."[32] This is not necessarily restricting in the case of concrete model construction ("Modellaufweis") for the mathematical theories discussed in (Carnap 2000). In these cases, the assumption calls for a the presentation of a particular logical model in the sense of Tarski's "effective interpretability in logic" or Bachmann's "presentation of a logically constituted model."[33] Where Carnap's assumption imposes real constraints compared to modern model theory is in cases that involve a generalization over all models of a theory. This is usually expressed in a separate metatheory in modern practice, e.g. in ZFC. In Carnap's account from the late 1920s, in contrast, talk of "all models" and also of "truth in all models" is expressed in a single STT. Recall that models are conceived in Carnap (2000) as n-tuples of relational constants of the form $M_1 = \langle R_1, S_1, T_1, \dots \rangle$. Given this, the generalization over models is then symbolized in terms of the higher-order quantification over relations (of the type-level of M_1). Thus, one central idea in Carnap's approach is the translation (or the embedding) of the generalization over interpretations of a theory into universally quantified formulas of the type-theoretic language. This decidedly metatheoretic use of higher-order quantification can be found at several places in his work on axiomatics from the time. Four examples are mentioned here:

(1) Carnap's treatment of the notion of *logical consequence* in terms of the higher-order sentence:

$$\forall R(f(R) \rightarrow g(R))$$

Here, the propositional function $f(R)$ expresses the axiom system and $g(R)$ expresses a consequence of $f(R)$. Such "Lehrsätze" are defined as follows in *Untersuchungen*: "gR is called a consequence of fR, if $\forall R(fR \rightarrow gR)$ is valid." (Carnap 2000, 95, notation slightly changed) Carnap gives a further specification: "If $f \rightarrow g$ is valid, that is, if all models of f are also models of g, we can also say: the extension ("Umfang") of f is a "part" of the extension of g." (ibid., 95) Clearly, R is understood here as a model variable that ranges over all possible models of f.[34]

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32 As mentioned above, this point was already made in (Awodey & Carus 2001), (Awodey & Reck 2002), and (Reck 2004) with respect to Carnap's flawed proof of his *Gabelbarkeitssatz* in (Carnap 2000). As the present section aims to show, Carnap's definability assumption for models has limiting effects beyond this that concern his semantic metatheory more generally.

33 It was shown by Bachmann (1934) that different models can be built for Peano arithmetic via explicit 'logicist' definitions of the primitive terms '0', 'Succ' and 'NN' (ibid. 10-27)

34 Compare (Reck 2007) for an extensive discussion on Carnap's understanding of the notion of consequence. Reck points out that there is "an ambiguity in his definition of the notion of deducibility, or of logical consequence more generally" that concerns the conditions under which a "Lehrsatz" is valid for Carnap (see ibid., 188-189).

(2) The formal explication of "extremal axioms" in the projected second, unpublished part of *Untersuchungen* (and later, in published form, in (Carnap & Bachmann 1936)). Briefly, these 'meta-axioms' are introduced into a theory in order to impose minimal or maximal conditions on the possible interpretations of the theory. Carnap's main example of a mathematical maximal axiom is Hilbert's *axiom of completeness* in the second edition of *Grundlagen*. Carnap's formal reconstruction of it is this:

$$Max(F; M) =_{df} \neg(\exists N)(M \subset N \wedge M \neq N \wedge F(N))$$

Informally, the axiom states that for a given theory F and a model M of F, there exists no proper extension of M that also satisfies F (Carnap & Bachmann 1936, 178).

(3) The formal notion of categoricity (in Carnap's terms "monomorphism") in *Untersuchungen*. Based on a formal presentation of the notion of "model isomorphism," a theory f is categorical if

$$(\exists)f \wedge \forall P \forall Q[(fP \wedge fQ) \rightarrow Ism_q(P, Q)]$$

is true in STT. The first conjunct expresses that the theory f is satisfied and the second conjunct that for any two models P and Q of f there exists an "isomorphism correlation" (of type-level q) between P and Q (ibid., 128-129).

(4) An implicit generalization over the possible models of a theory can be identified in Carnap's notion of the "Explizitbegriff" of an axiom system presented in (Carnap 1929). According to him, an axiom system not only provides implicit definitions of its primitive terms. It also explicitly defines a higher-order concept, i.e. a (logical) "Explizitbegriff," whose extension is the class of models that satisfy the theory. For instance, for a theory with n primitive terms, the respective "Explizitbegriff" denotes a higher-level n-ary relation (in modern terms, a set of n-tuples). Each of these tuples presents a model of the theory. Carnap's original formalization of this in terms of set comprehension (ibid., 71-72) can be reformulated as an universal conditional sentence of the form

$$\forall R \forall S \forall T(AS(R, S, T) \rightarrow EX_{AS}(R, S, T))$$

where R, S, T express the theory's primitive terms and EX_{AS} its "Explizitbegriff."

(1) to (4) clearly express 'metatheoretic' statements about the (class of models of an) axiomatic theory. Again, they are not expressed in a seperate metalanguage, however, but in a single, universal background logic. Note that Carnap understood STT as a fully interpreted system.[35] Thus, the constant expressions as well as the

35 Compare the following related remark in *Untersuchungen*: "Every treatment and investigation of an axiom system therefore presupposes a logic, specifically a contentual logic, i.e. a system of sentences that are not merely combinations of signs but that have a particular meaning." (Carnap 2000, 60)

quantifiers come equipped with a fixed semantic interpretation. It follows from this that each of the metatheoretic sentences above expresses a truth of STT, i.e. it is *true in the intended interpretation* of STT. Carnap's strategy in *Untersuchungen* (as well as in related work on axiomatics) was thus to express metatheoretic claims in terms of logical truths of the universally interpreted background system. For instance, the modern model-theoretic of logical consequence:

$$\text{For all models } M: \ \Gamma \models_M \varphi$$

is expressed by Carnap's (1), which – again in modern terms – can be reconstructed as:

$$\models_S \forall M(\Gamma(M) \rightarrow \varphi(M))$$

where S is Carnap's intended interpretation of STT.[36] Obviously, this convention of expressing formal semantics is dependent on the tacit semantics of STT. More precisely, the generalization over models of a theory expressed in the notions above is strongly is directly related to the range of the higher-order quantifiers used in (1) to (4). Unfortunately, Carnap did not get specific about his intended interpretation of his "applied logistic." Neither did he make any remarks on the connection between the semantics of STT and the formal explication of metatheoretic notions that involve higher-order quantification. Nevertheless, there is strong reason to believe that he conceived the higher-order domains of his background language in a nonstandard sense. It follows from Carnap's logicist premise concerning formal models that the quantifiers used in (1) to (4) are relativized to those models of a theory that are expressible in STT. Thus, quantification over models is effectively restricted to those higher-order entities of the type-theoretic universe whose elements are definable in the system. This nonstandard treatment of quantifiers is also directly implied by his substitutional conception of models. Generally, it can safely be said that prior to *Logical Syntax of Language* (Carnap 1934), he upheld a substitutional conception of higher-order (as well as of the first-order) quantification.[37]

Now, it is obvious that the adequacy of formal reconstructions of metatheoretic notions along the lines of (1) to (4) depends crucially on the interpretation of

36 A different, decidedly non-semantic reconstruction of consequence and comparable notions can be found in (Bachmann 1934). Here, consequence is characterized purely syntactically in terms of the provability, i.e. deducibility from the logical axioms of the background logic: $\vdash \forall M(\Gamma(M) \rightarrow \varphi(M))$.

37 This becomes obvious in Carnap's correspondence with Gödel from 1932 on the proper understanding of the notion of 'analyticity' for formulas in a type-theoretic language. In an early version of analyticity for formulas with a second-order quantifier presented to Gödel, Carnap apparently suggested that a sentence $(X)X(0)$ is analytic iff $F(0)$ is analytic for all predicate constants F of the language. Gödel objections to this use of substitutional quantification eventually led Carnap to a change in mind concerning his conception of higher-order quantification and, eventually, to the adoption of an extensional treatment in *Logical Syntax*. Compare (Awodey & Carus 2007, 37-38) and (Coffa 1991, 290-293) for detailed discussions of this correspondence and its effects on (Carnap 1934).

the higher-order quantifiers of the language in use. It was recently shown that several early contributions to modern axiomatics – e.g. Hilbert's work on Euclidian geometry in 1899, Dedekind's work on natural numbers in (Dedekind 1888), etc. – include an investigation of the theory's own metatheory, i.e. of several metatheoretic notions (such as completeness properties, categoricity, consistency and independence results, etc.) ((Hintikka forthcoming), (Hallett 2008), (Awodey & Reck 2002)). Moreover, it was argued that the informal logic used for the discussion of these notions was understood to be fully interpreted. For instance, Shapiro (2005) shows convincingly that in Hilbert's *Grundlagen*, one can identify an "assertoric," full use of (higher-order) logic in the discussion of the semantic metatheory of his system. Thus, when Hilbert uses the prefix "for all systems of things" in his work (e.g. in his *axiom of completeness*), he likely means quantification over every mathematically possible model of the theory. In other words, talk about "all (possible) models" of a theory seems to be based on a full, standard interpretation of the underlying informal logic. One could add that the set-theoretical resources necessary for expressing this are best codified in a higher-order system (or alternatively in ZFC) with a standard interpretation.[38] It follows from this that any formal reconstruction has to employ equally strong logical resources if the aim is to capture the *Hilbertian*, i.e. the modern axiomatic practice. Now, it was shown that, in Carnap's account, the effective range of the quantifiers in metatheoretic sentences like $\forall M[\Gamma(M) \rightarrow \phi(M)]$ is restricted to those instances of M that are explicitly expressible in his "basic system." Thus, the quantifiers in (1) to (4) effectively determine the set of logical models and not the set of possible mathematical models of an axiomatic theory. Carnap's early formal metatheory therefore remains limited with respect to the model-theoretic innovations of modern axiomatics.

Outlook

Carnap's project in *Untersuchungen* was eventually abandoned as a direct reaction to the subsequent developments in metalogic, in particular Gödel's incompleteness theorems and Tarski's truth theory (see (Awodey & Carus 2001) and (Goldfarb 2005)). With Carnap's later turn to a purely syntactical investigation of mathematics in *Logical Syntax of Language*, also the explicitly semantic framework of his "general axiomatics" was given up. This holds in particular for his notion of formal models. The concept is not mentioned in (Carnap 1934). More surprisingly, also his later work on semantics, most notably the three-volume book project *Studies in Semantics*, remains silent on the notion. Instead of discussing formal models, Carnap introduces here the substitute concepts "state of affairs," and "state

38 This point is obvious when looking at the modern formal treatment of these metatheoretic notions. For instance, it is well known that the proofs of categoricity for geometry, analysis, and Peano arithmetic are usually given in a set-theoretical metatheory that allows the generalization over all models of the respective theory. See (Shapiro 1991, 82-84).

descriptions" (see (Carnap 1942) and (Carnap 1947)). Only in his very late work, in his 'Replies and Expositions' in the Schilpp-volume (Schilpp 1963), the notion is eventually reintroduced in print. In Section 10 of the book – titled "My conception of semantics" – the following definition of a 'model' is given:

> A model for a language (in the extensional sense of "model" customary in mathematics, as in the definitions by Tarski, Kemeny, and others) is an assignment of extensions of the following kind: To every type of variables a class of entities of this type is assigned as the range of values, and to every primitive constant of the type system an extension of the same type is assigned. (ibid., 902)

The notion outlined here essentially conforms with the way models are understood today. Interestingly, following a brief remark on the 'structure' of a model, Carnap also refers to his early work on axiomatics here. In a footnote he adds: "For more exact definitions, especially with respect to axiom systems, see Carnap and Bachmann [1936]." This reference can give the impression of a continuity between his original understanding of models of axiomatic theories in *Untersuchungen* and the late conventionalized account in 1963. However, in light of the present paper, this shows to be more of a retrospective idealization than an adequate account of the theoretical evolution of this notion in his work. It was shown that the conception of models in 1928 (and still in 1936) differed significantly from the above specification of "model(s) for a language." It would be interesting to retrace Carnap's evolving views on semantics and on formal models in particular in his subsequent work up to Schilpp (1963). As we see it, at least three pressing questions are still open for closer discussion: (1) Which influences led to the shift from Carnap's substitutional conception of models to a modern understanding in terms of an "assignment of extensions" outlined above? (2) At what point did he replace his 'universalist' ideal of a pure and fully interpreted logic by the use of schematically understood languages with non-logical terminology? (3) Finally, when did Carnap adopt the modern notion of domain variation for models. Addressing these questions would call for a closer study of the few documents (mostly in informal correspondence and in discussion notes in the *Nachlass*) where the notion of models surfaces in Carnap's later philosophy. This includes his notes of the Harvard discussions with Tarski and Quine from 1940/41 as well as his correspondence with Kemeny and Bar-Hillel on issues in inductive logic in the early 1950s. Closer investigation of these discussions would allow a deeper understanding of the conceptual transitions in Carnap's thinking about formal semantics throughout his intellectual career. This is work for another day.

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Realism as a Problem of Language – From Carnap to Reichenbach and Kaila

1. Introduction

Rudolf Carnap's role in the debate over scientific realism is fairly unclear. In a certain sense, Carnap must be regarded as the one who rendered the whole issue irrelevant. However, it cannot be ignored that Carnap sometimes spoke of himself as an 'empirical realist.' So the question to be answered is: in what sense, if at all, did Carnap play a *constructive* role in the scientific realism debate. It is the aim of the present paper to tackle this question by investigating the relationship between Carnap's approach toward the realism issue, on the one hand, and the (presumably) realist positions defended by his logical empiricist fellows Hans Reichenbach and Eino Kaila, on the other. It will be shown that Carnap agreed with Reichenbach that realism has essentially to do with language, but that he disagreed with him over the significance of probability in defending the scientific realist stance. My point will be that realism is *not* a 'problem of language.' Furthermore, it will be argued that Carnap was *correctly* criticized by Reichenbach for neglecting the role of probability in science. Nevertheless, what can be *learned* from Carnap's approach toward the realism issue is that scientific realism cannot be defended in the way that Reichenbach himself suggested, namely by arguing inductively for the adequacy of the realistic 'language form.' Rather, scientific realism can only be defended on the ground that language itself is dependent on the 'structure of the world.' And this is exactly the point where, eventually, Kaila will enter the scene.

2. Carnap's Original Contribution: The Realism Issue as a 'Pseudo-Problem'

To begin with, Carnap's initial systematic treatment of the realism issue is found in his 1928 booklet *Scheinprobleme in der Philosophie* (translated as *Pseudoproblems in Philosophy*). As is well known, Carnap attempts to clearly state that any talk of the 'reality' or 'non-reality' of (both) the outer world (and other minds) is completely devoid of meaning. More exactly speaking, the realism issue *as such* is, for Carnap, incapable of being solved by rational means. It is a "pseudo-problem" and therefore to be excluded from science. In Carnap's own words: "In the realism

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controversy, science can take neither an affirmative nor a negative position since the question has no meaning."[1]

Carnap's rationale for removing the realism issue from the context of science is nicely illustrated by his example of the two geographers, the one a realist, the other an idealist: Commissioned to find out whether a mountain that is supposedly somewhere in Africa really exists, they both, Carnap maintains, will come to the same (be it positive or negative) result. They will agree on the relevant criteria concerning the *empirical reality* of the mountain. Furthermore, given that the mountain exists, they will come to the same result about its position, shape, height, etc. "In all empirical questions," Carnap states, "there is unanimity."[2] Yet, as soon as the two geographers begin to speak as philosophers, that is, as soon as they attempt an *ontological interpretation* of the empirical results, disagreement will unavoidably arise. While the realist will claim that the mountain, which they have both found, not only has the ascertained geographical properties, but is, in addition, also real, the idealist will contest this claim, arguing that the mountain itself is not real, but only our perceptions and conscious process are real. However, this ontological divergence between the two geographers does not occur in the empirical domain. Neither of the disputants, says Carnap, would suggest that his thesis be tested experimentally or by other empirical means. Therefore, Carnap concludes, the choice of a philosophical viewpoint has (besides certain "emotional accompaniments") "no influence upon the content of natural science."[3]

In his early major work, *Der logische Aufbau der Welt* (translated as *The Logical Structure of the World*), which also appeared in 1928, Carnap makes the same point by stressing the 'linguistic character' of the realism debate. By 'linguistic character,' I mean Carnap's description of the realist position as a certain form of *language*. It is well known that the central aim of Carnap's *Aufbau* was to establish a *Konstitutionssystem der Begriffe* or, as it is commonly translated, a constructional system of concepts. This constructional system of concepts is characterized by Carnap as equivalent with what might be called the 'practising scientist's empirical realism.' Thus, in § 52 of the *Aufbau* Carnap writes:

> The realistic language, which the empirical sciences generally use, and the constructional language have actually the same meaning: they are both neutral as far as the decision of the metaphysical problem of reality between realism and idealism is concerned. It must be admitted that, in practice, linguistic realism [*sprachliche Realismus*], which is very useful in the empirical sciences, is frequently extended to a metaphysical realism; but this is a transgression of the boundaries of science […]. There can be no objection against such a transgression, as long as it influences only the mental representations which accompany the

1 Rudolf Carnap, *Pseudoproblems in Philosophy*, translated by R. A. George. London: Routlegde 1968, p. 333.

2 Ibid.

3 Ibid.

scientific statements; this transgression is objectionable only if it influences the content of the statements of science.[4]

Thus, a distinction must be drawn between a *scientifically useful* form of realism and a *metaphysically 'puffed up'* form of realism, the latter being declared as transgressing the boundaries of science. But what is the criterion for making that distinction? As Michael Friedman has recently pointed out, in the *Aufbau* it is not the (infamous) principle of verifiability that underlies Carnap's "anti-metaphysical attitude".[5] Rather, it is Carnap's *equating of science and rationality*,[6] by which the demarcation is effected. That is, for the Carnap of the *Aufbau*, the "empirical concept of reality" is *implied* by the rational methods of science, whereas the "metaphysical concept of reality" is *ruled out* by these very methods. It can be said without much exaggeration that this way of putting the issue leaves open more questions than it resolves.[7]

However, it is interesting to see that Carnap's point of view during the 1930s became the predominant attitude toward the realism issue within the Vienna Circle. The circle's founder, Moritz Schlick, for instance, published in 1932 a paper titled "Positivismus und Realismus." In that paper, Schlick arrived at the conclusion that "[l]ogical positivism and realism are […] not opposed; anyone who acknowledges our principle must actually be an empirical realist"[8]. By "our principle," Schlick is referring to the principle "that the meaning of every proposition is exhaustively determined by its verification in the given."[9] Thus, in contrast to the Carnap of the *Aufbau*, Schlick laid claim to the principle of verifiability in order to demarcate science from metaphysics and with it empirical from metaphysical *realism*. This divergence notwithstanding, the premises from which Schlick started were in fact the same as Carnap's: For him, as for Carnap, the realism problem was essentially a problem of language, concerned mainly with the "meaning of statements."[10]

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4 Rudolf Carnap, *The Logical Structure of the World*, translated by R. A. George. London: Routledge 1968, pp. 86-87.

5 Michael Friedman, "The *Aufbau* and the rejection of metaphysics," in: Michael Friedman / Richard Creath (eds.), *The Cambridge Companion to Carnap*. Cambridge: Cambridge University Press 2007, p. 147.

6 Cf. *The Logical Structure of the World*, § 176.

7 In a reference note to § 178 of the *Aufbau*, Carnap refers the reader to his *Pseudoproblems in Philosophy*, pointing out that one can find in that work "detailed expositions of the difference between the empirical and the metaphysical concept of reality and more exact reasons why the realism debate should be banished from science and placed within metaphysics." Unfortunately, Carnap does not explain (or at least indicate) what these "more exact reasons" are.

8 Moritz Schlick, "Positivism and Realism," in: Henk L. Mulder / Barbara F. B. van de Velde-Schlick (eds.), *Moritz Schlick: Philosophical Papers*, volume II (1925-1936). Dordrecht: Reidel 1979, p. 283.

9 Ibid.

10 Ibid., p. 263.

And for him, as for Carnap, *metaphysical* realism had to be banished from science because "the 'problem of the reality of the external world' is a meaningless pseudo-problem."[11] Conversely, both Schlick and Carnap claimed that what they called *empirical* realism was a viable position compatible with the central tenets of logical empiricism. Pretty much the same was claimed by (Schlick's former student) Herbert Feigl who, in a 1936 paper titled "Sense and Nonsense in Scientific Realism," explicitly equated empirical with *scientific* realism and furthermore established that "the principle of ontological transcendence leads quite obviously back to all the substantialistic metaphysical and theological pseudo-assertions which even the realists had hoped to abandon"[12]. In order to avoid the fallacies of 'substantialistic' metaphysics, Feigl recommended empirical (or 'scientific') realism as the most plausible alternative.[13]

3. Reichenbach's Appropriation and Critique of Carnap's View

It has been shown so far that a certain form of realism, namely empirical realism, played a constructive role within the logical empiricist movement. Carnap was the initiator of this restriction to the empirical facet of realism, because it was he who, in his 1928 contribution, declared that the *metaphysical* problem of an external world was nothing but a pseudo-problem. Moreover, Carnap must be seen as the one who initiated the logical empiricists' account of the whole realism issue as a problem of language. However, the thinker who initially tried to *systematically exploit* Carnap's ideas was, without a doubt, Hans Reichenbach. According to Reichenbach, the concept of *meaning* is the central point of reference for a "rational reconstruction" of science. Thus, in his seminal 1938 book *Experience and Prediction*, Reichenbach categorically states: "Language […] is the natural form of knowledge. A theory of knowledge must consequently begin with a theory of language. Knowledge is given by symbols ~~so~~ symbols must be the first object of epistemological enquiry."[14] And he adds, by way of emphasis: "*Meaning is a function which symbols acquire by being put into a certain correspondence with facts*."[15]

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11 Ibid.

12 Herbert Feigl, "Sense and Nonsense in Scientific Realism" in: *Actes du Congrès international de philosophie scientifique*. Vol. 3: *Langage et pseudo-Problèmes*. Paris: Hermann 1936, p. 54.

13 It should be noted that Feigl believed that empirical realism amounted to a significantly *liberalized* form of logical empiricism which, in particular, he distinguished from the *early* logical empiricist "dogma" (ibid., p. 53) of direct verifiability (viz. translatability).

14 Hans Reichenbach, *Experience and Prediction. An Analysis of the Foundation and the Structure of Knowledge*. Chicago: The University of Chicago Press 1938, p. 17.

15 Ibid.

§ 17 of *Experience and Prediction* is entitled "Positivism and realism as a problem of language." It is here that Reichenbach attempts to argue in favor of the realist position. His indebtedness to Carnap is more than obvious. At the very beginning of § 17, Reichenbach writes:

> With the reflections of the preceding section [on the possibility of an "egocentric language"] our inquiry about the difference of the positivistic and the realistic conception of the world has taken another turn; this difference has been formulated as the difference of two languages. This form of consideration, which has been applied particularly by Carnap, seems to be a means appropriate to the problem in question, and we shall make use of it for an illustration of our results.[16]

Reichenbach's support of Carnap's general outlook in § 29 of *Experience and Prediction* is even more explicit. In it, Reichenbach declares that it is an "important result of Carnap's investigations"[17] that a sharp distinction between *fact* and *proposition* (viz. between "object basis" and "sentence basis") cannot be drawn. The idea that such relations as *implication* are essentially "relations between sentences" had motivated Carnap's conception of philosophy as the "analysis of scientific language."[18] Whereupon Reichenbach comments: "We ourselves made use of this conception when we reduced the question of the existence of external things to a question of the meaning of sentences."[19] The result, then, is a highly interesting and at the same time intricate *qualification* of this alliance with Carnap. Reichenbach writes:

> I should say, nevertheless, that such a definition of philosophy is not in opposition to the view that philosophy is concerned with the analysis of the more general relations holding for the physical world. This second interpretation is valid because language is not arbitrary but constructed in correspondence to facts. There are only some features of language which have no relevance for the object world; among these are the idealized concepts [like 'strict implication']. There are, however, other features of language which have their origin in certain features of the world. Thus an analysis of language is at the same time an analysis of the structure of the world.[20]

What does this mean? In order to adequately answer this question, we must readdress the thesis that *realism itself* is (part of) a "problem of language."

Thus, returning to § 17, it should be noted, first, that Reichenbach's own account of scientific realism is built on a principled critique of the early Vienna Circle's verifiability criterion of *meaning*. In opposition especially to the view defended by Schlick, Reichenbach explains that, and why the verifiability criterion,

16 Ibid., p. 145.
17 Ibid., p. 269.
18 Ibid., p. 270.
19 Ibid.
20 Ibid.

or what he also calls the "verifiability theory of meaning"[21], is doomed to failure.[22] As he sees it, verificationism is unable to account both for 'prediction' sentences (or sentences about the future) and 'indirect' sentences (or sentences about not directly observable events). Since both kinds of sentences play an essential role in the practice of science, Reichenbach rejects verificationism and pleas for what he calls the "probability theory of meaning." Thus already in § 8 of *Experience and Prediction* Reichenbach points out that "[t]he key to a theory of meaning corresponding to the intentions of physics lies in the probability problem."[23] And in § 17 he then goes on to explicate his point of view as follows:

> [P]robability meaning, applied to any basis whatever, leads to an unrestricted language. This, it seems to me, is a decisive argument for preferring probability meaning. We may begin with a rather small domain of basic elements and construct upon it statements concerning elements of another domain without being obliged to borrow their meaning from statements about the basic domain. Thus probability meaning leads to the realistic language of actual science; we start from the rather small domain of our own observations and construct the whole world upon it.[24]

The Carnapian 'constructive rhetoric' is clearly echoed in this passage. But the crucial point is that Reichenbach, in fact, intends to argue *against* the *Aufbau* project.[25] That is, for Reichenbach, there exists a fundamental difference between 'construction' in Carnap's understanding of the term and 'construction' in his own, probabilistic, sense.

Reichenbach elucidates this difference between the two understandings of 'construction' by drawing a principled distinction between "projection," on the one hand, and "reduction," on the other.[26] Carnap's program in the *Aufbau* was, according to that distinction, a case of *reduction*. That is, for the Carnap of the *Aufbau*, all scientific concepts are to be constructed (or "constituted") from a small number of "basic" concepts, so that the former can ultimately be reduced to the latter by a step-wise definitional procedure.[27] Reichenbach compares this sort of reductive

21 Ibid., p. 57.

22 Actually, Reichenbach's point of reference is Ludwig Wittgenstein (cf. *Experience and Prediction*, p. 74); but, as is well known, Schlick himself was deeply influenced by Wittgenstein in this connection.

23 *Experience and Prediction*, p. 75.

24 Ibid., p. 153.

25 For a similar appraisal of Reichenbach's attitude toward the *Aufbau* project, see Wesley Salmon, "Carnap, Hempel, and Reichenbach on Scientific Realism," in: Wesley Salmon / Gereon Wolters (eds.), *Logic, Language, and the Structure of Scientific Theories*. Pittsburgh and Konstanz: University of Pittsburgh Press and Universitätsverlag Konstanz 1994, p. 240.

26 Cf. *Experience and Prediction*, § 13.

27 Cf. *The Logical Structure of the World*, § 1. For the details of that program, see Alan Richardson, *Carnap's Construction of the World: The* Aufbau *and the Emergence of Logical Empiricism*. Cambridge: Cambridge University Press 1998. See further

relation with the relation between a wall and the bricks of which the wall is built.[28] If the arrangement of the elements – in this case the configuration of the bricks – is changed, then the complex – in this case the wall – will change. It can be said that the very *existence* of the complex is dependent on the existence of the elements in such a way that the complex may cease to exist without the elements ceasing to exist. If the elements themselves cease to exist (if, for example, the bricks are destroyed), then the complex (in this case the wall) can no longer exist either. "This," Reichenbach writes, "is what we mean by reducibility of existence: the existence of the complex is dependent on the existence of the elements in such a way that the nonexistence of the elements implies the nonexistence of the complex."[29] Going one step further, one might say that conceptual complexes have the status of *logical constructions*: they are constituted on the basis of the elements, but they do not avail themselves of an independent ontological status. They are, in Reichenbach's words, "abstracta,"[30] that is, entities which are conceptually derived and which, according to the "positivistic construction of the world,"[31] can in the last analysis be reduced to one's own immediately experienced "impressions" (Carnap's "elementary experiences" / "autopsychological basis").[32]

In contrast to this reductionist understanding of 'construction,' *projection* implies the *independence* of the 'higher' domains (or levels) from the respective basis. In order to illustrate the central aspects of 'projection,' Reichenbach brings the well-known story of the cubical world.[33] His main intention is to convince the reader that talk about entities which *cannot* be observed directly is more than just highly derived talk about one's own impressions. He therefore imagines a setup in which the whole of mankind is confined to a huge cubical room with translucent walls. Outside the cube there are birds, the shadows of which are projected on the ceiling of the cube. The birds themselves can neither be seen nor heard. Furthermore, there is a mirror which causes a second set of shadows of the birds on one of the vertical walls of the cube. Reichenbach's question is whether the inhabitants of the cube would discover that "there are things outside their cube different from the shadow-figures."[34] And he comes to the result that, after some time, there will appear a Copernicus who will direct a telescope to the walls and

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Michael Friedman, "Carnap's *Aufbau* Reconsidered," in: *Noûs* 21, 1987, pp. 521-545 and Michael Friedman, "Epistemology in the *Aufbau*," in: *Synthese* 93, 1992, pp. 15-57.

28 Cf. *Experience and Prediction*, p. 105.

29 Ibid.

30 Ibid., p. 101.

31 Ibid., p. 100.

32 For a critical discussion of this 'standard account' of Carnap's program, see especially Friedman, "Carnap's *Aufbau* Reconsidered."

33 Cf. *Experience and Prediction*, §14. See further the discussion in Salmon, "Carnap, Hempel, and Reichenbach on Scientific Realism," pp. 241-244.

34 *Experience and Prediction*, p. 116.

discover that the moving black spots have the shape of birds and, moreover, that there are corresponding *pairs* of black spots, consisting of one spot on the ceiling and one spot on the side wall, both with "a very similar shape."[35] Reichenbach then imagines a positivist who contends that the alleged birds are nothing but logical constructions and that they must actually be identified with pairs of black spots. But this, Reichenbach maintains, would be rejected by the physicist. Reichenbach writes:

> The physicist […] would not accept this […] theory. […] It is not because he wants to combine with the term "causal connection" some metaphysical feelings, such as "influence from one thing to another" or "transubstantiation of the cause into the effect." […] Freed from all associated representations, his inference has this form: Whenever there were corresponding shadow-figures like the spots on the screen, there was, in addition, a third body with an independent existence; it is therefore highly probable that there is also such a third body in the case in question. It is this probability inference which furnishes a different weight for the projective complex and the reducible complex.[36]

So, what Reichenbach calls "projective complex" fundamentally differs from what he calls "reducible complex." Whereas the reducible complex is something whose existence is guaranteed by its being a logical construct *out of the observational evidence*, the projective complex is something whose existence is *inferred with probability on the basis of similar cases in the past*. Accordingly, projective complexes are not to be conceived of as abstracta, but rather as "illata," that is, as inferred entities.[37]

The main reason why Reichenbach argues in favor of the projective understanding of 'construction' is that he regards probabilistic relations as capable of accounting for the ampliative character of scientific inference. This ampliative character is specifiable by interpreting 'projection' *in causal terms*. Hence, for Reichenbach, the explanation of observable by unobservable entities "depends on postulates about causality."[38] It is plausible to assume that the resulting causal-inductive argument for the existence of unobservable entities has the form of an *inference to the best explanation*. The decisive point is that Reichenbach assumes that the projective complex (in the cubical world story the birds) is posited as a causal hypothesis by which certain observable effects can be explained and which itself can be evaluated by its probabilistic "weight." On the Carnapian – reductive – version of 'construction,' however, the ampliative character of scientific inference remains unaccounted for because "abstracta" (logical constructions) have no explanatory function whatsoever. And indeed, there can be little doubt

35 Ibid., p. 117.

36 Ibid., p. 123.

37 Cf. ibid., p. 212: "We use the participle *illatum* of the Latin *infero* to denote this kind of thing."

38 Ibid., p. 139.

that Reichenbach is completely justified in criticizing the Carnap of the *Aufbau* for neglecting the role of probabilistic reasoning in science.[39]

Summing up so far, we have seen that for Reichenbach realism is obviously *not* a pseudo-problem because the realist position can, on his account, be justified inductively.

4. Carnap (and Feigl) *versus* Reichenbach

The development of Carnap's attitude toward the realism issue is, as Friedman has correctly noted, characterized through "deep continuities."[40] Carnap himself – in one of the replies in the Schilpp volume pertaining to his philosophy (1963) – retrospectively stated:

> If 'realism' is understood as preference for the reistic language over the phenomenal language, then I am also a realist. However, if 'realism' is understood, in the customary sense, as an ontological thesis, then the arguments against it were given in my monograph [*Scheinprobleme in der Philosophie*]; I do not know of any refutation or even a thorough critical discussion of my arguments.[41]

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39 It is well known that there was already a controversy between Reichenbach and Carnap over probability in the early 1930s. See in this respect especially the short discussion between Reichenbach and Carnap in *Erkenntnis* 1, 1930, pp. 268-270 and Reichenbach's review of Carnap's *Aufbau* in *Kant-Studien* 38, 1933, pp. 199-201. The latter text contains the following interesting statement: "[T]he epistemological question is whether such an interpretation of propositions about reality [which confines itself to elementary experiences] includes everything that we actually mean in asserting them. If Carnap's system were an exhaustive answer to this epistemological question, it would certainly have established a foundation for knowledge of the greatest security, for nothing is presupposed but the reports of immediate elementary experiences and the principles of pure logic. But it seems to me to be at least doubtful whether this reduction to perceptual reports and pure logic exhausts everything we mean to include in our assertions about reality. These doubts are principally aroused when we consider the use of the concept of probability in the natural sciences, for if we accept Carnap's reduction of scientific assertions, we forfeit the indisputable basic principle that such assertions are not merely reports of past perceptual experiences, but are also invariably predictions of future perceptual experiences. It is a puzzle to me just how logical neo-positivism proposes to include assertions of probability in its system, and I am under the impression that this is not possible without an essential violation of its basic principles." (Quoted from the English translation in: Maria Reichenbach / Robert S. Cohen (eds.), *Hans Reichenbach – Selected Writings 1909-1953*, volume I. Dordrecht: Reidel 1978, p. 407.) Thus, reality and probability are tightly connected for Reichenbach (but not for the Carnap of the *Aufbau*). It seems to be clear (but cannot be further investigated here) that *quantum mechanics* played a crucial role in this connection.

40 Friedman, "The *Aufbau* and the rejection of metaphysics," p. 152.

41 Rudolf Carnap, "Replies and Systematic Expositions", in: Paul Arthur Schilpp (ed.), *The Philosophy of Rudolf Carnap*. The Library of Living Philosophers. Vol XI. La

And he immediately added:

> Later, Reichenbach gave to the thesis of realism an interpretation in scientific terms, as asserting the possibility of induction and prediction; a similar interpretation was proposed by Feigl. On the basis of these interpretations, the thesis is, of course, meaningful; in this version, it is a synthetic, empirical statement about a certain structural property of the world. I am doubtful, however, whether it is advisable to give to old theses and controversies a meaning by reinterpretation; I have similar doubts about Quine's reinterpretation of the term "nominalism."[42]

It is necessary to recognize that Carnap's dissociation from Feigl's variant of realism is rather misleading. In fact, it was Feigl, who in a paper dating from 1950 entitled "Existential Hypotheses" argued in terms of "linguistic frameworks" and referred the reader to an article by Carnap, wherein Carnap explicitly states: "I am using here the customary realistic language as it is used in everyday life and in science; this use does not imply acceptance of realism as a metaphysical thesis but only of what Feigl calls 'empirical realism'."[43] This suggests a large agreement, especially since Carnap at another place, namely in his famous 1950 paper "Empiricism, Semantics, and Ontology," repeats his 'realist commitment' by pointing out that "a closely related point of view on these questions [concerning the decision of accepting or rejecting kinds of entities]"[44] could be found in Feigl's "Existential Hypotheses." Furthermore, it must be noted that it was Feigl who *explicitly rejected* Reichenbach's "inductive argument for realism"[45], thereby implying that a direct (probabilistic) justification of the realist position would not suffice. That is, in Feigl's opinion, "the legitimacy of applying the probability concept to the whole realistic frame, instead of merely to inferences within it, remains painfully questionable."[46]

In order to understand the point of Feigl's "repudiation of Reichenbach's justification of scientific realism by means of the argument from 'projection',"[47] it may suffice to focus on the underlying 'Carnapian agenda.' The conception of "linguistic frameworks," upon which Feigl's critique of Reichenbach's position is based, was in a sense *introduced* by Carnap who, especially in "Empiricism, Semantics, and Ontology," hoped to clarify by this conception the status of abstract

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Salle: Open Court 1963, p. 870.

42 Ibid.

43 Rudolf Carnap, "The Two Concepts of Probability," in: *Philosophy and Phenomenological Research* 5, 1945, p. 528.

44 Rudolf Carnap, "Empiricism, Semantics, and Ontology," in: *Meaning and Necessity*, 2nd edition, with Supplementary Essays. Chicago: The University of Chicago Press 1956, p. 214, fn. 4.

45 Herbert Feigl, "Existential Hypotheses," in: *Philosophy of Science* 17, p. 54.

46 Ibid., p. 53.

47 Ibid., p. 54. For a similar critique, see Ernest Nagel, Review of *Experience and Prediction*, in: *The Journal of Philosophy* 35, 1938, pp. 270-272.

entities like properties, classes, numbers, and so on. In the most general terms, a linguistic framework was characterized as a system of speaking about "new kinds of entities."[48] That is, for Carnap, *ontology* was, as before, essentially dependent on language. Speaking in more specific terms, Carnap distinguished between two kinds of questions concerning the existence of entities: first, questions of the existence of certain entities *within the framework* – these he termed *internal* questions; and second, questions concerning the existence of the system of entities *as a whole* – these he termed *external* questions. As Carnap made plainly clear, only internal questions were, in his opinion, 'genuine' ontological questions. External questions, on the other hand, were a matter of practical decision. In Carnap's own words:

> To be real in the scientific sense means to be an element of the system; hence this concept cannot be meaningfully applied to the system itself. Those who raise the question of the reality of the thing world itself have perhaps in mind not a theoretical question as their formulation seems to suggest, but rather a practical question, a matter of a practical decision concerning the structure of our language. We have to make the choice whether or not to accept and use the forms of expression in the framework in question.[49]

Thus, Carnap's attitude toward the realism issue had not essentially changed, the *only* difference being that he now thought of the question of the reality of an external world as a "practical question" and not, as in his 1928 contribution, as a mere "pseudo-problem" completely devoid of sense.[50]

Coming back to Feigl's critique of Reichenbach, it is now quite easy to recognize why on Feigl's view the probability concept cannot be applied to the "whole realistic frame." According to Reichenbach's 'argument from projection,' this is obviously required in order to defend the realist position *as such*. The cubical world story in particular seems to suggest that it makes perfectly good sense to suppose that the assumption of the existence of an external world is a highly probable hypothesis.[51] In particular the assumption of the existence of theoreti-

48 Cf. "Empiricism, Semantics, and Ontology," p. 206.

49 Ibid., p. 207.

50 In his *The Logical Syntax of Language* (1934), Carnap still holds that "*[t]he controversy between positivism and realism is an idle dispute about pseudo-theses which owes its origin entirely to the use of the material mode of speech*" (Rudolf Carnap, *The Logical Syntax of Language*, translated by Amethe Smeaton. London: Routledge 1967, p. 301). In "Empiricism, Semantics, and Ontology", Carnap similarly points out that "[a]n alleged statement of the reality of the system of entities is a pseudo-statement without cognitive content." But he then immediately *qualifies* his claim by saying: "To be sure, we have to face at this point an important question; but it is a practical, not a theoretical question; it is the question of whether or not to accept the new linguistic forms." ("Empiricism, Semantic, and Ontology", p. 214)

51 In his *The Rise of Scientific Philosophy* (1951), Reichenbach makes this point entirely clear by saying: "[T]he statement 'there is a physical world' can be very well distinguished from the statement 'there is no physical world', because we can depict

cal ('unobservable') entities, like for instance atoms, seems to be defendable on probabilistic grounds.[52] Given Carnap's distinction between internal and external questions, however, there can be no inductive argument *for the framework itself*. That is, on Carnap's view it is completely unproblematic to make assertions about *particular* theoretical entities, as for instance specific configurations of atoms, since this concerns an internal question, that is, a question *within* a certain linguistic framework. Yet, on the other hand, to make assertions about atoms (and their existence) *in general* is, for Carnap, not an internal but an external question, that is, a question which can only be answered by a *practical decision*. In other words, in Carnap's opinion, the introduction of the atomistic framework depends on such criteria as simplicity, coherence and other pragmatic features. These criteria are *definitely not* dependent on facts about the world and therefore *definitely not* defendable on probabilistic grounds. To be sure, in a given specific situation, the concrete behavior of a given sample of atoms can be probabilistically inferred. But in order to be able to draw this sort of inference at all, the atomistic framework as such must first be introduced, and this is possible only on the basis of a practical decision. Speaking more generally, the foregoing introduction of the realistic framework (with its assumption of the existence of an external world including unobservable entities) *first of all enables* probabilistic inferences within it. And from this we can draw the conclusion that Reichenbach's probability theory of meaning *presupposes* the realistic framework but does not prove (or at least support) it.[53]

However, as Feigl correctly observed, Reichenbach himself was not entirely clear as to what he intended the realist framework to be; for there are passages in *Experience and Prediction* where Reichenbach *refrains* from claiming that an inductive justification of the realist framework should be possible. Especially by a careful reading of the already mentioned § 17 of that work it becomes obvious that Reichenbach attempts to characterize the very adoption of the realist framework as a matter of a practical decision (and thus in Carnapian terms). Thus, directly at the beginning of § 17, Reichenbach points out:

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experiences which would make the one statement probable and the other improbable. The two statements differ as to their predictive content." (Hans Reichenbach, *The Rise of Scientific Philosophy*. Berkeley–Los Angeles: University of California Press 1951, p. 267).

52 Cf. *Experience and Prediction*, pp. 212-213: "The atoms have been discovered by the physicists in a way analogous to the discovery of the birds in the cubical world."

53 Moreover, it has often been argued that Reichenbach's *frequency interpretation* of probability is too weak to drive home the point that the realistic framework as such can be justified on inductive grounds. For further details, see Nagel's review of *Experience and Prediction* (esp. p. 271) and Feigl, "Existential Hypotheses", p. 53; see further the discussion in Hilary Putnam, "Hans Reichenbach: Realist and Verificationist," in: Juliet Floyd / Sanford Shieh (eds.), *Future Past: The Analytic Tradition in Twentieth-Century Philosophy*. Oxford: Oxford University Press 2001, p. 283.

If we now proceed to regard the differences of the positivistic and the realistic languages, we pass from the descriptive to the critical task of epistemology; with this turn we consider meaning as a matter of free decision, and ask for the consequences to which each form of decision leads, and thus for the advantages and disadvantages which may be used to determine our choice if we ourselves want to make a decision.[54]

It is, as Feigl puts it, "the need for definitional or conventional stipulation"[55] for which Reichenbach argues here. And indeed, scientific method as such is characterized for Reichenbach to a great extent by "volitional decisions" (which, in turn, he equates with conventions).[56] Moreover, Reichenbach assumes that both the positivistic and the realistic language are introduced by such volitional decisions (or conventions), but that both languages are to be *evaluated* by their "entailed" decisions, that is, by the decisions which necessarily follow as soon as the respective languages are introduced.[57] All of this, however, definitely *prevents* a probabilistic justification of the realistic framework, because the introduction of the realistic framework ultimately depends on what Feigl calls a "basic convention"[58] and because this basic convention qua convention falls outside the range of probabilistic frequencies. Rather, the adoption of the realistic language form is *required* in order to make sense of the kind of probabilistic argument Reichenbach has in mind. Or, again, in Feigl's words:

The strongest justification for the adoption of the realistic frame is to be found precisely in that it makes intelligible what we mean by the probability of existential hypotheses. The introduction of new *basic* and *irreducible* concepts (as, for example, in electromagnetics during the last century) may be reconstructed as an expansion of the empirical language. Only after our language has thus been enriched, can we significantly assign probabilities

54 *Experience and Prediction*, p. 144.

55 "Existential Hypotheses," p. 57.

56 Cf. *Experience and Prediction*, p. 9.

57 Cf. ibid., p. 13: "The system of knowledge is interconnected in such a way that some decisions are bound together; one decision, then, involves another, and, though we are free in choosing the first one, we are no longer free with respect to those following. We shall call the group of decisions involved by one decision its *entailed decisions*." Cf. further ibid., p. 147: "If the languages in question are not equivalent, if the decision between them forces a case of a volitional bifurcation, this decision is of the greatest relevance: it will lead to consequences concerning the knowledge obtainable. The man who speaks the egocentric language cannot express certain ideas which the man with the realistic language may formulate; the decision for the egocentric language, therefore, entails the renunciation of certain ideas, and may, consequently, become highly relevant. We do not thereby say that the egocentric language is 'false'; such a criticism would be a misunderstanding of the character of a volitional decision. It is rather the method of entailed decisions which we have to apply here; we can show that the decision for the egocentric language leads to a scientific system of a restricted character which does not correspond to the system constructed by the realistic language in its full extension."

58 "Existential Hypotheses," p. 57.

(degrees of confirmation) to specific predictive or explanatory hypotheses. The step of expansion of language cannot itself be justified on the grounds of probability, except perhaps in the sophisticated pragmatic sense of the question: Will this expansion be methodologically fruitful?[59]

Carnap would certainly have agreed: The assignment of probabilities to specific 'existential hypotheses' – or, in Reichenbach's terms, to specific 'projective complexes' – presupposes the adoption of the realist framework, and the adoption of the realist framework itself depends *not* on the structure of material reality but rather on a practical decision.

Based on the weight of the preceding factors, it can be concluded that Reichenbach – by way of appropriating and at the same time criticizing Carnap's point of view – faces the following dilemma. On the one hand the realist framework cannot be justified inductively because the very definition of such a framework implies its being introduced by a practical decision. More specifically, the assignment of probabilities to 'projective complexes' is presupposed by the foregoing – and only conventionally justifiable – adoption of the realist framework.[60] On the other hand, if this last point is conceded, then we actually do not enter the domain of scientific realism, but we rather end up with a modified version of *Kantianism*. Reichenbach's (and Carnap's) 'Kantian roots'[61] notwithstanding, the assumption of a 'mind-independent' reality is, in fact, *sacrificed* by conceiving of realism as a problem of language. Or, if this appears too crude, conceiving of realism as a problem of language means, in any case, that science does not conform to reality in an ontologically unconditioned manner. In the same way that for Kant the presupposition of "empirical realism" implied "transcendental idealism,"[62] for Reichenbach – given his idea of "volitional decisions" – a certain form of *conventionalism* is presupposed as the enabling ground of 'scientific realism.' In both cases, the strategy is *transcendental* insofar as realism is brought into dependence on something mind-related. In the case of Kant, realism is dependent on the facul-

59 Ibid.

60 This is, again, Feigl's point when he – clearly alluding to Reichenbach's position – states: "The customary probabilistic realism in trying to justify 'transcendent' hypotheses on the basis of experimental findings has put the cart before the horse. Only after the introduction of the realistic frame can we legitimately argue inductively either from the theory to the outcome of as yet unperformed experiments; or vice versa from the results of experiments to *specific* postulates of the theory. But the presupposed introduction of the realistic frame, i.e. the semantic-realistic interpretation of the theory, is a step that can be justified only instrumentally: It furnishes the very possibility of a theory that is inductively fruitful." (Herbert Feigl, "Logical Reconstruction, Realism and Pure Semiotic," in: *Philosophy of Science* 17, 1950, p. 195)

61 See in this respect in particular Hans Reichenbach, *Relativitätstheorie und Erkenntnis Apriori*. Berlin: Springer 1920.

62 For the details of this view, see, for example Henry E. Allison, *Kant's Transcendental Idealism: An Interpretation and Defense*. New Haven: Yale University Press 1983.

ties of human cognition (or, more exactly, on the "pure forms" of sensibility and understanding); and in the case of Reichenbach, realism is dependent on the pragmatic dimension of adopting certain language forms. Neither approach, however, can be reconciled with the 'metaphysics of mind-independence' that underlies the *naturalistic* methodology of scientific realism.[63]

5. Kaila's Non-Linguistic Way Out

How can Reichenbach's dilemma be avoided? The answer seems to be clear: *The view that realism is a problem of language must be abandoned.* But would that not bring us back to *metaphysical* realism? – Probably. But what exactly is the problem? Surely it would be absurd to aim at a reinstallation of *speculative* metaphysics (in the sense of Hegel and Schelling). Yet, there are other forms of metaphysics that, I think, are thoroughly compatible with the logical empiricists' idea(l) of a "scientific philosophy."[64] These other forms of metaphysics form part of the project of a 'modern,' non-speculative, *philosophy of nature*. Michael Esfeld, one of the contemporary promoters of this project, writes in connection to this:

> The new metaphysics of nature distinguishes itself from the older essays in speculative metaphysics by being close to science: metaphysical claims are based on scientific theories. Consequently, the metaphysical claims about nature are as hypothetical as our scientific theories: there is no more certainty to be gained in metaphysics than there is in science. In other words, scientific knowledge claims are fallible and metaphysics, insofar as it draws on those claims, is as fallible as science.[65]

Among the logical empiricists it was especially Eino Kaila who came fairly close to such a conception of philosophy as 'metaphysics of nature.' As regards the realism issue, this set the stage for a promising 'third option,' the fundamentals of which are briefly outlined in what follows.[66]

63 For a rather recent update of the scientific realist position, see, for example, Howard Sankey, *Scientific Realism and the Rationality of Science*. Aldershot: Ashgate 2008, esp. ch.s 1 and 2. See further Anjan Chakravartty, *A Metaphysics for Scientific Realism: Knowing the Unobservable*. Cambridge: Cambridge University Press 2007 and Elie Zahar, *Why Science Needs Metaphysics: A Plea for Structural Realism*. Chicago: Open Court 2007.

64 For the details of that idea (and its history), see Alan Richardson, "Toward a History of Scientific Philosophy," in: *Perspectives on Science* 5, 1997, pp. 418-451 and Michael Friedman, *Dynamics of Reason: The 1999 Kant Lectures at Stanford University*. Stanford, CA: CSLI 2001, pp. 3-24.

65 Michael Esfeld, "Hypothetical Metaphysics of Nature", in: Michael Heidelberger / Gregor Schiemann (eds.), *The Significance of the Hypothetical in the Natural Sciences*. Berlin: de Gruyter 2009, p. 341.

66 For a very instructive discussion of Kaila's point of view, see Ilkka Niiniluoto, "Eino Kaila and Scientific Realism," in: Ilkka Niiniluoto / Matti Sintonen / G.H. von Wright

The methodologically most remarkable point in Kaila's philosophical thinking is that the reflection on the structure of language is *subordinated* to the reflection on the structure of the world. Kaila – himself professor of theoretical philosophy at the university of Turku and (after 1930) at the University of Helsinki – stood in contact with the members of the Circle since 1927 and visited Vienna in 1929 in order to discuss the issue personally with Carnap. Although he was to a large extent inspired by the "exact philosophical method of the Vienna Circle"[67], he refused to take the 'linguistic turn.' Instead, he focused on the explanatory constituents *of science itself*. By taking this broadly naturalistic perspective, Kaila was in a position to circumvent the conception of the realism issue as a pseudo-problem. Thus, in his 1930 *Der logistische Neupositivismus. Eine kritische Studie*, he fundamentally criticizes Carnap's *Aufbau* program and points out:

> Such results – presented in a tone of superior calmness and brilliant logical clarity – have profound effects. If they are correct, they in fact mean the end of all philosophy. Moreover, if they are correct, they are apt to deprive even empirical research of its *élan*; for the 'realist language' of science is actually far more than a mere manner of speaking: it is the expression of the living *soul* of science.[68]

What Kaila has to offer instead is a realistically inspired theory of science and nature which essentially is motivated by the concepts of *invariance* and *probability*.

First, in regard to probability, Kaila completely agreed with Reichenbach that probabilistic reasoning played a fundamental role both in the practice of science and in defending the scientific realist position:

> *If* our life is neither 'illusion' nor 'dream', if *our perceptions are sample-like segments of an n-dimensional real manifold*, what inferences can then be made from their given content to their not-given content? *This is precisely the question which empirical science is to answer*. […] The presupposition of empirical science that our perceptions are 'sample-like segments' means that probability inferences can be drawn from the given to something not given, that the truth frequencies present in the given can be generalized to the not-given.[69]

Accordingly, Kaila maintained, "[a] certain *realism of all science*"[70] is able to be postulated. That is, for Kaila, the inference to Reichenbachian 'projective complexes' was both epistemologically legitimate and scientifically fruitful.

(eds.), *Eino Kaila and Logical Empiricism*, Acta Philosophica Fennica 52. Helsinki: Societas Philosophica Fennica 1992, pp. 102-116.

67 Niiniluoto, "Eino Kaila and Scientific Realism", p. 103.

68 Eino Kaila, "Logistic Neopositivism: A critical study," in: *Eino Kaila: Reality and Experience. Four Philosophical Essays*, edited by Robert S. Cohen. Dordrecht: Reidel 1979, p. 4. For an extended discussion of Kaila's critique of Carnap's *Aufbau* program, see A.W. Carus, *Carnap and Twentieth-Century Thought: Explication as Enlightenment*. Cambridge: Cambridge University Press 2007, pp. 209-221.

69 *Reality and Experience*, p. 48.

70 Ibid., p. 49.

However, as already indicated, Kaila dissociated himself from Reichenbach's (as well as from Carnap's and from Feigl's) approach toward the realism issue by his refusal to take the 'lingustic turn.'[71] As he saw it, the concept of *invariance* was strong enough to provide us with a convincing *defense* of the probabilistically inspired scientific realist position. The *prima facie* irritating point in that, though, was that Kaila attempted to integrate the invariance concept in a thoroughly Carnapian framework. This thoroughly Carnapian framework entailed the assumption that invariance be conceptualized in terms of a certain form of 'constitutional system.' This is at least what Kaila attempted to accomplish in his two "contributions to logical empiricism." The first of these two contributions appeared in 1936 and was titled *Über das System der Wirklichkeitsbegriffe. Ein Beitrag zum logischen Empirismus*[72]; the second contribution appeared in 1941 and was titled *Über den physikalischen Realitätsbegriff. Zweiter Beitrag zum logischen Empirismus*[73]. In both essays, Kaila argued from an 'invariantist' point of view: In his opinion, invariances formed the proper subject of scientific inquiry. What is more, according to Kaila, invariances were *ontologically basic*. In his own words: "The 'essence' of a thing consists of the invariances of this thing."[74]

Concerns of space prevent an investigation into the details of Kaila's invariantist ontology. The point to be stressed, however, is that such a notion as invariance is both scientifically *and* philosophically relevant if, like in the case of Kaila, it is used as a *criterion of reality*. The British physicist Paul Dirac, for example, once claimed that "[t]he important things in the world appear as the invariants […] of […] transformations."[75] Moreover, the Hungarian-American physicist and mathematician Eugene Wigner focused in his Nobel Prize lecture from 1963 on invariance principles in order to explain the essence of the laws of nature.[76] So it

71 For the otherwise close connections between Kaila's and Reichenbach's points of view, see Arto Siitonen, "Kaila and Reichenbach as Protagonists of 'Naturphilosophie'," in: Juha Manninen / Friedrich Stadler (eds.), *The Vienna Cirlce in the Nordic Countries: Networks and Transformations of Logical Empiricism*, Vienna Circle Institute Yearbook 14. Dordrecht: Springer 2010, pp. 135-152

72 English translation, "On the System of the Concepts. A contribution to logical empiricism," in *Reality and Experience*, pp. 59-125.

73 English translation, "On the Concept of Reality in Physical Science. Second Contribution to logical empiricism," in *Reality and Experience*, pp. 126-258.

74 *Reality and Experience*, p. 228. In general, the notion of invariance implies that a property or system remains unchanged regardless of changes in the conditions of measurement. For example, the area of a surface remains unchanged if the surface is rotated in space; thus the area exhibits rotational invariance. Another example would be the principle of relativity in Einstein's Special Theory, according to which all laws of physics are the same for all inertial observers; they are the same in every inertial reference frame, and so are invariant under Lorentz transformations. Another example for invariance would be the law of conversation of energy.

75 Paul Dirac, *The Principles of Quantum Mechanics*, 3rd ed. Oxford: Clarendon Press 1947, p. vii.

76 Cf. Eugene Wigner, "Events, Laws of Nature, and Invariance Principles", in: *Sym-*

can be said that Kaila, by invoking the notion of invariance, was very close to the physicists' understanding of reality and nature. Furthermore, by taking invariance seriously, Kaila was in the position to furnish Carnap's constitutional system of the *Aufbau* with an *ontological foundation*. That is, he correlated the conceptual hierarchy of Carnap's system with a realist interpretation of invariance. As a consequence, Kaila conceived of reality as a *matter of degree*. In Kaila's own words: "The more lawlike something is, the more 'real' it is. The different levels of reality […] correspond to the different degrees of invariance."[77] From this it was only a short leap away from the following formulation of the aim of science:

> The aim of exact science is to discover the higher invariances of the domain of experience in question. We shall show that 'physico-scientific reality' (as to its content) consists in nothing other than the system of higher invariances of the everyday physical world and thus (in the last analysis) 'immediate experience'.[78]

Given the underlying realist interpretation of invariance, it is indeed apt to claim, as Georg Henrik von Wright has done, that "Kaila's own 'constitution theory' is original and rather different from Carnap's"[79].

Going one step further, Kaila's constitutional system can be characterized by a *hierarchy of relational invariances*. Thus Kaila distinguishes between 'phenomenal' or '–objects', 'physical' or 'f-objects' (*physische Gegenstände*) and 'physico-scientific' or 's-objects' (*physikalische Gegenstände*).[80] Kaila's point is that the higher elements of this hierarchy are constituted by relational invariances among the elements of the lower level. Thus, s-objects (as, for example, atoms or electromagnetic fields) are more invariant and hence more real than f-objects (as, for example, stones or trees); and these f-objects, in turn, are more invariant and hence more real than –objects (as, for example, colour or sound perceptions). Since the epistemological counterpart to the invariance criterion is *objectivity*, it can be concluded that everyday experience, when understood as the realm of f-objects, is, on this account, to be characterized as *less* objective than scientific knowledge.[81]

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metries and Reflections: Scientific Essays of Eugene P. Wigner. Westport: Greenwood Press 1967, esp. pp. 46-47.

77 *Reality and Experience*, p. 102.

78 Ibid., p. 152.

79 Georg Henrik von Wright, "Eino Kaila's Monism," in: Niiniluoto/Sintonen/von Wright (eds.), *Eino Kaila and Logical Empiricism*, p. 80.

80 Cf. *Reality and Experience*, pp. 130-32. See further the reconstruction in Niiniluoto, "Eino Kaila and Scientific Realism", pp. 108-110.

81 It should be noted that Kaila, when talking about invariance, repeatedly refers to Ernst Cassirer's 1910 *Substanzbegriff und Funktionsbegriff* (see, for example, *Reality and Experience*, p. 102, fn. 44 and p. 154, fn. 35). As is well known, Cassirer himself had developed a "universal invariant theory of experience" which was inspired by Felix Klein's "Erlangen program" in metageometry (for further details, see Karl-Norbert Ihmig, *Cassirer's Invariantentheorie der Erfahrung und seine Rezeption des "Erlanger*

But it is not unconnected with scientific knowledge. As far as f-objects can be conceptualized as invariant relational systems of ~~–o~~bjects, s-objects can be conceptualized as invariant relational systems of f-objects. The essential aspect is that by these dynamics of increasing invariance our knowledge of the world becomes more and more structural and thereby more objective. In a sense, therefore, it can be said that Kaila was a 'structural realist.' For him, the objective structure of the world was prior both to language and to 'things' (in the traditional 'substantialist' sense of that word). [82]

6. Concluding Remarks

In this paper, I hope to have made plausible (a) that, as can be learned from Kaila, the concepts of invariance and probability play a crucial role in defending the scientific realist position and (b) that, as can be learned from Kaila too, realism is not (in any case, not in the first place) a problem of language. Eventually coming

Programms." Hamburg: Meiner 1997). The main difference between Kaila und Cassirer, however, is that Cassirer was an "idealist" and not, like Kaila, a realist in the philosophy of science.

82 It is interesting to see that Kaila's realist interpretation of the notion of invariance is echoed in the writings of more recent authors such as Robert Nozick who straightforwardly asserts that "[a]n objective *fact* is invariant under various transformations. It is this invariance that constitutes something as an objective *truth* […]." (Robert Nozick, *Invariances: The Structure of the Objective World*. Cambride, Mass.: Harvard University Press 2001, p. 76; my italics) However, it might be objected that Kaila would not have gone so far as Nozick. Niiniluoto ("Eino Kaila and Scientific Realism", p. 112), for example, argues that Kaila was an 'internal' realist in the sense of the later Hilary Putnam (see in this respect especially Hilary Putnam, *Reason, Truth and History*. Cambridge: Cambridge University Press 1981). This implies especially that, on Kaila's account, reality and its *conceptualization* cannot be disentangled in such a way that a realm of 'objective', mind-independent 'facts' can be claimed to exist. Yet, I do *not* think that Kaila was an 'internal' realist. For Kaila would not have accepted the strongly *Kantian element* in Putnam's point of view; nor would he, like Putnam, have (more or less enthusiastically) welcomed the 'linguistic turn.' As I see it, Kaila's 'invariantism' implies that the relatively highest invariance of concepts related to s-objects is not, as Putnam would have it, a mere arrangement within our conceptual system, but rather something that is *caused* by the relatively highest invariance of the s-objects *themselves* and therefore something that can be abductively inferred. Furthermore, it is Kaila's (anti-conventionalist) conviction that invariances must *be presupposed* in order to execute measurements (cf. *Reality and Experience*, pp. 187-188). They are, in a sense, that *what gets measured* and it is therefore plausible to invest them with as much independence from the conceptual as possible. Putnam's 'internal' realism is, in my opinion, a step in the opposite – and insofar false – direction. Rather, there is a close connection with current 'ontic structural realism' (for an overview over the latter, see Peter M. Ainsworth, "What is ontic structural realism?", in: *Studies in History and Philosophy of Modern Physics* 41, 2010, pp. 50-57).

back to Carnap, we might readdress the question whether and, if so, in what sense Carnap's position in the philosophy of science was that of a realist. After what has been said, it is almost unnecessary to point out that, from Carnap's perspective, such an invariantist ontology like Kaila's would have been a further contribution to the realm of metaphysical pseudo-statements.[83] To be sure, it might, as Richard Creath has done, be argued that Carnap was an in some sense a scientific realist.[84] Given that, "it makes no sense to suggest that none of what we say is ontologically committing"[85], it is downright inevitable to assume that Carnap *must* have been a realist in Creath's sense:

> [S]ince Carnap treats the observational and theoretical domains similarly in all respects relevant to ontological commitment, we have no choice but to conclude that on Carnap's interpretation we are ontologically committed to theoretical entities.[86]

This might be so; but it must be kept in mind, as Creath does, that Carnap confined his 'realist confession' to *empirical reality*. As we have seen, it was Carnap's aim to *dissolve* the talk about the existence of theoretical entities and to convert it to an 'external,' practical, question. His ontological commitment was that of a 'Kantian in disguise.'[87]

I claim that scientific realism must offer more than that. And there are indeed some interesting options. Especially Kaila stands for the (still promising project) of a *post-positivistic articulation* of logical empiricism which is rich enough to absorb the scientific realist position. I think this train of thought merits further exploration. However, so much is clear: It is not an easy task to overcome the view that the realism issue is "essentially linguistic"[88].

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83 In fact, Carnap had reviewed Kaila's *Der logistische Neupositivismus* (see *Erkenntnis* 2, 1930, pp.75-77). However, Kaila's invariantism played no role there.

84 Cf. Richard Creath, "Carnap's Scientific Realism: Irenic or Ironic?", in: Nicholas Rescher (ed.), *The Heritage of Logical Positivism*. Lanham: University of America Press 1985, pp. 117-131.

85 Ibid., p. 131.

86 Ibid.

87 For a similar appraisal, see the instructive discussion in Paolo Parrini, "With Carnap, Beyond Carnap: Metaphysics, Science, and the Realism/Instrumentalism Controversy," in: Salmon/Wolters (eds.), *Logic, Language, and the Structure of Scientific Theories*, pp. 255-277.

88 Rudolf Carnap, *Philosophical Foundations of Physics*. New York: Basic Books 1966, p. 256.

Richard Creath

Analyticity in the Theoretical Language: Is a Different Account Really Necessary?

Recent essays by Michael Friedman[1] and William Demopoulos[2] on Carnap's late approach to analyticity in the theoretical language make a convincing case for the continuing philosophic interest of this part of Carnap's work. The present essay is intended not to disagree with any of these essays but to raise a logically prior worry as to whether Carnap's account of analyticity here is well motivated and consistent with other attractive aspects of his view. To do this I outline, in §1, Frank Ramsey's approach to theories and the so-called Ramsey sentence. This will allow us to trace the steps by which Carnap came to use the Ramsey sentence in developing an account of analyticity for the theoretical language. Then, in §2, I articulate my own uneasiness with what I see as Carnap's motivation. Finally, in §3, I express my practical reservations about how well Carnap's approach fits with other aspects of his view. This is not intended as a refutation but rather as a reflection on *how* we can learn from Carnap and as a reminder of how much more *we* have to do.

§1. The Road to the Carnap Sentence

In 1931 Frank Ramsey's paper "Theories" appeared in a posthumous collection of his work.[3] There he showed that scientific theories in their standard formulations could each be replaced with a single sentence, its so-called Ramsey sentence. This sentence would not include any theoretical terms, or more precisely any theoretical predicate constants, yet it would have all of the same consequences in the non-theoretical language. Not only is there such a sentence, we have directions for

1 Friedman, "Carnap on Theoretical Terms: Structuralism Without Metaphysics", forthcoming in *Synthese*.

2 Demopoulos, "Carnap on the Rational Reconstruction of Scientific Theories", in *The Cambridge Companion to Carnap*, M. Friedman and R. Creath, eds., (Cambridge: Cambridge University Press, 2007), 248-72, and "On Extending 'Empiricism, Semantics and Ontology' to the Realism-Instrumentalism Controversy", unpublished.

3 Ramsey, "Theories" in *The Foundations of Mathematics and Other Logical Essays*, R. B. Braithwaite, ed., (London: Kegan Paul, Trench, Trubner & Co., 1931), 212-36.

R. Creath (ed.), *Rudolf Carnap and the Legacy of Logical Empiricism*,
Vienna Circle Institute Yearbook 16, DOI 10.1007/978-94-007-3929-1_4,

producing it, given the theory, T, where T is finitely axiomatizable, and some division of the vocabulary of T into observational and theoretical terms.[4]

To produce the Ramsey sentence of T, *first*, conjoin all of the postulates of T. If we let O_1 through O_k be the observational constants of T and T_1 through T_n be the theoretical constants of T, then we can schematize T as:

$$\ldots T_1 \ldots O_1 \ldots T_2 \ldots O_2 \ldots T_n \ldots O_k \ldots$$

In this it makes no difference how many occurrences of the various predicate constants there may be or in what order. *Second*, systematically replace each theoretical constant, T_j, with a corresponding second-order variable, $Ø_j$, to yield:

$$Ø_1 \ldots O_1 \ldots Ø_2 \ldots O_2 \ldots Ø_n \ldots O_k \ldots$$

Third, close this sentence by prefixing it with appropriate second-order quantifiers. The result is the Ramsey sentence of our original theory, T, and can be schematized:

$$(\exists Ø_1)(\exists Ø_2) \ldots (\exists Ø_n)(Ø_1 \ldots O_1 \ldots Ø_2 \ldots O_2 \ldots Ø_n \ldots O_k \ldots)$$

For ease of expression, represent this as R(T).

In R(T) all of the theoretical descriptive constants have been replaced. If we think of the observational vocabulary as extended to include mathematical and set theoretical terms, it is possible to show that R(T) has exactly the same logical consequences in this logically extended observational vocabulary, as does T.

Now it might be thought that theories could be replaced by their Ramsey sentences. This is not Carnap's strategy, though on his account this would be possible for certain purposes. Instead, Carnap says that theories are semantically equivalent (L-equivalent) to their Ramsey sentences. Before we can evaluate this conclusion, we must trace the sequence of events that led him to it. It is not always possible to be sure of all the relevant steps, but what follows is the best I can do in reconstructing this bit of Carnap's history.

In 1954 Hempel's contribution[5] to what was to become *The Philosophy of Rudolf Carnap* arrived on Carnap's desk. Carnap read it as challenging him to do three things: (1) He should provide a viable account of empirical *significance*. It should specify which claims and expressions are empirically meaningful. This challenge is most pressing where the claims in question contain theoretical terms. (2) Carnap should provide a general account of empirical import or *empirical content*. This should indicate the empirical content of any given claim, but here

4 Ramsey himself speaks of a "primary system" and a "secondary" one rather than of observational and theoretical terms respectively. But he does say that these systems are composed of terms and propositions, and the examples he gives show that Carnap's later treatment that speaks of observational and theoretical vocabularies is for present purposes equivalent to what Ramsey says. For the present paper, T is assumed to include terms form both vocabularies and so to be what Carnap would later express as TC.

5 C. G. Hempel, "Implications of Carnap's Work for the Philosophy of Science", in *The Philosphy of Rudolf Carnap*, P.A. Schilpp, ed., (LaSalle, IL: Open Court, 1963), 685-709.

again the really pressing issue is for theoretical claims. (3) Carnap should provide a general account of *analyticity* for scientific theories.

On the issue of (1) empirical significance, Carnap had for decades[6] sharply and syntactically divided the descriptive, that is non-logical, vocabulary of science into non-overlapping observational and theoretical vocabularies. In this context Carnap's division is entirely reasonable. The issue is directly about expressions. And the division is reasonable even though there is no uniquely obvious place to draw it, and the psychological differences underlying it are at best matters of degree. This is because Carnap's purpose is to defend the empirical significance of theoretical expressions (while avoiding metaphysical claims). For this purpose where one draws the line between the observational and the theoretical does not much matter. Carnap even argues that wherever one draws it, the distinction between the theoretical and the metaphysical is undisturbed. Correspondingly, had his purpose been to disparage theoretical terms, Carnap would have had to defend both the location and manner of his distinction between the observational and the theoretical.

In 1956 Carnap published "The Methodological Character of Theoretical Concepts".[7] This deals with Hempel's first challenge. It is on the whole successful. Purported counterexamples have been published, for example by David Kaplan,[8] but those can be met by very natural and minor corrections.[9] So we can set challenge (1) aside.

Concerning challenge (2), on content, Carnap starts with a distinction between the observational and theoretical vocabularies already established. Moreover, since the days of *Logical Syntax*,[10] Carnap held that the *content* of a claim was the class of non-analytic consequences of that claim. Combining these two, we might say that the *empirical content* of a claim is the class of its non-analytic consequences in the observation vocabulary, or more precisely in the logically extended observational vocabulary. Since R(T) has exactly the same consequences as T in the logically extended observational vocabulary, R(T) seems perfectly constructed to express the empirical content of T.

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6 Since at least "Testability and Meaning", *Philosophy of Science*, 3 (1936): 419-71, 4 (1937): 1-40.

7 Carnap, "The Methodological Character of Theoretical Concepts", in *The Foundations of Science and the Concepts of Psychology and Psychoanalysis*, H. Feigl and M. Scriven, eds., Minnesota Studies in the Philosophy of Science, Vol. 1, (Minneapolis: University of Minnesota Press, 1956), 38-76.

8 D. Kaplan, "Significance and Analyticity: A Comment on Some Recent Proposals of Carnap" was given as a talk at the same 1959 meeting as Carnap's "Theoretical Concepts in Science" (see note 11 below). Though this paper was widely circulated and cited, it was not published until *Rudolf Carnap, Logocal Empiricist: Materials and Perspectives*, J. Hintikka, ed., (Dordrecht, Holland: D. Reidel, 1975), 87-94.

9 See R. Creath, "On Kaplan on Carnap on Significance", *Philosophical Studies*, 30 (1976): 393-400.

10 Carnap, *Logische Syntax der Sprache*, (Vienna: Springer, 1934).

Strictly speaking the Ramsey sentence says that there are abstracta, classes or properties in this case, that bear certain systematic relations to a large body of observable goings on. Of course if there are classes at all, and their existence is not seriously at issue here, there are plenty to go around. But the Ramsey sentence is still not true unless the empirical facts are as the theory says they are. Even so, I have reservations about whether R(T) expresses the empirical content of T. But in the context of the sequence of issues on which Carnap had been working, this understanding of empirical content seems utterly natural.

This leaves only challenge (3), *analyticity* in the theoretical language. Carnap's answer to challenge (2) materially affects how he can address challenge (3). The constraints of the problem are these: First, a theory might be thought of as having two parts: (A) an empirical part, that is, a part that – rather too informally – might be thought of as depending on the world for its truth, and (B) a part that depends for its truth wholly on us and how we have endowed the language with meaning – in short, the analytic part. Second, these two parts should be so related so that they "add up to" the theory. That is to say that the theory should imply each of the two parts, and the conjunction of the two parts should imply the theory. As we have seen, R(T) can be thought of as well suited to be the first part, (A). T certainly does imply R(T). Hempel's challenge then is to find the second part, (B), namely what goes into the blank in:

$$[R(T) \,\&\, \_\_\_\_\_\,] \leftrightarrow T$$

where '↔' expresses mutual entailment. For anyone with a modicum of logical training the answer to the challenge is easy, namely

$$R(T) \supset T.$$

This is what is now called the Carnap sentence for T. We are to take the Carnap sentence, R(T) ⊃ T, as the *analytic part* of T. It is to provide the interpretation of the theoretical vocabulary of T to the extent that any is needed.

This is technically all very neat. It seems to meet the demands of Hempel's challenge. Carnap was very pleased with it. He presented the idea in a talk at a national meeting.[11] He published it in two separate papers.[12] He published it again in his reply to Hempel[13] in *The Philosophy of Rudolf Carnap*, and he discussed it

11 The talk was "Theoretical Concepts in Science" given at the American Philosophical Association meeting in Santa Barbara, CA in December 1959. The text of the talk has been published (pp. 158-72) together with a commentary in S. Psillos, "Carnap's 'Theoretical Concepts in Science'", *Studies in History and Philosophy of Science*, 31 (2000): 151-72.

12 Carnap, "Beobachtungsprache und theoretische Sprache", *Dialectica: Revue internationale de philosophie de science*, 12 (1959): 236-48, and also in Carnap, " On the Use of Hilbert's ε-Operator in Scientific Theories", in *Essays on the Foundations of Mathematics, Dedicated to A. A. Fraenkel on His Seventieth Anniversary*, Y. Bar-Hillel et al., eds., (Jerusalem: The Magnes Press, 1961), 156-64.

13 Carnap, "Carl G. Hempel on Scientific Theories" in *The Philosophy of Rudolf Carnap*,

most extensively in the *Philosophical Foundations of Physics: An Introduction to Philosophy of Science*.[14] So this is the Carnap sentence approach to analyticity in the theoretical language that Friedman and Demopoulos discussed. I have done the best I can to make it plausible so that when I express reservations, as I shall presently, it will be plain that I am trying to treat Carnap fairly.

§2. Carnap's motivations

So let us turn to the question of motivation. Why do we need a special account of T-terms? Remember, we already have an account of analyticity that says we lay down for each term as it is introduced whatever definition, transformation rules, reductions sentences or meaning postulates would be most useful. This can be done for one term at a time or, when new terms are interrelated, for clusters of terms together. This is not to say that terms have their meanings entirely in isolation; we can still consider that it is the whole set of meaning rules that gives each term its meaning.[15]

Different languages are likely to require different accounts, even different kinds of accounts; no one-size-fits-all approach will do just as it does not for truth. In "Two Dogmas of Empiricism" Quine had demanded an account of 'S is analytic for L' where 'S' and 'L' are variables.[16] As Carnap put his response in a note that he left unpublished:

> In case Quine's remarks are meant as a demand to be given one definition applicable to all systems, then such a demand is manifestly unreasonable; it is certainly neither fulfilled nor fulfillable for semantic and syntactic concepts, as Quine knows.[17]

What is not necessary is a general procedure applicable to all theories such that if we plug in a theory, out comes the meaning postulate(s), much less different meaning postulates for each theory plugged in.

There is a second consideration that undermines the motivation for a special account of analyticity for theoretical terms. In the very book, *Philosophical Foundations of Physics*, where Carnap gives his best known version of the Carnap

P. A. Schilpp, ed., (LaSalle, IL: Open Court, 1963), 958-66.

14 Carnap, *Philosophical Foundations of Physics: An Introduction to the Philosophy of Science*, M. Gardner, ed., (New York: Basic Books, 1966).

15 As is perhaps obvious, this would not thereby give all terms the same meaning. This is because, informally put, the meaning of a given term would be its role with respect to the total set of rules.

16 W. V. Quine, "Two Dogmas of Empiricism" *Philosophical Review*, 60 (1951): 20-43, p. 32.

17 Carnap, "Quine on Analyticity", in *Dear Carnap, Dear Van: The Quine-Carnap Correspondence and Related Work*, R. Creath, ed., (Los Angeles: University of California Press, 1990, p.430.

sentence approach he gives another perfectly serviceable account of analyticity – though he does not call it that.

His account there of quantitative terms[18] argues subtly and powerfully that if we are to have quantitative concepts such as temperature, (geometrical) length, and temporal length, there are a number of things we need to do: We have to make certain pragmatic decisions; these are essentially conventional in the sense that we could have decided otherwise, and there is no fact of the matter about which way is the correct way. And we need to relativize our concepts and judgments to these decisions. In effect, this treats these pragmatic decisions as laying down meaning postulates and effectively introduces an analytic/synthetic distinction. These quantitative terms are arguably already theoretical especially if satisfying the measurement procedures is thought of as giving only strong reason for believing that the quantitative predicates apply.[19] Defining or partially defining these terms, as Carnap and his readers know, serves to partially contain an otherwise rampant underdetermination in measurement and theory.

So given that we have an account of analyticity that seems to apply to theoretical terms, and we do not need an account that is the same for all theories, why do we need more of an account than we already have? I cannot answer that question. But I can tell you what Hempel and Carnap said.

In *The Philosophy of Rudolf Carnap* Hempel raised the question of of analyticity in the theoretical languages (T-language) and then gave essentially the same argument that Quine gave in "Two Dogmas". Carnap responded that (a) the solution of the problem of analyticity for observation terms (O-terms) is given in "Meaning Postulates".[20] This claim is more implied than stated. He adds that (b) the solution there is restricted to O-terms,[21] and (c) that for O-terms the "meanings are completely known".[22]

I have worries about both Hempel's and Carnap's comments. Regarding Hempel, there is nothing in Quine's argument, and so nothing in Hempel's that applies only or especially to theoretical terms. Regarding Carnap, we should set the first part, (a), aside. Quine's worries about analyticity are not addressed by "Meaning Postulates" or even by the Carnap-sentence approach.[23] The second part

18 Carnap, *Philosophical Foundations of Physics*, pp. 51-95.

19 There are a number of reasons why one might want to do this. For example, one might think that sometimes the measurement instruments are not properly calibrated or are otherwise defective. It would have the effects of making the connection between satisfying the measurement procedures and the applicability of the quantitative predicates inductive, of making the quantitative terms ineliminable, and in turn making the system somewhat more flexible.

20 Carnap, "Meaning Postulates", *Philosophical Studies*, 3 (1952): 65-73.

21 Carnap, "Carl G. Hempel on Scientific Theories", p. 964.

22 Ibid.

23 In "Two Dogmas" Quine had challenged Carnap to give behavioral criteria that would indicate which sentences in actual languages were meaning determining and which were not. This challenge may not have been clear, and Carnap may, reasonably, have

(b) of Carnap's response is doubtful as well. "Meaning Postulates" is not restricted to observation terms at all. Nothing in that paper depends on "knowing the meanings" of the terms in question. It specifically says that there is no need for introspection or statistical surveys of use.[24] According to "Meaning Postulates", what is needed is a practical decision of whether to lay down postulates.

The only sense I can attach to (c), the idea that for O-Terms the meanings are completely known, would make it indefensible. Indeed, Carnap's own reply to "Two Dogmas" makes my point. In his paper Quine claims to understand 'green' and 'extended' completely perfectly well, but still to be in the dark as to whether 'Everything green is extended' is analytic, that is, true in virtue of meaning.[25] Carnap's response is that in certain respects the meaning of 'green' is far from clear. And he gives other examples from the observation language to the same effect.[26]

Thus, it seems that (a), (b), and (c), are inadequate as motivations for seeking a separate account of analyticity for the theoretical language. Remember that we already have an account of analyticity for quantitative terms, and these are already theoretical. Unfortunately, my worries on Carnap's approach are not confined to its motivation. I am also uneasy with the approach itself. So to this I now turn.

§3. The Carnap-Sentence Approach

There are a number of features of Carnap's account of analyticity in the theoretical language that make me uneasy. They do not quite show that the account is misguided, but they do make it somewhat less than fully attractive. While they do not suggest how to replace Carnap's account, they do suggest a source for my misgivings and hence a promising avenue to pursue.

One feature of Carnap's approach is that on this account a theory, T, is logically equivalent to its Ramsey sentence. The theory logically implies its Ramsey sentence by existential generalization. And because the Carnap sentence, $R(T) \supset T$, is analytic, the Ramsey sentence will L-imply the theory. Carnap does not say that T is to be replaced by R(T). But as far as one can tell, T is to be understood

taken the challenge to have been to make the boundaries of analyticity precise. In response, Carnap says in "Meaning Postulates" that in an artificial language one can lay down meaning postulates that specify which sentences, over and above the purely logical ones, are to count as analytic. This gives precision in an artificial case but no behavioral criteria. From Quine's perspective, this leaves completely unclarified what feature is being attributed to the precise class of sentences thus specified. It should be noted that "Meaning Postulates" also addresses successfully an issue raised by John Kemeny. This issue is internal to Carnap's approach to probability and need not be discussed here.

24 Carnap, "Meaning Postulates", p. 68.

25 Quine, "Two Dogmas of Empiricism", p. 31.

26 Carnap, "Quine on Analyticity", pp. 427-28.

as already saying the same thing as R(T). So in what sense does T introduce new entities at all?

This question is sharpened when we note that because R(T) seems to be about only abstracta and observable entities, and T is L-equivalent to R(T), T is likewise about only abstracta and observable entities. This by itself is not an objection. But we often want to attribute theoretical features to concrete observable particulars or to talk of specific theoretical particulars themselves, for example by giving them a location. I might want to say that the molecules at the end of my nose contain hydrogen atoms or that just there off to the southwest and just above the horizon is a massive discharge of electrical energy. In general these claims will not follow from T by itself. And it is far from clear how to integrate T, newly interpreted via the Carnap sentence, with other beliefs to make these particularistic attributions.

Carnap's approach, moreover, requires a sharp, syntactic observational/theoretical distinction. That sort of distinction was introduced in the discussion of empirical meaningfulness. It is appropriate there but less so on the issues of content and meaning. Carnap concedes that the underlying psychological phenomena on which the observational/theoretical distinction is based are matters of degree. Granted, sometimes it is useful to have sharper outlines in the model than in the thing modeled. And Carnap is not describing, he is investigating proposals. But given the underlying phenomena, the proposal is likely to be clumsy for certain purposes in a way that it is not when the issue is whether some term is empirically significant. In particular, the sharp observational/theoretical line is likely to guide us away from exploring the uncertainties of observation, that is, away from the idea that the confidence we ought to have in a given sort of observational claim will vary in degree given the circumstances.

It is also worrisome that not everything that can be said in the observation is an observational matter. Consider:

> There are physical objects not larger than 10^{-10} times the diameter of the smallest physical object any human has ever seen with the naked eye.

This seems to be in the logically extended observation language, and yet it would not be even remotely an observation sentence. Moreover, it is a claim to which our theories of the electron combined with our theories of human perception commit us. Or consider the sentence:

> There are concrete unobservables.

This is a claim to which our best theories seem to commit us. I do not know whether this is in the observation language or not, but if it is in the observation language I do not see that it can be an observation sentence. I suspect that Carnap would treat it as in the semantical metalanguage. If theories are construed in Carnap's way, then this would not be a commitment of our theories, whatever they say, because those theories commit us only to observable entities and to abstracta.

Another important feature of the theoretical vocabulary is that it can *acquire* a reporting use, that is, an observational use – at least if the theory is stable enough. For example, when my brother bridges a pair of live wires (in the US!) he calmly reports "That is about 120 volts" or (not so calmly) "[Expletive deleted] that is 240 volts!" Now has the entire vocabulary of electricity migrated to the observational vocabulary? Let us grant that when heretofore theoretical terms acquire a reporting role, their meanings have changed. They have changed somewhat but not completely. But now the Carnap-sentence approach to endowing them with meaning would seem wholly inapplicable. And it is difficult to see how to model along Carnapian lines the fact, if it is a fact, that in these changes the meaning of 'voltage' for example is pretty much what it was before.

The whole Carnap-sentence approach is predicated on the idea that the situations in the observation language and the theoretical language are very different. But much of what I find best and most attractive in Carnap's work pushed in exactly the opposite direction: He would be happy to acknowledge that the observation language is not given but conventional. We can construct it in various ways. Observational claims are uncertain, and observational terms are *not* completely understood. Remember Carnap's response to Quine's comment about 'Everything green is extended'. Overall, unlike some traditional forms of empiricism, Carnap rejects the idea that once one has phenomenal experience, then within the domain of expressions closest to that experience, meaning is clear and judgments are unproblematic.

I suspect that the reason that these aspects of Carnap's view are so little emphasized and sometimes denied outright by either Carnap or his readers is that Carnap did not have a full account of observational judgment that satisfied him. So Carnap set aside considerations of observation in favor of considering the special problems of theory. And this makes theory look more problematic even though many of the salient features of theory thus discussed are features that it shares with observation.

There is good reason to hope for a Carnapian account of observation. Richard Jeffrey, who besides being Carnap's student and collaborator had corresponded with Carnap on the issue, published a small amount of work after Carnap's death that goes a long way in the right direction. I am thinking here of his work on Bayes' factors in the context of trained observers.[27] Even this is not a full story, but it is the right sort of thing, and it can significantly help to restore a desirable evenhandedness in the treatment of theory and observation.

There is reason to be even more broadly hopeful about Carnap's account. For one thing his account, even the Ramsey-sentence dependent aspect of it, is profoundly right in calling attention to the idea that theoretical expressions refer in large part via their connections to observation. The inferential connections

27 R. Jeffrey, "Introduction: Radical Probabilism", in *Probability and the Art of Judgment*, (Cambridge: Cambridge University Press, 1992), 1-13. See also the references given there.

between observational and theoretical judgments are vital to understanding both reference and meaning. Indeed, the terms at the theoretical level serve as placeholders in a vast structural description of a system of inferences. Because of this, there is a certain openness in the reference of such terms. The Ramsey sentence approach quite insightfully highlights all of this. Unfortunately, the cost of the account is that it tends to disguise the idea that many of these same features appropriately apply to the terminology in which we express our observational judgments.

None of the various issues that I have discussed that make me uneasy about Carnap's motivation and his approach are fatal to the general project of providing a Carnapian account of meaning for theoretical terms. Whether or how the Ramsey sentence itself will figure into a revised version of that account I cannot say. But many of the features of the languages of science to which this account calls attention will, I suspect, continue to be central to our understanding.

In any case, the issues I have raised here show how much more *we* have to do. Carnap never intended to give us the final truth on all matters about science, but to invite us to join an ongoing project. Insofar as the various worries I have tried to articulate, or better the various issues that Carnap left unresolved, spur our own efforts (and I think they can and have), then Carnap has succeeded. And so, one would hope, have we all.

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Rudolf Carnap and Wilhelm Dilthey: "German" Empiricism in the *Aufbau*[1]

1. Introduction

Rudolf Carnap's formative years as a philosopher were his time in Jena (until 1919) where he studied mathematics, physics, and philosophy, among others, with Gottlob Frege, the neo-Kantian Bruno Bauch, and Herman Nohl, a pupil of Wilhelm Dilthey.[2] Whereas both the influence of Frege and of the neo-Kantians is quite well known,[3] the importance of the Dilthey school for Carnap's intellectual development was recently highlighted by scholars, such as Gottfried Gabriel and Hans-Joachim Dahms.[4] Although Carnap himself was interested mainly in the problems of logic and the philosophy of the natural sciences, the community in which he worked until he went to Vienna in 1926 was neither a community of neo-Kantian philosophers nor of logicians or philosophers of the natural sciences but a community of members of the Dilthey school that were interested in history of philosophy (Herman Nohl, Carnap's philosophy teacher in Jena, was concerned with the publication of a huge volume on the history of nineteenth Century philosophy),[5] pedagogic (this also is the case for Herman Nohl and Carnap's

1 I am grateful to A.W. Carus, Michael Friedman, Alan Richardson, and Tom Ricketts, for comments on the talk at the Carnap conference on which this paper is based, to Hans-Joachim Dahms, Richard Dawid, Christoph Limbeck-Lilienau, Thomas Mormann, and Matthias Neuber, for comments on different versions of this paper, and to the editor of this volume Richard Creath and the anonymous referees for very helpful suggestions.

2 For biographical information on the early Carnap, see his autobiography Carnap (1963, sections 1 and 2) and the (much longer) unpublished first version of this article in the Young Research Library, University of California at Los Angeles, Special Collections Department, Manuscript Collection No. 1029, Rudolf Carnap Papers, Box 2, CM3: M-A3, M-A4 and M-A5. Cf. also Carus (2007, ch. 1-7), Mormann (2000, ch. 1-4), Awodey & Klein (2004), and Flitner (1986, 118-128, 239-245, 272-276, 404-405).

3 Cf. Carnap (1963, section 1), Richardson (1998, ch. 4-6), and Friedman (1999, ch. 5 and 6)

4 Cf. Gabriel (2004) and Dahms (2004, 2011)

5 Nohl worked around 1910 (together with Max Frischeisen-Köhler) on the volume of Ueberweg's history of philosophy that was concerned with 19th century philosophy, but time reasons and the war forced him to give up that highly ambitious project. Cf. Flitner (1986, 119). The volume was later published in two parts as Oesterreich (1923 and 1928). Interestingly, Carnap had an annotated copy of the whole five volume set of Ueberweg's history of philosophy, including the two Oesterreich volumes, in his

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lifelong friend, Wilhelm Flitner),[6] aesthetics (Franz Roh, also a lifelong friend of Carnap, was one of the intellectual promoters of "neue Sachlichkeit"),[7] and sociology (Hans Freyer).[8] Carnap and his friends were all members of the so-called Sera-circle, a group of young people that met frequently in Jena and, between 1919 and 1926, also in Carnap's home in Buchenbach near Freiburg.[9] The first version of the *Aufbau* was written in close connection with this group of young people that were interested in a reform of the whole society, including arts, politics, sciences, and everyday life. In Carnap's Werkstatt in Buchenbach, the *Aufbau* and at least two more manifestos of a more or less philosophical nature were written: Franz Roh's "Nach-Expressionismus" and Wilhelm Flitner's "Laienbildung."[10] Given these historical facts, we must conclude that the *Aufbau* is the product of an intellectual enterprise that developed in close connection with the Dilthey school, but in which Frege and the neo-Kantians seem to have played only a small role.

Until recent times, the *Aufbau* is seen almost exclusively as a philosophical book that was influenced by Frege and Russell on the one hand and by the neo-Kantians on the other. Dilthey appears in that picture at best as an astonishing foreign substance that was removed by Carnap himself, as soon as he came under the influence of the Vienna Circle (and Otto Neurath in particular). This interpretation is supported by the fact that we can find only few mentions of Dilthey and Freyer in the *Aufbau*[11] (and no mentioning at all of Nohl and Roh) and that indeed the role of *Geisteswissenschaften* in the context of the constitutional system was played down by Carnap himself because in the systematic part of his book[12] he exclusively deals with the example of visual experience and mentions the rest of the constitutional system only in the context of some cursory remarks.[13] Moreover,

private library: see Archives of Scientific Philosophy, Hillman Library, University of Pittsburgh, Carnap Papers, Box 111, file 125-129 (henceforth, I quote the Pittsburgh-Nachlass in the format RC 111-125). – Nohl had published only few philosophical writings, but cf. his excellent Nohl (1935).

6 Cf. Flitner (1986, 120)

7 Cf. Dahms (2004)

8 Unlike Nohl, Roh and Flitner, Freyer was deeply involved with national socialism. This seems to be the reason why Carnap's friendship with him ended up abruptly around 1933. Nevertheless, there is an obvious influence of Freyer's cultural philosophy, as developed in Freyer (1928) on the early Carnap.

9 Cf. the unpublished manuscript of Carnap's autobiography (see footnote 1), B29-B36 and Flitner (1986, 140-171 and 272-276).

10 See Flitner (1921) and Roh (1925). Cf. Flitner (1986, 272ff). It seems likely that even Freyer (1928) is a book that was written under the influence of discussions in Carnap's Werkstatt in Buchenbach.

11 See Carnap (1998 [1928], §§ 12, 19, 23, 56). Henceforth, I quote the *Aufbau* by the paragraphs and without mention of "Carnap (1998 [1928])." The translation of Rolf A. George is modified in one respect: I use the term 'constitutional' instead of 'constructional.'

12 Cf. §§ 108-122

13 Cf. §§ 123-156

because Carnap in the *Aufbau* does not even mention history and sociology of science as something important for the study of constitutional systems, the most plausible interpretation of this book seems to take it as a peace of a purely formalist philosophy of the natural sciences with no connection to history and sociology of science and without any substantial connection to the *Geisteswissenschaften*.

However, a few things should be noted. First, it was Carnap's opinion, at least in 1928, that there must be some sort of *division of labor* in philosophy, between people like him that are concerned with logic, mathematics, and the natural sciences, and others (i.e., people like Neurath and, one may add, even Dilthey, Nohl, Freyer, etc.) that are concerned with the sociological, historical, and psychological background of the sciences. In a letter to Neurath from 7 October 1928, Carnap wrote:

> […] a logic, a method of concept formation must be constructed which takes account of the fact that we are always presented with a mixture of crystals and dirt, which tells us therefore what demands can be imposed on scientific concepts and statements, as long as the 'ideal language' is not available. And second […] it would be important to concern oneself with problems in history and sociology. Of course, these two things hang together since in sociology there's more dirt than in physics. I have now seriously resolved to make a start on the first task; i.e., not immediately by way of writing but of course only by thinking [about it]; I already have a few vague ideas. As to the second, I will concern myself more than so far with the sociological problems, but more out of a human interest, thus as a layperson; to get beyond that stage I cannot expect. RC 029-19-01[14]

On that basis, one may guess that sociology and history of science was much more important for Carnap than the formal character of his philosophical theories suggests, which seems to be supported (at least partly) by the fact that the *Aufbau* was presented explicitly as *a combination* of modern logic with the purely empirical task of "analysis of reality."[15] Second, Carnap made some (significant) changes to the original manuscript of the *Aufbau* (from 1926) on the basis of criticism from people like Neurath, Schlick, and Reichenbach so that the *Aufbau*, in its published version from 1928, must be seen as the product of *both* the intellectual scenario of the Jena Circle *and* the Vienna Circle. This, in particular, may have caused a tendency to underestimate some of the original influences on that work.[16] Third, it was Carnap himself who conceded at the end of his life that the Dilthey school may have been much more important for his philosophical development than it seemed to him for a long time. In a letter to Wilhelm and Elisabeth Flitner from 11

14 Translation quoted from Uebel (2007, 107).

15 Cf. § 3

16 Cf. in particular Carnap's correspondence with Schlick RC 029-27ff. The best way to support that thesis would be a comparison between the versions from 1926 and 1928. Unfortunately, the manuscript from 1926 seems to be lost. See also Friedman (1999, 146 n.52).

December 1968, Carnap wrote, with reference to Günter Patzig's commentaries to his "Overcoming of Metaphysics":

> Patzig says there that my view that metaphysics has no cognitive content but is only an expression of *Lebensgefühl* is evidently influenced strongly by Dilthey. I told him it seemed doubtful to me, for as far as I can remember I have never myself read anything by Dilthey, but only heard occasional references to him by Nohl. A short time ago my friend Arne Naess, from Oslo in Norway, was here and brought me his new book *Four Philosophers*. One of the four parts of the book is about me (the others are about Wittgenstein, Heidegger, and Sartre), I'm in rather strange company there.
>
> Naess has quotes there of Nohl's and of Dilthey's, and from them I saw with amazement how strong Dilthey's influence on me in this particular respect, via Nohl, really was. (The whole difference lies, of course, in the fact that Dilthey and Nohl didn't draw the conclusion from this insight that metaphysics doesn't matter.) Gabriel (2004, 16-17)

Given these historical insights, the present paper shall suggest in a rather systematic way that the Frege-Russell-aspects and the neo-Kantian aspects are *not sufficient* for a proper understanding of the intellectual background of the *Aufbau*. There also is a profound Diltheyian aspect in the *Aufbau* that diverges from both Russell's sense-data empiricism and the neo-Kantian accounts of the Marburg and the southwest German school. The constitutional system of the *Aufbau* can be seen as a proposal in the tradition of an idiosyncratic version of *empiricism* that was developed in the nineteenth Century by Dilthey and other German philosophers to find *empirical* access to the *mental objects* ("geistige Gegenstände") that Kant and Hegel had attempted to analyze in a purely aprioristic way. The former accounts, such as Carnap's proposal, are somewhat *intermediate* between classical empiricism and the accounts of the (neo-)Kantian tradition. This certainly does not necessarily lead to a *rejection* of the neo-Kantian readings of the *Aufbau* and a rejection of those interpretations that mainly point out the influences of Frege and Russell. What I mainly want to argue for in the present paper is simply that the intellectual background of the *Aufbau* is *even broader* than it is suggested by those classical interpretations.

2. Dilthey's "German" Empiricism

Wilhelm Dilthey (1833–1911)[17] was one of the key figures of philosophy in Germany after Hegel and the so-called breakdown of German idealism and before the

17 For a more detailed account of Dilthey's epistemology, see my Damböck (under review). Cf. also Makkreel (2010) and Lessing (1984) and the editorial introductions to the relevant volumes (especially I, V, XIX, and XII) of Dilthey (1914-2006). Volumes I and XIX of the *Gesammelte Schriften* are quoted here from the translation of Dilthey (1989); other translations are my own and are accompanied by the German original in a footnote.

development of the crucial currents of twentieth century philosophy, such as neo-Kantianism, fundamental ontology, phenomenology, and critical theory. Dilthey shared with philosophers, such as Eduard Beneke, Jakob Friedrich Fries, Adolf Trendelenburg, Friedrich Ueberweg, Moritz Lazarus, and Heymann Steinthal,[18] the attitude of a rejection of Hegel's "pure logic". Like those philosophers, Dilthey attempted a reconciliation of empiricism and positivism with the characteristic feature of using empiricism as a method to make accessible the classic field of enquiry of Kant's transcendental philosophy and Hegel's pure logic, in an *a posteriori* way. An important reference point of all these approaches was John Stuart Mill's logic which also tried to develop such an *empirical* foundation of logic, partly in accordance and partly in disagreement with Auguste Comte.[19] However, Dilthey and his Berlin contemporaries did not arrive at a strict empiricism à la Mill and Comte. They shared the principal programmatic stance of Comte and Mill but rejected their concrete empirical strategies. Whereas Comte and Mill attempted to find a foundation for sociology and the human sciences in the natural sciences,[20] Dilthey argued that it is impossible to understand the abstract notions of these (and other) sciences on a basis that stems from natural science exclusively. Believing that they could access the abstract background of reasoning *on a meta level*, using only natural sciences, Comte and Mill turned out to be no less *metaphysical* thinkers than Kant and Hegel: whereas the latter tried to find access to the abstract categories of reasoning in a transcendental or pure logic, respectively, and thus in a way that has no connection at all to the *empirical process of reasoning*, Comte and Mill also did not study the empirical process of reasoning but only the physical surrogate of that process. Thus Comte, Mill, Kant, and Hegel were all *metaphysicians* for Dilthey:

> Previous epistemology – Kant's as well as that of the empiricists – has explained experience and cognition in terms of facts that are merely representational. No real blood flows in the veins of the knowing subject constructed by Locke, Hume, and Kant, but rather the diluted extract of reason as a mere activity of thought. Dilthey (1989, 50)

Both the a priori approach of transcendental and pure logic *and* the a posteriori approach of physiology and associative psychology attempted to analyze "mental objects" ("geistige Gegenstände") merely on a meta-level (of pure logic or natural

18 On academic philosophy in nineteenth century Germany cf. Köhnke (1986), Schnädelbach (1984), and Oesterreich (1923).

19 Cf. Mill (1976 [1843]). See also Köhnke (1986, 81) who points out that philosophers like Beneke and Trendelenburg developed their empiricist views more or less independently from the British and French empiricist tradition: "In Germany there was no need for a Comte or Mill, in order to develop a notion of positive philosophy" ("In Deutschland bedurfte es keines Comte oder Mill, einen Begriff von positiver Philosophie zu fassen."). However, at least in the case of Dilthey, there is an obvious direct influence of the British and French empiricist tradition.

20 Cf. Mill (1976 [1843], book VI, especially ch. 4)

science) but did not study the empirical process of reasoning as such. However, the concrete structure of mental objects is accessible only if we analyze them *in the context of their development.*

This consideration led Dilthey to a rejection of the ahistorical methods of Kant and Hegel and also led him to a critique of the historical and psychological methods of Comte, Mill, and Buckle.[21] Dilthey's approach led to a completely new understanding of the term "empirical" and to the idea of a new method in psychology that shall allow us to systematize that new empirical world (as something in between the a priori world of German Idealism and the a posteriori world of the natural sciences).[22] Thus, the "German" empiricism of philosophers, such as Dilthey, Trendelenburg, and Ueberweg, is a quite special form of empiricism because it is based on an empirical version of something (i.e., the a priori world of transcendental and pure logic) that, for a full-fledged (French or British) empiricist, *does not exist at all*. In other words: the charm of "German" empiricism lies in its hybrid nature that makes it both very German and very un-German at the same time. (This is the reason why I always put the term "German" within quotation marks here.)

The tactic of "German" empiricism that we may call an "empirization of the transcendental" was generally restricted to those parts of the sciences that Dilthey called *Geisteswissenschaften*: history, psychology, sociology, and those parts of philosophy that remain after the overcoming of metaphysics. Unlike Kant who would have claimed his transcendental logic to be a method for a deductive treatment of the natural sciences *in particular*, Dilthey firstly restricted his philosophical method to those sciences that deal neither with pure formal constructions (like in mathematics) nor with spatiotemporal facts (like in physics) but with "facts of consciousness" ("Tatsachen des Bewusstseins"). Mathematics and the natural sciences are both based on assumptions a priori plus (in the case of the natural sciences) an empirical basis of spatiotemporal facts. The structure of those sciences is strictly *objective* (in the sense of being not influenced by the scientific *subject*). According to Dilthey, philosophy has no access at all to that objective side of the natural sciences. Nevertheless, even in philosophy and in the *Geisteswissenschaften,* the assumptions of the natural sciences may come into focus because as Dilthey points out, the historical perspective of the *Geisteswissenschaften* provides us with a technique that allows us to turn the assumptions of *all* "particular sciences" ("Einzelwissenschaften") *into the empirical*, namely insofar as they are taken as facts of consciousness, implying that, in contrast to mathematics and the natural sciences, philosophy and the *Geisteswissenschaften* and, in that context, even the "natural sciences" as taken from a historical point of view are in no respect based on assumptions a priori but are *purely* empirical things:

21 Cf. Dilthey (1989, 48ff)

22 Cf. Dilthey's paper from 1894 "Ideas for a Descriptive and Analytic Psychology" Dilthey (2010, ch. 5), but also his writings from the Nachlass in Dilthey (1914-2006, vols. XIX and XII).

> What appears from the standpoint of a particular science to be an ultimate truth or an axiom is given with evidence as a fact of consciousness for this comprehensive empirical science [namely philosophy and the *Geisteswissenschaften*, C.D.]. This fact enters the domain of the analysis of consciousness possibly to be clarified in this context, or possibly to be subjected to further psychological analysis. If I regard an axiom from the point of view of the evidence that other propositions, derivative from it, borrow from it, as the moon borrows its light from the sun, then this axiom is for me an ultimate truth. This is the standpoint in which the individual sciences ground their axioms, and from which they develop their systems. If I regard this evidence, however, in the context in which it is originally given, I assume the standpoint of the general experiential science [again, philosophy and the *Geisteswissenschaften*, C.D.] which has the nexus of facts of consciousness for its object. Dilthey (1989, 270-271)

This establishment of a *second point of view* that transforms the assumptions of the sciences into external objects of scientific study is a turn in epistemology which is characteristic also for neo-Kantian philosophy. Both the neo-Kantians of the southwest German and of the Marburg school turn Kant's absolute realm of the synthetic a priori into a relativized realm of structures that are historically relative (in the case of the Marburg school) or simply a question of stipulation of values (in the case of the southwest German school).[23] However, neo-Kantian philosophers never claim that *the science* that allows us to study that relativized realm of a priori structures is a *positive science* in Dilthey's sense (e.g., history, sociology, or psychology). On the contrary, they demand for that realm to be an exclusive business for philosophy which ultimately must provide us with some sort of an a priori method that allows us to access that realm of the relativized a priori or of values, respectively. *The content* of philosophy is the same for both Dilthey and the neo-Kantians, but *the method* to make that content accessible is quite different. For the neo-Kantians, that method is still an aprioristic one; for Dilthey, the method is empirical. Because philosophy, for Dilthey, deals only with facts of consciousness, it turns into an empirical ("positive") science:

> When Kant undertook a pure analysis of the subject and its scientific knowledge with the intention of solving the problem definitely, he divorced his philosophical analysis from the positive human sciences.
>
> Once one recognizes that these problems are connected with those of comparative grammar, mythology, and cultural history, then the task of philosophy cannot be distinguished from that of the positive science of history either by its method or by its means, or even fully by its object. The barrier between philosophy and the positive sciences collapses, just as it could not be upheld between philosophy and the principles of natural science. It derives from the unavoidable narrowness of human nature, which favors one sort of means

23 The main representatives of the Marburg school were Hermann Cohen, Paul Natorp, and Ernst Cassirer; the southwest German school was mainly represented by Wilhelm Windelband and Heinrich Rickert. For an overview of the two schools cf. Holzhey (2004).

and problems over another, but need no longer be seen to reside in any difference regarding [philosophy's] overall object, method, or means.

This suggests a solution to the problem of the unlimited progress of positive knowledge: its limits are only those of the epoch in which we live; there is no absolute philosophy. Dilthey (1989, 279)

While even Cassirer arrived in his "philosophy of symbolic forms" at a notion of philosophy that makes it indistinguishable from the "positive science of history" by its object but demands an exclusive philosophical *method* with no connection to the "positive sciences" of history and/or psychology (Cassirer turns the "Critique of pure reason" into a "Critique of culture,"[24] a culture which is *historically changeable* and thus a posteriori but which is *accessible* only in an aprioristic way); for Dilthey, the whole business of philosophy turns into a business of the "positive sciences" of history and psychology: he turns the "Critique of pure reason" into a "Critique of historical reason."[25]

It is important to note that *both* Dilthey *and* the neo-Kantians developed a notion of philosophy that is *not* completely relativistic. A complete relativism would *only* be given in the context of a *naturalization* of the mental objects or facts of consciousness that makes them an exclusive business of the natural sciences. This is the case in Comte's positivism, Mill's empiricism, and in the materialism of German philosophers, such as Büchner and Moleschott.[26] In this respect, Dilthey and the neo-Kantians share the attitude of F. A. Lange's "History of Materialism"[27] that points out (1) that philosophy has to be naturalized as far as possible (because there is no absolute philosophy), but (2) that a *full* naturalization of philosophy is impossible. *The methods* of philosophy are not identical with the methods of the natural sciences. For Lange, the neo-Kantians, and even Dilthey all have the same reason for distinguishing philosophy from the natural sciences: they are all *Kantians in the broadest sense* (stated in section 4). However, whereas the neo-Kantians search for a method that remains to be exclusively philosophical (an attitude that they share with Husserl and his "transcendental phenomenology"), Dilthey's philosophical method is an introspection-based "descriptive psychology" (plus the history and sociology of science that can be established on the basis of that method). The neo-Kantians (and Husserl) try to carry on the old tradition of a *philosophical* (transcendental, pure or epistemological) *logic* in the new age of relativized philosophy, whereas Dilthey tries to find *a replacement* for that method in a nonmaterialistic "descriptive psychology" and thus *inside* the realm of the "positive sciences."

24 Cassirer (1997 [1923], 11)

25 Dilthey (1989, 165)

26 Dilthey is certain that all those kinds of materialism are inacceptable to him. See, for example, Dilthey (1914-2006, XXII, 140ff).

27 Lange (1866)

3. Dilthey's Nonreductionism

Another convergence between Dilthey and the empiricist tradition is found in his rejection of psycho-physical *dualism*. Like Comte and Mill, Dilthey rejects the idea of mental objects that exist in isolation from their physiological representation. On the other hand, Dilthey also rejects *reductionism* which is a crucial aspect of all classical versions of empiricism and positivism. According to Dilthey, the mind is *hierarchically* organized. There are some "lower" regions in the mind – everything that is directly connected with sense impressions – that are not only *reducible* to physical phenomena but rather *identical* with them. On the other hand, there are "higher" regions – everything that is the product of abstract reasoning and only indirectly connected with sense impressions. Those higher regions are *independent* of the basic physical aspects of the mind (because they are products of the logical process of reasoning and not products of the physical process of perception). They are also dependent on the physical world: the higher level objects of the mind are not only represented as physical objects (in the brain) but results of a causal process which is deeply connected to the lower physical parts of the mind:

> [The] higher phenomena of consciousness […] are doubtlessly products of the lower ones. The lower ones build their basis. But they are not only composed of compounds that can be completely derived from the elementary ones. The notions of development, evolution, unfolding express correctly the mode of causality that is at work here. Dilthey (1914–2006, XXII, 12)[28]

Dilthey's hierarchical conception of the mind also implies a rejection of so-called psycho-physical *parallelism*, that is, the claim that (1) every mental state can be univocally identified with a physical state and (2) causality exists only on the mental and physical levels but never between these two levels. Although Dilthey shares the first part of that claim with the parallelists, his hierarchical conception implies a rejection of the second part, simply because he takes the higher level parts of the mind *as the results of a causal process* that starts with purely physical states. If it is true that the higher level parts of the mind "develop, evolve, unfold" from the lower level (and purely physical) parts, then this would imply that there are causal relations between the physical and the mental levels. Moreover, these relations are most important because, according to Dilthey, only a *historical* reconstruction of the development of the mind (including psychological, biological, and sociological aspects) allows us to understand what kind of things high level mental objects are. Because parallelists reject the existence of causal relations between

28 „[Die] höheren Bewußtseinsphänomene […] gehen ohne Zweifel aus den niederen hervor. Die niederen bilden ihre Grundlage. Aber sie sind durchaus nicht bloß zusammengesetzt von Verbindungen, die aus den elementaren gänzlich abgeleitet werden können. Die Ausdrücke Entwicklung, Evolution, Entfaltung sprechen zutreffend die Art von Kausalität aus, welche hier waltet".

the physical and the mental, they necessarily arrive at a rather artificial and static conception of the mind. Thus, Dilthey accepts a full-fledged parallelism only on the level of low level objects of the mind. However, the more complex and abstract aspects of the mind are something that must be studied in a rather holistic way, as results of the "psychophysical life unit," including both physical and mental factors:

> Mental facts comprise the uppermost limit of natural facts, and the latter the underlying conditions of human life. Because the realm of persons, including human society and history, is the highest phenomenon of the empirical world, knowledge of it must at countless points be based on the system of presuppositions which accounts for its development within the whole of nature. Dilthey (1989, 69)

Dilthey's position in the context of the psycho-physical problem is a version of *weak parallelism* that shares part (1) of the full-fledged parallelism previously described but rejects part (2) of it. Dilthey specified his position on the basis of the following "Three fundamental laws concerning the universal connection between the physical and the mental":

> 1. The first recognizable lawful relation between material and mental facts is that of the dependence of mental activities, directly from the brain and the nervous system, indirectly from the physical processes proceeding in the body, therefore even further mediated from the whole physical environment in which man is living. […] There is no mental activity that is not determined by the condition of the brain and the nervous system. […]
>
> 2. The overview of the organic world, then, delivers us a second comprehensive lawful relation. We will call this the correspondence or the parallelism between the physical and the mental. The notion psychophysical parallelism became ambiguous. As correspondence I understand the fact that in the whole organic world a particular condition, structure and differentiation of the nervous system is connected with a particular condition, structure and differentiation of the mental activities. […]
>
> 3. […] Inside of the mental world there is a process of differentiation of the mental life; in analogy with the physical differentiation of the organism, in the animal kingdom there always developed a higher level of mental life on the basis of a lower one. […]
>
> [in the following §11 this third law is further clarified:] The mental processes appeared to us to be in parallel with the physical. Equally true and important, however, is the awareness of the incommensurability of the two regions. […] [O]n closer inspection, […] how mental entities are interconnected is totally different from how we determine physical phenomena through the medium of thought. Dilthey (1914-2006, XXII, 148-150)[29]

29 „1. Das erste erkennbare allgemeine gesetzliche Verhältnis zwischen den materiellen und den psychischen Vorgängen ist das der Abhängigkeit psychischer Vorgänge direkt von dem Gehirn- und Nervensystem, mittelbar von den im Körper verlaufenden physischen Prozessen, demnach auch weiter vermittelt von dem ganzen physischen Milieu in welchem der Mensch lebt. […] Es gibt keine psychische Leistung, welche nicht von der Verfassung des Gehirns und Nervensystems bedingt wäre. […]
2. Der Überblick über die organische Welt liefert uns alsdann ein zweites umfassendes gesetzliches Verhältnis. Wir wollen dieses als Korrespondenz oder Parallelismus des

In these fundamental laws, Dilthey's hierarchical conception of the psychophysical world can be found. The mind is nothing independent but is deeply determined by the physical milieu in which it develops. In the case of lower animals, such as protozoa or frogs, we have a de facto *identity* between the physical and the mental, as well as in the case of simple perceptions in higher animals and human beings. However, in the course of evolution, the latter develop better and better mental abilities whose independence increases insofar as the possibility of a complete analysis on the exclusive basis of physiological analysis decreases. This does not change the fact that even the mental life of a higher animal or a human being is deeply dependent on its physical milieu (fundamental law 1). Psychophysical parallelism (fundamental law 2) implies that every mental state must have a physical correlate or a suitable functional explanation in the field of physiology. Nevertheless, the mental and the physical world are not *identical*; they rather build two *incommensurable* epistemic fields (fundamental law 3).

Dilthey's conception of the psychophysical world is *both* a reductionist *and* a nonreductionist conception. Dilthey shares with the reductionist conceptions of the strong parallelists and the materialists the attitude to take an independent world of mental states as an absurdity. However, at the same time, he holds that the mental world is not only *a product* of the evolution of the physical world, but as such a product it forms a new kind of supervening "entities" being incommensurable with the purely physical objects of the brain and the nervous system. For Dilthey, both dualism and strong parallelism fail to provide adequate theories for that situation. His alternative is a weak parallelism that allows some sort of causality between the mental and the physical, a theory that may be seen as a compromise between strong parallelism and dualism. The ultimate reason why Dilthey takes these nonreductionist elements of his theory as indispensable is obviously his rejection of the idea of human sciences are just a special form of natural sciences (as it was defended by Hume, Comte, and Mill). Thus, Dilthey's conception of human sciences ("Geisteswissenschaften") as something *relatively independent* of the natural sciences, may be seen as a compromise between the reductionist

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Physischen und Psychischen bezeichnen. Der Ausdruck psychophysischer Parallelismus ist vieldeutig geworden. Ich verstehe unter Korrespondenz die Tatsache, daß in der gesamten organischen Welt eine bestimmte Beschaffenheit, Struktur und Differenzierung des Nervensystems mit einer bestimmten Beschaffenheit, Struktur und Differenzierung der seelischen Leistungen verbunden ist. […]

3. […] Es gibt innerhalb der psychischen Welt einen Vorgang der Differenzierung des psychischen Lebens; analog der physischen Differenzierung des Organismus hat sich in der Tierwelt stets auf der Grundlage einer niederen Stufe des psychischen Lebens eine höhere entwickelt. […]

[§11 …] Die seelischen Vorgänge zeigten sich uns den körperlichen parallel. Ebenso wahr und wichtig aber ist die Erkenntnis von der Unvergleichbarkeit beider Gebiete. […] [B]ei genauerem Zusehen […] zeigt sich […] die gänzliche Verschiedenheit der Art und Weise, wie Psychisches untereinander verbunden ist, und der Art, wie wir physische Erscheinungen durch Denkmittel bestimmen."

positions of Comte, Mill, and Hume and the (extremely) nonreductionist conceptions of the rationalist and idealist tradition of Leibniz, Kant, and Hegel.

4. Dilthey in a broader historical context

Before we turn to the *Aufbau,* we give a sketch of the broad historical background of the philosophy of the *Aufbau*, as it starts with rationalism, empiricism, and Kantianism (cf. the table below). In pre-Kantian philosophy, we have two fundamentally different notions of the world of concepts: the rather platonist conception of *rationalism* that situates the world of concepts in a transcendent realm of ideas and the *empiricist* conception that tries to reconstruct concepts as mere causal *results* of the physical world. In both cases, we have conceptions here that imply that we are able to understand the world of concepts without any direct reference to our own reasoning. Reasoning is relevant here only insofar as it *distorts* the world of concepts that is completely externally given, either in a platonic or in a purely physical realm.

the world of concepts is	rationalism	empiricism	Kant	Hegel	Dilthey	Marburg neo-Kantianism
nonphysically transcendent	X					
transcendental			X			
historically variable				X	X	X
actually convergent				X		
dynamically-transcendental						X
nonphysically empirical					X	
physically determined		X				

"German" empiricism

Kantianism in the broadest sense

In sharp contrast to these conceptions, there is a whole family of conceptions that hold for the world of concepts being something that can be understood *only* by means of a study of reasoning. We may call these conceptions *Kantian in the broadest sense* because it was Kant who famously formulated, in his *Critique of Pure Reason*, the doctrine of so-called *Copernican turn* that switches the focus of philosophy from the external world to the reasoning subject.[30] I will mention four examples for Kantianism in the broadest sense here. First, Kant's own position that is characterized by its *static* character: concepts, according to Kant, do not have a history; thus, every person must arrive at the end at the same conceptual world.

30 See Kant (1998, BXVIf).

The other three conceptions reject that static character of Kant's transcendental philosophy. Insofar, they all share the attitude of Hegel's absolute idealism, according to which the world of concepts is historically dynamical. The remaining two conceptions, however, disagree with Hegel (and Kant) in that they both claim that there is no absolute world of concepts at all, neither a static world, in Kant's original sense, nor a dynamic world that leads us with necessity to a particular result, as it was stated by Hegel. We could say that the two remaining positions that we consider here (namely Dilthey and the Marburg school of neo-Kantianism) diverge from all the other positions insofar as they are instances of *conceptual relativism*: the world of concepts is something that is inevitably *historically relative*. Every historical situation (and at the end even each particular person) has its own world of concepts that reflects the respective status of arts and sciences and the external world and the respective psychological status of the persons that develop that world of concepts. Especially, the respective status of the sciences, unlike in the concepts of Kant and Hegel, is nothing that can be optimized or even proved to be the optimum by means of some internal conceptual argumentation. The only thing that philosophy and epistemology can do here is to point out the respective status of the world of concepts and to illustrate that world in a rather systematic way. Such a *rational reconstruction* of the sciences (and even of other cultural phenomena) may have a number of virtues and functions. It may allow us to put them into a broader historical context and to study the logical relations between particular parts of that world (e.g., to find inconsistencies or to point out interdisciplinary connections). Thus, *the aim* of the study of the world of concepts is not particularly different, in neo-Kantianism and in the Dilthey school. The attitude that epistemology merely can look at science and culture *from the outside* is something that is shared by all versions of neo-Kantianism, the Dilthey school (and by Carnap and the philosophers of the Vienna circle).

Given these preparations, the difference between the Marburg school of neo-Kantianism and Dilthey seems to be only the following, at a first glance, rather inconspicuous thing. Dilthey's opinion is that conceptual structures are something that must be present to the philosopher *in a purely empirical way* (i.e., in the way of his idiosyncratic "German" version of a nonmaterialist empiricism as it was pointed out in section 2). Therefore, the *empirical basis* must provide the epistemologist with the conceptual structures *as something completely objective*. What remains to do for her is simply to put these structures into some (historical, sociological, psychological, or even formal) context and to reconstruct them by means of these external aspects. In contrast to this, for the Marburg school in general and for Ernst Cassirer in particular, the conceptual structure, although present in the empirical material that science provides to the philosopher, is something that has to be firstly reconsidered by the philosopher to sift out something: the objective core, the very structure of that concept, or the like. Whereas for Dilthey, *objectivity* is something completely deflationary – the concept is objective simply because

it is reasoned by a particular person or by a group of persons, and this very fact makes it an object – for the neo-Kantians, there is a notion of objectivity at work that is much more philosophical (and, in a sense, quite *platonistic*): for the (neo-) Kantian, *it is only philosophy* that enables us to find out *in what sense a concept is objective*. The neo-Kantian of the Marburg school is thus concerned with the never ending process of sifting out the objective core of a concept, a task that is quite similar to the task of the neo-Kantian of the southwest German school who wants to determine *the value* of scientific concepts. *In both cases,* there is something missing in the concept because the concept is provided to the philosopher by the sciences, something that can be only sifted out in the context of some strictly aprioristic work in the philosophical laboratory.

For Dilthey, in contrast to this, it is simply not true that there is an objective core or a hidden value of the concept that *philosophy* has to sift out from the conceptual material that sciences provide to it. The world of concepts, of abstract content is *a proper part of the empirical world*. Thus, the task of sifting out the objective core of the conceptual material cannot be a philosophical task because the questions of what the objective content of an object may be or in what sense a particular concept is true or properly chosen or useful in a particular context are all questions that can be answered *only by the sciences themselves*. What philosophy can do here is only to reconstruct and to systematize the motivations and conventional decisions that the sciences provide to it. To conclude, what the neo-Kantians try to handle in a (more or less relativized and slimmed-down) *transcendental realm* is to be handled, according to Dilthey, in an *empirical realm* of history, psychology, and sociology of science.

5. "German" Empiricism in the *Aufbau*

The constitutional system of the *Aufbau* is an all-encompassing system that derives *every* concept from a limited range of basic concepts. According to Carnap, there are at least two possible versions of such basic concepts: the physicalistic basis of concepts that refer to observable objects and the phenomenalistic basis of concepts that refer to recollections of similarities between elementary experiences.[31] Carnap chose the latter because of its advantage that it describes the development of concepts exactly that way they actually develop in the human mind (epistemic primacy).[32] According to Carnap, *the structure* of any concept of a constitutional system thus developed must be *already given* as part of the structure that is specified by a fundamental relation Er of recollected similarities between the elementary experiences of a particular person. What logic contributes here

31 See §§ 59 and 60. Carnap mentions even a third form of constitutional system here, namely one that has a heteropsychological ("fremdpsychische") basis.

32 See § 54

is simply to *reconstruct* the formal substance of the highly complex relation Er in such a way that the hierarchical character of that structure becomes visible. *Rational reconstruction* ("rationale Nachkonstruktion") is nothing else than the systematic study of the order theoretic properties of a complex formal structure of that kind. Therefore, in particular, *the structure* or *the objective content* or even *the value* of the structure that is studied in rational reconstruction is nothing that *results* from the process of rational reconstruction but *is taken as its starting point.* This indicates a fundamental difference between Carnap's *Aufbau* program and (the Marburg school of) neo-Kantianism and a particular convergence between Carnap's program and the "German" empiricism of the Dilthey school.

In the *Aufbau,* Carnap explicitly criticized the idea of Marburg school of neo-Kantianism that there is some objective core of the concepts that the philosopher has to sift out from the conceptual material that science provides to her:

> According to the conception of the *Marburg School* […] the object is the eternal X, its determination is an incompletable task. In opposition to this it is to be noted that finitely many determinations suffice for the constitution of the object – and thus for its univocal description among the objects in general. Once such a description is set up the object is no longer an X, but rather something univocally determined – whose complete description then certainly still remains an incompletable task. (§ 179)

This statement does not imply that Carnap rejected (neo-)Kantianism *in every respect*. It is true that the project of a purely structural theory of knowledge converges with the Kantian tradition *in the broadest sense* but diverges from classical empiricism insofar as the world of concepts for Carnap is something that cannot be reduced to mere sense data (and thus to the external world). Beyond that general aspect, however, Michael Friedman, in my opinion, overdoes the neo-Kantian perspective in the *Aufbau* in criticizing Carnap's own account as unable to dissociate itself from the Marburg school of neo-Kantianism.[33] As Friedman describes the constitutional system of the *Aufbau*, the definition of concepts in the context of that system has a *conventionalist* element that is interpreted by him as being something that we do not find in the rough material of Er but something that is added *on a meta-level*, by the persons that build the constitutional system. Because the process of concept formation thus carried out fails to stop because of some failures of the constitutional system of the *Aufbau*[34], Carnap, according to Friedman, also fails to dissociate himself from neo-Kantianism:

33 See Friedman (1999, ch. 6, especially the "postscript").

34 See Friedman (1999, 161): „[…] the assignment of colors (and, more generally, of 'perceptual qualities') is continually and indefinitely revised as we progress through the hierarchy of types. This is because, first, the initial assignment – based on the 'observations' of a single subject – is subsequently revised on the basis of both the reports of other subjects and the scientific regularities discovered in the world of physics; and second, the construction of the world of physics suffers from a precisely parallel ambiguity: the methodological procedure leading (via the 'physico-qualitative coordination') from sensible qualities to numerical physical state-magnitudes also is

> Carnap's construction of the physical world therefore appears never to close off at a definite rank in the hierarchy of types: it is continually revised to infinity. And this means, of course, that the Marburg doctrine of the never completed 'X' turns out to be correct – at least so far as physical (and hence all higher-level) objects are concerned. Whereas autopsychological objects receive actual definitions locating them at definitive type-theoretic ranks (as sets of … sets of elementary experience), this is not and cannot be true for the higher-level objects. It follows, therefore, that Carnap's rejection to the synthetic a priori – according to which all characterizations of the objects of sciences are either definitions (conventional stipulations) or ordinary empirical truths (concerning already-constituted objects) also fails. (Friedman 1999, 161-162)

This analysis is obviously based on a picture of the nature of concept formation that is deeply inspired by the Marburg school in general and Ernst Cassirer in particular. According to that view, the task of philosophy is to show the formal structures of concepts to be *Grenzbegriffe* that converge in some sense to the empirical world.[35] Therefore, one of the main tasks of philosophy is to prove that our formal conceptual structures do really have *objective content* or fit into the empirical world. In that respect, the philosopher has to add something substantial to the concepts as provided to him or her by the sciences: some conventional decisions that can be made only on a philosophical meta-level.

In sharp contrast to this, for Carnap, *the philosopher on the meta-level of constitutional theory makes no conventional decision at all, neither concerning the formal structure nor the propriety of the formal structure of concepts; all conventional decisions we need take place already on the object level, that is, in the real world of the sciences*. The philosophical constructor of constitutional theory *only* reconstructs an order structure that is *completely* given by means of the relation Er of recollected similarities between elementary experiences.

However, if this is the case, then it may well turn out, at the level of constitutional *theory*, that a particular *system* of concepts fails to be a proper system, to be consistent, or to have finitely axiomatizable definitions (because it is circular in a way); it may well turn out even that some essential assumptions of the constitutional *system* (e.g., reductionism) fail. Clearly, such failures (as long as the logic that constitutional theory uses is consistent in itself and allows us to reconstruct every aspect of the empirical source) cannot be failures of constitutional *theory* but have to be failures of the constitutional *system*, that is, of the scientific constructs that build the basis of constitutional theory and that are given in the more abstract regions of the empirical material Er. A neo-Kantian may believe that the scientist provides us only with the rough conceptual material whose very *objective content* or even whose very *formal structure* has to be sifted out by the philosopher. Carnap left no doubt that this is "the task of the special sciences".[36] For philosophy, there remains only (1) the (rather insubstantial) task of metaphysical

continually revised as we progress through the hierarchy."

35 Cf. Cassirer (1994 [1910], 152ff and 292ff)

36 § 21

"essence problems" and (2) the purely formal task of constitutional theory (that was seen by Carnap as his own task). Carnap concludes:

> We have repeatedly pointed out that the formation of the constitutional *system* as a whole is a task of the whole of science ["Gesamtwissenschaft"], while constitutional *theory* is merely engaged in carrying out the appropriate logical investigations. (§ 179, my italics)[37]

Thus, in Carnap's conception, the task for "pure" philosophy is significantly *narrower* than in the neo-Kantian conception of the Marburg school:

	to sift out the objective content of concepts	to formally reconstruct the objective formal structure of concepts
according to the Marburg school of neo-Kantianism	is at least partly a task for philosophy	is a task for philosophy
according to Carnap	is an exclusive task for the sciences	is a task for philosophy

This implies in particular that the failure of the reduction of physical objects to phenomenal objects in the context of the constitutional system of the *Aufbau* is not at all a failure of constitutional *theory* in itself but a failure of the concrete constitutional *system* that is (re)constructed in the context of (part IV of) the *Aufbau*. Whereas the former is an exclusive task for philosophy, the latter is a much more general task, including the work of every particular science. Thus, the failure of reduction of physical objects to autopsychological objects in the *Aufbau* is a failure of the picture that it provides from the sciences and/or a failure of the picture that sciences themselves provide to the author of the *Aufbau*. But (as long as the logic that constitutional theory uses is consistent in itself and allows us to reconstruct every aspect of the empirical source) it cannot be a failure of the principal layout of constitutional *theory*.

If that diagnosis is correct, this would have a further consequence that is absolutely desirable. The neo-Kantian interpretations of the *Aufbau* imply that there is a significant amount of conventional decisions that are not provided on the object level of the particular sciences but have to take place on the meta-level, so to speak, in the philosophical laboratory in which the constructor of the constitutional system does his or her work. This neo-Kantian attitude establishes a particular kind of

37 Rolf A. George translates "Gesamtwissenschaft" as "unified science". However, I think that "Einheitswissenschaft" ("unified science") and "Gesamtwissenschaft" ("the whole of science") are something totally different. In particular, the term "unified science" may suggest here (quite incorrectly) that "Gesamtwissenschaft" is something constructed by philosophy and not by the sciences in themselves.

apriorism that – although relative in its nature – prevents epistemology from adding to its formal story another story that is *empirical* in a much more immediate sense than a "history of sciences" as based on relative apriorism ever can be.[38] The point is that in a "German" empiricist setting, we may analyze the conventional decisions (that are not decisions on the meta-level, in the philosophical laboratory, but decisions on the object level, in the real life of the scientists) *directly* by means of the methods of history, psychology, biology, and sociology. In contrast, in neo-Kantian conceptions, the typical way to study the historical dynamic of theoretical conventions and ideas is a quasi-Kantian conceptual analysis that does not employ the methods of the human sciences. Thus, a more Diltheyian account of a structural theory of knowledge is *complementary* to a naturalist account of the sciences, whereas in the neo-Kantian picture, naturalizations seem to be rather unnecessary additions to something that is a genuine task for the pure reasoning of the philosopher.

I think that it is an important and not sufficiently appreciated aspect of the rejection of neo-Kantianism as we can find it in Carnap's *Aufbau* that it implies exactly such a Diltheyian picture of a structural theory of knowledge that is *complementary* to a naturalistic understanding of knowledge. This picture in general and Carnap's rejection of neo-Kantianism in particular *only* fail (in the sense of Friedman's argumentation) if we take the conventional decisions that lead to the specification of abstract scientific concepts to be something that has to be done on the meta level of constitutional *theory*; however, if those decisions are something that we already find on the object level of the constitutional *system* (and I have argued that this was indeed Carnap's idea), then there is no need at all for a return to (neo-)Kantian ideas because we may replace aprioristic reasoning on the meta-level (as it is demanded by neo-Kantianism) with history and sociology of science on the object level.[39]

This certainly does *not* imply an uncritical commitment to science studies and to the strong program in sociology of science because it is the crucial point of Carnap's account that the core element of rational reconstruction of the sciences is something purely formal *and therefore a priori*.[40] What we learn from Carnap is

38 Cf. Friedman (2001) who points out that even Thomas Kuhn's approach to the history of science in fact was neo-Kantian in the sense just mentioned.

39 Cf. again Carnap's statement in his letter to Neurath as quoted on page 2, above.

40 Cf. the "strong program" in Bloor (1991 [1976], p. 7ff) that seems to totalize the sociological standpoint in such a way that particularly a *sociological purism* is implied that carefully avoids leaving the sociological meta-level. We can find a similar attitude in the "science studies" of Latour & Woolgar (1986 [1979]) and even in the historical epistemology of Daston & Galison (2007). – In general, the classical approaches in that field would not at all argue in favor of a *complementarity* between rational reconstruction as a formal task and empirical reconstruction as a task for history, psychology, and sociology of science but rather would claim that these two aspects of the sciences are something *completely different*, something that we should carefully avoid confusing, and not at all something that we may have *to combine* in one or another way.

rather that a sociological approach to a rational reconstruction of the sciences *only* makes sense if it is embedded in a formal framework that allows us to reconstruct the underlying formal structures of the empirical elements that sociology and history provides to the philosopher. Again, this may be seen as an example were Carnap is sort of a "Kantian in the broadest sense" because even if the Kantian picture of a total difference of the empirical and the conceptual side of reasoning is false, this does not change the fundamental truth of Kant's observation that there always is *both* an empirical and a conceptual and a sensual and a structural *aspect* in reasoning and that a theory of reasoning that completely leaves one of these aspects must be either "blind" or "empty."

One finally may object here that even the formal framework may be something that must have an empirical motivation and that Carnap in the *Aufbau* completely fails to provide anything like that. Indeed, if my understanding of the *Aufbau* is correct, then *the only* conventional element that remains for the meta-level of constitutional *theory* is the question of the choice of the *formal framework.* It is true that in *this* respect, the *Aufbau* completely fails to provide sufficient motivation. However, the reason for this "failure" is obviously given by the fact that Carnap, at the time when he wrote the *Aufbau,* thought that there ultimately is only one possible system of pure logic that has eternal validity and thus cannot be questioned at all by the philosopher. Therefore, Carnap thought at this time that there is no need at all for a motivation of the "formal framework" (i.e., Russell's theory of types) that is used by constitutional theory. However, if we reject that picture and consider a plurality of logics (as it was done by Carnap from his Viennese period onward[41]), then the motivation of theoretical decisions becomes a question even on the meta-level of constitutional *theory*. Even in that case, if we consider a plurality of logics and, possibly, even some empirical motivation for and against some of the frameworks thus considered, this would not change at all the *complementarity* of the formal and the sociological approach because *in no case* (neither on the object level of the constitutional system nor on the meta level of constitutional theory), the presence of a sociological reconstruction dispenses us from the task of a purely *rational* (and therefore *logical*) reconstruction. We neither reduce the formal side of the sciences to the sociological nor the sociological to the formal – a *complete* picture can only be obtained if we consider *both*. This is something fundamentally different to everything that was claimed by the rather purist accounts of science studies and sociology of scientific knowledge.

To conclude, I do not want to argue here for an *assimilation* of the *Aufbau* to sociology of science but rather for the claim that the *incomplete* formalist story of the *Aufbau* may find its *completion* by means of sociology, psychology, and history of science. A *complementarity* of that kind is quite typical for Dilthey and his "German" empiricist attitude. Moreover, there *is* a Diltheyian background in

41 Cf. the "principle of tolerance in syntax" in Carnap (1968 [1934], § 17)

Carnap's early philosophy. These two facts, one systematic, and the other historical, make it very likely that Carnap already had in mind a complementarity between a formal and a historical/sociological approach to the sciences, an approach that rules out every form of synthetic a priori. Carnap seems to have embraced this, not only in 1928 under the influence of Otto Neurath, but already at the time when he worked on the *Aufbau*, under the influence of the Dilthey school.

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Carnap's Encounter with Pragmatism

1. Introduction[1]

Logical empiricism and pragmatism shared an empiricist orientation, a close interest in the sciences and their methods, and skepticism about propositions which cannot be empirically tested or verified. Both movements came into direct contact in the first half of the 1930s, shortly after the beginning of the so-called public phase of logical empiricism (after 1929). Around 1930, Schlick and Feigl went to the United States and philosophers in the pragmatist tradition began to pay attention to the new Viennese movement. Only with the rise of this mutual interest did Carnap become acquainted with pragmatism. Contrary to other logical empiricists (Schlick, Neurath, Ph. Frank), there are almost no traces in Carnap's earlier philosophy of an interest for pragmatism. We will focus here on the historic episode of Carnap's encounter with pragmatism. This will permit to clear more general claims about the relation of logical empiricism and pragmatism. We can find contradictory claims on this relation in the recent literature on the history of philosophy of science and of analytic philosophy. On the one hand the differences and conflicts between logical empiricism and pragmatism are emphasized and the progressive divergence between these two movements is pointed out.[2] On the other hand the literature points out the pragmatic elements in Carnap's philosophy which facilitated a convergence with pragmatism.[3] First, we claim here that in the 1930s it is the convergence between pragmatism and logical empiricism that was prevailing and that it found its expression in Carnap's support of a "scientific

1 This paper is a short version of a much longer essay on Carnap's relation to the philosophy in America between the 1920s and 1942, published in German: „Rudolf Carnap und die Philosophie in Amerika. Logischer Empirismus, Pragmatismus, Realismus", in: Stadler (2010), p. 85-164. I want to thank Hans-Joachim Dahms for the many conversations on this topic as well as the members of the FWF-project („Vertreibung und Rückkehr der Wissenschaftstheorie: Carnap und Stegmüller") in which this work was developed: Christian Damböck, Allan Janik, Adelheid König-Porstner, Michael Schorner, Friedrich Stadler. And without the help of Brigitte Parakenings from the "Philosophischen Archiv" at the University of Konstanz this research would have been impossible.

2 Giere (1996). Particularly the differences on ethics is seen as a reason for strong divisions between pragmatism and logical empiricism, see Mormann (2007) and also Reisch (2005).

3 Richardson (2003) und Dahms (1997) emphasize the commonalities and the convergence between Carnap and pragmatism.

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empiricism" as conceived by the pragmatist Charles Morris. The strong impetus for an internationalization of scientific philosophy in the Unity of Science movement placed the project of an alliance of different empiricist movements at the forefront. Secondly we claim that Carnap's convergence with pragmatism is due to a liberalization of empiricism which took already place in the Vienna Circle, independently of any direct pragmatist influence. With this liberalization of empiricism the convergence with pragmatism became much easier than it would have been with an empiricism as defended in Carnap's *Aufbau*. We will show in a first section that Carnap's encounter with pragmatism was initiated by the pragmatist's criticism of verificationism and of the empricism of the *Aufbau*. In a second section we will describe Carnap's encounter with philosophers of the pragmatist tradition and his attempt to convince them that the pragmatist's criticism did not apply any more to his new position of the early 1930s (as developed in the protocol sentence debate). As response some pragmatists (Morris, Nagel) proposed models for a convergence of logical empiricism with pragmatism.[4] In a final section we will show that in Carnap's response to the pragmatists, he supported such a convergence.

2. Pragmatist Criticism of Verificationism

The first substantial confrontation of American philosophers with the positions of logical empiricism, and especially with Carnap, can be found in papers and talks by Lewis, Nagel and Morris in the years 1933-34. These reactions give an evaluation of the conception of meaning proposed by the logical empiricists. They analyze how logical empiricism determines the empirical meaning of statements and which criteria they provide to distinguish meaningful from meaningless sentences. It is not surprising that the American philosophers who reacted to this central problem of logical empiricism where closely connected to pragmatism. For pragmatism, as it was originally conceived by Peirce, an empirical meaning criterion for expressions was absolutely central (such a criterion was formulated by Peirce in his so called "pragmatic maxim"[5]). In pragmatism, there is a long tradition of discussions on verification, on the empirical conditions of meaning and on the method by which meaning can be determined through empirical tests. Especially the papers by Lewis ("Experience and Meaning") and Nagel ("Verifiability, Truth and Verification"), but also those by Morris ("Pragmatism and Metaphysics" and

4 Although Nagel did not declare himself a pragmatist, he is connected to the pragmatic tradition through his teacher John Dewey and he began to publish on topics which were central to pragmatism: the analysis of the scientific method, verification and confirmation. For this closeness to the pragmatic tradition, we consider him here as well as Quine, although both did not call themselves pragmatists, contrary to the declared pragmatists Dewey, Lewis and Morris.

5 Peirce stated this maxim for the first time in his paper "How to Make Our Ideas Clear" (1878).This paper is generally considered as the origin of pragmatism.

"The Concept of Meaning in Pragmatism and Logical Empiricism")[6] discuss and criticize the analysis of meaning of the logical empiricists. All these papers insist on the danger of a too narrow criterion for empirical meaning. They point out that the meaning of a sentence cannot be identified with its truth. The meaning must be distinguished from the process of verification; verification does not indicate the meaning of a statement, but only shows if the statement is true.[7] All the papers criticize that the meaning cannot be reduced to the immediately given. As background for the critical arguments by Lewis and Morris, there is also the fear that a too restrictive meaning criterion might transform some areas of philosophy they took as essential into meaningless topics. These areas include ethics, certain forms of inductive metaphysics and the metaphysical questions of the reality of the external world and of other minds.

The criticism of Lewis, Nagel, and Morris focused mainly on Carnap's *Aufbau* and on the *Manifesto* of the Vienna Circle.[8] The subsequent development of logical empiricism with the protocol sentence debate, physicalism and the unity of science was not addressed in this first pragmatist criticism of logical empiricism. Certainly, this can be explained by the first presentation of logical positivism in America by Feigl and Blumberg (1931), where these developments were not mentioned. Because of the significant change of Carnap's positions in the first years of the 1930s, the American philosophers attacked positions in their 1933/34 criticism that Carnap did not hold any more. Lewis complained for this reason, that criticizing logical empiricism was like shooting at a moving target.[9] With the allegation of a too narrow meaning criterion, the pragmatists attacked essentially the following two theses:

(1) methodological solipsism, which asks for a verification in one's own experience. Lewis opposes that thesis and emphasizes that there are several degrees of verifiability, which cannot all be reduced to direct subjective experience.[10] Morris opposes the thesis on social grounds, emphasizing the social nature of verification. The process of verification takes place inside a community of investigators and therefore cannot be reduced to individual subjective experience.

(2) complete verification through atomic propositions. Opposing that claim, Nagel formulates a holistic view of verification in which one sentence cannot be verified independently of others and where verification is never complete.

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6 Lewis (1934), Nagel (1934), Morris (1934) and (1937a). All but the last of these papers are based on talks given in the United States in 1933. The last paper is already a product of a direct and personal interaction between pragmatism and logical empiricism. The paper is based on a talk given by Morris at the 8th International Congress of Philosophy in Prague (1934).

7 Lewis (1934), Morris (1934).

8 *Wissenschaftliche Weltauffassung. Der Wiener Kreis* (1929).

9 Lewis (1941), 93.

10 Lewis (1934).

Of particular importance for the pragmatist's criticism is "Experience and Meaning", originally the "presidential address" given by Lewis at the 1933 APA Meeting. Schlick responded to it with his "Meaning and Verification" and Carnap planned also a reply, which was later on integrated into the much broader treatment in "Testability and Meaning".[11] On the one hand, Lewis pointed out the similarities between logical empiricism and pragmatism, because both demand for an empirical meaning criterion. Both require that the empirical conditions be given, which specify when a concept is to be applied. But on the other hand, Lewis objects to logical empiricism, because it has a much too narrow conception of the meaning criterion, a fact he attributes mainly to Carnap's adoption of methodological solipsism. This position requires that the meaning of a sentence is ultimately reduced to the actual "autopsychological basis" and considers any knowledge as a construction out of such autopsychological data. To expose the negative consequences of methodological solipsism, Lewis pushes this position to its limit, in a way Carnap never endorsed. For Lewis, Carnap's position leads eventually to an idealistic phenomenalism, close to that of Berkeley. Certainly this criticism is exaggerated, but its purpose is to insist on two points, which are essential for Lewis: (1) the meaning of an expression is to be distinguished from immediate experience. For Lewis, the meaning consists in the expectation of a future experience, not in the actual experience: "the *intention* to refer to what transcends immediate experience is of the essence of knowledge and meaning both".[12] (2) Knowledge is composed of two moments, which are not sufficiently separated in logical empiricism: a statement as prediction or expectation and an empirical verification of that statement. For Lewis a statement has also a meaning, if it cannot be verified in actual experience. But Lewis rejected statements for which no possible verification can be imagined. Statements must be verifiable in principle. Lewis undertook for this reason an analysis of different degrees of verifiability, which contradicted the too strong requirement of definitive verification in logical empiricism. He distinguished the following degrees:

(a) Expressions about directly observables

(b) Expressions about entities which are in principle observable, although for technical reasons the observation is not possible.

(c) Expressions about inferred entities, which are not observable (e.g. electrons)

(d) Expressions which refer and have an empirical meaning, although they are not verifiable (expressions about immortality). With such expressions we can imagine what would be the case if they were true.

11 Carnap wrote a draft for a reply to Lewis and sent it to different philosophers (e.g. to Schlick and Morris).

12 Lewis (1934), 264.

In *Scheinprobleme in der Philosophie* (1928) Carnap had already distinguished different degrees of verifiability and responds in "Testability and Meaning" to Lewis' distinctions with an analysis of different degrees of confirmation. Criticizing the thesis (2), Nagel questioned the possibility of a verification of single statements at the same APA conference, in his paper "Verifiability, Truth and Verification". Nagel anticipates Quine's holistic arguments against the verification of single statements, arguments which recur frequently among pragmatist philosophers (e.g. Peirce, Lewis). Nagel criticized particularly the idea of atomic propositions and indicated that the verification of one sentence requires the acceptance of other sentences. In the sciences, the test of an expression presupposes the knowledge about other facts: "any verifying process has evidential value only *within* a framework of pre-existing knowledge".[13] Because of the interconnectedness of the different expressions, the meaning of one of them cannot be given by a single verification. Nagel also rejects the idea that an actual verification can be definitive. In his criticism he combined holism with pragmatic fallibilism. As in the case of Lewis' criticism, it can be doubted that Nagel's criticism played a role in Carnap's philosophic evolution. Carnap anticipated in the protocoll-sentence debate holistic and fallibilistic arguments and formulated them explicitly in the *Logical Syntax*.[14]

Some philosophers (e.g. Hempel) indicated, that logical empiricism was liberalized because of the criticism by American philosophers and replaced the narrow conception of verification by a more liberal conception of degrees of confirmation und testability. Although Carnap's conception went through such a liberalization and although pragmatists pointed out the difficulties of a too narrow conception of verificationism, the liberalization did not take place because of the pragmatist's critique. Rather liberalization of the empirical meaning criterion already took place in Vienna during the protocol sentence debate. Although Carnap described this process of liberalization in his autobiography, he did not mention in this con-

13 Nagel (1934), 143.

14 See *Logical Syntax of Language*, § 82, p. 318. Concerning the fallibilistic argument: For Carnap, there is no definitive confirmation of hypotheses. He writes "Still less is there in a strict sense a complete confirmation (verification) of an hypothesis. When an increasing number of L-consequences of the hypothesis agree with the already acknowledged protocol-sentences, then the hypothesis is increasingly confirmed; there is accordingly only a gradual increasing, but never a final, confirmation." Also concrete protocol sentences must be considered as hypotheses "Not only laws, however, but also concrete sentences are formulated as hypotheses". For the holistic argument that no expression can be verified in isolation: "Further, it is in general, impossible to test even a single hypothetical sentence. In the case of a single sentence of this kind, there are in general no suitable L-consequences of the form of protocol-sentences, hence for the deduction of sentences having the form of protocol-sentences the remaining hypotheses must also be used. Thus *the test applies, at bottom, not to a single hypothesis but to the whole system of physics as a system of hypotheses* (Duhem, Poincaré)" (all quotes, p. 318 of the English translation. All these quotes are already present in the german edition of 1934). Carnap's *Logical Syntax* was already finished at the time of Nagel's and Lewis's talk (at the end of 1933) and was published in 1934.

nection the pragmatist critique, but situated the liberalization in the internal debates of the Vienna Circle. As a consequence. Carnap no longer held the position criticized by the pragmatists, and, as we shall see, he accepted and welcomed their critique of his earlier position.[15]

Despite the pragmatist's critique of 1933/34 and the apparent strong difference between logical empiricism and pragmatism, Carnap and the pragmatists actually recognized their mutual agreement when they first met. This convergence cannot be explained by the pragmatist's critique. For chronological reasons, this critique cannot explain the turn to a liberalized form of logical empiricism, rather this turn must be explained by the internal development in the Vienna Circle, and especially in the so called "left wing" of the Circle.[16] But the pragmatists certainly reinforced the turn to a more liberal empiricism and Carnap explicitly referred to the criticism by Lewis, Nagel and Morris in "Testability and Meaning" in order to show that a liberalized form of logical empiricism was necessary.

3. Two Models of Convergence

The early American reception triggered and enforced an interaction between logical empiricists and pragmatists already some years before Carnap's emigration. This reception stimulated direct contact between American philosophers and Carnap. These contacts began with Quine's presence in Vienna and Prague and were followed by the trips of Morris and Nagel to Prague and Vienna. A central event during this encounter of logical empiricism with American philosophers was the 8th International Congress for Philosophy in Prague in September 1934, where Nagel and Morris gave talks. After the first theoretical confrontation between the European and the American tendencies of empiricism we previously described, the congress gave the occasion for the first attempts of a fusion between the two tendencies. Through his contacts with logical empiricists at the Prague congress, Morris suggested such a fusion under the label "scientific empiricism". Nagel described after the congress a convergence of different tendencies into a movement he called "analytic philosophy". With these labels, it was intended to connect logical empiricism with other philosophical movements like pragmatism (and the Polish Lemberg-Warsaw school). As the correspondence of Carnap with Morris and Nagel shows, Carnap was instrumental in creating these projects of fusion and convergence.

This interaction with American philosophers is often reduced to the emblematic encounter between Carnap and Quine. This focus corresponds perhaps to

15 See §4 of this paper.

16 Nagel used expression "left wing" alternatively with the expresseion "the Carnap wing" in (1936), 40.

the philosophical importance Quine acquired since the 1950s. But in the 1930s, Quine was one among others, who were important for the rapprochement between American philosophers and logical empiricists and for the emigration of Carnap. Besides Quine’s stay in Vienna and Prague in winter 1932/33, the stays of Morris (summer and autumn 1934) and Nagel (autumn and winter 1934/35) were also significant for this rapprochement.

Animated by Feigl, Quine visited Vienna in autumn 1932, listened there to the lectures of Schlick and presented a summary of his dissertation in the Vienna Circle.[17] In March 1933 Quine came to visit Carnap in Prague. In his autobiography Quine writes with enthusiasm about his encounter with Carnap and his “intellectual *Wiedergeburt* in Prague”[18]: “I eagerly attended Carnap’s lectures. He expounded his *Logische Syntax der Sprache*, which Ina [Carnap’s wife] was typing. Carnap lent me the typescript sheet by sheet” and “It was my first experience of sustained intellectual engagement with anyone of an older generation, let alone a great man. It was my most notable experience of being intellectually fired by a living teacher rather than a book”.[19] After his return to Harvard, Quine wrote in 1934 a review of *Logical Syntax* and gave three lectures on the book[20]: “At any rate, there was a stir about Carnap after my three lectures. Professor Prall, Sheffer, and Lewis were curious and met with me repeatedly to pose questions”.[21] In the following year (1935), Quine gave a seminar on *Logical Syntax* in the presence of Nelson Goodman and Charles Stevenson.[22] Thus Quine played an essential role in Harvard to make Carnap’s position known.

More important for the rapprochement between pragmatism and logical empiricism and also for Carnap’s plans to emigrate to the United States was his encounter with Charles Morris. Morris studied in Chicago under George Herbert Mead and had a professorship at the Chicago philosophy department from 1931 on. This department was established and built by Dewey and Mead and was a stronghold of pragmatism until 1930. But the department changed radically its philosophic orientation due to a conservative turn in the academic policy of the university. Most of the pragmatists left the department and Morris was therefore an outsider in his department. Through his relations with the logical positivists, he

17 Quine (1985), 93-96. Concerning Quine’s relation to the Vienna Circle, see Creath (2007).

18 Quine (1985), 111.

19 Quine (1985). Besides a course on “Logic for advanced students” (“Logik für Fortgeschrittene”) and a seminar on logic given in the summer semester 1933, Carnap gave also a course on “Critical History of Modern Philosophy” (“Kritische Geschichte der Philosophie der Neuzeit”), see *Vorlesungsverzeichis der Deutschen Universität Prag*, *Sommersemester* 1933, 48.

20 Published by Creath (1990), 47-103.

21 Quine (1985), 117.

22 1937 Quine gave again a course “on logical positivism, primarily on Carnap”, Quine (1985), 130.

tried with success to break that isolation. Morris tried to combine the two tendencies of logical empiricism and pragmatism, although to some extent in an eclectic way. He tried to get logical empiricists to Chicago to reinforce his position in the department. Morris was successful in this attempt by getting Carnap to Chicago and also briefly Hempel at the end of the 30s. Hence Morris was a central figure for the emigration of Carnap.

As in the case of Quine, Morris was animated by Feigl to get into contact with the Vienna Circle. Thus he wrote to Carnap and Schlick, that he wanted to come to Vienna. To Carnap he wrote at the end of 1933:

> Meantime you have certainly heard from our common friend Mr. Feigl, so my request will not surprise you. In my opinion, pragmatism (Peirce, Dewey, Mead) and logical positivism are complementary movements. I hope my trip will help me to increase my knowledge of European empiricism. On the other hand I want to tell you about the development of pragmatism …[23]

Carnap responds:

> It would interest me strongly to talk with you on the problems of symbolism and empiricism, to be informed by you about the development of pragmatism, and to clear in a common reflection the kinship between pragmatism and logical empiricism.[24]

Thereupon Carnap invited Morris for a talk at the pre-conference, organized by the logical empiricists before the International Congress for Philosophy in Prague. Carnap writes to Neurath:

> I invited Prof. Morris from the University of Chicago to participate to the conference. At the main congress he talks about pragmatism-positivism. I proposed to him to talk at the conference on the same topic. Feigl wrote about him that he was among all Americans the closest to us: other Americans can only be taken in consideration insofar as they will come to the main congress.[25]

Morris reacted very positively to the invitation and tried to mediate between the pragmatists interested in logical empiricism and Carnap. He responds to Carnap's invitation:

> It would please me very much to attend the logical positivism meeting and take some part in it – perhaps in presenting something of present-day pragmatism, perhaps in discussing the way your Viennese group appears to our eyes. (…) I look forward with pleasure to meeting you and your group. Someday I hope you can pay us a visit (with lecture) in America, and especially at Chicago University. You will find genuine interest in your work. When I left New York Dewey was reading with great care your Philosophy of Science article. I trust

23 Morris to Carnap, 11/12/1933 [RC 029-04-25]. My translation from German.

24 Carnap to Morris, 11/23/1933, [RC 029-04-25].

25 Carnap to Neurath, 4/13/1934 [Neurath-Papers Nr. 219].

you have seen Lewis' article dealing with your views in the recent Philosophical Review – I hope to discuss this with you.[26]

At the Prague pre-conference, Morris was the only American to give a talk and fulfilled there the function of a mediator between the empiricist movements in Europe and America. He also discussed with Carnap, which other American philosophers should be invited to the pre-conference (particularly Ernest Nagel).[27] A note published in *Erkenntnis* states that the aim of the pre-conference was to enhance the acquaintance among those philosophers, who were interested in "an association of the different currents, who defend an *antimetaphysical empiricism*"[28]. The first talk at the pre-conference was given by Morris. The aim of the talk with the title "Scientific Empiricism" ("Wissenschaftlicher Empirismus") was to show the similarities between Vienna Circle and American pragmatism. Carnap gave an introduction to the talk.[29] In a letter to Carnap, Neurath summed up Carnap's introductory words: "the content was, that we have only to adjust our different formulations."[30] Carnap apparently already expressed then his position, which he formulated in "Testability and Meaning". There Carnap formulated the aim of his long paper in the following way:

> It aims rather to stimulate further investigation by supplying more exact definitions and formulations, and thereby to make it possible for others to state their different views more clearly for the purposes of fruitful discussion. Only in this way may we hope to develop convergent views and so approach the objective of *scientific empiricism* as a movement comprehending all related groups, – the development of an increasingly scientific philosophy.[31]

The label "scientific empiricism" was increasingly used in the 30s for the cooperation of different empiricist movements.[32] Later, in a dictionary entry to "scientific empiricism", Carnap identified this term with the "Unity of Science movement". Besides the European groups (Polish school, "Cambridge school of analytic philosophy", Berlin group), Carnap included into this movement also pragmatism,

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26 Morris to Carnap, 5/6/1934, [RC 029-04-21]. The mentioned "Philosophy of Science article" is Carnap's "On the Character of Philosophical Problems". Dewey published in *Philosophy of Science* a short reaction to Carnap's paper, see Dewey (1934). The mentioned article by Lewis is "Experience and Verification".

27 Carnap to Neurath, 8/17/1934 [Neurath-Papers Nr. 219].

28 "Prager Vorkonferenz der internationalen Kongresse für Einheit der Wissenschaft – 1934", *Erkenntnis* 5, 1935, 1.

29 See Neurath to Carnap, 10/26/1934 [Neurath-Papers Nr. 219]

30 Ibid.

31 Carnap (1936/37), second part, 38.

32 Besides Carnap (1936/37 and 1942) Morris (1937b), and Feigl (1943), Neurath also used this name in his paper „Die neue Enzyklopädie des wissenschaftlichen Empirismus" (1937).

American neo-realism and operationalism.[33] Feigl expressed a similar view in his paper "Logical empiricism".[34] In a historical note to his paper, Feigl describes the transformation of the Vienna Circle in the context of the " 'Logical (or Scientific) Empiricism' and the 'Unity of Science' project", underlining the "confluence of the European and American movements". For him the American movements derived from "Peirce, James, Dewey, Mead and Bridgman".[35] Although the Prague pre-conference attempted an association of the Vienna Circle and the Berlin group with philosophic movements from Poland, France, Scandinavia, England and America, due to WWII only the cooperation with the Americans seems to have succeeded on a broad scale.

After the Prague conference, Morris was strongly anchored in the camp of the logical empiricists and his initial resistance and opposition was transformed into a cooperation and suggestions for improvements. The correspondence between Carnap and Morris shows a lively exchange of papers and publications from summer 1934 onward.[36] After the Prague pre-conference, Morris was also a co-organizer of the congresses for the Unity of Science and of the "Encyclopedia for Unified Science".

Through the Congress in Prague Carnap became also in contact with Nagel. A few months after the Congress, Nagel returned for some weeks to Prague.[37] In

33 Carnap (1942), 285-6.

34 Feigl concludes his review of logical empiricism with the following remarks: "we attempted to survey (…) the major ideas of the philosophical movement which was known at its inception as the Logical Positivism of the Vienna Circle, the ideas which have since developed into what is also called "Logical (or Scientific) Empiricism" or the "Unity of Science" project.", Feigl (1943), 406. The paper is published in a volume, *Twentieth Century Philosophy*, edited by Dagobert Runes, who also was the editor of the previously mentioned dictionary.

35 Feigl (1943), 406.

36 Carnap read the talk "Pragmatism and Metaphysics" given by Morris at the APA and parts of the book *Six Theories of Mind* by Morris, his paper "Pragmatism and the Crisis of Democracy", as well as the book by George Herbert Mead, edited by Morris, *Mind, Self and Society, from the standpoint of a social behaviorist* (1934). Unfortunately it is not clear why Morris used this term 'social behaviorism' in the title. This expression was not used by Mead but was frequently used by Neurath after 1932, see Neurath (1932). It is probable that Morris wanted to establish here a further link between pragmatism and the Vienna Circle. Morris was regularly in contact with Neurath. Concerning Mead's book, Carnap wrote to Morris: "My best thanks for your kind sending of the volume of Mead's Papers. I am especially interested in those which deal with language and symbols. The Publisher sent a review copy to 'Erkenntnis'. I shall look for a good reviewer." Later on Carnap wrote to Morris that Kurt Grelling will review the book, Carnap to Morris 6/26/1935, [RC 029-04-02]. In *Erkenntnis* no review of Mead's book can be found.

37 Although Nagel grew up and studied in New York, he was born in Nove Mesto (in today's Slovakia), a small town seventy miles outside of Vienna. His reports on the philosophy in Central Europe often show a much greater sensitivity and comprehension of the intellectual and social atmosphere than Morris does.

his correspondence with Carnap, he thanks him for the numerous discussions with him.[38] Nagel was present afterwards at meetings of the Vienna Circle and reported on them to Carnap.[39] In his report about the Prague International Congress of Philosophy, Nagel writes also on the pre-conference of the logical empiricists and about the talks by Carnap, Neurath and Reichenbach. He gives a positive comment on Morris' attempt of an association of logical analysis with pragmatism. On the Vienna Circle generally, he writes: "both as individuals and as a group they were to me the most interesting and vital philosophers present."[40] Contrary to these positive comments, Nagel writes mostly negatively on the other talks at the Congress: "a majority of the papers were simple occasions of despair to all those who do not view philosophy as a substitute for music and poetry as expressions of emotions. There was a woeful lack of clarity, of analysis, of appeal to logic and empirical findings."[41] Nagel contrasts this type of philosophy with the analytic philosophy, represented among others by the Vienna Circle. After a trip of almost one year in Europe and after his return to the United States, he writes a long philosophical report under the title "Impressions and Appraisals of Analytic Philosophy in Europe" and gives the following motivation for this report: "I think the portrait is worth painting, if only to show that a student of philosophy interested in analysis need not despair that a romantic irrationalism has completely engulfed Europe, and that he may find stimulus and direction among the men with whom I spent a good portion of a year."[42] By these men he ment philosophers from "Cambridge, Vienna, Prague, Warsaw and Lemberg". Besides Wittgenstein, the central part of this report on "analytic philosophy" is dedicated to Carnap. Nagel defines the "analytic philosophy" of these philosophers by four characteristics:

(1) Philosophy consists in analysis: "Their preoccupation is with philosophy as *analysis*. They take for granted a body of authentic knowledge acquired by the special sciences, and are concerned not with *adding* to it in the way research in these sciences adds to it, but with *clarifying* its meaning and implications."[43]

(2) The central aspect of "analytic philosophy" is the formulation of an analytic method and not the formulation of a doctrine or dogmas. Because of this absence of dogmatism, no answer receives a definitive and irrevocable answer and the answers of analytic philosophy are the products of a cooperation.

38 Ernest Nagel to Carnap, 12/12/1934, [RC 029-05-18].

39 Nagel was present at two meetings of the Vienna Circle, at the beginning of 1935. These meetings discussed Schlick's response to the criticism of Lewis. This response was published in *Philosophical Review* in 1936 as "Meaning and Verification", see Schlick (1936).

40 Nagel (1934b), 591.

41 Ibid., 599.

42 Nagel (1936), 5.

43 Ibid., 6.

(3) An "unhistorical attitude"[44]: History of philosophy, its "great" representatives as well as its traditional problems receive not much interest in analytic philosophy. The historical origin of doctrines and the sociological conditions of their development receive no attention.[45] The traditional problems and questions are mostly seen as pseudo-problems.

(4) A "common-sense naturalism": "They do not believe that the everyday world is an illusion, or that science or philosophy reveal a contrasting reality."[46]

Nagel saw similarities of "analytic philosophy" with some philosophical positions in the United States. Particularly concerning the fourth point, i.e. the agreement of analytic philosophy with naturalism. He writes: "any one brought up in the atmosphere of analytic naturalism will find himself very much at home intellectually at the places on which I am reporting."[47] On the other hand Nagel agreed with those, who accused the philosophers in America of a lack of analytic precision.

In the part of his report on Carnap and the Vienna Circle, Nagel emphasises the fact that the Circle is already well known in the States.[48] But Nagel wants to correct this American reception by insisting on a "significant shift" or a "radical transformation" inside of Viennese positivism. By this shift he means the transformation initiated by Neurath and Carnap in the protocol-sentence debate. For Nagel, many of the objections to logical empiricism must be attributed to the fact that this radical shift has not been acknowledged. We will see in the next section that many of the pragmatist objections to the Vienna Circle became pointless because of this radical shift, particularly the objections to methodological solipsism and to logical atomism. In his description Nagel draws a strong opposition between the earlier position of logical empiricism, still shared by the "right wing" of the Circle, and a later position, that of the "left wing". The example of Nagel shows clearly how the direct contact with Carnap could dispel many of the misunderstandings in the American reception of logical empiricism. Discussions with Carnap informed Nagel about the essential changes in the position of the Vienna Circle. Nagel returned to Prague two months after the International Congress for Philosophy, in November 1934. There he wrote to Carnap: "I hope you will have time to see me and discuss with me some of the problems of logic and methodology."[49] Carnap and Nagel met for several discussions. After leaving Prague for Vienna, Nagel thanked Carnap:

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44 Ibid., 6.

45 Ibid., 7.

46 Ibid., 7.

47 Ibid., 10.

48 Nagel writes: "The *Wiener Kreis* has propagated its doctrines to all parts of the world with far too great a missionary zeal to require more than a perfunctory introduction for an American audience." (1936), II, 29.

49 Nagel to Carnap, 11/10/1934, [RC 029-05-21].

I tell you what a great experience it has been for me to get to know you, how much I have enjoyed seeing and talking to you, and what a precious memory the month I have spent in Prague will remain for me. I have tweaks of conscience at the thought of how much of your time I wasted, but your kindness was irresistible. I hope, too, that not all your effort on me was wasted, for I think I have learned more than may appear on the surface from the many conversations I had with you.[50]

Apparently Nagel noticed after these discussions that logical empiricism had become more compatible with pragmatist positions and that the many points of the pragmatic criticism does not apply any more to the renewed position of logical empiricism. Here Nagel differentiated clearly between the position of Carnap and the "left wing" on the one hand, and the position of Schlick on the other hand. He supported the first position and held the second to be untenable.[51]

After these meetings with Carnap, Nagel stayed in regular contact with him and supported Carnap in his search for a position in America. He helped Carnap also in his corrections for the English edition of the *Logical Syntax.* He published frequently reviews of the writings of Carnap and other logical empiricists.[52]

For Carnap the acquaintance with Quine, Nagel and Morris had the following consequences: (1) his reception in America was enhanced through reviews, papers, talks and courses given by these American philosophers. (2) The fusion of logical empiricism and pragmatism developed by Nagel and Morris could be used by Carnap to connect his own philosophical agenda with the projects pursued in America. (3) Through Quine, Morris and Nagel, and also Lewis, Carnap could realize on an academic level his plans to emigrate to the United States. In an interview given 1965, Carnap described the association between logical empiricism and pragmatism, which began in Prague, as well as the development of analytic philosophy:

Yes, I think, we must say that in Europe the development of the thoughts of the "Vienna Circle" were fatally interrupted (…); but through the connection with other ways of thinking in countries like England and America, our ideas were developed further, than would have been the case – this at least is my supposition –, if the "Vienna Circle" had continued to exist in Vienna.[53]

50 Nagel to Carnap, 12/12/1934, [RC 029-05-18].

51 See Nagel to Carnap, 7/17/1935, [RC 029-08-04]: "I have read Hempel's reply to Schlick with great interest and delight. He seems to me to have made the matter very clear, and I do not see what there remains for Schlick except to be convinced of his errors." The mentioned paper by Hempel is "Some Remarks on 'Facts' and 'Propositions'" (1935), in which Hempel defends the position of the "left wing" against Schlick.

52 In the *Journal of Philosophy* Nagel wrote reviews of Carnap's *Logical Syntax* and *Philosophy and Logical Syntax* and later on regularly reviews of Carnap's English publications.

53 Carnap (1967), 51. The interview was given in 1965.

Carnap particularly insisted on the role of pragmatism and described its connection with the philosophy of the Vienna Circle in the following way:

> I think for example that the overemphasis of the purely intellectual, as it appeared sometimes in our case, was brought into an equilibrium. Thus we saw the human being also more clearly from a biological and sociological point of view. This collaboration with the efforts overseas then lead to a movement, which properly has no name. There are many philosophers, for example Ernest Nagel, who claim that they do not follow any -ism and are not part of any movement. Nagel and some others come from pragmatism or from realism and have assimilated the ideas of logical empiricism. Thus there developed a big movement – sometimes it is simply called "analytic philosophy" –, which is the main characteristic of the philosophical situation in the United States now.[54]

Besides the direct contact with the younger philosophers close to pragmatism, there was also an indirect contact with the older generation of pragmatists, particularly with C. I. Lewis and Dewey. The intellectual confrontation with Lewis will be analyzed in the next section. It was essential for the pragmatist reaction to logical empiricism and also for the genesis of Carnap's "Testability and Meaning". Feigl was already around 1930 a mediator between Lewis and the logical empiricists, but a direct epistolary contact with Carnap was initiated through Quine from 1933 on.[55] In the case of Dewey, it was Morris who established a contact with his remark to Carnap that Dewey was very interested in his first American paper "On the Character of Philosophic Problems".[56] Thereupon, Carnap sent Dewey some other papers by him. A brief exchange on Carnap's paper between both philosophers was published in *Philosophy of Science.*[57] Contrary to the previously described rapprochement between Carnap and the younger generation of pragmatists, in the case of the older pragmatists Lewis and Dewey, an initial convergence was followed by strong divergences, although in the two cases for different reasons.[58]

54 Ibid. 51-52.

55 Carnap to Quine, 4/30/1933, see Creath (1990), 116.

56 But this was not the first contact between Dewey and the logical empiricists. Dewey met already Schlick in Vienna in spring 1933. Although Schlick mentioned this meeting in letters, there are no sources about the content of their conversation, see Schlick to David Rynin, 4/11/1933, [Schlick-Papers, Nr. 114].

57 Dewey (1934) and Carnap (1934c).

58 Lewis never gave up his initial friendly opposition to the logical empiricism, but increasingly insisted on the divergences in the later years, see Lewis (1941). Dewey's cooperation with the logical empiricists in their encyclopedia project was followed in the 1940s by an increasing bitterness about the formalism of Carnap's philosophy, see Dewey (1949).

4. Carnap’s Reply in “Testability and Meaning”

In “Testability and Meaning” we find the first extensive reaction of Carnap to the pragmatic criticism of his position. This long paper can be considered as Carnap’s confrontation with the positions of pragmatism in the 30s. The paper was written during Carnap’s first intensive acquaintance with the empirically oriented philosophy in America.

Carnap distinguished at the Paris Congress for the Unity of Science (1935) between the confirmation of empirical sentences and the truth of sentences.[59] The definition of truth does not say if a sentence is true and which criteria should be applied to test its truth. Confirmation is concerned with the criteria which permit to test an empirical sentence. “Testability and Meaning” is exclusively concerned with this second question of confirmation. After the internal and external criticism of his conception on verification, Carnap attempts here a new formulation of the criteria for testability and confirmation of empirical sentences. He draws the consequences of the protocol sentence debate, rejects explicitly his early conception of verification and formulates a liberalized conception of empiricism. The internal liberalization of empiricism in the Vienna Circle made a convergence with pragmatism possible, although this liberalization already happened before Carnap’s encounter with pragmatism. As noted, it is no coincidence that the discussion between logical empiricism and pragmatism began over the topic of verification and confirmation. That is different with the other topic, the question of a semantic concept of truth. With the semantic turn which became explicit in the case of Carnap at the end of the 1930s (although Carnap accepted already Tarski’s definition in 1935), he was the object of strong criticism from the part of some pragmatically oriented philosophers (Dewey, Nagel, Quine).

Still in Europe, Carnap began to work on “Testability and Meaning”. Carnap presented an early version of the paper a few days after his arrival in the United States in a talk at the APA meeting in Baltimore. In an extensive lecture tour in 1936, Carnap presented the paper before publishing it after revisions at the end of this same year. The beginning of “Testability and Meaning” which contains Carnap’s reaction to the criticism by Lewis, but also by Nagel, was originally intended as a separate response to Lewis. After Lewis had sent him his paper “Experience and Meaning”, Carnap wrote a response in may 1934, “Notes for a Reply to Lewis”, which he sent to Lewis, Schlick and Morris.[60] Carnap suggested to Schlick

59 Carnap, (1936).

60 Initially Carnap was not convinced of the usefulness of writing a reaction to Lewis, but was convinced by Feigl to write a detailed reaction to Lewis. Carnap wrote to Schlick (5/13/1934 [Schlick-Papers, Nr. 95]: “Lewis not only sent me his published paper, but already in February the corresponding manuscript, apparently with the desire that I should write a reply. Actually I did not really want to; but Feigl wrote me, that for my plans in America it would be of great importance, because I would need Lewis to realize them. He advised me to do that as extensively and as seriously as possible.”

to coordinate their replies to Lewis and wanted to concentrate on the question of empirical meaning and verification from a syntactic point of view. He suggested that Schlick focuses in his reply on the comparison between pragmatism and the Vienna Circle, because only Schlick had sufficient knowledge of the philosophy in America.[61] The suggestion of such a comparison came from Lewis, as Carnap wrote to Schlick:

> Lewis wrote me in his letter: "Some day I hope that some member of your group may publish a paper in which some comparison may be made between your views and the pragmatism of Peirce and James and Dewey. Of these three, James and Dewey lacked the system-building mind; and Peirce's work received less attention because most of it was not published during his lifetime. Nevertheless they represent the most vital philosophic interest which has been exerted in this country. And the background of their thought lies in the empiricism of Mach and J. S. Mill. Such a comparison would be of wide interest here." Perhaps you want to treat this somehow.[62]

In his response to Lewis, "Verification and Meaning"[63], Schlick did not include such as comparison between the two philosophic movements. As we already saw, this was done by Morris, who followed the suggestion of Carnap, at the pre-conference in Prague in 1934.

Carnap awaited Schlick's paper before working on his own reply to Lewis.[64] Schlick wrote his reply at the end of 1934 and sent it in February 1935 to Carnap. In april of the same year, Carnap told Schlick, that he works on a "paper for Lewis" he wanted to publish in the same journal where Schlick's "Verification and Meaning" would appear (i.e. *The Philosophical Review*).[65] As Carnap neither published in this journal, nor wrote a paper for Lewis, the material for this paper seems to have been integrated into "Testability and Meaning". In the year 1935-36 Carnap elaborated this long paper and presented first versions of it in America. After his lecture tour at different American Universities in Spring 1936, Carnap reworked the manuscript (in may and june 1936). During his visiting professorship at Harvard (in summer 1936) he discussed the paper in a group that included Feigl and Nagel among others. Thus, the paper could be thoroughly discussed with philosophers in America before publication.

In "Testability and Meaning" Carnap agrees with the pragmatists, that the criterion of verifiability was formulated in a much to narrow way. First, the criterion excluded even some common sentences of the sciences, e.g. laws of nature, which

61 See Carnap to Schlick, 5/13/1934, [Schlick-Papers, Nr. 95].

62 Ibid.

63 Schlick (1936).

64 Carnap to Schlick, 6/4/1934, [Schlick-Papers, Nr. 95]: "I suppose you will write me if and what you will reply to Lewis. I will wait until I see your manuscript and will complement it from my point of view. Morris also sent me a manuscript, which he probably sent you also. Perhaps we can refer to it at the same occasion."

65 Carnap to Schlick, 4/12/1934, [Schlick-Papers, Nr. 95].

cannot be definitively verified, because of their general nature. Second, Carnap even disagreed now with the claim that observation sentences could be definitively verified. Even from an observation sentence we can deduce infinitely many sentences, which must be tested to support the claim of the observation sentence. Here, Carnap agrees with the criticism by Lewis and Nagel, that an “absolute” verification is not possible, neither for general sentences, nor for sentences about particular facts (“particular sentences“). For Carnap the acceptance of any kind of sentence contains a conventional component and an objective component which is linked to observation. In no kind of sentences the conventional component can be completely eliminated. Thus, also the acceptance of an observation sentence contains a conventional aspect. Any synthetic sentence is a more or less confirmed hypothesis. This is the case for laws of nature, but as well for observation sentences. Carnap’s position is therefore compatible with fallibilism as the pragmatists defended it. Carnap noted that in this point there is agreement between him and the pragmatists.[66]

Instead of (absolute) verification, there are only different degrees of confirmation. It was the aim of Carnap’s paper to analyze the different criteria for the confirmation of empirical sentences, which were used in the different currents of empiricism. Carnap answered in a systematic way to Lewis’s analysis of the different criteria for the attribution of empirical meaning to sentences. But Carnap applied his principle of tolerance to the choice of a criterion for empirical meaning. For Carnap, we cannot find a “correct” criterion of empirical meaning, but we must analyze the different possible criteria and choose thereupon that criterion which is pragmatically the most fruitful for an empirical language. This choice is not a theoretical question, but a question of practical decision.

Carnap recognizes that there is a divergence between his earlier position, as formulated in the *Aufbau* and in *Scheinprobleme*, and the pragmatist’s view, but for him the difference disappeared when one considers the subsequent development of the Vienna Circle. Carnap underlines that this divergence with pragmatism still continues to exist if one considers the more “conservative wing” of the Vienna Circle[67], the position of Wittgenstein and Schlick. Apparently Carnap is refering here to Wittgenstein’s position on atomic sentences and Schlick’s theory of “Konstatierungen”, which were both rejected by the pragmatists.

66 Carnap (1936/37), 426.

67 Carnap (1936/37), 422. In his “Notes for a Reply to Lewis” Carnap had more explicitly mentioned the opposition between the “left” and the “right wing” of the Vienna Circle. But Schlick asked him to abandon this apparently politically connoted expressions. These labels for the two wings in the Circle were then taken over by Nagel in (1936). Nagel also emphasizes there, just as Carnap had, that the divergence between Vienna Circle and pragmatism after the physicalist turn in the “left wing” had diminished. Nagel himself initially agreed with the thesis of a strong divergence, but apparently abandoned this view after his encounter with Carnap.

But in Carnap's opinion there still persisted another divergence with pragmatism, at least if we consider the pragmatism of Lewis. Pragmatism ignores the distinction between the formal and the material mode of speech. Carnap attributes to this fact the major misunderstandings in the criticism of Lewis. The question in the Vienna Circle, about the reducibility of the language of science to a language about sense-data was misunderstood by Lewis as an ontological question. Therefore Lewis saw in the early verificationist position of Carnap a form of idealism. Lewis took the discussion about the choice of an empirical language for a "material" discussion about the fundamental epistemological entities. The question of the reduction of the concepts of science to a sense-data language was misunderstood by him as a question about the ultimate constituents of things. This confusion of the formal and the material mode of speech led Lewis to his view that Carnap defends an idealist position, to which Lewis opposed a realist one. For Carnap, Lewis is still attached to an untestable, metaphysical position (idealism vs. realism) which Carnap wanted to reject in his logic of science and with his formal analysis of language.[68] This divergence can also be explained by the different positions concerning the ontological implications of epistemology and of the logic of science. Contrary to Lewis, Carnap wanted to strictly separate such ontological questions from his logic of science. It can be doubted that this difference with Lewis represents a divergence with pragmatism in general. We can find in pragmatism similar tendencies as those expressed by Carnap, away from classical epistemology ("Erkenntnistheorie") towards a logic of science, away from ontological considerations towards an experimental test of empirical claims.[69]

After dealing with the pragmatist's criticism of logical empiricism at the beginning of "Testability and Meaning", Carnap turns to the central topic of his paper. This consists in an analysis of the different criteria, which can be given for the meaning of synthetic, i.e. empirical sentences. He gives different criteria for the confirmation and testability of empirical sentences. He distinguishes complete and incomplete confirmation, and more specifically complete and incomplete testability of empirical sentences. After the statement of these criteria, there can be a choice of a specific empirical language, a language in which all empirical sentences are either (completely or incompletely) confirmable or testable. Carnap opts for the most liberal version of the criterion of empirical meaning with the

68 Carnap (1936/37), 428f.

69 The writings of Peirce he published in 1877-78, which can be considered as the foundation of pragmatism, followed his vehement criticism of a Cartesian understanding of our cognitive abilities which form the basis of classical epistemology. Peirce published these writings under the title "Illustrations of the Logic of Science" (see Peirce (1986), p. 242-337). In these papers, Peirce abandoned classical epistemology and its question of the epistemological construction of the objects of the external world and turned to the logical principles of the method of science. In Dewey we can equally find a vehement criticism of the program of classical "Erkenntnistheorie". See for example Dewey (1929). Lewis certainly stood more strongly in the tradition of classical epistemology than these other pragmatists.

requirement of (incomplete) confirmability. The choice of this option is a practical decision and represents only one possible empirical language. Carnap undertakes here a liberalization of empiricism which consists in the abandonment of the strict verificationist criterion of meaning and the application of the principle of tolerance to the choice of the criterion of empirical significance.

The second aspect of Carnap's liberalization of empiricism consists in the abandonment of the attempt to define all empirical concepts through observational concepts. In the *Aufbau* Carnap still thought that such a reduction through explicit definitions is sufficient to explain all empirical concepts. But in such a system dispositional concepts (e.g. soluble as in "X is soluble in water") are excluded. An entity can be soluble in water, even if I do not observe this dissolution and even if it never is dissolved. "X is soluble in water" cannot be defined by: "X dissolves in water", a sentence which describes only an observation. Carnap introduced in "Testability and Meaning" so called reduction sentences to permit such dispositional concepts. Here Carnap does not define "X is soluble" by "If I put X in the water, X will dissolve", a sentence which is true even if the sentence "I put X in the water" is false, but he introduces the predicate "soluble" in the following reduction sentence: "If I put X in the water, then, if X dissolves, then it is soluble." The sentence gives the experimental conditions and the observable consequences which must obtain to attribute the predicate.

The introduction of predicates through their experimental conditions and also the use of dispositional concepts is very familiar to pragmatism. Peirce defined concepts through a series of conditionals which specify the experimental conditions and the observable consequences in which the concept can be applied.[70] But a direct influence of pragmatism on Carnap's conception is not probable. Carnap mentions in his intellectual autobiographical, that the liberalization of empiricism is mainly a result of the protocol sentence debate and of physicalism.[71] The abandonment of the strict principle of verification and the introduction of dispositional concepts already was suggested at the beginning of the 1930s.[72] An independent convergence of logical empiricism with certain positions of pragmatism is more probable. Only with the mutual contact between the two movements in 1934, they could reinforce their common positions. But neither the liberalization of the meaning criterion, nor the introduction of dispositional concepts was initiated through the contact with pragmatism.

However, reduction sentences were introduced later than dispositional concepts, because Carnap believed originally that dispositions could be defined by the relation between observable stimuli and observable reactions alone. From 1935 on, theoretical terms could be introduced not only through definitions, but also through reduction sentences. Following Thomas Uebel, the introduction of such

70 Peirce (1878).

71 See the section called "Liberalization of Empiricism" in Carnap (1963), 56-59.

72 Carnap introduces dispositional concepts in his (1932) paper.

reduction sentences was probably occasioned by the criticism of Carnap's defintion of dispositions by the British physicist L. J. Russell.[73]

The image of a dogmatic logical empiricism which was driven by a much more liberal pragmatism to a liberal turn cannot be sustained. Carnap and also Nagel pointed out the internal liberalization of logical empiricism initiated in the early 1930s. This internal liberalization facilitated a convergence with pragmatism and made most of the objections of the pragmatists less problematic for logical empiricism. Without this internal liberalization, the convergence with pragmatism would not be explicable. With "Testability and Meaning", Carnap undertook the explicit attempt to strengthen this convergence in order to "approach the objective of scientific empiricism" and develop an "increasingly scientific philosophy".[74] With WW II the idea of a convergence between logical empiricism and pragmatism disappeared. After 1945 the name for the second model for a convergence was revived: "analytic philosophy".

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73 See the section "The Liberalization of Empiricism" in Uebel (2007), 340-42. As already in Carnap's own description, Uebel does not mention in his analysis of this matter any influence of pragmatism.

74 As the last sentence of "Testability and Meaning" claims, see Carnap (1936/37), second part, 38.

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Toward a Theory of the Pragmatic A Priori: From Carnap to Lewis and Beyond

1. The Kantian Legacy

The notion of the *a priori* is an important legacy of Kant for modern philosophy of science. In the course of the 20th century, a variety of proposals was put forward all of which claimed to overcome the inadequacies of Kant's original proposal of *a synthetic a priori* for modern science. As Alberto Coffa put it in *The Semantic Tradition from Kant to Carnap* (Coffa 1991):

> Wittgenstein's domain of showing, his later grammar, Carnap's syntax, Sellars's categorial frameworks, and Kuhn's paradigms are some well-known members of the continuing series of attempts to find the right way of looking at that peculiar kind of knowledge that seems necessary and not vacuous, yet at the same time does not quite state any factual claims. (Coffa 1991, 138).

Although this "peculiar kind of knowledge" is not scientific knowledge proper, it is *related* to scientific knowledge. To elucidate this relation is task of philosophy of science. Kant's account of the *a priori* reflected the sciences of his times, i.e. Newtonian mechanics and Euclidean geometry. Similarly, Poincaré, Reichenbach, Carnap, and others adapted their versions of the *a priori* element in empirical knowledge to the requirements of the sciences of their times. This is a general trait. If there is something like an *a priori* element, it would be rather implausible if it could be neatly isolated from the rest of empirical knowledge such that philosophers could investigate it independently of what is going on in the sciences themselves. The philosophical search for *a priori* elements in empirical knowledge can be sucessful only if philosophy has an eye on the specifics of the scientific knowledge of its time. The search for the *a priori* element will not be a matter of philosophical armchair investigations. Rather, explicating the *a priori* element is a matter of reconstructing the existing scientific knowledge, a kind of conceptual analysis of scientific knowledge. This entails that the *a priori* will be a *moving* or a *relative a priori*, depending on the actual state of scientific knowledge.

As point of departure I'll take a version of the *a priori* that has been rather neglected by contemporary epistemologists and philosophers of science, to wit, Clarence Irving Lewis's theory of the *pragmatic* a priori that he put forward in *Mind and the World Order* (1929) (henceforth MWO) and other works. Even if in contemporary philosophy of science we experience a sort of revival of the *a priori*,

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Lewis's account certainly does not occupy centre stage. I think that this state of affairs is less than optimal. Lewis was a figure that in some sense stood between the various philosophical currents that dominated philosophy of science in the early 20th century, to wit, Logical Empiricism, American Pragmatism, and (neo)-Kantianism. Hence, his position may offer a perspective from which new light may be shed on the intricate and multifacetted issue of the *a priori*.

In *Dynamics of Reason* (Friedman 2001) Michael Friedman put forward a proposal for an updated Kantian *a priori*. In rough terms, Friedman's account of the *a priori* may be described as the result of a division of labour: Carnap's linguistic frameworks take care of the constitutive part, while a kind of regulative *a apriori* is taken from Cassirer's neo-Kantianism plus some ingredients from Kuhn's historicism. One aim of my paper is to argue there are other successor concepts of the Kantian *a priori* in 20th century philosophy of science that deserve to be reconsidered as well – in particular, if one is interested in connecting the *a priori* with contemporary naturalistic accounts of scientific knowledge. Among them there is Lewis's pragmatic *a priori*, or so I'll argue. A reconsideration of Lewis's *a priori* seems promising for several reasons:

- Lewis's version of the *a priori* element of empirical knowledge is not too far away from that of Carnap and other logical empiricists. Hence it seems possible to employ our understanding obtained of the logical empiricist concept of the *a priori* also for the elucidation of its Lewisian cousin.
- Lewis's pragmatic *a priori* does not suffer from some of the deficiencies of the theoretically biased accounts of the *a priori* that emerged in the context of logical empiricism and neo-Kantianism. It takes care of the *pragmatic* aspects in scientific knowledge neglected by logical empiricism.
- Lewis's pragmatic *a priori* may be used as a steppingstone to reach some contemporary accounts of the *a priori* that conceive it as a constitutive moment of the knowledge of an *embedded reason* – in contrast to the traditional "abstract" notions that explicitly or implicitly conceived reason as *disembodied*, *abstract* and *theoretical*.

In order to substantiate the last point I heavily rely on Hasok Chang's thought-provoking paper *Contingent Transcendental Arguments for Metaphysical Principles* (Chang 2008). Chang's reconsideration of Lewis's pragmatic *a priori* points to a *dynamics of an embodied scientific reason.* His account may be further elaborated by relating it to some concepts of contemporary *cognitive semantics* (cf. Lakoff 1986, Lakoff and Núñez 2000). Thereby an interesting relation can be established between the Lewis-Chang account of the pragmatic *a priori* and certain ideas that first emerged in the category-theoretical foundation of mathematics (cf. Awodey 2010, Mac Lane 1986).

In a nutshell, then, my sketch of a modernized *a priori* starts from the traditional account that conceives the *a priori* as an element of a universal disengaged

reason and eventually leads to a pragmatic *a priori* of a local embodied reason. More precisely, the outline of this paper is as follows: In the first section we will briefly recall the basic lines of the neo-Kantian and the logical empiricist discussions about the feasibility of a revised version of the Kantian a priori in the scientific context of the 20th century. This sets the stage for dealing with Lewis's account of the *a priori* as presented in MWO and some earlier work. Then we will discuss the reconsideration of Lewis's ideas recently proposed by Chang in his *Contingent Transcendental Arguments for Metaphysical Principles* (Chang 2008). There, Chang proposes as a general format for a Lewisian pragmatic a priori so-called principle-activity pairs (henceforth P/A-pairs). These pairs make explicit the *a priori* principles that are necessary to carry out all kinds of epistemic activities relevant for science. The aim of the next section is to show that Chang's P/A-pairs can be fruitfully related with some recent developments in cognitive science, particular with Lakoff's and Núñez's theory of conceptual metaphors that guide the embodied rationality of creatures like us (cf. Lakoff 1986, Lakoff and Núñez 2000). Finally it is pointed out that conceptual metaphors, and thereby P/A-pairs and eventually Lewis's pragmatic *a priori* as well, have a natural formal explication in terms of functors in the sense of category theory (cf. Awodey 2010, Mac Lane 1986).

2. The *A priori* in Logical Empiricism and Neo-Kantianism

According to their self-conception the various currents of neo-Kantianism were the philosophical heirs of Kant's philosophy whose "true essence" they claimed to preserve faithfully, while critically dismissing the obsolete features of the thought of their master. This self-image notwithstanding, fierce disputes arose among them what was to be understood as the true essence of Kantian philosophy. The logical empiricists, on the other hand, may be characterized as being in charge of arguing against all attempts to save the Kantian legacy from being sent to the philosophical dust bin.

In fact, this is a somewhat naïve picture of what really happened. On the one hand, one can hardly say that the various currents of Neo-Kantianism undertook very serious efforts to faithfully preserve Kant's legacy. A particularly telling example is how they dealt with one of the cornerstones of the Kantian philosophical archictecture, to wit, the role of intuition for scientific knowledge. In direct opposition to Kant's original position, virtually all Neo-Kantian currents *denied* that intuition played any role whatsoever for scientific knowledge. This deviation from Kantian orthodoxy has far-reaching ramifications also, in particular for the issue of the *a priori*. On the other hand, at least some logical empiricist at least for some time in their career subscribed to rather Kantian positions, even if they did not call them as such. For instance, Carnap held that a variant of Kantian intuition played

an essential role in geometry and physics long after neo-Kantian philosophers had dismissed intuition completely. Another example of this stance is Reichenbach. Early in his career, he was engaged in a partial defense of the Kantian *a priori* when he proposed to distinguish in Kant's *a priori* an *apodictic* and a *constitutive* moment. For him, the fact that the *a priori* was a „contribution of reason was „not expressed by the fact that the system of coordination contains unchanging elements, but in the fact that arbitrary elements occur in the system" (Reichenbach 1920/1965, 88-89). Virtually all philosophers who sought to maintain an *a priori* moment in scientific knowledge accepted a similar modification, for instance, C. I. Lewis: independently of Reichenbach but almost at the same time, he argued for a non-apodictic *pragmatic a priori* (cf. Lewis 1923, 1929).

Perhaps the most famous dispute between neo-Kantians and logical empiricists on issues concerning the Kantian *a priori* was the one between Schlick's and Cassirer's dispute on the "empiricist or criticist interpretation of relativity theory" (Schlick 1921, Cassirer 1921). Schlick argued that Einstein's relativity theory had shown once and for all that any kind of a Kantian *a priori* was untenable. If a Neo-Kantian wished to save a Kantian conception of philosophy of science he should propose a new constitutive and apodictic *a priori*. Otherwise he should renounce any version of an *a priori* element in scientific knowledge. In other words, for Schlick, in his anti-neo-Kantian bias, apodicticity was an essential feature of the *a priori*. Thereby he missed the interesting point of the postkantian evolution of this concept in 20th century philosophy of science, namely the various attempts to formulate a non-apodictic but nevertheless non-trivial *a priori*.

Philosophers of quite different taste and orientation were engaged in this endeavor (cf. Coffa 1990). In this paper I'd like to deal only with Carnap, Cassirer, and Lewis. A more detailed comparison of their different positions cannot be given in advance. Instead, in this section I am content to give a rough classification by locating them in the following table that describes their respective atttitudes with respect to certain important features of the *a priori:*

A PRIORI	apodictic	constitutive	regulative	pragmatic
Kant	yes	yes	yes	no
Cassirer	no	?[1]	yes	(yes)
Carnap	no	yes	no	no
Lewis	no	yes	no	yes

Some remarks on this schema may be in order. In th 20th century virtually all philosophers of science have given up the requirement of apodicticity. This is the only common feature shared by all authors. Further, I contend that Cassirer, Carnap,

1 There is no unanimity in this point. Friedman contends that Cassirer's *a priori* lacks any constitutive trait but only retains a regulative *a priori* (cf. Friedman 2001, 66). With Ihmig I prefer to interpret Cassirer's „relative invariants" as a constitutive relativized *a priori* (cf. Ihmig 1997, 246f).

and Lewis maintained that the *a priori* entails some kind of constitution. With respect to the regulative moment of the *a priori* their ways diverge. In Carnap and Lewis no regulative *a priori* can be found. For Cassirer, the situation was more complex. While Kant neatly distinguished between the constitutive and regulative principles by conceiving the former one as belonging to the faculty of understanding (*Verstand*) and the latter one to the faculty of reason (*Vernunft*) for Cassirer this Kantian distinction was not available. Rather, he gave a peculiar regulative twist to the constitution through relative converging invariants. According to him, the various stages of a relativized constitutive *a priori* internally converged to a limit somehow in the way as a Cauchy-convergent series of elements of a space approximates its limit (cf. Mormann 2011). Thereby "in the end" science will arrive at something quite close to the *a priori* in the original Kantian sense (cf. Friedman 2001). In contrast to Kant, however, Cassirer did not consider an immutable system of *a priori* categories as a *terminus a quo* but as a *terminus a quem* to be reached at the end of the conceptual evolution. Up to now, the problem of whether there are pragmatic aspects in Cassirer's account of scientific knowledge has hardly been considered. I contend that at least some kind of "theoretical pragmatism" may be attributed to him: according to his critical idealism, there was no experience of the given without concepts, and these concepts were *a priori* devices for the constitution of (relative) unity. They did not describe reality that was independent of us, but were rules that guided our activity:

> [Scientific] concepts are valid, not in that they copy a fixed, given being, but in sor far as they *contain a plan for possible constructions of unity* (my emphasis) which must be progressively verified in practice, in application to the empirical material. … We need, not the objectivity of absolute things, but rather the objective determinateness of the *method of experience*. (Cassirer 1910, 322)

The pragmatic component in Cassirer's account of the *a priori* is encapsulated in the claim that "valid" concepts contain "plans for possible constructions of unity". Concepts are not descriptions but blue prints for further experiences. Plans are or depend on some a priori assumptions. For him, concepts are plans for constructing an ever more thorough unity of scientific knowledge. To put it bluntly, concepts are devices for achieving certain goals, namely, to bring about an ever more comprehensive and profound unity of scientific knowledge. Hence it does not seem inappropriate to characterize Cassirer as a "theoretical pragmatist" for whom the aim of scientific activity was not to produce a faithful description of the world, but to bring about more and more comprehensive and unifying experiences. From a modern point of view, his emphasis on coherence and comprehensiveness as the most important values of the scientific enterprise may be criticized as somewhat one-sided, the important point of encounter is that for Cassirer's neo-Kantianism as well as for Lewis's pragmatism scientific knowledge, action and evaluation are essentially connected (cf. Lewis 1946, 5).

3. Lewis's Pragmatic *A Priori*

In his day Lewis (1883–1964) was one of most prominent American philosophers. Today he is much lesser known than the other three classical great pragmatists Peirce, James and Dewey. So some introductory remarks on his life and philosophy may be in order.[2] Lewis may be characterized as the most Kantian of all pragmatists although of a rather peculiar kind. He is reported to have characterized himself as "a Kantian who disagrees with every sentence of the *Critique of Pure Reason*." Beside Peirce he may be said to have been the "most logical" pragmatist. After having finished his dissertation *The Place of Intuition in Knowledge* (1911) under Royce his research interests switched to logic. He spent a lot of work to overcome the shortcomings of the standard extensional logic. Indeed, Lewis was one of the founding fathers of modern modal logic. More generally, he considered the question of what should be considered as the "correct" logic of science or of our everyday reasoning as an empirical problem the solution of which had to take into account the empirical facts of the practice of scientific investigation.

In the early 1930s he contacted Schlick and Carnap and invited them to make an effort to overcome the gap between the two philosophical currents of Logical Empiricism and American Pragmatism. For him, practice played a constitutive role in experience. In agreement with all other classical pragmatists Lewis contended that experience is active and interventionist. It is shaped by interactions with our surroundings and our specific interests and habits as agents. Beside the strict separation between facts and values, this thesis was one of the key differences between the two traditions of scientific philosophy of logical empiricism and pragmatism. In the end, this project was less than successful. Both movements did not succeed in finding a closer rapprochement and remained reserved allies each pursuing its own projects. For a final assessment of this issue from Lewis's side, see (Lewis 1941).

A first sketch of his theory of a pragmatic *a priori* is to be found in his paper *A Pragmatic Conception of the A priori* (1922). In mature form it is presented in *Mind and the World Order* (1929) Further elaborations may be found in his later book *An Analysis of Knowledge and Valuation* (1946). Let's start with a quote from (MWO) in which he subscribed to a non-apodictic interpretation of the *a priori* rather similar to the one that Reichenbach had put forward somewhat earlier:

> The *a priori* … represents the contribution of the mind itself to knowledge, it does not require that this mind be universal, or absolute … . The determination of the *a priori* is in some sense like free choice and deliberate action" (MWO, 231, 233).

2 For comprehensive presentations of Lewis's life and work the reader may consult Murphey (2005) or Rosenthal (2007); for a succinct presentation of Lewis's philosophy, and a comparison of his views with those of Carnap and Quine, the reader may consult Baldwin (2007).

Lewis's a priori kept only the constitutive element, apodicticity is given up, and intuition plays no role in it. For Lewis, the *a priori* was a variable that might change over time. Even the *a priori* principles of logic were not beyond the possibility of alteration. This was not merely an abstract possibility – Lewis was one of the leading figures in promoting alternative logics differing from that of the *Principia*.

Although no longer apodictic, Lewis's a priori kept a conditional kind of necessity. The *a priori* is true *no matter what* experience may bring. The acceptance of a concept as *a priori* is a matter of decision or legislation, something for which there are alternatives, but for which the criteria are not empirical but pragmatic. This necessity of the *a priori* has nothing to do with inescapability.

> The paradigm of the a priori in general is the definition. It has always been clear that the simplest and most obvious case of truth which can be known in advance of experience is the explicative proposition and those consequences of definition which can be derived by purely logical analysis.
> …
> If experience were other than it is, the definition and its corresponding classification might be inconvenient, useless, or fantastic, but it could not be false. (MWO, 239)

For Lewis, mathematics provided the best examples for such an analytical *a priori*. One may say, Lewis asserted, that the traditional conceptions of the a priori are the "historical shadow of Euclidean geometry" (MWO, 240-241). But Euclidean geometry gave the wrong impression that the *a priori* was unique and apodictic. The invention of a plurality of non-Euclidean geometries evidenced that the true *a priori* of scientific knowledge was an *a priori* of a different kind, which lacked uniqueness and apodicticity (MWO, 242). Rather, an essential feature of the *a priori* component in knowledge was that it could have been chosen differently. Although mathematics provided a good example of the *a priori* element, Lewis emphasized that the *a priori* element in the empirical sciences went far *beyond* the mathematical:

> All order of sufficient importance to be worthy of the name of law depends eventually upon some ordering by mind. Without initial principles by which we guide our attack upon the welter of experience, it would remain forever chaotic and refractory. In every science there are fundamental laws which are a priori because they formulate just such definitive concepts or categorial tests by which alone investigation becomes possible. (MWO, 254)

As an example of such an operational *a priori* Lewis discussed in detail Einstein's definition of simultaneity for events at a distance.[3] For him, it was a *stipulation* that one could make of one's own free-will in order to arrive at a definition of simultaneity (cf. MWO, 256). Hence Einstein's operational definition of

3 Recently, David Stump reconsidered and defended Lewis's interpretation of Einstein's definition of simultaneity (cf. Stump 2003).

simultaneity was an *a priori* law. Only by presupposing such laws one could enter upon the investigation by which further (empirical) laws were sought. This led him to the following sweeping generalization:

> Indeed *all* definitions and *all* concepts exercise this function of prescribing fundamental law to whatever they denote, because everything which has a name is to be identified with certainty only over some stretch of time. (MWO, 257)

The *a priori* element in knowledge is present whenever there is classification, interpretation, or the distinction of real from unreal – which means that it is present in all knowledge (cf. MWO, 266). Lewis vigorously argued that *a priori* laws could be abandoned if the structure, which was built upon them, did not succeed in simplifying our interpretation of the phenomena. Thereby he arrived at a kind of Kuhnian description of a revolutionary change in science forced by the pressure of new "anomalic" experience that does not fit well in the old framework:

> Beyond the principles of logic and pure mathematics … there must be further and more particular criteria of the real prior to any investigation of nature. Such definitions, fundamental principles and criteria the mind itself must supply before experiences can even begin to be intelligible. These represent more or less deep-lying attitudes, which the human mind has taken in the light of its total experience up to date. But a newer and wider experience may bring about some alteration of these attitudes even though by themselves they dictate nothing as to the content of experience, and no experience can conceivably prove them invalid. (MWO, 266)

Even if Lewis did not use this term, his account of a comprehensive a priori, which goes well beyond formal logic and mathematics, points towards a sort of transcendental logic similar to the one that Cassirer had put forward in his programmatic paper *Kant und die moderne Mathematik* (Cassirer 1907).

This brief review of the most important features of Lewis's pragmatic *a priori* suggests that it provided a framework for the conceptual and practical activities and operations of a community of scientists for some time. The pragmatic *a priori* determined what was to be understood as a meaningful problem and what counted as a solution, what methods were considered as admissible and what were the standards according to which the results were assessed. If the pressure of recalcitrant experiences became too strong, it could be given up and replaced by another one. In sum, the Lewisian *a priori* exhibited certain similarities with a Kuhnian paradigm. This result, after all, should not come out as overly surprising. On the contrary, it may be considered just as a confirmative extension of Coffa's catalogue of the various attempts of improving Kant's original notion of the *a priori* quoted in section 1.

4. Contingent Transcendental Arguments for Metaphysical Principles.[4]

In his paper *Contingent Arguments for Metaphysical Principles* (Chang 2008) the author endeavors to update Kant's transcendental arguments for the *a priori* in such a way that it takes into account to particular and contingent epistemic circumstances of the cognizing subject. According to Chang, such contingent arguments for metaphysical principles may be cast in the following form (cf. Chang (2008, 113).

- *If* we want to engage in a certain epistemic activity, *then* we must presume the truth of some metaphysical principles.

As point of departure he takes Lewis's "conceptual pragmatism" based on the central notion of the "pragmatic *a priori*". Going beyond Lewis's discussion, Chang proposes as an essential task of a theory of the pragmatic *a priori* to give an account of the "epistemic and conceptualizing activities of an embodied subject", i.e. an epistemic subject that has to come to terms with the contingent conditions of a spatio-temporal material world in which it is living. For such a subject the *a priori* makes sense only in a contingent way, not as a universal condition of cognition in general, but as a local factor of particular brands of cognition (ibid. 122).

Chang criticizes the Kantian universal and apodictic *a priori* as having ignored this local character of our cognition and as having been overly impressed by the then dominating Newtonianism, thereby conceiving some temporarily useful scientific ideas of limited scope as deep "metaphysical truths" (ibid. 114). Thus, with respect to the *a priori*, and more generally with respect to admissible scientific procedures and intelligible concepts, science has to educate philosophy of science, and not *vice versa*. This insight is actually not new, it may already be found in the writings of otherwise so divergent philosophers of science such as Philipp Frank and Ernst Cassirer.

After these general remarks let us now consider a simple example of Chang's contingent transcendental "If-then"-arguments, a version of which can already be found in Lewis (cf. Lewis 1923, 233-234) and whose ramifications in terms of cognitive science will be discussed further in the following sections:

- If we want to count things, we have to assume that they are discrete.

Otherwise, the activity of counting is simply impossible and unintelligible. In other words, discreteness is a metaphysical principle that we have to presuppose if we want to engage in the activity of counting. This necessity is a conditional pragmatic necessity. If we were jelly-fish, we would probably not engage in the task

4 The title of this section is borrowed from Chang (2008).

of counting, as already Lewis suggested (cf. Lewis 1929, 252). As Chang points out, the principle of discreteness is not empirical; it says nothing about the world itself, only that we need to take it as discrete, if we are going to count things (cf. Chang 2008, 123). In first approximation, then, a theory of the pragmatic *a priori* has the task of providing us with a comprehensive, perhaps even complete list of "principle-activity" pairs (henceforth P/A-pairs). Some of the P/A-pairs considered by Chang are gathered in the following list (cf. Chang 2008, 125ff):

Metaphysical Principle	Epistemic Activity
Discreteness	Counting
Uniform Consequence	Prediction
Sufficient Reason	Explanation
Subsistence	Narration
Transitivity	Ordering
Non-Contradiction	Assertion
…	…

In "real science" we are hardly ever engaged in carrying out these activities in isolation. Rather, the activities of real mathematics, physics, or of any other science are more complex than those mentioned so far. Hence, the sketch of a theory of the pragmatic *a priori* presented so far may seem to be utterly simplistic. Here, Kuhnian paradigms may come to the rescue. More precisely, I propose to conceive Kuhnian paradigms as complex systems of pragmatic *a priori* elements or systems of P/A-pairs that determine the scientific practice of a scientific community for some time. A scientific revolution takes place when essential components of such a system are replaced by new ones under the pressure of anomalies:

> In the typical case in which old methods of interpretation are discarded in favor of new ones, it requires new empirical data, which offer some difficulty of interpretation in the old terms, to bring about the change. … The advantage of the change must be considerable and fairly clear in order to overcome human inertia and the prestige of old habits of thought. (MWO, 269)

An important trigger for changing an established system of pragmatic a priori elements is the invention of new machines, measuring instruments and experimental set ups. They often lead to totally new problems, perspectives, and solutions. As examples Lewis briefly mentioned the invention of the telescope and microsope that brought it about that our categories of perception changed for ever (cf. MWO, 268). Since the advent of "Big Science" the importance of this kind of "instrumental a apriori" has steadily grown. To have some concrete examples, one may think of a particle colliders designed to collide particle beams, or the new types of protein sequencers in molecular biology. These machines allow us to formulate questions and to solve problems that before these devices came into being did not

make sense at all. These machines provide complex material *a priori* elements that determine for some time the practice of a scientific community. A still other type of machine-based pragmatic *a priori* are provided by the various novel methods of calculation and simulation that are indispensable tools for the constitution of many results in the advanced empirical sciences.

5. The Structure of the *A priori* according to Cognitive Semantics

According to Chang's account, the pragmatic *a priori* element of a scientific discipline is given by a system of P/A-pairs. Generally, these pairs may be characterized as devices that are used to organize the cognitive activities that a subject carries out to achieve certain goals. In first approximation, then, a *theory* of the *a priori* elements of scientific knowledge should provide lists of those P/A-pairs that help to organize important cognitive enterprises such as mathematics, physics, and other sciences.

Fortunately, philosophy of science is not alone in the task of developing a comprehensive theory of the "epistemic activities of an embodied subject and their underlying principles" from scratch. For some two decades now, cognitive scientists have been engaged in the task of elaborating such a theory as part of what has been called cognitive semantics conceived as an empirical theory of the rationality of embodied subjects (cf. Lakoff 1986, Lakoff and Núñez 2000). For reasons of space the following brief remarks on the issue of embodied rationality must suffice. In contrast to the traditional approach in epistemology that conceives reason as abstract and disembodied the new account emphasizes that

- Thought is *embodied*, that is, the structures used to put together our conceptual systems grow out of bodily experience and sense in terms of it; moreover, the core of our conceptual systems is directly grounded in perception, body movement, and experience of a material and social character.
- Thought is imaginative, in that those concepts that are not directly grounded in experience, employ metaphor, metonymy, and mental imagery – all of which go beyond the literal mirroring, or representation, of external reality. … Every time we categorize something in a way that does not mirror nature, we are using general human imaginative capacities. (Lakoff 1986, xiv)

The embodied human reason grows out of our biological nature.[5] This fact not only marks the more mundane parts of human reason and rationality such as everyday

5 The embedded character of human reason is, of course, not fully described by biological constraints. At least as important are social and historical factors of various kinds. These factors determine, to a large extent, what is assessed as rational and what is not

common sense reasoning, but also the more theoretical and abstract ramifications of human reasoning, e.g. *mathematics*. Lakoff and Núñez's key notion is the notion of *conceptual metaphor*. Conceptual metaphors enable us to use the inferential structure of one conceptual domain (say, geometry) A to reason about another domain B (say, arithmetic). According to them conceptual metaphors are the basic devices for mankind to come to terms with the ever-growing conceptual complexities of its life-world.

An elementary example is the "number line". The "number line" is the conceptual metaphor that conceptualizes "number facts" and "number relations" in terms of "geometrical facts" and "geometrical relations". Geometry – understood in a modest sense as sort of common sense knowledge about spatial facts and processes – may be considered as a more familiar realm to us than numbers, which are usually conceived as entities that are more abstract and more remote from our ordinary experience. We are accostumed with the qualitative concepts of distance, movements, neighborhood and the like long before we can trust in our capicity of dealing with numbers. Hence it might help us a lot if we could employ our capacities of coming to terms with geometrical problems when we are dealing with "number problems", i.e. if we could transfer the inferential patterns that we have used successfully for solving geometrical problems to the realm of numbers in order to solve the more abstract arithmetical problems arising there.

Let us denote such a conceptual transfer by A ⇒B, with A denoting the source domain of geometry, B the target domain of numbers, and ⇒ the conceptual transfer from A to B. I propose to interpret conceptual metaphors A ⇒B as principle-activity pairs P/A: The inferential structure of the source domain A plays the role of (a system of) a priori principles that are used to organize the conceptual praxis of the target domain B. Or, in other words, a conceptual metaphor A ⇒B encapsulates the program of organizing the exploration of B along guidelines that are proposed by the inferential patterns of the source domain A. In our case, this amounts to the attempt to deal with number problems by using conceptual devices borrowed from geometry. Whether this attempt will be successful cannot be known in advance. Rather, it is an empirical contingent fact (or not).

Let us now consider in some detail how this works in the case of the geometrization of numbers (cf. Lakoff and Núñez 2000, Chapter 3). In their book *Where Mathematics Come From* Lakoff and Núñez offer an elaborated account

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in a given historical situation. A further determinant of a more general character is the finitude of human beings: our capacities of reasoning are severely limited by restrictions of time, memory, lack of acuity, interest, energy and so on. All these factors contribute to the fact that human reason and rationality is rather different from an idealized universal general reason. If we want to investigate the *a priori* element and its role for human knowledge all of them had to be taken into account In this paper I'll only deal with the "biological embeddedness" of human reason seen from the perspective of cognitive science leaving for another occasion a broader account that also deals with the other ingredients.

of how large parts of mathematics may be conceived as arising from a complex net of constituting conceptual metaphors. Even for the constitution of elementary arithmetic they invoke not less than four grounding metaphors. In this paper only a succinct sketch of how constitution through conceptual metaphors works. Hence I am going to deal only with the so called "Measuring Stick metaphor" that may be characterized as a geometric metaphor in the sense that its source domain refers to elementary spatial structures and the inferences valid for them.

The measuring stick metaphor is based on the age-old practice of using a measuring stick or string for determining the lengths of various objects for practical purposes such as house-building, measuring physical distances etc. Thereby a stick or a string is used as a unit from which other lengths are derived as multiples. The stick or the string as a physical segment is the basic ingredient for a system of inferences that is applied to the domain of numbers by a kind of metaphorical translation manual (cf. Lakoff and Núñez 2000, 69):

• <u>Physical Geometry</u>	<u>Arithmetic</u>
• Physical segments	Numbers
• The basic physical segment	One
• The length of a physical segment	The size of a number
• Longer, Shorter	Greater, Less
• Acts of physical segment placement	Arithmetic operations
• A physical segment	The result of an arithmetic operation

These metaphorical correspondences lead, among other things, to the assumption that the concatenation of numbers has to be associative and commutative, since the corresponding concatenation of physical segments is. "Numbers" that do not fulfil these requirements are not considers as "real" numbers. A less elementary consequence is that *a priori* operations with physical segments are interpretable as arithmetical operations, and vice versa, that meaningful arithmetical operations should allow an interpretation in terms of manipulation of physical segments leads to the conclusion that the diagonal of a square with sides having length 1 also has a length, i.e. does represent a well-defined number, to wit, $\sqrt{2}$. This holds, even if we are unable to calculate $\sqrt{2}$ precisely, since it is "irrational".

One would considerably underestimate conceptual metaphors if one would take them just as devices for rendering plausible some more or less trivial pieces of elementary mathematics. In order to overcome this prejudice one may point out that Dedekind's completion of the rational numbers through his famous construction of all real numbers by "Dedekind cuts" may be conceived as based on some conceptual metaphors (cf. Lakoff and Núñez (2000, 292ff). Dedekind's method of completion is in no way restricted to the special case of the completion of rational numbers. As was pointed out already by Cassirer (Cassirer 1907) and amply confirmed by the evolution of 20th century mathematics after Cassirer, the method

of Dedekind completion may be considered as the prototype of a profusion of "idealizational" constitutions that turned out to be an essential ingredient of modern mathematics in general (cf. Mormann 2008). This evidences that the reconstruction of mathematical ideas in terms of conceptual metaphors can hardly be dismissed as a merely pedagogical device.

Conceptual metaphors do not live in isolation. They can be combined in various ways leading to ever more complex conceptual systems as is shown in detail by Lakoff and Núñez by detailed reconstructions of higher mathematical concepts as constituted by complex layers of conceptual metaphors.

There is no guarantee that metaphorical constitutions always work. A constitutive conceptual metaphor (A ⇒B) may fail in organizing the target domain B in accordance with the rules suggested by A. Then it is expedient to replace either the source domain A or the target domain B in some way or other, replacing the original metaphor by one that is pragmatically better suited. Most often the failure is a matter of degrees, i.e. the validity of a metaphor turns out to be limited. For instance, the container metaphor for sets is useful to some extent but fails if it is pushed too far. The guiding conceptual metaphors that constitute the epistemological practice of a discipline need not meet the eyes of a casual observer. Rather, they have to be reconstructed by a metaphorical analysis, so to speak, which uses to be a non-trivial task.

This description of the aim and structure of conceptual metaphors A ⇒B should suffice to make plausible the thesis that conceptual metaphors closely resemble P/A-pairs. Both P/A-pairs and conceptual metaphors A ⇒B exhibit a similar binary structure, and both serve as organizing the practice of their target domains according certain guidelines that are considered as *a priori*. For P/A-pairs the *a priori* element is characterized as a metaphysical principle that enables us to carry out a certain activity, and in the case of conceptual metaphors A ⇒B the *a priori* element is given as the inferential structure of the source domain A that is employed for the exploration of the target domain B. I do not contend, of course, that the approach based on conceptual metaphors and that on P/A-pairs are identical. This is certainly not the case. For instance, Lakoff and Núñez concentrate on the constitution of mathematical concepts, while Chang's P/A-pairs pairs have a broader scope. They contend to be relevant for all kinds of epistemological activities. This difference should not be regarded as a serious obstacle for the project of developing a unifying perspective that covers them both, or so I want to argue – with some help of Cassirer and Chang.

There is no reason to believe that conceptual metaphors are restricted to the domain of mathematics. At least in the empirical sciences in which mathematics plays an indispensable role it is to be expected that conceptual metaphors play an analogous role in the constitution of empirical knowledge as they do in mathematial knowledge. But, as P/A-pairs remind us, devices like metaphors and principle-activity pairs can be found in every cognitive endeavor. This is not to deny that there are differences between them – probably the metaphorical constitution of

empirical knowledge is more complex than that of purely mathematical knowledge. It may be that the indispensable role of experimental settings for empirical knowledge requires an adaption of the conceptual tools that describe the constitution of traditional mathematical knowledge. But there is no reason to expect that the constitutions in both realms were essentially different. Indeed, more than one hundred years ago Cassirer put forward the thesis that the constitutional methods in both realms are the same:

> Only when we have understood that the same foundational syntheses on which logic and mathematics rest also govern the scientific construction of experiential knowledge, that only they enable us to speak of a strict, lawful ordering among appearances and therewith of their objective meaning: only then the true justification of the principles is attained. (Cassirer 1907, 44)

Or, to put it in the vernacular of this paper, a theory of the relative *a priori* of empirical knowledge should be cast in the theoretical framework of principle-activity pairs and conceptual metaphors.

6. Conceptual Metaphors and Category Theory

In order to elucidate further the structure of conceptual metaphors (and of principle-activity pairsP/A-pairs) in the rest of this section I'd like to show that the "metaphorical" constitution of mathematical concepts has striking affinities with the foundational account of category theory inaugurated in the 1940s by Saunders Mac Lane and Samuel Eilenberg (Mac Lane 1986, Awodey 2010). More precisely, conceptual metaphors have the same formal structure as functors in category theory. This is hardly a coincidence (cf. Lakoff 1986), and leads to interesting ramifications for a comprehensive theory of the a priori, which can be pointed at here only in briefest outline.

According to Mac Lane, one of the great figures of 20th century mathematics, the real nature of mathematical concepts does not reside in their set-theoretical formalization but in their relation to basic human activities. In terms of Lakoff and Núñez's approach based on the notion of conceptual metaphors this means that the important point for understanding mathematics is to make explicit the grounding conceptual metaphors of the various mathematical disciplines. In *Mathematics – Form and Function* (Mac Lane 1986) the author points out that mundane human activities such as collecting, computing, observing, measuring etc. give rise to the mathematical disciplines such as set theory, arithmetics, the theory of transformation groups, the theory of metric spaces, etc. (ibid. 35). His proposal to make explicit the natural sources of mathematical knowledge may be understood as the first step to conceive mathematical knowledge as part of an embedded human reason. The cognitive semantics of Lakoff and his collaborators makes an essential

step beyond Mac Lane by offering us a detailed empirical theory, based on the findings of cognitive science and neuroscience, of how we succeeded to erect the awe-inspiring edifice of modern mathematics from the humble beginnings of the elementary activities of collecting, measuring, observing and so on.

Category theory enters the stage by offering a means for describing the structure of conceptual metaphors in detail. Recall that a conceptual metaphor A ⇒B is to use the inferential structure of the source domain A to reason about the target domain B. This idea of transfering (inferential) structures from one domain to another is captured by notion of a functor that may be considered as basic concept of category theory. For the sake of definiteness, let us succinctly recall the relevant definitions:

6.1 Definition
A category **A** consists of the following data:

- Objects: A, B, C, …
- Arrows: f, g, h, …
- For each arrow f, there are given objects dom(f), cod(f) called the domain and the codomain of f. Then f: A → B indicates that A is the domain and B is the codomain of f.
- Given arrows f: A → B and g: B → C there is an arrow g · f: A → C called the composite of f and g. For each object A there is an arrow 1_A: A → A called the identity arrow of A.

These data are required to satisfy the following laws:

- Associativity: (h • g) · f = h • (g • f) for all f: A → B g: B → C, h: C → D.
- Unit: 1_A • f = f • 1_A for all f: A → B.

Intuitively, categories may be conceived as "universes of (mathematical) discourse" giving us a frame for talking about objects and their relations. Categories abound in mathematics and elsewhere. Ample lists of categories can be found in every textbook of this subject (cf. Awodey 2010, Mac Lane 1986). The real usefulness of the category-theoretical perspective shows up, if one has to deal with relations between various universes of discourse. A translation from one universe of discourse to another one is defined as a functor, i.e. a "homomorphism of categories":

6.2 Definition
A functor F: **C** ⇒**D** between categories **C** and **D** is a mapping of objects to objects and arrows to arrows, in such a way that

- F(f: A → B) = F(f): F(A) → F(B)
- $F(1_A) = 1_{F(A)}$
- F (g · f) = F(g) · F(f).

In a similar way as the arrows of a category functors F: **C** ⇒**D** and G: **D** ⇒**E** may be concatenated to yield a functor G • F: **C** ⇒**E.** In this manner complex networks

of categories related by various functors can be built up. As has been shown in the last forty years or so, this austere base suffices to reconstruct large parts of mathematics as a complex net of functors (cf. Adamek, Herrlich and Strecker 1990). Indeed, the functorial transfer of structures is a familiar procedure in the practice of contemporary mathematics. In particular, the study of functorial relations between geometrical and algebraic categories has led to tremendous progress in both areas. In the last decades ample evidence has been gathered that the usefulness of category-theoretical concepts is in no way restricted to the realm of mathematics. Rather, category-theoretical methods spread into various other disciplines such as computer science, linguistics, cognitive science, philosophy and many other areas. This may be taken as a confirmation of Cassirer's sameness thesis according to which in both realms of scientific knowledge essentially the same constitutions are operative. Be this as it may, it should give at least some initial plausibility to the following proposal:

Conceptual metaphors $\mathbf{A} \Rightarrow \mathbf{B}$ can be considered as functors in the sense of category theory. The source domain **A** and the target domain **B** are to be conceived as categories and the transfer from the interferential structure of the former to the latter is conceived as a functor between them.

In other words, our excursion into the category-theoretical foundations of mathematics should not be considered as an idle detour into alien territory but as an attempt for tapping some new conceptual sources in order to accomplish our original task, the elucidation of the structure of the relative *a priori* in empirical sciences.

7. Concluding Remarks

Let us have a look back on the road that we have walked down in search for an adequate formulation of the *a priori* element in scientific knowledge. It started with Kant's *synthetic a priori* characterized as universal, constitutive, regulative and apodictic element of scientific knowledge. Due to the post-Kantian developments in the empirical sciences, logic and mathematics the Kantian proposal came under heavy attack from various quarters. In particular, the apodictic character of the Kantian *a priori* was considered as untenable by virtually all currents of post-Kantian philosophy of science. On the other hand, the other aspects of the Kantian *a priori* survived in some form or other well into the 20th century and are living on up to this day. For instance, even philosophers who dismissed the rest of Kantian philosophy of science as obsolete subscribed to a constitutive *a priori* in some form or other. Perhaps the most prominent example group was Reichenbach, at least during a certain stage of his philosophical career. Other logical empiricists such as Schlick and Carnap had a less positive assessment of this remaining Kantian ingredient in modern philosophy of science. Nevertheless, as been pointed out by Friedman and others, even in Carnap's mature philosophy one finds a Kantian *a*

priori in disguise in the notion of linguistic (or ontological) frameworks (cf. Friedman 2001). Indeed, as Coffa observed, the *a priori* element is a kind of protean entity that shows up in a variety of versions in virtually every current of 20th century philosophy of science, although, of course, not necessarily under that name. A rather neglected version has been Lewis's *pragmatic a priori* (Lewis 1929). Both Carnap's and Lewis's accounts of the *a priori* are non-apodictic, relativized, and constitutive. An important difference, however, is that Carnap's *a priori* is cast into a strictly theoretical account of scientific knowledge. Carnap explicitly excluded pragmatic aspects of knowledge from the proper realm of science; and consequently they were not an issue for philosophy of science proper (cf. Carnap 1935). This attitude was in stark contrast with the pragmatist philosophy of science. Pragmatists like James, Dewey, or Lewis subscribed to a different conception of knowledge vigorously put forward by Lewis at the very beginning of *An Analysis of Knowledge and Valuation* (Lewis 1946):

> 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)

As Lewis's account evidences pragmatic aspects of scientific knowledge does not leave its *a priori* element untouched. Pragmatization renders untenable to conceive the *a priori* as a universal and eternal contribution provided by a general transcendental reason. Rather, the *a priori* becomes a component of knowledge rooted in the contingent and local character of an "embedded reason" which conceives our rationality as the rationality of a finite species living in a contingent material world. This idea, which may be traced back to Lewis, is further elaborated in Chang (2008). There, Chang proposes to elaborate and systematize Lewis's pragmatic *a priori* through the concept of "principle-activity pairs" (P/A-pairs).

A major thesis brought forward in this paper contends that P/A-pairs are closely related to conceptual metaphors $B \Rightarrow C$ that play a central role in the cognitive semantics of Lakoff and Núñez. Their account seeks to explain the constitution of (mathematical) concepts in a naturalistic framework of an "embedded reason" that strongly depends on the biological and environmental contingencies of the species to which it belongs. As it turns out, the structure of conceptual metaphors $B \Rightarrow C$ can be further elucidated with the aid the concept of a functor $F: \mathbf{D} \Rightarrow \mathbf{E}$ borrowed from category theory.

In sum, then, today a rich arsenal of conceptual tools is available for explicating the function and structure of the *a priori* element in scientific knowledge. If this is true, the Kantian legacy of the *a priori* may be relevant even for 21st century philosophy of science.

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Carnap, Philosophy and "Politics in its Broadest Sense"

Is there anything new that can be learnt about Carnap and his philosophy from recent findings about Carnap's participation in the production of the Circle's inofficial manifesto of 1929, *Wissenschaftliche Weltauffassung. Der Wiener Kreis* (Carnap, Hahn, Neurath 1929)? "More than one might think", is my answer. To be sure, what there is to be learnt is not something radically new, but that is still enough to make a difference in an ongoing dispute over whether Carnap's philosophy was of a purely academic nature. What there is to be learnt is enough to contradict the quite common picture of Carnap as quietist.[1] Instead, Carnap emerges as an *activist* and his philosophy as *part of his activism*. Accordingly, the thesis to be defended here is that throughout his life, though perhaps most strongly so during his Vienna Circle period, Carnap intended his philosophy to make a difference to everyday life: it was to be *political* in the broadest sense.

I

When talking of Carnap and politics, of course, there is a need for qualifications and caveats. As we will see, anti-metaphysics was not always part of his politics. And it is true that over the course of his life and career he seemed to mellow somewhat and did not die as quite the anti-metaphysical firebrand he was in Vienna.

It is also true that Carnap was no Neurath and that even though he followed Neurath into the Austrian Social Democratic Party (having been a member of the anti-war Independent Social Democrats in Germany ten years earlier), he insisted, against him, on a certain "separation" of his "philosophical work" from his "political aims", as he put it later when he wrote his autobiography in the 1950s (1963, 23). As I argued elsewhere (2005), this passage is often misinterpreted – both with regard to Neurath and Carnap himself. It is not the case that Neurath gave political arguments for philosophical theses, nor that Carnap rejected the idea that philosophy is of relevance to the culture and politics of a society. Rather, to use Carnap's own words, Neurath provided "more pragmatical-political" arguments "for the desirability or undesirability of certain logical or empirical investigations" (namely of certain research programmes: investigations of parapsychology and methodological

1 This view has been challenged, in various ways, in Reisch (2005), Uebel (2005), Carus (2007).

R. Creath (ed.), *Rudolf Carnap and the Legacy of Logical Empiricism*,
Vienna Circle Institute Yearbook 16, DOI 10.1007/978-94-007-3929-1_8,

solipsism) and Carnap himself accepted that there existed a "connection between [their] philosophical activity and the great historical processes going on in the world" (1963, 23). (That Carnap and Neurath disagreed about the relevance of political arguments for assessing the desirability of research programmes is no doubt significant, but this disagreement must not be exaggerated.)

Indeed, Neurath's emphasis on the connection between philosophy and worldly affairs was, Carnap wrote, "of particular importance for me personally" (ibid.). As he put it: "Philosophy leads to an improvement in scientific ways of thinking and thereby to a better understanding of all that is going on in the world, both in nature and in society: this understanding in turn serves to improve human life." (Ibid., 24) Clearly, this is neither wide-eyed idealism, nor does it seem particularly radical. But if we now add to the claim that "philosophy" bears such a relation to "human life", the further claim that a particular philosopher engages in philosophy under this description, then, according to both claims together, that philosopher has what I call an "activist understanding" of his or her philosophy. Just this holds true of Carnap. In his understanding – held "at least since the Vienna time, if not earlier" but avowed still some thirty odd years later in his autobiography – the improvement which his engagement aimed at spelt "socialism in some form" and a "gradual development toward a world government" (ibid., 83). Given the extremely formal nature of his philosophy, it may sound strange that Carnap should have thought of his philosophy as ever so remotely related to these socio-political goals. Evidently, however, Carnap did so and regarded his work as contributing, in however abstract a fashion, to the Enlightement project of transforming the socio-economic and cultural condition of humanity in the light of rationally transparent principles.

II

But what does all this have to do with the Circle's manifesto, readers may ask. "A lot", is the answer. For in the production of the manifesto Carnap demonstrated precisely the activism which I have just described. Moreover, this was by no means the only occasion when Carnap showed himself to be so engaged. First, consider the manifesto.

As I have shown elsewhere (2008), according to Carnap's and Neurath's correspondence and Carnap's diary, the production history of the manifesto falls into nine distinguishable stages.

1. The inception of the idea by Carnap with likely early input by Waismann;
2. Carnap's and Feigl's first efforts;
3. Neurath's first draft;
4. Carnap's and Feigl's second effort (partly dictated by Feigl);
5. Neurath's second draft (dictated to Carnap);

6. Carnap's editing together of what had been produced so far;
7. the incorporation by Carnap of final comments by Hahn, Feigl, Frank and Neurath;
8. corrections in proof by Carnap, Feigl and Neurath;
9. last checks and joint *imprimatur* by Carnap and Neurath.

So, most importantly, Carnap was the initiator of the entire project. Now clearly, the role of Neurath, previously thought to have been the major if not sole driving force behind it, is somewhat lessened by this but it should not be diminished too much. After all, it was Neurath who admonished Carnap and Feigl after their first efforts "not to be so unworldly", as Carnap recorded in his diary. (Neurath's contribution remained essential to the manifesto as we know it.) But note also that Carnap served as overall editor, producing the final version and approving last changes – then in its considerably more "worldly" form.

Now this is not the Carnap we tend to think of, but that is what Carnap did: *initiate* and *see through production* the manifesto of a group of philosophers. This was not just to take a public stance, but to take a very controversial one! After all, the manifesto introduced what seemed startlingly new philosophical principles and announced to colleagues and the public at large that "[t]he vitality that shows itself in the efforts to rationally transform the social and economic order permeates the movement for a scientific world-conception as well" (Carnap, Hahn, Neurath 1929 [1973, 306, cf. ibid., 318-319]). If this isn't activism, what is?

It might be thought that this kind of emphasis in the text did not come from Carnap but rather from Neurath. But even if that were true, it would not discount the fact that Carnap was happy to keep it in the final version – as the correspondence shows, he by no means took all of Neurath's suggestions on board – and that he was happy to sign the manifesto. In fact, I believe that Carnap was wholly in agreement with the claim that "we have to fashion intellectual tools for everyday life, for the daily life of the scholar but also for the daily life of all those who in some way joining working at the conscious reshaping of life." (Ibid., 306). This claim is but an elaboration of the phrase in the "Aufruf" which had invited the public in the previous November to attend the founding of the Verein Ernst Mach: "In this way, the intellectual tools of modern empiricism are to be developed, tools that are also needed in public and private life."[2] And lest the hand of Neurath is suspected again, let me remind you that Carnap was happy to serve as one of the secretaries of the Verein. There are no grounds then to dissociate Carnap from the manifesto's affirmation that

The Vienna Circle does not rest content with collective work as a closed group. It is also trying to make contact with the active movements of the present, in so far as they are well

2 See the appendix of the original manifesto and Stadler (1997 [2001, 332-333]) for a reproduction of the original fly-poster.

disposed toward the scientific world-conception and turn away from metaphysics and theology. Today the Ernst Mach Society is the place from which the Circle speaks to the wider public. (Carnap, Hahn, Neurath 1929 [1973, 305])

To declare that "we have to fashion tools …" is, of course, very much in agreement with wanting philosophy to make a difference.

III

Taking a public stance with his philosophy was not a novel thing for Carnap to do in 1929. I need only recall the fiery ending of the Preface to the *Aufbau*, in which Carnap only one year earlier affirmed

an inner kinship between the attitude on which our philosophical work is founded and the intellectual attitude which presently manifests itself in entirely different walks in life … [including] … movements which strive for meaningful forms of personal and collective life, of education, and of external organisation in general (1928 [2003, xviii])

Nor does this Preface stand out as a singularity. Instead, it links rather neatly with what André Carus and Michael Friedman called "the young Carnap['s] … utopian dreams" (in press, xiv). In "Deutschlands Niederlage – Sinnloses Schicksal oder Schuld?", a manuscript dated 29 October 1918, Carnap outlined what they called a "vision of the role of the intellectual in the reconstruction of society" (ibid.) and what Thomas Mormann called "a kind of manifesto of his early political-philosophical convictions" (2009).[3]

Summarising the situation days before Germany's capitulation, Carnap spoke of a "turning point between two ages", one in which war and brute force dominated relations between the nations and one in which the "consciousness of common humanity" finds expression in international law and supranational organisations while respecting the autonomy of individual nations.[4] For Carnap, German intellectuals, "people who share the life of the mind" bore their share of the guilt – and he included himself in this – for the fact that during the war Germany stood on the wrong side of the epochal struggle: "Our guilt for limiting ethical demands to the realm of theory or at best to personal life is all the more weighty since it

3 The manuscript coded RC 089-72-04 is held by the Archive of Scientific Philosophy, Hilman Library, University of Pittsburgh. Quoted below by permission of the University of Pittsburgh. All rights reserved. Because this manuscript is not published I include the relevant portions of the German original in footnotes and my translation in the text.

4 "Wendepunkt der beiden Zeiten", "Menschheitsbewusstsein" (RC 089-72-04, p.12, 13). Translations from sources where no English version is indicated in the list of references are by the present author.

was German science in particular that worked them out."[5] These demands, Carnap noted in an earlier footnote, possessed "objective validity" and included "political value judgements and demands".[6] What's needed to remedy the situation was to pay attention to "politics in the broadest sense".[7] Carnap specified this in the closing paragraph "III. What Shall We Do?":

> The larger the guilt, the more pressing the task. Let us not fight against the awakened guilty conscience! But also let us not give in to bitterness or resignation! For that there is neither sufficient cause nor time. Time is pressing for the next years will be decisive in every respect for the formation of a world-structure as well for that of individual peoples. The experience of recent years has led us to give one particular relation a special significance, namely politics in its broadest sense. If we believe that this is where we must now apply the lever, we have no fear that by doing so our sphere of activity will be too narrowly circumscribed or too one-sided. For us everything belongs to politics that has some connection with the public communal life of people, not only the spirit that the community embodies, but also its structure, the relations among the constituent masses, the peoples, [and] larger and smaller organs all the way down to the individual human atoms. For us all professions – those concerned with education and maintenance of bodies and minds, research into the interconnections in nature, mind and world events, the shaping of human relations according to personal intuition, the production and distribution of the objects that body and mind require for life – [all these] are specialised functions according to their kind but their effects are contributions to the same project. In order to wrest from chaotic arbitrariness the manifold pursuit of all these efforts and subject them to goal-directed reason, a form of [world] community is required, that affords to every member freedom and achievement and still to every atom of each member the room to cultivate their divine soul. To create and develop this form – more organism than organisation – is for us the task of politics. In this sense politics as a science is the other branch of practical philosophy along with individual ethics, that is, a theory of value, and politics as activity consists in the realisation of these values. That is the task of an individual or of a working group of those who recognise the same values and want to realise them. The significance of this task and the responsibility that one shoulders with it have become clear to us through the recognition of the guilt that we who share the life of the mind bear for the fate of our people and humanity, and also for the catastrophe of the most recent years.[8]

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5 "Je klarer die ethischen Forderungen gerade von der deutschen Wissenschaft herausgearbeitet worden sind, umso schwerer ist unsere Schuld, dass sie auf das Gebiet der Theorie beschränkt oder bestenfalls auf das Privatleben angewandt wurden." (Ibid., 16).

6 "Mir wenigstens scheint es so, als seien wir uns nicht nur einig in dem Glauben an die objektive Geltung auch der politischen Werturteile und Forderungen…" (ibid., 5).

7 "Politik im weitesten Sinne" (ibid., 17).

8 "III. *Was sollen wir tun?* Je grösser die Schuld, um so dringender die Aufgabe. Wehren wir uns nicht gegen das aufkeimende Schuldbewusstsein! Aber verfallen wir dann auch nicht in Verbitterung oder Resignation! Hierzu ist weder genügender Grund noch Zeit vorhanden. Die Zeit drängt, dann die nächsten Jahre werden für die Gestaltung des Weltgefüges wie für die des Volksaufbaus in jeder Beziehung entscheidend sein. Das Erlebnis der letzten Jahre hat uns dazu geführt, einer dieser Beziehungen eine besondere Bedeutung beizulegen, nämlich der Politik im weitesten Sinne. Wenn wir

As Mormann has noted (2009), this passage is notable in respect not only of its objectivist view of ethics, but also in respect of its still plainly metaphysical undercurrent. For instance, in an earlier passage, Carnap had affirmed the "honour" of the "German Reich" to consist in nothing less than "serving the divine spirit in its place, with the German people as bearer of this spirit."[9] (It may be added that this was asserted in a non-chauvinistic sense.) But these undercurrents are not my concern here.

It is rather the very conception of "politics in its broadest sense" that is relevant to my thesis. Again, what is striking is the continuity with this idea of the Circle's manifesto's avowed task "to fashion intellectual tools for everyday life, for the daily life of the scholar but also for the daily life of all those who in some way joining working at the conscious reshaping of life". What follows from this continuity is that Carnap's view of philosophy as relevant to the life of humanity and as political in the broadest sense did not find its first expression in the Vienna Circle and its unofficial manifesto, but was a conviction of very long standing. To be sure, there are very significant differences between the political ideas hinted at as of inner kinship with the scientific world-conception in 1929 and the conceptions that

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glauben, gerade hier den Hebel ansetzen zu müssen, so befürchten wir nicht, dadurch könne unser Tätigkeitsbereich zu eng umgrenzt oder zu einseitig werden. Denn zur Politik gehört uns alles, was mit dem öffentlichen Gemeinschaftsleben der Menschen zusammenhängt, sowohl der Geist, den die Gemeinschaft verkörpert, wie auch ihre Struktur, der Aufbau ihrer Gliedermassen, der Völker, grosser und kleiner Organe bis herab zu Menschenatomen. So sind uns alle Berufe – Erziehung und Pflege der Körper und der Seelen, Erforschung der Zusammenhänge der Natur, des Geistes und des Weltgeschehens, Gestaltung von Dingen oder menschlichen Beziehungen nach dem innerlich Erschauten, Erzeugung und Vermittlung der Dinge, deren Leib und Seele zum Leben bedürfen – zwar nach ihrer Art gesonderte Funktionen, ihrer Wirkung nach aber Leistungen am gleichen Werk. Um das vielfältige Getriebe all dieser Leistungen der chaotischen Willkür zu entziehen und der zielbewussten Vernunft zu unterwerfen, bedarf es einer Gemeinschaftsgestalt, die jedem Gliede Freiheit und Leistung zumisst und noch einem jeden Atom jedes Gliedes Raum zur Entfaltung seiner göttlichen Seele verschafft. Diese Form, mehr Organismus als Organisation, zu schaffen und zu entwikkeln, ist uns die Aufgabe der Politik. In diesem Sinne ist Politik als Wissenschaft neben der Individualethik der andere Zweig der praktischen Philosophie, also einer Wertlehre; und Politik als Tun besteht in der Verwirklichung dieser Werte. Das ist Aufgabe eines Einzelnen oder einer Arbeitsgemeinschaft solcher, die gleiche Werte anerkennen und durchsetzen wollen. Die Bedeutung dieser Aufgabe und die Verantwortung, die mit ihr übernommen wird, sind uns klar geworden durch das Erkennen der Schuld, die wir geistigen Menschen am Schicksal unseres Volkes und der Menschheit, und auch an der Katastrophe der letzten Jahre tragen." (Ibid., 17-18, emphases suppressed) In the text above I followed where appropriate the part-translation of this passage in Carus and Friedman (in press, xv-xvi).

9 "In unseren Augen hat das Deutsche Reich kein andere Ehre, als die, dem göttlichen Geiste an seinem Platz zu dienen, also dem deutschen Volke als einem Träger dieses Geistes." (Ibid., 2)

informed his early manuscript of 1918. But that does not matter for my purposes either. What matters is that both set a similarly activist agenda for philosophy.

IV

Sceptics may concede this but counter that nothing like the early manuscript or the manifesto can be found in Carnap's work after his Vienna Circle period. This point in turn I am happy to concede. It is indeed tempting to conclude that after the less than enthusiastic reception that the manifesto found in the man to whom it was dedicated – Moritz Schlick – Carnap may have decided to forego such extra-academic activity in future. (He got his fingers burnt.) But that only means that Carnap dropped the stance of agitator – though even in his American exile he was not afraid to sign controversial declarations of support and public letters of protest. (Consider, for instance, his support for the 1948 Waldorf conference in the face of vehement intimidation by Sidney Hook, his 1951 letter on academic freedom in protest against the loyalist oath required to teach in California state institutions, and his 1970 report in support for imprisoned Mexican philosophers.)[10]

In his autobiography, Carnap put the matter faithfully as follows:

> I have always had an intense interest in moral problems, both those concerning the life of individuals and, since the First World War, those of politics. I have not been active in party politics, but I was always interested in political principles and I never shied from professing my point of view. All of us in the Vienna Circle took a strong interest in the political events in our country, in Europe, and in the world. These problems were discussed privately, not in the Circle which was devoted to theoretical questions. I think that nearly all of us shared the following three views as a matter of course which hardly needed any discussion. The first is the view that man has no supernatural powers or enemies and that therefore whatever can be done to improve life is the task of man himself. Second, we had the conviction that mankind is able to change the conditions of life in such a way that many of the sufferings of today may be avoided and that the external and internal situation of life for the individual, the community, and finally for humanity will be essentially improved. The third is the view that all deliberate action presupposes knowledge of the world, that the scientific method is the best method of acquiring knowledge and that therefore science must be regarded as one of the most valuable instruments for the improvement of life. In Vienna we had no names for these views; if we look for a brief designation in American terminology for the combination of these three convictions, the best would seem to be 'scientific humanism'. (1963, 82-3)

What Carnap clearly did not change was his view of the activist role of philosophy as such – though we may concede, as we did earlier, that this activism became more rarefied. And while his anti-metaphysics were less pronounced in later years, this did not mean that he came to like metaphysics any better – only that the

10 See the account in Reisch (2005), and the documents Carnap (1951) and (1970a), respectively.

metaphysicians he was confronted with in the U.S. in the 1950s and 60s tended to be less obviously dangerous than those he was confronted with in Central Europe in the 1930s.

If it were objected that in the last quotation Carnap just spoke of science, not philosophy, then it must be remembered how Carnap viewed philosophy. In a 1964 interview with Willy Hochkeppel for German television Carnap put the matter clearly and succinctly. Distinguishing between science and philosophy in a way that is consistent with regarding philosophy as a second-order discipline, as science in a self-reflective mode, Carnap stated that

> I believe that philosophy is or should be scientific, and we try to make it so, but only in the sense of making the same demands, namely to observe standards of objectivity and rationality in argumentation. … So science has the task not only to collect data, but to compare them, interpret them, explain them by finding general lawlikenesses; whereas philosophy only makes clear to us what the task of science is, so to speak to contribute to understanding science, not to its content. (1964 [1993, 135])

Philosophy so understood is clearly part of science (its methodological conscience, as it were) and therefore is immediately implicated in the three views that Carnap said were shared by the members of the Vienna Circle as a matter of course.

It would be easy to question the effective bite of this attitude of "scientific humanism" (compare also Feigl 1949), but I don't think it deserves to be mocked as "feel-good philosophy" (Mormann 2009). Just focussing on the attitude of wanting to make a difference, to begin with, this is not an attitude that philosophers nowadays take as a matter of course. (There are plenty of analytical philosophers who have no such ambitions for their work whatsoever, perhaps even more who would have liked to have them but became disillusioned.) Nor was it an attitude widely taken in Carnap's European days when the mandarins of German academia rather looked backwards and abhorred the "utilitarian" values that came with increasing democratisation (see e.g. Ringer 1966).

V

Lest it be thought I'm arguing for a stronger thesis than I do let me specify that by saying that Carnap intended his philosophy to be "political in the broadest sense" I do not mean to suggest that Carnap held that his philosophical theses entailed one political position rather than another. (Importantly, that Carnap himself aimed for "socialism in some form" and a "gradual development toward a world government" was not determined by his theoretical philosophy.) This brings us back to the "inner link" which manifesto detected between the scientific world-conception and "endeavours toward a new organisation of economic and social relations, toward the unification of mankind, toward a reform of school and education" (Carnap,

Hahn, Neurath 1929 [1973, 305]). What precisely did this "inner kinship", as Carnap put it elsewhere (1928 [2003, xviii]) consist in? (This will also help us to deepen our answer to a sceptical question concerning the credo of the late Carnap: just what role does theoretical philosophy play in scientific humanism?)

Well what, according to its advocates, was "the essence of the new scientific world-conception … in contrast to traditional philosophy"? It was – they answered with particular deference to Schlick and, they thought, Wittgenstein – that "[n]o special 'philosophical propositions' are propounded, but propositions are merely clarified" (Carnap, Hahn, Neurath 1929 [1973, 315]). What effected this clarification was logical analysis which, in turn, served two purposes. The first was that of "foundational" inquiry properly understood:

> [L]ogical clarification of scientific concepts, propositions, and methods liberates from inhibiting prejudices. Logical and epistemological analysis by no means wants to limit scientific inquiry; on the contrary analysis provides science with as complete a range of formal possibilities as is possible, from which to select the one which fits each empirical finding best … (Ibid., 316)

Yet logical analysis also served – to use an apt expression of Harry Frankfurt's (2005) – as "bullshit detector", as Carnap demonstrated so spectacularly in his "Überwindung der Metaphysik" (1932). But already the manifesto had claimed: "The metaphysician and the theologian believe, thereby misunderstanding themselves, that their statements say something, or that they denote a state of affairs. Analysis, however, shows that these statements say nothing but merely express a certain feeling of life." (Carnap, Hahn, Neurath 1929 [317]) So what distinguished the scientific world-conception was "the spirit of the empiricist, anti-metaphysical attitude" (ibid., 315). And it was precisely in this that the inner link in question consisted.

> The increase of metaphysical and theologizing leanings which shows itself today in many associations and sects, in books and journals, in lectures and university courses, seems to be based on the fierce social and economic struggles of the present. One group of combatants, holding fast to traditional social forms, cultivates traditional attitudes of metaphysics and theology whose content has long since been superseded; while the other group, especially in central Europe, facing a new age, rejects these views and adopts empirical science as its basis. (Ibid., 317)

Thus the conviction that "[t]he scientific world-conception serves life and life embraces it": "We are witnessing how the spirit of the scientific world-conception penetrates in growing measure the forms of personal and public life, of education, of childrearing, of architecture, and how it helps shape economic and social life *according to rational principles*." (Ibid., italics added) The inner kinship between the scientific world-conception and the various reform movements with which

Carnap, Hahn and Neurath sympathised in 1929 lies in the (believed to be) shared commitment to principles of logical argument, evidence-based reasoning and the rational transparency of decisions taken in favour of certain positions or courses of action.

So no doctrinal incongruity was involved in that claim to inner kinship. Needless to say, perhaps, that politics in its broadest sense is not party-political does not make it any less political in the sense defined by Carnap in 1918. The demand for clarity, perspicuity and intersubjective intelligibility that animated Carnap's theoretical philosophy concerned with understanding science also animated scientific humanism in general no less than the critical analysis of ideologies in particular.

VI

Those sceptical of the coherence of Carnap's ever so broadly political philosophy may instead press another worry. Did Carnap's turn to ethical non-cognitivism – which his "Intellectual Autobiography", written in the mid-to-late-1950s, dates to "about thirty years earlier" (1963, 82) – not rob him of the means to engage in discussion and analysis of these value-laden issues denounced as meaningless?

Of values Carnap said in that 1964 interview that

> we do not exclude them as totally meaningless – even though we earlier used the term 'meaningless' in this context on occasion – but only as statements which do not have the kind of meaning that occurs in the field of knowledge, both in common sense and in the more systematised scientific knowledge. (1964 [1993, 146])

So what was the point of Carnap's non-cognitivism?

> In the past philosophers often believed that they were the ones to tell us which value-statements were valid, that they decide what is good and bad. … I believe this is not correct, that philosophers cannot decide this. That is, I think that that is a decision, an individual decision of every human being according to their conscience or feeling for value or whatever you want to call it. That too is nothing absolute, but it develops and gets refined in the course of education and throughout the whole life of a human being. I believe that philosophers in modern scientific philosophy have nothing to say about the content of value statements, but only about the logical relations between value statements … (Ibid., 145-146)

Now discerning the logical relations between value-statements does demand that we can discern their meaning, likewise discerning the logical relations between value-statements and factual statements in practical syllogisms. It follows that Carnap – like other non-cognitivists – faces the so-called Frege-Geach problem. (This problem concerns the apparent change in semantic status required to retain the validity

of syllogisms in which a value-statement is embedded in a conditional.)[11] But we may let that pass for now and note instead that Carnap said, after all, that "the logical analysis of value-statements is a very important *problem*" (ibid., 145. emphasis added). The critics' charge at issue now, in any case, is not that non-cognitivism is incoherent in itself, but that non-cognitivists were prevented from participating in moral discourse (and acting on it).

So how could a philosopher after Carnap's fashion contribute to the discussion and analysis of political belief systems or ideologies?

> I believe that if one pays careful attention to the difference [between factual and value questions], then one better proceeds as follows: begin with clearing up the differences in views about facts and only when one has reached agreement in the most important points of this sort start with the discussion of the real value questions. I believe that in this way philosophy will not help us decide the value questions themselves in one way or another, but it will help us to find a clearer basis for the discussion of these questions. (Ibid., 146)

So the idea is this. First get some agreement on the pertinent facts, then consider what the evaluations differ about. Then try to find out what motivates the different evaluations. If what is at issue are conditional value-judgements, then find out whether they are empirically supported and what unconditional value-judgements are entailed by or compatible with them. If right away unconditional value-judgements are at issue, one can explore not only what other unconditional value-judgements are entailed by or compatible with them, but also explore what factual states one would be committed to consent to. These explorations would be comparative and deepen the understanding of one's own and others' positions and in doing so show up points and angles of argument that may make oneself or others reconsider their position. What such considerations cannot do, however, is logically prove one point over others, for it remains open to discussants to stick to their unconditional value-judgement despite all attempts at persuasion.[12] If the logical niceties of argumentation are observed, opponents will not be able even to call such a dissenters irrational – though they may question their maturity or wisdom (which would be veiled value-judgements in turn).

This relates to the discussion and analysis of ideologies as follows: if it does not turn up factual errors at the first stage and so show a set of beliefs to be insuffiently based on factual evidence, then it will, at the second stage, exhibit the underlying value-judgements concerning principles of social organisation and justice. These can be discussed as just described, in terms of their logical and likely empirical consequences, and a stance towards them – a decision in favour

11 See Schroeder (2010) for a discussion of treatments of this problem in contemporary expressivism.

12 Whether Carnap could treat such value-judgements that are impervious to persuasion as analytic is here left as an open question. As Richard Creath suggested to me, if analytic, they would then determine in part the meanings of the various value-judgements made with their help and would be subject to the Principle of Tolerance. A full discussion of this possibility is not needed here and is not needed for the argument at hand.

or against them – can be motivated accordingly. So non-cognitivism is no bar to critical political discussion or to personal political activism.

VII

Before closing let me return to Carnap's role in the production of the manifesto. Without wishing to push this idea too hard, I'd at least like to flag the possibility that Carnap there and elsewhere raised the possibility of thinking not only of his philosophy as broadly political, but of the philosophy he promoted as a significantly collective undertaking

We noted that Carnap wanted philosophy to be or become scientific. It strikes me as significant that he noted in the 1964 interview about philosophy: "The scientific character makes possible the cooperation of different people." (1964 [1993, 135]) Moreover, when he affirmed the progressive nature of philosophy, Carnap expressly traced it to this consequence of being scientific:

> The essence of scientific progress is that different people can work on the same task and gradually over generations pass on their results for further improvement and completion. That is now possible in philosophy itself due to its stricter objectivity and – I think this should be added – to a very large degree also by our efforts to introduce a better language in philosophy, to improve our understanding of terminology. … And so we believe that in philosophy too progress is made by making cooperation possible, by bringing about more precise training in the use of its tool. Logic, we believe, is the most important tool of philosophical analysis. (Ibid., 135-136)

For Carnap, progress in philosophy depended on its becoming scientific in the sense of constituting a cooperative venture on the basis of a common clear terminology and the sharpened competence in logical analysis of its practitioners.

When Carnap stated in his autobiography that "philosophy had been one of the most tradition-bound fields of human thinking" and that "[p]hilosophers, like anybody else, tend to follow the customary patterns of thinking", he was referring to the "cultural lag" of "certain philosophical views which seemed to me long superseded by the development of critical thought" but were still "treated as deserving serious consideration" (1963, 42). His experience of philosophy at Chicago was not of a progressive discipline. What made him hopeful for progress, of course, was the attitude of the then new generation of philosophers to cooperative work in logical analysis. Carnap, it seems, was looking forward to a time when philosophy itself was regarded less as the activity of particular individuals, but as the joint activity of groups of individual building upon each other's work and sharing together the result.

Again this was not only the view of the late Carnap. About the Vienna Circle the manifesto remarked that it did not "rest content with collective work as a

closed group" (Carnap, Hahn, Neurath 1929 [1973, 305]) What was this "collective work"?

> Over the years, a circle formed around Schlick which brought together the various endeavors toward the scientific world-conception. This concentration produced a fruitful mutual inspiration. … over the years, a growing uniformity emerged; this too was a result of the specifically scientific attitude: "What can be said at all, can be said clearly" (Wittgenstein); where there are differences of opinion, it is in the end possible to agree, and agreement is therefore demanded. It became increasingly clearer that a position not only free of metaphysics, but opposed to metaphysics was the common goal of all. (Ibid., 304)

Here it may be objected that this is but the rational convergence of views and aims. That the views and aims of the members of the group became more "uniform" does not yet mean that there was collective work. Indeed we read also:

> It is understandable that one can still clearly tell which of the individual members of the Vienna Circle come from which of the various areas of problems. This often results in differences in lines of interests and points of view, which in turn lead to differences in conception. But it is characteristic that what divides them is diminished by their efforts towards precise formulation, toward application of an exact logical language and symbolism, and toward accurate differentiation between the theoretical content of a thesis and notions merely associated with it. Step by step, the array of shared views is enlarged; it forms the core of the scientific world-conception to which its outer layers, subject to stronger subjective disagreement, are connected. (Ibid., 316).

Nevertheless the authors of the manifesto clearly discerned a tendency. Thus we also read: "Unified science is envisaged as the goal. The endeavor is to link and harmonize the achievements of individual researchers in their various fields of science. From this follows the emphasis on *collective efforts*, and also the emphasis on what can be grasped intersubjectively … " (Ibid., 306, orig. emphasis) Here the stress is on philosophical work that is thought of by the thinkers involved as a collective enterprise. Of course, these are no longer the philosophical systems of old that are at issue. Instead, very much in the manner of the sciences, a research programme concerning a certain well-delimited subject-matter is pursued with different people working on different parts of it whether centrally directed so or not. The division of labour has reached philosophy.

But be that as it is, note that the idea of philosophy as a collective effort was not entirely new to Carnap either when he composed the manifesto. Consider once more the Preface to the *Aufbau*:

> The *basic orientation* and the line of thought of this book are not property and achievement of the author alone but belong to a certain scientific atmosphere which is neither created nor maintained by any single individual. The thoughts which I have written down here are supported by *a stratum of active or receptive collaborators*. This stratum has in common especially a certain *basic scientific orientation*. … This new attitude not only changes the

style of thinking but also the type of problem that is posed. The individual no longer undertakes to erect in one bold stroke an entire system of philosophy. Rather, each works at his special place within the *one* unified science. … If we allot to the individual in philosophical work as in the special sciences only a partial task, then we can look with more confidence into the future: in slow careful construction knowledge and more knowledge will be won. Each collaborator contributes only what he can endorse and justify before the whole body of his co-workers. Thus stone will carefully be added to stone and a safe building will be erected at which each following generation can continue to work. (1928 [1967, xvi-xvii], trans. altered)

Rarely has the idea of philosophy as a collective venture been expressed more forcefully. Carnap, it seems, was prepared to subscribe to it. As is evident, the manifesto, rather than enforcing this collectivist tendency, somewhat lessened its force. (Thoughts of the likely attitude of its dedicatee may well have prompted this.) Yet Carnap's – and Neurath's – forceful advocacy of this view nevertheless raises a point that the many discussions of the legacy of logical empiricism tend to overlook. Carnap and Neurath developed the outlines of an alternative conception of philosophy as a discipline, an alternative not only to the metaphysical system-builders of old, but also the still current view of the philosopher as a solitary explorer. It is clear, as we saw, that Carnap never rejected the idea of scientific philosophy as a collective enterprise as an ideal to be striven for. As for philosophy being political "in the boadest sense", that remained not only his ideal but his practice throughout his long career.[13]

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13 I wish to thank Thomas Mormann for drawing my attention to Carnap's "Deutschland's Niederlage – Sinnloses Schicksal oder Schuld?" and our related discussions and Richard Creath for helpful editorial suggestions.

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A Virtual Debate in Exile: Cassirer and the Vienna Circle after 1933

Ernst Cassirer, 2011, *Symbolische Prägnanz, Ausdruckphänomen und „Wiener Kreis", Nachgelassene Manuskripte und Texte*, vol. 4, ed. Christian Möckel, 478pp., Hamburg, Felix Meiner Verlag.[1]

1. Cassirer and Other Philosophers

Cassirer was one of the leading philosophers and public intellectuals in Germany in the last years of the Weimar Republic. In philosophy of science one might recall his discussion with Schlick on the philosophical interpretation of Einstein's relativity theory in the early 1920s. The famous *Davos Disputation* of Cassirer and Heidegger in 1929 was considered a major philosophical event by his contemporaries. The participants included Carnap who on this occasion got to know Cassirer personally. Later, Cassirer, Schlick, and Carnap met several times in Vienna. Carnap had received essential ideas for the *Aufbau* from Cassirer and other neo-Kantians, and he referred to Cassirer's works already in his first philosophical publication *Der Raum* (Carnap 1922). Cassirer's contacts were not restricted to the members of the Vienna Circle – he was on friendly terms from 1915 till the end of his life with his former student Reichenbach.[2] Not all members of the Vienna Circle held Cassirer in high esteem, however. Neurath dismissively characterized him as a "Kantian, who sometimes stood more closely to the basic conception of modern science than other Kantians …" (Neurath, 1936, 694). In a similar vein, Philipp Frank used to characterize Cassirer as a representative of "school philosophy". Only later, in a review of Cassirer's *Determinismus und Indeterminismus in der modernen Physik* (Cassirer 1937), did he reluctantly extend a poisoned accolade to Cassirer by describing the book "as a highly successful attempt to continue

1 In this paper, the following abbreviations for Cassirer's works are used: ECW = Ernst Cassirer Werke, ECN = Ernst Cassirer Nachgelassene Manuskripte und Texte, ECB = Ernst Cassirer Briefe, SF = Substanzbegriff und Funktionsbegriff, PSF = Philosophie der Symbolischen Formen. All Cassirer citations are translated from the German into English by the reviewer.

2 The last but one letter that Cassirer wrote before his unexpected death on April 13 of 1945 was to Reichenbach, who had invited him as a visiting scholar to California (ECB, pp. 238-239, April 10, 1945).

R. Creath (ed.), *Rudolf Carnap and the Legacy of Logical Empiricism*,
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the adjustment of the traditional idealist philosophy to the progress of science", which, in his opinion, could end "only with the complete disintegration of the traditional philosophy" (Frank 1938 (1955), 184/185). *Pace* Frank's contribution. The award for the most curious dispute with Cassirer should probably go to Kurt Grelling who thirty years earlier, in the youthful polemical paper *Das gute, klare Recht der Freunde der anthropologischen Vernunftkritik, verteidigt gegen Ernst Cassirer* (Grelling 1908), had taken issue with Cassirer defending Leonard Nelson in the latter's dispute with Hermann Cohen, Cassirer's mentor and the head of Marburg neo-Kantianism.[3]

In sum, in the 1920s Cassirer and the members of the Vienna Circle and the Berlin group were involved in quite a few, sometimes polemical discussions with each other that dealt with a broad spectrum of themes from science and philosophy. After the Nationalsocialists seized power in Germany and the Clerical Fascists in Austria most members of these groups and many other philosophers and scientists were forced to emigrate. Most went to the US, but some also to other countries, such as Great Britain (Cassirer, Neurath), Sweden (Cassirer), or even Turkey (Reichenbach), at least temporarily. The intellectual connections that had existed since the first decades of the last century were thus interrupted or at least seriously damaged due to the often difficult circumstances under which the emigrants had to live. One might assume that the vivid intellectual exchanges that had taken place during the Weimar years would have considerably diminished. Cassirer and the logical empiricists defy this conjecture – at least in one direction: Cassirer's interest in the philosophy of the Vienna Circle reached its peak *after* the Circle had ceased to exist in Vienna (cf. Krois 2000, 136). This is amply evidenced by Cassirer's posthumous writings ECN (*Ernst Cassirer Nachgelassene Manuskripte und Texte*) whose publication is now well under way in a lavish critical edition under the general editorship of John Michael Krois (†), Klaus Christian Köhnke, and Oskar Schwemmer. Twenty volumes are projected from which about twelve have been already published. ECN drags its material from the holdings of the *Beinecke Rare Books and Manuscripts Library* at Yale University, and of other libraries and privately owned manuscripts.

According to the publisher, the volumes of ECN are arranged thematically. This must be understood in a broad sense: on the one hand, the volume under review (ECN4) brings together many texts that do not have much to do with Cassirer's relation to the Vienna Circle, on the other hand many other volumes of ECN do contain a wealth of papers that deal extensively with precisely this topic. Thus, a reader who wishes to gain a broad understanding of Cassirer's later thought on a specific topic is well advised to read across the different volumes of ECN. The present review will follow this strategy, i.e., focus only those texts of ECN4 that are relevant to the topic of this essay, but at the same time consider pertinent texts

3 Twenty years later, Neurath, Carnap, and Hahn considered Grelling's paper as important enough to be included in the bibliography of *The Scientific Conception of the World: The Vienna Circle* (Manifesto).

from other volumes of ECN. Nonetheless, it seems expedient to give the reader a short description of the topics that are treated in ECN4. The volume has two main parts: (I*) Symbolische Prägnanz, Ausdrucksphänomen und „Wiener Kreis“*, and (II) *Beilagen* that provide among other texts some lecture notes of Cassirer's from the early 1920s. The volume is rounded off with an extensive appendix (*Anhang*) of approximately 160 pages containing critical editorial comments and further elucidations regarding the published material.

While Cassirer's continuing interest in logical empiricism in general and in the Vienna Circle in particular is evidenced by a wealth of texts now available in ECN, the interest of members of the Vienna Circle in Cassirer after 1933 is more difficult to substantiate. Carnap, for instance, hardly ever mentioned Cassirer in his later writings. This should not be interpreted as meaning that he was not interested in Cassirer's later philosophy. As I would like to show in the following, a kind of virtual debate took place: Cassirer's philosophy of symbolic forms may be considered as an implicit target of Carnap in the early thirties.

More precisely, the aim of this essay is to discuss some aspects of the virtual disputes that Cassirer had in ECN with the leading figures of the Vienna Circle, in particular with Carnap.[4] This may give us an idea of how a debate between two competing currents of German enlightenment-oriented philosophy could have looked like under more fortunate historical circumstances. This may not be only of historico-philosophical interest: It does not seem unreasonable to contend that such a debate has remained a matter still to be resolved for German philosophy to this today.[5]

The publication of Cassirer's posthumous writings in ECN is, of course, not only important for elucidating his relations with the logical empiricism of the Vienna Circle, it also sheds new light on his philosophy in general. The writings of ECN show that his thought after the completion of his opus magnum *The Philosophy of Symbolic Forms* (1923–1929) (PSF) underwent a further evolution that brought many new aspects to the fore. This is evidenced in particular by the so-called "fourth volume of the *Philosophy of Symbolic Forms*" (ECN1) in which Cassirer sought to address philosophical currents that had not yet found their place

4 Although Cassirer was well aware of the fact (cf. ECN4, 186) that the Vienna Circle was not a monolithic philosophical movement, after 1933 he mainly dealt with Carnap's version of logical empiricism. To some extent, he later tended to identify the Vienna Circle's logical empiricism with Carnap's. Before 1933, his main addressee had been Schlick, as is exemplified, for instance, by Cassirer (1927). Schlick had started the debate with Cassirer already in 1921 with the highly influential paper *Kritizistische oder empiristische Deutung der neuen Physik? Bemerkungen zu Ernst Cassirers Buch „Zur Einsteinschen Relativitätstheorie“* (Schlick 1921).

5 As is well known, after the end of the Second World War, in Germany and Austria anti-enlightenment and reactionary philosophies continued to dominate the philosophical scene for decades. Many intellectual figures, who had been prominent already in the Weimar Republic, kept on being influential in West Germany. Cassirer was not one of them.

in his philosophical universe. The most prominent ones were perhaps Husserlian phenomenology, *Lebensphilosophie*, and the rising star of Heidegger.

These new ingredients had a considerable influence on his stance toward the logical empiricism of the Vienna Circle. Or, seen from the opposite perspective, Cassirer's numerous references to logical empiricism in the texts of ECN show that he was at pains to defend his own account of philosophy (dubbed "criticial idealism" or "philosophy of symbolic forms") against the rivaling one of the logical empirists that had been put forward most vigorously by Carnap.

While Cassirer was in the process of developing an all-encompassing philosophy of culture, at the same time Carnap's style of philosophizing evolved in a quite different direction. In the *Aufbau* he had still opted for a "comprehensive scientific philosophy" that dealt not only with empirical and formal sciences, but also sought to include a theory of *Geisteswissenschaften* dealing with cultural objects, in particular values (cf. Mormann (2006)). Around 1930, however, he began to favor a "restrictive scientific philosophy" according to which values and other cultural objects ceased to be respectable objects of study for scientific philosophy. In his post-*Aufbau* works Carnap concentrated more and more on formal and logical aspects of philosophy of science, and hardly ever mentioned the work of traditional philosophers as is evidenced in *Logische Syntax der Sprache* (Carnap 1934) and the programmatic article *On the Character of Philosophical Problems* (Carnap 1934a) written for the newly founded journal *Philosophy of Science*. Thus, after 1930 Cassirer and Carnap headed in quite different directions. While Cassirer sought to reach an all-embracing panoramic understanding of the sciences, the humanities (*Geisteswissenschaften* or *Kulturwissenschaften*) and other symbolic forms, Carnap concentrated on the logic of science as the very essence of a (post) philosophical understanding of science and human reason.

This does not mean that Carnap ignored traditional philosophy altogether. On the contrary, it remained an important concern for him. *The Elimination of Metaphysics by through Logical Analysis of Language* (Carnap 1932), *The Unity of Science* (Carnap 1932a) and *Philosophy and Logical Syntax* (Carnap 1935) may be read as relentless, although anonymous, attacks on then contemporary non-empiricist philosophical currents. While *Overcoming* targeted Heidegger and, on a different level, Rickert's *Wertwissenschaft*, *The Unity of Science* (Carnap 1932a) and *Philosophy and Logical Syntax* (Carnap 1935) targeted central theses of Cassirer's philosophy of the symbolic forms, namely, the meaningfulness of the expressive function. In these papers neither Rickert nor Cassirer nor any other "traditional" philosopher is mentioned by name. Not even Heidegger was considered as an individual philosopher but just as a typical metaphysician, as Carnap dismissively asserted in a footnote. This peculiar style was probably due to the fact that these articles were not meant as contributions to an open-ended discussion with philosophical adversaries but as "official announcements" of the doctrines of the logical empiricism of the Vienna Circle.

Thus, one may speak of a virtual debate between Cassirer and Carnap (whom Cassirer seemed to have considered as a sort of representative of the Vienna Circle) that took place after 1933 when most of members of the Vienna Circle and Cassirer had been exiled from their home countries.

Following the Second World War, the fates of Cassirer's and Carnap's philosophies were quite different: Carnap's version of logical empiricism became part of mainstream analytic philosophy in the US, while Cassirer's philosophy rapidly fell into oblivion.[6] In Germany and the rest of Europe the twelve years of the Third Reich had sufficed to wipe out his memory almost completely.[7] This dark age for Cassirer scholarship definitively belongs to the past. Since the mid-eighties of the last century a Cassirer-renaissance is well under way. Meanwhile the immense project of a critical edition of his collected works ECW and his posthumous writings ECN has been realized to a large extent. A wealth of secondary literature is constantly being produced, and Cassirer may safely be considered a recognized classical author of 20th century philosophy.

Cassirer's critique of logical empiricism concentrated on the issue of physicalism. Independently of this topic, however, he considered the Viennese way of philosophizing as resulting in a serious impoverishment of philosophy, and he vigorously argued against the allegedly reductionist conception of knowledge, science, and philosophy favored by the Viennese philosophers. For this endeavor, he drew on a variety of arguments from his Philosophy of symbolic forms, phenomenology, and *Lebensphilosophie*.

The outline of this paper is as follows. In section 2 the project of physicalist logical empiricism is contrasted with Cassirer's philosophy of symbolic forms: Physicalism is characterized by its thorough-going monism. According to it there is only one kind of science to be formulated in one language, to wit, the language of physics. In contrast, the philosophy of symbolic forms insists on an irreducible

6 Evidence for this is the fact that for more than thirty years after Cassirer's death (until the 1980s) no serious efforts were made to make accessible Cassirer's philosophical *Nachlass*.

7 For German-speaking philosophy, this is witnessed by Stegmüller's *Hauptströmungen der Gegenwartsphilosophie* (1952 (1989[7])). Stegmüller mentioned Cassirer only in a footnote as „one of the most important and knowledgeable Kant scholars." On the other hand, he saw no problem ranking Scheler, Hartmann, Jaspers, Haeberlin, and Reininger among the protagonists of the most important contemporary currents of philosophy – still in the latest edition of 1989. For several decades, *Hauptströmungen* was a very popular reference work, but nobody complained, as far as I know, about the author's strange selection of the "main currents of contemporary philosophy". Stegmüller not only didn't take notice of Cassirer, he also preferred to ignore marxism and pragmatism (cf. Mormann 2010). Even in Schnädelbach's *Philosophy in Germany 1831–1933* (Schnädelbach 1984) Cassirer is mentioned in passing only once as the author of "his classic work of *Substanzbegriff und Funktionsbegriff*" (ibid., 87) In contrast, Schnädelbach dedicates detailed discussions to the works of Cassirer's Weimar contemporaries Rickert, Scheler, and Spengler.

plurality of symbolic forms and their languages, all of which philosophy has to recognize without dogmatically singling out the physical one as the only one that has cognitive content.

As Cassirer had argued in PSF an important source for the irreducible plurality of symbolic forms was the so called "expressive function" of symbolization (cf. (PSF III, Part I)). While in PSF the expressive function was primarily discussed in its relation to the symbolic form of myth, in many writings of ECN this concept became also important for the constitution of psychology and *Kulturwissenschaften*. As will be shown in section 3, Cassirer's account of the expressive function directly clashes with that of Carnap, for whom expressive propositions had no cognitive content at all.

Section 4 deals with Cassirer's attempt to find a middle way between *Lebensphilosophie* and logical empiricism (cf. also Ikonen 2011). He considered both to be one-sided philosophical currents suffering from complementary shortcomings, namely, a dogmatic acceptance of the contentions of the expressive function from the side of *Lebensphilosophie*, and an equally dogmatic skepticism with respect to the expressive function from the side of logical empiricism. The aim of section 5 is to assess the affinities and the differences between Cassirer and logical empiricism. On the one hand, Cassirer clearly recognized that both accounts shared a philosophical legacy that may be roughly characterized as enlightenment-oriented, scientific philosophy. On the other hand, the two accounts conceived of the task of philosophy in quite different, perhaps even opposite ways: for Carnap philosophy and science were theoretical, while Cassirer saw science and philosophy as activities of a collective subject that aimed to constitute a complex network of symbolic meanings.

2. Physicalist Monism and Symbolic Pluralism

For Cassirer, the core of the Vienna Circle's logical empiricism was physicalism. According to him, the physicalist doctrine most clearly marked the differences between his "philosophy of symbolic forms" and Viennese empiricism.

In the following I will mainly deal with the manuscript *Symbolische Prägnanz, Ausdrucksphänomen und "Wiener Kreis"* from ECN4. Symbolic pregnance and expression phenomenon being key concepts of the philosophy of the symbolic forms, already the title of this text indicates what was at stake here for Cassirer, namely, the defense of his philosophical position against the Vienna logical empiricism. (ECN4) was, however, in no way the only text in which Cassirer was struggling with Vienna. Disputes about logico-empiricist theses are to be found in many volumes of (ECN), see for instance, ECN 1 (118 – 120), ECN2 (7, 135ff), ECN4 (153ff, 205, 209f), or ECN5 (72 – 74).[8]

8 *Symbolische Prägnanz, Ausdrucksphänomen und „Wiener Kreis"* was written

Cassirer's main references for his discussion of physicalism were the *Aufbau* (Carnap 1928), *Pseudoproblems in Philosophy* (Carnap 1928a), and *The Unity of Science* (Carnap 1932a). In this booklet Carnap formulated physicalism as the thesis about the global architectonics of science. According to it, the traditional separation between the natural sciences, the humanities, and psychology was obsolete. They were all are parts of the same unified science. The language of unified science was to be a physical language, i.e., a language of an ideal physics, in which all scientific statements are statements about spatio-temporal events and processes. Moreover, philosophy was not to be conceived of as an extra science having its own domain; rather, the task of philosophy was the clarification of the concepts and propositions of science (cf. Carnap (1932, 433)).

Cassirer vigorously rejected these monopolist claims. According to him, the language of physics was not a universal language, but just a special language. Moreover, philosophy had to take into account *all* languages since they all were cultural products in their own right. Thereby it had to become a pluralist philosophy of symbolic forms (cf. ECN 4, 205). Philosophy was not primarily engaged in determining *a priori* which were admissible and which were not.

As Carnap had already expected, physicalism met the fiercest resistence in the case of psychology (cf. Carnap (1932, 36f)). Complementarily, the partisans of physicalism believed that a physicalist translation of psychology was of strategic significance for their program: If only psychology fell prey to physicalism, the physicalist translations of all other sciences, dealing with historical, cultural, and economic issues, would easily follow (ibid. 72). But, as Carnap pointed out, this would be the case only for the really scientific propositions in this the area – the many pseudo-concepts, which cluttered the *Geisteswissenschaften* or *Kulturwissenschaften*, would, of course, not be translatable into properly scientific, genuine physicalist terms. In other words, for Carnap, translatability into physicalist language served as a criterion for scientificity.

Cassirer agreed with Carnap in that psychology and *Kulturwissenschaften* were crucial for physicalism. Consequently, he concentrated his attacks against physicalism exactly on this point, namely, the physicalist contention that psychology could be reformulated in physicalist, i.e., behaviorist terms. For Cassirer, the basic flaw of all physicalist attempts to explain the psychical was located in the implicit positivist assumption that "originally" only the physical was given. Taking the physical as starting point, the task for physicalism was to explain how from this base the psychical could be constituted in some way or other. According to Cassirer this project was doomed to fail from the outset, since an analysis of the transcendental presuppositions revealed that the physical was not originally given

1935/1936 probably as material for an article that Reichenbach had commissioned for *Erkenntnis*. Cassirer had planned to take Schlick as the target of his contribution (cf. ECN4, 340). After Schlick's assassination Cassirer no longer pursued this project, although, as he wrote to Reichenbach, "the thing is pretty well finished inside of my head" (ECB, September 1, 1936, p. 151).

(cf. ECN4, 153). To back this claim, he relied on arguments from phenomenology according to which the phenomena of the "I" and the "Thou" are basic phenomena not reducible to any other phenomena such as the physical "It"– neither by analogy, empathy, or by any other method.[9] Rather, for positing an objective physical world, a community of subjects that already share a common world was necessary. Taking the physical as an ultimate basis was a positivist prejudice.

Carnap's confessed adherence to physicalism did not entail that the *Aufbau* project did not share important features with his "Critical philosophy" and Husserl's phenomenology (cf. ECN4, 153), namely, that the concept of constitution played a central role in all three of them. Indeed, in constituting the realm of *Kulturwissenschaften*, Cassirer may be seen as continuing – on a much broader and more detailed scale than Carnap – a project that the latter had already sketched in the *Aufbau*, to wit, the constitution of "cultural objects" (cf. *Aufbau* (§§ 150ff) and Mormann 2006).

Cassirer virtually rehearsed Carnap's constitution of cultural objects as witnesses of an "objective spirit" that the latter had developed in the *Aufbau* (§55ff, §150-152). This is evidenced by the fact that both Cassirer (cf. ECN5, 7, 131) and Carnap (cf. *Aufbau §12,* §56) referred to Hans Freyer's Neohegelian *Theorie des objektiven Geistes* (Freyer 1923).[10]

A naive physicalism conceived of the physical as something given. In contrast, neo-Kantian transcendental philosophy understood the physical not as "given" ("gegeben") but as "aufgegeben", i.e.. as something to be constituted in an ongoing process of investigation. Cassirer conceded that Carnap was not a naive physicalist, since in the *Aufbau* he did not take the physical as given, but rather as constituted by the method of quasi-analysis. Nevertheless, Carnap unfortunately clung to a positivist bias when he sought to reconstruct the psychical in terms of the physical. This flaw bereft his constitution theory of the conceptual means to deal adequately with questions concerning psychical and related concepts. As a result, many traditional problems of philosophy were disqualified as pseudoproblems (cf. ECN4, 210, Footnote 11).

In particular, physicalism was mistaken in contending that the expressive function was devoid of cognitive meaning. In fact, logical empiricism had only shown that the expressive phenomena and utterances had no meaning *within* the realm of physicalist discourse, i.e., from the standpoint of physics. It was, however, erroneous to conclude from this that they would be altogether without meaning.

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9 For Husserl's phenomenological constitution of the psychical, see *Cartesian Meditations* §42 – §62 (Husserl 1931).

10 Not for long, however, Freyer's "objective spirit" enjoyed a good reputation in Carnap's idearium. Only a few years later, it had become a typical example of a pseudoconcept that could <u>not</u> be translated into honest physicalist terms (cf. Carnap (1932, 73)).

There was cognitive meaning beyond physicalism. Or, as Cassirer put it, "metaphysicalism" does not coincide with "metaphysics" (cf. ECN4, 210).[11]

For Cassirer philosophy was more than a critique of knowledge. It was essential for a philosophy in its proper sense that it deals with the universe of human symbolization in all its dimensions. Against the Vienna Circle's thesis that only decidable problems were meaningful problems, Cassirer argued that, although the problem of the psychical may not be decidable on theoretical grounds, it nevertheless was meaningful since it made a practical difference. Carnap admitted such a practical difference (cf. *Aufbau* §11), but insisted, as always, that practical differences were scientifically irrelevant. For him, the practice of science was not an issue which could be discussed in philosophy of science proper, since science as such was concerned only with theoretical knowledge (cf. Carnap (1935, 32)). Consequently, the only task of philosophy of science proper was the purely theoretical analysis of the formal structure of the language of science (ibd., 99).

Although Cassirer rejected physicalism he did not regard it as completely useless. Physicalism had made an important contribution to philosophy of science by clarifying how to distinguish the natural sciences from *Kulturwissenschaften* by pointing out that the expressive function was an indispensable ingredient of the latter, since they necessarily went beyond the physical. For Cassirer a phenomenological analysis revealed that also the expressive function had to play an essential role for an objective human world (cf. ECN 4, 207f). In other words, Cassirer and Carnap are involved in a vigorous virtual debate on the metaphysical character of the expression function.

3. Expression Perception

In PSF "expression perception" was mainly related to the symbolic forms of art, language, and myth. In particular, the mythical world conception was characterized by the primacy of expression perception over object perception. For it, there still does not exist a world of things. Everything is perceived as expressing, so to speak, a personal meaning. Only later does science replace expression qualities by sense qualities. It should be noted, however, that the expression function placed an important role for Cassirer's approach that had not much to do with myth. For instance, it enables us to perceive the three basic phenomena of "I", "Thou", and "It" that are needed to get a comprehensive understanding of the world. These basic phenomena do not have much to do with myth but a lot with the *Lebenswelt* in the sense of Husserlian phenomenology. Their irreducibility to phenomena that

11 In German, this can be expressed elegantly by distinguishing between "metaphysisch" and "metaphysikalisch": "Was den 'Wiener Kreis' betrifft, so entstehen hier viele Schwierigkeiten daraus, daß viele Probleme als meta-physisch bezeichnet und als solche denunziert werden, die nur meta-physikalisch sind." (ECN 4, 210).

can be understood in purely physicalist terms was the basis for Cassirer's rejection of physicalism: "Experiences of pure expression are not of mediated but of an original character" and "Understanding of expression is prior to knowledge of things" (PSF III, 65).

Carnap's attempts in the *Aufbau* to constitute the heteropsychological and the physical from an autopsychological base were doomed to fail from the outset for Cassirer since they sought to reduce two of the three basic phenomena, namely "Thou" and "It" to the third ("I").[12]

In sum, the opposed assessments of the expressive function were the point where Cassirer and Carnap parted ways. I thus propose to read Carnap's thesis, put forward in (Carnap 1935) that expressive phenomena are sheer metaphysics, as a direct, although anonymous, attack against Cassirer's philosophy of symbolic forms:

> Metaphysical propositions express something, … but nevertheless they have no sense, no theoretical content.
> …
> Metaphysical propositions – like lyrical verses – have only an expressive function. … they lie completely outside the field of knowledge. (Carnap (1935, § 5, *Metaphysics as Expression*, 27, 29))

Cassirer was not alone in contending that the phenomena of "I", "Thou", and "It" were irreducible to each other and to anything else. The later Husserl argued for similar theses (cf. ECN 4, 154; Husserl (1931, §49)). According to Husserl, the first "non-I" was an other "I" (the "Thou"), not an "It". Only later, the subject came to differentiate between various aspects of its world and the objective thing-world appeared. For Cassirer, expression perception was a genuine source of cognition, it was crucial for the foundation for *Kulturwissenschaften*:

> The "expression" must be added as a second dimension – as the key for the world of "life", "soul", and "mind". Without it these three worlds would remain closed for ever. From the mere perception of things no path leads to them. (ECN1, Über Basisphänomene, 118)

In contrast, Carnap sought to find access to these allegedly non-physicalist worlds through a radically behaviorist reduction that Cassirer rejected as implausible. Instead, he turned the physicalist argument upside-down. The expression perception constituted for every subject the original phenomenon of being in a common world that it shared with other subjects (koinos kosmos). Drawing on results from

12 It may seem doubtful whether Carnap's "autopsychological" can be identified with the "I" in Cassirer's or Husserl's sense (cf. *Aufbau* §65). If this is denied, in Cassirer's eyes, the expectations for Carnap's reductionist constitution project looked even bleaker, since then the *Aufbau*er was forced to constitute all three basic phenomena from something more basic than all of them, which, according to Cassirer, was quite impossible.

gestalt psychology Cassirer pointed out that this phenomenon manifested itself already in new-born infants who very early distinguished between faces as friendly and unfriendly, respectively, but did not distinguish between different color spots as a reductionist psychology contended (cf. ECN 4, 153). According to the neo-Kantian "transcendental method" of philosophy to which Cassirer subscribed throughout his entire philosophical career, an unprejudiced scientific philosophy had to acknowledge this kind of facts instead of getting engaged in futile reductionist endeavors.

4. *Lebensphilosophie*

If there was a philosophical current characteristic of philosophy in Germany in the later years of the Wilhelmine Empire and the Weimar republic, it certainly was *Lebensphilosophie* (philosophy of life) (cf. Kusch 1995, Ringer 1969). On the surface, the relation of the logical empiricists of the Vienna Circle to *Lebensphilosophie* was simple. They dismissed *Lebensphilosophie* as unmitigated metaphysical nonsense. Actually, as will be seen, matters were not thus simple. Before coming to this issue let us briefly recall Cassirer's differentiated attitude to *Lebensphilosophie*. Although neo-Kantian philosophy in general was critical with respect to *Lebensphilosophie* it did not dismiss it out of hand. Cassirer cast his criticism of *Lebensphilosophie* in the same framework as his criticism of metaphysics in general. Already in *Substance and Function*, he had put forward the thesis that a metaphysical philosophical stance usually was characterized by certain absolutized dualistic schemes (cf. SF, 271). Twenty years later, in the so-called fourth volume of *Philosophy of Symbolic Forms*, he criticised *Lebensphilosophie* as the then reigning version of metaphysics, as an example of such a dualistic thinking:

> The opposition of "life" and "spirit" is in the centre of the metaphysics of the 19th and the beginning 20th century. It turns out to be thus determining and decisive that it swallows more and more all the other metaphysical dualisms that have been coined in the history of metaphysics, thereby making them disappear. The oppositions of "being" and "becoming", "unity" and "plurality", "matter" and "form", "soul" and "body" all appear to be dissolved in that one basic antithesis. (ECN1, 7-8).

Cassirer traced back *Lebensphilosophie* to 19th century's romanticism and took it as evidence of the profound influence that romanticism still had on the "modern and most modern currents of philosophy" in Germany (cf. Cassirer (1993, 33ff.)). The dualistic tendency of lebensphilosophical metaphysics stood in stark contrast to the philosophy of symbolic forms that aimed to overcome fruitless oppositions, in particular that between *Geist* and *Leben*.

For Carnap the opposition between *Geist* and *Leben* was not an issue that could be discussed in a rational discourse. *Leben* was a realm determined by one's

Lebensgefühl, not something belonging the ken of rational deliberations and decisions. *Leben* for him was a matter of living one's life and expressing one's feelings and emotions in terms of literature, music, and other arts. There was no point in arguing about one's *Lebensgefühl*.[13] Nevertheless, "Leben" played an important subliminal role in the Vienna Circle's philosophical *Weltanschauung*. For instance, the *Manifesto* closes with the cryptic remark that "Science serves life, and life receives it" (*Manifesto* 1929, 318). Certainly a resounding final phrase, but its meaning is far from clear, even in its original German. Similarly, in the preface to the first edition of the *Aufbau* one finds the wooly remark:

> [W]e feel that there is an inner kinship between the attitude on which our philosophical work is founded and artistic movements … and in movements which strive for meaningful forms of personal and collective life. … It is an orientation which demands clarity everywhere, but which realizes that the fabric of life can never quite be apprehended. (*Aufbau*, xviii)

At the end of the day, Carnap subscribed to an unbridgeable gap between science and life, when, at the very end of the *Aufbau*, he approvingly quoted the *Tractatus*:

> … We feel that even if *all possible* scientific questions are answered, the problems of life have not been touched at all. Of course, there is then no question left, and just this is the answer. (*Aufbau, § 163)*

For Carnap, the dualism between *Geist* and *Leben* was something that could not be dealt with in a rational, scientific manner, it just had to be accepted as such. Nevertheless, although Carnap and the other members of the Vienna Circle hardly ever discussed explicitly philosophers such as Scheler, Klages, or Spengler[14], who counted as protagonists of *Lebensphilosophie,* there is a curious episode in Carnap's most radical physicalist period in which he sought to employ some of Klages's "results" in graphology to foster his project of the physicalization of psychology. In (Carnap 1932/33) Carnap seriously put forward the claim that the physicalization of psychology had already made enormous progress in the area of graphology, mainly due to the achievements of Klages's *Handschrift und*

13 His radical noncognitivism may be considered as an enduring vestige of this strict separation between "Leben" and "Geist". Still in 1963 Carnap contended that there was no definitive argument in favor or against a democratic or an aristocratic organization of society. Rather, he claimed that it was a matter of one's "character" which one is preferred (cf. Carnap (1963, 1009)).

14 An exception is Neurath's *Anti-Spengler* (Neurath 1921). In this booklet the author straightforwardly attacked Spengler's irrationalism and sought to refute it by rational arguments pointing at its lacunae and non-sequiturs. To put it mildly, the success of *Anti-Spengler* was limited. In contrast, Cassirer in his later writings, e.g. in *The Myth of the State* (Cassirer 1946), showed a much deeper understanding of the role of quasi-mythical thinking in politics.

Charakter (Klages 1920) (cf. Carnap 1932/1933, *Physikalisierung in der Graphologie* 130–136).[15] On the other hand, neo-Kantian philosophers such as Cassirer (but also Rickert) were not prepared to hand over *Leben* and the affairs of social and political practice to irrationalist *Lebensphilosophie*. They sought to come to terms with *Lebensphilosophie* as a discourse that at least partially was susceptible to reasons. In particular, Cassirer vigorously refused to leave the various expression phenomena entirely to the irrational *Leben (*cf. ECN1, *Geist und Leben*).

5. Affinities and Differences

Compared with the often simplistic caricatures of logical empiricism of the Vienna Circle that dominated the opinions of the general philosophical public in later decades Cassirer's image of it was surprisingly modern and detailed – he did not have to wait for modern research of history of philosophy of science to know that the idea of a monolithic Vienna Circle was mistaken. He cleverly spotted the Circle's inner tensions resulting from the different assessments of the roles that perception and logic played as criteria of reality. He set up the following "dialectical" couples: Schlick (objectivistic, "realistic", "rationalistic") vs. Mach (sensualistic, psychologistic), Carnap (formalistic, objectivistic, logicistic) vs. Neurath (empiristic, "anarchistic") (cf. ECN4, 186). While the members of the Vienna Circle always were at pains to mark the allegedly abysmal difference between Vienna and "school philosophy" Cassirer emphasized that, in some respect, there was a close affinity between him and the Viennese logical empiricists:

> With respect to the "world view" ("Weltanschauung"), i.e., what I consider to be the ethos of philosophy, there is no other "school" to which I feel closer than to the thinkers of the Vienna Circle – striving for determinateness, exactness, elimination of the only

15 This was not Carnap's only reference to Klages's oeuvre. Klages's opus maximum (app. 1500 pages) is *Der Geist als Widersacher der Seele* (Klages 1929–1933). Together with Spengler, Klages may be considered as one of the leading figures of the „politics of cultural despair" (Fritz Stern) that plagued Weimar Germany and eventually led to desaster. According to Klages "the essence of the historical process of mankind, often called 'progress', is the victorious battle of the spirit (Geist) against life (Leben) with the logical end of the latter." (Klages (1929–1933), 68). Klages made a great impression on Carnap. In some notes that he had jotted down for a talk in Dessau in October 1929 (RC-110-07-49--1) one reads: "Can science be a guide for life? The answer will be No. Or does the spirit kill life? Also No. … Klages 'Leben' contra 'Geist'. If the powers of life are mighty enough, they need not fear the spirit (Goethe)." Traces of Klages's "characterology" may still be found in his later works when Carnap referred to the individual's "character" as the main source of his moral convictions (cf. Carnap 1963, 1009). Around the same time, Neurath pursued the abstruse (and eventually abandoned) physicalist project to translate Freud's psychoanalysis into a physicalist language. A mischievous observer might have come to the conclusion that in the 1930s physicalism had a curious inclination toward pseudo-sciences.

subjective and "feel-good philosophy", application of the analytical method, rigorous conceptual analysis – these are all requirements that I also recognize. (ECN4, 206)

Nevertheless, Cassirer pointed out, there remained fundamental differences between him and the Viennese thinkers in what they considered as the *task* of philosophy. For the logical empiricists of the Vienna Circle philosophy was philosophy of science. In Cassirer's terms, for them, philosophy was restricted to *Erkenntniskritik*. In contrast, his own conception of philosophy was much more comprehensive. As the texts of ECN evidence Cassirer took into account virtually the entire range of philosophical currents in German philosophy, from Klages to Carnap, Husserl, Scheler, Heidegger, so to speak, to say nothing about his literacy in linguistics, theory of art, psychology, and ethnology. Compared with this wide spectrum that of the Vienna Circle's was utterly narrow. From the 1930s onwards the Vienna Circle's attitude became more and more that of a philosophical movement that had largely lost interest in the theses and opinions of those who did not belong to the movement. Traditional philosophical currents were routinely disqualified as "metaphysical" without further discussion. This did not exclude the possibility of forging strategic alliances when this appeared to be expedient, but, by and large, the members of the former Vienna Circle were sure they were standing on the right, anti-metaphysical side. The verdicts on metaphysical aberrations basically remained intact, even if they underwent some verbal cosmetics insofar as allegedly non-empirical and non-analytical assertions were no longer harshly dismissed as meaningless, but classified as "cognitively meaningless". Even after the turn to "tolerance" none of the usual suspects was acquitted.

A certain shortsightedness in Cassirer's perspective of the Vienna Circle may be seen in the fact that he considered physicalism as an essential trait of logical empiricism. Nevertheless, he had taken notice of *Syntax* (ECN 4, Footnote 539) and explicitly admitted that his critique of a dogmatically physicalist empiricism no longer was applicable to this version of the allegedly new "tolerant" empiricism. To me, it seems doubtful whether the new tolerance announced in *Syntax* had any measurable effect on the Carnap-Cassirer debate.[16]

Be this as it may, there remained other essential differences between Carnap's logical empiricism and Cassirer's critical idealism that survived the abandonement of strict physicalism. According to Cassirer, logical empiricism was deeply "un-Kantian"[17] in that it put foundational "structure" at center stage, neglecting the role of "function". In contrast,

16 In contrast, Carus recently proposed to interpret the Carnap of *Syntax* as the founding father of a new kind of philosophy based on the notion of tolerance and characterized by an irreducible plurality of conceptual frameworks, each of which being allowed to flourish in its own right (cf. Carus 2007).

17 Anti-Kantianism was especially virulent among the members of the "left wing" of the Vienna Circle, i.e., Neurath, Frank, and Hahn. But also for Schlick quite a lot of anti-Kantian statements can be found.

[w]e emphasize the functional side, not the foundational side, but of course we do not deny the necessity of a base. In this respect we are really empirists. All our activity never leads us beyond the basis in an absolute sense, it leads us to orientation, articulation, "structuring", and systematization of the base. On the other hand, we point out that this structuring is not given as such, but constituted by certain "functions" – it has not only to be found, but constructed. (ECN4, 215)

The principle of the primacy of function over structure is just another formulation of the basic principle of the "transcendental method" characteristic for Marburg neo-Kantianism in general. According to it, philosophy does not operate in empty space but had to rely on the historically established facts of science, language, ethics, art, religion, and myth that provided it with its proper content. The task of philosophy is to "justify" these symbolic productions of the human spirit by elucidating their basic assumptions and principles thereby understanding and making proper sense of them. Thereby, along with the function of cognition the philosopher had to strive to understand the functions of linguistic thinking, mythical and religious thinking, and the function of artistic perception, all of which disclosed to humanity not substantial different worlds but rather different ways of world making – to borrow a phrase from Nelson Goodman who may be considered as Cassirer's most kindred spirit among analytic philosophers.

In Cassirer's philosophy of symbolic forms the critique of reason becomes a critique of enlightened culture, i.e., a culture for which science plays a pre-eminent role but which does not neglect the other symbolic forms. As Cassirer pointed out, the concept of culture, however, cannot be detached from the fundamental forms and directions of human activity: in the general framework of a philosophy of culture "being" can be apprehended only in terms of "doing".

As has been observed by many authors, in the logical empiricist account practical and pragmatic aspects of science have remained strangely underdeveloped. According to the Viennese conception, philosophy and science were essentially theoretical. Arguably, in its most radical form, this claim was put forward by Carnap (cf. Carnap 1934, 1963), but in the final analysis, practical reason in some classical Kantian sense did not exist for virtually all members of the Vienna Circle.[18] Instead, practical problems were ultimately relegated to the realms of "character", *Lebensgefühl*, and merely instrumental rationality (cf. Carnap 1963). Cassirer had a more comprehensive idea of philosophy and its role in the ongoing struggle for a rational and enlightened society.

18 This contention needs some further arguments, in particular, for the case of Neurath, whom many consider as the representative of a full-blown pragmatist philosophy of science. Evidence for the claim that Neurath's "pragmatism" was perhaps less pragmatist than often believed is the fact that he sided with Carnap against Morris when a reconciliation of logical empiricism and American pragmatism was discussed on the *International Congress for Unified Science* that took place 1935 in Paris (cf. Mormann 2012).

It may be tempting to somehow relate the virtual debate between the Vienna Circle's logical empiricism and Cassirer's critical idealism to contemporary philosophical debates dealing with the relation between analytical and continental philosophy in our time. I'm not sure whether this is really useful. In particular, it may be rash to interpret the Cassirer-Carnap debate as an early attempt to overcome the allegedly obsolete gap between continental and analytic philosophy. Rather, taking notice of Cassirer's immensely rich philosophical legacy, as is now possible in the excellently edited volumes of ECN, constitutes in itself a philosophical pleasure that no one should forego, who has more than a slight interest in the thought of one of the great figures of 20th Century German enlightenment philosophy.

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Emergence – still trendy after all these years

Corradini, A., and O'Connor, T. (Eds.), (2010). *Emergence in Science and Philosophy*, New York: Routledge

Macdonald, C. and Macdonald, G. (Eds.), (2010). *Emergence in Mind*, Oxford: Oxford University Press.

1. Emergence: an inherently interdisciplinary topic

Ever since the heyday of British Emergentism in the late nineteenth and early twentieth centuries (notwithstanding a relatively silent period for a few decades after British Emergentism fizzled out in the 1930s), discussions of emergence have been a fairly constant source of titillation as well as controversy and confusion. Different authors have used the terms "emergence" and "emergentism" to characterize a myriad related but distinct conceptions, spanning fields as various as physics, chemistry, biology, sociology, psychology, robotics and philosophy.

The basic challenge confronted by all of these authors, in rough terms, has been to articulate how novel and unpredictable phenomena may arise within a system and be dependant upon and constrained by the lower-level constituents of the system while still having some significant form of autonomy. This sort of intellectual balancing act has been thought to be especially (but not exclusively) important for understanding how the phenomena of life and mind are related to the physical, bodily structures in which they arise. Given the obvious importance as well as the inherent interdisciplinarity of this topic, it is encouraging to observe that cooperation among researchers from various fields has been intensifying in recent years. Two recent volumes of collected papers on emergence (Corradini and O'Connor 2010; Macdonald and Macdonald 2010), which will be the focus of this review essay, display and support this welcome trend aptly. Both volumes include contributions by philosophers interested in developing their ideas in a way that both draws upon and can inform empirical science, as well as contributions by empirical researchers committed to taking empirical findings from other fields into consideration and to granting philosophers an important role in clarifying concepts and distinctions, and in facilitating discourse across disciplinary boundaries. The two volumes also complement each other nicely, with one of them – edited by Antonella Corradini and Timothy O'Connor – providing an overview of current research on emergence in philosophy and science more generally, and the

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other – edited by Cynthia and Graham Macdonald – focusing more narrowly on the connection between emergence and the nature and status of mind.

The very concept of emergence is notoriously protean, employed in subtly different ways by different authors. Indeed, this is so well-known that a great many authors have sought to bring order into the discussion by systematically distinguishing different concepts of emergence. Unfortunately, no single taxonomy has "emerged" that everyone could agree on. Although a pessimist might take this lack of consensus at the very conceptual foundation of the discussion as a sign that there has been no progress, it is probably misguided and unfair to expect such a consensus – at least not until many more specific substantive issues have been resolved. Rather than starting out with a consensus concerning the concept of emergence and then looking for empirical examples and working out the details of the definition later, it is equally likely that the real work of attaining a consensus will occur with respect to choosing and analyzing empirical examples and working out conceptual details, and that the general concept(s) of emergence will gradually take shape over time. Nevertheless, even if no single concepts or taxonomy of concepts can be expected at this stage, it would be helpful in cultivating a fruitful context for communication and collaboration to have an overview of the issues, questions and terms that are of interest to most or all researchers involved in the discussion. I would therefore like to begin by pointing out a number of these core issues, questions and terms, and to use them as a backdrop for the ensuing review of the articles collected in these two volumes.

2. Core issues, questions and terms

Epistemological versus ontological: Is emergence an objective feature of the world or merely an epistemological phenomenon resulting from practical limitations upon technology or upon our ways of learning about and/or representing the world? Although the concept of emergence is, as already noted, protean and controversial, this distinction features explicitly or implicitly in almost all accounts. The same goes, in fact, for the following distinction.

Synchronic versus diachronic: To characterize a phenomenon as diachronically emergent can mean, for example, that its appearance in a system S cannot be predicted beforehand on the basis of what is known about S. Once it has appeared and been observed on some number of occasions, it may be possible to formulate a law that will make it possible to predict the appearance of similar phenomena in similar systems. If so, the phenomenon can now be explained by appealing to that law and is therefore not synchronically emergent. Thus, synchronic emergence is generally taken to be a stronger form of emergence than diachronic emergence.

Autonomy: A central issue in discussions of emergence is of course the notion of autonomy that can be ascribed to emergent phenomena. It is generally agreed

that an adequate notion of emergence should balance dependence upon more basic phenomena with a measure of independence or autonomy. That autonomy can be explicated by appealing to various notions, such as inexplicability, unpredictability, non-reducibility, novelty, conceptual distinctiveness, functional distinctiveness, multiple realizability, supervenience, and downward causation. This is by no means an exhaustive list of notions appealed to in characterizing the relevant notion of autonomy, but it should pick out the most prominent notions that are at the core of the discussion. For our purposes, it is useful in thinking about and assessing different proposals to bear these notions in mind and to consider how any particular proposal relates them to each other.

Categories of emergent entities: Another highly important issue has to do with the category or categories of entity that are regarded as candidates to be emergent. In other words, is it substances that emerge? Or properties of substances? Or perhaps processes, patterns or laws?

Scope: A further important question has to do with the scope that is claimed for emergence or emergent phenomena. Sometimes, theorists appear to take it to be a widespread phenomenon characteristic of many kinds of complex system in nature, while others regard it as a more specific hallmark of living systems or of consciousness. Limiting emergence to living or to conscious systems may, on the one hand, be attractive insofar as it could potentially fulfill the ambition of identifying some specific distinguishing criterion for living or conscious systems, thereby providing a substantive underpinning to the intuition that such systems are in some particular sense unique in nature. On the other hand, a broader concept of emergence would presumably be fruitful in a wider range of scientific contexts, and thus more useful overall.

Levels in nature: Many or most theorists advocating emergentism assume the existence of various levels in nature. These levels are generally taken to have specific kinds of objects, properties and laws, and to stand in a hierarchical relation to each other, i.e. from the most basic, physical level to the more complex levels associated with life, consciousness, and the social world. It will be necessary to consider what criteria can be appealed to in distinguishing such levels, and also whether such a hierarchy of levels is an objective feature of the world, a product of our contingent interests and cognitive and bodily abilities, or disciplinary boundaries existing in science at a particular historical moment.

3. Corradini and O'Connor

Corradini and O'Connor's collection is divided into three sections. The articles in the first section, entitled "Emergence: General Perspectives", are intended to provide an overview of recent developments in debates about fundamental issues surrounding emergence.

In the first article in the collection, "The Secret Lives of Emergents", Hong Yu Wong defends emergentism against some criticisms developed by Jaegwon Kim, but also articulates reasons for expecting the scope of emergence to be more restricted than is generally appreciated. According to Kim, emergence must be given a deflationary, epistemological interpretation, because no sense can be made of the notion that emergent properties exercise causal influence upon the basal levels from which they emerge – i.e. downward causation. Wong first takes issue with Kim's argument that the notion of *synchronic* downward causation is incoherent. If an emergent is dependent on a microstructural base consisting of numerous parts, it may appear incoherent to suggest that it could simultaneously exert a causal influence on that same base. And yet, according to Wong, the appearance of incoherence can be dispelled by considering that the emergent may be determined by a core microstructural basis, consisting of some but not all of the parts of its entire microstructural basis, and that the downward causal effects could fall outside the core, namely upon other parts of the microstructural basis. Wong goes on to offer reasons to resist Kim's argument that *diachronic* downward causation would have to involve causal overdetermination in a way that is not consistent with the casual closure of the physical world. Apart from observing that emergentists could simply thumb their noses at causal closure, Wong gives a modal argument to the effect that overdetermination does not entail violation of causal closure. On the other hand, Wong argues that emergence must be restricted to cases where properties not only supervene on basal properties but do so in a way that is fixed by fundamental emergent laws.

The next four articles illustrate two welcome trends in recent theorizing about emergence. First, they abandon the project of identifying a single correct account of emergence, and embrace the plurality of conceptual options, and attempt to make a virtue of this necessity by using various conceptions of emergence to make sense of various kinds of empirical phenomena. Secondly, they are also all marked by a commitment to letting philosophical discussions of emergence be informed and guided by empirical examples, and to endeavour to contribute to ongoing scientific work by honing concepts and distinctions that are likely to be of use to scientists in developing theories and articulating hypotheses.

Carl Gillett's article, like Wong's, engages critically with Kim's position vis-à-vis strong reduction. Gillett, however, starts out by looking carefully at concrete empirical research in a broad range of different areas. For example, he points to the condensed matter physicist Robert Laughlin's work on the phenomenon of "symmetry breaking", whereby "matter collectively and spontaneously acquires a property or preference not present in the underlying rules themselves" (Laughlin 2005:44). In order to account for such cases, in which composed entities non-causally determine the nature of their constituents, Gillett carves out a notion, which he calls "machresis". Machresis presents a kind of strong emergence that flies in the face of philosophical analyses, such as that offered by Kim, that hold strong emergence to be incoherent. The upshot is that we would do well to let our

philosophical theorizing be guided and constrained by thorough consideration of relevant empirical cases.

The articles by Mark Bedau and Michele Di Francesco both make the case that we should give up on the idea of picking out one single, correct account of emergence, and embrace a plurality of concepts of emergence. Bedau distinguished three general types of emergence: nominal, weak and strong. Nominal emergence, for Bedau, occurs whenever logical or conceptual reasons prevent a higher-level predicate from being applicable to the lower-level. It is plain that there are many such properties, e.g. liquidity, but also that it is a fairly trivial sort of the case. Weak emergence occurs, for Bedau, whenever a macro-property could in principle be reduced to a micro-level, but when doing so would be prohibitively complex. Bedau's key term, "explanatory incompressibility", refers to cases in which a reduction to a micro-level could not abbreviate or replace but merely re-iterate all the steps of a macro-level explanation. Like Wong, Bedau maintains that strong emergence, which involves downward causation, is a coherent option, and – again like Wong – he also argues that it is likely to be highly restricted in its scope. In fact, his discussion of strong emergence complements Wong's insofar as he – unlike Wong – focuses on empirical evidence rather than conceptual considerations.

In the final article in the section, Georg Theiner and Timothy O'Connor investigate a particular test case, what they call the "Group Mind Thesis"– i.e. "the claim that groups as a whole can be the subjects of mental states" (78). In order to assess this thesis, they distinguish three conceptions of emergence: First, organization-dependence, which is a concept inspired by Wimsatt's influential work on aggregativity, subsumes various ways in which complex systems and/or their constituents can have properties that depend not only on the properties of those constituents taken in isolation but also but upon the ways in which those constituents relate to each other within the system. Secondly, the notion of an absence of intentional design picks out the feature of novelty often attributed to emergents in a way that is applicable to social groups. Thirdly, mulitiple realizability expresses the failure of reduction that is at the core of the notion of autonomy associated with emergence. They then consider how well those conceptions apply to relevant empirical evidence, and conclude that it can make sense to speak of group minds in the sense of complex distributed cognitive systems, but that the basis for ascribing consciousness to groups is far less sound.

Insofar as Theiner and O'Connor's article is primarily concerned not with emergence as such but with using the concept(s) of emergence to bring clarity into a particular discussion, one may argue that it would have been better placed in the second section, "Self, Agency, and Free Will", which focuses on issues in philosophy of mind and in empirical research areas dealing with questions bearing upon the nature and status of mind.

As it happens, the first three articles in the second section – by E. Jonathan Lowe, Martine Nida-Rümelin and Uwe Meixner – develop novel and challenging

conceptual arguments that buttress realistic positions about the self, subjectivity and consciousness respectively. Although all three of these theorists articulate positions that depart importantly from reductive physicalism, they are at pains to do so in a way that steers clear of extreme anti-naturalistic positions such as Cartesian dualism. The notion of emergence guides all three of these authors by providing a framework in which complex living systems are on the one hand dependent on and constrained by their constituent parts, but on the other hand not fully explicable or predictable on the basis of those constituents. Meixner, in particular, offers a naturalistic explanation of the emergence of a novel, non-reducible kind of entity, namely "non-physical active rational substances", by considering the evolutionary advantage that they would have brought to organisms in which they arose. All three contributions are fresh and thought-provoking, and yet they might have benefited from a careful consideration of the arguments brought forth by critics of emergence, such as Jaegwon Kim. Indeed, all three articles point in the direction of espousing downward causality and thus at least flirting with a rejection of the causal closure of the physical world. A more sustained engagement with canonical criticisms of these positions would be interesting and important – even if only for the purpose of making fully explicit just how they differ from rival viewpoints, and in the best case for articulating the challenge that they present to Kim and other emergence-skeptics.

The following two articles, by Achim Stephan and Mario De Caro, both devoted to the issue of freedom of the will, are more concerned with ongoing empirical research. Stephan discusses three different positions – libertarianism, eliminativism and compatibilism – and seeks to clarify what each position is committed to. Although he considers empirical work bearing upon each, his gloss is that the empirical work is important for clarifying the various positions but cannot ultimately decide among them. This decision, according to Stephan, will depend on more fundamental theoretical decisions we make and attitudes we adopt toward science and its relation to everyday conceptions of ourselves and the world (cf. the "manifest image"). De Caro, on the other hand, engages directly with Libet-style neuroscientific experiments on free will, offering methodological as well as conceptual criticisms that lead him to the conclusion that neuroscientific evidence does not at present provide us with sufficient reason to give up on the commonsensical belief in the freedom of the will. De Caro's discussion is fair and insightful, and yet one is left wondering what motivates his choice of neuroscientific research to engage with. Obviously, it would be too much to ask to review or criticize all the relevant evidence from neuroscientific experiments concerning free will, but one might expect the choice of which research to engage with to be justified or at least clarified. After all, not all neuroscientists espouse the eliminitavist position that philosophers so routinely ascribe to neuroscientists, and which the research discussed here aims to support. Indeed, some researchers (e.g. Haggard 2006) advocate more nuanced positions that philosophers would likely find far more interesting, challenging, and fruitful.

The third section, "Physics, Mathematics, and the Special Sciences", is made up of articles addressing the significance of emergence for assessing the status of the special sciences and their relations to other, more fundamental, sciences (i.e. physics). It opens with an article by Patrick McGivern and Alexander Rueger, which can be said to follow up on Gillett's discussion insofar as it takes a critical perspective upon the supposed truism that stronger forms of emergence are in tension with a physicalist worldview. Thus, they argue that a proper understanding of some physical phenomena, such as heat conduction in a one-dimensional rod, in fact suggests that the emergence of novel and non-reducible phenomena is common. Indeed, they go further than Gillett in giving this kind of emergence an explicitly causal interpretation – i.e. they consider downward causation to be a live option. Interestingly, however, they also point out that the emergent phenomena they discuss do not appear to be marked by any special complexity, and therefore suggest that "the traditional fixation on emergence as associated with complexity is a mirage" (220). This contribution truly fills a gap in the research landscape: given that the commitment to respecting the presumed causal closure of the physical world is a widespread constraint that most philosophers impose upon theories of emergence, it is paramount that that commitment be articulated in a way that is informed by current research in physics. And if a significant number of physicists are critical of that commitment, it is important news that should be reflected upon by philosophers and others interested in emergence.

Articles by Sergio Galvan and Arturo Carsetti also add important and original nuances to the pluralist picture of emergence that is painted throughout the volume by applying emergentism to a very different field from what is usually thought about in connection with emergence, namely mathematics. Alessandro Antonietti, in contrast, focuses on a more standard field, namely psychology, but offers the sort of detailed analysis of concrete cases that is often missing from discussions of emergence in philosophy of mind but have great potential to enrich them.

In the final article in the collection, Antonella Corradini engages with Kim's critical discussion of Fodor's seminal defence of the autonomy of the special sciences. She acknowledges that Kim's analysis picks out a tension inherent in non-reductive physicalism between, on the one hand, committing to the view that micro-physical facts determine all facts, and, on the other hand, defending the autonomy of the special sciences. Her solution is to drop the former commitment and develop a position that defends the autonomy of the special sciences by embracing downward causation. It is surprising, then, that she does not engage with interventionst accounts of causality, which would seem to offer her an attractive option in charting conceptual space for considering taking mental causation seriously within a naturalistic worldview.

Indeed, it is odd that none of the contributions in this volume discuss interventionst theories of causality, as the broad and plural notion of causality that they enable has buttressed at least two of the theoretical projects that resound throughout the collection: de-fanging Kim's causal exclusion argument, and thinking critically

about the commitment to causal closure. Indeed, in Macdonald and Macdonald's collection, to which I now turn, interventionist theories pop up in a number of places.

4. Macdonald and Macdonald

Macdonald and Macdonald's volume has a narrower focus than Corradini and O'Connor's, concentrating upon issues surrounding emergence in philosophy of mind as well as the sciences of mind. One virtue of this collection is that (nearly) every article is followed by a commentary written by an expert in the field. By enabling this dialogue to take place, the editors go beyond the mere organized presentation of perspectives and findings that a collection normally achieves and in fact make a positive contribution to ongoing scientific work. In addition, this feature makes it easier for non-specialists to pick out the central issues and to engage critically with the articles.

Tim Crane's contribution brings helpful clarity into the debate by analyzing the relations among reductive physicalism, non-reductive physicalism and emergentism. Crane argues persuasively that non-reductive physicalists must find a way to close the "explanatory gap", i.e. to explain correlations between lower-level and higher-level entities rather than merely accepting them with natural piety. If they don't do so, their position appears to be at serious risk of collapsing into emergentism.

The next four articles, all focusing in one way or another on causality, add grist to the mill of emergentists. Timothy O'Connor and John Ross start out by arguing that Kim's causal exclusion argument is implicitly committed to a "causal-powers metaphysics", which locates causality in ontologically primitive causal powers of particulars. Making this commitment explicit, they go on to argue (also contra Shoemaker's defence) that non-reductive physicalism is not a sustainable position, and maintain that the most plausible option for accounting for conscious intentional and phenomenal aspects of the mind is a strong emergentism that rejects causal completeness and even the thesis that mental properties are realized by physical properties. Their account nevertheless remains committed to the unity of nature insofar as emergent properties do not magically appear out of the clear blue but result from the actualization of dispositional properties of the components of complex systems, which cannot be inferred on the basis of observations of those components outside of the particular organization in the complex system in question.

Paul Nordhoof introduces a particular distinction between non-reducible but broadly physical properties and emergent properties. Whereas the former may be highly difficult to derive from or explain by appealing to their narrowly physical basis, the latter are linked to their physical basis only by an inexplicable

nomological necessity. He then develops a counterfactual theory of causation, which he uses to demonstrate that not all emergent causation involves property causation. Looking at some examples from the philosophy of mind, he articulates reasons for being skeptical about claims of the latter kind of causation.

In the next paper, Peter Menzies and Christian List home in on a weak point in Kim's criticism of emergence, namely on the lack of a clear and well-motivated account of casuality to undergird the crucial causal exclusion argument. Menzies and List draw upon Woodward's interventionist theory of causality, and argue that higher-level properties can be said to be causally efficacious when certain conditions are met. The upshot is that those higher-level properties must be insensitive to their realization basis to a sufficient degree that they constitute the most specific causes that can be intervened upon in order to modulate a particular class of effects in predictable ways.

Developing an innovative account of events, Cynthia and Graham Macdonald argue that one event can exemplify two different properties, i.e. a mental and a physical property. This enables them to defend a strong emergence that allows mental property *instances* to be causally efficacious. Their conclusion – contra Kim – is that downward causation does not entail violation of causal closure.

Robin Hendry's contribution complements these other generally pro-emergentist articles by looking at evidence from chemistry that, on his construal, does not support the thesis of causal closure of the physical. Like the article by Patrick McGivern and Alexander Rueger, this contribution is important in bringing to light the possibility that causal closure may need to be examined more closely.

Achim Stephan's contribution to this volume, like his contribution to the last, attempts to apply emergentist concepts to an issue in philosophy of mind, namely to the free will debate. Again, he reviews a range of positions, observing that the resolution of the debate will depend (at least) upon assessing the viability of psychological reduction. His very sensible, if unexciting, conclusion is that it is not presently possible to decide the matter.

David Papineau's contribution is more critical of stronger forms of emergentism than most of the others in the collection. His main target is the Fodorian account of the autonomy of the special sciences, which, he argues, entails a feature that appears highly implausible. The Fodorian account, of course, is that higher-level predicates can be multiply realizable, and that the realization base can be so various that the generality expressed by a law linking the higher-level predicate to its effects would not be captured by a translation into the physical language applicable to the realization base. Papineau's concern is that it appears quite mysterious that all of a range of different set of physical realizers would all lead to the same result if they did not have something in common by virtue of which they led to that effect. If, for example, it were a true generalization that reheated Brussels sprouts gave rise to inflamed knees, we would expect to find some process by virtue of which this were the case. If not, we would find it very strange. On Papineau's view, it does not seem wise to bet the autonomy of the special sciences upon the

prevalence of this sort of non-reducible generalization. It is a virtue of Papineau's discussion that it also includes a positive account of when we might expect to find such generalizations, namely when selection pressures act upon different physical states to yield the same effects. The trouble is that these physical states will have little else in common, thus undermining the possibility of projecting knowledge about one to the others. As a result, the states figuring in such generalizations will not be strong candidates for the status of natural kinds.

The collection closes with a very interesting article by Philip Pettit, which, like the article by Georg Theiner and Timothy O'Connor in the other collection, seeks to assess the applicability of emergence to groups of individuals. His question is whether and when it can make sense to speak of group agency. He diligently lays out an account of what goes into sustaining the modicum of rationality that must be present in order to be able to apply the concept of agency to a system. He emphasizes that agents must keep track of the relations among various propositions that they endorse in order to avoid contradictions that would undermine rationality, and comes to the conclusion that this appears to depend upon the presence of some feedback mechanism that garners and compares information from the entire system and makes that information available to processes that can monitor the entire system in order to pick out any dispositions that threaten the rational integrity of the system.

5. Summing up

As already noted, both of these two volumes exemplify and further a recently intensifying trend toward more interdisciplinarity in research on emergence. Given that commitment to such a project entails confronting and embracing the diversity of interests, aims and backgrounds among the various researchers involved in the emergence discourse, it is perhaps unsurprising that the affirmation of a plurality of concepts of emergence should be as clear and thoroughgoing as it is throughout these two volumes. Nevertheless, it is important to note that this meta-theoretical position marks a departure from the philosophical attitudes that have prevailed until recently. Thus, these two collections must be credited with identifying and supporting an important recent development.

It is also worth emphasizing that the contributions in these two volumes make a strong case that causal closure needs to be examined more closely. This challenge to the causal closure thesis is motivated both by theoretical considerations stemming from the development of interventionist accounts of causality (in particular in Macdonald and Macdonald's collection), and also from careful consideration of empirical evidence. Philosophers have for the most part accepted causal closure as a constraint to be worked around rather than questioned, but it must after all be evaluated in light of empirical evidence – and, as Hendry and McGivern and

Rueger point out, it is not presently clear that the weight of the evidence supports this claim.

Another important recent trend that these volumes have integrated is the application of the concept of emergence to groups of people and to social interactions. This theoretical move has gained in popularity in recent years, partly due to the development of enactivist accounts of cognition in general, and social cognition in particular. According to enactivist approaches, cognition occurs when autonomous systems actively regulate their interactions with the external world in such a way that the conditions for their own continued existence are maintained (Varela et al. 1991, Thompson 2007). Applying enactivism to social cognition, some theorists argue that social interactions sometimes satisfy these criteria and thus count as autonomous systems that perform (social) cognition, and that there is a range of cases in which the social cognition performed by such emergent systems (i.e. by interactions) 'replaces individual mechanisms' (De Jaegher et al., 2010, p. 441) and thus cannot be accommodated by individualist accounts. There are questions about the viability and generalizability of such accounts, and also about the claim that such "emergent systems" really are as autonomous from the individuals comprising them as these theorists maintain (Michael 2011). A different way of applying emergentist ideas in conceptualizing the relationship between social interaction and social cognition without marginalizing individuals or individual cognitive processes has been articulated by Somogy Varga (2011). Varga argues that normal individual development builds upon emergent systems spanning multiple individuals. At any rate, these various intriguing and challenging theoretical developments demonstrate the fruitfulness of emergentist ideas to groups and social interactions. One can only hope that the proponents of these approaches will benefit from the contributions in these two volumes.

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Anne Siegetsleitner (Ed.), *Logischer Empirismus, Werte und Moral*, Wien–New York: Springer, 2010.

As the programmatic declarations of the "scientific worldview" show, not all the members of the Circle of Vienna devoted themselves to pure epistemological inquiry on the "icy slopes of logic". Otto Neurath, Rudolf Carnap, Hans Hahn and others had a passionate interest for the social, cultural and political life of the German speaking countries. Though the debate within the Circle revolved entirely around the 'new logic', the philosophy of mathematics, the analysis both of scientific and common language, the "protocol sentences" and "physicalism" and, last but not least, the enigmatic statements of Ludwig Wittgenstein's *Tractatus*, most of the members of the Circle were deeply committed – as Carnap emphasized in his late *Autobiography* – to socialism and to political projects. In the preface to the *Aufbau*, Carnap expressed his awareness of the cultural and ideological implications of the "scientific worldview" as well as of the practical significance of his polemic against metaphysics and antiscientific tendencies widespread in Germany and Austria. According to Carnap, this "new style of thought" was linked to a "new style of life" in economical, social, cultural and political terms. A rationalistic way of thinking exemplified by the architectural movement of *Bauhaus* was now emerging and in the framework of a scientific-oriented worldview the hope for a democratic organisation of society became conceivable. Neurath, the "big locomotive" of the Circle and the most influential spokesman of the "left wing" of logical empiricism in Vienna, is known to have held quite similar and perhaps even more radical beliefs. The history of the "forgotten Vienna Circle" is indeed the history of this framework and of intellectual adventures which ought to be considered – as in the case of Carnap, Neurath, or Philipp Frank – within a larger context and not as mere philosophical projects.

Recent scholarship on the Vienna Circle has clearly shown the importance and the fruitfulness of such an approach. Today the received view of the Vienna Circle no longer holds ground and nobody would tell the story of the logical empiricism by reducing the social and cultural engagement of his members to a simple biographical detail. Nonetheless, some relevant aspects of this story still require more in-depth study. In particular, the received view is misleading with regard to the role played by ethics, values and moral conceptions within the Circle. As Anne Siegetsleitner rightly states in the opening essay of the highly stimulating collection of studies about *Logischer Empirismus, Werte und Mora*l that she has edited as volume 15 of the "Veröffentlichungen des Instituts Wiener Kreis", two main opinions have for a long time dominated the traditional interpretation of this issue. On one hand, it is commonly maintained that logical empiricists were in no way very interested in ethics. But this is unfortunately wrong: moral engagement and scientific activity were complementary aspects of the cultural and philosophical scene shaped by the Vienna Circle (Anne Siegetsleitner, "Logischer Empirismus, Werte und Moral: Anmerkungen zur vorherrschenden Sicht", p. 13). Moreover,

R. Creath (ed.), *Rudolf Carnap and the Legacy of Logical Empiricism*,
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this scene was itself the result of a long history, as can be seen in the example of the "Ethische Gemeinde" founded by Friedrich Jodl and others at the end of 19th century. Still active at the time of the first Austrian Republic, this "society" aimed mainly at providing spiritual guidance (*Seelensorge*) in an attempt to respond to the practical needs of man and to secularize moral convictions (see Sonja Kato-Mailáth-Pokorny, "Die Ethische Gemeinde in Wien – Politik und Ethik während der Ersten Republik", pp. 61-80). On the other hand, logical empiricism has dealt with ethics not only on the basis of a rough version of emotivism as depicted by Alfred Jules Ayer in his influential book *Language, Truth and Logic* (1936). Quite the contrary, the problem of values, the normative dimension of moral and the practical commitment of philosophy undoubtedly figured on the agendas of the Vienna Circle (Anne Siegetsleitner, *Logischer Empirismus, Werte und Moral: Anmerkungen zur vorherrschenden Sicht*, pp. 15-17). As Edgar Morscher suggests, all of the members of the Vienna Circle were convinced that humanistic and moral beliefs played an important role in the battle against traditional metaphysics (Edgar Morscher, "Metaethik – Feind oder kritischer Begleiter von Moral und normativer Ethik?", p. 25). On the basis of this perspective, which contends old prejudices and long accepted 'paradigms', it will be also possible to sketch a new image both of logical empiricism and ethics within scientific philosophy, by proceeding from a historical and philosophical approach that aims to reconsider the usual view of this golden age of philosophy in the 20th century.

First of all, it is noteworthy that Moritz Schlick, the leading figure of the Vienna Circle, was deeply committed to ethics throughout his intellectual development – which in fact begins with his book on the *Lebensweisheit* (1908), indeed the starting point both of his "philosophy of life" and his ethics of pleasure (see Th. Mormann, "Zwischen Weisheit und Wissenschaft. Schlicks weites philosophisches Spektrum", in: *Grazer Philosophische Studien*, 80, 2010, 263-285). Schlick's position was for sure quite different from the radical vision explicitly near to socialism which was endorsed by the left wing of the Circle, namely by Neurath and, to some extent, by Carnap and Frank. Nevertheless it would be difficult to deny that Schlick considered ethics as a central point of 'philosophical activity' in Wittgenstein's sense, i.e., as analysis of moral concepts and propositions that would enable one to clearly show how and why the statements of ethics may be or not be meaningless. Schlick defends a similar view in the programmatic presentation of his *Fragen der Ethik* (1930), the most important and unique ethical book that appeared in the Vienna Circle's heyday. Yet there are striking differences between Schlick and Wittgenstein. As Dietmar von der Pfordten emphasizes, Wittgenstein's skeptical attitude towards ethics as a normative discipline was joined with a radical non-cognitivistic, meta-ethical conception, based on a sharp, ontological distinction between state of affairs and values – a conception well documented in his Cambridge lecture on ethics (1929) and only later modified within the new framework of language as a game, which also allowed for significant moral sentences (Dietmar von Pfordten, "Höchster Moralismus und tiefste Skepsis gegenüber der

nomativen Ethik – Zu Wittgensteins Metaethik", pp. 55-56). Schlick's insight was, in contrast, a cognitivistic one, aiming to stress that ethical propositions have a theoretical content or, more precisely, that ethics is a *theory* and represents a form of knowledge, the knowledge of human moral behaviour. Moreover, according to Anne Siegetsleitner Schlick didn't renounce any normative component of ethics: as the last chapter of the *Fragen der Ethik* suggests, the ethical ideal of goodness is in no way a pure psychological fact or the object of a conceptual description, but first and foremost a normative guide for human action ("Schlicks Fragen der Ethik und die vorherrschende Sicht logisch-empiristicher Ethik", pp. 131-155).

There is no doubt that Schlick's ethical views stood in sharp contrast to Carnap's and Neurath's, or more generally with the "left wing" of the Circle. However, here, too, we are dealing with a rather complicated philosophical story. Thomas Mormann attempts to make plausible that even in the *Aufbau* Carnap was influenced by Heinrich Rickert's theory of values; and it seems to Mormann that only later, more precisely between 1928 and 1932, Carnap abandoned all commitment to the problematic of values, endorsing instead a radical, non-congnitivistic ethical position. According to Mormann, this turning point was motivated by a kind of philosophical disagreement Carnap had with himself, namely with his neo-Kantian and *lebensphilosophisch* orientated thought of the early twenties. At the core of this disagreement was the question of solipsism and, through the intellectual exchange with Neurath, the other important question of physicalism, namely a theory of constitution starting from a materialistic option. The final outcome of this philosophical Odyssey was Carnap's strict dichotomy facts vs. values as well as his rejection of values as meaningless concepts or metaphysical entities: this finally paved the road to Carnap's radical ethical non-cognitivism (Thomas Mormann, "Wertphilosophische Abschweifungen eines Logischen Empiristen: Der Fall Carnaps", pp. 81-102). Is such a reconstruction of Carnap's detachment from values indeed convincing? The answer that Thomas Uebel gives to this question is only in part affirmative. In a very well documented paper Uebel suggests that already in the *Aufbau* Carnap no longer adhered to Rickert's neo-Kantianism; and values, in particular, had long since lost any objective character in Carnap's eyes. The non- cognitivistic turn was justified, on the other hand, by the ideological orientation Rickert manifested in the meantime, exhibiting a strong sympathy towards the *völkisch* atmosphere and the myth of "Blut und Boden" that became increasingly rampant in German culture in the early 1930s. Carnap's polemic against metaphysics was also a polemic (clearly supported by Neurath) against Rickert's defense of values as unhistorical, timeless entities and yet considered as a typical 'German' cultural legacy (Thomas Uebel, "BLUBO-Metaphysik: Die Verwerfung der Werttheorie des Süddeutschen Neukantianismus durch Carnap und Neurath", pp. 103-129).

The volume edited by Anne Siegetsleitner is an important contribution to scholarly research about the Vienna Circle and has the great merit of also opening up new directions for future inquiry. The essays by Jan Radler, Wolf Kellerwessel,

Anne Siegetsleitner / Hannes Leitgeb and finally Elisabeth Nemeth about Victor Kraft, Herbert Feigl, Karl Menger and Philipp Frank (regarding his affinities with Ernst Cassirer) complete the unusual picture of logical empiricism from the point of view of ethics and moral questions. All these papers deserve further discussion and detailed analysis, which unfortunately is impossible in a short review. It is important, in any case, that the volume attracts serious readers not only among scholars working on the Vienna Circle, but also within contemporary debates on ethics and moral philosophy. We are still dealing, today and after the Vienna station, with a philosophical challenge that involves ethics *and* science, moral *and* knowledge, human needs *and* worldview.

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Matthew Eve and Christopher Burke (Eds.), Otto Neurath: *From hieroglyphics to Isotype. A Visual Autobiography*. London: Hyphen Press 2011.

Our knowledge of polymath Otto Neurath's multifaceted and voluminous oeuvre in economy, sociology, philosophy of science and visual education has continued to grow. It was not until the mid-1990s that a full version of his manuscript "Visual Education" previously known only in an abridged form was published for the first time in the Vienna Circle Yearbook.[1] And now, more than 65 years after Neurath's death, there is even a complete text version of his visual autobiography, which had previously only been published in parts. Neurath, however, wrote both texts simultaneously between December 1943 and December 1945 (he died on the 22nd). Both are complementary key works which are essential for an understanding of his approach to visual communication and picture pedagogy.

Editors Matthew Eve and Christopher Burke have examined and compared different versions of the text held by the Otto & Marie Neurath Isotype Collection at the University of Reading. In the end, as Eve explains in the preface, they opted for Neurath's fourth draft and also consulted a later version rewritten by Paul Rotha and Marie Neurath in 1946 and 1947. Christopher Burke has written a very profound introduction that discusses the context of the manuscript's emergence. Additionally he adds some enlightening reflections on Otto Neurath's childhood and the rarely addressed issue of his Jewish background (he was the son of an assimilated Jew, economist Wilhelm Neurath). Moreover, he provides some

1 Otto Neurath, *Visual Education. Humanisation versus Popularisation*, in: Elisabeth Nemeth and Friedrich Stadler (Eds), Encyclopedia and Utopia. The Life and Work of Otto Neurath (1882-1945) (Vienna Circle Institute Yearbook 4). Dordrecht, Boston, London: Kluwer Academic Publishers 1996, pp. 245-335.

useful information on Neurath's approach to the particular text type of an "autobiography". Remarkably enough, he had planned another autobiography (which remained unrealized) and wanted to co-write a dialogue memoir together with his American cousin Waldemar Kaempffert. Aside from that, Burke also stresses the significance of Neurath's approach to visualisation for educational reform, which was definitely not a mainstream approach in Red Vienna where his project emerged. In the particular cultural context of poorly educated masses and partly illiterate people, however, it turned out to be a very effective instrument for democratizing knowledge.

What can we learn from the well-edited text and the excellent introduction accompanying it? It starts with a chapter on "Why I am writing a visual autobiography" that is followed by four more or less chronologically ordered sections portraying the forerunners and the development of Isotype (International System of Typographic Picture Education) in Neurath's life. There are, I would argue, three points in this simply written but nevertheless very sophisticated text that are worth emphasizing.

First, the manuscript contains valuable information not only on his life but also on the history of the Neurathian concept of visual education. Or to be more precise: it addresses the question of how his biography relates to the development of Isotype. His "visual career" started when he was a child. He occupied himself with "shinies" and various forms of visual arrangements and became acquainted with the illustrations in books such as the Berghaus-Atlas or Humboldt's Cosmos. He explains precisely and understandably in what ways certain visualisations influenced the subsequent making of Isotype. He points out that there were two currents: one just apperceptive (looking at pictures, etc.) but also another one that was mainly active and combinatory (putting together different visual elements). It was this latter kind, however, that he was to practice almost throughout his entire life. Beyond that the political and cultural context may also have played a role in the fascination that visual communication exerted upon him. The Austro-Hungarian Empire with its many different nations and languages seemed to stress the need for visual forms of communication that were able to transcend and overcome the different cultural frontiers within pre-war society.

Second, the manuscript also includes relevant biographical information. Neurath repeatedly stresses his father's crucial role in his intellectual development. His father's enormous and legendary library contained about 13,000 volumes he was free to use. Beyond that he impressively describes the different influences and forms of (even scientific) knowledge he was interested in as a child. Already as a boy he attended university extension lectures and was fascinated by popularized scientific knowledge. The young Neurath grew up in a scholarly home, which seemed to have had a rather liberal atmosphere and was definitely not authoritarian when it came to educational matters. Already as a boy, he had the opportunity to become familiar with the Viennese museums (such as the art history museum where he became acquainted with Egyptian wall paintings) and the intellectual

climate of the Viennese coffee houses. His parents, moreover, always seemed to support his early ambitions. At least until his father's death in 1901, Otto Neurath lived under comparatively privileged circumstances, in social, educational and especially cultural terms.

Third, the crucial connection between pictures and education, between visualisation and educational reform becomes strikingly transparent. For Neurath, visual techniques should always be of educational value regardless of their particular aesthetic form. In contrast to any form of picture pageantry, he always raised the question: what effect do pictures and visualisation have? These are the sort of questions that concerned Neurath in all his writings on picture statistics and visual education – from his beginnings in Vienna to the final days in Oxford. Even if people do not consciously recognise certain items in pictures or differences between different images, he was convinced that they might *feel* differently when beholding them and therefore realise the difference between the well-made visualisations and the bad ones. He was convinced that the decisive element is educational purpose and not aesthetic beauty.

Despite these undoubtedly excellent qualities, there may be some objections to Neurath's approach. As in many of his other visual educational writings, he stressed the neutral character and the neutrality of picture language (in comparison to written and spoken language) even in his visual autobiography. But he hardly addressed the problem of the manipulability of pictures themselves and in particular contexts. From a present-day perspective, however, this optimistic approach has been challenged substantially by pictorial criticism and 20th century historical experiences as well. A second problem concerns the perspective of the history of Isotype. In contrast to other writings on the topic (including those of Neurath), the emergence and development of Isotype seems to be first and foremost related to an individual person. The importance of team work and the collaboration of many other people in Vienna, The Hague and Oxford are reflected to a much lesser extent. This may be a necessary consequence of the type of text (visual autobiography) but could nevertheless make a false impression on people not familiar with Neurath's work and the history of visual communication.

Nevertheless, it is of particular importance that this text is now accessible to a wider community interested in Neurath and in questions of the history of visual education and graphic design. The way in which this book is made, moreover, contributes to a broad reception. It has been published by Hyphen Press, which also recently published the edition of Marie Neurath's hitherto unpublished text "The Transformer" (together with Robin Kinross).[2] In a way, these two books excellently complement each other. In both texts, Neurath reflected in a highly sophisticated way on the relation between democracy and "humanized" knowledge and its use for participation and deliberation. Most of the pictures Neurath selected

2 Marie Neurath and Robin Kinross, *The Transformer. Principles of Making Isotype Charts*. London: Hypen Press 2009.

to accompany the text are printed in excellent reproductions and, if they were not available, quite similar ones were selected. An appendix, moreover, portrays Neurath as a collector and enables the reader to study the rich and colourful collection of graphic material he left. To a great extent, this brilliantly designed book follows Neurath's initial plans. Although we do not know how the book would have looked if Neurath himself had finished it, this edition with its superb design and outstanding introduction (which is available for quite a reasonable price) would have certainly met with his approval.

Günther Sandner (Wien)

Giovanni Vailati, *Logic and Pragmatism. Selected Essays by Giovanni Vailati*. Edited by Claudia Arrighi, Paola Cantù, Mauro de Zan, and Patrick Suppes. Stanford: CSLI, 2010.

This book is a collection of some of the most important essays by the Italian historian and pragmatist philosopher Giovanni Vailati (1863–1909). The volume includes nineteen essays by Vailati. There are also two introductory essays. The first, entitled "Life and works of Giovanni Vailati", is by Paola Cantù and Mauro de Zan. The second, entitled "Reflections on Valilati's pragmatism", is by Maria Caamaño and Patrick Suppes. The volume includes an extensive Bibliography and an Index.

This book has many merits. It is the first time that an edited anthology in English has been devoted to Vailati. Thus, one would hope that the book will contribute to enriching the international debate about one of the most prominent figures in the history of contemporary philosophy. But there is much more. The book has the merit of including a ground-breaking essay by Vailati on the relevance of the history of science for philosophical discussions about the nature of science ("On the importance of research regarding the history of science", pp. 3-21). In fact it has not always been appreciated how extensive Vailati's knowledge of the history of science was and how deeply it informed his views about the nature of science. This deeply felt need for integrating the history of science and the philosophy of science, which runs through most of Vailati's philosophical output, is all the more remarkable when one considers that subsequent philosophy of science, especially analytical philosophy of science, lost sight of the importance of the history of science for the philosophy of science.

Paola Cantù and Mauro de Zan offer the reader a thorough introduction to Vailati's life and works. It is impossible to do justice here to the wealth of informa-

tion and analyses that Cantù and de Zan masterfully combine in their introductory essay. Hence I will simply summarize their main points. Cantù and de Zan begin by discussing Vailati's education and early academic activity in Turin, focusing on the scientific, philosophical and historical reading material that would have been accessible to Vailati in the Turin intellectual milieu. Next Cantù and de Zan move on to Vailati's formative collaboration with Giuseppe Peano and the landmark journal *Rivista di matematica*. One has to remember that Vailati got a degree in mathematics before turning to philosophy and history of science. The next two sections of Cantù and de Zan's essay focus on Vailati's inaugural lectures on the history of mechanics and his ground-breaking essay on the importance of the history of science already mentioned above. The next two sections are devoted to two essays by Vailati, the first to "On the deductive method" (1897), the second to "On the role of language in the history of science and culture" (1898). Subsequently, Cantù and de Zan describe the research done by Vailati outside of the academic world while he was a high school teacher in Sicily and Lombardy. Cantù and de Zan also emphasize the importance of Vailati's participation in international conferences, and his realization that at the turn of the twentieth century the intellectual geography of Western civilization was rapidly shifting. Subsequently, Cantù and de Zan focus on the emergence of the Italian pragmatist movement with the appearance of the "Pragmatism Club" in Florence, where Vailati spent some time while associating with Giovanni Papini and Giuseppe Prezzolini. Finally, Cantù and de Zan discuss Vailati's research and social-political activities in the field of pedagogy, his pedagogical theories and educational projects. Cantù and de Zan conclude their introductory essay by summarizing as follows. "In the variety, if not serendipity of Vailati's interests, there are some important themes to be traced: one is surely Vailati's adhesion to pragmatism […]. Another main theme can be detected in Vailati's interest for the definition of concepts. This 'pragmatist' theme is related to Peano's logical inquiries on the topic, but it also deeply connected to several aspects of Vailati's own research, and especially to the interpretation of the common features of pragmatism and mathematical logic." (p. lix)

I invite the reader who wishes to complement this introductory essay by Cantù and de Zan with further up-to-date information about Vailati scholarship to consult the website of the "Centro Studi *Giovanni Vailati*" maintained by Mauro de Zan (http://www.giovanni-vailati.net/).

The second introductory essay by Maria Caamaño and Patrick Suppes appears to me much less felicitous than Cantù's and de Zan's. I am not saying that Caamaño and Suppes do not offer interesting and valuable insights into Vailati's own philosophical work. On the contrary, they pinpoint important issues that are certainly central to Vailati's cultural project. What I am suggesting is that their approach is vitiated by a tendency to measure Vailati's achievements in the light of subsequent developments in contemporary analytical philosophy ~~de~~velopments which are then taken by Caamaño and Suppes as the norm with which Vailati's work should be compared and contrasted. In other words, Caamaño and Suppes have a ten-

dency to read Vailati in an anachronistic fashion that often obscures Vailati's work rather than clarifying it. For example, in the section entitled "Some anticipatory achievements in the development of semantics", Caamaño and Suppes assert that "the ideas underlying Vailati's discussion of such issues turn out strikingly valid according to current developments in the philosophy of language" (p. lxxviii). But we are then left in the dark about the extent to which current developments in the philosophy of language are valid and why they should be regarded as such. There is an underlying preoccupation on the part of Caamaño and Suppes with making Vailati appear "up to date", so to speak, and in line with recent developments in analytical philosophy. The fact is that, in my view, Vailati's integrated work in philosophy and history of science, whose originality was rooted in his profound knowledge of the history of philosophy and especially of the history of ancient and early modern science, cannot be reduced to the narrow interests of later analytical philosophy, especially analytical philosophy of science. The latter appears to me to have lost touch with the historical developments that have shaped Western intellectual civilization over the last two and half millennia. This state of affairs would appear to Vailati as highly unsatisfactory. A more helpful approach would have been to compare and contrast Vailati's philosophical work with the intellectual, social, religious and political context in which it evolved.

The translator of Vailati's essays, Claudia Arrighi, has done a superb job of rendering into English Vailati's complex but fascinating prose. Interestingly, she reports in the "Translator's Notes" that as "Patrick Suppes pointed out in one of our exchanges, the sometimes impenetrable style of thinking and writing in German philosophy of the 19th century has often been commented on. This tendency was present in more philosophers of the last half of the 19th century than we care to mention, not only those writing in German but also in Italian, French, and English. Vailati was affected by this style of writing, even if much less so in his thinking" (p. xv).

I would like to offer a courteous but strong rejoinder to this comment by Suppes on Vailati's prose. Suppes is certainly right in his remarking that nineteenth century German philosophical prose was impenetrable at times, and I would add that it is striking and bizarre how this virus has infected much of the thinking of much of the analytical philosophy against which Vailati's thought is infelicitously measured by Suppes and Caamaño. However, Vailati's prose is far removed from the impenetrable style of writing in late nineteenth-century German philosophy that Suppes has pointed out. Indeed, Vailati's prose is always crystal clear, though complex and elegantly articulated, and has a classical quality in the construction of sentences which is reminiscent of the best Greek classical writers. A far cry from whatever bad style can be found in nineteenth-century German, or any other philosophical prose.

This issue of prose leads me to my final comment on this remarkable book. It is to be hoped that this anthology will eventually stimulate analytical philosophers to read Vailati in his original Italian language, and to revisit the framework of

ideas that informed his thinking. It is so unfortunate that contemporary analytical philosophers seem to have lost that passion for languages that was so characteristic of the Renaissance and of the early modern epoch. Ignorance of languages cannot but result in fuzziness of thinking.

Paolo Palmieri (Pittsburgh)

The Significance of the Hypothetical in the Natural Sciences. Ed. by Michael Heidelberger and Gregor Schiemann. Berlin: De Gruyter, 2009, ISBN 978-3-11-020694-4.

It is worth starting this review by pointing out that the word 'significance' in the title of this collected volume should be read as 'philosophical significance'. The book concerns the philosophical discussion of the hypothetical dimensions of scientific statements and theories that arose around developments in modern science from the 17th century onwards. It is well suited to those interested perhaps in the background of topics that occupied Vienna Circle participants, and of course the origins of modern philosophy of science, particularly the modern scientific realism debate, which is in some ways the more familiar theme that this book tackles through the notion of the 'hypothetical'. One article, that of Esfeld, indeed shows how this hypothetical nature can be extended to metaphysics itself through a realist account. This is quite a shift. As Bartels points out in his contribution whatever 'realism' might have meant even 50 years ago is not the realism of today. For philosophers like Duhem and Popper the easy separation we now make between the hypothetical nature of science and the question of the truth or otherwise of such hypotheses was not so apparent. Indeed one of the principal insights to emerge out of the combined works in this text is that our modern notion of science as hypothetical and its philosophical significance, cannot be historically extrapolated backwards. The interplay of hypothesis and philosophy is complex and contextual. Freudenthal in her paper on Maimonides discusses the historiographical boundaries of such accounts in this respect, which are well followed by the rest of the authors.

Schiemann contends in his paper on Heisenberg's epistemological response to the emerging physics of the 20th century, that there are really two modern distinct meanings of the notion of hypothesis. The first refers to a statement that is unverified but considered verifiable, the second to a statement that is unverifiable in principle. The major historical assertion that emerges out of this collection of articles is that it took time for the notion of natural science as hypothetical in

either of these senses to emerge and to be distinguished. Schiemann's own assertion through Heisenberg historical conception of theories is that this philosophical work was a late 19th and early 20th century project. Most of the papers act as good support for this conjecture. Rainer Sprecht for instance in his contribution on the British empiricists, reveals that the distinctions between rationalism and empiricism were only so clearly formulated after the work of Boyle and Locke, influenced as they were by Gassendi. Locke himself looked to metaphysics through the impetus of God and of man's innate nature in order to justify empiricism as naturally suited to our capacities, and reject our ability to ever access true particulate causes. Only later in the 19th and 20th centuries when the successes of science could be enrolled to support a hypothetical approach, could empiricism stand on its own feet. Snyder in her paper on British philosophy in the 19th century takes heed with the tendency amongst historians to place thinkers like Whewell and Herschel as hypothetico-deductivists, when the more contextually accurate description places them in continuity with Bacon's inductivism, much closer to Mill. Snyder identifies their inductivism with their principled belief in the necessity of inductive reasoning in the production of hypotheses. The reach of Bacon was long in this respect, and it is a mistake to ascribe modern notions of hypothesis to these actors that supposedly anticipate Popper.

The rise of hypothetical thinking hence had a different later source. It took its shape against the background of rational mechanics on the continent, and was the outcome in this respect of new thinking about the role of mathematics in physical accounts of nature. Helmut Pulte in his study of Carl Neumann's "Principles of the Galilean-Newtonian Theory "reveals the early currents of thinking that were to reconceptualise the application of mathematics as hypothetical. Neumann in this respect precedes Mach, and Poincaré. He was in turn influenced by C. G. I. Jacobi who had to some extent intuited Popper's own hypothetico-deductivist theory. As Pulte puts it, from the perspective of rational mechanics axioms were held as formal principles of organisation rather than principles with empirical content, and the whole system was held together by logical coherence rather than by 'material' truth. Jacobi according to Pulte was the first to argue that the epistemological standards applied to a formal theory of pure mathematics like number theory, should not be that applied to the mathematical-deductive system of mechanics. Neumann takes this further, moving much closer to something that looks like Popper's theory, insisting on the arbitrariness of the first principles of such a mathematical system. Successful testing itself can never justify a dogmatic attitude towards these principles. One can also see this transitional development somewhat in Hertz's own philosophical theories, but as Huettemann documents, while an important figure in the later 19th century, he shouldn't be interpreted as a stepping stone in the increasing hypothesization of science, as Boltzmann thought. Rather his work is more demonstrative of the complexities of thinking about this issue for actors at the time who didn't have this distinction. Hertz's pluralism is not easily fit in any category.

It is in fact perhaps thus no surprise that a significant portion of collection concerns Poincaré, who employs the term hypothesis extensively, and gives his own taxonomies of them. Poincaré represents a junction of various emerging themes from Hertz, Kant and others, pulling together not only underdetermination and structural realism, but also the two senses of hypotheses mentioned above. These papers examine both the content of his perspective and its proper historical situation. Heinzmann argues that despite the Kantian influences on his thinking, Poincaré is firmly non-Kantian in his belief that all types of hypotheses, whether conventional or not, are empirical. The use of word "hypothetical" to describe convention was thus not ill-chosen on his part. For Walter, Poincaré's overarching aim in promoting the hypothetical view of science, was to defend Galilean relativity, identifying his principle of physical relativity (covariance with respect to certain group formations) as the kernel of any space-time theory, Lorenzian or Galilean. Again such choices are not conventions at the outset but hypotheses that are later transformed as such. It was wrong for contemporaries of Poincaré to treat Galilean space-time as if it had been empirically disproved given the general acceptance of the Lorenzian model. Both could be fit to the available data when modifications of other physical principles were allowed. Both were hypotheses of equal standing empirically. The acceptance of the Lorenzian model had rather to be understood as a conventional choice, not as a justified truth.

Showing again the complexity of these issues when interwoven with the philosophical opinions of the context, Nordmann and Bouriau discuss the role of pragmatism in the development of concerns with the hypotheticity of science. On the pragmatic viewpoint, hypotheses are part of scientific professes that serve to generate the world. Nordmann discusses Charles Sanders Pierce engagement with the rising appreciation of the hypotheticity of science. Pierce saw his view as an antidote to the anti-realism becoming increasingly popular at the time. Bouriau raises the issue of the potential pragmatic interpretation of Poincaré when compared to the contemporaneous work of the French philosopher Vaihinger. The comparison with Vaihinger is enlightening in this respect. Both had according to Bouriau strong pragmatic elements underlying their position on the roles of theories and their relations to reality. Hypotheses were not for either to be judged or assessed simply on their truth-value, rather than their practical operation as principles for producing scientific theories. Nonetheless it seems Bouriau would come down in favour of reading Poincaré like Vaihinger, who talked of hypotheses as fictions. Hypotheses are statements about a mind-independent reality, whether true, false or unknowable. The balance of these accounts fits Poincaré within a movement for the increasing hypothetization of science and distinction between scientific statements and their truth.

This narrative that the hypothetical image of science linearly emerged from the developments of the late 19th early 20th century uniquely at least is not shared by the all the papers. McMullin argues that the treatment of science as hypothetical, albeit in an inconsistent fashion, was part of the break with Aristotelian

traditions that formed the scientific revolution and may well be its most defining element. However more modern familiar sounding notions of Boyle, who precipitated the transformation of hypothesis to something provisional or transitory must be balanced against the integration of the notion of Descartes with his own first principles philosophy and emphasis on explanatory strength. McMullin documents what might be the shifting understanding and appreciations of hypotheses through the 17th century. Newton however set aside the role of hypotheses in the *Principia*, devaluing it to the status of a query, thus setting the scene for the anti-hypothetical philosophy of rational mechanics.

The most interesting paper however is probably that from Heidelberger who discusses the philosophical position of Emile Boutroux of the latter half of the 19th century. Although Heidelberger wants to trace its influence to Poincaré, as he fairly notes it has modern resonance with the discussions on the disunity of science and the abstract nature of laws of nature. Boutroux denies the necessity of mathematical principles in nature and in turn the hierarchy of science back to physics. Sciences are rather driven by their own conceptual frameworks, making higher sciences intrinsically autonomous. Heidelberger compares his views on laws to Nancy Cartwright, but I would also mention the perspectivism of William Wimsatt as a useful comparison here too.

This consideration of Boutroux raises perhaps my main complaint about the text. This is the lack of consideration of the higher sciences, and the philosophies of its practitioners, including Whewell and Mill, who clearly had biological classification in mind in their thinking, or the British (like Darwin) and the German biologists (like Haeckel) of the 19th and early 20th century. This restricts the perspective of the book to a very traditional one, invoking what is a very traditional history of philosophy of science, to be corrected perhaps about the edges. It is one that of course puts mathematical physics at the centre of the discussion. No doubt modern philosophy of science was heavily influenced by this picture, but this just constrains the historical picture by modern concerns. In any case, theories like Boutroux's have their own modern resonance. To neglect these other perspectives on hypotheses seems to miss an opportunity to add to our knowledge of the history of philosophy of science in this respect, where the notion of hypothesis was centrally important if not more so.

This raises the other point, which is that the papers centre around questions that more broadly concern philosophy of science, like realism and instrumentalism, rather than actual practices. This puts the focus on the more refined elements again of mathematical physics, and the attendant philosophies like conventionalism that arose in particular response to it, whereas the more interesting and significant aspects of scientific practice in general are what Poincaré calls verifiable hypotheses. This would require I think a closer account of what scientists were doing and the strict methodologies they applied. Such a project would of course test whether the philosophical accounts of these mainstream figures corresponded

to what was really going on or were led by metaphysical presumptions and debates stemming from particular rarefied contexts.

Nonetheless the collection sticks to its themes well, and raises good evidence for the origins at the turn of the 20th century for the hypothetical image of science (where scientific statements and theories are considered hypotheses) and its various contingent causes. This image we now largely take for granted and project back through the history of science, yet it was far from obvious for earlier thinkers about science, and had its contingent and complex origins in the scientific developments of the late 19th and early 20th century.

Miles MacLeod (Altenberg, Austria)

Karl von Meyenn (ed.), *Eine Entdeckung von ganz außerordentlicher Tragweite. Schrödingers Briefwechsel zur Wellenmechanik und zum Katzenparadox.* Springer-Verlag: Berlin Heidelberg 2011. vol. 1, xix + 1-508 pages, vol. 2, xix + 509-929 pages, ISBN 978-3-642-04335-5.

It no longer seems timely to speculate about future editorial work with the scientific correspondence of eminent scientists and the fate of their electronically stored communication – especially with the promising claims made by the managers of institutional repositories of knowledge about the never-ending capability to store and retrieve electronic information for the next millennia. Paper belongs to the past. The carefully composed handwritten letter including drawings and formulas that informed colleagues about the latest deliberations and speculations is a social technique of scientific communication that is now history. Future scholars of the history of science will no longer have to decipher unreadable handwritings or private shorthand (as in the case of Ludwig Boltzmann or Erwin Schrödinger). They will print out the correspondence or even read it on a screen – a distinctly non-sensual experience compared to one's struggle to decipher an old-fashioned letter. Of course even this view simply reflects a romanticism, and is just as outdated as the hand-written letter.

Probably, Karl von Meyenn, physicist and historian of science, belongs to the species of those old-fashioned scholars who can derive many years of pleasure from mulling over sheets of paper exchanged by the luminaries of 20th century physics. He certainly invested a good part of his scientific career to the monumental (multi-volume) edition of the scientific correspondence of Wolfgang Pauli (together with the co-editors Victor Weisskopf and Armin Hermann). These volumes

have become indispensable sources for the history of physics and in particular quantum mechanics during the last century, serving the scientific community and historians of science. (Meyenn has edited many more books and important source papers, including one on the Forman thesis.)

Here we will report on the recent publication of von Meyenn's long-standing project, the edition of Erwin Schrödinger's scientific correspondence between 1920 and1960: *Eine Entdeckung von außerordentlicher Reichweite. Schrödingers Briefwechsel zur Wellenmechanik und zum Katzenparadox* in two volumes. Chronologically grouped, a selection of 294 letters from Schrödinger and his 34 correspondents are presented in nine chapters following the lines of Schrödinger's distinct periods of life. Two thematic topics served as orientation in incorporating the letters in the edition at hand: the formation of Schrödinger's *Wellenmechanik* and his contributions to the interpretation of quantum physics (in short, Schrödinger's cat). The title of the book under review – *a discovery of quite extraordinary consequences* – is taken from a letter to Max Planck, dated Zürich, 26 February 1926, in which Schrödinger reports on his search for an equation of matter waves resulting in what is today known as the Schrödinger equation. Apart from the many archives, where smaller parts of Schrödinger's correspondence are located as a result of his peripatetic life, the bulk of the letters presented here reside at the house in Alpbach where he spent the last five years of his life and at the Zentralbibliothek für Physik in Wien.

The first three chapters of Volume 1 give an excellent introduction to Schrödinger's life and science from his early days as a student in Vienna to the year 1921 when he accepted a chair at the University of Zürich. All of the following chapters (Volume 1 and 2) are again opened by concise and most helpful illustrations of the main problems in physics that are tackled in the subsequent exchange of letters; certainly these comments do not always make for easy reading, especially for the non-specialist, but they are very rich in details despite the necessary brevity. These introductory notes could very well qualify as a shorter scientific biography of Schrödinger when read together. Von Meyenn's scholarship clearly emerges when he comments in great detail on the content and context of a specific letter; here a breadth of information is provided, highly valuable for all those who want to dig deeper into the very often quite technical questions discussed in these letters.

Meyenn's editorial work is also complemented by a chronology of Schrödinger's life concentrating on his science and followed by an index of Schrödinger's 34 correspondents, with the letters listed in chronological order and the correspondents cited alphabetically. A bibliography of 104 pages, including Schrödinger's works is most welcome, especially since Schrödinger's Collected Papers (*Gesammelte Abhandlungen*. Band 1-4) are out of print.

This edition of Schrödinger's scientific correspondence with many of the prominent colleagues and antagonists of the development of quantum theory over a period of forty years will certainly serve as a major source of reference for de-

tailed studies in the history of physics of the 20th century. The reviewer can refrain from making a special recommendation , since it goes without saying that these two volumes are indispensable for any further study on Schrödinger and his contributions to science. Certainly, an edition of Schrödinger's complete correspondence – similar to that of Einstein's correspondence included in his *Collected Papers* or von Meyenn's edition of the *Wissenschaftlicher Briefwechsel* of Wolfgang Pauli – is more than just a one man enterprise and could keep a team of specialists busy for years. We are grateful to Karl von Meyenn for having presented this scholarly edition of Schrödinger's correspondence.

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and Institute of Contemporary History, University of Vienna

Activities of the Institute Vienna Circle

Activities 2011

International Conferences and Workshops

11th Vienna International Summer University
Scientific World Conceptions (VISU/SWC)
Main Topic: "The Nature of Scientific Evidence"
Main Lecturers: Hasok Chang (Cambridge University), David Lagnado (University College London), Tal Golan (University of California, San Diego)
Venue: Campus der Universität Wien
Date: July 4–15
In co-operation with: University of Vienna, Institute of Philosophy and Institute for Contemporary History
www.univie.ac.at/ivc/VISU

Theorienstrukturalismus – Eine kritische Neubewertung
Workshop of the FWF-research project, 2009–2011
In co-operation with: Universität Innsbruck, Forschungsinstitut Brenner-Archiv
Keynote lecture: Carlos Ulises Moulines (München)
Date: May 5 and 6
Venue: Institut Wiener Kreis
www.univie.ac.at/ivc/haopos

The Philosophy of Science in a European Perspective
Continuation of the 5-year programme of the European Science Foundation (ESF) with 22 countries participating
5 workshops on "Explanation, Prediction and Confirmation"
www.pse-esf.org

Workshop
Moritz Schlick Project: Critical Edition of the Complete Works and Intellectual Biography
Phase 3: 2011–2013
Publication of the writings from the estate (manuscripts and correspondence)

R. Creath (ed.), *Rudolf Carnap and the Legacy of Logical Empiricism*, Vienna Circle Institute Yearbook 16, DOI 10.1007/978-94-007-3929-1,

In co-operation with Moritz Schlick Forschungsstelle, University of Rostock (D) and and the Academy of Sciences Hamburg and the Institute of Philosophy, University of Torino (I)
Date: February
Venue: University of Rostock
www.moritz-schlick.de
www.univie.ac.at/ivc/Schlick-Projekt

Wissenschaftsgeschichte und Wissenschaftsphilosophie
Together with: Deutsche Gesellschaft für Wissenschaftsgeschichte (GWG)
Date: May 19–21
Venue: University of Vienna

International Symposium celebrating the 20th anniversary of the Institute Vienna Circle
Wissenschaftsphilosophie in Europa – Das Wiener Erbe
Philosophy of Science in Europe – The Viennese Heritage
Date: December 5–7
Venue: University of Vienna
Selected papers of the conference will be published in the Vienna Circle Institute Yearbook 17

Lectures and lecture series

19. Wiener (Kreis) Vorlesung / 19th Vienna Circle Lecture
Part of the anniversary event
Hans Jürgen Wendel (University of Rostock)
Über Moritz Schlick
Date: December
Venue: University of Vienna

Philosophy of Science
Lecture series in co-operation with the Institute of Philosophy at the University of Vienna
http://wissenschaftstheorie.univie.ac.at/vortragsreihe/

Wissenschaftsphilosophisches Kolloquium
Weekly lectures on the philosophy and theory of science given by scholars from Austria and abroad.
www.univie.ac.at/ivc/koll/

Publications

Anniversary edition
On the occasion of the 10th anniversary of the Vienna International Summer University / Scientific World Conceptions (VISU/SWC):
Allan Hobson, *The William James Lectures*. With comments of numerous researchers in the Cognitive and Life Sciences. Edited with an Introduction by Owen Flanagan. Dordrecht: Springer 2011

Vienna Circle Institute Yearbook 15
Brian McGuinness (ed.)
Friedrich Waismann – Causality and Logical Positivism
Dordrecht–Boston–London: Dordrecht: Springer 2011

Veröffentlichungen des Instituts Wiener Kreis, Bd. 16
András Máté, Miklós Rédei and Friedrich Stadler (eds.)
The Vienna Circle and Hungary / Der Wiener Kreis und Ungarn
Wien–New York: Springer 2011

Veröffentlichungen des Instituts Wiener Kreis, Bd. 17
Anna Bro~~-ek~~, *Kazimierz Twardowski. Die Wiener Jahre*
Wien–New York: Springer 2012

Moritz Schlick Gesamtausgabe, Hrsg. von Friedrich Stadler und Hans Jürgen Wendel. Wien–New York: Springer Verlag
Abt. I, Band 4: *Zürich – Berlin – Rostock. Aufsätze, Beiträge, Rezensionen, 1907–1916*. Hrsg. von F. O. Engler. 2011
Abt. II, Band 6: *Erkenntnistheoretische Schriften 1925–1936*.
Hrsg. von Johannes Friedl.

Schlick Studien 2: Matthias Neuber, *Die Grenzen des Revisionismus. Schlick, Cassirer und das „Raumproblem“*. 2011
www.springer.com/series/7287

Ernst Mach Studienausgabe in 9 Bänden. Berlin: xenomoi Verlag
Band 2. *Erkenntnis und Irrtum*. Hrsg. von Elisabeth Nemeth und Friedrich Stadler. 2011
Band 3: *Die Mechanik*. Hrsg. von Gereon Wolters und Giora Hon. 2011.
Band 4: *Populärwissenschaftlich Vorlesungen*. Hrsg. von Elisabeth Nemeth und Friedrich Stadler
www.xenomoi.de

Research projects

Research Project and edition:
Moritz Schlick Project: Critical Edition of the Complete Works and Intellectual Biography
Phase 3: 2010–2013

Schriften aus dem Nachlass (Abteilung II). Gemeinsam mit der Hamburgischen Akademie der Wissenschaften. Institut Wiener Kreis/Institut für Philosophie der Universität Rostock
www.univie.ac.at/ivc/Schlick-Projekt/

History of Science and/or Philosophy of Science?
Together with: *Universität Innsbruck, Forschungsinstitut Brenner-Archiv*

www.univie.ac.at/ivc

Preview 2012

12th Vienna International Summer University Scientific World Conceptions (VISU/SWC)
Main topic: "Applied Science. Historical, Epistemological, and Institutional Characteristics"
Main lecturers: Martin Carrier (Bielefeld University), Rose-Mary Sargent (Merrimack College), Peter Weingart (Bielefeld University)
Venue: Campus der Universität Wien
Date: July 2–13, 2012
In co-operation with: University of Vienna, Institute of Philosophy and Institute for Contemporary History
www.univie.ac.at/ivc/VISU

Publications

Anniversary edition
Wissenschaftliche Weltauffassung. Der Wiener Kreis (Wien: Artur Wolf Verlag 1929).
New edition of the manifesto with translations into English, French and Italian
Edited with an Introduction by Friedrich Stadler und Thomas Uebel
Wien–New York: Springer 2012.

Index of Names

Not included are: Footnotes, Tables, References and Figures

R. Creath (ed.), *Rudolf Carnap and the Legacy of Logical Empiricism*, Vienna Circle Institute Yearbook 16, DOI 10.1007/978-94-007-3929-1,