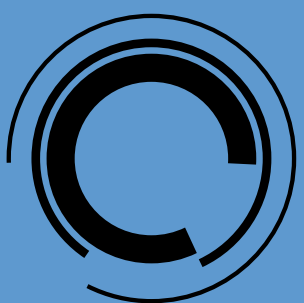
The Vienna Circle in the Nordic Countries

Networks and Transformations of Logical Empiricism

edited by
Juha Manninen and Friedrich Stadler



Vienna Circle Institute Yearbook

Springer

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NETWORKS AND TRANSFORMATIONS OF LOGICAL EMPIRICISM

VIENNA CIRCLE INSTITUTE YEARBOOK [2008]

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Institut 'Wiener Kreis'
Society for the Advancement of the Scientific World Conception

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Juha Manninen • Friedrich Stadler
Editors

The Vienna Circle in the Nordic Countries

Networks and Transformations of Logical Empiricism

Springer

Editors
Jutta Manninen
Helsinki Collegium for Advanced Studies
University of Helsinki
Fl-00014 Helsinki
Finland

Friedrich Stadler
Inst. Zeitgeschichte
Inst. Wiener Kreis
Universität Wien
Hof 1, Spitalgasse 2-4
1090 Wien
Austria
friedrich.stadler@univie.ac.at

ISBN 978-90-481-3682-7 e-ISBN 978-90-481-3683-4
Springer Dordrecht Heidelberg London New York

Library of Congress Control Number: 2009938005

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Printed on acid-free paper

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Table of Contents

B. General Part

Report/Documentation

Review Essays

Reviews

Editorial

Networking was a crucial part of the activities pursued by the community of scholars known as the Vienna Circle. After 1929, the informal discussion group around Moritz Schlick sought to reach increasingly wider audiences through conferences and publications. Even before the Vienna Circle 'went public', it had already aroused interest in the northern countries of Europe. Empiricism was the common ground, soon joined by modern logic. A peculiarity of the Nordic countries and their small universities was that professors of philosophy were usually responsible for psychology as well. This only increased these professors' interest in Vienna; after all Charlotte and Karl Bühler were working there too.

After he had read Hans Reichenbach's *Relativitätstheorie und Erkenntnis A Priori* in 1923*,* Eino Kaila was the first to seek contact with the exact philosophy emerging at the time. Reichenbach's reprints of his papers on probability and induction helped forge a long-lasting tradition around these topics in Finland. In 1926, Kaila coined the name "logical empiricism" to point out the distinguishing features of the new attitude. Correspondence with Schlick and Rudolf Carnap led to Kaila's being invited to engage in discussions with the Circle – both with and without Schlick – during the days the Circle's manifesto was being drafted in 1929. In the Vienna of the early 1930s, Kaila did an empirical study on how young children respond to the human face. Together with his students, especially Georg Henrik von Wright, Kaila was able to make Helsinki a northern center of logical empiricism, to be enriched later by von Wright's own close contact with Wittgenstein in Cambridge.

Danish modernism had been influential all over in the Nordic countries. The radical philosopher Jørgen Jørgensen joined the network of the Circle after publishing a treatise on formal logic in 1931. Important for the philosophy of law was Alf Ross' visit to Hans Kelsen. A number of Danish psychologists were interested in the Vienna Circle, and one of Jørgensen's main works was a study on the biological foundations of psychology.

From the perspective of the Vienna Circle, the famous physicist Niels Bohr had made Copenhagen an especially interesting city. Carnap lectured in Copenhagen on the character of philosophical problems in November 1932, presenting his ideas on logical syntax and/or semantics in transition. He went on to speak in Stockholm, Lund and Gothenburg, as well as Oslo. Two years later Carnap's first publication in English in the new journal *Philosophy of Science* was based on his Scandinavian lecture notes. One central thought was that philosophical "proposals" – inseparable from empirical research – should replace more traditional "theses". Otto Neurath made similar trips preparing the ground for "The Second International Congress for the Unity of Science" on the problems of causality, with

special consideration of physics and biology, held in Copenhagen in June 1936. This conference was the single most important step in consolidating the new philosophical attitude in the Nordic countries, especially among young philosophers.

Sweden had an antimetaphysical tradition, centered in the main university in Uppsala, but this tradition was neither empirical nor logical in the sense of modern logic. Åke Petzäll visited Vienna in 1932. A small book he published presents the results of his conversations, especially with Friedrich Waismann. In 1935 Petzäll launched a new journal, *Theoria*. It proved to be an important forum for the exchange of ideas and criticism between the networks of Logical Empiricism – or: Unified Science – and philosophers from the Nordic countries. In addition, Ernst Cassirer was a refugee in Sweden, and a friend of Petzäll; thus he was able to continue his unique neo-Kantian career and dialogue with the logical empiricists.

In 1934, Arne Naess, a young Norwegian, joined the discussions of the Circle. Together with Ernest Nagel and A. J. Ayer, he became part of Schlick's circle very late in the day. At the "The Third International Congress for the Unity of Science" in Paris 1937 Naess joined Neurath's group for empirical semantics against Carnap's logical semantics. This would later be a line of research for him and his students, although only one line of the many topics he dealt with.. Naess received the only chair for philosophy in Norway at the age of 27, with the help of evaluations from Kaila and Jørgensen. He led a very active life until his death in January 2009.

Shaped by his experience in the resistance movement, Naess was a man who could not be easily pulled away from his convictions. He was active in his own country – unlike the Vienna Circle refugees in the U.S.A. – and thus largely uneffected by the climate of the Cold War. Naess' career seems unusual compared with those who had to leave their home countries. But it can also be a test case of what could have happened if it would have been possible for logical empiricism to flourish in the areas of its origin.

One of the least known networks of the Vienna Circle is the "Nordic connection". This connection had a continuing influence for many of the coming decades, beginning with the earliest phase of the Vienna Circle and continuing with a number of adaptations and innovations well into contemporary times. Some of the individual members of this network are remembered, such as Georg Henrik von Wright. But little attention is now given to the fact that these individual members communicated intensively with each other as well as with the Vienna Circle and its international continuation in the Unity of Science movement.

An attempt to correct the earlier somewhat restricted view of the European perspective of the Circle was made by the Institute Vienna Circle in co-operation with the Helsinki Institute for Advanced Studies, where both of the editors of this volume were able to work together for some time. We also wish to thank the Helsinki Center for Nordic Studies, especially Johan Strang. This co-operation resulted in a symposium entitled "Networks and Transformations of Logical Empiricism: The Vienna Circle in the Nordic Countries", which took place in Helsinki in

September 2007.

The interest aroused by the Helsinki symposium was very encouraging. We are happy to publish in the present volume most of the papers that developed out of presentations in this symposium.

On January 12, 2009 Arne Naess passed away in the age of 96. One of the editors was happy to have met this extraordinary philosopher and man for the last time in Oslo just before the conference took place in Helsinki. After this impressive visit, Arne Naess sent the following message which was read by the organizers at the opening of the conference:

"I was so glad when Friedrich came to see me in Oslo some days ago, more so when he told me about his symposium. My stay in Vienna in the 1930's was a significant time in my life and to be able to attend some of the seminars there played an important role in my development as a philosopher, even as a person.

Probably to console an old man almost 96 years old, Friedrich mentioned the possibiliy of my attending *this* conference. It was a very nice thought and I would have jumped at the offer 10 years ago, or even 5 years ago, insofar as a 90 year old could jump! But no, I can only envy you from afar the chance to inspire one another and to wish you and the conference every success. Arne Naess (Oslo, Norway)"

This volume is dedicated to Arne Naess in commemoration of his unique life and work.

Helsinki and Vienna, June 2009

Juha Manninen
(Helsinki Collegium for
Advanced Studies, University of Helsinki,
and Universiy of Oulu, Finland)

Friedrich Stadler
(University of Vienna, and
Institute Vienna Circle,
Austria)

Friedrich Stadler

Arne Naess – Dogmas and Problems of Empiricism

Arne Naess – A Biographical Sketch

Arne Dekke Eide Naess was born on January 27, 1912 in Oslo. After a long and successful life he passed away on January 12, 2009 in Oslo as the most renowned Norwegian philosopher, where he was honoured with a state funeral. He was one of the most important public figures in Norway and in his later years became known all over the world as a pioneer of the ecological movement.

Given this publicity in recent decades his earlier life was forgotten or obscured in a sense – especially his student years in Paris, Vienna and Oslo before the outbreak of World War II, when he attended the famous Vienna Circle around Moritz Schlick during a stay in Vienna 1934–36. Here he wrote his dissertation *Erkenntnis und wissenschaftliches Verhalten* (Knowledge and Scientific Behaviour) which was published in Oslo by the Norwegian Academy of Science in 1936.[1]

Before the beginning of his unique academic career, he had studied philosophy, psychology, mathematics, and astronomy in Paris and in 1933 he submitted two Master's theses on the concept of truth and behavioural psychological principles (in Norwegian). From 1938 to 1939 he was in Berkeley, where he conducted empirical behavioural studies together with the psychologists Edward C. Tolman and Clark L. Hull. Before, he actively participated in the "Third International Congress for the Unity of Science – Encyclopedia Conference" in Paris, July 1937, where he discussed in some private meetings his conception of truth intensely with Rudolf Carnap, Otto Neurath and others. As a result of his studies in the context of these discussions Naess published *Truth as Conceived by Those Who are not Professional Philosophers* (1938).[2] In parallel he critically dealt with the Vienna

1 Arne Naess, *Erkenntnis und wissenschaftliches Verhalten*. Skrifter Utgitt av Det Norske Videnskaps-Akademi i Oslo. II. Hist.-Filos. Klasse. 1936. No. 1. Oslo: I Kommisjon Hos Jacob Dybwad 1936 (249 pp.).

2 Arne Naess, *Truth as Conceived by Those Who are not Professional Philosophers*. Oslo 1938. I am grateful to Juha Manninen (Helsinki) for indicating to me the context of this publication: cf. his manuscript *Developments and Tensions within the Vienna Circle*. Helsinki 2009. (forthcoming).
The Congress is described in my *The Vienna Circle. Studies in the Origins, Development and Influence of Logical Empiricism*. Wien–New York: Springer 2001, pp. 377-382. The Proceedings of that Congress were published as volume 6 of the series *Einheitswissenschaft/Unified Science/Science Unitaire,* ed. by Otto Neurath and Jørgen Jørgensen. (The Hague 1938). This issue includes a discussion of Egon Brunswik's paper "The Integration of Psychology into the Exact Sciences" by Carnap, Naess and

Circle's doctrine of empiricism in 1937-1939 in his manuscript *Wie fördert man heute die empirische Bewegung? Eine Auseinandersetzung mit dem Empirismus von Otto Neurath und Rudolph Carnap.* (How Can the Empirical Movement Be Promoted today? A Discussion of the Empiricism of Otto Neurath and Rudolph Carnap") which was published in Oslo only in 1956 (Reprinted in Naess 1992 and 2005).[3] In his Vienna times he did additional research with the Viennese psychologist and Vienna Circle member Egon Brunswik (a proponent of the Karl Bühler school), which was to be continued in Berkeley in exile. Before World War II Naess contributed several related articles to the Swedish Journal *Theoria* 1937ff.[4], which was an important forum for the scientific communication between Viennese and Nordic philosophers even during the war-time years.

At the age of 27 Naess was appointed Professor of Philosophy at the University of Oslo in 1939, a chair which he held until his early retirement in 1970. During the War he participated actively in the Norwegian Anti-Nazi resistance movement. After 1945 Naess became head of an UNESCO project on the East-West Conflict (1948–49) and served subsequently as editor of the philosophical journal *Synthese* (1950–1963)[5], while in 1958 he founded and edited the (still existing) interdisciplinary journal of philosophy *Inquiry.*[6]

In this period (1940–1955) Arne Naess became well known as a an activist of the international peace movement and as an adherent of Mahatma Gandhi, before he succeeded as the founder and philosopher of the ecological movement (coining the terms "deep ecology" and "ecosophy") after 1970.

Following his retirement as a professor he travelled a lot as visiting professor all over the world (to Vienna again in 1984 at the Institute for Advanced Studies) and later on, from 1991 on, continued his highly productive intellectual life at the still existing "Centre for Development and Environment" at the University of Oslo.[7]

His numerous publications include some 30 books and a large number of articles, most of them collected in the 10 volumes of the *Selected Works of Arne Naess*

Edgar Rubin. The unpublished manuscripts are located in the Neurath papers (Haarlem, NL): Nr.. 196K. 30-33.

3 Naess, *Wie fördert man heute die empirische Bewegung? Eine Auseinandersetzung mit dem Empirismus von Otto Neurath und Rudolph Carnap*. Oslo: Universitetsforlaget 1956. (48 pp.). English: SWAN VIII, pp.163-216.

4 Cf. the contribution of J. Strang in this volume.

5 *Synthese. An International Journal for Epistemology, Methodology and Philosophy of Science.* Dordrecht: Reidel Publishing Company. The issue in 1971, pp. 348-352, contains a bibliography of the writings of Naess in English and German on the occasion of his sixtieth birthday. Currently, *Synthese* is published by Springer.

6 *Inquiry* is currently ed. by Wayne Martin (University of Essex) and is published with Routledge. On the role of Naess decisive for introducing modern social science in Norway see Fredrik W. Thue in this volume and his *In Quest of a Democratic Order. The Americanization of a Norwegian Social Scholarship 1918–1970*. Oslo 2006.

7 See the website: www.sum.uio.no.

(= SWAN), ed. by Harold Glasser. (Dordrecht: Springer 2005).[8] This edition is an impressive source of Naess's life and work as a reprint of the most important publications together with introductions by the author, which were written by him exclusively for this unique edition project.

Arne Naess And The Vienna Circle / Logical Empiricism

In March 1936 Naess lectured in the Schlick-Circle on "Logic and Scientific Behavior" (Logik und wissenschaftliches Verhalten), presenting the main claims of his forthcoming book *Erkenntnis und wissenschaftiches Verhalten*:[9]

His approach was methodological-psychological, and was an endorsement for basic research fostered by a consistent behaviouristic analysis (language behaviour). It addressed the descriptive and normative perspective and offered a formal calculus of discussion. He also argued for a relativistic and contextual meaning theory, providing rules of discussion. From this followed an anti-foundationalist approach as opposed to classical (propositional) logic principles. It focused on the problem of non-ambiguity, which is not attainable, favoring a sort of sociological behaviourism, e.g., discussing common-sense behaviour. It thus challenged the dualism of an exclusive true-false dichotomy, while maximizing the comparisons of statements according to the principle of intersubjectivity, conceived of as an objective-psychological methodology.

Naess also presented his insight that propositional logic is not applicable to every day life and science, which is based primarily on empirical research on laymen (cf. Naess 1938).[10] By the way, the contested notion of a (scientific) fact shows surprising parallels to the claims of Ludwik Fleck's contemporary sociology of science.[11] As a summary of this presentation one can speak of an early psychological and sociological turn in epistemology and philosophy of science rejecting pure formal logic.

Naess's dissertation *Erkenntnis und wissenschaftliches Verhalten* (Knowledge and Scientific Behaviour) – regrettably not included in his *Selected Papers* – was finished in 1934/35 in Vienna and published in Oslo 1936 by the Norwegian Academy of Science.

8 SWAN = *The Selected Works of Arne Naess*. Harold Glasser, Series Editor. Alan Drengson, Associate Editor. 10 Vlms. Dordrecht: Springer 2005.

9 Naess, „Logik und wissenschaftliches Verhalten". Shortened and reworked manuscript. 18 pp. Vienna Circle Archives (VCA) Haarlem (NL), Neurath Papers R 45c. (18 pp.).

10 Arne Naess, *'Truth' as conceived by those who are not professional philosophers*. Oslo 1938. Det Norske Videnskap-Akademi I Oslo, Skrifter. II. Hist.-Filos. Kl. 1938, No. 4.

11 Ludwik Fleck, *Entstehung und Entwicklung einer wissenschaftlichen Tatsache. Einführung in die Lehre vom Denkstil und Denkollektiv*. Basel: Benno Schwabe 1935. (Frankfurt/M.: Suhrkamp 1980).

In his preface the young philosopher refers to the Vienna Circle members of his generation Rose Rand, Heinrich Neider, and Walter Hollitscher thanking them for their help.[12] In four chapters the book deals with 1. an objective-psychological description of objects and contents of knowledge, 2. existing investigations into the intersubjective and objective-psychological description of the objects and contents of knowledge, 3. the way of behaviour as unity of scientific behaviour, and 4. on the properties of objective-psychological judgments and the development of scientific behaviour in relation to the judgments of classical logic and to the statements of the natural sciences. An overview and epilogue closes this publication in the spirit of the Viennese empiricism.

According to the author[13] the aim of his book was a solution of objective-psychological problems by an objective-psychological description of behaviour and an attempt to "objectivate"(*objektivieren*) the problems of epistemology, psychology of knowledge, and classical logic – similar to Egon Brunswik's work in psychology. The method was an objective-psychological description, which could replace the description of knowledge and contents of objects (despite of difficulties). The conclusion was that any epistemology, the subjective psychology of knowledge, and the theory of meaning can be replaced completely and practically by an objective-psychological description of scientific behaviour (with references to Ernst Mach). Accordingly, there is no room for foundationalism with a logic of inference, but rather a dynamics of models. It is not surprising that Naess underscores the relativity and time dependence of all models of knowledge, referring to a certain frame and context, which, e.g., seems to anticipate Paul Feyerabend's "contextual meaning".

In retrospect Naess wrote on Logical Empiricism in the Vienna Circle as follows:[14]

> My doctoral thesis in philosophy of science was an effort to remind us that in science the content of a theory is not independent of research behaviour – the activities of observing, confirming, disconfirming, and so on, and that these are set within a deep context of place, history, and culture. Later, as a postdoctoral researcher at the UC Berkeley, I studied the behaviour of experimental psychologists doing animal research.
>
> In 1934 and 1935 I studied in Vienna and … became a member of the famous Schlick seminar, the main discussion group of the Vienna Circle. Their quest for clarity and cordial cooperation in pursuit of knowledge led me to appreciate that 'What do I mean?' is an open question. I concluded that we never intend to express anything extremely definite, even in mathematics or symbolic logic. I saw the importance of using empirical methods to find out how we actually use certain expressions and sentences. I developed and applied a wide

12 "Ich möchte hervorheben, daß die Verfasser, die mich bei der Ausführung dieser Abhandlung am meisten beeinflusst haben, zum Teil am wenigsten darin erwähnt sind. Dies gilt für die europäschen und amerikanischen Pragmatisten und für die Verfasser des Wiener Kreises …", Ibid., p. 6.

13 Naess, *Erkenntnis*, p. 243ff.

14 Naess, "Author's Introduction to the Series", SWAN I, 2005, p. lxiii f.

variety of such methods, which became part of the core for empirical semantics that runs through my work. I continued to do this type of research into the 1990s …

And continuing on the background of the dissertation he writes:

In one of my earlier studies, I reviewed about 700 articles from philosophers concerning their use of the word truth. For the most part, I found these unconvincing and soon started on empirical studies of the use of truth among ordinary non-professional people and schoolchildren … Many philosophers seemed to assume that ordinary people hold very naive views about these deep matters. I found through research that, on the contrary, the views articulated by these 'ordinary' people were every bit as sophisticated as those held by professional philosophers. This reinforced my conviction that, generally, we greatly underestimate ourselves. Much academic philosophy was narrowly focused and abstract. …

As a consequence, Naess arrived at his life-long attitude of a value-laden scepticism:

My empirical and historical research led me to realize that there are no certainties and that there is a great diversity in our spontaneous experience as well as endless ways to describe and appreciate the complexities and values of the world. Thus, I realized that I am one of those lifetime seekers that the ancient Greeks called a zetetic … From my research on scepticism and the foundations of science and logic, it became clear to me that pluralism (every event has many descriptions and possible outcomes), possibilism (anything can happen), and a healthy scepticism (always seeking truth but never claiming it) make up the most consistent approach to respecting the perspectives and experiences of others, human and nonhuman.

From my empirical studies of semantics, and from my knowledge of several languages, I came to appreciate the complexity of communication …One of the most important discoveries coming from this research, leading to my major book, *Interpretation and Preciseness* … , was the insight, that we cannot avoid values in any field of endeavour or research.[15]

In 1934 Naess lectured in the Gomperz-Circle,[16] a discussion group around the philosopher Heinrich Gomperz at the periphery of the Vienna Circle. He spoke "on dichotomies of propositions, in particular the dichotomy of 'true' and 'false'" (Über Zweiteilung der Sätze, insbesondere die Zweiteilung in „wahre" und „falsche").[17] After – according to Naess – a theory on an absolutistic notion of reality was skipped, the table of content indicates his approach with an empiricist

15 Ibid.

16 Stadler, *The Vienna Circle* op.cit, p. 442-452; Martin Seiler / Friedrich Stadler (Eds.), *Heinrich Gomperz*, *Karl Popper und die österreichische Philosophie*. Amsterdam: Rodopi 1994.

17 Naess, „Über Zweiteilung des Sätze, insbesondere die Zweiteilung in ‚wahre' und ‚falsche'". (13 pp.). VCA Haarlem, Neurath Papers R 45-46.

and behaviourist theory of science as exemplified by the ways the notions of truth were used and applied:
1. Classical logic, basic research and the notion of truth: theories of truth are constructed and interpreted with a lack of aims. There is an arbitrary and undefined usage; e.g., Hilbert's propositional calculus, and the need for rules for the application of concepts is obvious.
2. A comparison of the notions for statements in different types of text shows that 'true' has no exceptional status: there are many variants of opinions , most of them diffuse and tentative, but with a similar function like "it is true, that …". Empirical semantics documents this communication in every day life and science.
3. The "true/false"-dualism does contribute nothing to linguistics and the practice of science because of an arbitrary and undefined usage that asks for rules of application
4. Rather, the "true/false"-dualism contributes to ontology, especially to absolutistic conceptions of reality. Why then theories of truth and logics in history of philosophy? Which type of logic should be chosen? (Is there any temporal or modal logic?). Without doubt, the notions of truth and reality were important in the history of philosophy (classical logic) for ontological reasons, but leading to generalisations and absolutist interpretations. As a consequence the codification of the dualism with ontological commitments followed.

In his summary Naess argued that in science and every day life the dualism of true and false generated a sort of pseudo-science "logic" comprising the whole set of judgments and disregarding all other possible and applied related notions. There exist many types of concepts and opinions, most of them diffuse and tentative, but with a similar function. An axiomatization of all these usages is impossible and a *choice* has to be made between them – which converged strongly with the empiricist and common sense position held by Neurath and Richard von Mises.
Here the question is whether we are accepting a "pragmatic turn" in logic or succumbing an ideological-metaphysical tendency.

The German manuscript *Wie fördert man die empirische Bewegung? Eine Auseinandersetzung mit dem Empirismus von Otto Neurath, und Rudolph Carnap* was written by Naess in 1937–1939 and published only in 1956.[18] The reason for this delay in publication was according to Naess the lack of knowledge about Logical Empiricism in Norway before World War II. It deals critically with the writings of Neurath and Carnap in the 1930s, but also with some of their later developments. Naess formulated its general aim as an empiricism "without dogma" and with "research", but not with "science" (as scientism). The reworked and expanded English edition of this manuscript appeared as "How Can the Empirical Movement be Promoted Today?" in *From an Empirical Point of View*[19] and was reprinted in

18 Naess, „Wie fördert man heute die empirische Bewegung? Eine Auseinandersetzung mit dem Empirismus von Otto Neurath und Rudolph Carnap". Oslo: Universitetsforlaget 1956. (48 pp.).

19 Ed. by (Ed. by E.M. Barth, J. van Dormael and F. Vandamme. Gent: 1992.

his *Selected Works*[20] (with appendices on references and remarks on the empirical movement).

In this study, Naess made a strong case for empiricism inspired by Neurath's and Carnap's positions regarding physicalism and Logical Empiricism. The chapters will be described as follows:

1. There are no universally valid demarcation criteria.
2. Carnap's and Neurath's demarcation criteria: physicalism
3. Carnap's term *Language*
4. What are the Terms of Science?
5. Carnap's Term *Physical Language*
6. Carnap's Term *Reducible*
7. Concluding Remarks on the Formulation of Physicalism
8. Is it at All Possible to Determine the Meaning of a Statement?
9. Specification of Space and Time Does Not Protect Us from Anti-empiricism
10. Research Programs Instead of Theses. Models Instead of Systems
11. On the Reification ("Vergötzung") of Theoretical Constructs in Psychology
12. Physicalism and Some Proposals Concerning the Technique of Discussing
13. The *Encyclopedia* Project - Its Significance for the Empirical Movement
14. Summary

Appendix I. 1956: Notes and comments on the paper written between 1937 and 1939

Appendix II. Remarks on the empirical movement

The main arguments of Naess are as follows:
– There are no universally valid (demarcation) criteria for decision: Naess is in accordance with Carnap, Neurath, Frank, but raises objections against some of their formulations. Like Neurath he prefers a "behavioristics of scholars" (*Gelehrtenbehavioristik*), versus "pseudo-rationalism" and rejects an "operational view" (P.W. Bridgman).
– Carnap's and Neurath's decision (demarcation) criteria – physicalism:
Naess finds the formulations of physicalism as rather speculative. What is the meaning of "language" and "language of science": it is rather a system of (implicit?) rules for scientific terms. What are the concepts of science? Also reducibility is not useful as criterion for decision making. Therefore, physicalism is not unambiguous, especially without contents (a similar critique like Edgar Zilsel's). Instead, there is a need for proposals, for presentation and discussion, thereby avoiding the "theses-style".
– Can one determine the meaning of a proposition at all?
The case of psychology shows a pluralism of psychologists and theories, questioning the equation of physicalism = empiricism. Regarding statements on "the meaning of a proposition" the Duhem-Poincaré thesis is applicable (cf. Quine)

20 SWAN VIII, pp. 163-216.

and provides a valuable tool against "absolutism" (as "pseudo-rationalism" does according to Neurath), which is therefore indispensable for empiricism.
– Space-time references do not protect us from anti-empiricism because they are not a sufficient requirement for empiricism. This postulate is informed by references to Descartes and Rudolf Steiner.
– Research programs instead of theses. Models instead of systems: The success of physics can be explained by sociology of science, as an argument against too much "systematisation".
– On the idolatry *("Vergötzung")* of auxiliary constructions in psychology (on the reification of theoretical constructs in psychology): In agreement with Philipp Frank's reference to Ernst Mach, Naess supports his fight against the "idolatry of auxiliary terms" (Vergötzung der Hilfsbegriffe). Naess prefers thought-models in psychology and psychoanalysis (which he underwent in Vienna) instead of a "universal science" of biology, sociology and psychology
– Physicalism and some proposals for the technique of discussing: Physicalism may serve as a tool for discussion techniques. Empiricism is the ideal tool and instead of anti-metaphysics rather the notion of a-metaphysics is proposed by Naess.
– The *Encyclopedia* project; its significance for the empirical movement: Naess is in favor of the *International Encyclopedia of Unified Science*, as a further development of an a-metaphysical and empirical movement: "model" instead of "system" is the recommended program as a collective enterprise with a more general approach. Whereas the old French encyclopaedists were an ideological group, the new encyclopaedists are fighting against an "autistic way of thinking" (E. Bleuler). Because there are no universal solutions, only empiricism is the clue for any further progress.
– In his summary, Naess explained how this empiricism was to be promoted (again with the preference for "research" instead of "science") by

a) promotion of collective work, also in isolated fields
b) work on questions related to the unification as a collective enterprise with establishing bridges (*Querverbindungen*)
c) proposals in order to sharpen the tools of language of certain collectives
d) fight against "autistic thinking", and promotion of studies in scientific behavior as a type of "complex behavior"
e) fight against a tendency of insufficient expanding *systems*, instead of *models.* Finally, he argues against all formulations of empiricism, which are definitive or closed: "*This is an expression of the position that the relative persistent manifestation of empiricism is a* general attitude*, but not a* habit of language (*Sprachgewohnheit*)." [21]

Naess continued his critical assessment of Carnap in the *Festschrift* dedicated to the latter with a contribution on "Typology of Questionnaires Adopted to the

21 Naess, *Wie fördert man heute die empirische Bewegung*, op. cit., p. 29.

Study of Expressions with Closely Related Meanings".[22] It is a survey of questionnaires used by Naess in his studies on synonymity, as a contribution to empirical semantics and its terminology. For the adaptation of the semantic concepts of extension and intension to pragmatics (with Carnap and others) a detailed analysis of concrete empirical procedures is desirable. And Naess voiced objections to intensional concepts in agreement with Carnap.[23]

A remarkable transcending of the program of Logical Empiricism can be found in Naess's book on *Four Modern Philosophers. Carnap, Wittgenstein, Heidegger, Sartre*. (1968),[24] which irritated Carnap, but did not lead to a break in their lifelong friendship – as is also apparent in Naess's obituary of Rudolf Carnap in his journal *Inquiry*:[25] On this amusing episode Naess writes retrospectively with reference to the richness of amateur philosophies:[26]

> What about the richness and equivalidity of professional philosophies? Provoked by mutual distrust between existentialists and the analytically minded, I published *Four Modern Philosophers* … , taking not care to reveal any differences in my estimation of Carnap, Wittgenstein, Sartre, and Heidegger. Carnap complained mildly that I had compared him to a schoolboy.

This is not really surprising given Carnap's critique of Heidegger since the 1920s[27] and his refusal to appear in a volume in P.A. Schilpp's renowned "Library of Living Philosophers" if a volume were to be dedicated also to Heidegger in same series.

In this context, the unpublished correspondence between Naess and Neurath (from 1934 to 1945, comprising some 85 letters),[28] as well as between Naess and Carnap (in the 1960s)[29] illustrates the ongoing interaction and communication between agreement and divergence in the context of a scholarly family resemblance.

In 1934, after Neurath was forced into his first exile in the Netherlands because of the Civil War in Austria, Naess wrote to the former from Vienna that he very

22 *Logic and Language. Studies Dedicated to Rudolf Carnap*. Ed. by B.h. Kazemier and D.I. Vuysje. Dordrecht 1962. Reprinted in: SWAN VIII.

23 Carnap, *Meaning and Necessity: A Study in Semantics and Modal Logic*. University of Chicago Press 1956.

24 Naess, *Four Modern Philosophers*. *Carnap. Wittgenstein, Heidegger, Sartre*. Chicago 1968.

25 Naess, "Rudolf Carnap, 1891–1970", in: *Inquiry* 13, 1970, pp.337f.

26 Naess, "How My Philosophy Seemed to Develop", in: SWAN IX (Philosophical evelopment, Environment, and Education).

27 Michael Friedman, *A Parting of the Ways* . Chicao: Open Court 2000. German Translation: *Carnap. Cassirer, Heidegger*. *Geteilte Wege*. Frankfurt/M.: Fischer 2004.

28 Correspondence Neurath-Naess, Vienna Circle Archives (VCA), Haarlem (NL).

29 Correspondence Carnap-Naess, Archives of Scientific Philosophy, University and of Pittsburgh, and Philosophical Archives, University of Konstanz. Thanks for the permission of quoting from these collections.

much deplores Neurath's absence and expressed some deep agreements between Neurath's and his own philosophical positions.[30] This scholarly and personal exchange, enriched during meetings in Copenhagen 1936 and Paris 1937, lasted up to 1945 reflecting the whole terrible war period. It ends with an elucidating and moving correspondence focusing on the life and work of both proponents:

On June 25, 1945, Neurath wrote to Naess from Oxford:

> Now the flood is over. We hope you are in good health. Please write by return. Short information to you: 14th May 40, jumped into a motor life boat; picked up by British destroyer, interned, released, lecturing two terms at Oxford University "Logical Empiricism and the Social Science". Married. New institute founded together with Susan Stebbing … Going on very well.
> Encyclopedia going on well, your half of the paper you are writing together with Br.(unswik) urgently needed. We are now continuing Journal, Library etc. and we hope you will help us, as usual.
> This is only the dove looking out for you, with best wishes, greetings and kind regards (what can I do for you here?), Ever yours[31]

Naess replied with a long and last letter to Neurath, dated August 16, 1945, from his home Holmenkollen in Oslo: he mentions that he had delivered his part of the paper on psychology to Charles Morris already before the war waiting for Brunswik's contribution, but – he continued regarding the *Encyclopedia* project:[32]

> now, my part is to old, and (I) am very happy that it is not published. I am not sure that I can bring the work up to date here in Norway. I will have to discuss that question with Br.(unswik). We will get all books we want from England an(d) America, but when people from those countries ask which books we want, we cannot but answer "those we ought to have"…

With reference to his own war experiences Naess writes impressively:

> I am still somewhat groggy and disheartened because of lost friends and collaborateurs, but I hope soon to recover. The very brilliant young philosopher Ludvig Lövestad died this year. He was my close friend in all kinds of work, also the 'illegal'. He was tortured to death, remaining silent about my hiding-place. Another close friend and collaborateur in philosophy, Wickström-Nielsen, was killed when jumping from parachute. He came from England and jumped with documents and Russell's new book on Truth etc. and Lundbergs new book on the methods of sociology. Also other young people who wished to go on with philosophy and mathematics are missing. This field got an exceptionally hard blow …

30 Naess – Neurath, March 30, 1934; Neurath – Naess, 1934. Vienna Circle Archives (VCA) Haarlem (NL).

31 Here Neurath signed in his typical manner as in most of his letters to friends with an elephant sketched by himself individually according to his mood.

32 Naess – Neurath, August 1945. Vienna Circle Archives (VCA), loc.cit.

Regarding his philosophical work Naess expressed his dissatisfaction with his previous publications:

As an exception I published a book "Filosofiske problemer, deres inndeling og egenart". You certainly understand the title. It has the function to show Norwegian amateur philosophers of the elder or more speculative kind, that their professor is not a "real" philosopher and that they ought not to send me their MS's.

He further on mentions his mimeographed volumes on symbolic logic employing the matrix method for the functional calculus:

Most of my mimeographed works deals with interpretation, precision, and how to detect misunderstandings, the different kinds of misunderstandings and the fraudulent use of ambiguity. I try to work out a scientific description of these things. Logical empiricism and allied tendencies presupposes a moderately precise and sound description of precision.

And remembering the pre-war *Encyclopedia* movement, Naess renewed his former offer as follows:

We were very sad that the Congr. of unif.(ied) sc.(ience) could not be realized here i(n) Oslo, and feel morally justified in claiming the next congress to be here. How about that? … Conclusion: Encycl.(opedia) paper must wait till literature arrived, if haste and you have others who can write, I resign. (Gladly).

But Naess ended optimistically with regard to a possible future cooperation:

Write more about you and your plans. … I subscribe herewith for the journal and other periodicals of central interest. Are there young philosophers or clever philosophical students who could think of a stay at our university? We think to send some to England and America. There are institutions willing to arrange the economic side of the question. With the very best wishes etc., ever yours Arne Ness"[33]

As is well known, these plans could not be realized because of the unexpected early death of Otto Neurath, who died after a heart attack in Oxford in December 1945.

In 1963 Naess questioned Carnap about his long version of his autobiography, and asking him specifically whether he had met Martin Heidegger in his student years and what prevented Carnap from endorsing nationalism, on Friedrich Waismann's lost manuscript "Logik, Sprache, Philosophie", the role of Hans Driesch for Carnap's *Aufbau*, also on Wittgenstein's influence on the Vienna Circle [34]. In his answer (November 19, 1963), Carnap mentioned the impossibility of sending the extended version of his autobiography, that he certainly did not meet Heidegger

33 After his hand-writing signature "Arne Ness" he adds: "e, if you prefer".
34 Naess to Carnap, September 28, 1963.

in his student years[35] and that probably his own war experiences saved him from adhering to any nationalism. Furthermore, Carnap doubted that Driesch himself thought the *Aufbau* was a similar book project to his philosophy. Some years later, in 1967 Naess announced to Carnap the proofs of his *Four Modern Philosophers* and told him about his visit to Los Angeles. In addition, the correspondence documents the efforts of Naess awarding Carnap the honorary doctorate of the University of Oslo and his own visit of Carnap in Los Angeles in 1968.

The participation of Naess in the Unity of Science movement reveals his sympathy with the *Encyclopedia* project but also the divergences from some of its basic commitments: At the 2nd International Congress for the Unity of Science in Copenhagen, June 21-26, 1936 he commented on E. Tolman's Lecture "An Operational Analysis of 'Demands'"[36]. Afterwards he was invited by the editors Carnap, Neurath and Morris to contribute to the first 2 volumes of the *Encyclopedia* on psychology together with Egon Brunswik but because of theoretical differences regarding a broader frame of reference only Naess withdrew and Egon Brunswik later delivered his *Conceptual Framework of Psychology* (1952).[37]

During the 3rd International Congress for the Unity of Science – Encyclopedia Conference in Paris, Sorbonne July 29-31, 1937, Naess had discussed Brunswik's "The Integration of Psychology into the Exact Sciences", together with Carnap and the Danish psychologist Edgar Rubin. Inofficially, he participated in the discussion with Carnap and Neurath, which was not published.[38]

As participant of the 4th International Congress for the Unity of Science in Cambridge (England), July 14-19, 1938[39] Naess spoke "On the Function of Generalization"; and contributed several times with "Remarks on the Overall Discussion on the Lecture of Williams", "On Woodger's lecture", "On the Lecture of Grelling/Oppenheim", "On Kokoszynska's Lecture on Unified Science", and with a "Reply to a Question by Hollitscher in a Discussion".

At the end of the conference he announced an "Invitation to Oslo" for a forthcoming meeting in 1940, which could not be realized because of the outbreak of World War II. The 5th Congress took place in Cambridge, Mass. in 1939 just at he beginning of the War and the 6th and last Congress was held at the University of Chicago, September 2-6, 1941.[40]

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35 This is a surprising note given the fact that in 1929 Carnap attended lectures of Heidegger and Cassirer in Davos and participated in the subsequent discussions. Cf. Friedman, loc.cit.

36 *Erkenntnis* VI, 1936, p.397

37 Naess, "A Philosophical Note on Brunswik's Work", in: Kurt R. Fischer/Friedrich Stadler (Eds.), *„Wahrnehmung und Gegenstandswelt". Zum Lebenswerk von Egon Brunswik*. Wien–New York: Springer 1997, 177f. Egon Brunswik, *The Conceptual Framework of Psychology*. International Encyclopedia of Unified Science I/10. The University of Chicago Press 1952.

38 See also Fn. 2 and below in the subsequent section.

39 In German in: *Erkenntnis* VII, 1937/38, 198-210, 369ff., pp. 382-85.

40 On the history of the Congresses cf. Friedrich Stadler, *The Vienna Circle*, loc.cit, pp.

In parallel Naess contributed to the journal *Theoria. A Swedish Journal of Philosophy and Psycholoy,* edited by Ake Petzäll, Gunnar Aspelin, Konrad Marc-Wogau, Torgny T. Segerstedt. (Gothenburg and Copenhagen). This journal was an important print medium for the "Nordic Connection" between Vienna Circle and the Nordic philosophers before and during World War II. In the 3rd volume we find a review of his own *Erkenntnis und wissenschaftliches Verhalten* [41] to which he replied critically ("Erkenntnis und wissenschaftliches Verhalten. Entgegnung von Arne Ness auf die Bemerkungen von H.J. Pos"[42]). With "Common-sense and Truth"[43] he summarized his claims in the previous aforementioned publications in which he endorsed common sense philosophy. After the War he was again reviewed[44] and he continued contributing with "Toward a Theory of Interpretation and Preciseness"[45] and "Husserl on the Apodictic Evidence of Ideal Laws".[46]

Arne Naess: Late Remembrances of the Vienna Circle

In his first recollection on "Logical Empiricism and the Uniqueness of the Schlick Seminar: A Personal Experience with Consequences" [47] Naess praised the pluralism and style of discussions in the Vienna Circle:

> The central members and visitors of the seminar – about 10 people – were seriously engaged in one and the same great undertaking. There was an atmosphere of eager cooperation. Something very great was built and any cooperation, however modest, was appreciated. There was room for all. Opinions differed, but then it was essential to ask: Is the difference serious? Exactly how serious? Perhaps minor, perhaps all to the good: there ought to be no Gleichschaltung (streamlining of opinions).

And he continued to reflect on the relation between philosophy and ideology:[48]

> A participant puts forward an opinion, using a sentence T. A second participant, probably thinking the opinion is not tenable, interferes , saying "*Würden Sie (würdest du) die Formulierung U akzeptieren?*"(Would you accept the formulation U?) … What struck me as *müstergültig* (worthy of being a model) about this procedure was the effort not to declare lack of agreement before careful verbal investigation so that undesirable effects of termino-

328-393.

41 H.J. Pos. Vol. III/1937, 117-124

42 Vol I/1938, pp. 62-68.

43 Vol. IV/1938, pp. 39-58.

44 *Filosofiske problemer* (1941) by Paul Lindblom. Vol.II/1942, 181f. and *Wie fördert man heute die empirische Bewegung?* by Eivind Storheim. Vol.II/1959, 187-191.

45 Vol. 1949, pp.220-241.

46 Vol I/1954, p.53-63.

47 Ness 1993, p. 11. Reprint in: SWAN VIII, pp. 261-278.

48 Ibid., p. 12.

logical idiosyncracies were eliminated, and the choice of conciliatory, building up the other way of clarification.

And finally Naess turned to the culture of communication which he characterizes as a unique "Gandhian non-violent approach", which was lost after 1938:[49]

> Looking back I feel sorry that the combined analytical and social initiative of the logical empiricists petered out. It constituted in the 30's a cultural force … When Quine and others took over the analytical leadership, the movement was largely robbed of its social and political aspects.

Some years later, in 1997, Naess reported on his cooperation with the psychologist and philosopher Egon Brunswik (1903–1955) in his short "Philosophical Note on Brunswik's Work".[50] Brunswik had published in Vienna *Wahrnehmung und Gegenstandswelt. Grundlegung einer Psychologie vom Gegenstand her* (1934), influenced by Brentano, Bühler, as well as neo-behavorism and Logical Empiricism, which converged in an "ecological psychology" of humans and their natural environment. He can be seen as a pioneer of "probabilist revolution" in psychology (having invented key notions such as "vicarious functioning" and "lense model" of perception, "representative design").[51] Naess mentioned him because of "the psychological *actuality* of my close friend Egon's work", who "showed us a new way of transcending Cartesian subject/object dualisms, and contributed to a new form both of behaviourism and gestalt thinking".[52] And he concluded, that Brunswik's "psychology from the point of view of the object may be interpreted as a psychology conceived of as a subject/object neutral *field*."[53] From 1934 on Naess and Brunswik had planned a joint contribution on psychology to the *International Encyclopedia of Unified Science*, which – in the end – failed as a consequence of differences on the significance of empiricism. The monograph appeared only in 1952 authored by Brunswik solely under the title *The Conceptual Framework of Psychology*.[54] In 1998, in a published lecture delivered in Frankfurt, Naess again re-evaluated "The Spirit of the Vienna Circle Devoted to Questions of *Lebens- and Weltauffassung*", alluding to Spinoza and philosophical scepticism.[55]

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49 Ibid., p. 13

50 Naess 1997. This contribution is not mentioned and included in SWAN.

51 Kurt Fischer / Friedrich Stadler (Eds.), *Wahrnehmung und Gegenstandswelt*, loc. cit.

52 Ibid., p. 177.

53 Ibid.

54 Egon Brunswik, *The Conceptual Framework of Psychology*, loc.cit. Naess reports nicely and moving on the "typical European" Brunswik in his late Berkeley times with a typical anecdote: "In the big department of psychology at Berkeley Brunswik felt eminently European and he certainly was more sophisticated than most. He would suddenly rise from the sofa in his office, open the door, and shout to an unfortunate student 'Must you whistle Bach!'". (Ibid.).

55 Naess 1998. Reprint in: SWAN VIII, pp. 279-290.

Regarding the attitude towards research and cooperation within the Vienna Circle he contrasted *Wissenschaftliche Weltauffassung* vs. *Lebens-und Weltauffassung*: "In my work within the realm of what I call total views, comprising the classical questions of *Lebens-und Weltauffassung*, I have been heavily influenced by what I personally experienced in the Schlick-Seminar in 1934 and 1935."[56] A necessary total view is normative *and* descriptive, compatible with being a sceptic questioning absolute certainty or dogmas. Therefore, analytic clarity and research attitude are applicable to general orientations, too. Basic views on life and world have been put forward in academic and non-academic philosophy, and the increasing interest in them is rooted in the transition from *Gemeinschaft* to *Gesellschaft* (Tönnies), leading up to the Club of Rome's *Limits of Growth* (1972) and *Beyond the Limits* (1992): sustainability has becom the crucial concept of all these human enterprises. Naess observed a development from "*Weltauffassung*" to "*Lebensanschauung*" culminating in his concept of "ecosophy". The focus of the Vienna Circle was more on language than on life, society and world (à la Wittgenstein), and although Spinoza was certainly venerated by members of the Vienna Circle, he was not taken seriously enough. Naess himself undertook a detailed "reconstruction" of Spinoza's ethics employing modern symbolic logic with the application of "Occams razor", because "it is my contention that the way I work shows the influence of the spirit of the Vienna Circle"[57]: the aim being to show "equivalences" among centrally important terms, logical clarity and empirical work within the framework of creative metaphysics. In this regard Naess appreciated Tarski's and Carnap's work on truth as logical analysis on "truth", but the so called Mach-Poincaré-Duhem theorem is preferably warranted: "Consequently, in my life a reconstruction, including the symbols, is significant in my practical life".[58]

In his last account on the Vienna Circle Naess dealt with the "Pluralism of Tenable World Views" (2003) from a combined logical and empirical point of view, a development from *Wissenschaftliche Weltauffassung* to a manifoldness of tenable world views. A "scientific" attitude was seen as compatible with science, humanities and historiography including values and norms. Again, Naess made a plea for "research" instead of "science", promoting a research attitude instead of a "logic of science" (logical syntax of language). This was an implicit critique of his friend Carnap, with whom he discussed these issues of Spinozan vintage in Los Angeles. In this case he sided with the naturalist and empiricist Neurath, who had also warned against an excessive respect for mathematics and logic.

From that perspective it is not surprising that Chinese and Indian philosophy could be interpreted also as relevant to questions of scientific compatibility

56 Ibid.

57 Ibid., p. 364.

58 Cf. Naess, *Freedom, Emotion and Self-subsistance. The Structure of a Central Part of Spinoza's Ethics* Oslo: Universitetsforlaget 1975; *Ecology, Community and Lifestyle. Outline of an Ecosophy.* Cambridge University Press 1989.

– which, by the way, had already been addressed by Heinrich Gomperz, who had commemorated Ernst Mach as modern "Buddha of Science".[59]

Even if it was difficult in terms of the choice of terminology, Naess argued for an "acceptance of the existence of a variety of different world views, all compatible with science",[60] because he saw globalization as being inevitable, maybe leading to *Gleichschaltung,* whereas in the 1930s there existed the antagonisms of communism vs. anticommunism, Nazism vs. Anti-Nazism, etc.

As contemporary movements comparable to Logical Empiricism one could cite, for instance, ecological movements with comparative studies of life quality (similar to Neurath's contributions on the standard of living). Naess closed with the following mission statement in the spirit of the Vienna Circle:[61]

> … it is important for many of us to try to influence movements we wish well to reach the high level of creative dialogue and sober mutual respect in spite of differences of opinion which characterized the logical empirical movement.

Summary

Although the main focus of his work after World War II turned away from Logical Empiricism and towards the ecological movement by way of the peace movement, Naess once again focused on the Vienna Circle increasingly in the 1990s – marking a sort of emotional and intellectual return to his Viennese roots (comprising the Schlick Circle, Freud's psychoanalysis, Brunswik's Gestalt psychology). Evidence of this can be found in his correspondence with Neurath (up to 1945) and Carnap (up to 1969) as well as in his book publications till the end of the 1960s, but also in his articles on A.J. Ayer, G. Mannoury, P. Feyerabend , as well as in his contributions to *Theoria* from 1938 on.

With this philosophical and personal return to some of the Vienna Circle issues in the 1990s there emerges a re-assessment of Logical Empiricism "from an empirical point of view". The anticipation of Quine's "Two Dogmas" is striking, but while endorsing a sort of radical empiricism (a variation of "psychologism") and naturalized epistemology he continued to employ modern symbolic logic for his studies in empirical semantics. Moreover, he favored Spinoza, Mach, James, and Russell over Quines's "From a Logical point of View", and the late Neurath of the *Encyclopedia* movement over Carnap or even Wittgenstein.

His message "models instead of theories" is embedded in the context of his continuous monistic world view. Naess's involvement and engagement in the Unity of Science movement was only interrupted by World War II. The planned conference in Oslo 1940 (probably on "Science of Science" or "Theory and Re-

59 Heinrich Gomperz, „Ernst Mach", in: *Archiv für Philosophie* 22/4, pp. 321-328.
60 Naess 2003, p. 6.
61 Naess 2003, p. 7.

search") could have led to a different development towards pragmatism, empiricism and research-oriented philosophy/psychology (with a consideration of values and ethics). His "ecosophy" seems to be compatible with these commitments and in some sense further developments, which makes Naess a pioneer of contemporary "experimental philosophy".[62]

Naess continued to admire the mode and style of thinking and discussing in the Schlick-Circle as rendered in his image of the "Gandhian way of communication". There also seems to be a striking similarity with the aforementioned Ludwik Fleck's sociology of science, his thought style and thought collective in context of Gestalt theory.

These observations are confirmed in Naess's book *The Pluralist and Possibilist Aspect of the Scientific Enterprise* (1972) that deals with science as a social project. The author appears as a cosmopolitan intellectual and scholar bridging the gap between the sciences and humanities, as well as different cultures and societies, as for example, the philosophy in China, where he had met again his friend and colleague from the Vienna years, Schlick's student Tscha Hung (Hong Qian) in Beijing after the Cultural Revolution.[63]

As a tentative conclusion of this short account I would like to emphasise the remarkable continuity and commitment to the Vienna Circle in the life and work of Arne Naess. In my view this is compatible with his multi-tasking way of life and intellectual complexity, and does not so much indicate a clear turn from "scientism to life wisdom" as the editor of his works suggests.[64] This conclusion can be drawn from his late writings on Logical Empiricism with his intellectual return to the legacy of the Vienna Circle.

In his autobiographical "How my Philosophy seemed to Develop" (1983/2005) we read about Naess's lifelong reworking and revising of his writings, inspired by principles drawn from his psychoanalysis ("*Lehranalyse*") he underwent with

62 Cf. the Presidential Address, Eastern Division APA, December 2007, by Kwame Anthony Appiah entitled "Experimental Philosophy". A critical study in this context: Benjamin Howe, *The History o Arne Naess's Environmental Philosophy and its Reception.* Doctoral Dissertation. University of Leuven 2008.

63 Arne Naess and Alastair Hannay (eds.), *An Invitation to Chinese Philosoph: Eight Studies.* Oslo: Universitetsforlaget 1972.
In August 9-15, 2007 I had the pleasure to organize an affiliated meeting at the 13th conference of the IUHPS/DLMPS in Beijing: "East meets West in the Philosophy of Science: Moritz Schlick and Hong Qian (Tscha Hung)", together with Arne Naess's good fried Bob Cohen. In this regard I am agreeing to Don Nilson (Akita University, Japan), who delivered a paper on Naess in Beijing entitled "Arne Naess on Pluralism in the Scientific Enterprise", summarizing that Naess "can be seen as having developed a view of science which in certain ways illuminated or anticipated aspects of philosophical positions of Quine, Lakatos, Feyerabend, Kuhn, proponents of the sociology of scientific knowledge approach, and others" (Conference *Abstracts*, p. 269).

64 Harold Glasser, "Arne Naess – A Wandering Wonderer: Bringing the Search for Wisdom Back to Life", in: SWAN I, 2005, p. xvii-lxii. See also the review essay by Elisabeth Nemeth in this volume.

Eduard Hitschmann also in Vienna.[65] – by the way, like F.P. Ramsey in the 1920s and R. Carnap later on in the U.S.:

> Early in 1934, at the age of twenty-two, I found myself in Austria eager to climb mountains and study. Psychoanalysis was at that time very close to the center of cultural attention in Vienna. It was therefore inevitable that I should ask myself, if I am to be an honest philosopher of science, would it be not prudent to go through psychoanalysis? Was it not suspicious that in the first draft of my doctoral thesis I had introduced the notion of "achievement" (*Leistung*) rather than the "meaning" of a sentence as a basic dynamic concept? Might it have to do with my own, perhaps unwise, achievement-mindedness?
>
> Soon I was in a deadly serious fourteen-month analysis, 8-9 A.M. every morning except Sundays, with the old collaborator of Freud, Edward Hit(s)chmann. We were both somewhat astonished to find that I had suffered from a pronounced childhood neurosis. It had obvious consequences for later life, and the analysis turned into a combined character analysis and analysis of my philosophical inclinations. Some of our findings may be of interest in tracing the genesis of philosophical inclinations in general.

I think, only against the backdrop of these early experiences one can fully understand Naess's reconstruction of his intellectual journey from psychoanalysis, history of life, naturalist epistemology, amateur philosophies, Vienna Circle and empirical semantics, diversity and nonviolence, which leads up to the open concept of his late "ecosophy".

Personal concluding remark: I met Arne Naess several times since the 1990s in Vienna, Frankfurt, and the last time at the end of August 2007 in Oslo. I was always impressed by his sophisticated personality, combined with a sense of humour, (self-)irony, intellectual power, and moral commitment. Arne Naess was a humanist with a non-violent world view rejecting any form of totalitarian attitudes and dogmatic approaches. He struck me as the Spinoza of our globalised age – certainly with some attitudes and methods reflecting the spirit of the Vienna Circle.

65 Naess 1983/SWAN 2005/IX.

Arne Naess and Friedrich Stadler. Oslo, August 30, 2007

Addendum

Arne Naess, Selected Published Autobiographical Sources related to the Vienna Circle and Logical Empiricism:

Arne Naess (1993), "Logical Empiricism and the Uniqueness of the Schlick Seminar: A Personal Experience with Consequences", in: Friedrich Stadler (Ed.), *Scientific Philosophy – Origins and Developments*. Dordrecht: Kluwer 1993, pp. 11-26. (= Vienna Circle Institute Yearbook 1/1993). (SWAN VIII)

David Rothenberg, *Is it Painful to Think? Conversations with Arne Naess.* Minneapolis-London: University of Minnesota Press 1993. (Esp. Ch. II)

Arne Naess (1997), "A Philosophical Note on Egon Brunswik's Work", in: Kurt R. Fischer and Friedrich Stadler (Eds.), *"Wahrnehmung und Gegenstandswelt". Zum Lebenswerk von Egon Brunswik*. Wien–New York: Springer 1997, 178f.

Arne Naess (1998), "The Spirit of the Vienna Circle Devoted to Questions of *Lebens- and Weltauffassung*", in: Werner Leinfellner and Eckehart Köhler (eds.), *Game Theory, Experience, Rationality. Foundations of Social Sciences, Economics and Ethics*. *In Honor of John C. Harsanyi*. Dordrecht: Kluwer 1998, pp. 359-367. (= Vienna Circle Institute Yearbook 5/1997). (SWAN VIII).

Arne Naess (2002), *Life's Philosophy: Reason and Feeling in a Deeper World.* (First publication 1998 in Norwegian, also Danish, Swedish, Latvian).

Arne Naess (2003), "Pluralism of Tenable World Views", in: Friedrich Stadler (Ed.), *The Vienna Circle and Logical Empiricism. Re-evaluation and Future Perspectives*. Dordrecht–Boston–London: Kluwer 2003, pp. 3-8. (= Vienna Circle Institute Yearbook 10/2002).

Arne Naess (1991), "Paul Feyerabend – a Green Hero?, in: *Beyond Reason. Essays on the Philosophy of Paul Feyerabend*. Ed by Gonzalo Munévar. Dordrecht-Boston-London: Kluwer 1991, pp. 403-416.

Main Books by Arne Naess related to Philosophy of Science:

— *Erkenntnis und wissenschaftliches Verhalten.* Oslo 1936.
— *Truth as Conceived by Those Who Are Not Professional Philosophers.* Oslo 1938.
— *Notes on the Foundation of Psychology as a Science.* Oslo 1948
— *Interpretation and Preciseness. A Contribution to the Theory of Communication.* Oslo 1953.
— *An Empirical Study of the Expression 'True', 'Perfectly Certain' and 'Extremely Probable'.* Oslo 1953.
— *Wie fördert man heute die empirische Bewegung? Eine Auseinandersetzung mit dem Empirismus von Otto Neurath und Rudolph Carnap.* Oslo 1956.
— *Communication and Argument. Elements of Applied Semantics.* Oslo 1966.
— *Scepticism* (1968)
— *For Modern Philosophers: Carnap, Wittgenstein, Heidegger, Sartre.* Chicago 1968.

The Selected Works of Arne Naess (SWAN). Harold Glasser, Series Editor. Alan Drengson, Associate Editor. 10 Volumes. Springer: Dordrecht 2005:

Vol. I: *Interpretation and Preciseneness. A Contribution to the Theory of Communication*.
Vol. II: *Scepticism. Wonder and Joy of a Wandering Seeker*.
Vol. III: *Which World is the Real One? Inquiry into Comprehensive Systems, Cultures, and Philosophies*.
Vol. IV: *The Pluralist and Possibilist Aspect of the Scientific Enterprise. Rich Descriptions, Abundant Choices, and Open Futures*.
Vol. V: *Gandhi and Group Conflict. Explorations of Nonviolent Resistance, Satyagraha*.
Vol. VI: *Freedom, Emotion, and Self-Subsistence. The Structure of a Central Part of Spinoza's* Ethics.
Vol. VII: *Communication and Agreement. Elements of Applied Semantics*.
Vol. VIII: *Common Sense, Knowledge, and Truth. Open Inquiry in a Pluralistic World. Selected Papers*.

Vol. IX: *Reason, Democracy, and Science. Understanding Among Conflicting Worldviews. Selected Papers*.
Vol. X: *Deep Ecology of Wisdom. Explorations in Unities of Nature and Cultures. Selected Papers*.

University of Vienna
Department of Philosophy and Department of Contemporary History
Institute Vienna Circle
c/o Campus der Universität Wien, Hof 1, Spitalgasse 2-4
A-1090 Wien
Austria
friedrich.stadler@univie.ac.at

Jan Faye

Niels Bohr and the Vienna Circle

Logical positivism had an important impact on the Danish intellectual climate before World War Two. During the thirties close relations were established between members of the Vienna Circle and philosophers and scientists in Copenhagen. This influence not only affected Danish philosophy and science; it also impinged on the cultural avant-garde and via them on the public debate concerning social and political reforms. Hand in hand with the positivistic ideas you find functionalism emerging as a new heretical language in art, architecture, and design. Not surprisingly, you may say, since the logical positivists' wishes of stripping philosophy of metaphysics is spiritually similar to the functionalists' desire to get rid of symbols and ornaments.

One event more than anything confirmed the connection between the Vienna Circle, Denmark, and the rest of the Nordic countries. For a short while Copenhagen became the centre for the Circle's activities when in 1936 the 2nd International Congress for the Unity of Science was held there between June 21 and 26. A photograph, taken during the conference, shows many of the participants sitting in the hall of Carlsberg's honorary mansion where Niels Bohr was living at the time. Among the audience you find Otto Neurath (1882–1945), Carl Gustav Hempel (1905–1997) and Karl Popper (1902–1994), but also some of the more prominent Danish scientists and scholars whose world views were congenial with the logical positivists.

In the foreground Jørgen Jørgensen (1894–1964) stands half turned towards the photographer, half turned towards the participants whom he is about to welcome. Jørgensen was the general secretary of the conference and had been, together with Neurath, the primary motor behind the organization of the meeting in Copenhagen. Behind Jørgensen, to the right on the first row of seats, is Niels Bohr (1885–1962) sitting next to Philipp Frank (1884–1966). Right behind Bohr is George de Hevesy (1885–1966), and again behind him, in the third row, you see Harald Bohr (1887–1951) professor of mathematics. In some of the other rows you find Alf Ross (1899–1962), a Danish philosopher of law, and Edgar Rubin (1886–1951), a Danish philosopher and psychologist.

Many chairs in the first two rows are empty. This may due to the fact that several of the invited guests had difficulties getting to the opening of the conference because of the political situation in Germany and Austria. Philosophers like Moritz Schlick (1882–1936), Rudolf Carnap (1891–1970), and Hans Reichenbach (1891–1953) had all expressed their wishes to be in Copenhagen, but various reasons prohibited them from coming. Thus, Schlick had been denied a travel permit

from Austria which turned out to be fatal. A mentally deranged student killed him on June 22 on the steps of the University of Vienna. The conference in Copenhagen received the message about Schlick's death with horror. At that time Reichenbach was living in Turkey as a refugee and could not afford the long journey to Denmark. And several of the most prominent members of the Circle had fled to America where Herbert Feigl (1902–1988) arrived in 1931 and Rudolf Carnap (1891–1970) in 1935 from Prague.

Bohr's instrumental view on quantum mechanics

The congress in 1936 was not the first time that Neurath visited Denmark. Two years earlier he came to Copenhagen twice. Jørgensen had invited him to give a series of lectures at The Society for Philosophy and Psychology. The first time was April the 6, 1934 when he gave a speech on the topic "Psychologie und Sociologie auf physikalicher Grundlage". The second time was between October the 18 and the 24, 1934 when he gave six seminars concerning issues in epistemology.[1] Niels Bohr took part in two of these sessions.

Less than a month later, on November 14, 1934, Neurath wrote Carnap a letter in which he described his first experience with Bohr:

> Bohr. Idiosyncratic. An intense man. Came to two lectures and joined the discussion enthusiastically ... Basic line: he does not want to be considered a metaphysician. And he is able to express himself relatively non-metaphysically, when he is careful. Yet obviously there lies a certain tendency in the selection of problems, insofar as the question of life, etc. is discussed, as well as in the stress on uncertainty. In addition, his printed remarks are full of crass metaphysics. But he possesses certain basic attitudes which agree with mine, e.g., that in science one cannot clear up everything at once, but that the individual scientific-logical actions have to pay a price, as it were. An idea of compensation, which with him naturally tends to be connected with the uncertainty relation. Obviously tries to come into agreement with us. But since his circle confirms him in his habit to express himself somewhat unclearly, one would have to be able to work on him for a long time, which he would be prepared to do.[2]

1 Mødeprotokollen for Selskabet for Filosofi og Psykologi, 1934.

2 "... Also erst Kopenhagen ... Bohr. Einzigartig. Intensiver Mensch. Kam zu zwei Vorträgen und diskutierte mit vollem Eifer. Und zwar Rede und Gegenrede. Es interessierte alle sehr – ausserdem diskutierte man des Nachts wieder. Grundzug: Er möchte nicht als Metaphysiker eingeschätzt werden. Und er kann, wenn er vorsichtig ist, sich relativ metaphysikfrei ausdrücken. Aber offensichtlich liegt in der Auswahl der Probleme, soweit die Frage des Lebens usw. erörtert wird, und die Betonung der Unbestimmtheit eine Tendenz. Überdies sind die gedruckten Ausführungen voll derber Metaphysik. Aber er hat gewisse Grundeinstellungen, die sich mit meinen berühren, z. B. dass man nicht alles gleichzeitig wissenschaftlich aufhellen könne, sondern dass die einzelnen wissenschaftlich-logischen Aktionen sozusagen einen Preis zahlen müssen. Eine 'Kompensations'-Vorstellung, die jetzt natürlich bei ihm tendiert sich mit der

Apparently, Neurath saw an obvious similarity between the ideas of logical positivism and Bohr's thoughts on complementarity, although he was dissatisfied with the way Bohr articulated them. Neurath also hinted at his own analogy according to which knowledge is like a boat in open sea. It is impossible to change all the beams at once, but one can change one plank at a time. Here Bohr seemed to have agreed.

The very same day Neurath left Copenhagen, Bohr sent him one of his books, possibly the German version of *Atomic Theory and the Description of Nature* (*Niels Bohr's Philosophical Writings Volume I*) along with a letter in which he states his pleasure about the fact that their ideas were not so far apart from each other as one might otherwise think from their different ways of expression. Bohr and Neurath corresponded over the next couple of years, and it is not unreasonable to suggest that Neurath's criticism of Bohr's metaphysical language bore fruit when Bohr had to face Einstein's last challenge the following year.

What was it then that Neurath thought was so metaphysical about Bohr's expressions? We can only guess. But if we look at what Bohr said before 1935 and what he said afterwards, certain hints seem to reveal a possible answer.[3] In 1935 Einstein published, together with two other physicists, Boris Podolsky and Nathan Rosen, a paper containing a strong criticism of Bohr's interpretation of quantum mechanics, including a thought experiment which apparently showed that quantum mechanics did not add up. It turned out that this criticism had an important influence on Bohr's future formulation of his view of complementarity.

The situation in physics of the day was that Heisenberg in 1925 had discovered a new physical theory which was able to describe atomic phenomena whereas Bohr's own successful theory of the atom from 1913 eventually had failed. Heisenberg's theory seemed to jeopardize any classical description of a physical system such as a joint ascription of momentum and space-time coordinates to an atomic object. Two years later Bohr had suggested that the understanding of the atomic object still demanded the use of classical concepts by which he meant concepts such as energy, momentum, and space-time coordinates. But the application of these concepts had to be restricted to particular circumstances in which the corresponding properties had a definite measured value. The consequence was that quantum mechanics did not allow a precise ascription of dynamical and kinematical properties simultaneously, as classical mechanics did. The different attributions, which in classical mechanics happened at once, were according to Bohr complementary to one another.

Unschärferelation zu verbinden. Offenbar bemüht mit uns in Einklang zu kommen. Aber, da sein Zirkel ihn in seiner etwas unbestimmten Art sich zu äussern bestärkt, müsste man ihn lang bearbeiten können, wozu er sich bereit finden würde." Letter to R. Carnap, 14 November 1934, RC-029-10-10, University of Pittsburgh. I want to thank Thomas Uebel for drawing my attention to this letter.

3 For a further discussion of these changes in Bohr's arguments, see Jan Faye (1991), Chap. 7.

Up to 1935 Bohr believed that physicists, through their measurement of an atomic object, *disturbed* the object in such a manner that they could not exactly determine its position and momentum at the same time. This way of talking made it sound as if the atomic object could be considered as a Kantian thing-in-itself. The atomic object had some values or properties, when nobody interacted with it, but it took on different values or properties during its observation when it was disturbed by the experimental equipment. It was just this impression Einstein seemed to have gotten by his discussions with Bohr and by reading his papers. Einstein therefore believed that quantum mechanics was incomplete (after he first had failed to show that it was inconsistent) because it could not account for these atoms-in-themselves. Neurath, however, contrary to Einstein, would find any talk of the disturbance of such things-in-themselves very problematic if not complete nonsensical.

Thus, in order to defend quantum mechanics as complete Bohr was forced by the challenge of Einstein, Podolski, and Rosen's paper to get rid of any formulation that indicated the existence of a physical reality behind the world of experience having quite different properties than we were able to observe. The only thing a physicist could rely on was that the atom as an observable phenomenon was describable in relation to a certain measuring apparatus. The reference to the experimental circumstances therefore became the conditions under which it made sense to apply a certain observable property, parameter, or eigenstate.

Bohr's first philosophical essay after the EPR exchange, "Causality and Complementarity" was his contribution to the Second International Congress for the Unity of Science, in Copenhagen. It was published the next year in English, German, and Danish. Here he first clearly distinguished his view from the "disturbance" interpretation suggested by his earlier statements of the late 1920s, which interpreted the uncertainty relations as merely an *epistemological* limitation on what can be *known* due to the "uncontrollable interaction" on the object of investigation. It is not clear whether Bohr was ever tempted to hold such a view earlier, but at least after EPR, he flatly states that "the whole situation in atomic physics deprives of all meaning such inherent attributes as the idealizations of classical physics would ascribe to such objects."[4] Bohr's adjustment of his philosophical statement to this more semantic formulation which rejects the "metaphysical" notion of real but unknowable properties of objects is surely in line with the positivistic outlook and leads Bohr to take a more "linguistic turn" in the expression of complementarity. Reflecting this shift in his outlook, Bohr henceforth dropped his earlier reference to Heisenberg's relations as "uncertainty relations" in favour of the expression "indeterminacy relations." (Although Bohr returns to using "uncertainty" in his next paper delivered in Warsaw in 1938, that paper was rewritten from an earlier lecture delivered in Edinburgh; after this time, he consistently uses "indeterminacy" and never "uncertainty" in the post-war papers.)

4 Bohr (1937/1998), p. 86.

Bohr's idea of complementarity thus understood was not so different from Neurath's and Carnap's view of relating all statements about theoretical entities to statements about observable things expressed in terms of protocol sentences. Against Einstein's metaphysical attitude towards a physical reality consisting of things-in-themselves, Bohr could just reply that it does not make sense to operate with a conception of reality other than one which can be described in sentences concerning our empirical knowledge. If experimental knowledge does prohibit an ascription of a precise position and a precise momentum at the same time, it does not make sense to talk about a free, undisturbed electron having such values to begin with.

During the period in which Bohr was in touch with Neurath, he also corresponded with Philipp Frank, another leading member of the Vienna Circle and a professor of physics in Prague. In a long letter of January 9, 1936, to Bohr, Frank expresses his opinion about the recent discussion between Bohr and Einstein, attributing to Bohr a positivistic view of physical reality but to Einstein a purely metaphysical view. After his statement, he asks Bohr whether he had understood the matter correctly; Bohr answers in a letter of January 14, 1936:

> I am very glad to hear from your kind letter that you have given such care to the papers of Einstein and myself concerning the question of reality. I also think that you have caught the sense of my efforts very well.[5]

In combination with what was just said about Neurath's criticism, it seems fair to say that Bohr shared with the positivists the view that physical reality could not be meaningfully referred to as something existing behind the observable phenomena.

There was another point of Bohr's philosophy which in the eyes of the positivists (and Bohr's) seemed to match their basic tenets. The positivists believed, after they came to ground their claims of experience on a physicalist notion, that all scientific statements should be relatable to a language of physical things which was capable of satisfying a publicly agreed constraint and thereby come to refer to observable entities. (Carnap's so-called reduction sentences no longer required eliminative reduction of non-observational terms to count as meaningful.) They drew a distinction between the language of observation and the language of theory. The language of observation contained terms for only those phenomena that could be observed, whereas the language of theory contained words for entities postulated by theory. Observational terms and sentences acquire their meaning from a correlation between words and visible things – so-called ostensive definitions – while theoretical terms receive their meaning from being partially translatable into

5 "Es freute mich sehr, aus Ihrem freundlichen Brief zu erfahren, dass Sie so eingehend mit den Aufsätzen von Einstein und mir über die Realitätsfrage beschäftigt haben. Ich glaube auch, dass Sie ganz den Sinn meiner Bestrebungen getroffen haben." Letter of January 14, 1936 from Ph. Frank to N. Bohr (Niels Bohr Archive, BSC 19.3).

observational terms. At the same time observational statements (and those theoretical statements that are reducible to observational statements) are, in contrast to irreducible theoretical statements, truth-bearers. The result was that the positivists treated theories as a kind of logical instrument which could not be given a realist interpretation as far as it could not be translated into a language of observation.

Bohr for his part believed that atomic objects were real. A couple of times around 1929 he had emphasized that the experimental evidence for their existence were overwhelming. Nevertheless, he thought that the theory of quantum mechanics was an instrument of prediction and organization of the observable phenomena. So you may say that Bohr was an entity realist but an antirealist with respect to theories. Thus Bohr referred to the state vector or the wave function as a *symbolic* representation. Usually symbolic language stands in contrast to literal language. Bohr associated the latter form of representation with what can be visualized in space and time. Quantum systems are not visualizable because they cannot be tracked down in space and time as classical systems. The reason is, according to Bohr, that the mathematical formulation of quantum states consists of imaginary numbers. Thus, the state vector is symbolic. But what if "symbolic" means that the state vector's representational function should not be taken literally but be considered as a *tool* of calculation of probabilities of observables? Let me present one quotation of Bohr's in which he directly says what I just have indicated:

> The entire formalism is to be considered as a tool for deriving predictions of definite or statistical character, as regards information obtainable under experimental conditions described in classical terms and specified by means of parameters entering into the algebraic or differential equations of which the matrices or the wave-functions, respectively, are solutions. These symbols themselves, as is indicated already by the use of imaginary numbers, are not susceptible to pictorial interpretation; and even derived real functions like densities and currents are only to be regarded as expressing the probabilities for the occurrence of individual events observable under well-defined experimental conditions." (Bohr 1948[1998]: 144)

Also consider the following: (a) in many places Bohr talks about the mathematical formalism of quantum mechanics as the mathematical *symbolism*, and he talks about *symbolic operators*; (b) concerning the aim of science Bohr says: "In our description of nature the purpose is not to disclose the real essence of phenomena, but only to track down as far as possible relations between the manifold aspects of our experience" (Bohr 1929[1985]: 18); (c) "within the frame of the quantum mechanical formalism, according to which no well defined use of the concept of "state" can be made as referring to the object separate from the body with which it has been in contact, until *the external conditions involved in the definition of this concept* are unambiguously fixed by a further suitable control of the auxiliary body" (Bohr 1938b[1998]: 102, my emphasis) – in other words, it makes no sense to say that a quantum system has a definite kinematical or dynamical state prior to any measurement. Hence we can only ascribe a certain state to a system given those circumstances where we epistemically have access to their realization. Based on these

and other considerations, I think it makes good sense to argue that Bohr was a realist with respect to atomic systems but antirealist with respect to their states. You may therefore say that Bohr shared a similar view of scientific theories as the logical positivists but that some of the arguments in favour of instrumentalism were different from theirs.

There is perhaps a third point where there were some affinities between Bohr and the logical empiricists. The unity of science movement accorded well with Bohr's attempt to apply the principle of complementarity outside quantum mechanics and physics. Bohr believed that complementary descriptions were also epistemologically necessary in biology, psychology and cultural sciences, and he wrote several papers in which he argued for this idea. But again Bohr's view on the unity of science was different from the positivists' (*cf*. Neurath's remarks to Carnap about Bohr's treatment of the question of life). It was not grounded in a reductivist approach. Instead, Bohr had the idea that holistic descriptions of an organism, a mind, or a culture was not reducible to any common physical-chemical description or any other low level descriptions. Rather holistic kinds of descriptions should be considered as complementary to reductive kinds of descriptions.

The anti-metaphysical and positivistic features of Bohr's philosophy were, as we shall see, also noticed by Jørgensen. Both Bohr and Jørgensen had been in touch with and influenced by Harald Høffding (1843–1931), whose philosophy more than anything was formed by Comte's classical positivism and Kant's and Spinoza's philosophy. In every aspect of his philosophy, Høffding discarded metaphysical speculations, but he also believed the empirical sciences gave rise to metaphysical questions which philosophy could not answer. So with the rise of the neo-positivistic movement both Bohr and Jørgensen found an intellectual kinship with the spirit of the Vienna Circle.

The positivists' reception of Bohr's philosophy

At the conference in Copenhagen a couple of talks beside that of Bohr's were dedicated to the philosophy of quantum mechanics. Philipp Frank gave one entitled "Philosophische Deutungen und Missdeutungen der Quantentheorie", and Moritz Schlick's last paper "Quantentheorie und die Erkennbarkeit der Natur" was read to the audience. Both papers hailed Bohr for the positivistic setting he had given the understanding of the atom. But Jørgen Jørgensen, not surprisingly, seemed to have been the philosopher among the positivists who had the most nuanced grasp of Bohr's view and was not directly influenced by logical positivism.

The relation between Jørgensen and the Vienna Circle seems to go back to the beginning of the 1930s.[6] The year before he was appointed professor of philosophy at the University of Copenhagen in 1926, he had written a prize paper on for-

6 See C.H. Koch's paper on Jørgensen in this volume.

mal logic and its history. The paper was invited by the Royal Society of Sciences and Letters, and Jørgensen was awarded a gold medal for his entry. In 1931 his very large manuscript was published in English with the title *Treatise on Formal Logic*. This publication immediately brought him recognition outside Denmark, and he apparently came in touch with members of the Vienna Circle around the same time. He was elected to the committee concerning the standardization of the logical terminology, and in 1934 he was asked, together with Neurath and Carnap, to be the editor of the book series called *Einheitswissenschaft*. After World War Two he published a book in Danish which was later translated into English with the title *The Development of the Logical Empiricism* and printed as the second volume of *The International Encyclopaedia of Unified Science*.

It has been said about Jørgensen that he was not a logical positivist in any orthodox sense.[7] In a narrow meaning of 'logical positivism' this seems a reasonable claim to make. But thereby one also obscures the fact that logical positivism is no uniform movement and that 'logical positivism' and 'logical empiricism' was used by different people to distinguish their position from some of the other members of the movement.[8] There exists no single dogmatic positivistic view which united everybody inside the movement other than their strong anti-metaphysical attitude and a common attitude to the unity of science. This is something Jørgensen understood well as he pointed out in the Introduction to *The Development of Logical Empiricism*. Here he says about neo-positivism:

> What unites its members is, therefore, not so much definite views or dogmas as definite tendencies and endeavors. An evidence of this is the often considerable divergence and lively discussion between its members and the amendments in the fundamental views that have occurred several times in the course of its development.[9]

So there is little basis for claiming that logical empiricism was not broad enough so that Jørgensen did not correctly think his own philosophical endeavour could be included in the positivistic programme. Thus, Jørgensen was the main philosophical spokesman of the movement in Denmark.

In his youth Jørgensen was influenced by Høffding's empiricism and by neo-Kantian idealism. Among neo-Kantians there was a clear anti-metaphysical approach to philosophy and they attempted to give an epistemological account of the logical foundation of science. As a consequence, they rejected Kant's idea that things-in-themselves could act as causes for things-as-they-are-for-us. Rather the concept should be understood negatively as a limitation of knowledge. With

7 See, Mogens Blegvad (1989), s. 2.

8 Albert Blumberg and Herbert Feigl suggested in their 1931 paper "Logical Positivism. A New European Movement" that logical positivism was renamed "logical empiricism" because of certain differences between the new and the older positivist movement.

9 Jørgensen (1951), p.1

this philosophical background Jørgensen's entrance into philosophy was not very different from many of the founding members of the Vienna Circle who were inspired by Ernst Mach's empiricism and Kant's critical philosophy.

In the end of the 1930s Jørgensen began writing on a large book about psychology in which he wanted to show that psychical phenomena could be explained based on the same approach to science as neo-positivism opted for. The work bore the title *Psykologi paa biologisk grundlag* (Psychology on A Biological Foundation) and published between 1941–1945. About the foundation of this work, Jørgensen says in *The Development of the Logical Empiricism* that he "has used logical-empiricistic viewpoints and methods."[10] He undoubtedly thought of this work as a contribution to the positivistic attempt of uniting psychology with the natural sciences. After World War Two, *Psykologi paa biologisk grundlag* had an important impact on the scientific education of several generations of students at the University of Copenhagen. This was due to the fact that Jørgensen introduced it as part of the curriculum of the propaedeutic philosophy which was mandatory for all enrolled students.

In his book Jørgensen wanted to demonstrate that psychological phenomena are closely connected to biological processes. He rejected vitalism as a pseudo-explanation based on our lack of precise knowledge of the chemical and physical processes which rule our body and brain. The mental life should just be considered as the most complex form of organic life. It is described as the mind but only known by its consciousness and the ways that expresses itself in virtue of feeling, experiencing, imaginating, thinking, and willing. These various activities appear to consciousness as phenomena in the same manner as external objects emerge as experiences to the consciousness. Jørgensen then tried, based on those phenomenological premisses, to set up a clear and decisive system of definition and classification for all the phenomena of consciousness. It was clearly his opinion that all life manifestations, including the appearances of consciousness, could be explained in terms of physico-chemical processes. Naturally he had, in his account of the most complex forms of mental phenomena, to face more and more difficulties of explaining in any concrete details what the mechanics of these processes consisted of because at that time there existed so little experimental evidence of how the brain and the mind operated together. Jørgensen believed, nevertheless, that psychological schools, like introspectivism, behaviourism, and gestalt and element psychology – apart from their metaphysical hypostatizations, – offered different scientific methods which each and everyone could be used with advance in the study of mental life.

It has often been claimed, even by the present author, that he had a critical attitude towards Bohr's interpretation of quantum mechanics.[11] This observation is both true and false. Because it was not until the third step in Jørgensen's philo-

10 Jørgensen (1951), p. 60.

11 See Jan Faye (1991), p. xv-xvi.

sophical development that he began to raise doubts about Bohr's basic thought that it was impossible to have a deterministic description of the atomic phenomena. In the mid 1950s Jørgensen articulated a critical realistic view on reality according to which he defended a reality that was entirely independent of the theoretical perspective which the knowing subject puts on the phenomena. We have only hypothetical knowledge of such an independent reality, although our beliefs concerning it become more and more confirmed concurrently with scientific progress.[12] This radical change in Jørgensen's view on the possibility of science had the consequence that he had to rewrite his chapter "What is psychology?" in *Psykologi paa biologisk grundlag*.

In the 1930s, when Jørgensen subscribed to the philosophy of the logical empiricism, he had no hesitations supporting Bohr's ideas against Einstein's objections. On this he says:

> But as far as I know it appears that none of these objections can stand a closer criticism, and therefore one must think that Bohr's and his fellow partisans' view suits the present experiences best, yes, that we up to now do not know any other view which accords with the experience.[13]

Although Jørgensen here supports Bohr, various statements in the paper seem to suggest that Jørgensen was not ready to follow Bohr and Heisenberg in their claim that it is in principle impossible to give a deterministic description of atomic processes. Because these two physicists, according to Jørgensen, say that such a description is meaningless, while the view Jørgensen seems to advocate is that experience supports Bohr and Heisenberg's approach up to now.[14]

A much better grasp of Jørgensen's understanding of Bohr's interpretation of quantum mechanics do we find in a small paper "Causality and Quantum Mechanism" from 1937 in which he discussed some misunderstandings of which he acknowledged that he himself had formerly been guilty.

> What I should like to emphasize in this place is the point that Niels Bohr and those agreeing with him in no way consider quantum-theoretical "indeterminism´" or "acausality" a *consequence* of a positivistic view (epistemological understood) adopted beforehand but that in their opinion it is based on circumstances which presumably necessitate the assumption of "indeterminism" quite regardless of definite epistemological standpoints. As far as I understand, it is deemed necessary to give up the idea that microphysical phenomena are causally determined in the classical sense in case one desires to avoid involving oneself in a series of paradoxes or absurdities which can be tolerated by no physical theory no matter whether the physicist be metaphysician, positivist, realist, or in any other way philosophically infected.[15]

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12 See Jørgensen (1942[1956]), p.

13 Jørgen Jørgensen: 'Er Mirakler nu blevet fysisk mulige?' (Are Miracles Now Physically Possible?) *in* Jørgensen (1934), s. 102. (my translation)

14 *Ibid.*, p. 98.

15 Jørgensen (1937) p. 98

Jørgensen continued to tell how Heisenberg's principle of indeterminacy helps physicists to remove paradoxes from the application of quantum mechanics and that indeterminism is the price they must pay to obtain a coherent and consistent theory of quantum objects. He also admitted that it would possibly be easier to reconcile oneself with indeterminism for the reason that it would otherwise produce absurdities than if the principle of positivism had been part of the argument. But Jørgensen then added:

> Quite a different matter is it that the results of atomic physics may serve to support a positivistic epistemology, since not only does quantum mechanics show that even a fundamental notion like the concept of causation is not absolutely necessary to physics but it also points out the danger of operating with assumptions (for instance of "causal determinateness") which cannot in principle be verified.[16]

So Jørgensen saw the Copenhagen interpretation of quantum mechanics as a vindication of the positivistic epistemology rather than a consequence that had its origin in the verification principle of meaning. By pointing that out, I think that Jørgensen in all fairness reported what Bohr had told him in their conversation.

In his *Psykologi paa biologisk grundlag*, we can follow how Jørgensen thought that the Copenhagen interpretation of quantum mechanics supported the epistemology of logical empiricism. Here he not only made a philosophical explication of Bohr's point of view but also directly defended it with respect to the conditions of talking meaningfully about a physical object:

> As Niels Bohr often has emphasized, the physical objects can strictly speaking only be defined in connection with the experimental situation or other observational circumstances in which their forms of appearance are present. To say that an object exists means that its form of appearance is actually present and a thing without any form of appearance is just a nice example of – nothing.[17]

A couple of pages later Jørgensen characterized this position as *neopositivism*, after he had rejected both naive and critical realism. The reason why he believed that Copenhagen interpretation was in support of neopositivism was that

> In modern atomic physics one has … discovered that every physical phenomenon is partly determined by the conditions of observation (instruments and experimental setups) under which they are experienced, and by then one has gone to the whole hog because we must now say that *every phenomenon is subjective conditioned, which just means that we can never experience any "things-in-and-of itself." Therefore this word does not make any sense, since it can never been shown what it could mean. All we can experience are phenomena, and the distinction of these into private and public or into subjective and objective is just a sign of the fact that each of these phenomena exist in certain, but different, con-*

16 *Ibid*., pp. 116-117.

17 Jørgensen (1941–1945), s. 162. (My translation)

nections with other phenomena which we call "the actual conditions of observation and conditions of description."[18]

These remarks led Jørgensen to draw some general epistemological consequences concerning the results of atomic physics with respect to logical positivism and its concept of science. In order to do so he emphasised the following statements: An expression such as "The object *G* exists" is just the same as the expression "At least two of *G*'s forms of appearances exist." Furthermore, a word can have meaning, only if one can, in principle, decide whether or not it can be applied on anything at all (the verification criteria of meaning). Thus, Jørgensen concluded:

> This view on the meaning of the word "existence" is characteristic for the so-called neo-positivism, and it seems to be the only view, which is compatible with the results of modern logic and the natural sciences. When all is said and done this view consists of the idea *that all things which can be experienced are phenomena and that a distinction of these into subjective and objective is due to the lawful connection of every phenomenon with other phenomena which are called its "conditions of observation." The task of every concrete science consists then in the investigation under which conditions a particular phenomenon appears, i.e. in virtue of which phenomenal connections it occurs*.[19]

In other words: around 1940 Jørgensen thought that the empirical foundation of cognitive meaningfulness, which positivists demanded of scientific knowledge, was being confirmed by the development of the atomic physics as it was understood by Bohr and Heisenberg.

Conclusion

The objections to Bohr's metaphysical formulations presented by Neurath on the one hand, and by Einstein on the other, seemed to have born fruit. This does not mean that Bohr was or became a logical positivist. For although much of what the Vienna Circle stood for must have been attractive to Bohr, there were also issues that distinguished him from the movement. Their conclusions were similar but they arrived at them from different premisses. The positivist's analysis was based on a logical-conceptual approach whereas Bohr took his departure in the empirical discovery of the quantum of action and what he considered to be the principal use of classical concepts. But naturally enough the metaphysical animosity of the positivists influenced him when he was in the amidst of his most important debate with Einstein, and their strong emphasis on an empiricist criterion of significance supported his view about the experimental conditions under which classical concept in quantum mechanics could correctly be used. For him the important thing

18 *Ibid*., s. 165-166. (My translation).
19 *Ibid.*, s. 166-167. (My translation)

was that there exists no physical reality behind what can be grasped in terms of ordinary language and its precise scientific amendments, which is also the language of physical things to which the positivists had turn in the beginning of the thirties. So it seems right to conclude that Bohr received some philosophical inspiration and moral support from his discussion with the members of logical positivism.

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Faculty of Humanities
University of Copenhagen
Njalsgade 80
DK-2300 Copenhagen S
faye@hum.ku.dk

Juha Manninen

Between the Vienna Circle and Ludwig Wittgenstein – The Philosophical Teachers of G. H. von Wright

I

Georg Henrik von Wright always mentioned that his academic teachers had been Eino Kaila and Ludwig Wittgenstein. He even spoke of the two as his "father figures". Georg Henrik was a sunny boy, but his "fathers" appear to be quite enigmatic. An industry of philosophical literature is needed to interpret Wittgenstein. Kaila seems to be at most a minor figure with some contacts to the Vienna Circle. It is not wrong to see von Wright as a follower of Wittgenstein, and von Wright's life-long work was decisive for the fact that all of Wittgenstein's *Nachlass* is now available.

In what follows, I will concentrate more on Kaila and his Viennese connections than on Wittgenstein. I make an attempt of trying to see the two "fathers" from a perspective that was or at least could have been von Wright's contemporary view. Vienna – or, more accurately – the recent past of Vienna was also von Wright's city of dreams. Kaila is an interesting case as concerns the networking typical of the Vienna Circle, especially as an example of Rudolf Carnap's rich scientific contacts at that point of his career. It was Kaila who made the start of von Wright's career possible and determined a number of his philosophical interests and orientations, including the specific way in which von Wright's work can be said to be linked to the Vienna Circle and logical empiricism. Of course, after World War II "analytic philosophy" was the acceptable designation for that kind of work that von Wright was pursuing in Cambridge, but his story can not be told without attention to the impulses from Vienna.

When von Wright began his studies at the University of Helsinki in 1934, he had a discussion with Kaila who was responsible for an undivided chair for philosophy and psychology. Without any preparation he had to answer a question: Would he be more interested in psychology or logic? Von Wright explained that in the recent years he had been reading Bergson, Nietzsche, some of Plato's dialogues and also Kant. Kaila was not satisfied and when pressed von Wright gave the answer: Logic.[1] The answer proved to be significant for all of von Wright's career.

1 G. H. von Wright, *Elämäni niin kuin sen muistan*. Helsinki: Otava 2001, p. 57. A number of the following informations are drawn from these memoirs by von Wright.

II

In practice, the textbook for logic in Kaila's courses was Rudolf Carnap's *Abriss der Logistik*. It was accompanied by *Der logische Aufbau der Welt*. Kaila had obviously heard that his new student, von Wright, was not completely uninitiated in philosophy. Kaila even mentioned to him the new *Die logische Syntax der Sprache*. But this was something that could be read only later on. In fact, Kaila would himself be struggling painfully through the book for a long time.

During the first year of studies Kaila directed von Wrights interest towards induction and probability. It meant the writings of Richard von Mises and Hans Reichenbach. Karl Popper's brand new *Logik der Forschung* was read immediately when it appeared. In Kaila's opinion, this was not enough. He gave to von Wright his own copy of *A Treatise of Probability* by J. M. Keynes. Von Wright complained that he could not read English. Kaila's reply was simply that after reading the book he would be able to do so at least to some extent. Von Wright, of course, followed the advice. In the final examination, there was only one small book, Wittgenstein's *Tractatus Logico-Philosophicus*.

Kaila was promoting modern logic and its applications to philosophical questions. During the latter half of the decennium he chaired a Logic Club with such advanced students as Max Söderman, Oiva Ketonen, Erik Stenius and von Wright. Simultaneously, he was leading other students to empirical and experimental psychology. Kaila's influence was not restricted to his own country. In 1932, he delivered an expert's evaluation to the Uppsala University, pointing out how old fashioned he found the work of the then fashionable local school:

> It is a curious state of affairs that the 'Uppsala philosophers' who prefer to be seen as logicians do not seem to posses any knowledge of the enormous width and development of logical research in the recent decennies […]; I mean the exact research which has its best known exponents in Frege and Russell as regards the elder generation, and among the younger probably in Wittgenstein and Carnap.[2]

These Swedish philosophers were entangled in an unacceptable psychologism: "They always talk about 'conceptions', 'judgements', 'mental images' etc. without sharply enough separating from these acts the sole interest of logic, the *objects* of these psychological acts."[3]

Only in 1945 Kaila was pleased to write to the Uppsala university concerning its candidates for a philosophical chair:

> Docent *Konrad Marc-Wogau* […] has begun partially to find his own ways. His latest works show that he has intensively studied the English Cambridge School, and not even the Vi-

2 E. Kaila, 'Till Filosofiska Fakultetens Humanistiska Sektion, Uppsala', 15 August 1932, p. 2, Archives of the Uppsala University.

3 *Ibid*., p. 7.

enna Circle and its logically motivated epistemology are any longer unknown to him. In his work, likewise in that of another candidate docent [Ingemar] Hedenius, one can now see a developing Uppsala philosophy.[4]

The isolation in Sweden was at last broken.

In 1939, Kaila and Professor Jørgen Jørgensen from Copenhagen, another logical empiricist, succeeded in convincing the faculty that the most promising candidate for the only chair of philosophy in Norway, Oslo, would be the 27 years old Arne Naess despite his still lean publication profile. His programmatic empiricist orientation, developed with studies in Vienna and communications within the Vienna Circle, would promise a bright development. Admittedly, there was a youthful radicalism in the philosophical writings of Naess. For instance, during the Paris conference of the Unity of Science movement he had joined Otto Neurath's defence of empirical semantics against Carnap's logical semantics. He had even done research among the Norwegian population in this sense, especially as concerned the concept of truth among ordinary people. According to Kaila and Jørgensen, together with the Norwegian psychologist, Professor Harald Schjelderup, Naess was the philosophically most gifted among the candidates.[5]

III

Eino Kaila was born in 1890. His family was theologically oriented, but he chose his own ways. He belonged to the same generation as Rudolf Carnap, Hans Reichenbach and Ludwig Wittgenstein. Instead of gaining war experience, he was able to complete at the University of Helsinki a Ph.D. thesis entitled *Über Motivation und Entscheidung* (1916). It was an experimental-psychological study mainly connected with the Würzburg School of thought psychology, quite different from the Wundtian mainstream.[6] Later on, Kaila was fascinated by the Berlin School of Gestalt psychology.[7]

In 1923, Kaila began a correspondence with Reichenbach[8] who advised him

4 E. Kaila, 'Till större akademiska konsistoriet vid universitetet i Uppsala', 7 November 1945, p. 12, Archives of the Uppsala University.

5 *Universitetet i Oslo, Årsberetning 1939*. Oslo 1940, pp. 66-116.

6 For a study of the Würzburg School, see M. Kusch, *Psychological Knowledge: A Social History and Philosophy*. London: Routledge 1999.

7 Cf. M. G. Ash, *Gestalt Psychology in German Culture 1890–1967: Holism and the Quest for Objectivity*. Cambridge: Cambridge University Press 1995.

8 E. Kaila to H. Reichenbach, 1 March 1923: "Ich habe mit grossem Interesse ihre Schrift über *Relativitätstheorie und Erkenntnis a priori* gelesen und möchte sie zum Bedeutundsvollsten zählen, was über diesen Gegenstand von philosophischer Seite geschrieben worden ist." – Archives of Scientific Philosophy, University of Pittsburgh, Hillman Library, HR-015-56-02 (=ASP). In the book Kaila had found references to Reichenbach's writings about probability. Now he was asking for reprints on these

to contact Schlick, Kaila's first connection with the Vienna Circle in formation. In his book *Die Prinzipien der Wahrscheinlichkeitslogik* (1926), Kaila quoted Schlick's interpretation according to which Mach and Einstein had been guided by the "principle of observability": "… if the principle is recognized and evaluated in its true significance , it can, I believe, be elevated to the supreme principle of all empirical philosophy…"[9] In the very same monograph, Kaila once used about the new philosophical standpoint the name "*logical* empiricism".[10] Kaila was himself still leaning towards critical realism.[11]

After an exchange of publications and letters, read critically in Vienna, Kaila was invited early in 1929 to the meetings of the Circle by Carnap, also quite officially by Schlick. The background was that Kaila had sent to Schlick a manuscript about Carnap's *Aufbau*. In a letter, Kaila explained that Carnap's book had moved him to reconsider critically his earlier views. He now agreed with Carnap that a traditional philosophical controversy had no longer any point. Still a number of disagreements remained. This was something that Carnap and Schlick could see this from the manuscript he enclosed. They could consider its publication. Presently, it would be difficult to find space for it in any of the few journals. The manuscript, entitled 'Die Logisierung der Philosophie und die Überwindung des Gegensatzes zwischen Realismus und Phänomenalismus', could also be published as a small book, even together with Carnaps objections.[12]

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matters. This was the beginning of the contacts from the Nordic countries to the proponents of a new philosophy, developing later on quite frequently.

9 M. Schlick, 'Kritizistische oder empiristische Deutung der neuen Physik', *Kant-Studien* 26, 1921, p. 107; transl. in: M. Schlick, *Philosophical Papers*, Vol. I (1909-1922), ed. by H. L. Mulder and B. F. B. van de Velde-Schlick.Vienna Circle Collection, 11. Dordrecht: D. Reidel 1979, p. 331.

10 Kaila meant that all knowledge about reality should be seen as a logical, probabilistic function of the experiences: "Dieser *logische* Empirismus scheidet sich aber scharf von dem klassischen *psychologistischen* Empirismus." E. Kaila, *Die Prinzipien der Wahrscheinlichkeitslogik.* Annales Universitatis Fennicae Aboensis, Ser. B, Tom. IV, No. 1. Turku 1926, p. 35. Kaila's use of this designation was drawing on the earlier context of the *Psychologismusstreit*. For the backgound, see M. Kusch, 'Psychologism', in: http://plato.stanford.edu/archives/sum2007/entries/psychologism. For a short introduction to Kaila's thought in the mid-twenties, see his 'On Scientific and Metaphysical Explanation of Reality', in L. Haaparanta and I. Niiniluoto (eds.), *Analytic Philosophy in Finland*. Poznan Studies in the Philosophy of the Sciences and the Humanities, 80. Amsterdam: Rodopi 2003, pp. 49-67.

11 See I. Niiniluoto, 'Eino Kaila and Scientific Realism', in: I. Niiniluoto, M. Sintonen and G. H. Von Wright (eds.), *Eino Kaila and Logical Empiricism*. Acta Philosophica Fennica, 52. Helsinki: Societas Philosophica Fennica 1992, pp. 102-116.

12 E. Kaila to M. Schlick, 28 September 1928: "Sie haben mir seit Jahren so freundliche Briefe geschrieben ..." – Wiener-Kreis-Archiv, Noord-Hollands Archief, Haarlem (=WKA). Kaila asked for a publication possibility for his paper inspired by Carnap's *Aufbau*. This paper came a surprice to Carnap, see R. Carnap to M. Schlick, 27 October 1928: "Du wirst auch wohl ein MS von Kaila vorgefunden haben. Er schreibt

After reading the text, Carnap wrote to Kaila: "You are right to say that the constitution theory should pay more attention to the inductive method of empirical science, and that to do this, it would have to give an account of the logical character of the concept of *probability*. I'm clear about the 'that', not about the 'how'."[13] However, contrary to Kaila and Reichenbach, probability inferences should be just as analytic and tautological as all other inferences. Carnap ensured that in Vienna Kaila would find an athmosphere that was congenial, in contrast to Germany, to strictly scientific philosophical endeavours. Actually, during his stay in Vienna in the following May and June, Kaila met Carnap repeatedly, some 20 times, often in the company of Feigl,[14] once also with Gödel. In Vienna, Kaila discussed his plan of a book concerning the *Aufbau* together with Schlick, accompanied by Carnap.

In a meeting of the Circle, after Schlick had left for the U.S.A., Kaila defended probabilistic thought and "possible protocols" against Carnap's by now strict truth-functional positivism. All could not be reduced to the given, he emphasized. However, Kaila's opinions were not fixed, a fact that Feigl described excellently in one of his letters to Schlick.[15] Kaila wavered between his earlier realism and Carnap's, Feigl's and Friedrich Waismann's definite denial of it, shocking the lecturing Waismann with a defence of realism. The others tried to assure Kaila that in addition to science there was room for poetry. When Kaila left Vienna, he felt helpless. He had not been able to formulate where the "surplus meaning" of probability consisted. In a letter to Reichenbach Kaila explained Carnap's argument that nothing else but the content of perceptually given could be expressed. He disagreed with this restriction:

I am inclined to think that the matter concerns here primarily the thought construction of the "protocol" on the given. Carnap accepts it as self-evident that the "protocol" can be thought as "self-contained" [*geschlossen*]. For me it appears equally self-evident that the

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mir, dass er anstatt einer brieflichen Antwort (wir hätten zunächst in sehr erfreulicher Weise korrespondiert) einen Aufsatz geschrieben habe und um Dich um Vermittlung zur Veröffentlichung bitten wolle." – ASP RC 029-30-23.

13 R. Carnap to E. Kaila, 28 January 1929, – G. H. von Wright's collection. The National Library of Finland, Helsinki (= GHvW). For a discussion of this phase in the development of Carnap's philosophy, including Carnap's replies to Kaila, see A. W. Carus, *Carnap and Twentieth-Century Thought: Explication as Enlightenment*. Cambridge: Cambridge University Press 2007.

14 In his Ph.D. thesis in 1927, Feigl had considered among others even Kaila's views, see H. Feigl, 'Zufall und Gesetz: Versuch einer naturerkenntnistheoretischen Klärung des Wahrscheinlichkeits- und Induktionsproblems', in: R. Haller and T. Binder (eds.), *Zufall und Gesetz: Drei Dissertationen unter Schlick: H. Feigl – M. Natkin – Tscha Hung*. Studien zur Österreichischen Philosophie, 25. Amsterdam: Rodopi 1999.

15 Quoted in J. Manninen, 'Beginning the Logical Construction of Cognition', in: S. Pihlström, P. Raatikainen and M. Sintonen (eds.), *Approaching Truth: Essays in Honour of Ilkka Niiniluoto*. London: College Publications 2007; and www.filosofia.fi/aineistoarkisto/tekstit/, p. 6.

foundation must be provided by a "not-closed" [*unabgeschlossenes*] protocol, i.e. that one can imagine elementary matters of fact that can not be designated as false although they do not appear in the protocol.[16]

Kaila's and Reichenbach's objections to Carnap were reminders that the *Aufbau* was without any theory of probability and induction, so important for the practice of science. Carnap was sure that no realistic metaphysics was needed, but he had to tackle with these problems. How could inferences from the given to the not-given be justified? Carnap had coined a new concept, the "analytic equivalence", which he explained to Kaila in Vienna and later on also in a letter.[17] In October 1929, Carnap was lecturing in Reichenbach's seminar in Berlin on the constitution of the non-given. He extended the analyticity principle to an analysis of the given:

Empirically equivalent concepts (functions) need not have the same meaning […] But *analytically equivalent* concepts and propositions do. Put differently: If two propositions P and Q are to have different meanings, a form of the world [*Weltgestalt*] (a form of the given) must be *thinkable* in which one holds and not the other. This is the *decisive argument against every form of realism*! And not the popular slogan of 'verifiability' […] (now please do your best to forget my pamphlet [*Scheinprobleme in der Philosophie*]!)[18]

During the next weeks Carnap continued to write an extensive study *Über die Konstitution des Nicht-Gegebenen*, called in his diary also as the "Kaila-essay", only a small part of which has survived.[19] The matter was one of continued interest. In March 1930, Carnap discussed it with Feigl, Albert Blumberg and C. G. Hempel. The task was one of complementing the theory of constitution, only re-organization, and adding an axiom of induction.[20] Carnap returned to this writing process again in October, again with important additions: a metric for ranges [*Spielräume*] and contents.[21] The Kaila-essay was never completed. It is not known when exactly Kaila's book on the *Aufbau*, his *Der logistische Neupositivismus*, appeared, or whether Carnap's return to the theme of not-given was occasioned by the receipt of it.

Beginning with the fall of 1930, Kaila was nominated for a professorship at the University of Helsinki, resposible for philosophy as well as psychology. In an official document to the university he described himself as allied to the new method of the Vienna Circle consisting in Kaila's words of "Schlick, Carnap, Wittgenstein,

16 E. Kaila to H. Reichenbach, 7 August 1929. – ASP HR 014-09-12.

17 R. Carnap to E. Kaila, 12 December 1930. – GHvW.

18 R. Carnap, 'Über die Konstitution des Nicht-Gegebenen (für Vortrag in Berlin)', 8 November 1929. Yough Research Library, University of California at Los Angeles, Ms Coll. 1029, Box 4, CM13, item 3. Quoted according to Carus, *op.cit*., p. 201.

19 Carus, *op.cit*., p. 217.

20 R. Carnap, Tagebücher 1927–1930, 22 March 1930. – WKA 585/X.47-1.

21 R. Carnap, Tagebücher 1930–1933, 29 October 1930. – WKA 585/X.42-2.

Zilsel, Kraft and others", a Circle he believed would in the end be victorious in the philosophical world.[22] Kaila even related that he had been part of the founding of the Vienna Circle which is not true as such, though he participated in preliminary planning sessions skeching the agenda to inaugur the Circle's public phase in connection with the Prague conference under the new name coined by Otto Neurath. On 24 June 1929, Carnap had explained to the Circle the plan for the pamphlet containing in compressed form their leading thoughts, 'Leitgedanken',[23] which would later become known under its final title, *Wissenschaftliche Weltauffassung: Der Wiener Kreis*. But Kaila's own booklet on the *Aufbau* was still published in the series of his old Turku university.

IV

One of Kaila's and Carnap's main disagreements concerned the knowledge about other minds. They had an extended discussion on three days in June 1929 about the *Fremdpsychische*. Kaila denied the primacy of "I", the methodological solipsism of starting from "my" experiences, *i.e.* Carnap's auto-psychological world construction. In addition, the perception of *Gestalts* was for Kaila an innate capacity and consequently not a product of learning or inferences by analogy.

Kaila's book on the *Aufbau* was the first one of its kind. However, much of its contents can be understood only against the background of the discussions in Vienna and Kaila's correspondence with Carnap: the strict Wittgensteinian truth-functionalism and a new understanding of analyticity were not yet to be found in the *Aufbau*. Kaila explained with references to psychology why he could not accept any autopsychological basis:

> Studies of '*Gestalt* theory' and 'developmental psychology' […] have led us to views such as that a human being from the very beginning experiences himself as being embedded in a 'field' and, moreover, does so in such a way that the very 'center' of the field, all that which contains the germs of the later-developing 'ego' with his thoughts, remains *unconscious* first; the first specific reactions, including recognition, are directed on phenomena on the 'periphery' of the field: recognition of faces, instinctive imitations of expressions, and the like. Once these have arisen, the field of experience will already have differentiated into a *social* field – long before there can be any question of awareness of the ego, or 'auto-mental' states. The famous saying 'the thou is older than the I' is to the point: one is aware of the mental states of others *earlier* than his own. The inference-by-analogy [to one's own mental states] theory is wrong …[24]

22 E. Kaila, *Valitut teokset*, 1 (1910–35), ed. by I. Niiniluoto. Helsinki: Otava 1990, p. 536.

23 R. Carnap, *op.cit*. For this aspect in the initiating of the public phase, see T. Uebel, 'Writing a Revolution: On the Production and Early Reception of the Vienna Circle's Manifesto', in: *Perspectives on Science* 16 (2008), 1, pp. 70-102.

24 E. Kaila, *Der logistische Neupositivismus: Eine kritische Studie*. Annales Universitatis

The *Gestalt* theory was apparently a late and insubstantial addition to Carnap's *Aufbau*. It was easy for Kaila to point out that the choice of unanalyzable elementary experiences as basic elements did not agree with the views of *Gestalt* theorists who in fact rejected earlier "atomistic" phenomenologies of perception but considered the *Gestalts* as exhibiting original internal manifolds. They could not be described as utterly simple *quales* in the sense of Carnap's elementary experiences.

Much more should be taken into account at the basic phenomenal experiential level. The quasi-analysis was in Kaila's opinion applied at a too low level, to a wrong kind of units. Carnap's logical methods were excellent as concerned the most advanced contemporary science, but at the level of lived experiences [*Erlebnissen*], principles such as the extensionality thesis, the principle of analytic equivalence or the requirement of decidability led to a great impowerishment, not unlike the traditional empiricisms. Instead of that, a radically anti-empiristic psychology of knowledge was needed in this specific sense. Experienced time and experienced space, all of the experienced world, should be taken seriously in the psychology of knowledge. Accordingly, Kaila explained the results of recent research into their constitution with references to David Katz, Wolfgang Köhler, Kurt Koffka and others, even to Edmund Husserl.

The experienced world was not a chaos. It had laws and principles of its own. They could be studied without rejecting the specific amount of realism necessary for the practice science. To make his point, Kaila quoted Leibniz:

> Yet the most powerful criterion of the reality of phenomena, sufficient even by itself, is success in predicting future phenomena from past and present ones […] Indeed, if this whole life were said to be only a dream, and the visible world only a phantasm, I should call this dream or this phantasm real enough if we were never deceived by it when we make good use of reason.

Kaila's agreement with this was complete: "This means nothing other than that 'reality' is defined only in terms of the '*successus praedicandi'* and its presuppositions – the interpretation of perceptions as *samples from a probability field*."[25] In the natural scientific observations, on the other hand, the experienced qualities were replaced by the corresponding spatio-temporal real-dimensional relations, "tones with various string lengths oscillating with correspondingly different frequencies, colors with various thicknesses of light-refracting layers, etc."[26] The same applied to measurements.

But what was it that made possible this move from the perspectival world

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Aboensis, Ser. B, Tom. XIII. Turku 1930, p. 38. Quoted according to E. Kaila, *Reality and Experience: Four Philosophical Essays*, ed. by R. S. Cohen. Vienna Circle Collection, 12. Dordrecht: D. Reidel 1979, p. 20.

25 Kaila, *op.cit*. (the English translation), p. 57.

26 *Ibid*., p. 50.

of lived experiences to the world of real-dimensional spatio-temporal relations? Kaila did not attempt to give any transcendental arguments or other a priori reasons:

> From a logical viewpoint, […] it is a curious 'lucky accident' [*ein merkwürdiger 'glücklicher Zufall'*] that anything can be natural-scientifically observed and measured at all. For it is conceivable that tones and colors, for instance, while occurring in lawful manner, still were not lawfully (*i.e.,* in a sufficiently simple extrapolatable way) dependent on *spatio-temporal* relations. That this is not so, that on the contrary qualities apparently without residue exhibit knowable dependences on spatio-temporal relations is for philosophy of nature one of the most significant properties of reality. For it follows from this that, on the one hand, the natural-scientific approach to reality, the definition of reality as a system of '*nudae quantitates'*, becomes possible, while this system, on the other hand, is only a certain aspect of total reality, a projection, as it were, of the latter onto the real manifold.
>
> It is particularly important in the present context that the *method of physical science*, as it is actually given, gives *the real manifold a privileged position in principle*.[27]

There is a puzzle concerning the mention of the 'lucky accident' permitting the shift from a language type into another. The very same designation can be found in Carnap's first draft of his presentation of physicalism, 'Die physikalische Sprache als Universalsprache der Wissenschaft', written in June 1930. This draft was not yet informed by Carnap's metalogic, unlike the published version that was completed in January 1932, and the first draft presented *two* universal languages, the phenomenal and the physical.[28] The coincidence of an experienced quality on a physical state depended on an empirical fact, on a 'lucky accident' about the orderliness of the world [*einem glücklichen Zufall, nämlich einer gewissen Ordnungsbeschaffenheit der Welt*].[29] Not only intersensuality but also intersubjectivity depended on a 'happy regularity of nature'.[30] Probably this convergence between Carnap's and Kaila's views was not only a 'happy accident'. As no documents on the matter seem to survive, one can only surmise that the possibility was discussed between the two during Kaila's five weeks in Vienna.

In the *Physikalische Begriffsbildung* (1926), Carnap had written:

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27 *Ibid*. Kaila was speaking all the way about *naturwissenschaftliche* observations and measurements. In the translation this was rendered as "scientific", but I have changed the translation, because psychological observations were "scientific" for Kaila as well.

28 T. Uebel, *Empiricism at the Crossroads: The Vienna Circle's Protocol-Sentence Debate*. Full Circle, 4. Chicago: Open Court 2007, p. 192 ff.

29 R. Carnap, 'Die physikalische Sprache als Universalsprache der Wissenschaft', p. 16. Ms written on 3 June 1930, based on lectures in Verein Ernst Mach, 20 May, and in Karl Bühler's colloquium, 28 May 1930. The name "physicalism" was occasionally used by Bühler in his *Krisis der Psychologie* (1927) as the designation for a trend in psychology and the humanities.

30 Carnap, *op.cit*., p. 17, 19.

One could think that the possibility of measuring the pitches at all would depend on the availability of the mentioned fact [that the pitch is uniquely correlated to the vibration of a string], consequently, so to say, on a happy accident. However, this is not the case.[31]

If the metric scala was not possible, there would be some scala anyway. Kaila agreed with this but he emphasised that the case of perceived qualities was different from the spatio-temporal measurement: "The difference between two distances and the distance between two such line segments are themselves distances. […] The difference in pitch of tones is not itself a tone."[32] According to Kaila, the experienced qualities could have regularities diverging from the physical ones. Thus the kind of measurement that was possible depended on facts of the matter.

The very same two *glückliche Umstände*, now adopted by Carnap, are to be found as explanations for intersensuality and intersubjectivity in the final, published paper on physicalism. Furthermore, these two features were responsible for making the physical language universal.[33]

V

Kaila's book was discussed both in Reichenbach's colloquium in Berlin and in the Vienna Circle. In Berlin, the young C. G. Hempel presented an objection to Kaila's attempt to understand relations directionally. In Vienna, Rose Rand gave a summary of Kaila's book.[34] Hempel's letter about his objection was read in the discussion, followed by comments by Gödel, Carnap, Hans Hahn, Felix Kaufmann and others.

The very next day after the discussion in Vienna, 12 December 1930, Carnap wrote a five pages letter to Kaila.[35] After presenting Hempel's refutation of Kaila's "directed" relations, the Kuratowski definition of an ordered pair, and Gödel's clarificatory remark, Carnap went on to Kaila's psychology of knowledge. Carnap was quite prepared to admit that Kaila could be right as concerned these matters, but he reminded that his logical method was not affected at all by the possible corrections to the constitutional system. In all empirical matters discussed, also in those that concerned their earlier discussions with Waismann, he admitted that there was not yet a definite answer about the atomic sentences and that he had an

31 R. Carnap, *Physikalische Begriffsbildung*. Karlsruhe: G. Braun 1926, p. 48.

32 Kaila, *op.cit*., pp. 49-50.

33 R. Carnap, 'Die physikalische Sprache als Universalsprache der Wissenschaft', in: *Erkenntnis* 1931, p. 445-447 (appeared in 1932) and R. Carnap, *The Unity of Science*. Psyche Miniatures, Vol. 63. London: Kegan Paul, Trench, Trubner & Co. 1934, p. 61, 64, 65.

34 See F. Stadler, *The Vienna Circle. Studies in the Origins, Development, and Influence of Logical Empiricism*. Wien: Springer 1997, pp. 242-244.

35 R. Carnap to E. Kaila, 12 December 1930. – GHvW.

open mind. His earlier comments about the limitedness of the phenomenal world should not be understood dogmatically.

Some of Carnap's remarks to Kaila can only be interpreted with a reference to the status of his constitutional system at the end of 1930, not with a reference to the *Aufbau* or the previous common discussions in the summer of 1929:

> Every proposition about the past as well as about the future (both concerning the physical world about which the science speaks) are in the constitutional system presented exactly as in the empirical science [*Realwissenschaft*] only as probabilistic propositions.[36]

Carnap's review of Kaila's book in the *Erkenntnis* was very much along the same lines.[37] Carnap readily admitted that there could be an internal structure on the groud level with consequent corrections to the constitutional system but the logic to be used remained the very same. Kaila's proposals about the "realism" of science were too unexact to be discussed. Even so, Carnap welcomed the book.

When Hempel heard from the discussion in Vienna through a letter of Carnap's, he sent Kaila a friendly letter of his own where he presented in six pages of logical demonstrations mainly a warning against a tacit glide from psychological propositions to physical ones.[38] In the practical use of relation theory, the direction of a relation was needed, although it could not be expressed in the extensional language of quasi analysis. Hempel was not opposing the constitution theory as such, merely explaining to Kaila different logical possibilities.

The next year, after a lecture on the Circle in Marburg, Kaila was again for a week in Vienna. Now Carnap noted, probably to his surprise, that Kaila at last agreed with him in the denial of realism and in the adoption of behaviorism. Kaila suggested a conference together with philosophers from the Scandinavian coutries and discussed the difference between mental images and theoretical content.[39] A radicalization of Carnap's own views was also going on, towards what would soon be known as physicalism. Most importantly, he was developing his "metalogic", the embryo of the logical syntax of language.

On 26 June 1931, Kaila and Viktor Kraft were Carnap's guests, together with Feigl, and asking questions about the phenomenal and physical languages. Did the new metalogic mean that there no longer could be verification by comparison

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36 *Ibid*.

37 The review is in *Erkenntnis* 1931, pp. 75-77.

38 C. G. Hempel to E. Kaila, 3 January 1931. – Eino Kaila's collection. The Archives of the Finnish Literature Society, Helsinki (=FLS).

39 R. Carnap, Tagebücher, 24 June 1931. – WKA 585/X.42-2, 1930-1933. Actually, Carnap made a preparatory lecturing tour to Copenhagen, Gothenburg, Stockholm, Oslo and Lund presenting a lecture 'Über den Charakter der philosophischen Probleme', dated 2.-7. November 1932, ASP RC 110-07-26:1. These notes were also the draft for Carnap's first publication in the U.S.A., badly translated by the editor of *Philosophy of Science*, at least in Schlick's opinion.

with the states of affairs? Carnap admitted that the exclusion of metaphysics was now more difficult, because there could only be internal syntactical methods for it. There would be no more questions about states of affairs.[40]

After a meeting with Carnap again one year later in Vienna, Kaila expressed his doubts about Carnap's expanding metalogic project in a letter to Åke Petzäll. He did not believe that it was the right way to overcome the problems of earlier extreme positivisms. The Humean problems were extremely serious, but what was needed, in Kaila's opinion, was "a *new* Kant".[41]

VI

Kaila had founded the first Finnish psychological laboratory in Turku and activated another one when he got the chair in Helsinki. It was natural of him to be in contact also with the Viennese psychologists, Charlotte and Karl Bühler, who were just then extremely influential.[42] Karl Bühler had his intellectual roots in the Würzburg School and Gestaltism. His student and wife Charlotte advanced in Vienna to chair in child and youth psychology, against many prejudices.

During the spring of 1932, Kaila was in Vienna studying mainly three month old infants in Charlotte Bühler's Kinderübernahmestelle der Gemeinde Wien. He was able to establish what could be called the "Kaila effect". The positive attention of the infant is focused on the area of the two eyes of a moving people. Seeing only one eye, a mask or a picture did not produce a similar effect. Kaila excluded the possibility of imitation, attempts at which appeared only at a later stage. All in all, Kaila's book was a study of the birth of intentionality.[43] But in Kaila's opinion intentionality, understanding or rule following did not exclude a causal approach in the humanities or social sciences.

In the spring 1934, Kaila was again in Vienna, now writing a theoretical work as a philosopher-psychologist. The result was a book on personality in Finnish which many have considered as Kaila's best. It received a wide audience in the Nordic countries, but for some reason a planned English translation failed to materialize. The central idea focused on the symbolic function of language. Kaila was using the concepts of "signal" and "symbol" very much in a similar way as Karl Bühler.

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40 Carnap, *op.cit*., 26 June 1931.

41 E. Kaila to Å. Petzäll, 24 March 1932. – The archives of Lund's university library.

42 See G. Benetka, *Psychologie in Wien: Sozial- und Theoriegeschichte des Wiener Psychologischen Instituts 1922-1938*. Wien: WUV-Universitätsverlag 1995.

43 E. Kaila, *Die Reaktionen des Säuglings auf das menschliche Gesicht*. Annales Universitatis Aboensis, Ser. B, Tom. XVII. Turku 1932. This was Kaila's last and in his own opinion best empirical study. The results were presented in later handbooks, see e.g. C. Bühler, *Psychologie im Leben unserer Zeit*, München: Knaur 1962, and J. Sants, *Developmental Psychology and Society*, New York: St. Martin 1980.

VII

As we have seen, von Wright selected "logic". This was in close connection with Kaila's lectures on the theory of knowledge which was on his agenda in 1934-35. Much of Kaila's discussion culminated in David Hume's problem of induction. Eventually he even prepared and introduced a Finnish translation of Hume's *Inquiry Concerning Human Understanding* which appeared in 1938. The problem of induction would also be the theme that occupied the young von Wright. Kaila initially suggested that von Wright should write his Ph.D. thesis on Galileo.[44] But von Wright was already well on his way towards a clarification of the philosophical problems connected with induction and probability.

Kaila's fixed point was that the frequency theory of probability should be preferred, because otherwise the 'uniformity' of processes can not be justified.[45] Kaila admitted the reduction of concepts to conscious experiences. A similar reduction of propositions he found untenable. Science should be explanatory, not purely descriptive. It was permissible transcend the factual phenomena, presupposing that the theories had empirically observable consequences. Every single "thing" was more than the phenomena connected with it..

Much later, in 1990, von Wright remembered in positive terms a contribution of his former charismatic teacher:

> Kaila's own 'constitution theory' is original and rather different from Carnap's. It is much to be regretted that it never that it never attracted the attention internationally which, in my opinion, it amply deserves. To this contributed no doubt the intervention of the war and the 'emigration' of a whole tradition of philosophy from the German to the English-speaking world. The only noteworthy trace which Kaila's contributions have left are with Alfred Ayer, who in his *Foundations of Empirical Knowledge* [1940] acknowledged indeptedness to Kaila.[46]

With the book *Über das System der Wirklichkeitsbegriffe: Ein Beitrag zum logischen Empirismus* (1936) testability became for Kaila *the* thesis of logical empiricism, accompanied by the principles of induction and simplicity together with the analyticity of the formal sciences. This book was the presentation of Kaila's sketch for a constitution theory, intended by von Wright's remark and in contrast to the earlier critical essay on the *Aufbau*. Indeed, it provided most of the arguments for Ayer's constitution of the material things.

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44 Cf. Kaila, *Reality*, p. 108: "Science […] knows only one epistemology; it is contained in the method of science itself; it is logical empiricism. The basic elements of this conception of knowledge are, indeed partially in a completely clear form, already present in Galileo."

45 E. Kaila, 'Zur Logik der Annahmen'. A note on 26 March 1933. – Eino Kaila's collection. Box 6. FLS.

46 G. H. von Wright, 'Eino Kaila's Monism', in: Niiniluoto et al. (eds.), *Eino Kaila and Logical Empiricism*, p. 80.

For one who was acquainted with the empirical research concerning the phenomenology of perception and who had himself carried on studies of it, like Kaila, it was impossible to outrightly reject languages other than the physical scientific one. There was, according to him, the phenomenological language in which "we do not wish to make any predictions about future phenomena, but only to describe in plain manner the encountered phenomena themselves".[47] And there was the everyday physical language in which "'to perceive' means encountering a so-called 'sensual' phenomenon of the kind that it presents a sufficiently reliable indication of a physical state of affairs".[48] Mobility and reversibility and a number of fundamental inductive inferences together with some principles from the psychology of perception were necessary for the constitution of physical space.

An imaginary flying being living in an eternally changing smoke could not form such a concept, at least when equipped only with similar perceptual apparatuses as its human counterparts. The world of the smokeman would be without the order of our everyday life. "The fact that we have the concept of 'physical space' is", Kaila concluded, "due to an 'accidental' empirical structure of certain of our perceptual sequences, especially the visual and tactual sequences; that some of these sequences are reversible is no more *a priori* than the fact that other sequences again are irreversible."[49] Kaila subscribed to an invariance view of reality in a very broad sense and with different levels, beginning with the invariances of everyday perception and continuing up to those of mathematical physics. Kaila saw the aim of science to be the search for ever higher invariances.

When Kaila sent the book to Carnap, he got a polite reply: "I have read it with lively interest and also with complete agreement in the essential points. Diverging opinions in details, of course, are inevitable."[50] In fact, now Carnap had presented in the Paris conference his new idea of the logic of science, according to which the search for the structure of science should be purified from its former psychological and epistemological elements.[51] Consequently, Kaila was in his opinion on a wrong track.

On the other hand, in his review of Carnap's *Logische Syntax der Sprache*, published in the Swedish *Theoria*, Kaila was not at all satisfied with radical physi-

47 Kaila, *Reality*, p. 68.

48 *Ibid*.

49 *Ibid*., p. 77. Not even Carnap's later inductive logic was possible without factual presuppositions concerning the orderliness of the world expressed by the lambda-parameter, see, for instance, J. Hintikka's remarks in his book *Socratic Epistemology: Explorations of Knowledge-Seeking by Questioning*. Cambridge: Cambridge University Press 2007, p. 199.

50 R. Carnap to E. Kaila, 21 December 1936. – GHvW.

51 R. Carnap, 'Von der Erkenntnistheorie zur Wissenschaftslogik', in: M. Stöltzner and T. Uebel (eds.), *Wiener Kreis. Texte zur wissenschaftlichen Weltauffassung von Rudolf Carnap, Otto Neurath, Moritz Schlick, Philipp Frank, Hans Hahn, Karl Menger, Edgar Zilsel und Gustav Bergmann*. Hamburg: Meiner 2006, pp. 260-266.

calism. Commenting, for instance, Carnap's use of the word "autonymous" Kaila remarked:

> How can one define such a term without using the concept of 'meaning'? […] But how can one at all talk about the 'syntax language' without presupposing meanings? The reason for this is that signs are not always distinguished from other things because of definite physical features, *e.g.* some geometrical forms, but because they *mean* something.[52]

Eventually, Kaila came to accept physicalism and logical behaviorism as the intersubjective languages of science, but with the proviso that a phenomenological language dealing with subjective experiences was still necessary. He even gave a new behavioristically acceptable definition of the symbolic function as "intermodal transponability" of delayed reactions, something that was so far not found to appear in empirical studies of animals.[53]

Kaila was accuratery aware in a number of his writings from different periods that more than one method of identification is needed. He saw one of the difficulties of phenomenology in the fact, formulated by the Viennese psychologist Egon Brunswik, that "often the mere datum is already designated by the name of the corresponding object".[54] Kaila's favourite example was that of a telephone call where the other person begins by simply saying: "It's me." The person who received the call identifies exactly the subjective *quality* of the voice and the manner of speaking, but it takes a while before he can identify *who* is the person.[55] Thus Kaila was clearly aware of the distinction between perspectival and public identification.[56]

VIII

After completing his Master Thesis von Wright wrote on the subject a scientific paper for the journal *Theoria*, 'Der Wahrscheinlichkeitsbegriff in der modernen Erkenntnisphilosophie'. The editor, Åke Petzäll, was inclined to reject the paper, but after a strong intervention of Kaila in support of von Wright it was published in 1938.

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52 *Theoria*, 1936, p. 86.

53 E. Kaila, 'Physikalismus und Phänomenalismus', in: *Theoria*, 1942, pp. 85-125.

54 Kaila, *Reality*, p. 69.

55 E. Kaila, *Beiträge zu einer synthetischen Philosophie*. Annales Universitatis Aboensis, Ser. B, Tom. IV, No. 3. Turku 1928, p. 162; *Tankens oro. Tre samtal om de yttersta tingen*. Helsingfors: Söderströms 1944, pp. 161-162. *Valitut teokset*, 2 (1936-58), ed. by I. Niiniluoto. Helsinki: Otava 1992, pp. 519-520.

56 For a contemporary discussion of this distinction, see J. Hintikka, 'Wittgenstein's Times (And Ours)', in: F. Stadler and M. Stöltzner (eds.), *Time and History*. Proceedings of the 28. International Ludwig Wittgenstein Symposium, Kirchberg am Wechsel, Austria 2005. Frankfurt: Ontos Verlag 2006.

In the summer of 1937 von Wright was travelling through Europe with Italy as his destination. Max Söderman, another student of Kaila with research interest in Bernard Bolzano's logic and contacts to Hans Kelsen in Prague, was then in Vienna. He organized for von Wright a meeting with Kurt Gödel. News about the logical semantic developments initiated by Alfred Tarski were apparently heard. Von Wright had the opportunity to meet Viktor Kraft, probably also Bela von Juhos, the only two members of the Vienna Circle who would remain in the country during the difficult years.

Nearly all of von Wright's philosophical education, including the reading of *Tractatus*, was in the sense of the Vienna Circle and related philosophical developments. The influence of Kaila can be seen in the fact that von Wright did not restrict his interest to recent contributions but extended it also to a historical view of the problems. It would have been most natural for von Wright to go to Vienna and write a dissertation there. The changed political conditions had made this impossible. There was hardly anything left in Vienna of the Circle.

Von Wright's second choice was Cambridge where he arrived early in 1939. He was especially warmly welcomed by C. D. Broad who helped to open him all possible philosophical doors in England also in continuation. To his great surprise von Wright heard that Wittgenstein was teaching in Cambridge. After some initial trouble, there was enough of common cultural background to make the meeting of the two fruitful. There is no evidence that Wittgenstein ever bothered to read much of von Wright's work, but quite contrary to his attitude towards most of his students he did what he could do to advance von Wright's career. Very soon von Wright wrote to his teacher:

> Then we have the great Wittgenstein whose lectures I am planning to listen during the next term. I have met him twice and I must say that he has been astonishingly friendly. […] A discussion with him is very difficult, because he does not know any philosophical doctrines except his own, but if one can get grip of some concrete point the discussion will be very interesting. His clarity is thoroughgoing and overwhelming and for this reason one will soon feel that it is better to be silent. I believe that what he actually means is something that we must leave for coming generations to finally interpret and apply. He distances himself with an utmost condemning gesture from everything that concerns the Vienna Circle. The syntactical approach is apparently disgusting him deeply. It is forbidden to mention Russell's name. Only from Frege he is talking with real emphasis and he thinks that Ramsey had some 'good ideas'. […] Although his philosophy is rather far apart from the traditions that we are seeking to advance in Helsinki, I believe that there still is a joint core. It would be good to try to find this core in the coming years and to emphasize it. I personally believe that a small correction of our course is needed.[57]

In fact a trace of Wittgenstein appeared in von Wright very soon. He was speaking for the Cambridge Moral Science Club at the end of May. The unpublished paper, entitled 'The Justification of Induction', was not very different from the manner

57 G. H. v. Wright to E. Kaila, 5 April 1939. – GHvW.

in which von Wright explained his work in progress to Kaila. But in the end there was a remark that seemed rather independent from the rest. No proof that the future would be in uniformity with the past would be forthcoming:

> I think that to realize the full amount of this truth, is to see – what I indeed have not explicitely tried to show here – that the problem of finding a justification of induction is no problem at all in the proper sense of the word, that what matters is not that the justification of induction is lacking, but rather: that there is nothing to justify at all. The inductive problem – as so many problems in philosophy – is like a mist, and to solve the problem is merely to make the mist disappear.[58]

When the end of his creative and happy period in Cambridge was nearing, in a letter from 9 July 1939, von Wright was pressing Kaila harder than earlier. Kaila was having his logic group in Helsinki with Ketonen and others. Now he had to read a letter written by someone who had chosen "logic" and recently been in touch with Wittgenstein:

> Of course it is us utterly important to be familiar with the modern logical calculus and the theory of the foundations of mathematics. Training in logic must in fact play a central role in our curriculum for the next ten or fifteen years. But, to speak frankly, logic is not philosophy any more than Darwin's theory was it fifty years ago (when no philosopher could by-pass it as a material), and for this reason I suspect that the future will look upon Carnap's Logische Syntax with the same pity which we now look on Haeckel's monism. Philosophy has always become frozen when it has reached a stage where one tries to demonstrate something either deductively or with references to facts. It lives only as long as it is a fight against those unclarities and false expectations that lie at the bottom of our systematization.[59]

Philosophy was for von Wright not a doctrine, but an activity, the clarification of thoughts. Earlier von Wright had read such a description again and again from Schlick's article that opened the journal *Erkenntnis*. Kaila definitively did not agree with the petrification component of von Wright's letter. But the times in Europe were hard, and he answered mildly: "Your declaration of independence is for me solely a joy; jurare in verba magistri is always harmful."[60]

IX

Von Wright characterized to Kaila his stay in Cambridge, together with a very interesting visit to Oxford, as his "spiritual rebirth". Especially Wittgenstein had opened his eyes, although he did not feel to be able to explain what was going on

58 The paper is preserved among von Wright's letters to Kaila. – GHvW.

59 G. H. v. Wright to E. Kaila, 9 July 1939. – GHvW. Jaakko Hintikka's translation.

60 E. Kaila to G. H. v. Wright, 13 July 1939. – GHvW. This postcard was sent from Austria, now a part of Greater Germany, where Kaila was still able to meet Viktor Kraft.

in Wittgenstein's philosophy. Back in Finland, von Wright continued his work, but there was not much time left before Soviet Union attacked Finland in agreement with Hitler in the fall of 1939, beginning the Winter War.[61] This happened to be the year when Kaila's excellent book on logical empiricism appeared, entitled *Inhimillinen tieto* (Human knowledge). The very same year the book was printed also in Swedish, translated by von Wright and used for a long time as a textbook in the Nordic Countries. Still 40 years later von Wright judged the book to be the best introduction ever to logical empiricism.[62]

When Carnap received the Swedish edition in Chicago, he was able to read it, although not without difficulty. He sent Kaila a letter, looking at the news pictures from bombarded Helsinki and commenting with great sympathy the fight of the Finns against the Russian attack, condemned by "not only by us Europeans but also by all Americans". About the book he expressed the wish that it should be published in English: "When the conditions were normal, I would think that it could fit well to our 'Library of Unified Science'…"[63] In addition, Carnap praised especially the broad historical stage presented in the book and he made also some logical objections and suggestions. However, the end of the Library of Unified Science came soon, when Holland, where it was published, was invaded.

During Finland's Winter War C. D. Broad published twice some of von Wright's letters about the situation in *The Cambridge Review*, actually the first writings of von Wright that were printed in English. There followed a peace between the U.S.S.R. and Finland, but it proved to be only an interim peace. Von Wright succeeded in defending in Helsinki his Ph.D. thesis *The Logical Problem of Induction* in May 1941.[64] A second revised edition of the book, published by Basil Blackwell in Oxford, appeared in 1957, now dedicated to Kaila. An added new chapter on the goodness of inductive policies shows Wittgenstein's influence.[65] In 1943, von Wright published a book, entitled *Den logiska empirismen* (Logical empiricism). It was an informed survey of the writings of the movement, although not as enthusiastic as Kaila's book.[66]

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61 See J. Lavery, *The History of Finland*. Westport, Conn.: Greenwood Press 2006.

62 See his introduction to Kaila, *Reality*, p. xxxiii.

63 R. Carnap to E. Kaila, 15 January 1940. – GHvW.

64 For a survey and evaluation of von Wright's work on these topics, see I. Niiniluoto, 'G. H. von Wright on Probability and Induction', in: I. Niiniluoto and R. Vilkko (eds.), *Philosophical Essays in Memoriam Georg Henrik von Wright*. Acta Philosophica Fennica, 77. Helsinki: Societas Philosophica Fennica 2005, pp. 11-32.

65 This is an observation by Ilkka Niiniluoto.

66 An up to date bibliography of von Wright's publications is included in J. Manninen and I. Niiniluoto (eds.), *The Philosophical Twentieth Century in Finland. A Bibliographical Guide.* Acta Philosophica Fennica, 82. Helsinki: Societas Philosophica Fennica 2007, pp. 434-461.

X

Despite great losses, Finland survived the two wars as an independent democracy and without being occupied. After the wars von Wright was again able to go to England and, of course, meet Wittgenstein. The frienship with Wittgenstein did not lead to any betrayal of von Wright's promises to Kaila concerning logic. Less known is the fact that in these times von Wright did not think that logical empiricism is dead. He wrote from Dartmoor a long letter to his friend Max Söderman. It portrayed the philosophical situation in England, including also a surprising twist as regards Wittgenstein.

Von Wright arrived first to his old supporter Broad, but his three moths long journey extended to the whole golden triangle of Cambridge, Oxford and London. His contacts with Trinity College were good and as an occasional member of the High Table he could meet a great number of personalities. He held lectures on 'Some Aspects of the Logic of Science', listened Wittgenstein's lectures and his seminar and participated in the meetings of the Moral Science Club where he also read a paper on the nature of philosophical activity. In London, A. J. Ayer and Karl Popper, the last one recently returned from New Zealand, were the dominating figures. Von Wright, by the way, never distanced Popper from the Vienna Circle. Von Wright had three lectures in Bedford College, entitled 'Some Problems of Methodology'. He was hosted in Oxford by C. D. Price and Gilbert Ryle and lectured on 'Induction and Probability'. Friedrich Waismann had left Cambridge and he was now influential in Oxford. Von Wright expressed his impressions:

> One could say that philosophy in England is experiencing a positivistic or logico-empiristic phase. Ten years earlier Ayer appeared to be an isolated figure in the tree of British thought. Today it would be right to characterize him as quite typical among the younger English philosophers. He has and he will certainly continue to have great influence. It is curious that he has himself been very much influenced by our teacher Kaila. It can also be mentioned that Kaila's name was unknown in Oxford, when I was there in 1939, but now he is everywhere mentioned with respect. Ayer's approximative counterpart in Oxford is the somewhat elder Ryle, and Price represents a more conservative type like Broad in Cambridge.[67]

The influence of Bertrand Russell and G. E. Moore was waning, although it could be seen "in the contemporary positivism of English thought". In Cambridge, a counterpart to Ayer and Ryle could be seen in R. B. Braithwaite. However, the most interesting of all was Wittgenstein:

> His influence is behind everything, not only modern English thought, but actually also the whole of the logical empiricist stream of thought. I do not mean especially Tractatus, the youthful work that he has left behind himself a long time ago. Although he has not published anything since then, his thoughts penetrate the philosophical atmosphere here. This

67 G. H. v. Wright to M. Söderman, 12 June 1947. – GHvW.

does not mean that he is beloved, rather a feared and hated one.

He has researched the country of modern philosophy with a perfection, seriousness and depth that probably has counterparts only among the greatest thinkers in history. 'Language', 'meaning', 'truth', 'thought', 'conscious states', 'logic', 'consequence', the basic concepts of mathematics – all that he has thought thorougly. And when one meets this enormous lifetime work, one is inclined to say: It is done. […] This is a horrible truth for the one who sees it. It means that every attempt at improving it or developing it further will be seen as mannerism or decline. It seems to me that if something new and lasting should be created, then it should be in opposition against and not along the lines of thought that Wittgenstein has drawn.

Contrary to von Wright's expectations, it was exactly Wittgenstein's thought and style that invaded the British minds, not Ayer's.

Respect and independence was an attitude that suited Wittgenstein well, as it did for Kaila. As we know, von Wright was not Kaila's *Nachfolger* in Helsinki, but Wittgenstein's in Cambridge, although he returned to Finland after some years. In Cambridge he met a Finnish mathematician returning from the U.S.A., with whom he had corresponded extensively about questions of logic, Jaakko Hintikka. In the summer of 1949, von Wright wrote to Kaila: "Hintikka is a very gifted young man and it would not surprise me if he will accomplish much."[68] There is an unbroken lineage from Kaila and the Vienna Circle to present-day philosophy in Finland.

XI

When did the Vienna Circle (or: Logical Empiricism) end, if ever? One can say that the end came when Schlick was murdered in 1936. However, this is not an altogether satisfying answer, because the spirit of the Circle is still alive and even growing stronger in a number of parts of the world. Or maybe its death was the passing away of its organizing talent, Neurath, in 1945? Von Wright had another answer. In one of his last reminiscences he related how Margarethe Stonborough, Wittgenstein's sister, had invited him and his wife to Vienna. It was the year 1952. Wittgenstein had died the previour year. Von Wrights could stay in the house planned by Wittgenstein in the Kundmanngasse.

The short period when Viktor Kraft was permitted to be a professor in Vienna was nearing its end. Von Wright wrote about the philosopher who would soon be retired:

I contacted him, and he friendly invited me to his research seminar called 'privatissimum'. I participated in a couple of meetings. I met in them among others Paul Feyerabend who then accompanied me in Vienna.

68 G. H. v. Wright to E. Kaila, 3 July 1949. – GHvW.

Von Wright continued:

I remember especially the last meeting of Kraft's *privatissimum*. The topic was the difference between a regularity and natural law. When the end of the session was nearing, Kraft delivered a small oration. He said that the meeting this evening could be seen as the absolutely last meeting of the Vienna Circle. […] I did not have the opportunity to get acquainted with the Vienna Circle during its time of flourishing, but in a sense I participated, if it is right to say so, in its funeral. Kraft's speech in his seminar's last meeting was deeply moving me.[69]

Helsinki Collegium for Advanced Studies
P.O. Box 4
FIN-00014 University of Helsinki
Finland
juhamann@mappi.helsinki.fi

69 G. H. von Wright, 'Wienin piiri – henkilökohtaisia muistoja', in: I. Niiniluoto and H. J. Koskinen (eds.), *Wienin piiri*. Helsinki: Gaudeamus 2002, pp. 201-202.
For their generous help in making available unpublished materials and for permission to quote from them the author wishes to thank The Archives of the Finnish Literature Society, Helsinki; The Archives of Lund's University Library; The National Library of Finland, Helsinki; The Archives of Scientific Philosophy, University of Pittsburgh, Hillman Library; The Archives of Uppsala Library and the Wiener-Kreis-Archiv, Noord-Hollands Archief, Haarlem.

Johan Strang

Theoria and Logical Empiricism
On the tensions between the National and the International in Philosophy

Theoria – a Swedish journal for philosophy (*and Psychology* until 1965) was founded in 1935, at a time when the conditions for the logical empiricists on the European continent were deteriorating as a result of the rise of fascism and Nazism. In a letter, dated August 11 1936, to the editor-in-chief of *Theoria*, Åke Petzäll, the Finnish philosopher Eino Kaila claimed that it was only a matter of time until the journal *Erkenntnis* would be closed down and suggested that *Theoria* could step in as a replacement.[1]

Kaila's proposal was by no means preposterous. Even if *Erkenntnis*, against all odds, was able to continue with Felix Meiner Verlag until 1938 (and two more years with Van Stockum & Zoon in Holland) the main organ of the logical empiricist movement was certainly experiencing difficulties. And besides *Erkenntnis* and the British *Analysis* there were few, if any, philosophical journals available for the logical empiricists in Europe. Furthermore, by August 1936 a number of leading Nordic philosophers had established themselves within the logical empiricist movement, not least through the *Second International Congress for the Unity of Science* which had been arranged by Jørgen Jørgensen in Copenhagen in June 1936. Kaila was, of course, an old acquaintance of the Vienna Circle, mentioned already in the pamphlet *Wissenschaftliche Weltauffassung* 1929, and in Arne Næss, logical empiricism had recently gained a young and gifted representative in Norway. And most importantly, the editor-in-chief of *Theoria*, Petzäll, was evidently interested in logical empiricism himself as he had introduced the movement to Sweden with his *Logistischer Positivismus* (1931) and *Zum Methodenproblem der Erkenntnisforschung* (1935).[2]

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1 I wish to thank Juha Manninen for persuading me to write this article and for allowing me to use material he has gathered from different archives around the world. I also want to thank Svante Nordin for a stimulating discussion and for pointing my attention to the archive of *Institut International de Collaboration Philosophique*, which together with the archive of *Theoria* and Petzäll's personal archive, constitute the *Nachlass* of Åke Petzäll, preserved at the University Library in Lund, Sweden. Unless otherwise explicitly stated, the letters referred to in this article are found in these archives. The volumes of *Theoria* are available on line through Blackwell Publishing.

2 Åke Petzäll, *Logistischer Positivismus. Versuch einer Darstellung und Würdigung der philosophischen Grundanschauungen des sog. Wiener Kreises der wissenschaftlichen Weltauffassung*. Göteborg: Göteborgs högskolas årsskrift 37:3 1931; Åke Petzäll, *Zum*

However, *Theoria* did not replace *Erkenntnis*. As we know, most of the Central European logical empiricists moved over to the United States from where a Swedish journal was rather distant. Instead, they succeeded well in integrating themselves in the American scholarly world and became leading voices in the newly founded American journals *Philosophy of Science* (1934) and *Journal of Symbolic Logic* (1936).[3] Moreover, despite Petzäll's efforts and the success of the movement in the neighbouring Nordic countries, logical empiricism had some difficulties in breaking through in Sweden. Since the 1910s, the Swedish philosophical scene had been dominated by the so called Uppsala School led by Axel Hägerström (1868–1939) and Adolf Phalén (1884–1931). Combining elements from Neo-Kantianism and phenomenology in an original way, Uppsala philosophy marked a break with the Swedish idealistic tradition that followed Christopher Jacob Boström (1797–1866). Associated with modernist currents such as functionalism in architecture and Social Democracy in politics, the Uppsala School enjoyed a rather similar cultural position in Sweden as the Vienna Circle did on the European continent.[4] The Uppsala School shared many philosophical themes with logical empiricism, most notably the non-cognitive moral theory, the emphasis of logical analysis, and the vehement refutation of metaphysics.[5] But instead of conceiving of logical empiricism as an ally, the Uppsala philosophers felt threatened by the rapid advance of this foreign philosophy, especially as it appeared to be sanctioned by the editor-in-chief of the only Swedish philosophical journal at the time. *Theoria* became not only the forum in which the scholarly confrontations between Uppsala philosophy and logical empiricism took place; it was also, to a large extent, the object of the struggles.

The tensions between Uppsala philosophy and logical empiricism surfaced as Petzäll strived to internationalise *Theoria*. He was continuously forced to balance his international ambitions with the expectations of his Swedish co-editors who often looked upon foreign contributions with suspicion, especially if they were published at the expense of Swedish or Nordic articles. Petzäll's cosmopolitan attitude caused him much frustration in an era when nationalism was triumphing all over Europe. The international projects and engagements were time-consum-

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Methodenproblem der Erkenntnisforschung. Göteborg: Göteborgs högsskolas årsskrift 41:1 1935.

3 Cf. Herbert Feigl, "The Wiener Kreis in America", in Fleming & Bailyn, *The Intellectual Migration. Europe and America 1930-1960*. Cambrige: Harvard University Press, 1969, pp. 630-673; George A. Reisch, *How the Cold War Transformed Philosophy of Science*. Cambridge: Cambridge University Press, 2005.

4 The political and cultural position of Uppsala philosophy has been explored in e.g. Staffan Källström, *Värdenihilism och vetenskap – Uppsalafilosofin i forskning och samhällsdebatt under 1920- och 30-talen.* Göteborg: Gothenburg Studies in the History of Science and Ideas 6 1984.

5 Cf. Svante Nordin, *Från Hägerström till Hedenius – den moderna svenska filosofin*. Lund: Doxa 1984.

ing and often unrewarding with respect to scholarly recognition. These considerations may also, at least partly, explain why Petzäll has been somewhat neglected in the history of Swedish philosophy. In the case of logical empiricism, however, a constricted national perspective is particularly misleading, as international collaboration was one defining feature of the movement. One of the main merits of the recent discussion on cultural transfers and entangled history (*histoire croisée*) is that it brings attention to transnational actors such as Petzäll.[6] Moreover, such perspectives can also articulate and discuss the different ways in which philosophical ideas move from one context to another. Often, foreign ideas have to be reinterpreted and re-described in order to fit into a new national context. In Sweden, logical empiricism failed to break through until a young Uppsala philosopher, Ingemar Hedenius (1908–1982), presented it as a natural continuation of the Uppsala legacy.

The internationalisation of *Theoria*

The journal *Theoria* was very much the result of one man's efforts. Serving as editor-in-chief from the very beginning until his death in 1957, Åke Petzäll (1901–1957) virtually personified the journal. He was a man of great social and organisational skills; he spoke several languages and had the gift of being able to communicate with people of very different philosophical and intellectual backgrounds. At the philosophical congress in Prague in 1934, Petzäll took the initiative to establish an international philosophical institute (*Institut International de Collaboration Philosophique*), which was founded in Paris 1937 with Petzäll as its director. Petzäll's own philosophical interests were manifold; he participated in discussions on epistemology, ethics, sociology, as well as on the history of philosophy – his dissertation from 1928 treated Locke's concept of innate ideas. He had his background in the diverse philosophical congregate of Lund and Gothenburg, which to a large extent was forced to define itself in opposition to the rather loud Uppsala school.[7]

Theoria was launched in 1935 as a Swedish journal with the intention to promote dialogue with the philosophical communities of the neighbouring Nordic countries.[8] Accordingly, the first volume of *Theoria* was written exclusively in the three Scandinavian languages. But already the next year *Theoria* accepted

6 Cf. Michael Werner & Bénédicte Zimmermann, "Beyond comparison: *histoire croisée* and the challenge of reflexivity", *History and Theory*, vol. 45, no. 1, 2006, pp. 30-50; Gunilla Budde, Sebastian Conrad & Oliver Janz (Hrsgb.), *Transnationale Geschichte. Themen, Tendenzen und Theorien*, Göttingen: Vandenhoeck & Ruprecht 2006.

7 Cf. Nordin, "Åke Petzäll och Wienkretsen", in Nygård & Strang (red.), *Mellan idealism och analytisk filosofi – den moderna filosofin i Finland och Sverige 1880-1950*, Helsingfors: Svenska Litteratursällskapet 2006, pp. 197-219.

8 "Theoria" [editorial], in *Theoria*, vol. 1, no. 1, 1935, p. v.

contributions in German, English and French. In 1937 the editorial language was changed from Swedish to English, and by 1939 the journal was entirely written in the three great European languages.

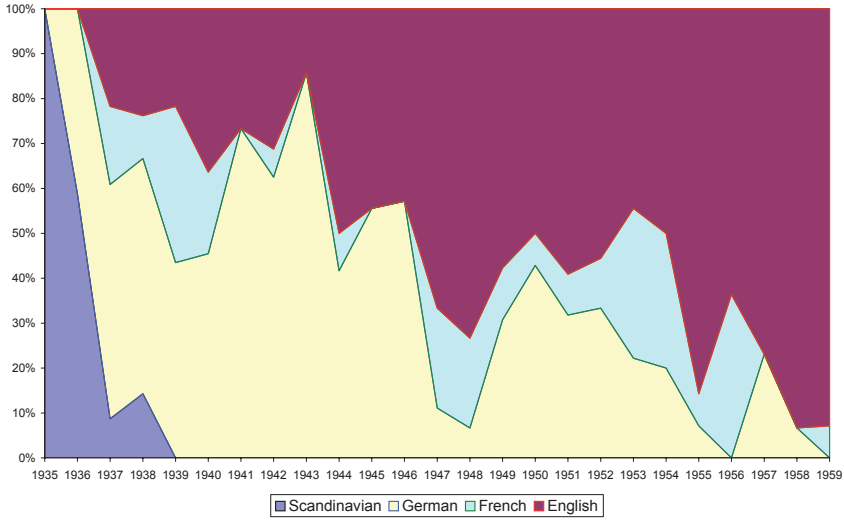

Figure 1 shows the percentage of articles and entries in the discussions section in *Theoria* in the different languages.

This was a conscious strategy by Petzäll, but his plans were repeatedly challenged by his co-editors.[9] Already when planning the journal, Petzäll ensured Kaila – who was doubtful towards the idea of a scientific journal in minor languages – that it had been his personal wish to publish the journal in English and French (and perhaps German), but that the editorial board had persuaded him that it would be better to start with the Scandinavian languages.[10] And a few years later, when discussing the change of editorial language into English for the 1937 volume, the initiative was questioned by especially the Uppsala philosopher Konrad Marc-Wogau (1902–1991) who disapproved of any internationalisation that would take place at the expense of Swedish contributions. Marc-Wogau, who was in charge of the journal's finances, warned that a radical internationalisation of *Theoria* could provoke a negative reaction from the sponsors of the journal, who had granted funds primarily in order to support Swedish philosophy. At least, Marc-Wogau argued, it would be very unwise to trumpet the fact that the journal was becoming

9 The co-editors were Gunnar Aspelin, Konrad Marc-Wogau and (from 1937) Torgny Segerstedt. In 1940, there was a reorganisation so that Petzäll was solely named "editor", Marc-Wogau "managing editor", while Frithiof Brandt, John Elmgren, Eino Kaila and Alf Nyman joined Aspelin and Segerstedt as "consulting editors".

10 Petzäll to Kaila, May 29, 1934.

less Swedish.[11] As a compromise therefore, the first English editorial of *Theoria*, that of 1937, stated that the intension was to promote *Swedish* philosophy by bringing it into contact with the very best of foreign scholarship.[12]

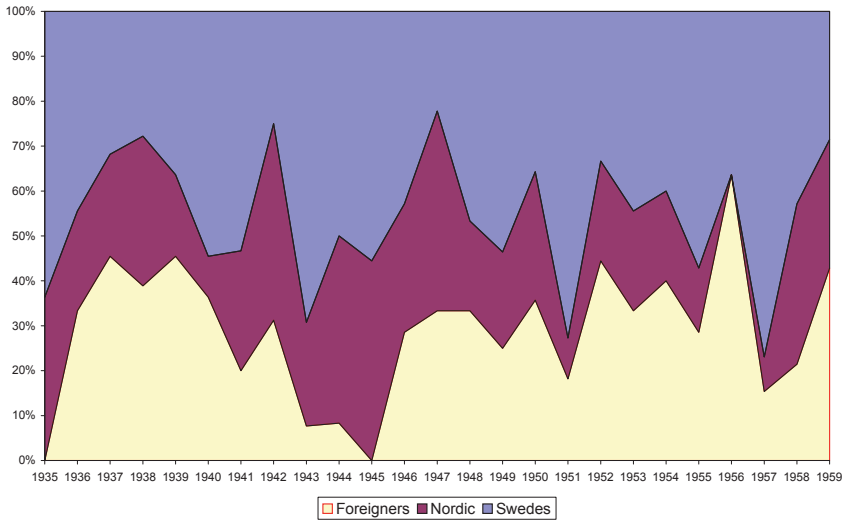

Figure 2 shows the percentage of the nationality of the authors of articles and of entries in the discussions section in *Theoria*.

The internationalisation was not only a matter of language. Petzäll worked hard to invite and persuade internationally recognised philosophers to participate in the discussions in *Theoria*, not least in order to ventilate what he thought was a stifling Swedish philosophical environment, dominated by a self-content and introvert Uppsala sect. With his international philosophical institute taking form in Paris, Petzäll nursed hopes that his two projects could create positive synergetic effects.[13] But he quickly found that his colleagues in Paris were marginally enthusiastic of his provincial journal. Therefore, in the autumn of 1937, Petzäll suggested that the subtitle of the journal would be changed from "a *Swedish* journal for philosophy" to "an *international* journal for philosophy". "Would you read a Hungarian journal for philosophy", he asked Marc-Wogau rhetorically.[14] This

11 Marc-Wogau to Petzäll, October 22, 23, 26 and 30, 1936. *Theoria* received grants from the Swedish government, from *Fornanderska fonden* and from the philosophical societies in the Nordic countries.

12 But Petzäll was still able to sneak in a small dig at what he conceived of as a peripheral and sectarian Uppsala School by claiming that "a small country with comparatively few workers in each branch of study easily runs the risk of being isolated, and fresh impulses are a vital necessity, not least in philosophy", see "Theoria 1937" [editorial], in *Theoria*, vol. 3, no. 1, 1937, p 1-2.

13 Petzäll to Aspelin, November 11, 1937.

14 Petzäll to Marc-Wogau, November 27, 1937. This and all subsequent translations from

time, however, Marc-Wogau's financial argument prevailed and the name of *Theoria* remained unchanged.

The repeated resistance that Petzäll's initiatives to internationalise *Theoria* provoked illustrates the tensions that cosmopolitan ambitions can rise in a national context. But the defensive reaction of Marc-Wogau was also characteristic of the Uppsala philosophers, who had a habit of greeting foreign ideas with great suspicion. The grand figures of Uppsala philosophy, Hägerström and Phalén, did not indulge themselves in international networks and rarely published their writing in international forums. Following the example and tradition of his predecessor Boström, Hägerström enjoyed being celebrated as a sovereign philosophical prophet.[15] Moreover, by the mid-1930s Uppsala philosophy was divided in two antagonistic wings with the disciples of Hägerström on one side and the pupils of Phalén on the other. The disputes concerned the ownership of different theories and aspects of Uppsala philosophy, most notably the method of conceptual analysis and the critique of subjectivism and metaphysics, and these tensions did most certainly play an important role in furthering sectarian tendencies among the Uppsala philosophers.

But in his international outlook, as well as in his tremendous social and organisational skills, Petzäll had a faithful compatriot in Otto Neurath, who had emerged as the driving force of the logical empiricist movement. In many letters Petzäll complained about the lack of understanding he was met with by his nationalistically blinded co-editors. "Möchte der Teufel die Nationalität aller Abstufungen hohlen!", Petzäll cried, volunteering to write the piece on "die Verderblichkeit aller Bodenständigkeit" in Neurath's projected Encyclopaedia.[16] Neurath gave Petzäll his utmost support and argued that it should been regarded as an honour for Sweden to host an internationally recognised philosophical journal.[17] However, as Petzäll was forced to balance between his cosmopolitanism and his national loyalties, he returned to Neurath a month later with considerably more understanding for Marc-Wogau and the other co-editors. Apparently, Petzäll wrote, "they had been afraid that I was turning *Theoria* into an organ for my institute".[18]

Scandinavian or German to English are by the present author.

15 Cf. Nordin, *Från Hägerström till Hedenius*, pp. 25-59.

16 Petzäll to Neurath, January 18, 1938. The similarities between the personalities of Petzäll and Neurath are striking. While their scholarly contributions have been somewhat marginalized (at least until recently in the case of Neurath), they have been highly appreciated as organizers of different scholarly relations, forums and projects. The correspondence of Petzäll is almost as vast, but no less minutely organised as the archives of Neurath. Petzäll had same the habit of preserving carbon copies of his letters, which means that the correspondence between Neurath and Petzäll can be studied in both Lund and in the Vienna Circle Archives.

17 Neurath to Petzäll, January 20, 1938.

18 Petzäll to Neurath, February 19, 1938.

The new *Erkenntnis*?

There was little doubt that Marc-Wogau and the other co-editors feared for the future of the journal when Petzäll moved to Paris in order to direct his international institute in the summer of 1937, and Petzäll soon abandoned the idea of combining his two projects. However, the discussions on the internationalisation of *Theoria* were also to a considerable extent, especially in the autumn of 1936, connected to the advance of logical empiricism in *Theoria*. In fact, one key event in the internationalisation process was a gathering in connection with the *Second International Congress for the Unity of Science* in 1936 which Petzäll arranged at Hotel Cosmopolite in Copenhagen. Here Petzäll discussed the future of his journal with leading logical empiricists (such as Frank, Neurath, Oppenheim, Hempel, Grelling, Somerville, Næss and Jørgensen) inviting them all to participate in *Theoria*. After the congress, throughout the autumn and winter of 1936–37, Petzäll spent much time at his typewriter discussing the internationalisation of *Theoria* with friends and colleagues. This was the context for the dispute with Marc-Wogau and, of course, for Kaila's suggestion that *Theoria* would replace *Erkenntnis* as the main organ of logical empiricism.

While the Uppsala philosophers conceived of the advance of logical empiricism as a threat to their position, Kaila saw it as a double opportunity. First, if *Theoria* became an organ for logical empiricism, this would most certainly influence the direction of Nordic and Finnish philosophy creating more opportunities for Kaila and his pupils. Secondly, as a main organ of the movement, *Theoria* would also be able to change the direction of logical empiricism as a whole, which by the mid-1930s had taken a couple of turns that Kaila did not support. Kaila was a European scholar in every sense of the term. He belonged to a generation of Finnish scholars to whom Germany was the cultural centre of the world, and now he was witnessing how this centre was moving across the English Channel, or even worse, over the Atlantic Ocean. Moreover, he was not particularly impressed with the philosophical achievements of these Anglo-American philosophers, and in particular with Alfred J. Ayer's *Language, Truth and Logic* (1936), which according to Kaila (and many other continental logical empiricists) gave a misleading picture of logical empiricism.[19] For Kaila, logical empiricism had little to do with the reductive and psychologising British empiricism to which Ayer had tried to connect it. In addition, Kaila was repulsed by the intensified political profile of logical empiricism which was a result of Neurath's increased influence on the movement.[20] Kaila knew that Petzäll had a conscious policy – strikingly parallel

19 Kaila, *Über den physikalischen Realitätsbegriff. Zweiter Beitrag zum logischen Empirismus*. Acta Philosophica Fennica, Fasc. IV, Helsinki: Societas Philosophica, 1941, p. 49.

20 In a letter dated November 10, 1935, to the Uppsala philosopher Einar Tegen, Kaila declared that he had lost his interest in the Vienna Circle as the "kulturfientlige" communist Neurath had become its leading soul (the letter is preserved in the Tegen-col-

to Sweden's official neutrality policy – of excluding all political declarations from *Theoria*.[21] From Kaila's point of view therefore, the possibility of *Theoria* replacing *Erkenntnis* equalled the prospect of a European and less politically engaged refuge for logical empiricism.

Theoria did undoubtedly become increasingly important for the logical empiricists in the late 1930s. In connection with the Copenhagen congress 1936, Petzäll agreed to Neurath's request of printing 80 separate copies of the advertisement in *Theoria* 1936:2 which were used as programme leaflets at the congress.[22] It also became natural for *Theoria* to review the publications of the movement.[23] And soon, the logical empiricists who were still residing on the European continent accepted Petzäll's invitations and started contributing to the journal themselves. Neurath wrote a general presentation of logical empiricism and engaged himself in a detailed discussion on "Physikalismus und Erkenntisforschung" with Petzäll.[24] From Prague, Philipp Frank exchanged ideas with both the Uppsala School and Ernst Cassirer,[25] who was a regular contributor to the journal as he was living in Swedish exile.[26] And before moving over to the United States, Carl Hempel

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lection at the University Library of Uppsala). And in his *Über den physikalischen Realitätsbegriff* (1941) Kaila stated that the publications of Neurath and Frank "belonged to Marxist literature rather than to general philosophy" (p. 49). Although by no means sympathetic with the fascist movements, Kaila's conservative political sympathies were at odds with the socialist agenda of Neurath and Frank.

21 In a letter dated November 5, 1934, Petzäll had persuaded Kaila to delete a passage on the foolishness of anti-Semitism in his article on "Einstein's religion", published in *Theoria*, vol. 1, no. 1, 1936.

22 Neurath to Petzäll, April 24, 1934; Petzäll to Neurath, May 9 and 28, 1936.

23 Such as Carnap's *Logische Syntax der Sprache* (1934, reviewed by Kaila in *Theoria*, vol. 2, no. 1, 1936), Ayer's *Language, Truth and Logic* (1936, reviewed by Edith Davidsson in *Theoria*, vol. 2, no. 2, 1936), Hempel and Oppenheim's *Der Typusbegriff in Lichte der neuen Logik* (1936, reviewed by Kaila in *Theoria*, vol. 2, no. 3, 1936), Popper's *Logik der Forschung* (1935, reviewed by Kurt Grelling in *Theoria*, vol. 3, no. 1, 1937), Tarski's *Einführung in die mathematische Logik und die Methodologie der Mathematik* (1937, reviewed by Jørgensen in *Theoria*, vol. 4, no. 1, 1938), and of course, the *International Encyclopaedia of Unified Science* (vol I-II, 1938–1939, reviewed by Hempel in *Theoria*, vol. 6, no. 1, 1940).

24 Neurath, "Den logiska empirismen och Wienerkretsen", in *Theoria*, vol. 2, no. 1, 1936; Neurath & Petzäll, "Physikalismus und Erkenntisforschung", in *Theoria*, vol. 2, no. 1, 2 & 3, 1936.

25 Frank, "Was versteht der Physiker unter der ‚Grösse' eines Körpers? Bemerkungen zu A. Phaléns Kritik der Einsteinschen Relativitätstheorie", in *Theoria*, vol. 3, no. 1, 1937; Frank, "Bemerkungen zu E. Cassirer: Determinismus und Indeterminismus in der modernen Physik", in *Theoria*, vol. 4, no. 1, 1938.

26 Cassirer, "Descartes' Wahrheitsbegriff. Betrachtungen zur 300-Jahresfeier des ‚Discours de la Méthode'", in *Theoria*, vol. 3, no. 2-3, 1937; Cassirer, "Zur Logik des Symbolbegriffs" and "Über die Bedeutung und Abfassungszeit von Descartes' 'Recherche de la Vérité par la lumiére naturelle'. Eine kritische Betrachtung", in *Theoria*, vol. 4,

launched his famous paradox of confirmation in *Theoria* – in French on the request of Petzäll who was about to promote the journal at the ninth international congress of philosophy (*Congrès Descartes*) in Paris 1937.[27] For Victor Kraft, who stayed in "inner exile" in Austria throughout the war, *Theoria* represented nothing less than an intellectual lifeline during many difficult years. In 1939, on the initiative of Kaila, *Theoria* appointed Kraft as a regular reviewer, and thereby Kraft was able to get his hands on foreign philosophical literature which he would have been very difficult for him to retrieve otherwise.[28] The lively correspondence between Kraft and Petzäll was suspended between August 17, 1942, and February 26, 1946, when Kraft contacted Petzäll reminding him of the small fee that he had been promised for an article back in 1937. Now, Kraft humbly requested the sum in provisions as his family was suffering from "Hungerödem". For a period of time, Petzäll sent monthly packages of food to Vienna, which Kraft compensated by submitting an article on "Logik und Erfarung" to *Theoria*.[29]

By any standards, the relations between *Theoria* and the logical empiricists were close, and it soon became quite natural for the editors to compare their journal with *Erkenntnis*. When Jørgensen had published a rather dull review of Carnap's *Logische Syntax der Sprache* in *Erkenntnis*,[30] Petzäll saw this as a golden opportunity and repeatedly urged Kaila to produce something superior.[31] And when Petzäll was about to reject Georg Henrik von Wright's essay on probability in 1937, Kaila wrote a furious letter in defence of his pupil and emphasised, among other things, that the article was well informed and up-to-date with the latest discussions in *Erkenntnis*.[32]

Still, there were many things that made *Theoria* different from *Erkenntnis*. Edited by Hans Reichenbach and Rudolf Carnap, and published jointly by the *Gesellschaft für empirische Philosophie* in Berlin and the *Verein Ernst Mach* in Vienna, it was beyond all doubt that *Erkenntnis* was an organ of the logical empiricist movement. Furthermore, as stressed by Hegselmann and Siegwart in their "Zur Geschichte der 'Erkenntnis'" (1991), the journal served primarily as a forum for *internal* discussion among the logical empiricists themselves, and not as

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no. 2, 1938; Cassirer, "Was ist Subjektivismus?", in *Theoria*, vol. 5, no. 2, 1939; Cassirer, "Thorild und Herder", in *Theoria*, vol. 7, no. 1, 1941.

27 Hempel, "Le probléme de la verité", in *Theoria*, vol. 3, no. 2-3, 1937; Letter from Petzäll to Hempel, dated March 4, 1937.

28 Kaila to Petzäll, August 5, 1939.

29 Kraft to Petzäll, May 30, 1946. The article was published in *Theoria*, vol. 12, no. 3, 1946.

30 Jørgensen, "Logische Syntax der Sprache", in *Erkenntnis*, vol. 4, no. 1, 1934.

31 Petzäll to Kaila, August 22 & October 19, 1935. However, unfortunately Kaila struggled to find anything philosophically exciting in what he thought should have been titled *Logische Syntax des Kalkuls* as it appeared to him as little but a systematisation of Gödel's ideas. Kaila to Petzäll, February 2, and August 13, 1935.

32 Kaila to Petzäll, March 7, 1937.

a platform for external confrontation.[33] *Theoria* on the other hand, continued to publish articles that represented many different philosophical convictions. To a certain extent, this strategy was forced upon the journal as there would hardly have been room for a journal devoted to a single philosophical movement in the small European periphery of Scandinavia. However, Petzäll did his best in turning eclecticism into an advantage and trademark of his journal. In the editorials of 1935 through 1937 it was repeated that *Theoria* "did not represent or favour any special trend of philosophical opinion" and that it aimed at creating "a forum for discussion between representatives of different methods and points of view".[34] Petzäll even tried to gather popular presentations of different contemporary philosophical movements in *Theoria*, hoping that these would serve as a starting point for further discussion. He had been promised such an article on neo-Thomism by Pater Bochénski, and he also tried to persuade representatives of the phenomenological movement to contribute. But ultimately, it was only the Uppsala School and the logical empiricists who accepted the invitation.

The confrontation between Uppsala philosophy and logical empiricism

Neurath was, of course, more than happy to write a presentation of logical empiricism for *Theoria* – "just tell me what your readers are expecting".[35] The result was a rather descriptive historical presentation, translated into Swedish as "Den logiska empirismen och Wienkretsen" and published in *Theoria* 1936:1. The mid-1930s was a hectic time for Neurath. His international networks had become increasingly important as he had been forced into exile in early 1934. Accordingly, he spent a lot of time travelling in Russia, the United States, and Scandinavia, lecturing and promoting his different endeavours – the Museum of Economy and Society, the method of pictorial statistics (ISOTYPE), and the Vienna Circle which he had gradually transformed into the Unity of Science project.[36]

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33 Rainer Hegselmann & Geo Siegwart, "Zur Geschichte der 'Erkenntnis'", in *Erkenntnis*, vol. 35, nrs. 1-3, 1991, pp. 461-471. On page 464 they refer to a 1935 letter to Carnap, in which Neurath explicitly opposed the idea of engaging in discussions with "traditional philosophy" in *Erkenntnis*, as this would happen at the expense of the much more important internal affairs.

34 "Theoria" [editorial], in *Theoria*, vol. 1, no. 1, 1935; "Theoria 1936" [editorial], in *Theoria*, vol. 2, no. 1, 1936; "Theoria 1937" [editorial], in *Theoria*, vol. 3, no. 1, 1937.

35 Neurath to Petzäll, November 14, 1934.

36 Cf. Cartwright, Cat, Fleck & Uebel, *Otto Neurath – Philosophy between Science and Politics*. Ideas in Context 38, Cambridge: Cambridge University Press, 1996, pp. 56-88.

After one of his guest lectures in Copenhagen in the spring of 1934, Neurath was contacted by the Danish philosopher of law Alf Ross who spoke highly of Hägerström for whom he had been studying in the late 1920s.[37] Neurath became interested, and turned to Petzäll for more information on this "unsichtbare Kirche Skandinaviens". Petzäll, however, did not understand what Neurath was referring to, and answered that the only Scandinavians interested in logical empiricism were Kaila, Jørgensen and himself. In the eyes of Petzäll, the Uppsala School had nothing in common with logical empiricism and it was only when Neurath explicitly mentioned Ross and Uppsala ("Es wäre sehr intressant, die Leute etwas kennen zu lernen") that Petzäll gave him the address of Marc-Wogau, who Petzäll deemed to be "ein ganz kluger Kopf", apparently *despite* being an Uppsala philosopher.[38]

Later the same year (1934), on another "missionary trip" to Scandinavia, Neurath met the Uppsala philosopher Einar Tegen in Lund, and invited him to give two presentations, one historical and one theoretical, on "Antimetaphysik im Uppsala" at the *First International Congress for the Unity of Science* in Paris 1935.[39] Tegen accepted and it was these presentations that formed the basis for the article on Uppsala philosophy in the first issue of the second volume of *Theoria* (1936). But Tegen's article "Kritisk objektivism" was of a completely different character than Neurath's presentation of logical empiricism. It was written as an explicit polemic against the Vienna Circle. Tegen attacked what he conceived of as the basic pillars of logical empiricism, i.e. empiricism and formal logic. In Uppsalian vein Tegen claimed that empiricism is epistemologically confused, as it fails to separate the content of sensation from the sensation itself. Uppsala philosophy on the other hand, refuted such subjectivistic positivism. Tegen called for a "revision of Kant's Copernican revolution" in order for philosophers to be able to turn their focus towards the objects instead of consciousness. Echoing a form of intentionality-thesis of Husserlian phenomenology, Tegen argued that we have to presuppose a direct and unimpeded access to the objects themselves.[40] These objects are by no means products of human consciousness; they are part of objective reality, i.e. part of the consistent, determinate and non-contradictory context

37 Ross to Neurath, April 23, 1934. The *Nachlass* of Alf Ross is preserved at the Royal Library in Copenhagen.

38 Neurath to Petzäll, November 21 and December 6, 1934. Petzäll to Neurath, December 4 and 11, 1934. However, in the *Nachlass* of Neurath, there are no letters between Marc-Wogau and Neurath until the Copenhagen congress in 1936. Cf. Nemeth & Stadler, *Encyclopedia and Utopia – the life and work of Otto Neurath (1882–1945)*. Dordrecht: Kluwer Academic Publishers, 1996.

39 Neurath to Tegen, April 10, July 3 and 28, 1935. The Tegen-collection at the University Library in Uppsala is not as complete as the Otto Neurath *Nachlass* at the Vienna Circle Archives, Noord-Hollands Archief, Haarlem, the Netherlands. Accordingly, my references as to the Tegen-Neurath correspondence are to the latter archive.

40 Tegen had been studying for Husserl in 1921 and 1922. Cf. Jan Bengtsson, *Den fenomenologiska rörelsen i Sverige – mottagande och inflytande 1900–1968*. Göteborg: Diadalos, 1991, p. 104-105.

of time and space.[41] Tegen agreed with the Vienna Circle that logical analysis was the supreme method of philosophy. But unfortunately, in the hands of the logical empiricists, it had been reduced into a formal logical syntax that treated language as a mere mathematical calculus.[42] Instead, Tegen argued, logical analysis had to be of "real significance" revealing contradictory ("dialectical" in the terminology of Phalén) ideas infecting the concept. In this way, logical analysis was of guiding significance with respect to natural science. Both quantum mechanics and the theory of relativity were refuted by Tegen, as they could not be expressed without presupposing something "real" which corresponds to our traditional conceptions of causality, time and space. In connection with the theory of relativity, Tegen gave special credit to his teacher Phalén and the book *Über die Relatvität der Raum- und Zeitbestimmungen* from 1922.[43]

Although Petzäll was unimpressed by Tegen's arguments, he was thrilled as he anticipated many replies from the logical empiricist camp. After all, Tegen had successfully articulated the main differences between Uppsala philosophy and logical empiricism: i.e. the different understandings of the method of logical analysis, the diverging attitudes towards empiricism, and the opposing views on the relation between philosophy and the special sciences.[44] And indeed, among others, Kaila announced that he was preparing a comment, and there were even rumours of Niels Bohr taking part in the discussion.[45] But as it turned out the only submitted replies to Tegen were Ernst von Aster's "'Kritischer Objektivismus' und 'Neopositivismus'" (*Theoria* 1936:3) and Jørgensen's "Causality and Quantum Mechanics" (*Theoria* 1937:1). After being dismissed from his chair in Giessen in 1933, and before joining Reichenbach in Istanbul in 1936, von Aster had found a refuge in the native country of his Swedish wife. He confronted Tegen by claiming that he had overlooked the nominalism of positivistic philosophy. That is, while the Uppsala philosophers claim that their logical analysis of a concept is of a

41 Tegen, "Kritisk objektivism", in *Theoria*, vol. 2, no. 1, 1936, p. 34-35 & 41-42.

42 Ibid., p. 53-54.

43 Ibid., p. 51-52. Adolf Phalén, *Über die Relatvität der Raum- und Zeitbestimmungen*. Uppsala: Skrifter utgivna av Kungliga humanistiska vetenskapssamfundet 21, 1:4, 1922. The Swedish debates on the theory of relativity and especially the repeated criticisms from the Uppsala philosophers are analysed in Thord Silverbark, *Fysikens filosofi – diskussioner om Einstein, relativitetsteorin och kvantfysiken i Sverige 1910–1970.* Stockholm/Stehag: Symposion, 1999.

44 With Nordin one should probably add the different attitudes towards international collaboration, and, most apparently, to each other, as a fourth distinguishing feature. See, Nordin, *Från Hägerström to Hedenius*, pp. 51-52 & 157-158. However, Nordin's emphasis on the Neo-Kantian roots of Uppsala philosophy (as opposed to the origins of logical empiricism and analytic philosophy) has been challenged by the recent interest in the Neo-Kantian roots of logical empiricism. Cf. Michael Friedman, *Reconsidering Logical Positivism*, Cambridge: Cambridge University Press, 1999.

45 Petzäll to Kaila, August 8, 1936; Kaila to Petzäll, August 11 and September 3, 1936; Petzäll to Tegen, March 11, 1936.

real/factual significance, the logical positivists acknowledge that a concept does not have a meaning on its own. Therefore, the Uppsala quest for something real or factual that corresponds to the concept "space" is "nicht sinnvoller, als die Frage, ob der von uns Jupiter genannte Planet wirklich so heisst."[46] Jørgensen's main point was that Tegen was wrong in claiming that there is such thing as a positivistic physics. It was not the epistemological starting point that determined the physicist; rather, it was the claims of the physicist that had certain epistemological or philosophical consequences that philosophers had to take into consideration.[47] Neither of these comments did much to bring the positions closer to each other, as they merely voiced the differences of opinion regarding the nature of logical analysis and the precedence of physics vis-à-vis philosophy.

The relative silence from the logical empiricists was a disappointment not only to Petzäll, but also to Tegen who had hoped to gain the attention of the Vienna Circle after his frustrating experiences at the congress in Paris 1935. Due to the large turn-up at the congress, there had only been time for Tegen to present one of the two presentations he had been asked to prepare, and this in only half the time he had been promised. In a couple of furious letters to Neurath, Tegen complained about the disrespectful treatment at the congress and the patronizing comments especially by Carnap. "Er glaubt ja wie ein Kind an alles was Einstein einmal gesagt hat" Tegen cried and stated that he would gladly choose metaphysics if it was the antimetaphysics of Vienna that constituted the alternative.[48] He had also been complaining about his experiences to Petzäll who confronted Neurath by declaring that the arrogant attitude of people like Carnap and comments like "Dies ist nicht unsere Sprache" and "Dies verstehen wir nicht", was rapidly giving logical empiricism a reputation of an exclusive members-only club.[49]

If Tegen and Petzäll had been German or Austrian philosophers, Neurath would hardly have bothered with them any further.[50] But as the world was developing, international relations were vital to Neurath and he did his best not to discomfort his Scandinavian allies. He responded by claiming that in comparison with other contemporary movements, logical empiricism was quite exceptional in its interest in dialogue with scientists from different countries and from different disciplines. "And if you are not satisfied with us," Neurath continued, "who are you going to turn to instead? It is not very likely that the Phenomenologists, the Neo-Kantians, the speculative metaphysicians or the theologising philosophers

46 von Aster, "'Kritischer Objektivismus' und 'Neopositivismus'. Bemerkungen zu Einar Tegen: Kritisk objektivism", in *Theoria*, vol. 2, no. 3, 1936, p. 351.

47 Jørgensen, "Causality and Quantum Mechanics", in *Theoria*, vol. 3, nr 1, 1937, pp. 115-117.

48 Tegen to Neurath, November 29, December 17 and 28, 1935.

49 Petzäll to Neurath, November 6, 1935.

50 Noted by Thomas Uebel at the *Networks and Transformations of Logical Empiricism: The Vienna Circle and the Nordic Countries* symposium in Helsinki, September 2007.

will show your thoughts and ideas any more interest and sympathy than we have already done".[51] After all, Neurath concluded, it is important that we unite the forces against traditional metaphysics.[52] Neurath also agreed to find a logical empiricist that would comment on Phalén's book in the *Erkenntnis*, and he asked Tegen to name some physicist who was applying the doctrines of Phalén, "as this would be helpful for the physicist we assign". According to Tegen, however, contemporary physicists were all blinded by Einstein and therefore utterly incapable of understanding Phalén. Neurath was perplexed and replied that that these ardent claims for autonomy would most surely cause the Uppsala philosophers serious problems: "How can your thoughts be merged with the results of the other sciences?"[53] But of course, the Uppsala philosophers did not share such ambitions and the correspondence between Tegen and Neurath did little to bring about an understanding between Uppsala and Vienna.

Eventually, however, Neurath assigned Frank to write the comment on Phalén, and at the congress in Copenhagen 1936, probably at the meeting arranged by Petzäll at Hotel Cosmopolite, it was decided that the Uppsala philosopher Gunnar Oxenstierna would write a reply and that *Theoria* would host the confrontation (and not *Erkenntnis* as Tegen had suggested earlier).[54] By the Phalénians this was conceived of as an extremely important opportunity to make a mark for themselves. When Petzäll was complaining about the length of Oxenstierna's response, Marc-Wogau rejoined that instead of submitting four or five separate replies, the Phalénians had assigned Oxenstierna to write a common response which therefore had to be of appropriate length.[55] Marc-Wogau and Tegen also fought back Petzäll's suggestion that the confrontation would be published in the discussions section and not as full-length articles.[56] These quarrels took place in the very same letters in which Petzäll was trying to launch his plans for an internationalisation of *Theoria* (see above), which made it natural for Marc-Wogau to associate it with the advance of logical empiricism.

However, the debate between Frank and Oxenstierna, published in *Theoria* 1937:1, did not bring the combatants any closer to each other. Frank characterised Phaléns book as a rather typical, albeit exceptionally well-informed, criticism of the theory of relativity from the point of view of traditional philosophy. But if the phenomena that Einstein referred to were observable when a hammer is thrown

51 Neurath to Petzäll, November 7, 1935.

52 Neurath to Tegen, December 8 and 23, 1935, and January 31, 1935.

53 Tegen to Neurath, February 17, 1936; Neurath to Tegen, January 31 and February 27, 1936.

54 It is not unlikely, that the editors of *Erkenntnis*, as well as Neurath himself, would have found this discussion beyond the limits of an *internal* affair.

55 Petzäll to Marc-Wogau, October 21; Marc-Wogau to Petzäll, October, 22 and 30, 1936.

56 Petzäll to Marc-Wogau, October 28, 1936; Marc-Wogau to Petzäll, November 6, 1936; Tegen to Petzäll, November 1, 1936.

in the air, Frank argued, the whole idea of space or time independently of a defined co-ordinate system would be utterly inconceivable even for Phalén. Frank conceded that it might be possible to give meaning to Phaléns concept of "real space". This would only require that one construes a co-ordinate system with, for example, the sun or some fixed star as the centre point. However, such a terminology would hardly be useful, Frank claimed, and by contrast Einstein's theory has already proved quite fruitful among contemporary physicists.[57] Oxenstierna's main point was that Frank had misinterpreted Phalén's intentions. Despite its title, Phalén's book had not been intended as a criticism of Einstein, but as a careful philosophical analysis of the concepts of time and space. Frank was also mistaken in his insinuations that the Uppsala philosophers were intruding into a scientific field of which they had little theoretical expertise, i.e. physics. Quite contrary, Oxenstierna argued, it was Einstein who was embarking on the territory of philosophy, and this prompted a response from the philosophers. The final part of his article, Oxenstierna devoted to an elaboration of Phalén's central argument, that it is impossible to define the concept of 'length' without presupposing related concepts such as 'distance' and 'extension', i.e. without presupposing that there was a "real length" of objects.[58] The confrontation between Uppsala philosophy and logical empiricism seemed to have stalled at the opposing views on the nature of logical analysis and on the relation between philosophy and the natural sciences, and Petzäll made no further efforts to promote the dialogue.[59]

The consolidation of Uppsala philosophy and logical empiricism

The 1930s was a troublesome time for the disciples of Phalén. Not only were they treated harshly by the logical empiricists in the discussion on contemporary physics, they were also trailing in their fierce local battle against the other wing of the Uppsala School, the Hägerströmians. Much due to the stormy debate on Hägerström's emotive value theory, pejoratively labelled "value nihilism" by its critics, Hägerström and his disciples were generally considered, both within the academia

57 Frank, „Was versteht der Physiker unter der ‚Grösse' eines Körpers? Bemerkungen zu A. Phaléns Kritik der Einsteinschen Relativitätstheorie", in *Theoria*, vol. 3, no. 1, 1937, pp. 76-89. The confrontation between Frank and Oxenstierna is portrayed in greater detail in Silverbark, *Fysikens filosofi*, chapter 8, especially pp. 286-292.

58 Oxenstierna, „Was versteht der Physiker unter der ‚Grösse' eines Körpers? Bemerkungen zu Philipp Franks vorherstehenden Artikel", in *Theoria*, vol. 3, no. 1, 1937, pp. 90-114.

59 There was, however, one more contribution to the debate. Viktor Kraft, probably motivated by his increasing isolation in Austria, published an article called "Die Grösse eines Körpers gemäss der Relativitätstheorie" (*Theoria*, vol.6, no. 1, 1940) in which he tried to bring the positions closer to each other. But his efforts were rather late, as Oxenstierna had died in 1939 and Frank had migrated to the United States in 1938.

and in the public debate, to be the leading representatives of Uppsala philosophy. This caused much frustration among the followers of Phalén, who, especially after the death of their master in 1931, found themselves set aside and repeatedly neglected in the competitions for the precious philosophical chairs in Sweden.[60] The situation called for action, and eventually, after one decade of different efforts, the Phalén-wing succeeded in obtaining the right to the philosophical legacy of Uppsala. Of decisive importance was the third issue of *Theoria* in 1939, which both symbolically and theoretically marked the death of Uppsala philosophy in its original form. Symbolically, as it contained obituaries of two leading representatives of the movement – both Hägerström and Oxenstierna passed away during the summer and autumn of 1939, and theoretically, as it witnessed the first attempts at consolidating Uppsala philosophy with logical empiricism by the young Phalénian Ingemar Hedenius.

The opportunity occurred on the occasion of Cassirer's book *Axel Hägerström, Eine Studie zur schwedishen Philosophie der Gegenwart* (1939).[61] For Petzäll, this book constituted yet another chance to confront Uppsala philosophy with the outside world, and this time he had managed to persuade Hägerström himself to promise a comment on the book.[62] Unfortunately, Hägerström passed away on July 7th before producing a manuscript. Only eleven days later, the Phalénian pupil Hedenius, apparently encouraged by Marc-Wogau, contacted Petzäll volunteering to replace Hägerström in the debate. Petzäll urged Hedenius to submit a manuscript as soon as possible, thereby effectively giving a Phalénian a grand chance to make his mark as the new Hägerström.[63] Hedenius set out to make the most of this opportunity, but his article evolved beyond all reasonable limits and the resulting opus had little to do with both Cassirer and Hägerström. It was therefore decided that Hedenius would extract the parts that directly commented on Cassirer's book and publish them separately in the discussions section. Moreover, Petzäll (reluctantly) agreed to Hedenius' suggestion that the main article would be divided into two parts, of which the first would be published in *Theoria* 1939:3, while the second – in which Hedenius promised to engage more directly in confrontation with Cassirer and Neo-Kantianism – would appear in a forthcoming issue.

The first part of the article, called "Begriffsanalyse und Kritischer Idealismus (I)", was intended as an exposition of the Uppsalian method of logical analysis.

60 These frustrations rocketed in 1933, when Hägerström chose to support the outsider Anders Karitz, and not the Phalénian pupil Oxenstierna, in the competition for Phalén's old chair in theoretical philosophy. Cf. Nordin, *Från Hägerström till Hedenius*, pp. 93-114.

61 Cassirer, *Axel Hägerström, Eine Studie zur schwedishen Philosophie der Gegenwart*. Göteborg: Göteborgs högskolas årsskrift XLV, 1939. For an analysis on this book and its historical background, see Hansson & Nordin, *Ernst Cassirer: The Swedish Years*. Bern: Peter Lang Verlag, 2006.

62 Hägerström to Petzäll, May 12, 1939.

63 Hedenius to Petzäll, July 18, 1939; Petzäll to Hedenius, July 29, 1939.

To a large extent, it was a reiteration of the "dialectic" method of Phalén, but there were also some important signs of an impending change, especially as Hedenius referred to "the rising awareness of the importance of logical analysis, evident among philosophers both in Sweden and abroad", thus implicitly connecting Uppsala philosophy to the Cambridge School and the Vienna Circle.[64] Hedenius also seemed to subscribe to certain elements of these philosophies, which resulted in a somewhat ambivalent account of the nature of logical analysis. On one hand, he appeared to break with the psychologistic nature of the Phalénian doctrine, i.e. its equation of conceptual analysis with an analysis of the ideas ("Vorstellungen") associated with the concept. Instead, Hedenius claimed, logical analysis had to focus on the class of facts ("Tatsachenklasse") that the concept denotes.[65] On the other hand, Hedenius did not criticise Phalén in the article, on the contrary, he seemed to be on a mission to defend his master. Hedenius disapproved of the principle of verification, and criticised Arne Næss' idea of doing conceptual analysis by asking 300 non-philosophers of their understanding of 'truth'. Such Neurathian "Gelehrtenbehavioristik" contains nothing of philosophical interest as it fails to reach the "innere Struktur", "wirklichen Inhalt" or "richtige Deutung" of the concept.[66] Hedenius still adhered to an Uppsalian form of conceptual realism believing that a concept has a "real" meaning which can be found by means of logical analysis. This view was quite remote from the nominalistic approach of logical empiricists such as Carnap. As to the relation between philosophy and natural science, Hedenius conceded that physicists occasionally can contribute with important insights to the field of philosophy, in particular concerning the logical analysis of such concepts as "time" or "space". But these new theories are often presented in dilettantish and contradictory ways by the physicists and are therefore in desperate need for improvement by a philosopher trained in conceptual analysis. According to Hedenius, this had been the main source for the disagreement between Oxenstierna and Frank.[67] Hedenius maintained that it was the philosophers who were the main authorities on conceptual analysis, and that it was their task to "correct" the ideas of both the general public and the natural scientists. Such a view was characteristic of traditional Uppsala philosophy, but, of course, incompatible with the common sense approach of the Cambridge School on one hand, and with the constructivism of logical empiricism on the other.

A second part of "Begriffsanalyse und Kritischer Idealismus" was never published. Hedenius was delayed by military service, and when he finally submitted a manuscript, in February 1941, it was again far too long to Petzäll, and Hedenius was no longer interested in reworking the paper.[68] But his ideas continued to de-

64 Hedenius, "Begriffsanalyse und kritischer Idealismus (I)", in *Theoria*, vol. 5, no.3, 1939, p. 287.
65 Ibid., p. 289 & 292.
66 Ibid., pp. 294-298.
67 Ibid., p. 312, note 1.
68 Hedenius to Petzäll, February 2, 1941; Petzäll to Hedenius, February 4, 1941.

velop in the direction of logical empiricism and the Cambridge School. In 1943 Hedenius tried to reconcile Moore's common sense realism with the logical positivist thesis that absolute knowledge of empirical things is impossible (i.e. such statements are always hypothetical), thus openly pledging to new philosophical authorities.[69] And the next year, with the article "Überzeugung und Urteil" (1944) Hedenius not only parted with, but also explicitly criticised, the psychologism of Phalén. The subject of the analysis was no longer isolated concepts or the ideas attached to them, but sentences, judgements or statements. This article also witnessed Hedenius' familiarity with formal logic, something which had been alien to traditional Uppsala philosophy.[70]

However, by this time Hedenius had already made a name for himself developing another theme which he also introduced in the third number of *Theoria* in 1939. Ironically, while Hedenius' extensive article remained incomplete, his comment "upon a small, but important detail in Cassirer's critique of Hägerström's moral theory"[71] marked the start of his very successful career as a moral philosopher. The small detail emphasised by Hedenius was that Cassirer incorrectly associated Hägerström's theory with ancient Sophist moral theory. According to Hedenius, this was a crucial mistake, because contrary to Hägerström, a Sophist moral theory – given its most eloquent formulation in the so called *homo mensura* thesis – accepted that value judgements *can* be true or false. Translated into modern philosophical vocabulary, Hedenius argued, the *homo mensura* thesis entails that the sentence "A is good" is logically equivalent with "A is good *for me*" – which is an expression of a judgement that is true if the person who utters it actually thinks that A is good, and false if he does not. And therefore, while Cassirer was correct in claiming that the *homo mensura* thesis entails a moral relativism, he was wrong in claiming that such a relativistic or subjectivistic theory is compatible with Hägerström's theory, which not only denies the existence of objective values, but also the possibility of (true and false) value judgements.[72]

It was not in his support of the emotive theory as such, that Hedenius brought Uppsala philosophy closer to the Vienna Circle (as we know, emotivism was not univocally supported by the logical empiricists). Rather, it was the way in which Hedenius presented the theory that echoed Ayer and Carnap.[73] For Hägerström, the value theory had primarily been a psychological theory on the nature of moral

69 Hedenius, "*Über sog. Common-sense-Realismus*. Bemerkungen zu Folke Leanders Aufsatz 'Analyse des Wirklichkeitsbegriffs (I)'", in *Theoria*, vol. 9., no. 2., 1943, pp. 162-173.

70 Hedenius, "Überzeugung und Urteil", in *Theoria*, vol. 10, no. 2, 1944, p. 120-170.

71 Hedenius to Petzäll, August 12, 1939.

72 Hedenius, "Über den alogischen Character der sog. Werturteile. Bemerkungen zu Ernst Cassirer: Axel Hägerström. Eine Studie zur schwedischen Philosophie der Gegenwart", in *Theoria*, vol. 5, no. 3, 1939, pp. 314-329.

73 Cf. Nordin, *Ingemar Hedenius – en filosof och hans tid*. Stockholm: Natur och kultur 2004, pp. 102-115.

representations ("Vorstellungen"). His main influences were the anthropological relativism of Edvard Westermarck, and, more importantly, the Austrian *Werttheorie* of Brentano, Meinong and von Ehrenfels. Following the act-psychological terminology, Hägerström argued that a judgement ("Urteil") is a representation of something as existent, and that only judgements can be true or false. But a moral representation cannot be true or false as it does not involve a representation of something as objectively existent. Instead the moral representations always involve a feeling.[74] Hedenius on the other hand, found the core of value nihilism in the distinction between a linguistic expression and a judgement.[75] The central claim of the theory, according to Hedenius, is that there are many linguistic expressions which have the same grammatical form as a judgement but which nevertheless do not express something true or false. So, in the hands of Hedenius, Hägerström's theory was stripped of its psychological preconditions and turned into a semantic theory on the proper analysis of value statements. Hedenius' comment to Cassirer was also the first time that Hägerström's value theory was explicitly related to Ayer.[76] Such a relation was, for instance, not noted by Cassirer in his book, even if Cassirer's ambition had been to place Hägerström and Uppsala philosophy in a historical and contemporary philosophical context.[77]

After the third issue of *Theoria* 1939 – and to the frustration of Petzäll who was still waiting for the second part of the epistemological article – Hedenius concentrated on writing semi-popular articles on Hägerström and value theory for the Social Democratic journal *Tiden* (later gathered and published as the successful monograph *Om rätt och moral*, 1941). Hedenius adopted the pejorative label "value nihilism", launched in the early 1930s by Hägerström's critics, and turned it into a commonly accepted name for the theory in Swedish. By colonising the value nihilistic theory of Hägerström, Hedenius and the Phalénians stood out as the unquestioned champions of the battle over the legacy of Uppsala philosophy. Hedenius became the new Hägerström, updated with the latest innovations in logical empiricism and analytic philosophy. Logical empiricism made its break-

74 For an analysis of the value theory of Hägerström see Bo Petersson, *Axel Hägerströms värdeteori*, Filosofiska Studier utgivna av Filosofiska Föreningen och Filosofiska Institutionen vid Uppsala Universitet, nr. 17, 1973.

75 Hedenius, "Über den alogischen Character", p. 315-316. Moreover, Hedenius also emphasised that the precise nature of these 'judgements' is unimportant for the theory, but remarks that they do not necessarily have to entail anything psychological. Two years later, Hedenius had moved on to distinguishing between sentence ("sats") and statement ("påstående"). See Hedenius, *Om rätt och moral*. Stockholm: Tiden 1941.

76 Hedenius, "Über den alogischen Character", p. 315 (footnote). In the same footnote, Hedenius explicitly states that he is expressing the theory in a different way than Hägerström himself.

77 A similarity between Hägerström and logical empiricism was noted by Cassirer only with regard to the refutation of metaphysics, but even here Cassirer stressed the different conditions for the refutations. See, Cassirer, *Axel Hägerström*. chapter 1.

through in Sweden only after having been integrated within, or even disguised as, a national tradition.[78]

Conclusions

Hedenius' move is hardly exceptional in the history of 20th century analytic philosophy. The story resembles the fate of logical empiricism in England, where it was introduced by Ayer as "the logical outcome of Berkeley and Hume" – this on the very first page of *Language, Truth and Logic* (1936).[79] It also resembles the fate of logical empiricism in the United States, where it was amalgamated to the American pragmatic tradition by such scholars as Charles Morris and Willard Van Orman Quine (of course, with the important exception that in the US, the emigrated logical empiricists, most notably Carnap, were of operative significance themselves). Indeed, it should not be considered as extraordinary that philosophical ideas are transformed and moulded anew when they move from one context to another. Cultural transfers are not mechanical processes of import and export; they always involve adjustment and re-interpretation. When the Vienna Circle was forced to leave its original Central European, Austrian and Viennese environments, logical empiricism merged with various other local philosophical trends and became an ingredient in that what was later to be called analytic philosophy.

In this sense, there is nothing false in the received view according to which Hägerström and the Uppsala School are considered to be the origin of the Swedish analytical tradition. However, the picture must be completed with a proper appreciation of the role of cultural transfers and transnational actors. Petzäll was of pivotal significance in changing the direction of Swedish philosophy. His two large items on logical empiricism may have failed to reach proper recognition, but Petzäll nevertheless provided the instrument for the transformation and internationalisation of Swedish philosophy – the journal *Theoria*.

Petzäll's cosmopolitan ambitions were not always appreciated by his Swedish colleagues. This is perhaps the fate of an internationally oriented intellectual in a small European periphery, where a passionate enthusiasm for a foreign movement easily compromises your credibility in the national context. But the national context may provide you with different openings to confront this dilemma. Although by no means overlooking their national commitments, Kaila, Næss and Jørgensen

78 It should be noted that this was not solely Hedenius' achievement. Marc-Wogau and Anders Wedberg (1913-1978) made similar conversions from Uppsala philosophy to analytic philosophy during the 1940s. For Marc-Wogau the Cambridge School was even more important than it was for Hedenius, and in the case of Wedberg, it seems as if his training in formal logic was a result of his stay in the USA 1939-43. Cf. Nordin, *Från Hägerström till Hedenius*, pp. 147-157.

79 Ayer, *Language, Truth and Logic*. London: Victor Gollancz, 1936, p. 1.

were nevertheless able to take advantage of their connections to the Vienna Circle in their domestic scholarly debates and meritocratic struggles. This did not work for Petzäll in the Uppsala dominated Sweden, and instead he tried to make his way by an extremely inclusive and eclectic approach, always looking for an opportunity to confront the Uppsala School with other philosophies. His efforts were not without success, but he received little credit for them. It is a great irony from Petzäll's perspective, that when logical empiricism eventually did break through in Sweden, it was presented, not as a celebrated foreign innovation, let alone as a cosmopolitan philosophy, but as a natural continuation of a national tradition.

The journal *Theoria* did not replace *Erkenntnis*, but Kaila's wishes from 1936 were still, at least partly, realised. With the "converted" Uppsala philosopher Marc-Wogau succeeding Petzäll as editor-in-chief, *Theoria* became a leading forum for analytic philosophy in Sweden and the Nordic countries. And for many years, it was one of the very few mediums for analytic philosophy outside the Anglophone world.

Centre for Nordic Studies
Department of Philosophy
Unioninkatu 38 A (P.O. Box 59)
00014 University of Helsinki
Finland
johan.strang@helsinki.fi

Carl-Göran Heidegren

Positivism before Logical Positivism in Nordic Philosophy

The concept of "style of thought" or *Denkstil* is today probably primarily associated with the Polish microbiologist and philosopher of science Ludwik Fleck and his writings from the 1930s. It was however used quite extensively already by Karl Mannheim in his writings on the sociology of knowledge from the 1920s. Quite interestingly, the concept of style of thought was also used twice by Rudolf Carnap in the preface to *Der logische Aufbau der Welt* from 1928. No doubt, the concept must have been in the air during the interwar period.

In what follows I will characterize the early positivism in Nordic philosophy in terms of a certain style of thought or as a certain habitus of thought. I will single out six basic convictions about philosophy and how to do philosophy that make up a more or less common ground and starting point for the early philosophical positivists in the Nordic countries. However, to begin with I will motivate why I prefer to use the designation "the positive spirit" rather than "positivism". In the latter part of the article I will then dwell on some more specific topics and figures relating to the positive spirit or positivism before Logical positivism in Nordic philosophy.

I

Although Comte himself published a *Discours préliminaire sur l'esprit positif* in 1844, I prefer to use the expression "the positive spirit" for a broad philosophical tendency or movement of thought that grew strong during the second half of the 19th century, a movement of thought much broader than the positivism of Comte and his adherents, and also much broader than the positivism of Ernst Mach and Richard Avenarius. This distinction was by the way made quite explicitly already in the 1870s by the French psychologist and philosopher Théodule Ribot. In his book *La psychologie anglaise contemporaine* from 1870 Ribot distinguishes between "positivism", as the doctrine of Comte, and what he calls *l'esprit positif*, which he characterizes as "the modern scientific spirit" or "the pure scientific spirit".[1] Later in the same book Ribot writes about Herbert Spencer: "He is completely imbued with the positive spirit", but no adherent to "the positive

1 Théodule Ribot, *La psychologie anglaise contemporaine (école experimentale)*, deuxième édition, revue et augmentée. Paris: Baillière 1875, p. 102.

philosophy".[2] Some years later, in an article written for the newly founded British philosophical journal *Mind*, Ribot states: "Positivism, which is a rounded off and finished doctrine claiming to be unchangeable, must not be confounded with the positive spirit, which is but a method of philosophizing."[3] The very same distinction is found by the Danish philosopher Harald Høffding (1843–1931) in his book *Den engelske Philosophi i vor Tid* [Contemporary English Philosophy] from 1874. No doubt inspired by Ribot, Høffding there characterizes *den positive Aand*, the positive spirit as "an intellectual movement, which has emanated from the whole of modern culture and science, and of which positivism itself", i.e. the doctrine of Comte, "is but a particular form".[4] Thus, my use of the terms the positive spirit and positivism is derived from the use made by significant philosophers from the period under study.

My characterization of the philosophical style of thought that I call the positive spirit will be situated between what I take to be a too broad characterization on the one hand and a too narrow characterization on the other hand. Too broad and unspecific is the characterization that Fritz Ringer gives of positivism in the German university culture from the 1840s into the 1880s: "The positivism of the intervening decades was less a set of stated theories than a cluster of vaguely scientific attitudes, tacit assumptions, and research practices."[5] Too narrow or too specific is on the other hand the self-characterization that Friedrich Albert Lange makes in a letter to a friend from 1858: "I regard all metaphysics as a kind of madness possessing only an aesthetic and subjective justification. My logic is calculus of probabilities, my ethics are moral statistics, my psychology rests on physiology; in a word, I try to operate only within the exact sciences."[6]

Trying to steer a middle way between a too unspecific and a too specific use, my characterization of the positive spirit as a certain philosophical habitus or style of thought is contained in the following six points. My examplifications are collected from a number of Nordic philosophers active in the period from the 1870s and onwards.[7]

1. Philosophy must have its foundation in experience. This conviction does

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2 Ibid., p. 244.

3 Théodule Ribot, "Philosophy in France", *Mind* 2, 1877, pp. 375-376.

4 Harald Høffding, *Den engelske Philosophi i vor Tid*. Kjøbenhavn: Philipsen 1874, p. 9.

5 Fritz Ringer, *Fields of Knowledge. French Academic Culture in Comparative Perspective 1890-1920*. Cambridge: Cambridge UP 1992, p. 197.

6 Quoted in Klaus Christian Köhnke, *The Rise of Neo-Kantianism. German Academic Philosophy between Idealism and Positivism*. Cambridge: Cambridge UP 1991, p. 151.

7 For a comprehensive presentation and analysis of the developments and trends in Nordic university philosophy in the late 19th and early 20th century, see my book *Det moderna genombrottet i nordisk universitetsfilosofi 1860-1915*. Göteborg: Daidalos 2004.

not necessarily entail a radical empiricism. Sometimes it even amounts to the formula: experience – yes, empiricism – no. For example Høffding writes that his investigations in the field of psychology led him "to maintain the basic thoughts of criticism as the result of psychological analyses and hypotheses", i.e. the basic thoughts of Kantian philosophy.[8] In this way Høffding characterizes the essential tendency of his book *Psykologi i Omrids paa Grundlag af Erfaring* [Psychology in Outline Based on Experience] from 1882, a book that soon was to be translated into several languages and which made its author famous in the philosophical world. "The criterion of truth", Høffding writes, "we must seek within the world of consciousness, not outside it. And that criterion can be nothing else than the inner harmony and consistency between all thoughts and experiences."[9] A key science for the men of the positive spirit was empirical psychology, including experimental psychology. The Norwegian philosopher Arne Løchen (1850–1930) wrote in a letter to Høffding from 1891: "Psychology is the great science of the future."[10] And his fellow-countryman Anathon Aall (1867–1943) some years later laid down that psychology makes up "a main part of philosophy, and a part on which the whole discipline rests".[11] It deserves to be mentioned that Aall, like the Finnish philosopher Hjalmar Neiglick (1860–1889), was trained in experimental psychology.[12]

2. Philosophy must be based upon inductive logic and experimental method, not upon a priori development of concepts. Speculative philosophy is polemically discarded by the advocates of the positive spirit as *Begriffsdichtung*, as conceptual poetry. The Danish philosopher Kristian Kroman (1846–1925) criticises what he calls "the speculative, lyrical systems of the Romantic age" for letting "affection, imagination and passion" run away from rigorous "thought".[13] For the Finnish philosopher and sociologist Edvard Westermarck (1862–1939) ethics is not a normative science, but a discipline whose task it is "to find out the laws which our ethical will actually follows", by way of "careful inductive investigation".[14]

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8 Harald Høffding, "Die Philosophie in Dänemark im 19. Jahrhundert", *Archiv für Geschichte der Philosophie* 2, 1889, p. 72.

9 Harald Høffding, *Psykologi i Omrids paa Grundlag af Erfaring*. København: Philipsen 1882, p. 266.

10 Letter from Arne Løchen to Harald Høffding (January 28th, 1891), Det kongelige Bibliotek, København.

11 Anathon Aall, *Vort sjælelige og vort ethiske liv. Kort fremstilling af den filosofiske videnskab*. Kristiania: Aschehoug 1900, p. 23.

12 Neiglick conducted research at Wilhelm Wundt's psychological laboratory in Leipzig in the mid-1880s, and was a close friend of the French sociologist Émile Durkheim. In the years before his early death Neiglick seems to have been preoccupied with studies on the origin and development of the concepts of society and law.

13 Kristian Kroman, *Den exakte Videnskabs Indlæg i Problemet om Sjælens Existens. En kritisk Undersøgelse*. Kjøbenhavn: Schubothe 1877, p. 3 and p. 7.

14 Protocol of the Philosophical Society in Helsinki (May 8th, 1896). See Juha Manninen/Ilkka Niiniluoto (Eds.), *Ajatuksen laboratorio. Filosofisen yhdistyksen pöytäkirjat*

And Høffding summarizes this conviction in the words: "The epoch seems on the whole to be gone, when it was deemed possible to grasp the Absolute by way of dialectical development of concepts."[15]

3. Philosophy should deal with clearly delimited research questions and open problems, instead of trying to construct all-inclusive and finished philosophical systems. Thus philosophy should be transformed into a research science in the sense of a continuous quest for knowledge, where every result is preliminary and might be revised in the light of new experience. Høffding on several occasions underlines the necessity of *Enkeltundersøgelser*, of particular investigations. And Kroman, in his dissertation from 1877, conducts what he calls a *Smaaundersøgelse*, a minor investigation, on a clearly delimited research question: "*Has* science proved that there is a soul, or *has* it proved that there is no soul?".[16] Late in life Høffding summarized his life-long work in the field of philosophy in the following way: "My philosophy, if I may use this word, became a doctrine of problems … A rounded off system could never be the result of my strivings …".[17]

4. There is no fundamental difference between philosophy and the special sciences. The most radical adherents of the positive spirit held that philosophy has neither a specific method nor a subject matter of its own. The Norwegian Løchen for example rejects any "attempt to rescue philosophy as an autonomous science by way of putting it in an *exclusive* relation to the other sciences".[18] Rather than a qualitative difference there exists only a quantitative one, i.e. a difference in scope, and just as the other sciences philosophy strives for genetic explanations, i.e. for explanations in terms of cause and effect. The Swedish philosopher Axel Herrlin (1868–1935) also refuses to see any radical difference between philosophy and the special sciences. In his view philosophy is itself a special science, which "has its basis in *all* the sciences, insofar as they strive to explain the connection between the phenomena, the spiritual just as well as the corporeal".[19] Thus Herrlin stresses the continuity between philosophy and the special sciences, rather than their qualitative difference. The task of philosophy is to summarize and systematize the results of the special sciences, and to develop a general theory of science.

5. Metaphysical hypotheses, if put forward at all, rounds off the philosophical activity, rather than giving philosophy its foundation. Høffding writes: "The really superior metaphysician is the one whose ideas move in the direction that

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1873-1925. Helsingfors: Suomen Filosofinen Yhdistys 1996, p. 276.

15 Høffding, *Den engelske Philosophi*, op. cit., p. 16.

16 Kroman, *Den exakte Videnskabs Indlæg*, op. cit., p. 2.

17 Harald Høffding, *Erindringer*. København: Gyldendal 1928, p. 70.

18 Arne Løchen, "Hvad er filosofi? En prøveforelæsning for den filosofiske doktorgrad", *Nyt Tidsskrift* 5, 1886, p. 305.

19 Axel Herrlin, *Filosofi och fackvetenskap*. Lund: Gleerup 1905, p. 76.

the dominant tendency of empirical knowledge already points out."[20] And Løchen writes: "[T]he picture of the world that metaphysics project is not a fixed and eternal result; it changes and develops just as the special sciences themselves."[21] This position was by the way also defended by the Finnish philosopher Thiodolf Rein (1838–1919), who was no adherent of the positive spirit: "Metaphysics ought to be not an a priori science, but should directly or indirectly rest upon the empirical sciences."[22] The quotation shows that there existed no watertight bulkheads between the different tendencies in late 19th century philosophy. What I call the positive spirit is an *ideal typical construct*, which individual philosophers in the Nordic countries fit more or less well. However, more radical about metaphysics is the Norwegian Aall in a series of lectures that he delivered in 1898. There he states that "among the so called metaphysical questions we find nothing that doesn't belong to either the science of religion, mathematics, history, physics, ethics or psychology".[23] Ten years later, in his inaugural lecture at the university, Aall is more moderate. The metaphysical questions are now said to belong to the research program of philosophy: "To repress them would be to use force on the human spirit."[24]

6. Philosophical knowledge is no privileged wisdom for the few, but is and should be a common property that in principle can be communicated to every thinking being. To stick to the scientific method for Kroman means to keep a close watch on the dividing line between what on the one hand "can be guaranteed logically", and which therefore can be "transported from one hand to another without losing its force",[25] i.e. by way of compelling arguments, and what on the other hand is nothing else than free imagination, subjective inclination or wishful thinking, and which no one else can be forced to accept. Philosophy is certainly no science within easy reach for everyone, but neither is it a hidden wisdom accessible only for the chosen few.

The following quotation, taken from Høffding's book on the contemporary English philosophy from 1874, sums up very well several key aspects of the philosophical habitus that I call the positive spirit:

> Through conducted special investigations as well as through the assimilation of the results of the empirical sciences is developed a common ground for all philosophical research. Thus philosophy is about to enter what Comte calls the positive stage. (…) The different schools lose their significance the more clarity is reached about on the one hand the impor-

20 Høffding, *Psykologi i Omrids*, op. cit., p. 16.
21 Løchen, "Hvad er filosofi?", op. cit., pp. 322-323.
22 Thiodolf Rein, *Försök till en framställning af psykologien eller vetenskapen om själen*, senare delen, förra afdelningen. Helsingfors: Edlund 1891, p. 50.
23 Anathon Aall, "Grundtræk af filosofien i vor tid", *Samtiden* 9, 1898, p. 325.
24 Anathon Aall, "Filosofien, dens metoder og maal", *Samtiden* 19, 1908, p. 547.
25 Kristian Kroman, "Om Filosofiens Væsen og Betydning", *Vor Ungdom. Tidskrift for Opdragelse og Undervisning*, 1883, pp. 439-440.

tance of the results of exact experience and on the other hand the subjective character of speculative deductions and the impossibility of their verification.[26]

Now let me try to indicate some lines that can be drawn from here to the philosophers of the Vienna Circle. In comparison with the men of the positive spirit in Nordic philosophy the representatives of the Vienna Circle advocated a far more radical empiricism and also a far more radical critique of metaphysics. With the men of the positive spirit they shared, even radicalized the conviction that philosophy, just as the special sciences, is a cooperative undertaking, and that general comprehensibility is an essential characteristic of good philosophy. They also shared the conviction that philosophy ought to deal with clearly delimited and open questions, furthermore with problems that are in principle solveable, i.e. the view that philosophy too should be a research science able to make progress. There is also an even stronger emphasis on the unity of science among the members of the Vienna Circle. New in comparison with the men of the positive spirit in Nordic philosophy is the focus within the Vienna Circle upon the logical analysis of language, of the concepts and propositions that are used in philosophy and in the special sciences. The general picture that emerges is that the positivism of the Vienna Circle can be regarded as a radicalization of the convictions held by the men of the positive spirit. What was it that gave the members of the Vienna Circle the confidence to be that radical? My answer is that in the first hand it was their confidence in the new method of logical analysis. In their programmatic statement *Wissenschaftliche Weltauffassung – der Wiener Kreis* from 1929 we read: "The method of logical analysis is what distinguishes the new empiricism and positivism from the earlier one, whose orientation was more biological-psychological."[27]

II

In this section I will take a closer look at some specific topics and figures in Nordic philosophy from the 1870s and onwards that are of relevance for the discussion of positivism before Logical positivism in Nordic philosophy.

1. My first topic is the reactions of two Nordic Hegelians to the challenge from the positive spirit: Marcus J. Monrad (1816–1897), professor in Kristiania, today Oslo, and Johan Jakob Borelius (1823–1909), professor in Lund. The two were close friends and together they formed a powerful Hegelian axis in Nordic philosophy that lasted well into the 1890s. Monrad and Borelius fought a combat against the positive spirit in philosophy for several decades. Here I can only pro-

26 Høffding, *Den engelske Philosophi*, op. cit., p. 189.

27 Quoted from Rainer Hegselmann (Ed.), Otto Neurath, *Wissenschaftliche Weltauffassung, Sozialismus und Logischer Empirismus*. Frankfurt/Main: Suhrkamp 1979, p. 87.

vide some glimpses of their continuous efforts to turn the tide.

In 1874 Monrad published a comprehensive critical overview over contemporary movements of thought. There he defines positivism as "any attempt to establish a standpoint outside reason and the idea, in something that is *posited* as real, and upon which all thought is said to be dependent".[28] As a consequence of this definition Monrad quite interestingly is able to identify two forms of positivism: an irreligious and a religious one, the latter taking its point of departure from the Scriptures and the historical tradition as something given. Thus for Monrad positivism, just like Hegelianism, has its right-wing and its left-wing. The religious positivism of the right begins with Schelling's late philosophy and culminates in orthodox theology; to this form of positivism belongs, according to Monrad, also Kierkegaard and Grundtvig. The irreligious positivism of the left begins with Feuerbach and runs via Comte to St. Mill. In his counter-attack on these two strands of positivism Monrad applies a classical Hegelian strategy: to interpret the opponent as a moment in the development of the idea, a moment whose radical truth-claim can be rejected, but which at the same time is justified as a moment in a more comprehensive whole. Towards the end of his book Monrad is in full control of the situation: "What we have called positivism or abstract realism is nothing but the moment of the idea forgetting itself."[29]

Two decades later, in 1896, at the age of 80, Monrad gave a lecture with the title "Blik ud i Philosophiens Fremtid" [A Look into the Future of Philosophy]. His optimistic prognosis read that thought is now about to free itself from its "babylonian captivity".[30] In Germany the interest in Kant will, according to Monrad, sooner or later give way to a renewed interest in Hegel, a development that is already visible in British philosophy. Monrad is therefore quite convinced that "the promised land of philosophy" lies not behind but ahead of us.[31]

Borelius travelled extensively in Germany, but also in France and Italy, in the summers of 1876 and 1877. During his travels he visited no less than fifteen of the German universities. Back home again Borelius summed up his impressions: for metaphysics there is a downward trend, for history of philosophy and psychology, to be more precise, empirical psychology "founded upon experiment and induction", there is a clear upward trend.[32] All in all there is, according to Borelius, "a strong tendency towards realism" in German philosophy, which goes hand in hand with a high appreciation of French and in particular British philosophy.[33]

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28 Marcus J. Monrad, *Tankeretninger i den nyere Tid. Et kritisk Rundskue*. Christiania: Aschehoug 1874, p. 138.

29 Ibid., p. 399.

30 Marcus J. Monrad, "Blik ud i Philosophiens Fremtid", *Forhandlinger i Videnskabs-Selskabet i Christiania aar 1896*, No. 7, 1896, pp. 5-6.

31 Ibid., p. 14.

32 Johan Jakob Borelius, "En blick på den nuvarande filosofien i Tyskland", *Nordisk Tidskrift* 2, 1879, p. 342.

33 Ibid., p. 310.

In a huge work entitled *Metafysik* [Metaphysics], which wasn't published as a book until a year after his death in 1909, but whose different parts were distributed among his students since the early 1880s, Borelius launched a counter-attack on the positive spirit. "I threw myself into studies of the natural sciences", he writes in a letter to his friend Monrad from 1890, "in order to be able to show with the help of them that empirical research can't dispense with teleology."[34] Borelius for example made an attempt to mobilize the so called second law of thermodynamics, the law of entropy, as a weapon against the mechanistic worldview. If we assume, Borelius argued, that the world has no beginning in time, all movement must already have come to a standstill, which however is contradicted by experience. This is an indirect argument for the world having a beginning in time and the existence of a hyper-physical first principle.[35] Thus Borelius' strategy is to take the results of modern natural science and turn them against his philosophical enemies, making them an indirect argument for his own "true monism" or "objective idealism".[36]

Both Borelius and Monrad died, so to speak, with their philosophical boots on. They had become Hegelians already in the 1840s, and fifty years later, as the 19th century was approaching its end, they were still fighting for the cause of Hegelianism.

2. Harald Høffding was the most internationally well-known and influential Nordic philosopher from the 1880s and well into the new century. He was, as we have seen, also an important advocate of the positive spirit in philosophy. Next I'm going to draw a kind of philosophical-political profile in outline of Høffding by putting him in the context of some of his closest philosophical friends in Europe. I will be focusing upon a certain *family resemblance* among them, with the underlying thesis that the step to the philosophical-political profile of the left-wing of the Vienna Circle is indeed not very big.

Among the closest philosophical friends of Høffding outside the Nordic countries were Friedrich Paulsen (1846–1908), Georg von Gizycki (1851–1895), Ferdinand Tönnies (1855–1936) and Friedrich Jodl (1849–1914). None of them had a very central position within German philosophy, in comparison with for example the three Wilhelm: Dilthey, Windelband and Wundt. Paulsen, professor in Berlin, who by the way knew Danish since his childhood in Schleswig, had a very tense relation to Dilthey. Philosophically he had a pronounced social-political orientation and took a strong interest in the so called social question. Gizycki, associate professor in Berlin, had the reputation of being a dangerous *Katheder-Sozialist* and an ateist. He was strongly influenced by British utilitarianism. Tönnies, who for a long time had no secure position at a university, was

34 Letter from Johan Jakob Borelius to Marcus J. Monrad (December 30th, 1890), Lunds universitetsbibliotek.

35 Cf. Johan Jakob Borelius, *Metafysik*. Lund: Gleerups 1910, p. 309.

36 Ibid., 341.

suspected of having sympathies for the Social Democratic Party and therefore seen as objectionable. Finally Jodl, professor in Prague and from 1896 in Vienna, worked in the tradition of the enlightenment, represented a pronounced anti-clericalism and had strong social-political interests.

Paulsen and Gizycki knew Høffding personally since the mid-1880s, and both were active in making him known in the German speaking philosophical world. Paulsen took the initiative to the translation of Høffding's book on psychology into German, and Gizycki reviewed some of his early books in a very positive vein. Tönnies and Høffding had a correspondence since the late 1880s.[37] They met personally in the mid-1890s and Tönnies visited Høffding in Copenhagen in 1899, and some years later he lent a helping hand by the translation of Høffdings philosophy of religion into German. Jodl had reviewed some of Høffdings writings very positively already in 1890, and Høffding visited him at his sommer house in Bohemia in 1896. After the visit Jodl wrote in a letter to his Finnish friend Wilhelm Bolin: "We understood each other just as well in conversation as literarily."[38]

The above-mentioned were not only good friends of Høffding, but there were also many similarities in their philosophical views. In a review from 1890 of a book by Høffding on ethics Jodl draws attention to the affinity between Høffding, Paulsen and Gizycki in matters of ethics, and talks about "an important concord".[39] This concord can be illuminated by what Jodl wrote in a review from the same year of a book by Gizycki on moral philosophy, namely that in this book is found "the best thoughts of the West-European positivism", or, which to him amounts to the same thing, "the best thoughts of the Enlightenment" in connection with "the more mature and dispassionate insight of the 19th century".[40]

All in all an intellectual constellation can be discerned characterized by an adherence to the ideals of the enlightenment and the positive spirit in philosophy, by a more or less radical anti-clerical stance, and by a political stance reaching from left-wing liberalism to sympathies for socialism. All of them were also active in the German Society for Ethical Culture – Gizycki and Jodl as key figures. Høffding contributed some articles to the journal of the Society, but he didn't follow Gizycki's urge to found a Danish section of the Society in Copenhagen.[41] There were doubtless differences in the views among the above-mentioned, but

37 Cf. Cornelius Bickel / Rolf Fechner (Eds.), *Briefwechsel. Ferdinand Tönnies; Harald Høffding*. Berlin: Duncker & Humblot 1989.

38 Georg Gimpl (Ed.), *Unter uns gesagt. Friedrich Jodls Briefe an Wilhelm Bolin*, mit einer Einführung von Juha Manninen und Georg Gimpl. Wien: Löcker 1991, p. 171.

39 Friedrich Jodl, "Harald Höffding, Ethik", *Deutsche Literaturzeitung* 11, No. 9, 1890, p. 299.

40 Friedrich Jodl, "Moralphilosophie gemeinverständlich dargestellt von Georg von Gizycki", *Philosophische Monatshefte* 26, 1890, pp. 216ff.

41 See letters from Georg von Gizycki to Harald Høffding (November 26th and December 4th, 1892), Det kongelige Bibliotek, København.

also what I would call a family resemblance. And there is in my view only a rather short step from the philosophical-political profile of this group to the profile of the left-wing of the Vienna Circle.

3. Next I want to draw attention to a no doubt minor Nordic philosopher, which I presume is almost completely unknown today even in Sweden. His name is Paul Cavallin (1868–1901). He was the son of a professor in Greek language at Lund university. He took his doctoral degree in philosophy in 1894, but died only a few years later at the age of 33. Cavallin is of interest because during his short philosophical career, which by the way wasn't much of a career, he strikes *a new tone* in Swedish philosophy, perhaps also in Nordic philosophy. He was to begin with a rather odd fellow in the philosophical circles in Lund because his sympathies and preferences were by the British philosophical tradition rather than by German philosophy (compare Westermarck in Finland). Furthermore, Cavallin was a spokesman for a radical nominalism and realism in philosophy. His *Doktorvater*, the Hegelian Borelius, who otherwise was known for his tolerance towards views that differed from his own, was very annoyed with Cavallin.

In his dissertation from 1894 – *Identiska och syntetiska satser* [Identical and Synthetical Propositions] – Cavallin tries to give a more precise formulation of Kant's distinction between analytical and synthetical judgements, and to appraise the value and significance of this distinction. He shows a strong interest in conceptual analysis and clarification, and among other things he draws attention to the fact that "one and the same grammatical sentence can be the expression of two different judgements … and two different grammatical sentences can be the expression of one and the same judgment".[42] This distinction bears some resemblance to Russell's distinction between the grammatical and the logical form of propositions in his famous theory of definite descriptions.

A few years later Cavallin published a book with the title *Determination och multiplikation. Logisk-matematiska undersökningar* [Determination and Multiplication. Logical-Mathematical Investigations] (1899). Here too Cavallin undertakes certain basic terminological and conceptual clarifications. His book also shows a reception of modern logicians such as Boole, Delboeuf, Jevons, Venn, Peirce, Schröder and several others.[43] And in an additional note towards the end of Cavallin's book there is a reference to an article by Frege from 1895.[44] Is this the first explicit sign of a reception of Frege by a Nordic philosopher? As far as I know, it is.[45]

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42 Paul Cavallin, *Identiska och syntetiska satser. En kritisk-antikritisk undersökning*. Lund: Gleerup 1894, pp. 45-46.

43 Cf. Paul Cavallin, *Determination och multiplikation. Logisk-matematiska undersökningar*. Lund: Gleerup 1899, p. 92.

44 Ibid., p. 182. The article by Frege is "Kritische Beleuchtung einiger Punkte in E. Schröders Vorlesungen über die Algebra der Logik", which was published in *Archiv für systematische Philosophie*.

45 Cavallin's reference to Frege was first noticed by Thorild Dahlquist in "Adolf Phaléns

All in all, Cavallin strikes a new tone in Swedish philosophy through his strong interest in conceptual analysis and clarification, through his nominalism and realism, through his attention to the eventual difference between the grammatical and the logical form of sentences, and especially through his interest in and reception of, what he himself calls, "the modern mathematical (algorithmic, symbolic) logic".[46] Several years later Herrlin wrote in a dictionary article on Cavallin that he "exhibits a wide orientation in the field which nowadays arouses a strong interest in the form of the 'logistic' that is represented by Bertrand Russell, Peano, Hilbert and others".[47] It is hardly justified to call Cavallin a *logical* positivist before Logical positivism, but in certain ways he represented something radically new in Nordic philosophy. It is of course impossible to say in what direction his investigations in philosophy would have taken him if had had a longer life, including a more successful academic career.

4. Finally, a few words about the so called Uppsala philosophy and its relation to the Vienna Circle. However, I will not focus on the explicit discussion by Einar Tegen (1884–1965) in his article "Kritisk objektivism" [Critical Objectivism] from 1936, published in the Swedish philosophical journal *Theoria*, but instead on the formative phase of the Uppsala philosophy, i.e. the decade from 1905 to 1915.[48] The founding fathers and also the two most well known representatives of the Uppsala philosophy are Axel Hägerström (1868–1939) and Adolf Phalén (1884–1931). To the first generation also belonged the today less known Karl Hedvall (1873–1918).

The Uppsala philosophy combined a militant critique of metaphysics with a just as militant critique of empiricism. An example of the latter is Hedvall's dissertation on Hume from 1906: *Humes Erkenntnistheorie kritisch dargestellt*, with the subtitle *Eine Untersuchung über empiristische Prinzipien*. In fact, the target of critique is not only Hume, but also the different forms of modern empiricism. Hedvall cites with approval the Hegelian Adolf Lasson: "To reject Hume is to reject also the most modern."[49] As representatives of modern empiricism Hedvall mentions among others Wundt, Avenarius, Mach, Spencer and Høffding, all of them men of the positive spirit in philosophy. Hedvall's main critique of empiricism goes as follows: "Given the sensualist standpoint there can be no idea of objective validity."[50] Empiricism is according to Hedvall a form of *subjectivism*,

efterlämnade skrifter", *Lychnos. Årsbok för idé- och lärdomshistoria*, 1990, p. 270.

46 Cavallin, *Determination och multiplikation*, op. cit., p. 92.

47 Axel Herrlin, "Cavallin, Paul", *Svenskt biografiskt lexikon* 7, 1927, p. 711.

48 Einar Tegen, "Kritisk objektivism. En grundståndpunkt och en kritik", *Theoria* 2, 1936, pp. 27-55. For a more detailed exposition of the genesis of the Uppsala philosophy, see my article "Den nya Uppsala-filosofin och brytningen med transcendentalismen", *Lychnos. Årsbok för idé- och lärdomshistoria*, 2002, pp. 71-102.

49 Karl Hedvall, *Humes Erkenntnistheorie kritisch dargestellt. Eine Untersuchung über empiristische Prinzipien*. Uppsala: Uppsala universitets årsskrift 1906, p. 74.

50 Ibid., p. 57.

saying that all we can immediately know are our own representations. However, our knowledge has certain a priori presuppositions which only philosophical analysis can lay bare. This thesis is developed in Hägerström's programmatic text *Das Prinzip der Wissenschaft* from 1908. According to Hägerström must for example the general concept of reality be presupposed in all empirical knowledge, and what that concept entails can only be clarified by a philosophical conceptual analysis.[51] This and other presuppositions of the empirical sciences must be clarified through purely logical-epistemological investigations.

The general drift of the early Uppsala philosophy was towards conceiving philosophy in terms of conceptual analysis and clarification of the problems of philosophy. An essential part of doing philosophy was in their view to show that at least some philosophical problems are nothing but pseudo-problems. Showing them to be pseudo-problems mean to dissolve them as philosophical problems. This line of thought can be exemplified through Phalén's dissertation on the problem of knowledge in Hegel's philosophy from 1912. What Phalén calls the problem of knowledge is there formulated as follows: How is knowledge of an object that is independent of the subject possible? Formulated in this way the problem rests on certain assumptions, the most important one being the assumption that the only thing that we can immediately know is our own subjective representations. This is according to Phalén a mistaken assumption. And:

> If certain assumptions are mistaken, the solution to the problem is to show that this is the case, which makes the problem disappear. If what is mistaken in the presuppositions is not noticed the problem remains unsolved, and the line of argument which is meant to be a solution to the problem … must be mistaken.[52]

This is according to Phalén the case with for example Hegel's solution to the problem of knowledge. Furthermore, towards the end of his dissertation Phalén suggests that the assumptions on which the problem of knowledge rest "in some way have their roots deep down in the common way of representing things", and the problem of knowledge can therefore be seen "as a consequence of some deeply rooted prejudices" in our ordinary way of thinking.[53]

To sum up, through their critical stance toward metaphysics, their understanding of philosophy in terms of conceptual analysis and clarification, and their way of conceiving certain problems of philosophy as pseudo-problems which are to be dissolved through philosophical analysis, the early Uppsala philosophers can be said to point forward towards the Vienna Circle. However, through their

51 Cf. Axel Hägerström, *Das Prinzip der Wissenschaft. Eine logisch-erkenntnistheoretische Untersuchung. I. Die Realität*. Uppsala: Skrifter utg. af Hum. Vetenskapssamf. i Uppsala 1908.

52 Adolf Phalén, *Das Erkenntnisproblem in Hegels Philosophie. Die Erkenntniskritik als Metaphysik*. Uppsala: Akademiska bokhandeln 1910, p. 242.

53 Ibid., p. 448.

radical rejection of empiricism, their Yes to experience and firm No to empiricism, the Uppsala philosophers marched under a different banner than the men of the Vienna Circle. Furthermore, there are, as far as I can see, no signs of a reception of modern logic among the early Uppsala philosophers.

Department of Sociology
Lund University
Box 117, Paradisgatan 5,
221 00 Lund
Denmark
Carl-Goran.Heidegren@soc.lu.se

Marja Jalava

The Earliest Extensive Receptions of Mach in the North[1]

Introduction

The 1880s marked a fundamental change in Finnish academic philosophy as well as in Finnish intellectual life as a whole, for German Idealism, which had dominated the scholarly community, had to give way to a critical-empirical approach. The younger generation took the natural sciences as a model for the humanities, insisting on quantitative methods, repeated experiments and statistically proven laws. At the same time, they took part in a more general ideological discussion, raised by Darwinism, about the possibility of explaining everything in nature and in the human being scientifically. As the Finnish botanist Fredrik Elfving (1854–1942) stated in his polemical article in 1884, he and his contemporaries were witnessing an intellectual upheaval, comparable to the breakthrough of the Copernican system. This would finally put an end to all fallacies originating in the naïve childhood of humankind.[2]

In this program aimed at the elimination of all superfluous and metaphysical assumptions, the Austrian physicist Ernst Mach (1838–1916) was used as an authority.[3] In Finland, Mach's ideas were favorably received in 1889 at the latest by Hjalmar Neiglick (1860–1889), the Finnish pioneer of experimental psychology and psychophysical research, who had made his doctoral dissertation in 1887 in Leipzig at Wilhelm Wundt's first psychological laboratory in the world. In Finnish academic philosophy, Neiglick was the leading advocate of "*eine Psychologie ohne Seele*."[4] In this profoundly anti-metaphysical approach, he used Mach's *Beiträge zur Analyse der Empfindungen* (1886) to defend his thesis that all abstract concepts as well as concrete ideas and recollections had to have a certain sensual correlate (in most cases, an audio feature, spoken words, or an optical picture, writing) to occur and to be reproduced.[5] His early death from typhoid in 1889,

1 In writing this article, the author has benefited from the comments of Prof. Juha Manninen and Prof. Ilkka Niiniluoto who kindly read the manuscript.

2 Fredrik Elfving, "Olika verldsåskådningar", in: *Finsk Tidskrift* II, 1884, pp. 161-162, 177-180.

3 See also Mach's preface to the fourth edition of *The Analysis of Sensations*, London: Routledge/Thoemmes Press 1914/1996, p. xl.

4 See e.g. Hjalmar Neiglick, "Om exakta metoder i psykologin", in: *Finsk Tidskrift* II, 1887, pp. 269-281, 339-349.

5 Hjalmar Neiglick, "Svar", in: *Finsk Tidskrift* II, 1889, pp. 238-240.

however, tragically broke off his promising career, and his reference to Mach was left without further explication.

In Finland, extensive treatment of the topic began only at the beginning of the 20th century. These receptions of Mach were also among the earliest receptions in the Nordic countries as a whole. Firstly, in this article, I explore the socialist Otto Ville Kuusinen's Machian ideas, presented in 1905 and 1916. Secondly, I analyze the ways in which the moral philosopher Rolf Lagerborg utilized Mach's phenomenalism in his theory of knowledge, especially in his main study on the subject in 1920. Thirdly, I deal briefly with the earliest sceptical reception of Mach, presented in the 1910s by Eino Kaila, a future leading figure in Finnish academic philosophy. Finally, my discussion raises for analysis various ways in which these interpretations were intertwined with the more general political and religious views of those in question, for in this early stage, the positive reception of Mach was clearly connected with leftist sympathies and anti-clericalism, already cherished by many of "the 1880s generation." In this respect, the Finnish scholars differed from their Nordic colleagues, such as the Swedish philosopher Vitalis Norström (1856–1916), who, despite his occasional positive references to Mach, developed his "philosophy of practical reason" predominantly in connection with Kant and Fichte, without taking Mach as the basis for his discussion.[6]

Otto Ville Kuusinen's "Farewell to the Angels of Metaphysics"[7]

Arguably the earliest extensive reception of Mach in the North was presented in 1905 by the young student of aesthetics and philosophy Otto Ville Kuusinen (1881–1964) in his Master's thesis *Ernst Haeckelin monismi ja puhtaan kokemuksen kanta* ["Ernst Haeckel's monism and the position of pure experience"].[8] This final project at the Imperial Alexander University of Finland (the present University of Helsinki) is of special interest because the author soon became the ideological leader of the Finnish Social Democratic Party and, after the Finnish Civil War and his flight into Soviet-Russia in 1918, a prominent leader of the Comintern (Communist International) as well as a member of the Soviet Union's Politburo (the Central Committee of the Communist Party), the Presidium of the Supreme Soviet of the USSR and the Soviet Academy of Sciences. In 1961, he was even nominated the Hero of Socialist Labor. It has been claimed that at the summit of his power, this son of a humble village tailor was the most influental Finn ever in world politics.

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6 See Vitalis Norström, *Tankelinier*. Stockholm: Hiertas Bokförlag 1905, pp. 558-561.

7 Kuusinen's own expression, see Otto Ville Kuusinen, "Ernst Mach III–IV", in: *Työmies* no. 73, 15.3.1916.

8 In his Master's thesis, Kuusinen referred extensively to Mach's *Die Analyse der Empfindungen* (1886) and *Populär-wissenschaftliche Vorlesungen* (1896).

It is impossible to find out with certainly how and why Kuusinen, in the first place, became interested in Ernst Mach, because the best potential sources, the records of the Philosophical Society of Finland 1904–1905, have disappeared. This society, in part, acted as a seminar for students preparing a Ph.D. or Master's thesis in philosophy, so it is presumable that Kuusinen, too, discussed his subject there. In his only presentation recorded in the minutes, "Socialism and individual freedom" in March 1906, he already set forth fully socialist views on capitalist inequality contrasted with the real freedom achieved after socialist revolution, without any specific philosophical references.[9] It has been hypothesized, however, that Kuusinen's supervisor might have been the Finnish social anthropologist and philosopher Edvard Westermarck, who in the spring semester of 1903 acted as a substitute for the professor of philosophy at the Imperial Alexander University, and, in the following year, discussed publicly the mind-body problem, a central question in Kuusinen's Master's thesis.[10] This assumption is strengthened by the fact that in 1912, Westermarck criticized material monism in matching tones with Kuusinen, stating that materialism was only a hypothesis, similar to spiritualism.[11]

Another possibility for Kuusinen to have become acquainted with Mach was his aesthetic studies. In December 1903 in a meeting of the University Students' Aesthetic Society, he gave a lecture on the Austrian author Peter Altenberg, one of his favorites,[12] who, in turn, based his impressionist view on art and literature on Mach's phenomenalism, on the idea of the world in endless flow without a substance.[13] Unfortunately, the records of the University Students' Aesthetic Society are very insufficient concerning this meeting, and it is not clear whether Kuusinen made reference to Mach.

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9 Records of the Philosophical Society of Finland, March 16, 1906, published in: Juha Manninen/Ilkka Niiniluoto (Eds.), *Ajatuksen laboratorio: Filosofisen yhdistyksen pöytäkirjat 1873–1925*. Helsinki: Suomen Filosofinen Yhdistys 1996, pp. 360-362; see also the extended version of the speech, Otto Ville Kuusinen, "Sosialismi ja yksilönvapaus", in: *Sosialistinen Aikakauslehti*, specimen number 2, 1906, pp. 11-14 and *Sosialistinen Aikakauslehti*, specimen number 3, 1906, pp. 39-44.

10 Juha Manninen, "Suomen ensimmäinen machilainen", in: I. A. Kieseppä/Sami Pihlström/Panu Raatikainen (Eds.), *Tieto, totuus ja todellisuus*. Helsinki: Gaudeamus 1996, pp. 296-307.

11 Records of the Prometheus Society 1905–1914, December 5, 1912, signum E: 3, Åbo Akademi University Library, Turku, Finland.

12 Records of the University Students' Aesthetic Society 1900–1903, December 2, 1903, signum E. I. 47, The National Library of Finland, Helsinki; see also Thomas Henrikson, *Romantik och Marxism. Estetik och politik hos Otto Ville Kuusinen och Diktonius*. Helsingfors: Söderström & Co. 1971, pp. 88-89, 97, 172; Päivi Huuhtanen, *Tunteesta henkeen. Antipositivismi ja suomalainen estetiikka 1900–1939*. Helsinki: SKS 1978, p. 120.

13 Friedrich Stadler, *Vom Positivismus zur „wissenschaftlichen Weltauffassung". Am Beispiel der Wirkungsgeschichte von Ernst Mach in Österreich von 1895 bis 1934*. Wien–München: Löcker Verlag 1982, pp. 55-56.

Since Kuusinen was interested in socialism ever since the epoch-making 1903 Party Congress of the Finnish Social Democratic Party in Forssa, where the Marxist program of principles was approved,[14] one might suggest yet another motive for his interest in Mach. The young academic socialists, such as Kuusinen, wanted to be scientific in their world-view, in their social theory and in their conception of history, which included hostility to religion and to metaphysics. One of the most popular authorities in this respect was the German biologist Ernst Haeckel (1834–1919), famous for his materialistic monism, which postulated that all aspects of life were ultimately reducible to physical-chemical processes. There existed, nevertheless, one major problem in Haeckel's thinking; namely, that he strongly rejected socialism, considering its claim for social equality contradictory to the Darwinian idea of the "survival of the fittest." To disprove Haeckel's interpretation of Darwin, Kuusinen and other young socialists referred, among others, to the Italian criminologist Enrico Ferri's *Socialism and Positive Science* (1894), published in Finnish in 1905.[15] While Kuusinen in his Master's thesis harshly criticized Haeckel's monistic materialism and took a positive stand for Mach, it is obvious that he also wanted to use Mach's philosophical methods as a new basis for "scientific" socialist theory.[16]

Kuusinen's study was based on the claim that the position of pure experience, which Haeckel insisted on representing, was actually something quite different than what Haeckel's materialism stood for.[17] For Kuusinen, Haeckel's idea of organic life as a mere series of physical-chemical processes was a one-sided view, containing materialistic metaphysics far beyond positive facts.[18] Correspondingly, Kuusinen contested Haeckel's equation of the functioning of living organisms with the operation of mechanical automatons. According to Kuusinen, developed living organisms manifested in their functions an astonishing diversity of activities, and thus also had a far better ability for self-preservation than unanimated force systems.[19] As Mach with cause remarked, this inner activity – for instance, various phenomena of human memory – was not to be confused with age-old vitalism, let alone the existence of a soul. Nevertheless, contrary to Haeckel's efforts, neither

14 See e.g. Vesa Salminen, "Estetiikan opiskelijasta poliitikoksi vuosina 1900–1906", in: Vesa Salminen (Ed.), *Nuori Otto Ville Kuusinen 1881–1920.* Jyväskylä: Gummerus 1970, pp. 27-53.

15 Otto Ville Kuusinen, "Enrico Ferri: Sosialismi ja uuden ajan tiede", in: *Sosialistinen Aikakauslehti*, specimen number 2, 1906, pp. 21-22; Pseudonym H. R. [Hannes Ryömä?], "Sosialismi ja uuden ajan tiede", in: *Sosialistinen Aikakauslehti* no. 8, 1906, pp. 181-188; for the interpretation of Haeckel by the Finnish socialists, see Hannu Soikkanen, *Sosialismin tulo Suomeen*. Helsinki: WSOY 1961, pp. 92-93, 138-139.

16 See also Otto Ville Kuusinen, "Ernst Mach I–II", in: *Työmies* no. 72, 14.3.1916.

17 See the unnumbered preface, Otto Ville Kuusinen, *Ernst Haeckelin monismi ja puhtaan kokemuksen kanta*. Unpublished Master's thesis, signum B2 Konseptit, The People's Archives, Helsinki 1905.

18 *Ibid.*, p. 4.

19 *Ibid.*, pp. 19-24.

could one explain them by mere causal mechanical principles and the laws of nature. Referring to Kant and the Neo-Kantian interpretation of Hermann Cohen, Kuusinen insisted that in the case of living organisms, scientific study could only discover certain regularities. He also rejected Haeckel's idea of the development of a living organism as a mere passive adaptation to the changes in its external circumstances. Anticipating the gradual rehabilitation of Kant's conception of teleology in modern biology, he suggested that the concepts of goal-directedness and purposefulness were essential to the very definition of living beings.[20]

In the second part of his Master's thesis, Kuusinen hammered Haeckel's psychological ideas, linked to the more general mind-body problem. According to Kuusinen, psychology was for Haeckel only a subsection of biology, and a mental phenomenon, respectively, without any specific quality of its own. Haeckel, thus, assumed that atoms had sensations and plants had mental life, which for Kuusinen was as meaningless and metaphysical as Haeckel's conceptions of "psycho-plasma" and "cell-soul."[21] An even more serious shortcoming in Haeckel's explanation was his neglect of the biological significance of mental phenomena: how various development stages of consciousness contributed to the self-preservation of a living organism. Being preoccupied with naïve materialism, Haeckel solved the mind-body problem in a fully causal, mechanical way.[22]

Following the Machian ideas of Joseph Petzoldt and Rudolf Willy, Kuusinen claimed that there was no rift between the psychical and physical, and thus, no need for a postulate of psychophysical parallelism.[23] A living creature was an indivisible whole, and although only a living, embodied individual could think and feel, the interaction between mind and body was merely functional, not causal.[24] The ability to sense and think was developing simultaneously with physical development, especially with the development of the nervous system, and it was impossible to define exactly when a response to an external influence turned into a mental ability involving sensations and emotions.[25] Concerning the mind-body problem, Kuusinen introduced once again the Neo-Kantian opposition of general rules and rigid laws, arguing that a scholar studying human activities (for example, a historian) could never set forth any tenable laws. On the basis of this theoretical discussion, he also drew social conclusions. Instead of considering social life "the war of all against all" and "the struggle for survival," the highest and refined abilities of the human mind should be seen on the grounds of biology, and cherished as crucial prerequisites for the integrated development of mind and body, which,

20 *Ibid.*, pp. 25-51; cf. Mach, *The Analysis of Sensations*, *op. cit.*, pp. 80-81.
21 Kuusinen, *Ernst Haeckelin monismi ja puhtaan kokemuksen kanta*, *op. cit.*, pp. 58-63.
22 *Ibid.*, pp. 68-82.
23 *Ibid.*, pp. 86, 92; See also Mach, *The Analysis of Sensations*, *op. cit.*, p. 310.
24 Kuusinen, *Ernst Haeckelin monismi ja puhtaan kokemuksen kanta*, *op. cit.*, pp. 85-93.
25 *Ibid.*, pp. 63-64.

from the evolutionary viewpoint, was ultimately at the service of self-preservation.[26]

In the third part of his Master's thesis, Kuusinen took epistemological questions into consideration, noting that they certainly were not the strongest part of Haeckel's argumentation. For example, it was unclear what Haeckel's conception of 'soul' ultimately meant.[27] Following Mach, Kuusinen contested the common notion of an antithesis between "appearance" and "reality" as well as the need to find those mysterious entities of which the world was supposed to consist. "The things *are* as they *appear*," he crystallized Mach's basic idea.[28] Instead of atoms and molecules, our perceptions, presentations and emotions, in short the whole inner and outer world, were put together out of a number of elements, such as colors, sounds, smells and pressures, which were commonly called 'sensations'. Strictly taken, there did not exist a distinction between the "inner" (mental) and "outer" (material) world; the question was only about two different viewpoints.[29]

Generally taken, the only significant point on which Kuusinen disagreed with Mach was the question about the 'self' or 'ego', briefly discussed at the end of his Master's thesis. According to Mach, the primary fact was not the ego, but the elements; in Mach's words, "Bodies do not produce sensations, but complexes of elements (sensations) make up bodies."[30] Hence, Mach took 'body' and 'ego' simply as practical unities and makeshifts, put together for purposes of provisional survey and for definite practical ends, whereas Kuusinen attached greater importance to the experienced distinction between an individual and his/her surroundings. As Kuusinen pointed out with reference to Rudolf Willy and Wilhelm Wundt, an experience that was not the experience of a determinate subject was unthinkable, which demonstrated that the subject had to have some consistence.[31]

However, in 1916 in his obituary for Mach, Kuusinen praised the Machian conception of ego, saying that the need for 'self' and 'soul' reminded us of the old peasant who saw a steam engine and asked, "Where are the horses that drive the machine?"[32] Since Mach himself made this same comparison in his *Erkenntnis und Irrtum*, published in the same year as Kuusinen's Master's thesis (1905),[33] Kuusinen might have taken it directly from Mach. In part, this testifies to his

26 *Ibid.*, pp. 95-107.
27 *Ibid.*, pp. 109, 111.
28 "Sellaisina kuin kappaleet meille *ilmenevät*, sitä ne *ovat*", *Ibid.*, p. 126, author's italics; see also Mach, *The Analysis of Sensations*, *op. cit.*, pp. 10-11, 29.
29 Kuusinen, *Ernst Haeckelin monismi ja puhtaan kokemuksen kanta*, *op. cit.*, pp. 126-130; Ernst Mach, *Knowledge and Error*. Dordrecht–Holland/ Boston–USA: D. Reidel Publishing Company 1905/1976, pp. 6-8.
30 Mach, *The Analysis of Sensations*, *op. cit.*, p. 29.
31 Kuusinen, *Ernst Haeckelin monismi ja puhtaan kokemuksen kanta*, *op. cit.*, pp. 130-135; cf. Mach, *The Analysis of Sensations*, *op. cit.*, pp. 23-29.
32 Kuusinen, "Ernst Mach I–II", *op. cit*.
33 Mach, *Knowledge and Error*, *op. cit.*, p. 8.

long-standing interest in Mach.[34] Moreover, the Machian conception of ego suited Kuusinen's ethical ideal, which precluded the disregard of other egos and the overestimation of one's own. Both men condemned the ideal of an overweening Nietzschean "superman," considered by some of their contemporaries the opposite of the socialist "herd mentality."[35] At this later stage, Kuusinen's criticism of Mach was concentrated merely on the way Mach limited the aspects of his discussion to the autonomous field of science, without taking into account how scientific advancement depended on economic development and, on this basis, on historical circumstances, conflicts and struggles.[36]

On a more general level, Kuusinen's long-standing interest in Mach testified to the strong West European orientation of early Finnish working-class ideology. Although the general strike in 1905 brought the Finnish and Russian working-class movements closer to each other, Kuusinen was still in 1916 oriented towards Germany. His most important ideological exemplar was Karl Kautsky, whom he considered "the most reliable guide to Marx's doctrine."[37] In the Social Democratic parliamentary group (Kuusinen was elected a Member of the Finnish Parliament in 1908), he leaned on Kautsky to such an extent that his fellow representatives gave him the nickname "little Kautsky."[38]

It is a well-known fact that V. I. Lenin had already insisted in 1908 in his *Materialism and Empirio-Criticism* that the Bolsheviks had to choose Mach or Marx. According to Lenin, the supposedly "new" "phenomenalist" view of Mach and his followers repeated the old absurdity of philosophical subjective idealism. As Lenin put it, "The philosophy of the scientist Mach is to science what the kiss of the Christian Judas was to Christ."[39] The ideological connection between

34 For Kuusinen's continuous interest in Mach, see also Henrikson, *op. cit.*, p. 172; Manninen, *op. cit.*, p. 306; Sulo Vuolijoki, *Asianajoa ja politiikkaa*. Helsinki: Oy Suomen kirja 1945, p. 148.

35 Kuusinen, "Ernst Mach III–IV" *op. cit.*; Mach, *The Analysis of Sensations*, *op. cit.*, p. 25; for the political uses of Nietzschean arguments at the beginning of the 20th century, see e.g. Marja Jalava, "Lidelse och bragd. Nietzschereceptionen i Finland ca 1890–1910", in: Stefan Nygård/Johan Strang (Eds.), *Mellan idealism och analytisk filosofi. Den moderna filosofin i Finland och Sverige 1880–1950.* Helsingfors/ Stockholm: Svenska litteratursällskapet i Finland/Bokförlaget Atlantis 2006, pp. 51-78.

36 Kuusinen, "Ernst Mach I–II" *op. cit*.

37 Otto Ville Kuusinen, "Sananen sivistyskysymyksestä ja meidän sosialistisesta kirjallisuudestamme", *Sosialistinen Aikakauslehti* no. 22-24, 1906, pp. 521-525. There is no evidence of a connection between Kuusinen and the Machian Austro-Marxists, such as Friedrich Adler. For the Austro-Marxist reception of Mach, see Stadler, *op. cit.,*p. 85ff.

38 Soikkanen, *op. cit.,* pp. 333-334; Hannu Tapiola, "Kuusisen kuva Suomen työväenliikkeen aatevirtausten kehyksissä", in: Vesa Salminen (Ed.), *Nuori Otto Ville Kuusinen, op. cit.,* pp. 145-172.

39 Vladimir Lenin, *Materialism and Empirio-Criticism. Critical Comments on a Reactionary Philosophy*. Moscow: Progress Publishers 1908/1972, Chapters 1.6 and 6.5;

Kuusinen and the Russian revolutionary movement was, however, established as late as 1917 when Kuusinen met the exiled Lenin in Helsinki. Later on, Kuusinen claimed to become acquainted with Lenin's literary production for the first time in 1918 after his flight into Soviet-Russia. Only then could he find out Lenin's harsh judgement of Mach, dominating Soviet philosophy ever since.[40] When Tuure Lehén, the Finnish emigrant communist and the rector of the Petrozavodsk State University in the Soviet Union, in 1950 called Eino Kaila "the Machian idealist," it was certainly not a compliment.[41]

Rolf Lagerborg in the fight against "scientific misconceptions"

While O. V. Kuusinen preferred political action to academic research after the completion of a Master's degree, the moral philosopher Rolf Lagerborg (1874–1959) was the first (and arguably only) Machist in Finnish academic philosophy. At the beginning of the 20th century, Lagerborg was a well-known and controversial character in Finnish intellectual life. Not only was he a pioneer in many fields of academic study, he was, for example, responsible for the earliest positive receptions of Friedrich Nietzsche, Emile Durkheim and John B. Watson in Finland, but he also wrote a few works of fiction and countless polemic articles which repeatedly were subjects of heated public debates. Lagerborg's conception of the world was firmly rooted in the liberal, anti-clerical and anti-metaphysical ideas of the radical French Revolution and "the 1880s generation." He and his tutor and life-long friend Edvard Westermarck were the driving forces in the Prometheus Society, a Finnish association for freethinking and freedom of religion in 1905–1914. In Lagerborg's case, it was impossible to distinguish scholarly ideas from extra-scientific interests, since for him, a philosopher was essentially a public figure taking part in topical issues, not a specialist in his/her ivory tower.[42]

During his long and turbulent academic career, Lagerborg had, roughly speaking, two leading ideas: firstly, he claimed that moral conceptions were social phe-

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for the reception of Mach by the Marxists in Russia/Soviet Union, see Robert S. Cohen, "Ernst Mach: Physics, perception and the philosophy of science", in: Robert S. Cohen/Raymond J. Seeger (Eds.), *Ernst Mach. Physicist and Philosopher*. Boston Studies in the Philosophy of Science, volume VI. Dordrecht–Holland: D. Reidel Publishing Company 1970, pp. 126-164.

40 Tapiola, *op. cit.,* pp. 148, 168-169.

41 Cited in Ilkka Niiniluoto, "Syvähenkisen empiristin hahmottuva maailma: Eino Kaila 1936–58", in: Ilkka Niiniluoto (Ed.), *Eino Kaila: Valitut teokset 2, 1939–58*. Helsinki: Otava 1992, pp. 7-31.

42 See e.g. Rolf Lagerborg, "Th. Rein om Nietzsche. En gensaga", in: *Finsk Tidskrift* I, 1901, pp. 511-520; Records of the Philosophical Society of Finland, November 28, 1919, *op. cit.*, p. 465.

nomena (for him, moral philosophy was a branch of sociology),[43] and secondly, that mental phenomena – for instance, emotions – had a physiological basis ("the reaction theory").[44] The latter theme was the center of his attention since 1905 when he decided to apply for the post of adjunct professor (*Privatdozent*) of psychology at the Imperial Alexander University of Finland, and, after various setbacks in Finnish academic life, even considered going into medicine.[45] Since he watchfully followed the international discussion about topical issues in his field of study, always looking for new evidence to support his own standpoint, it was actually only a matter of time before he found Ernst Mach, at that time famous for his contribution to the debate on atomic theory. Because of the social, political and cultural differences between Lagerborg and O. V. Kuusinen, (Lagerborg descended from a Swedish-speaking aristocratic family and supported French reformist socialism), it is plausible that their interests in Mach were in no way connected to each other.[46] However, instead of speculation on Lagerborg's possible influences, it is more important to pay attention to the pre-existing harmony of his epistemological views with those of Mach – for instance, his criticism of the concepts of 'ego' and 'soul' ever since his early interest in Nietzsche.[47]

It seems that Lagerborg had become acquainted with Mach's ideas in 1908 at the latest, for in his public lecture at the Prometheus Society in October 1908, titled "Själsproblemet inför fysiken" ["The problem of the soul in the eye of physics"], he already stated in a Machian way that the conception of 'soul' in psychology, similarly to the conception of 'mass' in physics, was only a common noun for certain phenomena, not the "thing-in-itself" with substantial properties of its own.[48] In a debate following his lecture at the Philosophical Society of Finland, he

43 See e.g. Rolf Lagerborg, "Etik – ett skolans undervisningsämne I–II?", in: *Euterpe* no. 31, 1903, pp. 389-394 and *Euterpe* 33, 1903, pp. 411-414.

44 See e.g. Rolf Lagerborg, *Das Gefühlsproblem. Studien zur peripherischen Mechanismus des Bewusstseinslebens.* Leipzig: Barth 1905.

45 Marja Jalava, *Minä ja maailmanhenki. Moderni subjekti kristillis-idealistisessa kansallisajattelussa ja Rolf Lagerborgin kulttuuriradikalismissa n. 1800–1914*. Bibliotheca Historica 98. Helsinki: SKS 2005, pp. 335-336, 338-340, 351-353.

46 Kuusinen propagated his Machian ideas in the Finnish literature circles; for example, he introduced Mach and Avenarius to the poets Elmer Diktonius and Eino Leino (Henrikson, *op. cit.*, p. 172; Vuolijoki, *op. cit.*, pp. 147-148). Although Lagerborg was not on friendly terms with Kuusinen (Rolf Lagerborg, *I egna ögön – och andras*. Helsingfors: Söderström & Co 1942, pp. 334-335), he was a friend of Leino, and thus, there might have been an indirect linkage between them.

47 Jalava, *Minä ja maailmanhenki*, *op. cit.*, pp. 322-323; for the epistemological similarities between Mach and Nietzsche, see Philipp Frank, "Mach's philosophy of science", in: Robert S. Cohen/Raymond J. Seeger (Eds.), *Ernst Mach. Physicist and Philosopher, op. cit.,* pp. 219-234.

48 Records of the Prometheus Society, October 19, 1908, *op. cit.*; for a summary of the lecture, see *Hufvudstatsbladet* 20.10.1908, "Själsproblemet inför fysiken" and *Nya Pressen* 20.10.1908, "Själsproblemet inför fysiken".

denied the charge of being a materialist, and gave his support to "philosophers of immanence," without mentioning anyone by name, however.[49]

Arguably Lagerborg made his first direct reference to Mach in 1915 during a debate on Haeckel's monistic philosophy. The debate started in 1914 with an article by Harry Federley, the Finnish geneticist and national expert in eugenics. In his article, based on his lecture at the Prometheus Society in December 1912,[50] Federley praised the achievements of Haeckel and the Monist League, an organization designed to perpetuate Haeckel's freethinking in 1905. According to Federley, the natural sciences had proved beyond question that all life was based on matter and, consequently, mental life on its material substrate, the brain. Together with the rejection of the dogma of an immortal soul, material monism rejected Christianity and the conception of God as well as other superstitious beliefs, hence insisting on an unconfessional school system, the disestablishment of the Church and complete freedom of religion. With a scientific certainty of belief, Federley proclaimed, "Science will finally command everything and totally displace religion."[51]

As was presumable, the polemic words of Federley raised a number of protests, among them a detailed analysis of Haeckel's philosophical errors, presented by the engineer Torsten Nybergh. Apart from critical arguments rather similar to Kuusinen's Master's thesis (although without a connection to Kuusinen), Nybergh equated the Prometheus Society with the Monist League, defining its radical program as "a positive and slack vulgar philosophy."[52] In this respect, he agreed with Eino Kaila, who had already in 1911 called Haeckel's monism "a lousy popular philosophy,"[53] and during the debate started by Federley in 1914, repeated his critical judgement on "the conceptual chaos of Haeckel."[54]

While Westermarck, himself critical of Haeckel's monism,[55] protested only against the equation of the Prometheus Society and the Monist League,[56] Lagerborg, instead, had an urge to defend Haeckel. Despite its awkward conceptions, he considered Haeckel's monism useful for the pursuit of a scientific world-view. As a whole, he compared it to a pleasant building with some fake ornaments and decayed parts, which, nevertheless, had a solid foundation and hence was eas-

49 Records of the Philosophical Society of Finland, November 27, 1908, *op. cit.*, pp. 371-372.

50 Records of the Prometheus Society, December 5, 1912, *op. cit.*.

51 Harry Federley, "Den monistiska världsåskådningen", in: *Finsk Tidskrift* II, 1914, pp. 27-46.

52 Torsten Nybergh, "Den haeckelska monismen I–II." in: *Nya Argus* no. 4, 1915, pp. 28-31 and no. 5, 1915, pp. 40-44, the quotation in part II, p. 44.

53 Eino Kaila, "Ensimmäinen kansainvälinen monistikongressi", in: *Aika*, 1911, pp. 668-672.

54 Eino Kaila, "Replik till dr Lagerborg", in: *Nya Argus* no. 7, 1915, p. 66.

55 Records of the Prometheus Society, December 5, 1912, *op. cit*.

56 Edvard Westermarck, "Fråga till herr Torsten Nybergh", in: *Nya Argus* no. 6, 1915, p. 55.

ily renovated with "a little critical cleaning." As the leader of this clean-up, he introduced Ernst Mach, whose phenomenalism seemed to offer a way to avoid Haeckel's "materialistic metaphysics." In an essentially Machian way, Lagerborg suggested that both consciousness and matter were phenomenal, which meant that the supposed distinction between them fell apart and turned out to be a result of our own narrow outlook.[57] In the following discussion, however, Lagerborg and Nybergh mainly missed each other's points, finally ending up trading personal insults.[58]

Lagerborg's reference to Mach was not left without a further explication, for in his main study on the theory of knowledge, *Vetenskapliga vanföreställningar* ['Scientific misconceptions'] in 1920, he utilized Mach's phenomenalism in an extensive way. Lagerborg's impulse to write this book came from Robert Tigerstedt, the internationally renowned Finnish physiologist specialized in the vascular system. As the professor of physiology at the University of Helsinki, Tigerstedt had given a lecture series in 1918–1919 about general philosophical and methodological problems in the natural sciences, published in 1919 under the title *Vetenskaplig kritik: Några allmänna betraktelser* [´Scientific criticism: Some general reflections'] and translated into Finnish by Eino Kaila in 1920.[59]

In his book, Tigerstedt stated that the sole purpose of scientific research was to reach the truth.[60] The main task was to advance from scientifically proven facts to general conceptions and laws, as comprehensive as possible.[61] As a cautious person, Tigerstedt warned repeatedly of the risk of considering scientific results too definitive and/or presenting them in an oversimplified form, using Ernst Haeckel as an example.[62] Same cautiousness was necessary for a scientific world-view. Referring to Du Bois-Reymond's famous *ignoramus–ignorabimus* thesis,[63]

57 Rolf Lagerborg, "Till frågan om den haeckelska monismen", in: *Nya Argus* no. 6, 1915, pp. 55-56; see also Ilkka Niiniluoto, "Tigerstedt, Kaila ja Lagerborg: Tieteenfilosofian varhaiskylvöä Suomessa", in: Simo Knuuttila/Juha Manninen/Ilkka Niiniluoto (Eds.), *Aate ja maailmankuva. Suomen filosofista perintöä keskiajalta vuosisadallemme*. Helsinki: WSOY 1979, pp. 370-409.

58 Rolf Lagerborg, "Monismen. Svar till herr Torsten Nybergh", in: *Nya Argus* no. 10, 1915, pp. 93-94; Torsten Nybergh, "Monismen, Några rättelser", in: *Nya Argus* no. 9, 1915, pp. 87-88. Nybergh claimed that Lagerborg solved the problems of his "romantic monism" by giving, as usual, only some effective quotations, which made it difficult to discuss with him on grounds of facts; Torsten Nybergh, "Monismen än en gång", in: *Nya Argus* no. 11, 1915, pp. 108-109.

59 Robert Tigerstedt, *Vetenskaplig kritik. Några allmänna betraktelser*. Helsingfors: Söderström & Co 1919; in Finnish, *Tieteellistä kritiikkiä*, Helsinki: Otava 1920.

60 *Ibid.*, p. 40.

61 *Ibid.*, pp. 48, 70.

62 *Ibid.*, pp. 73-75, 102-103.

63 The German physiologist Emil DuBois-Reymond stated in 1872 that there existed many mysteries in the material world that we did not yet know (*ignoramus*), but the mysteries of what matter and force (*Kraft*) are and how can matter and force 'think'

Tigerstedt stated that there were many questions, for instance questions about free will and the immortality of the soul, that were beyond the limits of scientific research. The only scientifically acceptable position was hence agnosticism, the philosophical recognition that at the present state of our knowledge, we could not obtain absolute certainty about the ultimate reality.[64] As Tigerstedt summed up his leading idea, "The truth discloses itself only to those who doubt."[65]

To quote from the anthropologist Gunnar Landtman's review of Lagerborg's book, Lagerborg did not consider Tigerstedt primarily his opponent but rather a person whose position he wanted to surpass.[66] For Lagerborg, as he noted straight off at the beginning of his response,[67] the crucial problem of Tigerstedt's position was that it was not doubtful enough: it left too much room for metaphysical conceptions, such as 'matter,' 'force,' 'will' and 'soul', and was unwilling to break away from an anthropocentric world-view or theology, with the exception of miracles. Bearing in mind that Tigerstedt was an authority in his domain, there was a danger that his statements were beneficial to theologians.[68] In fact, this had already been the case in the 1914–1915 debate on Haeckel's monism, in which Lagerborg's "arch-enemy," the professor of doctrinal theology G. G. Rosenqvist[69], had referred to Tigerstedt's former agnostic statements in his defense of the existence of God.[70] The same "danger" came true in 1921. In *Teologisk Tidskrift*, the Finnish theological periodical founded by Rosenqvist in 1896, Tigerstedt's *Vetenskaplig kritik* was warmly recommended to all students of theology as a work that defined the legitimate limits of natural scientific research.[71]

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we would never be able to know (*ignorabimus*); cited e.g. in *Ibid.*, p. 136 n.84. Mach, instead, was convinced that there was no problem which a man of science could regard as absolutely insoluble; Mach, *The Analysis of Sensations*, *op. cit.*, p. 358.

64 Tigerstedt*, op. cit.,* pp. 89-93, 100-101.

65 *Ibid.*, p. 107.

66 Gunnar Landtman, "Rf Lg kontra "vetenskapliga vanföreställningar", in: *Nya Argus* no. 24, 1920, pp. 196-197. Tigerstedt had guided Lagerborg greatly in the early stages of his physiological studies and had encouraged him to continue his work, see Lagerborg, *Das Gefühlsproblem, op. cit.,* p. 4; Lagerborg, *I egna ögön – och andras, op. cit.,* p. 321.

67 Lagerborg's response to Tigerstedt was first published as a series of articles in *Finsk Tidskrift* during the year 1920 ("Till Rob. Tigerstedt's Vetenskapslära. Några tillägg I–III", in: *Finsk Tidskrift* I, 1920 and "Naturuppfattningens nydaning I–II", in: *Finsk Tidskrift* II, 1920). The contents of these articles are identical to *Vetenskapliga vanföreställningar* (1920).

68 Rolf Lagerborg, *Vetenskapliga vanföreställningar.* Helsingfors: Söderström & Co 1920, p. 8.

69 Lagerborg's struggle with Rosenqvist had started at the end of the 1890s, and continued for decades; see e.g. Lagerborg, *I egna ögön – och andras, op. cit.,* p. 30.

70 Georg Gustaf Rosenqvist, "Naturvetenskap och världsåskådning", in: *Finsk Tidskrift* II, 1914, pp. 241-251.

71 Rafael Gyllenberg, "Litteratur: Robert Tigerstedt, Vetenskaplig kritik", in: *Teologisk Tidskrift – Teologinen Aikakauskirja*, 1921, pp. 24-27.

With theoretical armature mainly collected from Mach's *Die Analyse der Empfindungen*,[72] Lagerborg planned his *Vetenskapliga vanföreställningar* as a counterblow against theology and metaphysics, those two "undeveloped stages of knowledge," as Auguste Comte defined them in his 'law of three phases' of society.[73] A good starting point for the critical study was Mach's conception of 'matter', which provided a striking contrast to Tigerstedt's self-evident way of explaining the world with material entities (*materie*) and their motions. As Mach had stated, 'matter' had to be regarded merely as a highly natural, unconsciously constructed mental symbol for a relatively stable complex of sensational elements.[74] In Lagerborg's words, instead of a bearing substrate, 'matter' was only a series of events, similar to fire.[75] Hence there was no point in contrasting 'appearance' with 'reality'; a 'thing-in-itself', detached from the world of our sensations and experiences, was just an empty word.[76] Furthermore, Lagerborg challenged the conception of causality, claiming that 'cause' and 'effect' were simply our own views of a continuous, unbroken process.[77] Epistemologically taken, Lagerborg's Machian standpoint clearly challenged Tigerstedt's realistic theory of knowledge, which assumed (although in a cautious form) that our sensations corresponded to external reality, which made it possible for us to acquire reliable knowledge of the world.[78]

In relation to the metaphysical nature of 'matter', Lagerborg also briefly discussed atomic theory. While Mach was influential in many parts of Europe during the first decade of the 20th century, the Finnish physicists (those very few individuals who were capable of assessing the international research at the forefront of development) seemed to support atomic theory by common consent and leaned toward accepting the reality of atoms.[79] As Hjalmar Tallqvist, professor of physics at

72 Mach was by no means Lagerborg's only authority in epistemological questions. In *Vetenskapliga vanföreställningar* he eclectically referred e.g. to Heraclitus, Aristotle, Kant and Hume as well as to more recent philosophers, such as Avenarius, Riehl and Poincaré.

73 Cited in Lagerborg, *Vetenskapliga vanföreställningar, op. cit.,* p. 20.

74 *Ibd.*, p. 27; Mach, *The Analysis of Sensations, op. cit.,* p. 311.

75 Lagerborg, *Vetenskapliga vanföreställningar, op. cit.,* p. 29.

76 *Ibid.*, pp. 36-39; see also Mach, *The Analysis of Sensations, op. cit.,* pp. 10-12.

77 Lagerborg, *Vetenskapliga vanföreställningar, op. cit.,* pp. 15-17.

78 Tigerstedt*, op. cit.,* pp. 18-19; Niiniluoto, "Tigerstedt, Kaila ja Lagerborg: Tieteenfilosofian varhaiskylvöä Suomessa", *op. cit.,* p. 389.

79 Because of the chronic lack of resources, Finnish physical research was mainly focused on topics with local practical importance. During the first decade of the 20th century, only three Finns had direct contact with the leading international centers of physics, and only one of them, Gunnar Nordström, related his research to the most burning issues of the day, being among the first persons in the world who supported Einstein's theory of relativity; Tapio Markkanen, "Fysikaaliset tieteet", in: Päiviö Tommila (Ed.), *Suomen tieteen historia 3. Luonnontieteet, lääketieteet ja tekniset tieteet*. Helsinki: WSOY 2000, pp. 82-153.

the University of Helsinki, put it in 1913, since Einstein's theory of relativity had revolutionized the conceptions of time, space and mass, which in physics meant a shift from "a system at rest" to "a system in motion," one could find solace in the fact that "matter is and will be tangible reality."[80] In Lagerborg's ears, Tallqvist's half-humorous statement sounded suspiciously like an attempt to defend the existence of the 'thing-in-itself'. From his phenomenalist viewpoint, the discovery of subatomic particles gave proof of the divisibility of atoms, which was yet another verification of the claim that the 'atom' was merely an auxiliary concept, similar to 'matter' and 'force'. Thus, the model of the atom was useful only as an orienteering scheme, not as the ultimate truth of reality. "It will never mirror the whole diversity and multiplicity of nature," he said.[81] For some reason or other, Tallqvist did not say a word in reply to Lagerborg's view on atomic theory. For example, in 1922 while giving a brief report to the reading public in *Nya Argus* on the recent development of the relativity theory, the quantum theory and the new atomic theory, he did not even discuss the possibility that these theories could have been contested, let alone mention Lagerborg's or Mach's name.[82]

While Kuusinen suggested in his Master's thesis certain reservations about the Machian conception of 'ego', Lagerborg was eager to follow in Mach's footsteps to the very end in this respect, too. For Lagerborg, the crucial philosophical consequence of the relativity theory was that all hopes of achieving something absolute had crumbled to dust.[83] Just as there was no 'thing-in-itself' behind appearance, there did not exist any permanent "self-subject" (*jaget-subjektet*) behind our consciousness, and even our self-consciousness was consciously changing. This was manifested in a pointed way by psychiatric patients with a double personality, but the same held true for "normal" people as well. As the well-known British physiologist Michael Foster had aptly remarked, during a long life one's personality could alter so drastically that if all those personalities were introduced to each other, they would despise one another and separate as soon as possible without a desire to ever meet again.[84]

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80 Hjalmar Tallqvist, "Nutida åsikter om elektricitetens natur", in: *Finsk Tidskrift* II, 1913, pp. 331-346; Hjalmar Tallqvist, "Om relativitetsteorin I–II", in: *Nya Argus* no. 10, 1915, pp. 89-91 and *Nya Argus* no. 11, 1915, pp. 100-103.

81 Lagerborg, *Vetenskapliga vanföreställningar, op. cit.,* pp. 60-69; see also Records of the Philosophical Society of Finland, February 27, 1920, *op. cit.*, pp. 467-470.

82 Hjalmar Tallqvist, "Den moderna atomteorien", in: *Nya Argus* no. 10, 1922, pp. 130-133.

83 Lagerborg, *Vetenskapliga vanföreställningar, op. cit.,* pp. 71-72. In this respect, Lagerborg differed from Mach, who rejected Einstein's theory of relativity because of its "dogmatic assertion that atoms existed"; see Ryoichi Itagaki, "Three batches of reasons for Mach's rejection of Einstein's theory of relativity", in: John Blackmore (Ed.), *Ernst Mach – A Deeper Look. Documents and New Perspectives*. Boston Studies in the Philosophy of Science vol. 143. Dordrecht–Boston–London: Kluwer Academic Publishers 1992, pp. 277-295.

84 Lagerborg, *Vetenskapliga vanföreställningar, op. cit.,* pp. 72-78, quotation in p. 78.

If the conception of the ego as a definite, unalterable, sharply-bounded unity was given up, at the same time, we could get rid of a number of pseudo-problems. A good example for Lagerborg was Descartes' famous thesis *cogito, ergo sum*. From the Machian viewpoint, it went too far to say *cogito*, if translated by *I think*, for we know only the existence of our sensations and thoughts, not the existence of *I* as an agent. To quote from Mach, "We should say, *It thinks*, just as we say, *It lightens*."[85] In all, according to Lagerborg, the ego was just a spot where feebly connected lines of events temporarily crossed; "A subject is only a temporary projection point."[86] Since it was possible to consider this conception "an attack on the majesty of man," it was no wonder that it was opposed in the same passionate way as people in the past had opposed the idea that the earth rotated on its axis and around the sun. However, be it pleasant or not, science could make progress only with scholars who were not afraid to lose the steady ground under their feet. At the end of his book, Lagerborg finally agreed with Tigerstedt, "The truth discloses itself only to those who doubt."[87]

Although Lagerborg, faithfully to his personal style, defended the Machian ideas in a poignant manner,[88] his interest in Mach was relatively short-lived and should be seen mainly as one stage in his life-long battle against metaphysics and its "idols of spurious thought," such as 'soul', 'ego' and 'substance'. In 1922, he was already enthusiastic about psycho-serology, the attempt to explain changes in mental states by bodily fluids and hormones.[89] Five years later, in 1927, he introduced John B. Watson's behaviorism to the Finnish public, claiming that in many fields of study, for instance in sociology, the old terminology of "Geisteswissenschaften" should have been replaced by the concepts of 'stimulus' and 'response'.[90] While reading Lagerborg's visions of social engineering based on the determinist assumption that people responded in a programmed way to outside stimuli,[91] it may be difficult to believe that the same writer had just a few years earlier insisted on the elimination of 'causality' from the scientific vocabulary.[92] After the early 1920s, Lagerborg merely referred to Mach's theory of economy (*Die Ökonomie des Denkens*), stating that science should aim at the most simple

85 Lagerborg, *Vetenskapliga vanföreställningar, op. cit.,* pp. 83, 91; Mach, *The Analysis of Sensations, op. cit.,* p. 29, author's italics; see also Records of the Philosophical Society of Finland, November 5, 1920, *op. cit.*, pp. 476-478.

86 Lagerborg, *Vetenskapliga vanföreställningar, op. cit.,* pp. 93-95.

87 Lagerborg, *Vetenskapliga vanföreställningar, op. cit.,* pp. 95-96; cf. Tigerstedt, *op. cit.,* p. 107.

88 See e.g. Records of the Philosophical Society of Finland, February 27 and November 5, 1920, *op. cit.*, pp. 467-470, 476-478.

89 Records of the Philosophical Society of Finland, November 17, 1922, *op. cit.*, pp. 500-501.

90 Rolf Lagerborg, "Behaviorismi. Eräänlainen sielutieteellinen bolshevismi", in: *Ajatus. Filosofisen yhdistyksen vuosikirja II*, 1927, pp. 81-97.

91 E.g. *Ibid.*, pp. 94-95.

92 Lagerborg, *Vetenskapliga vanföreställningar, op. cit.,* pp. 18-19.

and 'economic' descriptions of perceived phenomena and their relations.[93] Later on, he did not show any interest in the Vienna Circle and Logical Empirism or Logical Positivism.

Eino Kaila's critical position on Mach

While the Finnish physicists seemed to pass phenomenalism over in silence, Eino Kaila (1890–1958), a future leading figure in Finnish academic philosophy and the professor of philosophy at the University of Helsinki (1930–48), presented the earliest critical reception of Mach in the 1910s. The international background of Kaila's early intellectual development was marked by a controversy between "atomists" (e.g. Max Planck), "phenomenalists" (Mach) and "energeticists" (Wilhelm Oswald), to which we still have to add the "trend philosopher" of the era, Henri Bergson with his *élan vital* ('vital force') as an explanation of life. While Kaila was at the same time modifying his scientific way of thinking, he had to take a stand on this topical issue.

It has been suggested that Kaila's constitutive experience of the world might be described as "monistic" or "unitaristic"; to quote Kaila's poetic expression, "We are passing ripples in the sea of all unity."[94] This, however, did not make him sympathetic towards Haeckel's materialistic monism. As has been already noted, he considered it "a lousy popular philosophy," and a banal "compilation of philistinism and superficial natural sciences," fully ignorant of the deficiency of our present knowledge.[95] Referring to Bergson, he stated that materialism was based on *homo faber*'s psychological inclination to cherish solid objects and to materialize things. Instead of materialistic monism, in this early stage he supported a non-dualistic version of psychophysical parallelism, stating that in every point, mental processes corresponded to certain processes at the cortex. Although mental phenomena depended on certain corporal preconditions, he did not accept the reduction of psychology to biology; "The metaphysical gulf between mental and material will exist just as before."[96] In the 1914–1915 debate on Haeckel,

93 See e.g. Lagerborg, "Behaviorismi", *op. cit.,* p. 93; Rolf Lagerborg, *Reaktionspykologi. En tillrättaläggning*. Stockholm: Bokförlaget Natur och Kultur 1943, p. 57.

94 Eino Kaila, "Filosofian klassillinen käsitys aineellisen ja sielullisen suhteesta (1958)", in: Simo Knuuttila/ Juha Manninen/Ilkka Niiniluoto (Eds.), *Aate ja maailmankuva, op. cit.,* pp. 436-456; Ilkka Niiniluoto, "Eino Kaila och Wienkretsen", in: Stefan Nygård/ Johan Strang (Eds.), *Mellan idealism och analytisk filosofi, op. cit.*, pp. 169-196.

95 Kaila, "Ensimmäinen kansainvälinen monistikongressi", *op. cit.*, p. 672; Eino Kaila, "Poroporvari ja kamarifilosofi", in: *Aika*, 1911, pp. 538-542; Eino Kaila, "Nykyinen materialismi", in: *Uusi Suometar* 3.10.1913.

96 Kaila, "Nykyinen materialismi", *op. cit.*

he insisted that every philosopher should acknowledge the empirical opposite of physical and psychical.[97]

In general, the young Kaila was not fully negative towards Mach's phenomenalism, although he considered it "exaggerated." In 1911 he agreed with Mach that for the exact sciences, the most crucial issue was how various phenomena related to each other. Since the ideas about the quality of phenomena were created solely for the purposes of controlling their mutual relations, Mach's theory of economy (*Die Ökonomie des Denkens*) should have been taken as a working hypothesis, not as "the latest result of science."[98] In some respect, the position on Mach kept Kaila occupied to his death in 1958.[99] He also transplanted this interest into some of his students, such as Oiva Ketonen (1913–2000), the professor of philosophy at the University of Helsinki in 1951–1977, who presented his interpretation of the Machian theory of economy in 1965.[100]

For Kaila, however, Mach's denial of the modern atomic theory was an unsurmountable error, for in 1913, he was already firmly convinced of the existence of atoms.[101] In 1919 in a meeting of the Philosophical Society, he stated that science had proved beyond question that atoms and molecules were "as real as various other things, which we cannot observe at the moment, for some reason or other, but which we have to consider existing."[102] In 1920, Kaila was ready to claim that there had to be a fundamental mistake in the seemingly logical argumentation on which phenomenalist physics were based. Because of our psychophysical mechanisms based on biology, we comprehended the world nowhere near in accordance with our initial sensations, for in our consciousness there existed mechanisms that strongly modified them. As Kaila put the question, "How could our sensations as such be sacred to a natural scientist, to a physicist, when they are not sacred to our consciousness, which treats them according to higher purposes?"[103] In a heated debate with Lagerborg in a meeting of the Philosophical Society in 1921, Kaila

97 Kaila, "Replik till dr Lagerborg", *op. cit.,* p. 66.

98 Kaila, "Poroporvari ja kamarifilosofi", *op. cit.,* p. 541.

99 See Niiniluoto, "Tigerstedt, Kaila ja Lagerborg: Tieteenfilosofian varhaiskylvöä Suomessa", *op. cit.,* p. 401.

100 Oiva Ketonen, "Die Ökonomie der Wissenschaft", in: *Acta Philosophica Fennica Fasc. XVIII*. Studia logico-mathematica et philosophica in honorem Rolf Nevanlinna die natali eius septuagesino 22. X. 1965. Helsinki: Societas Philosophica Fennica 1965, pp. 63-69.

101 Kaila, "Nykyinen materialismi", *op. cit.*.

102 Records of the Philosophical Society of Finland, November 14, 1919, *op. cit.*, p. 463.

103 Eino Kaila, "Filosofisia huomautuksia relativiteettiteoriaan", in: *Aika*, 1920, pp. 269-285. In the same article, he also criticized Einstein's theory of relativity, claiming that it was based on the same phenomenalist error as Machian physics; see *Ibid.,*pp. 276-285. Two years later, however, he had changed his view and accepted the theory of relativity, see Records of the Philosophical Society of Finland, February 17, 1922, *op. cit.*, pp. 490-492.

unhesitatingly remarked that whoever denied atomic theory, in a certain sense denied thinking at the same time.[104]

Final remarks

One might consider it an example of historical irony that while Mach primarily desired an understanding of the natural scientists, repeating, "There is no such thing as 'the philosophy of Mach'," [105] at least in Finland at the beginning of the 20th century, his position was solely discussed by philosophers and students of philosophy, interested in the theory of knowledge. The only Finnish physicist active in public discussion, the professor of physics Hjalmar Tallqvist, did not seem to pay any attention to phenomenalist physics.[106] Even in philosophy, Mach's long-term influence was rather marginal. The most enduring part of his thinking seemed to be the theory of economy (*Die Ökonomie des Denkens*), afterwards positively appraised by both Lagerborg and Kaila.[107]

On a more general level, the earliest debate on Mach can be considered an epistemological contest between Machian phenomenalism (Lagerborg) and critical scientific realism (Kaila), ending up with the clear victory of the latter.[108] However, as the Machian neo-positivist Richard von Mises remarked in 1938, it is impossible to estimate the true effect of Mach's doctrines by the number and the success of those who were directly inspired by his writings and/or tried to continue his work in a similar direction.[109] For instance, when Lagerborg explained his phenomenalist theory of knowledge at a meeting of the Philosophical Society of Finland in February 1920, even those who did not agree with him admitted

104 Records of the Philosophical Society of Finland, April 15, 1921, *op. cit.*, p. 485.

105 Mach, *The Analysis of Sensations, op. cit.,* pp. 368-369; Mach, *Knowledge and Error, op. cit.,* pp. 3, 9. To a certain extent, Mach's statement should be taken as a provocation, for he did not deny the philosophical implications of his and other scientists' work. However, he considered the scientist's primary task to be "removing false problems that hinder scientific enquiry […] whether his ideas fit into some given philosophical system or not," Mach, *Knowledge and Error, op. cit.,* p. 9; see also Stadler, *op. cit.,* p. 37.

106 In this respect, Finland resembled the Netherlands; see Henk Visser, "Mach, Utrecht, and Dutch philosophy", in: John Blackmore (Ed.), *op. cit.,* pp. 403-430.

107 Eino Kaila, "Inhimillinen tieto: Mitä se on ja mitä se ei ole (1939)", in: Ilkka Niiniluoto (Ed.), *Eino Kaila: Valitut teokset 2, 1939–58, op. cit.*, pp. 49-228; Lagerborg, "Behaviorismi", *op. cit.,* p. 93.

108 Niiniluoto, "Tigerstedt, Kaila ja Lagerborg: Tieteenfilosofian varhaiskylvöä Suomessa", *op. cit.,* pp. 399-400.

109 Richard von Mises, "Mach and the empirist conception of science (1939)", in: Robert S. Cohen/ Raymond J. Seeger (Eds.), *op. cit.,* pp. 245-270.

that the shaking of the foundations of dogmatic belief had a "purifying impact on modern thought."[110]

Finally, it is important to note that the effect of Mach's doctrines was not limited to a small circle of academic philosophers. As the case of Otto Ville Kuusinen manifested, the ideas of Mach shaped the thinking of young academic socialists and cultural bohemia, intertwined with more general political and anti-clerical views, and, at least to a certain extent, we may hence consider Mach influential on the Finnish intellectual avant-garde of the early 20th century. Seen in this context, Machist anti-metaphysics was a revolutionary standard within philosophical culture, clearly connected with freethinking and leftist sympathies, cherished both by Kuusinen and Lagerborg, and mistrusted by Kaila, who, despite his radical philosophical thinking, was politically conservative. Just as socialism was a political battle against social oppression and inequality, so Machist anti-metaphysics was a battle against dogmatic and idol-producing ways of thought. As the closing words of *Erkenntnis und Irrtum* testified, this socio-political connection had certainly crossed Mach's mind, too. It was not a secret to his contemporaries that he was a socialist and an atheist.[111] In his native land, Austria, this legacy was carried on by the *Verein Ernst Mach* as well as by some leading figures of the Vienna Circle, such as Rudolf Carnap, Hans Hahn and Otto Neurath, who were anti-metaphysical socialists.

University of Helsinki
Department of History
P.O. Box 4 (Yliopistokatu 3)
SF-00014 Helsinki
Finland
marja.jalava@helsinki.fi

110 Records of the Philosophical Society of Finland, February 27, 1920, *op. cit.*, pp. 469-470; see also Landtman, *op. cit.,* pp. 196-197.

111 Mach, *Knowledge and Error, op. cit.,* p. 361; Cohen, *op. cit.*, p. 158.

Ilkka Niiniluoto

Kaila's Critique of Vitalism[1]

1. Kaila's Inner Tension

In the gloomy year of 1943, when Finland was fighting against the Soviet Union in the turmoil of World War II, Finnish philosopher Eino Kaila published a highly personal book *Syvähenkinen elämä* ("The Depths of Spiritual Life" or "Deep-Mental Life"), with the subtitle *Keskusteluja perimmäisistä kysymyksistä* ("Discussions on ultimate questions"). An extended version in Swedish, *Tankens oro* ("The Disquietude of Thought" or "Restless Thought") appeared one year later.[2]

Kaila's *Syvähenkinen elämä* mixes discussions on the meaning of life with considerations on philosophical topics that occupied its author as a proponent of logical empiricism. The main part of the book is written in the form of dialogues between two characters: Aristofilos and Eubulos. The painter Aristofilos eloquently presents a colourful pantheist vision of the world: reality is a process where Life with a capital L presses against the surface of space-matter and flows through its holes; human minds constituted by this process are the Eyes of Life through which Life looks around at the surrounding space-matter. Eubulos, who represents the scientific world conception, points out that the ideas of Aristofilos resemble the vitalism of Bergson and Driesch (Kaila, 1986, p. 146). Eubulos asserts that the real content of the doctrine of Life with a capital L is nothing but an expression of a personality transformed by religious conversion (*ibid*., p. 184). As a metaphysical explanation, it appeals to superficial pseudo-concepts (*ibid*., p. 204), and at the same time goes beyond experience in an unacceptable manner (*ibid*., p. 186).

Kaila made the interesting remark that our inner monologues are usually dialogues and debates between our partial egos (*ibid*., p. 195). He thus recognized that in his own personality there was a tension between two modes of thinking – artistic and scientific. In the Preface to the second edition of *Syvähenkinen elämä* in 1954, Kaila admitted that the anxiety of Aristofilos is "close" to himself, but the critical dialogue is a revealing document of the fact that Eubulos or the scientific world view was stronger in Kaila's inner personality.

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1 An earlier version of this paper has been published in Finnish as "Eino Kaila vitalismin kumoajana", in Jussi Haukioja ja Juha Räikkä (eds.), *Elämän merkitys*, UNIpress, 2005, pp. 291-302.

2 See the extended Finnish edition Kaila (1986). For Kaila's bibliography, see Kaila (1979), Manninen and Niiniluoto (2007).

During the fifty years of his philosophical career, Eino Kaila was restlessly trying to solve the riddle of reality.[3] He kept searching for a "monistic" or "unitary" world view, without cleavages between the material and mental, nonliving and living, qualitative and quantitative (see von Wright, 1992). Rejection of vitalism had been a permanent aspect of Kaila's vision ever since his youth. Yet Kaila was unwilling to accept mechanistic reductionism. But is it possible to find a satisfactory version of non-reductive monism? Kaila attempted to solve this tension by formulating a synthetic philosophical interpretation of the best results of contemporary scientific research on matter, life, and mind.

2. Vitalism

Morton Beckner (1967) defines *naive vitalism* as the folk doctrine that the life of an individual organism is a material substance which animates its body. This special kind of active matter can be a fluid (like blood) or a gas (like breath). According to the Old Testament, "man became a living being" when "the Lord God formed man of dust from the ground, and breathed into his nostrils the breath of life" (*Genesis* 2). In Finnish, the words 'henki' (spirit), ''hengittää' (to breathe), 'olla hengissä' (to be alive), and 'heittää henkensä' (to die) are interrelated. Animism is the primitive form of vitalism which regards all reality to be spiritual.

More sophisticated forms of vitalism have been proposed by philosophers. Aristotle's metaphysics identifies the Life of an individual living being with its *psyche* or *entelecheia*. For Aristotle, this psyche is the "form" of the organism, in contrast to its "matter". It is also potentiality which manifests itself in the actual activities of plants and animals – "vegetative soul" in nutrition and reproduction, "animal soul" in sensation, and "human soul" in rational thinking. In early modern philosophy, Descartes challenged the Aristotelean teleological views by claiming that, while human beings have a non-material soul, animals are merely mechanistic machines. Vitalism had still wide acceptance among natural philosophers who proposed explanations of life based on new kinds of substantial entities and powers like "archeaus" (van Helmont), "nisus formativis" (Blumenbach), "vis essentialis" (Wolff), and "Lebenskraft" (Müller). In the romantic *Naturphilosophie* in Germany, vitalism was associated with idealistic and panpsychist tendencies. Among the 19th century pioneers of organic chemistry, Friedrich Wöhler and Justus von Liebig still supported vitalism, but its important opponents included Hermann von Helmholz, Claude Bernard, Emil Du Bois-Reymond, and Hermann Lotze.

According to Beckner (1967), "critical vitalism" typically assumes that each individual organism has its own entelechy, its Life, which is a particular substance. This substantial entity is not – at least not totally – composed of nonliving sub-

3 For evaluations of Kaila's career and philosophical works, see von Wright (1979) and Niiniluoto et al. (1992).

stance, and it gives the living organism the typical powers that are lacking in all nonliving things. Some vitalists think that this vital entity cannot exist independently of the material body - this is, for example, Aristotle's view of the psyche. But some dualists think that the vital entity is nonmaterial and capable of existence apart from its organism.

The neovitalism of Hans Driesch, elaborated in his *Philosophie des Organischen* (1909), fits well this description of *substance vitalism*. Driesch was a pioneer of experimental embryology. He defended the "autonomy of life" by assuming that the processes of life are controlled and guided by a substantial, nonmaterial and nonspatial entity or "entelechy". The main grounds for his position came from experiments with showed that biological systems have surprising powers of regeneration. Take a fertilized egg of a frog or a sea urchin, separate the two blastomeres after the first division, and you will see that both parts are able to develop into perfect organisms. Driesch concluded that eggs and embryos are not mechanical machines, but rather goal-directed organic unities or "harmonious equipotential systems".

Naive versions of substance vitalism resemble the medieval theory of elements which took fire to be a kind of matter. The 17th century phlogiston theory corrected this view by recognizing that fire is a process of combustion, but in the 18th century Antoine Lavoisier showed how combustion can be explained physico-chemically as the process of combining burning matter and oxygen. The assumption of the theory that heat is constituted by the "caloric" substance was rejected by the kinetic theory of gas which claimed that heat is molecular motion. In the same way, it is natural to take organic life as a process rather than as a substance. The progress of the 20th century biology (cellular metabolism and photo synthesis, DNA and RNA molecules) has repeatedly shown that the processes of life are carried forward by physico-chemical things and processes. The fate of the entelechies of substance vitalism resembles the once fashionable theoretical entities of modern science: the Newtonian absolute time and space, ether in electromagnetic theory, and caloric substance in the theory of heat disappeared when better theories were put forward.

However, neovitalism has also been formulated as a process theory. Henri Bergson's *L'Évolution créatrice* (1911) picturesquely described life as an ascending "vital wave" (*élan vital*) which struggles to overcome the downward drift of descending heavy matter. According to Bergson, matter is "inertia, geometry, necessity", while "life is freedom, which penetrates itself to necessity by moulding it to its own benefit" (see Bergson, 1907). In order to be victorious in this struggle, life is transported by a forceful "surge".

Bergson's metaphysics is thus a dualist process ontology. Reality is governed by continuous motion and qualitative duration. Our analytic understanding cannot adequately grasp this reality, which can be approached only by intuition. For Bergson life is not primarily individual, but instead a general cosmic power. This view is expressed also by Kaila's Aristofilos: individual egos are secondary results of

the process of Life, and "Life in all living things is essentially one and the same" (Kaila 1986, p. 148). Hence, the *process vitalism* of Bergson and Aristofilos is not the substance vitalism described by Beckner where each living being has its own entelechy. In the history of metaphysics, the doctrine of process vitalism resembles more objective idealism and absolute spirit (cf. Klaus and Buhr, 1972) than subjective or pluralist idealism.

Let us conclude this section with a terminological remark. Gereon Wolters (1996) distinguishes between *ontological vitalism* (living things have other substances and forces than nonliving ones), *epistemological vitalism* (the structures and functions of living things cannot be explained by physico-chemical natural science), and *methodological vitalism* (the methods of non-organic natural science cannot be used to the study of life). We have discussed above ontological vitalism, which is a largely rejected position today. However, the rejection of ontological vitalism may be compatible with epistemological and methodological vitalism in Wolters' sense. Therefore, I prefer to avoid the terms "epistemological vitalism" and "methodological vitalism", and instead to speak about "biological anti-reductionism" and "the autonomy of biology", respectively. As we shall see, these issues were important also in Kaila's critique of vitalism.

3. Kaila's Early Work

Eino Kaila (1890–1958) started his career as a philosopher and psychologist in 1911 with inspired essays on Henri Bergson and William James. From Bergson he adopted the view that the stable objectification of reality is an illusion created by intellectualism. The picture of reality as "the open sea of dynamic change" was in harmony with the Jamesian doctrine of the stream of consciousness. In the same year the young Kaila announced that Ernst Haeckel's scientific monism, based upon biological evolutionism, is a superficial and "bourgeois" world view.

However, already in 1916 Kaila described Bergson as "a full-blooded follower of ancient mystics" whose vitalist doctrine on the flow of life is "pure speculation". According to Kaila, Bergson attempts to "deprive science of its authority on the most significant questions, and to replace it with a poetic intuition without any control". Here we can hear the voice of Kaila's scientific personality. In the next year, in his work on *Ernst Renan*, Kaila characterized idealist philosophy as a generalization of the vitalist-teleological view to the entire process of the world.

Already in 1913 Kaila defended, as a part of scientific psychology, "psychophysical parallelism", which takes "the series of mental phenomena to be in all points correlated with a series of material events in the brain". This view is often regarded as a form of mind-body dualism, but among its supporters Spinoza was

charged with materialism.[4] Kaila did not treat this view as a metaphysical position, but rather as a challenge to explain mental phenomena by their material basis: the material series is chosen as the ground of explanation, since it is continuous, while the latter is "repeatedly broken". This programme of associationist psychology was defended by Kaila in his 1916 doctoral dissertation on the causal explanation of decision making.

In his extensive book *Sieluelämän rakenne* ("The structure of mental life", 1923), which showed influences of the Gestalt theory, Kaila followed G. E. Müller in his definition of psycho-physical parallelism: each mental event depends on a correlated brain event; experienced similarity and difference between mental phenomena are correlated with similarity and difference of brain events; and changes in the mental level correspond to psycho-physical changes. These conditions resemble the contemporary physicalist views on the supervenience of the mental on the physical (see Kim, 1996).

4. Mental as Biological

In 1920 Eino Kaila, who was then 30 years old, published a monograph *Sielunelämä biologisena ilmiönä* ("Mental life as a biological phenomenon"). Its aim was to settle once and for all the relations to vitalism. The work reflects his exceptionally wide reading of the relevant scientific literature on biology, physiology, and psychology. The main focus of criticism is Driesch's neovitalism, but it was also targeted at the extreme views of "psycho-Lamarchists" who attempt to explain all organic phenomena by mental activities.

Kaila introduces the main theme by citing an example of regeneration: in 1891 Gustav Wolff showed experimentally that a lens removed from the eye of a water lizard grows again in the same place. Neovitalists explain this phenomenon of "self-regulation" causally so that the law of the conservation of energy remains valid. However, Kaila argues, their explanation is in conflict with the "principle of mechanistic causality" which asserts that "the state of a material system at time b is a lawful consequence entirely of the material state of the system and its environment during the preceding time differential a". Kaila's aim is to show that with the properly understood Humean conception of causality this mechanistic principle is a valid "ruler" in the fields of physical, living and mental phenomena.

4 In Finland, in the meetings of the Philosophical Society in the 1890s, Edward Westermarck defended the parallel theory, while Professor Thiodolf Rein accepted the interaction theory between mind and body (see von Wright, 1982). It is interesting to note that Westermarck's work on moral philosophy was based upon naturalist evolutionism, but Rein defended the dualist position by appealing to Darwin's theory of evolution.

If W is state of the system to be explained, and A is the preceding material state of the system, then according to the mechanistic principle W is a lawful function of A, i.e.,

(1) $W = f(A)$.

The vitalists instead take W to be, in the teleological context, a function of the antecedent A and the succedent S:

(2) $W = f(A,S)$.

Kaila protests: if the succedent S is a future event relative to W, then by (2) a future not yet existing event would influence the present, which is impossible. If S is replaced by a "vital power", which expresses a present goal-directed effort, then the term "power" is misused in an anthropomorphic way, since no quantitative specification can be given to the causal relation.

Driesch replaced S by entelechy or "that which carries its goals in itself", but soon he started to speak about the "psychoid" which guides the organism. Thus, he started to use psychological terms, like the "will" to realize goals and the "knowledge" about the means to achieve these goals. So one is drawn to a psycho-Lamarchist position which allows souls to influence the activities of all organisms. Against Driesch's critique of the machine interpretation, Kaila refers to Wilhelm Roux's explanation of self-regulation in terms of the effects and mutual interaction of chromosomes: in each daughter cell of a parental cell there exists "the same machine". This approach goes precisely in the direction that molecular biology and genetics has later followed.

According to Kaila, an expression of vitalism in the field of psychology is the "faculty psychology" which assumes uniform abilities like "imagination" and "intelligence" to explain mental phenomena. For Kaila this would be a return to the questionable theory of mind-body interaction. It would also presuppose independent "psychic causation" – against the fact that the same psychological causes can bring about different consequences. The parallel theory takes psychological regularities – such as the laws of association and reproduction – to be instances of unknown physiological laws. Kaila's view resembles contemporary physicalism which denies the possibility of "mental causation" (cf. Kim, 1996).

Kaila also presents an argument for "the psychological refutation of vitalism": in some cases, all the "energetic" conditions of a functional reaction are satisfied, but the reaction is not realized, since some purely mechanical condition is not satisfied. An example is found in the behaviour of a frog with a deformed brain. But this refutes only that kind of vital "primary functionality" which is realized as soon as the energetic possibility exists. It seems to me that this criticism thus concerns only assumed laws of the form

(3) $W = f(S)$.

Such laws (3) have a simpler form than laws of type (2).

Even though Kaila argues for the validity of the mechanistic principle of causality, he points out that the mechanistic view of nature is only a "philosophical theory of nature" which is questioned by new directions in physics. While psychology is not autonomous in relation to biology, Kaila asserts that "biology is an autonomous science like physics and chemistry". It formulates its own laws, like Mendel's rules of heredity, which we cannot by present knowledge analyse into special cases of physico-chemical laws. Such laws may concern – in Oscar Hertwig's words – special kinds of "biological complexes". This kind of anti-reductionism should not be called "vitalism", Kaila argues.

The question about the "Darwinist principle", which claims that life has developed from nonliving, is left open by Kaila. He concludes his 1920 work with an open question: even though a "solid front" has been established against "dogmatic currents", vitalism and mechanism as historical doctrines are both "expressions of a speculative spirit, and the sceptical research directs its vessel between them into the free and open sea without a shore".

5. Kaila's later work

In his later work Kaila consistently rejected vitalism. But his search for a synthetic philosophy of nature was many times transformed by new scientific theories. His mature position also reflected the adoption of the key ideas of "logical empiricism" in 1926.[5]

The monograph *Beiträge zu einer synthetischen Philosophie* (1928), and its elaboration in Finnish in *Nykyinen maailmankäsitys* ("Contemporary World Outlook", 1929), outlines one of Kaila's synthetic attempts. In the Finnish book, the four main chapters deal with time and space, matter, life, and soul. The scientific background includes the new atomic physics, relativity theory, emergence theory, and psychological Gestalt theory. The central explanatory distinction is between additive and non-additive events and processes: the former are related to wholes which are mechanical and can be "added together", while the latter exhibit new qualities. Hence, a *non-additive* whole is more than the sum of its parts. Quantitative science eliminates from its focus qualities and thereby also non-additivity. Thereby an illusion is created that physical and biological processes are additive and only mental is non-additive. In fact non-additivity is a feature which unifies matter, life, and soul. This conclusion is an expression of Kaila's anti-reductionism.

In its discussion on life, *Nykyinen maailmankäsitys* repeats the criticism of vitalism: entelechies are presented as the "essence of life", but this can be reached neither in philosophy nor in science. Instead, the basic question should be the

5 See the translation of Kaila's 1926 essay in Kaila (2003). For Kaila's changing position in the philosophy of science, see Niiniluoto (1992).

nature of the laws about life. The non-additivity of these laws can be seen by the conjecture that variation within biological development is not purely random, but mildly directed toward some definite directions. Here Kaila seems to assume a theory of directed evolution that has been rejected in the modern Darwinian theory of evolution.

Syvähenkinen elämä (1943) was written in Kaila's active period of logical empiricism. In the same years, Kaila published his first study on the philosophy of quantum mechanics and an essay on the physicalist treatment of the Gestalt problem in psychology. Kaila developed a hierarchical conception of reality, from the lower level of phenomenal things to everyday physical things and the higher level of physico-scientific reality. Degrees of reality correspond to degrees of invariance: things in dynamic processes can be defined as systems of relations or invariances between entities on the lower order of reality (see Kaila, 1979). In the early fifties, Kaila gave up the thesis of translatability and returned to the critical scientific realism of his youth (see Niiniluoto, 1992).

In his later work on the philosophy of nature, Kaila was attracted by the idea of *holism* as a new way of characterizing non-additivity. This can be seen in the article "Elämän ongelma filosofisessa katsannossa" (The problem of life in the perspective of philosophy, 1952), where life is analysed by the conception of "autocatalyctic dynamic equilibrium". Holism was a key of Kaila's unfinished manuscript *Hahmottuva maailma* ("The world as a shaping or structuring whole") in 1958. Kaila accepted at this stage the possibility of reducing biology to physico-chemical regularities, but emphasized the mistakes of both mechanism and vitalism. In his essay "Kybernetiikan illuusio" (The illusion of cybernetics, 1952), Kaila criticized the new attempts of systems theory to analyse self-regulation in machines and organisms by the same kinds of feedback models, and instead called for considerations enlightened by quantum mechanics.

As one product of his philosophy of nature, Kaila published a book on "terminal causality" in atomic dynamics (Kaila, 1956). He planned to continue the project with books on terminal causality in biodynamics and neurodynamics. Kaila again thought that he had a found a new concept as a basis of his unitary approach, to serve in the "field-theoretical" treatment of physics, biology, and psychology. Kaila distinguished between *initial causality*, where the dynamic invariance of the system links the initial state and outcome, and *terminal causality*, where the final outcome depends also on "boundary, limit, and terminal conditions". For example, in the behaviour of a thermostat, the limit condition is a constant temperature to which the system returns from various initial states. Boundary conditions may include restrictions which help to find solutions to the partial differential equations governing the system.

Kaila's proposal remained unfinished. Kari Lagerspetz (1968) argued that, from the viewpoint of Ludwig von Bertalanffy's theory of biological systems, terminal causality is a "pseudo-problem", as Kaila's notion of initial causality is unnecessarily narrow. It is still fair to ask whether Kaila had drifted to such philo-

sophical speculations that he had so strongly criticized in his earlier work.[6] It is interesting to observe that in the proposed reconstruction by Tuomela and Toukonen (1992) initial causality is defined by a process law of the form S(t) = f(S(0)), where S(0) is the state of the system at time 0 and S(t) its state at a later time t, while terminal causality is defined by a law of the form S(t) = f(S(0),B(0,t)), where B(0,t) expresses the terminal conditions of the system during the time interval (0,t). This distinction is essentially the same as the difference between equations (1) and (2), the latter expressing the form of unscientific vitalism. According to Tuomela and Toukonen, Gustav Bergmann's sharp attack on Kaila misinterpreted initial causality to correspond to laws of the form (2) and terminal causality to laws of the form (3) where the terminal condition alone determines the outcome.

In defence of Kaila, one can conclude that his scientific integrity and critical sense of philosophical problems was never compromised. There is nothing to indicate that Kaila would ever have allowed for the boundary and terminal conditions B(0,t) to be replaced by any metaphysically questionable entities. While ontological vitalism is today rejected, just as Kaila consistently argued, questions about reductionism and teleological explanation are vital issues in the contemporary philosophy of biology and psychology. The status of field theories and quantum-theoretical holism also wait for satisfactory treatments and solutions.

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6 Herbert Feigl, Kaila's old friend from the heydays of the Vienna Circle, remarked in 1958 with a friendly tone that Kaila's new ideas are "stimulating but perhaps somewhat speculative" (Feigl, 1967). Gustav Bergmann's (1958) criticism was devastating. A relatively favourable discussion of Kaila's interpretation of quantum mechanics is given by Max Jammer (1974).

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Department of Philosophy
P.O. Box 9
00014 University of Helsinki
Finland
ilkka.niiniluoto@helsinki.fi

Arto Siitonen

Kaila and Reichenbach as Protagonists of 'Naturphilosophie'

Introduction

Eino Kaila (1890–1958) brought new ideas to Finnish philosophy and psychology. He studied at the University of Helsinki in 1908–10 and made study visits, first to Paris in 1911, where he listened to Henri Bergson's lectures, and also to Berlin in 1914. Kaila's dissertation, *Über die Motivation und Entscheidung*, appeared in 1916. He worked as a critic of theatre and literature and as a dramatist in the Finnish National Theatre, before being nominated professor of philosophy in 1921 to the newly founded University of Turku. There he initiated the founding of the first Finnish institute of experimental psychology. In addition to philosophical works, he published works in psychology. Kaila stayed in Turku until 1930, when he became professor of theoretical philosophy and psychology at the University of Helsinki. In 1948 he was invited to become a member of the recently established Academy of Finland. One may speak of Kaila's Turku period (1921–1930) and his Helsinki period (1930–1958).

Kaila considered himself a philosopher of nature, whose task is to articulate, using all available means of science, a coherent conception of the world and of mind's place within it. As fellow of the Finnish Academy, he devoted himself to a great project in natural philosophy. The project had two components: first, a rigorous systematic study aiming at a unitary conception of nature, and second, the explication of this conception in a style accessible to a broader audience. The systematic study was to be realized in three volumes; only the first of these (Kaila (1956)) appeared before his death. As to the more popular work, it was meant to be divided into four parts, of which only the first is complete. The entire planned work was entitled as *Hahmottuva maailma* ("The world as a structuring whole"). Its first part is devoted to the problem of reality; it concerns the perceptual and conceptual components of everyday experience, and it has been translated from Finnish into German (cf. (Kaila (1962)).

Among the influences that Kaila mediated to Finland was the new, empiricist, scientific philosophy that developed at the beginning of the century and was advanced by the Moritz Schlick-led Vienna Circle, and by the *Gesellschaft für wissenschaftliche Philosophie*, which can also be called the "Berlin Group". It was led by Hans Reichenbach (1991–1953). These circles were established in 1929. Kaila had a contact to Reichenbach and Schlick already before that. The

Viennese and the Berlin group cooperated in the organization of congresses, and in the journal *Erkenntnis*, which was jointly edited by Reichenbach and Carnap. (Cf. R. Haller / F. Stadler (1993) and F. Stadler (1997)). There were some differences between the Viennese and the Berliner philosophers. In his *Experience and Prediction* (1938), Reichenbach sees that the basic divergence is rooted in the controversy between *realism* and *positivism* (cf. especially chapters 17 and 25 of that work).

Reichenbach first studied engineering at Stuttgart in 1910. (He worked in 1917–1920 as a physicist in the radio industry). In 1911–15, he studied philosophy, mathematics, physics and pedagogy in Berlin, München, and Göttingen. He received his doctorate in 1915 at Göttingen, and his habilitation work was accepted in 1920 at Berlin. In 1926 the University of Berlin gave him the place of an extraordinary professor of natural philosophy. Reichenbach worked in that position until 1933, when he was expelled from the university. Soon later, he fled Germany. These events were due to the terror of the National Socialist regime. He continued his career in exile; in Istanbul from 1933 to 1938, and in Los Angeles from 1938 until his death. While in Istanbul, he began to write mainly in English, occasionally in French, and published only a few articles in his native German.

This article focuses on how Kaila and Reichenbach developed natural philosophy. Two main points must be stated at the outset. First, they were committed to critical realism – in other words, the distinction between reality itself and our knowledge of it. Second, they believed that natural philosophy required both detailed scientific analysis and a broad philosophical perspective. This study will follow how Kaila pursued the ideas that transformed philosophy in Vienna, in Berlin and elsewhere.

Kaila was influenced early on by Reichenbach. For his first Turku monograph (1925), he had studied Reichenbach's dissertation (1916), habilitation (1920) and philosophical criticism of probability calculus (1920b). Kaila not only admits that he is "much indebted to the inquiries by Reichenbach", but also states that "there is in essential points a full agreement between our basic ideas." (Cf. Kaila (1925), p. 62). This indicates a remarkable affinity. That these philosophers were in many respects congenial thinkers did not, however, prevent Kaila from stating his disagreement with some of Reichenbach's views. In particular, Kaila was dissatisfied with certain details in Reichenbach's dissertation and with Reichenbach's view on the relation between causality and chance. In the philosophers' correspondence, as well as Kaila's other works, one can detect additional influence from, and criticism of, Reichenbach's ideas.

Kaila and Reichenbach shared the following main *topics*: (1) probability and causality as these appear in nature and in our knowledge; (2) space and time, in the cosmos and as we perceive them; (3) the microcosmic world and quantum mechanics. As to the research *methods*, Kaila uses the expression "Erkenntnislogik" (cf. the subtitle of (1925)), whereas Reichenbach speaks of "analysis of science" (cf. (1920), p. 5 and (1938), p. 8). Essentially, the basic orientation is the same.

Regarding the *stream of thought* represented in their works, Kaila introduced the concept of "logical empiricism" and distinguished it sharply from the traditional empiricism that he called "psychologistic empiricism" ((1926), p. 35), and from positivism. His works (1936) and (1941) are, according to their common subtitle, contributions to logical empiricism. Reichenbach introduced the concept "probabilistic empiricism" in (1938), p. viii. Later in his textbook on symbolic logic, however, he used the same concept as Kaila (cf. (1947), p. ix, 8 and 275).

1. On Kaila's Works from the Turku Period

In Turku, Kaila wrote five philosophical monographs in German and one in Finnish. All of these works, except for his treatise on problems of deduction (1928b), deal in some way with Reichenbach's ideas. The two philosophers were in correspondence since March 1923. Kaila had read Reichenbach's work (1920), and asked this to send him the dissertation (1916) and two articles on probability, all of which were mentioned in that work (cf. (1920), footnote 30, p. 108). Reichenbach sent Kaila also his article on the present state of the discussion on the theory of relativity, published in 1921. (Cf. Manninen (2002, p. 245).

1.1. The Work on Chance and Causality (1925)

A common feature of Reichenbach's dissertation (1916) and Kaila's work (1925) is a duality of basic principles: for Reichenbach, that of probability and causality, and for Kaila, of chance and causality.

Let us first study the main lines of thought in Reichenbach's dissertation. It concerns probability and mathematical study of reality. He calls it "paradoxical" that in philosophy, causality is thought to be an objective relation between events while probability is associated with subjective expectation – even though mathematics has developed the very discipline of probability calculus. The question arises whether judgments about probability are ever objectively valid. Reichenbach answers affirmatively and tries to identify the conditions under which this claim is true. He criticizes Carl Stumpf's subjective interpretation of probability, while he approvingly analyses the view of Johannes von Kries. According to von Kries, probability judgments have truth value, state something of the structure of reality and concern future occurrences. In the first chapter, Reichenbach examines the problem situation in general, and in the second chapter, he analyses special probability problems. These problems are illustrated in a thought-experiment involving a "probability machine". This imaginary device is a rolling band on which there are uniformly distributed patches of black and white, into which a piston is striking holes. One may expect that in the long run, there will be as many holes in black as in white areas. The probability that the piston perforates a white area is

½. Other problems analyzed by Reichenbach are games of chance and the theory of errors. He aims to show that given a series of events, such as throws of a die, the frequency of its values is coordinated to the so-called "probability function". This function's particular manifestation is known through experience, whereas the function itself is a philosophical postulate. After all, observation informs us of only a finite number of cases. In this respect, probability is analogous to causality: particular causal connections are revealed by experience, whereby the very principle of cause and effect is presupposed. The laws of probability and of causality differ therein, that the cases determined by the former are exactly those that lack causal connection. The dissertation's third chapter is an attempt to justify what Reichenbach calls "the probability principle", and the final chapter addresses the relation between probability judgments and objective reality. (Cf. Reichenbach (1916)).

Let us then address Kaila's monograph. At the beginning (1925, p. 9), he compares and contrasts two basic principles of natural philosophy: contingency and connection. These operate on two levels: reality and our knowledge of reality. Contingent courses of events are made up of coincidences, whereas connections prevail between events that are bound together by dependencies. Our knowledge of reality is structured by statistical relations and causal connections. The former are based on what he calls "the principle of contingency" (*Kontingenzprinzip*), and the latter on the principle of causality. (Cf. the words 'Zufall' and 'Kausal' in the title of the work). The principle of contingency is fundamentally important in all areas of research that rely on statistical evidence, which raises the question whether all laws of nature are ultimately reducible to the contingency principle. It is even possible to speak of a *contingent* in contrast to a *causal* conception of the world. One task of logicians is to clarify the extent to which a world-view can be based on the concept of chance alone. This project would counterbalance the traditional causal view of reality that does not accommodate notions of chance or independence.

The contingency principle is a statement about the structure of reality to the effect that there is basically disorder among nature's independent entities and events (cf. p. 50 f.). This disorder is counterbalanced by chance. That the occurrences of the universe are uniform, stems from two sources: first, from the balancing effect of chance, and second, from causal dependencies. Kaila counters the common assumption that uniformity would be rooted in causality alone, by claiming that it is even more a result of chance balances (*Ausgleich des Zufalls*) (cf. p. 103).

Kaila considers his work a contribution to the general, axiomatic theory of science, whose task is to identify the conditions of statistical and causal thinking. Among the theoreticians of probability who have influenced his ideas, Kaila mentions especially Johannes von Kries, Hans Reichenbach and Edgar Zilsel. The impetus for his study, Kaila writes, is that (1) some details in their analyses do not satisfy him, and (2) these authors have not reached the highest attainable level of generality in the analysis of statistical thinking (cf. p. 12).

Kaila distinguishes three kinds of dependence relation between occurrences: logical, mathematical and real (or physical). Two otherwise independent occurrences are logically dependent iff knowing one influences knowing the other. In probability calculus, two occurrences are mathematically dependent iff the realization of one changes the probability of the other. Real independence can be attributed, for example, to two magnitudes iff there are no equations between them. Finally, two occurrences are physically independent iff they do not influence each other. The remaining task is to explicate as clearly as possible, what "dependence" actually means – particularly physical dependence.

Kaila challenges as a prejudice the causality conception of seventeenth- and eighteenth-century philosophy and science. In this conception, all occurrences in the world are a single causal chain, in which everything is interconnected. The supposition of real independence therefore clashes with this causal world-view. He refers to Reichenbach's dissertation as one example of this confusion. According to Reichenbach, complete independence of two occurrences "would contradict the principle of causality"; it would be "logically consistent but nevertheless unimaginable" (cf. Reichenbach (1916), p. 74). Reichenbach even writes: "Es hat keinen Sinn, den Begriff eines zufälligen, als eines nichtkausalen, Geschehens zu bilden" (*ibid*.).

In his criticism, Kaila argues that this claim, taken literally, lacks foundation. His argument is a *reductio ad absurdum*: Reichenbach's claim is that the principle of causality co-ordinates to every occurrence certain necessary and sufficient conditions. Thus, according to Kaila, this claim would imply that the conditions of any occurrence include the totality of all other occurrences in the world ("… zu diesen Bedingungen *jedes* Ereignisses das Weltgeschehen als Ganzes gehörte" ((1925), p. 16). But if this truly were the case, without any limitations, then the conditions of an occurrence in this room would include all events a moment ago in foreign stars – which is physically impossible, since the transfer of effects takes its time (*ibid*.).

Next, Kaila addresses the concept of independence. He gives the following definition: magnitude A is physically independent of another magnitude B when as large changes of B as possible influence minimally small changes of A, in relation to the absolute sum total of A. He mentions as an example the movements of my body (B) in relation to those of our "antipodes" (A) in Australia ((1925), p. 17 f). He relates this definition to Reichenbach's definition. Reichenbach states that the "probability machine's" moving band is independent of the movements of the piston – which means that even large changes in the piston's rate correspond only to small changes in the position of holes (cf. Reichenbach (1916), p. 17 f). Kaila paraphrases this as follows: occurrence B is independent of occurrence A when minimally small changes of B are coordinated to changes, however large, of A. (Kaila (1925), p. 18). One may note that Kaila in his own definition proceeds from B to A, and in the paraphrase from A to B; this complicates the comparison between the definitions.

Kaila states that although the difference between the definitions is not essential, nevertheless, certain cases that fit Reichenbach's definition still could not be considered examples of independence. Kaila gives as an example of this a small body in a star's gravitational field, the body being at a distance so great from the star that the acceleration given to it by the star is minimal compared to that given by other bodies. If, however, only this star is considered, then the star can be said to determine fully the movements of the body – in other words, that the movements depend on the star's gravitation, however slight. Kaila's conclusion is that Reichenbach should have made more explicit the *relative* character of physical independence ((1925), p. 18). To illustrate this relative quality, let us imagine a skating boy playing hockey. If only Sirius is taken into consideration, the movements of the puck (A) are *dependent* on Sirius' effect (B): large changes in Sirius' gravitational force are coordinated to minimally small changes in the position of the puck sliding on ice. This is the loophole in Reichenbach's definition. When we turn to Kaila's definition, we realize that when all the boy's movements and hits of the stick are taken into account, in relation to these the possible effects of Sirius on the puck become practically negligible.

Kaila maintains that the principle of contingency is a structural law of reality and a basic axiom of statistical physics. He studies games of chance, such as roulette, in order to show that the principle applies to them. On p. 58 he refers to Reichenbach's ((1916), p. 29) analysis of roulette. His main critical evaluation of Reichenbach's views can be read on pages 62-66. At the beginning, he speaks of a full agreement between him and Reichenbach in essential points (cf. p. 62; cf. above, *Introduction*). He mentions Reichenbach's claim that one may not speak of an objective application of probability without presenting, in a logically satisfactory way, the conditions that we thereby impose on nature. These conditions are twofold: the principle of causality or of lawful connection, which combines events *horizontally*, and the principle of lawful distribution, which binds events *vertically* (cf. Reichenbach (1916), p. 62, 73).

Although he accepts these views, Kaila presents two points at which he disagrees with Reichenbach. Kaila maintains that (i) the concept of independence is fundamental, but treated as subordinate in Reichenbach's theory: although Reichenbach defines this concept precisely, he soon loses it from sight. (On Reichenbach's definition of independence and Kaila's evaluation of it, cf. above). Moreover, Kaila states that (ii) from the subordination of the independence principle follows that the concept of chance is also insignificant in Reichenbach's theory (cf. (1925), p. 62 f).

To point (i): According to Kaila, Reichenbach seems to believe that also those relations that fall under the scope of contingency principle, should be understood as dependencies because total independence would contradict the principle of causality. Kaila sees the reason for this in the concept of independence typically used in probability calculus: the independence of single cases of each other. The principle of causality denies that an individual's throws of a die, for instance, would be

independent of each other, because these movements are causally connected to the same person. However, such independence of single cases is not the same as the concept of independence on which the principle of contingency is based. The latter concept is illustrated be the fact that certain patterns of phenomena are quite stable though their source may vary ((1925), p. 63). – Thus, for instance, the distribution figures of throws of a die (cf. Kaila's fine word *Würfelwürfe*, p. 30) would display the same basic features, no matter who or what produced those events.

To point (ii): Kaila takes it as his task to show that if "chance" means "the coincidence between independent elements", then the concept of chance has a fundamental significance in our conception of reality. Especially, probability theory as applied to reality is bound to be the mathematical theory of contingency ((1925), p. 63). He quotes Reichenbach's claim that physical knowledge consists in projection of a mathematical structure onto perceptions (cf. Reichenbach (1920b); (1989), p. 334). Correspondingly, statistical natural science is the projection of combinatory schemata onto the relations between independent elements – in contrast to causal physics, which coordinates analytical relations to perceptions (Kaila (1925), p. 64). Kaila aims to show, by analysing Reichenbach's "probability machine" thought-experiment, that "chance" actually means "coincidence of independent elements". The cooperation of the rolling band and the striking piston yields an equal distribution of holes in white and black areas (cf. above). According to Kaila, Reichenbach treats the rolling band as a mere scheme of counting and the machine only as an illustration. He cites Reichenbach's statement that the band could be replaced by a clock that measures the piston's strikes and shows the same lawful connection (Reichenbach (1916), p. 25). However, he notes, in that case there would also be mutually independent, albeit purely chronological, coincidences, viz. of measured durations and clock-hand positions. The principle of contingency retains its validity and independence: there are indeed coincidences of independent elements (Kaila (1925), p. 65).

Kaila refers three times to Reichenbach's habilitation from 1920. On p. 172 he agrees with Reichenbach's view on the role of axioms in science. Kaila states that although axioms contain *a priori* knowledge in the sense of objective conditions, this does not imply that axioms would have any definite content or eternal validity. (Cf. Reichenbach (1920), p. 74 and 100). On p. 188 he returns to the theme and rejects the idea that the Kantian forms of perception and categories of understanding would be apprehended as implicit axioms; instead of these, one should explicate logical axioms on knowledge. Here he appeals to Reichenbach's explication of Einstein's axioms in that work (cf. (1920, p. 50 ff). Finally, Kaila makes a critical note to Reichenbach's treatment of the question of a physical magnitude's continuity and fluctuation in the context of quantum mechanics (cf. Reichenbach (1920), p. 77 f). Kaila states that Reichenbach's formulations concerning this question "do not fully satisfy" him ((1925), p. 66 n). It is difficult to make conjectures here, because Reichenbach's approach is rather probing. Thus, he first speaks of the principle of probability function, invented by him in his dissertation, and then sug-

gests that given any principle of coordination, a more general one can be indicated (cf. Reichenbach (1920), p. 77 f.). Presumably, Kaila considered this to be too indefinite. His own view is based on his study of Max Planck's analysis of emission of heat (cf. Kaila (1925), p. 67 f).

Finally, let us address the second one of Kaila's objections, marked above as (2) – i.e., that Reichenbach (as well as von Kries and Zilsel) have not reached the level of generality that is possible in the analysis of statistical thinking (Kaila (1925), p. 12). In the final section of his work, Kaila returns to the idea of a purely contingent conception of the world (introduced by him on p. 9; cf. above), arguing that such a conception is not possible ((1925), p. 205). The principles of contingency and causality complement one another, both in the structure of reality and in our knowledge of reality. However, this knowledge can never be final and closed. The constant conditions of both causal and statistical thinking can be expressed only in approximation (p. 209). One may suppose that by these remarks, Kaila claims to have reached the required generality in the analysis of causal and statistical thinking.

1.2. The Works on Probability Logic (1926) and Synthetic Philosophy (1928)

Kaila begins his 1926 work by analysing the "state of problems" (*Problemlage*) in theory of probability. In the second part, he discusses the principles of probability logic (cf. the title of the work), and the final part is devoted to epistemological applications. He characterizes probability as a logical function of certain truths (cf. p. 32). Probability statements of various degrees make up our conception of reality (cf. p. 33). He opens the second part with a lengthy citation (p. 53f.) of Reichenbach's article on metaphysics and natural science, in which the latter formulates the problem as follows: Neither logically nor empirically can the probability inference be justified; nevertheless, it is indispensable for natural science (cf. Reichenbach (1925), p. 166ff.). Kaila sees in this formulation an expression of Hume's position; he thinks that a decisive step forward from it can be taken by admitting that probabilistic reasoning can be given a logical foundation (cf. Kaila (1926), p. 58). The key to this position is given by the idea of a "harmonic scheme" that comprises all probabilities (cf. p. 57); the relation of similarity gives further clues (cf. p. 59ff.). These yield the basis for the effort of solving the general problem of induction (cf. p. 92).

Kaila's early philosophy of nature reaches its peak in his 1928 work. Here he presents a monistic conception of the world, according to which reality in its innermost character is an unlimited qualitative manifold. Qualities are in themselves always simple. This leads to a rejection of additive, mechanistic and atomistic psychology. Philosophy is a scientific endeavour, and special sciences in turn are philosophical in their basic attitude. Kaila's term *synthetic philosophy* refers to the project of unifying and organizing the results of various sciences. He does this through his study of the world picture of physics, of the relation of inorganic to

organic, and of phenomenal to mental. The phenomenal part of reality gives us access to other levels: we proceed by probabilistic reasoning from the given, the phenomena, to other levels, which make up the not-given. He illustrates this point on p. 36 by introducing an image of an *inverted pyramid.* Our perceptions are the narrow, and shaky foundation upon which we build a huge structure of immeasurable spaces and times. In other words, perceptions are the logical foundation of all knowledge. If his phrase "unermessliche Räume und Zeiten" ((1928b, p. 36) in the simile is understood literally, then the pyramid must be without bottom (or rather, in its inverted position, without roof): its outlines grow indefinitely, and our task to comprehend reality is interminable. It is no wonder that Kaila was never to complete his life-work: the pyramid's area grows with every development in research.

According to Kaila, it is possible that reality has an "infinite depth" (cf. (1928b), p. 57). This view leads to rejection of determinism and strict causality: the presupposition that nature obeys laws means only that the constants known thus far will probably persist in the future (*ibid*.). Here he refers to Reichenbach, "who has also been led to rejecting the existence of a limit function" (*ibid*.). He quotes a passage from Reichenbach's essay (1925). In the English translation this reads as follows:

> Given the contents of experience, we can only make the assertion that for any field function there exists a more precise one possessing a higher degree of probability. To maintain the existence of a final function in this series possessing the probability 1 goes far beyond this assertion. This is the extrapolation that is contained in the deterministic hypothesis. (Reichenbach ((1978a, p. 83).

Here the two philosophers of nature come closest to agreement.

1.3. On the Modern Conception of the World (1929)

Kaila wrote a work in Finnish that presents, in a style accessible to non-experts, the world view that resulted from his research. The title of the work signals its close connection to physics, biology and psychology, and to certain modern streams of thought. He first mentions Reichenbach's work on space and time (1928); he also refers to Bertrand Russell, Rudolf Carnap, Wolfgang Köhler, Max Wertheimer and Eugen Goldstein (cf. Kaila (1929), p. 5 f). The work begins with a poetic prologue that describes a storm in the outer archipelago of southwest Finland, in which the narrator reflects upon the origins of life and thought in the sea. (Cf. p. 9 f). The main themes of the work are: time and space, matter, life, soul.

Other efforts to make natural philosophy familiar to a wider audience were Reichenbach's radio lectures, which were broadcast in Berlin during the winter of 1929-30. Reichenbach based his book *Atom und Kosmos* on these lectures; the book presents the view of the universe according to modern physics ((1930); the

English edition was first published in 1932). Its main chapters concern space and time, light and radiation, matter, and philosophical consequences. He describes "a new philosophy of nature" arising from "closest contact" with scientific investigations (cf. (1957), p. 280; cf. also the introduction to (1928), the booklet (1931), and the (1951) work).

1.4. On the 1929 Correspondence

On July 13, 1929 Kaila sent Reichenbach from his summer residence in southeast Finland a letter written in fine hand. Kaila begins the letter by thanking the addressee for the copy of the work "Ziele und Wege der physikalischen Erkenntnis" (cf. Reichenbach (1978), originally (1929)). Kaila writes of having known the text since May, when its proof-sheet had been the focus of lively discussion within Carnap's circle in Vienna. Kaila, who had spent part of May and June in Vienna, reports having been able to defend before Carnap a position shared by him and Reichenbach – viz. that the probability implications are the basic content of science and cannot be interpreted as truth-functions of the given (in the sense of Russell and Wittgenstein). (Cf. Kaila (1928b), p. 51 f, where he distinguishes truth-functions from probability functions; and (1926), p. 66 ff, where he distinguishes demonstrative from probabilistic reasoning; also (1928a), p. 81 on suppositions concerning future). Next, Kaila remarks that he can hardly maintain that he could have influenced in any way "the horrible dialectician Carnap". He closes the letter by stating that he hopes to be able to write more to Reichenbach in the coming weeks on these issues (HR 014-09-14; the document is quoted with permission from the University of Pittsburgh Libraries Special Collections Department. The University reserves all rights.).

Reichenbach's letter of reply dates July 23 (HR 014-09-13). He expresses regret that Kaila, on his way back from Vienna, had been unable to visit Berlin, where he could have met the circle that focused on questions of probability. He agrees fully with Kaila on the distinction between probability statements and truth functions. Reichenbach also writes of having recently developed a frequency theory of probability that appears to solve the problem of defining probability. He proposes that Kaila participate in an autumn meeting in Prague and support him in the debate with the Viennese "pure logicians". Kaila, in his August 7 response, thanks Reichenbach but declines the invitation on the grounds of September teaching duties. In a thorough treatment of Reichenbach's above-mentioned treatise, Kaila analyses those views which he expects will be criticised by Carnap. (HR 014-09-12).

1.5. A Critical Study of Logical Neopositivism (1930)

Kaila's (1930) work contains exposition and criticism of Rudolf Carnap's book, *Der logische Aufbau der Welt* (1928). In the introduction, Kaila examines Carnap's

work in the context of self- reflection in mathematics (formalization, axiomatization), in logic (Russell) and in physics (the theory of relativity). He believes that Carnap has concisely interpreted the modern scientific thinking. He even compares Carnap to Newton and Kant. However, Kaila notes that Kant presented the valid principles of his contemporary science as eternally valid, and that a corresponding danger looms before the logical neo-positivism. Moreover, Carnap's results would imply "the end of all philosophy" (Kaila (1930), p. 11; cf. p. 35). A paradox arises, because despite these "catastrophic consequences" (cf. p. 30, 73), it is difficult to identify the flaws in Carnap's program. Tracing the loopholes in the program is the largest, critical part of Kaila's work ((1930), pp. 42-93).

Kaila's exposition is concerned with Carnap's principle of analytic equivalence, its role in mathematics and empirical science, and also its philosophical consequences. According to this principle, all concepts that are coordinated by definition have the same meaning, despite the various contents that one may have in mind while expressing them. Carnap concludes from this that the issue of other minds is a pseudo-problem. Kaila also discusses Carnap's analysis of Hume's problem of induction. Because all statements about the future are, according to Carnap's analysis, actually statements on the past, Carnap considers also Hume's problem to be a pseudo-problem. Kaila does not accept these views. He clarifies the logical machinery, epistemological principles and method of "quasi-analysis" that make up Carnap's "system of constitution", i.e. the logical system of objects or of concepts. (Cf. Carnap (1928), p. 1).

In his criticism, Kaila finds "a severe inconsistency" (p. 42) and "a deep rupture" (p. 43) in it: it is not possible to determine the *direction* of a relation and the character of *time* in a purely extensional way, as postulated by the system. Rather, such a determination requires intentionality and time. This leads him to a study in phenomenology of time in the spirit of Edmund Husserl. Time always has a direction: it flows from past through present to future. The decisive question is whether the structure of time can be constituted by Carnap's method. Kaila's answer is negative: such a constitution takes time for granted rather than explaining it. A corresponding question concerns space. The infinity of perceptual space escapes the possibility of Carnapian constitution. Thus, the method fails in these cases. Kaila reveals its psychological and epistemological presuppositions. He distinguishes Carnap's theory of constitution from genuine, probabilistic science (cf. p. 80 f. and 87). Kaila states that Carnap's theory is rather a reinterpretation of the method of physics, than an articulation of its real nature. Which one to choose? (cf. p. 86).

One of Kaila's basic convictions is that the concept of probability defines reality (cf. (1930), p. 82 f, 87; cf. also Kaila (1928b), p. 54). As he admits on p. 83 of (1930), he basically agrees with Reichenbach's conclusions on this matter. Kaila even speaks of Reichenbach's "tiefgründigen Forschungen" (p. 83), referring here to Reichenbach's work on the aims and methods of physical knowledge ((1929); in English (1978)). One may note that only in this work does Reichenbach refer to Kaila. Reichenbach considers him to represent "the objective theory of prob-

ability" (Reichenbach (1978), p. 202). Cf. also (1978), p. 223f.; Reichenbach is here referring to Kaila's (1925) and (1926) works). Reichenbach discusses "the probability principle" ((1978b), p. 205) and mentions in his notes that "Kaila has proposed a 'contingency principle' in its place" (p. 224). Moreover, this is the text in which Reichenbach introduces an important concept, which he here calls "the probability inference" ((1978b), p. 150ff.), and later "the probability implication" (Reichenbach (1935), (1938)). Kaila read Reichenbach's (1929) work meticulously; after praising it, he states (Kaila (1930), p. 83) that he does not quite understand ("Nur verstehe ich nicht"), why Reichenbach characterizes the probability principle as a "metaphysical assumption" (Reichenbach (1978b), p. 150) while also claiming that "the laws of probability are our most secure possession" (Reichenbach (1978b), p. 172). Does not this show, asks Kaila, that the probability principle is the *definiens* of the concept of reality? He adds that if the principle is not valid, then "life is just an illusion or a dream" (Kaila (1930), p. 83). – One may note that in all of Reichenbach's later works, his use of the words 'metaphysics' and 'metaphysical' is always pejorative.

One may remark that Carnap published a review of Kaila's work in *Erkenntnis* 2 (1931), in which he considered it a clear presentation of his theory. Carnap presents Kaila's objections as numbered from 1 to 6, and replies briefly to each one. He concedes in one point, and in another criticizes Kaila's Leibnizian definition of reality as "not sharp enough in order to be analysed" (cf. p. 77). However, he admits that the decision of many still open questions will perhaps show that Kaila is right. Finally, let us mention Reichenbach's criticism of Carnap's work. He published a review (1933) of it, in which he claims that our assertions about reality exceed the scope of the phenomenalistic reduction assumed by Carnap.

2. On Kaila's Works from the Helsinki Period

Kaila and Reichenbach finally managed to meet in early June 1931. Kaila sent from Helsinki a postcard in which he lets Reichenbach know where he will be staying in Berlin. He writes that he would finally like to learn to know Reichenbach also in person (HR 014-53-05). In another postcard, written in Vienna on February 2, 1932, he thanks Reichenbach for the article on axiomatics of probability calculus. He praises the text's clarity and expresses hope for its development into a comprehensive presentation of the problems of "probability, causality etc." (HR 014-53-04). Reichenbach's response dates February 6. He thanks Kaila for encouragement and writes that he has been able to write about half of the planned book over the winter. (HR 014-53-03). Kaila, still in Vienna, sent his reply three days later, which expresses delight with Reichenbach's work and the hope that it will soon be published. (HR 014-53-02).

The work in question is entitled *Wahrscheinlichkeitslehre* (*The Theory of*

Probability). It would appear in Leiden in 1935, after many difficulties (German publishers did not take it), its author having then spent two years in the Turkish exile. Kaila read the text immediately and mentions it in his work *Über das System der Realitätsbegriffe* from 1936.

2.1. On the System of Reality Concepts (1936)

This work, according to its subtitle, is a contribution to logical empiricism. Kaila had introduced the name of this stream of philosophy on p. 35 of his (1926) work (cf. above). In the 1936 work, he defines logical empiricism as the view that the truth value of statements on reality is decided by experience; any such statement must be empirically confirmable as true or false. Kaila's method follows the idea that he had presented in the (1928b) work: our thought proceeds from the given to the non-given aspect of reality (cf. p. 36 ff of that work; cf. above). "Reality" is defined as regularity, i.e., invariance. Its basic level is the *phenomenal* world, which consists of optic, acoustic and haptic perceptions. From this level, we proceed (via *Ableitung*, i.e., reasoning) to the more regular, empirical (*physisch*) reality. We learn of the highest degree of invariance by rising (*Aufstieg*) to physical (*physikalisch*) level.

Kaila mentions Reichenbach in the final section of his treatment of empirical reality, in the context of discussing our conception of space. One may ask: what is the *logical* content of this conception? The position of Immanuel Kant was that this question can be answered on the basis of pure geometrical intuition. Kaila objects by arguing that the answer can be found only after the intuition has been applied to spatial bodies. He adds that Kant has "here, as also elsewhere" overlooked this logically basic problem of application (Kaila (1936), p. 62). Here he refers to Reichenbach's works (1928) and (1935). In *Wahrscheinlichkeitslehre*, Reichenbach argues that Kant's assumption of the *a priori* validity of the principle of causality does not explain the role of induction, that form of reasoning which helps us to discover empirical causal relations (for example, between an electric current and a magnetic needle); we need experiments (Reichenbach (1935), p. 10). In the same work, Reichenbach addresses the problem of justifying induction, which Kant had tried to solve by claiming that the principle of causality is based on synthetic *a priori* judgment. According to Reichenbach, such judgments have been rejected in modern natural philosophy. He also notes that Kant never applied his principles in any thorough analysis of inductive reasoning ((1935), p. 411).

2.2. Human Knowledge (1939)

This work is a textbook. It was immediately translated into Swedish by Kaila's pupil Georg Henrik von Wright, and was part of the curriculum of Scandinavian universities' philosophy departments. Readers can still benefit from it today. One may compare it to Kaila's work on the modern conception of the world (1929;

cf. above); now, in 1939, the theme is modern conception of human knowledge. According to its subtitle, the work is concerned with what human knowledge is and what it is not. It is divided into three main parts: (I) Theory building, (II) Formal truth of theory, and (III) Empirical truth of theory. The leading idea is that scientific knowledge is based on the search for *invariances*. This is exemplified by "economy of thinking" (Ernst Mach), rationalization of concepts, and explication of isomorphic structures. Kaila distinguishes two epistemological traditions: Aristotelian and Galilean. The latter is concerned with invariable laws that can be found specifically in the variability of certain processes, for instance in the acceleration of falling bodies.

Kaila mentions Reichenbach in part I, section 5, when discussing induction and probability. Kaila asks: can all probability be interpreted as relative frequency, or are there cases in which one can only speak of "likelihood" that cannot be exactly defined? He says that among modern logical empiricisists, there is only one remarkable scholar favouring the first alternative, namely Reichenbach (Kaila (1939), p. 103). From this statement, one may conclude that Kaila himself does not consider the frequency interpretation to be exhaustive. In part II, section 1, he discusses the relative character of logical truths and mentions Reichenbach's idea of a continuous scale of probabilities between 0 (falsity) and 1 (truth) (Kaila (1939), p. 151, referring to Reichenbach's work (1935), p. 368). In the third section of part II, p. 175, Kaila refers to Reichenbach's statement that psychological necessity is only a correlate to logical necessity (cf. Reichenbach (1928), p. 56). Finally, in part III, section 1, p. 211, Kaila mentions Reichenbach in his discussion of the concept of mathematical probability.

2.3. On the Physical Concept of Reality (1941)

This work is Kaila's "second contribution to logical empiricism" (cf. its subtitle), the first having been the 1936 work. Actually, the 1941 work may also be called the "third contribution", because the work on human knowledge explicates four principles of logical empiricism (cf. Kaila (1939), pp. 173 ff, 181ff., and 258ff.). One may also note that while the (1936) book concerned the phenomenal, empirical and physical levels of reality, the present work focuses on the physical level. In the introduction, Kaila says that the concept of physical reality has been crisis-stricken since the birth of modern quantum physics, to the effect of becoming questionable (Kaila (1941), p. 7). The work consists of six parts and an appendix. Its main topics are the method of logical empiricism; the principle of invariance; empirical (*physisch*) space; the theory of measurement; the content of microphysical theories; and the so-called "Euclidean intuition of space". In section 4 of part II, Kaila stresses that logical empiricism is not "positivism": the former is as compatible with epistemic realism as it is with phenomenalism, if not more so (cf. (1941), p. 47ff.).

Regarding the philosophy of space and time, Kaila mentions on p. 100 Re-

ichenbach's distinction between the roles of experience and of convention, but adds that Reichenbach's concept of "real time" is unclear (cf. Reichenbach (1928), p. 140). On p. 118, Kaila mentions Reichenbach's axiomatic treatment of the theory of space and time (cf. Reichenbach (1924)). On p. 128 he criticizes Reichenbach's concept of universal forces as being "a curious metaphysical remnant" and says that his own theory of measurement makes these redundant (cf. Reichenbach (1928), esp. sections 3-6 of Ch. I on space, sec. 17 of Ch. II on time, and sections 40-44 of Ch. III on space and time). – One may defend Reichenbach here by noting that he introduces, by means of a thought-experiment, the imaginary possibility of such forces, and distinguishes them from real, directly demonstrable forces, which he calls "differential forces". – On pages 131-36, Kaila rejects Poincaré's and Reichenbach's conventionalism. He cites Reichenbach's work (1928), p. 72, according to which space in itself is neither Euclidean nor non-Euclidean, but rather a continuous three-dimensional manifold. He challenges this view by asking: what is here understood by the term "space"? Phenomenal or geographic or physical space – or various mathematical spaces? (cf. Kaila (1941), p. 133). On p. 169 Kaila praises Reichenbach's detailed analysis of Helmholtz' theory of space. Finally, in the appendix Kaila discusses Reichenbach's view that there are non-Euclidean phenomenal experiences (p. 180; Kaila refers to Reichenbach's article (1931b)). He tells of having designed psychological experiments in order to test this view. These experiments seem to put Reichenbach's position in question, but the very problem must be formulated more precisely, so that it may be addressed fruitfully. Here, Reichenbach's distinction between the pictorial and the normative function of spatial experience will be helpful (Kaila (1941), p. 180 - 183 and p. 187; cf. Reichenbach (1928), p. 52).

2.4. Works on Quantum Mechanics ((1950) and (1956))

In 1944, Reichenbach published a work on the foundations of quantum mechanics. Kaila, in his work on the metatheory of quantum mechanics (1950), refers to that work, along with three other books ((1928), (1935), (1938)) and an article from 1948 by Reichenbach. Kaila speaks at the beginning of the Preface to his (1950) work of "das lehrreiche Werk von REICHENBACH …" (p. 3). Nevertheless, he does not adopt Reichenbach's approach of applying a three-valued logic to problems of quantum mechanics. Reichenbach distinguished three metalanguages: the corpuscle language, the wave language and the neutral language (cf. Reichenbach (1944), p. 146). Kaila's aims instead are to distinguish the classical from the quantum mechanical language, and to solve the problem of coordinating these languages to each other (Kaila (1950), p. 3).

In his (1956) work, Kaila continued his exploration of philosophical problems of microphysics. This work was meant to be the first part of a longer project; its theme is "terminal causality in atomic dynamics". In the Preface, Kaila is wondering whether his inquiry belongs to philosophy or to natural science. He concludes

that the adjective *naturphilosophisch* best captures the specific character of such inquiry (cf. (1956), p. 3). In comparison to the dichotomy of causality and chance in Kaila's (1925) work and beyond, he now introduces a continuum between the classical "strict law" on the one hand and pure chance on the other. This continuum is a broad gradual area of terminal-causal laws. He calls the area "the 'anti-classical', 'flexible' causality" (cf. (1956), p. 8f.).

The intended goal, viz., to create the basis for a unitary concept of nature, remained unfulfilled; the second part never appeared. The project of Kaila's last years, to reach a synthesis on all fields of natural philosophy, would exceed the life-span and energies of any human being. Perhaps such a goal is unattainable even in principle. The opening sentence of the Introduction has a symbolic significance in this respect; it says that "Terminus" means "limit and end" (cf. (1956), p. 7).

Death also prevented Reichenbach from completing an important work, *The Direction of Time* (cf. Reichenbach 1991). This work deals with the issues of time order of mechanics, time direction in thermodynamics and microstatistics as well as in macrostatistics, and the time of quantum physics. The introduction discusses time's emotive significance. The final chapter would have dealt with the relation between the subjective experience of time and its objective properties in nature.

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University of Helsinki
Department of Philosophy
Siltavuorenpenger 20 A
SF-00014 Helsinki
Finland
arto.siitonen@helsinki.fi

Carl Henrik Koch

Jørgen Jørgensen and Logical Positivism

"I believe that, of all of us, he alone does his worst as a critic of our era." Otto Neurath characterised the new co-editor of the series *Einheitswissenschaft*, the Danish philosopher Jørgen Jørgensen (1894–1969),[1] professor of philosophy at the University of Copenhagen (1926–1964) with these words in a letter to Rudolf Carnap in November 1934.[2]

At the time, Jørgensen was already a close acquaintance of both Neurath and Carnap. They met in 1930 at the Seventh International Congress of Philosophy in Oxford, at which Moritz Schlick had spoken of "The Future of Philosophy",[3] and Jørgensen himself gave a lecture on "The Principal Metaphysical Implications of Recent Physical Theories and Points of View".[4] He cut such an impressive figure that he was elected to the International Permanent Committee for Congresses of Philosophy, of which he remained a member until 1950, and was encouraged by

1 Jørgen Jørgensen's thinking on philosophy is depicted in C.H. Koch, *Dansk filosofi i positivismens tidsalder 1880–1950.* Copenhagen: Gyldendal, 2004, pp. 187–241. About Jørgensen's life and work, please also refer to O. Neurath, "Encyclopaedism as a Pedagogical Aim: A Danish Approach" in: *Philosophy of Science*, Vol. 5, 1938, pp. 484–492; J. Witt-Hansen, "Jörgen Jörgensen and the Grammar of Science" in: *Danish Yearbook of Philosophy*, Vol. 1, Copenhagen, 1964, pp. 159–172; J. Witt-Hansen, "Jørgen Jørgensen. 1 April 1894–30 July 1969" in: *Festskrift udgivet af Københavns Universitet i Anledning af Universitetets Aarsfest, November 1969,* Copenhagen: The University of Copenhagen, 1969, pp. 241–247; J. Witt-Hansen, "Obituary on Jørgen Jørgensen" in: *Logique et analysis*, N.S. Vol. 12 (No. 46), pp. 121–122; N.E. Christensen, "Jørgen Jørgensen as a Philosopher of Logic" in: *Danish Yearbook of Philosophy,* Vol. 13, 1976, pp. 242–248; J. Faye, "København og den logiske positivisme" in: Th. Söderqvist, J. Faye, H. Kragh & F.A.. Rasmussen (eds.), *Videnskabernes København,* Copenhagen: Roskilde Universitetsforslag, 1998, pp. 43–55; and C.E. Bay, "Den unge Jørgen Jørgensen som repræsentant for den kritiske idealisme" in: *Kulturradikale kapitler fra Georg Brandes til Otto Gelsted,* Copenhagen, C.A. Reitzel, 2003, pp. 127–146. An almost complete list of Jørgensen's publications is available in *Danish Yearbook of Philosophy*, Vol. 1, 1964, pp. 183–196. A number of Jørgensen's most important articles are collated in *Danish Yearbook of Philosophy*, Vol. 6, 1969.

2 Letter of 14 November 1934, quoted in Brian F. McGuinness (ed.), *Unified Science. The Vienna Circle Monograph Series.* Originally edited by Otto Neurath, now in an English edition, with an Introduction by Rainer Hegselmann. Dordrecht/Boston/Lancaster/Tokyo: D. Reidel, 1987, p.xv.

3 *Proceedings of the Seventh International Congress of Philosophy,* held at Oxford, Great Britain, September 1–6, 1930, Oxford: Oxford University Press, 1931, pp. 112–116.

4 "Some Remarks Concerning the Principal Metaphysical Implications of Recent Physical Theories and Points of View", ibid., pp. 1–8.

Schlick, Léon Brunschwicg and Federigo Enriques to contribute to their respective journals. A year later, Carnap sent his *Abriss der Logistik* (1929) to Jørgensen, whose letter of thanks mentioned that he had already read Carnap's earlier work, *Der logische Aufbau der Welt* (1928).[5] In March 1932, at Hans Reichenbach's invitation, Jørgensen gave a lecture in Berlin at the *Gesellschaft für empirische Philosophie* entitled "Über die Ziele und Probleme der Logistik". Later that year, he arranged for both Carnap and Neurath to visit Copenhagen and hold guest lectures.[6] In advance of the meeting in Berlin, Carnap sent Jørgensen parts of his manuscript for *Die logische Syntax der Sprache*, which they had discussed in both Berlin and Copenhagen. According to a first-hand account given by Jørgensen to the author of this article, it was he who suggested the title for Carnap's book. Following the book's publication, Jørgensen wrote an enthusiastic review of it in *Erkenntnis*.[7] Jørgensen had thus been accepted into the logical positivists' circle, a movement that supporters called "our circle" or "our movement", and within a short time he was involved in both editorial and organisational activities.[8] At the 1935 *Congrès international de Philosophie Scientifique*, held in Paris, a motion was passed that future congresses would sponsor a project to compile

5 See concept to letter of 5 September 1931 from Jørgensen to Carnap, "Jørgen Jørgensens Papirer", I. Letters, capsule 2, The Royal Library, Copenhagen. Jørgensen's correspondence with logical positivists consists mainly of letters from Otto Neurath concerning editorial and organisational subjects and conceptual drafts for answers. Part of the correspondence with Neurath regarding the planning of *Zweiter internationaler Kongress für Einheit der Wissenschaft,* which was held in Copenhagen, and for which Jørgensen acted as secretary. Jørgensen's opening address is printed in *Erkenntnis*, Vol. 6, 1936, pp. 278–285.

6 See letter from Carnap to Jørgensen of 4 November 1932 in "Jørgen Jørgensens Papirer".

7 *Erkenntnis*, Vol. 4, 1934, No. 6, pp. 419–422.

8 Much of the information about Jørgensen's relations with the logical positivists and his participation in their meetings is based on his autobiography, which was printed in *Festskrift udgivet af Københavns Universitet i Anledning af Universitetets Årsfest, November 1966*, Copenhagen: The University of Copenhagen, 1966, pp. 139–149; and in "The Development of Logical Empiricism", *International Encyclopedia of Unified Science*, Chicago: The University of Chicago Press, 1951, Vol. II, number 9, pp. 40–48. An extended version of Jørgensen's account of the history of logical positivism had been previously published in *Festskrift udgivet af Københavns Universitet i Anledning af Hans Majestæt Kongens Fødselsdag, 11 March 1948*, Copenhagen: The University of Copenhagen, 1948, pp. 1–97. In the same year, a special edition was published with the title *Den logiske Empirismes Udvikling.* On 4 February 1937, Neurath asked Jørgensen to write an outline of the history of logical positivism, and Jørgensen consented the same month, although he also made it clear that he would not be able to finish the work until late 1938. On 6 May 1937, Neurath accepted the proposed deadline for submission, but the outbreak of hostilities delayed the work until after World War II. The letters mentioned (and Jørgensen's draft letters) are available in "Jørgen Jørgensens Papirer", see Note 5.

an *International Encyclopedia of Unified Science*. The organising committee for the project consisted of Jørgensen, Neurath, Carnap, Philipp Frank, Charles William Morris and Louis Rougier. At the 1938 logical positivists' conference in Cambridge, Jørgensen, along with Carnap, Frank and Morris, became an associate editor of the series *Library of Unified Science*, which was edited by Neurath.

I

Jørgen Jørgensen was born 4 April 1894. His father, a church minister, died in 1901. His Christian upbringing seems to have turned Jørgensen against all forms of religiousity from an early age. In 1912, he started to study philosophy at the University of Copenhagen, and within a year was awarded a gold medal for a prize essay on Schopenhauer's epistemology and its relationship to Kant. In his essay Jørgensen followed the neo-Kantian Marburger School founder Herman Cohen's rejection of Schopenhauer's critique of Kant. It was as a young neo-Kantian, as a critical idealist, and as a supporter of the Marburger School's epistemological interpretation of Kant's transcendental philosophy that he embarked upon his philosophical career. He maintained this position throughout his time as a student, in conscious opposition to that of his teacher Harald Høffding (1843–1931), whose philosophy was more empirical and positivist, and who favoured a psychological interpretation of Kant.

The Marburger School saw Kant's critical idealism first and foremost as epistemology. In order to avoid accusations of subjective idealism, i.e. of presuming that reality does not exist outside of consciousness, Kant claimed that behind the sensory impressions that make up the material of human knowledge lies a world of the thing *per se* ("Dinge an sich") of which we have no cognition. The neo-Kantians de-ontologised this hypothesis, and instead considered the concept of actual reality to be an expression of an epistemological ideal that science, as part of an unfinished process, constantly tries to approach. Jørgensen never abandoned the idea that human cognition develops in a continuous approximation to the truth, even though over the years he replaced his critical idealism with empiricism and critical realism. For example, he wrote in 1926:

> It runs like a red thread [...] through all development that it leads to ever more clear and safer concepts of existence, and it can therefore be considered as *a number of successive approximations or approaches to the truth*.[9]

Along with Kant's demonstration that a metaphysical, holistic view of a reality that reaches beyond the empirical world is excluded, this de-ontologisation of

9 J. Jørgensen, *Filosofiske Forelæsninger som Indledning til videnskabelige Studier*, Copenhagen: Levin & Munksgaard, 1926–1927, p. 13.

"things *per se*" led to the Marburger School rejecting all metaphysical interpretations of Kant's critical idealism. For the Marburgers, the most famous of whom were Herman Cohen, Paul Natorp and Ernst Cassirer, philosophy was "the theory of principles of the sciences and consequently of all culture."[10] Throughout his life, Jørgensen maintained the school's anti-metaphysical posture. While still a student, he expressed his critical attitude in a short book about Henrik Bergson, published in 1917. He ends it with the words:

> There does not appear to be a single word in all of B's philosophy that designates a real scientific concept; and should there be one, then according to his view of conceptual cognition it would only be there because of inconsistency or negligence. This much is clear: that from an intellectual standpoint his theories are untenable, not so much because they postulate something that is wrong, but because in the final analysis they say nothing. They are mainly streams of words that often sound good, but are ultimately empty.[11]

Jørgensen graduated in 1918 as a Master in philosophy, which in those days was akin to a combination of a modern bachelor's and master's degree in Philosophy as well as a PhD. His major thesis, which corresponds to the present-day PhD thesis, dealt with Herman Cohen's philosophy. When Cohen died in the same year that Jørgensen was writing his thesis, he reworked it into a small book about Paul Natorp.[12]

II

In the years immediately following his graduation, Jørgensen radically changed his philosophical attitude. One reason for this was his increasing interest in formal logic, the philosophy of mathematics, and, in particular, Bertrand Russell's empirically oriented philosophy. In his autobiography he refers to Russell, whose personal acquaintance he made in the 1930s, as his "great model" of the time.[13] However, he also mentions Herbert Iversen (1890–1920), a legendary figure in Danish philosophy, who in 1918 had published *To Essays om vor Erkendelse* (*Two Essays on our Knowledge*).[14] With this book, Iversen made himself a spokesman

10 From H. Cohen and P. Natorp's foreword to E. Cassirer, *Der kritische Idealismus und die Philosophie des 'gesunden Menschenverstandes'*, Gieszen: Alfred Töpelmann, 1906.

11 J. Fr. Jørgensen, *Henri Bergson's Filosofi i Omrids*, Copenhagen: Nordiske Forfatteres Forlag, 1917, pp. 83–84.

12 J. Fr. Jørgensen, *Paul Natorp som Repræsentant for den kritiske Idealisme*, Copenhagen: Nordiske Forfatteres Forlag, 1918.

13 *Festskrift 1966*, op.cit., p.145.

14 H. Iversen, *To Essays om vor Erkendelse,* Copenhagen: H. Aschehoug & Co, 1918. Iversen's philosophy is described in E. Rubin, *En ung dansk Filosof og hans Værk samt Erindring og Erkendelse, en Dialog*, Copenhagen: Gyldendal, 1920, pp. 9–69;

for an extreme form of empiricism, and influenced Jørgensen in an empirical direction. Iversen's importance for Jørgensen's philosophical development was expressed, for example, in the lecture entitled "The Development of Empiricism in Scandinavia", which Jørgensen gave in 1935 at the International Congress for Scientific Philosophy in Paris. It outlined the main principles of Iversen's philosophy and drew parallels with contemporary developments in logical positivism.[15] As early as his student days, Jørgensen had read Ernst Mach's *Die Analyse der Empfindungen* (1900) and *Erkenntnis und Irrtum* (1905), and Mach's pupil Karl Pearson's *The Grammar of Science* (1892). However, it seems that it was not until he encountered Iversen that he accepted Mach's monism and his idea of the unity of science, and Pearson's assertion that "the universe is largely the construction of each individual mind" and that "the unity of all science consists in its method alone, not in its material."[16] In full agreement with both Mach and Pearson, Jørgensen wrote in 1928 that the physical picture of the world "is a construction, which is formed by connections on the basis of our direct experiences".[17] In the same year, Carnap's *Der logische Aufbau der Welt* was published, and considering the above quotes and his interest in formal logic, Jørgensen must have read the book with great interest. The perception of science as unified, and empirical monism's teaching that all science is ultimately based on experience, had from the early 1920s, become basic principles in Jørgensen's philosophical thinking.

III

In 1924, the Royal Danish Academy of Science and Letters, at Harald Høffding's request and with Jørgensen in mind, called for submissions for a prize thesis, the subject of which was announced as follows:

> To examine the principal forms that general logical theories have assumed in the work of Boole and his successors, with a demonstration of their historical development and their relation to classical logic and an indication of the position which logic should, according to these theories, occupy in relation to philosophy and mathematics.[18]

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and in Koch, *Dansk filosofi i positivismens tidsalder*, op.cit., pp. 395–418.

15 J. Jørgensen, "The Development of Empiricism in Scandinavia", in *Actes du Congres international de Philosophie Scientifique*, Paris: Hermann, 1936, Vol. 8, pp. 62–67.

16 K. Pearson, *The Grammar of Science,* Third ed., London: Adam & Charles Black, 1911, p. 12 & 15.

17 J. Jørgensen, *Filosofiens og Opdragelsens Grundproblemer*, Copenhagen: V. Pios Boghandel & Poul Branner, 1928, p. 36.

18 See *Det Kongelige Danske Videnskabernes Selskabs Forhandlinger June 1923–May 1924*, Copenhagen: The Royal Danish Academy of Sciences and Letters, 1924, pp. 136–137.

Jørgensen threw himself into the assignment with great vigour, and spent 18 months writing a 1,034-page response, all the while keeping up a full-time day job as secretary to an employers' federation. The huge scale was partly due to the fact that Jørgensen refused to restrict himself to what was necessary in order to respond to the assignment, because he wanted to write a handbook in modern, symbolic logic and to discuss the philosophical reflections to which it had given rise. Jørgensen's thesis was awarded a gold medal in 1926, and in the same year he was appointed professor of philosophy at the University of Copenhagen, a position he retained until his retirement at the age of 70 in 1964.

Jørgensen's prize thesis was published in English in 1931 in three large volumes, and entitled *A Treatise of Formal Logic*.[19] The work is a monument to formal logic and to the development of the philosophy of mathematics until 1924. Ironically enough, it was published during the same year that Bertrand Russell's attempt to develop mathematics from formal logic, a project supported by Jørgensen, was dealt its deathblow by the Austrian mathematician, logician and philosopher Kurt Gödel. Gödel succeeded in proving that there are mathematical truths that – given the finite, (i.e. finitely controllable) methods of proving them that logic employs – could never be proven within logical systems except at the expense of the systems' consistency.

The work's first volume described the historical development of logic from Ancient Greece to the present day. The second explained systematically classical logic, logical algebra and modern symbolic logic, culminating in a similarly systematic account of the attempt to derive mathematics from logic. The third volume focused upon Russell's logistics and the problems inherent in the attempt to derive mathematics from logic. Herein lies Jørgensen's independent contribution to the philosophy of logic and mathematics.

There are two discussions in Jørgensen's work that point towards his later interest in philosophy of logic and his increasing scepticism about formal logic's attempt to identify the conditions and criteria for logical implication. One of these addresses the relationship between logic and psychology, the other concerns whether, and to what extent, the meaning of statements and logical operations is relevant to the identification of such conditions and criteria. Where the first discussion focuses on the relationship between the real and formal sciences and their possible connection within a unified science, the second addresses whether, and to what extent, extensional logic presupposes intentional logic.

Very traditionally, Jørgensen assigned to logic the job of analysing and criticising human thinking, as expressed in linguistic utterances. While it is the job of logic to decide on the logical validity of inferences – i.e. whether the thinking is in accordance with itself – it is the job of psychology and linguistics to empirically

19 J. Jørgensen, *A Treatise of Formal Logic, its Evolution and Main Branches, with its Relations to Mathematics and Philosophy*, Vols. 1–3, Copenhagen/London: Levin & Munksgaard/Oxford University Press, 1931.

explore those forms of inference that actually exist. This entails empirical material being among logic's actual preconditions, which leads to the conclusion that the possibility of a complete logic can be excluded:

> Thus also logic can never be regarded as having a definitive foundation, for since the forms and rules of operation can only be arrived at by analysis of material procured by induction, we never know whether this is exhausted, or whether new forms and rules yet remain to be found.[20]

Jørgensen's view of logic in *Treatise* was quite different from the one espoused by his great role model Bertrand Russell, for whom the world of logic is a world of immutability, one which is explored by means of conceptual methods and not through experience. Jørgensen saw it differently, arguing that logic does not include a conceptual recognition of a timeless world, but is based on knowledge of processes of thought extracted by means of introspection, and on an analysis of linguistic utterances. Jørgensen, the anti-metaphysicist, had to reject the Platonic metaphysics underlying Russell's understanding of logic and mathematics.

In an implicit rebellion against a formalist conception of logic, Jørgensen rejected the idea of the logician as a nominalist – i.e. one who regards symbols as just signs, the meaning of which is given by dint of the rules that decide which combinations of signs are permissible:

> Logistic symbols and groups of symbols (definitions and propositions) [must] always have a meaning, and it is this meaning that determines the rules for manipulation of the symbols.[21]

Jørgensen also asserted the same opinion after he encountered logical positivism. For example, in his March 1932 lecture to *Gesellschaft für empirische Philosophie*, he remarked:

> Man kann vielleicht [...] behaupten, dass eine jede extensionale [Logik eine intensionale] voraussetzt, denn die Konstruktion der Wahrheitsfunktionen setzt voraus, dass die atomaren Sätze nicht völlig sinnlos sind, sondern wenigsten so viel Sinn haben, dass man voneinander und von ihren Negation unterscheiden kann. [...] In diesem Sinne ist also die intensionale Logik fundamentaler als die extensionale, und es scheint verfehlt, die Logik rein extensional aufbauen zu wollen, wie es in „Principia Mathematica" versucht ist.[22]

However, by the mid-1930s, Jørgensen had changed his mind, both on the role of empirical psychology and linguistics in connection with logic's theory of inference, and also on his assertion in *Treatise* that the meaning of the symbols

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20 J. Jørgensen, *Treatise*, op.cit., Vol. 3, p.207.

21 Ibid., p.145.

22 J. Jørgensen, "Über die Ziele und Probleme der Logistik", in *Erkenntnis*, Vol. 3, 1932, p.93. Jørgensen himself attributed special weight to the bit inserted in brackets.

determines the rules for their use. Five years after the publication of *Treatise*, he gave a series of lectures in which he briefly introduced the development of formal logic, as described both historically and systematically in the work, supplemented with an account of further developments since 1925. In accordance with Carnap in particular, he was now of the opinion that:

> The most important result of general significance which the most recent logical studies have brought us are [...] probably proving that in logic itself, this theoretical stronghold of absolutism, there is an extensive system of conventional factors, which to some degree or other can be changed arbitrarily. [...] Different logical games [i.e. formal systems with fixed rules for formation and transformation] are possible, and there is no particular compulsion to choose between them. However, if you want to play a particular one of them, then you have to observe its rules – otherwise it just is not the appropriate game you are playing, even though the pieces perhaps look the same. It is, you see, not the pieces but the rules of the game that define the game – both *that* on the whole it is a game, and *which* game it is.[23]

Any given system of logic can be compared to a board game like chess. The individual symbols are pieces whose movements are bound by rules; the axioms are the pieces' starting positions; and there are rules, so it is always possible to decide whether a position has been achieved in the correct manner. Following the latest developments in logic, and especially as a result of the impact of the opinions that characterised 1930s philosophy of logic, Jørgensen became a formalist and abandoned the idea that an intentional logic had to form the basis for an extensional logic. "The suggestions put forward for an intentional logic," he now wrote, "all suffer from the defect that they operate with highly uncertain and vague *concept of meanings*, which despite many efforts hitherto nobody has yet clarified."[24] He is referring here to Carnap, who had argued that when logic is asked to do its job, i.e. identify criteria for when a statement follows logically from one or more other statements, it is unnecessary to include the meaning of these statements, and who had therefore concluded: "A special logic of meaning is superfluous; 'non-formal logic' is a *contradictio in adjecto*. Logic is syntax."[25]

Jørgensen also expressed support for significant elements of logical positivism in a lecture entitled "Die logischen Grundlagen der Wissenschaften", which he gave at the Eighth International Congress for Philosophy hold in Prague in 1934.[26] In it, he described any given science as an orderly string of sentences in

23 J. Jørgensen, *Træk af Deduktionsteoriens Udvikling i den nyere Tid*, *Festskrift udgivet af Københavns Universitet i Anledning af Universitetets Aarsfest, November 1937,* Copenhagen: The University of Copenhagen, 1937, pp. 116–117.

24 Ibid., p. 102.

25 R. Carnap, *The Logical Syntax of Language* (1937, German-language edition, Wien 1934), London: Routledge & Kegan Paul, 1959, p.259.

26 J. Jørgensen, "Die logischen Grundlagen der Wissenschaften" in: *Actes du Huitiéme Congrés International de Philosophie, Prague 2–7 Septembre 1934*, Prag: Orbis, S.A., 1936, pp. 100–116. The lecture was also published in 1935 in Danish with the title

which logical entailment is the relation responsible for their order. Ideally, a science is an axiomatised theory that consists of some improvable basic principles and a number of consequences derived from them. The logical basis for a science therefore consists partly of its basic principles and partly of the applied rules for derivation, all of which are derived from logic and are therefore tautological. If the presupposed basic principles are to say anything about actual reality, they cannot be tautologies, but must be general and verifiable hypotheses about the nature of specific, empirically accessible objects. Human knowledge is therefore either tautological, i.e. *a priori*, or empirical.

At this point in time, Jørgensen's view of the nature of logic coincided with the official stance of logical positivism, i.e. that the formal sciences can be developed independently of empirical knowledge, but say nothing of the existing reality, whereas the real sciences are empirically based and have a real content.[27]

However, in 1939, at the Fifth International Congress for the Unity of Science, he returned to the view of the nature of logic that he had previously expressed in a more imprecise manner in *Treatise*. The lecture was published two years later.[28]

The background for Jørgensen's reflections was his view that both logic and mathematics can be considered as languages, and therefore that, just like other languages, they must be seen as empirically existing phenomena and as special types of human behaviour. "Logic and mathematics are thus transformed into empirical sciences about some special features of the psychological phenomena which are commonly called 'thinking'."[29] "Thinking" consists of manipulating concepts, and the more we observe, experiment with, talk, listen and read about what our concepts stand for (i.e. their objects), the more complete our concepts become, and the more capable we are of dealing with what they stand for. In order to stabilise thinking, words are introduced, whose meaning is the content of the concepts for which they stand, and which are formed on the basis of perceptions. Again, this means that the meaning of a word cannot in the final instance be learned with the help of verbal explanations and definitions, but only on the basis of direct observation, which is, Jørgensen thought, empiricism's basic principle.

Words form parts of sentences, and sentences function as names for states of affairs. Therefore, it can be said that an entailment exists between two names, N_1 and N_2 – both of which are names of states of affairs – if they are names of one

"Videnskabernes logiske Grundlag" in: *Festskrift tillägnad Axel Herrlin*, Lund: Carl Bloms Boktryckeri, 1935, pp. 20–37.

27 See for example *Wissenschaftliche Weltauffassung der Wiener Kreis*, Wien: Artur Wolf, 1929, pp. 20–24.

28 J. Jørgensen, "Empiricism and Unity of Science", in *The Journal of Unified Science (Erkenntnis)*, Vol. 9, 1941, pp. 181–188; also in: *Danish Yearbook of Philosophy*, Vol. 6, 1969, pp. 108-114. A more complete account of the points of view Jørgensen expressed here is found in his article "Reflexions on Logic and Language", in: *The Journal of Unified Science (Erkenntnis)*, Vol. 8, 1939/40, pp. 218–228.

29 *Danish Yearbook of Philosophy*, Vol. 6, 1969, p. 110.

and the same state of affairs, or if N_2 is the name of part of the state of affairs that N_1 is the name of. However, whether this is the case depends on the actual use of language, and Jørgensen thought that this could only be determined through empirical exploration and analysis of the language used. Accordingly, logic becomes an empirical science. Furthermore, as linguistic behaviour can be explored by scientific methods, logic therefore also becomes a natural science. Thus, Jørgensen abolished the traditional – and, for logical positivism, basic – differentiation between the formal and real sciences. Only once this was done could all the sciences be said to constitute a unity.

There is no doubt that Jørgensen gradually came to consider the idea of unified science to be the most important of logical positivism's theses. He distanced himself from its conception of the nature of logic, and his further development showed that he gradually dropped the central idea that, despite major differences, held the movement's supporters together – i.e. that philosophy is not a set of propositions, but a logical, analytical activity, the object of which is the language of science. For example, in a major textbook on psychology written during World War II,[30] Jørgensen attempted to solve the problem of other minds – i.e. the problem of the basis upon which we attribute consciousness to other people – with the help of a psychological and, in particular, developmental psychology analysis, combined with conceptual analysis.

The philosophy of logic, which makes up a significant part of Jørgensen's original contribution to philosophy, is the only area in which it can be proven that discussions within logical positivism had an influence, however short-lived, on his philosophical thinking. The thought of a metaphysics-free scientific philosophy and the thesis of the unity of science – both significant elements of logical positivism – had been an integrated part of Jørgensen's view of philosophy since the 1920s and remained so throughout his life. Jørgensen's support for logical positivism in the 1930s was due to the fact that the philosophers and scientists in the circle agreed with him on these two significant points.

Jørgensen also shared the socialist outlook that characterised several of the Vienna circle members, including Carnap and Neurath. Although never a member of the Danish Communist Party, he supported world communism in word and deed, and was a great admirer of Stalin. However, he was never a dialectical materialist, and was criticised by Danish Communist Party ideologues for his "neopositivism" and subjective idealism.[31] The nearest he came to a making a concession to Marxism was a noncommittal hint that:

> Perhaps the Hegelian–Marxist "dialectic", which in reality is a continuation of certain observations by Aristotle, contains a vague beginning of a comprehensive expansion of logic.[32]

30 J. Jørgensen, *Psykologi paa biologisk grundlag*, Copenhagen: Munksgaard, 1942-1945, revised edition, 1957.

31 See C.H. Koch, *Dansk filosofi i positivismens tidsalder*, op.cit., p.218.

32 J. Jørgensen, *Indledning til logikken og metodelæren*, København: Munksgaard, 1956

A. J. Ayer is therefore completely mistaken when, in his 1959 review of the history of logical positivism and its main points of view, wrote that "[Jørgensen's] positivism has been modified by an injection of Marxism."[33]

IV

From 1675 until 1971, anybody who intended to sit an exam at a Danish university first had to take an introductory test in philosophy, the *examen philosophicum* – or, as it was called, the "filosofikum". Since the late 19th century, psychology had made up a significant part of the material for the exam. In addition, elementary classical logic was taught, as were the main features of the history of philosophy since the Renaissance. When at the age of 32, Jørgensen was appointed professor of philosophy at the University of Copenhagen, which in those days was the only university in Denmark, the first thing he did was to completely reform the content of the teaching. The old textbooks were swept away, and during his first year as professor a new textbook was published sheet by sheet as the teaching progressed. In 1927, the whole work was brought together in a volume of almost 600 pages, entitled *Filosofiske Forelæsninge* (*Philosophical Lectures*).

Jørgensen opened the volume by defining philosophy as a science that actively deals with as yet unsolved problems regarding nature and human conditions. Greek philosophy originally encompassed all problems of this nature, but the solutions to a number of these problems established starting points for the formation of the special sciences. Sociology was only separated from philosophy relatively recently, followed by psychology and logic. What remains are problems of natural philosophy or metaphysics, ethical and aesthetic problems, epistemological problems corresponding to the philosophical disciplines of natural philosophy, moral philosophy and philosophical aesthetics, and, of course, epistemology itself, which Jørgensen considered to be the basic discipline.

This characterisation of philosophy is traditional. What is more unconventional is the fact that in his lectures Jørgensen supplied the materials that he thought philosophy should deal with. He used more than 400 of the book's 560 pages to discuss the main characteristics, history and current status of the special sciences – firstly mathematics and logic; then physics, chemistry, biology, psychology, and sociology; and finally cultural sciences such as history, religion, ethics and linguistics. The work concludes with a study of the history of philosophy since the Renaissance and a 35-page chapter on the main problems of philosophy. In this way, Jørgensen's *Filosofiske Forelæsninger* came to constitute an encyclopedia of the sciences, in which the real sciences' formal tools (i.e. logic and mathematics),

(1942), p.98.

33 "Editor's Introduction" in: A.J. Ayer (ed.), *Logical Positivism,* Glencoe, Illinois: The Free Press, 1959, p.7.

were treated first, followed by a review of the distinctive features of the history of the real sciences and their contemporary status.

A considerably reworked but unfinished version of the lectures was published in two parts in 1935 and 1939.[34] It lacked a treatment of sociology and the cultural sciences, as well as the history of philosophy section and the closing section about philosophy's problems. The most crucial change from the first version was that the treatment of psychology increased from 87 pages to 237.

Jørgensen's two philosophy colleagues, each of whom was responsible for their part of the introductory course, were mainly interested in psychology, as is evident in the material they covered. The increase in the size of the psychology section in the second edition of *Filosofiske Forelæsninger* should therefore be understood against this background. However, Jørgensen gave up on his attempt to complete the reworking. One reason may possibly have been that, since its publication in 1926, *Filosofiske Forelæsninger* had been repeatedly criticised for being too difficult for the students to understand. Instead, he wrote a major textbook on psychology during the war years, which, along with the logic and methodology sections of *Filosofiske Forelæsninger*, was to constitute his preferred material up until his retirement.[35] In his 1964 autobiography he described the psychology textbook as his main philosophical work, adding that this was a fact, which "many readers have probably not discovered."[36]

Jørgensen's choice of materials for the first edition of *Filosofiske Forelæsninger* implies that philosophy is an analytical activity that cannot actively deal with airy metaphysical constructions, only with scientifically verifiable materials. In his own direct manner, Jørgensen promised his listeners that his course would be "chemically cleansed of any type of 'philosophical' humbug."[37] Jørgensen wanted to lecture only on scientific philosophy.

In his systematic account of the philosophical disciplines, Jørgensen had identified metaphysics with philosophy of nature. Traditionally, metaphysics is a speculatively designed account of existence as a whole, of its nature and its general characteristics, such as being. However, Jørgensen asserted that there is no reason to differentiate between existence as a whole and nature as a whole. Everything is nature, and is therefore the object of the real sciences' empirically based exploration. Natural philosophy had thereby taken over metaphysics' traditional role, i.e. forming theories for existence as a whole.

In his philosophical lectures from 1926, Jørgensen was no less critical of traditional metaphysics than were the later logical positivists. On request, in a 1937 letter to Neurath, he recounted the central themes in his introductory lec-

34 J. Jørgensen, *Filosofiske Forelæsninger*, Vol. 1–2, København: Levin and Munksgaard 1935 and 1939.

35 J. Jørgensen, *Psykologi paa biologisk grundlag*, op.cit.; J. Jørgensen, *Indledning til logikken og metodelæren*, op. cit.

36 *Festskrift udgivet af Københavns Universitet 1966*, op.cit., p.146.

37 J. Jørgensen, *Filosofiske Forelæsninger*, 1. ed., op.cit., p.10.

tures. The scientific view of the world, he says, is developed in the lectures as a continually progressing critique of primitive, mystical and speculative concepts and colloquialisms, with due deference to ever more exact and more verifiable experiences through a logical clarification of concepts: "Deshalb ist das ganze Darstellungsweise anti-spekulativ, anti-mystisch, anti-religiös, anti-antropomorphistisch – oder positiv: logisch-rationalistisch (im guten Sinne) empiristisch, naturalistisch, kritisch."[38]

According to Jørgensen, the real sciences are unified in the sense that they are all based on experience, and also that the same scientific methods are used throughout. In the German cultural science tradition, Dilthey differentiated sharply between natural sciences on the one hand and cultural sciences on the other. Where the natural sciences use experiments, the cultural sciences are based on empathy, or "Einfühlung". Where the natural sciences actively deal with recurring phenomena and can therefore posit general regularities, the cultural sciences deal with the unique. However, Jørgensen thought that the difference between both the sciences and their respective subject areas is relative and not absolute. The natural sciences explore natural phenomena, the cultural sciences explore cultural phenomena; the former more usually occur in several instances that resemble one another, while the latter are more individuated and complex, but both types of phenomena are explored empirically. Consequently, the difference between the natural and the cultural sciences is only relative.

The unity of the sciences, as envisaged by Jørgensen, is a methodological unity. As previously mentioned, he attempted, within the frameworks of his empiricism, to unite the formal and the real sciences. The unity he sought did not therefore consist of some kind of reductionism. Jørgensen's lectures were an encyclopedia of the sciences based on the idea of the methodological unity of science.

Since the early 1930s, Neurath, more than any other German and Austrian logical positivist, had been heavily involved with the idea of such an encyclopedia,[39] and as mentioned previously it was decided at the congress in Paris to fund the publication of an *International Encyclopedia of Unified Science*. The work started to materialise in 1938, but never achieved the scope originally envisaged by Neurath, i.e. 26 volumes, consisting of a total of 260 monographs.[40] Only the first two volumes were published, with the subtitle "Foundations of the Unity of Science".

Against this background, it was natural that Neurath considered Jørgensen's lectures to be a type of precursor to his own great project. In a 1938 article, he enthusiastically mentioned Jørgensen's book from 1926, and ended with the words:

38 Draft of letter of 31 May 1937 from Jørgensen to Neurath, in "Jørgen Jørgensens Papirer", see note 5.

39 See, for example, McGuinness (ed.), *Unified Science*, op.cit., pp. xviii–xxi and D. Zola, *Reflexive Epistemology. The Philosophical Legacy of Otto Neurath*, Dordrecht/Boston/London: Kluwer, 1989, pp.83–106.

40 McGuinness (ed.), *Unified Science*, op.cit., p. xix.

Jørgensen is a robust empiricist, but he knows also very well the power of ratiocination within the logical framework of theoretical constructions. We may call his attitude [...] "Empirical Rationalism" the counterpart to "A priori Rationalism" [...] The term "Empirical Rationalism" may be used synonymously with the term "Logical Empiricism". Jørgensen emphasises that all the complicated and most important scientific theorising *starts* with the experience and language of our daily life, that we also have to *test* all the theoretical results of all the sciences by means of the same aids. Jørgensen gives in his lectures not only a program of the Unity of Science but he also shows this Unity as an actuality.[41]

In this way, Jørgensen taught students "the grammar of science" by illuminating how the leading scientists discovered and utilised the fundamental ideas and why changes were unavoidable.[42] In using the expression "the grammar of science", Neurath had acknowledged his link with Ernst Mach and Karl Pearson.

V

In the group of philosophers and scientists who, despite major differences of opinion, gathered under the banner of "logical positivists" or "logical empiricists", Jørgensen found the same empirical and anti-metaphysical stance, the same striving after scientific philosophy, and the same view of the unity of science that he himself had espoused in the 1920s following his break with neo-Kantianism. Despite this break, Jørgensen retained some basic features of the Marburger School's philosophy, i.e. the view of the development of science as a continuous – but necessarily unfinished – pursuit of truth, and that philosophy is philosophy of science. Only on a few points, namely in connection with deliberations about the nature of logic, did his encounter with logical positivism lead to a change of his basic points of view – and these changes were only short-term. There is no doubt that Jørgensen got on exceedingly well with many who gathered under the banner "logical positivists" in the 1930s, but Jørgensen was no more an orthodox logical positivist than were the others he met at the movement's congresses. Such a thing only ever existed in the minds of the critics.

Fredensvej 47
DK 2970 Hørsholm
Denmark
chk@koch-online.dk

41 O. Neurath, "Encyclopaedism as a Pedagogical Aim: A Danish Approach", op.cit., p. 492.

42 Ibid., p. 487.

Thomas Mormann

The Debate on *Begriffstheorie* between Cassirer, Marc-Wogau – and Schlick

0. Introduction

The aim of this paper is to reconstruct a peculiar debate between Ernst Cassirer and the Swedish philosopher Konrad Marc-Wogau on *Begriffstheorie* that took place in the late thirties of the 20th century. This debate may be conceived as sort of ersatz of the discussion between Cassirer's Neokantian *Begriffstheorie* on the one hand, and logical empiricist accounts on the other, in particular Schlick's *Begriffstheorie* as presented in his *Allgemeine Erkenntnislehre*.[1] Although Schlick did not participate *in personam* in the discussion that took place between Cassirer and Marc-Wogau, one may consider him as a "virtual" participant of the debate, since his *Begriffstheorie* played an important role in the background, in particular for Cassirer. More precisely, I'd like to show that the debate can be read as a dispute on the feasibility of a "rich" account of *Begriffstheorie*, favored by Cassirer, and the necessity of restricting *Begriffstheorie* to an "austere" approach whose protagonists were Marc-Wogau and Schlick, although in a quite different ways.

More generally, the debate on *Begriffstheorie* exemplifies the complex interactions – and non-interactions – between three important currents of scientific philosophy, namely, the Marburg Neokantianism of Cassirer, the scientifically minded philosophers of the Uppsala School, and, indirectly, the Logical Empiricism of the Vienna Circle.

To set the stage, first let us recall briefly some biographical details of the protagonists. After National Socialism had come to power in January 1933, Cassirer left Germany in April of the same year. First he went to England, in 1934 he settled down in Uppsala. When in 1941 a German invasion of Sweden seemed imminent, he went to the U.S. where he lived until his death in April 1945. Konrad Marc-Wogau (1902 – 1991) was Professor of Philosophy in Uppsala from 1946 till his retirement in 1968. During Cassirer's stay in Sweden he and Cassirer were engaged in a lively debate that mainly took place in the then newly founded journal *Theoria*. From 1936 to 1940 their exchange in *Theoria* comprises at least seven items. Moreover, already in 1936 Marc-Wogau had published the monograph *Inhalt und Umfang des Begriffs* in which he dealt with a variety of *Begriffstheorien*,

1 Another important current of *Begriffstheorie* flourishing in the Vienna Circle was the one put forward by Carnap in the *Aufbau*. For reasons of space I cannot deal with it here.

among them Cassirer's. He found all of them wanting, since they all led to "dialectical", i.e., inconsistent concepts of concepts.

For Cassirer *Begriffstheorie*, i.e., the philosophical theory of the formation of scientific concepts, was not just one philosophical topic among others. Rather, he considered *Begriffstheorie* as a truly central point of philosophy *überhaupt* (cf. Cassirer 1928, 163). Marc-Wogau agreed with Cassirer on the importance of the *Begriffsproblem*. He was well aware of the fact that he did not attack some minor point of Cassirer's approach, but launched an assault against the very center of Cassirer's philosophy. Moreover, he conceived his attack not only as directed against Cassirer's theory, but against the traditional philosophical account of concepts as a whole.

In the background of the debate on *Begriffstheorie* between Cassirer and Marc-Wogau, Schlick and his *Allgemeine Erkenntnislehre* played an important role. Marc-Wogau claimed that Cassirer's *Begriffstheorie* could not survive logical analysis in that it led to an inconsistent notion of the concept. Although in *Inhalt und Umfang* he did not deal with Schlick's account of *Begriffstheorie* as elaborated in *Allgemeine Erkenntnislehre* it transpires from his criticisms of the other theories of concepts treated that he would have judged Schlick's account as "dialectical", i.e., as inconsistent, as well.

On the other hand, Cassirer held Schlick's *Allgemeine Erkenntnislehre* in high esteem as a step in the right direction (cf. Cassirer 1927), but he criticized Schlick in that he took *Begriffe* as merely conventional symbols, laying "stress only on the negative moment of the function of "denoting" and its "conventional" character." According to him, "a sharper analysis of this function discovers immediately another, more positive aspect. (Cassirer 1927, 136). This "more positive aspect" of the symbolic character of scientific concepts he claimed to have unfolded in his constitutive account of *Begriffstheorie* as presented in *Substance and Function* (Cassirer 1910) and later in *The Philosophy of Symbolic Forms* (Cassirer 1923 – 1929). Complementarily, Schlick, in *Allgemeine Erkenntnislehre,* criticized (Neo-) Kantian accounts of *Begriffstheorie* as overstating the power of thinking without mentioning Cassirer by name. He pithily asserted:

> Thinking does not create the relations of reality… [R]eality does not obtain form and regularity first from consciousness; on the contrary, consciousness is only a section cut out of reality. … There are no synthetic judgments *a priori.* (GTK, §40)

For Schlick, *Begriffe* were merely conventional symbols. In this sense, he subscribed to a rather austere version of *Begriffstheorie*. But even this kind of modest theory Marc-Wogau would have blamed as untenable and logically flawed since it got involved in the pernicious dialectics of *Inhalt* and *Umfang*.

The outline of this paper is as follows. In the next two sections we recall the basics of the accounts of *Begriffstheorie* of Schlick and Cassirer. This requires dealing with Helmholtz's theory of concepts in some detail. In section 3 we will

deal with Marc-Wogau's critique of Cassirer's *Begriffstheorie* and Cassirer's counter-critique in some detail. In section 4 I put forward some arguments from modern *Formal Theory of Concepts* which show that Marc-Wogau's objections to Cassirer's *Begriffstheorie* are untenable. This does not mean that Cassirer's rich constitutive account of concepts was without problems, but at least it shows that there is no reason to suspect that every theory of concepts that subscribes to some kind of relation between *Inhalt* and *Umfang* is per se inconsistent. In section 5 we conclude with some general remarks on the complex relations between the Logical Empiricism of the Vienna Circle, the Marburg Neokantianism, and the Uppsala School as they show up in the debate on *Begriffstheorie*.

1. Knowledge as Coordination: Helmholtz and Schlick

The term *Begriff* is probably one of the most vague terms ever-used in philosophy, psychology, and other disciplines (cf. Weitz 1984, Marc-Wogau 1936). One cannot start with a neat and comprehensive definition. In this paper I propose to conceive *Begriffstheorie* as a result of two complementary influences: On the one hand, it may be understood as a result of post-Kantian epistemology, which no longer accepted Kantian "pure intuitions" as an important apriori ingredient for scientific knowledge. On the other hand, *Begriffstheorie* may be seen as a philosophical reaction of the conceptual evolution of the sciences, i.e., it was an attempt of philosophy to come to terms with the new conceptual developments of the sciences, in particular with those of logic, mathematics, the mathematized empirical sciences. Also insights of physiology and psychology that concerned the ways of human conceptualization required the attention of philosophy.

A convenient starting point is Helmholtz's "semiotic" theory of knowledge (cf. Helmholtz 1921). Helmholtz considered himself as a (Neo)Kantian, moreover he was a first-class scientist with an immense expertise in physics, physiology and other disciplines. Helmholtz's epistemology may be characterized as a rather special version of a "scientifically corrected" Kantianism. According to it, on one side there is the world W of Kantian things-in-themselves, on the other side there is the domain S of one's sensations. Things and sensations are correlated to each other in a 1-1-way in such that sensations are to be interpreted as signs of objects:

> Our sensations are precisely effects produced by external causes in our organs, and the manner in which one such effect expresses itself depends, of course, essentially on the type of apparatus which is affected. Insofar as the quality of our sensation gives us information about the peculiarity of the external influence stimulating it, it can pass for a sign – but not for an image. For one requires from an image some sort of similarity with the object imaged: … A sign, however, need not have any type of similarity with which it is a sign for. The relations between the two are so restricted that the same object, taking effect under equal circumstances, produces the same sign, and hence unequal signs always correspond to unequal effects." (Helmholtz 1878, 347)

Knowledge, then, is based on a mapping W⋯⋯f⋯⋯≫S of the world W of things-in-themselves into the domain S of sensations satisfying the requirement $f(x) \neq f(y) \Rightarrow x \neq y$. According to Helmholtz, this weakly "structure-preserving" relation between an outer world and a domain of inner sensations is sufficient to ensure that we are able to know the lawful structure of reality (cf. Helmholtz 1878, 348). In his *Allgemeine Erkenntnislehre* Schlick, in general, faithfully followed Helmholtz's semiotic approach.[2] A point where he deviated from Helmholtz was that he replaced "sensations" by "concepts". The reason was that he considered "sensations" or "mental images" as too vague and undetermined as that they could fulfil the symbolic role that Helmholtz had provided for them. In order to ensure stability and determinateness of our thought, he proposed to replace Helmholtz's sensory images (*Empfindungen*) by "concepts". Concepts were distinguished from images by the fact that they were completely determined and had nothing uncertain about them (cf. GTK, §5, 20). One may ask, how natural minds like ours with their continuously changing sensory images can handle such ideal entities as concepts as Schlick defined them. Schlick offered an answer apparently inspired by Vaihinger's *Philosophie des Als Ob*: Strictly speaking, concepts do not exist, what is important is their functional role:

> We operate with concepts as if they were (sensory) images (*Vorstellungen*) with exactly delineated properties that can always be re-cognized with absolute certainty. Their properties are called the characteristics or features (*Merkmale*) of the concept, and are laid down by means of specific stipulations which in their totality constitute the definition of the concept. In logic, the totality of the characteristics of a concept is called its "intension" (or "content"); the set of objects denoted by the concept is called its "extension.
> …
> Accordingly, a concept plays the role of a sign for all those objects whose properties include all defining characteristics of that concept." (GTK, §5, 20)

For later use it will be expedient to comment briefly on this piece of traditional concept logic to which Schlick subscribes here. In traditional logic a concept has two complementary components: on the one hand, its *Inhalt* (intension), given as the set of its defining characteristics, and on the other hand its *Umfang* ("extension"), given as the set of all objects whose properties include all its defining characteristics. This duality suggests the so-called "law of reciprocity" (cf. Marc-Wogau 1936, 10ff) according to which the following "reciprocity" between the *Inhalt* and the *Umfang* of a concept holds: the larger the *Inhalt* of a concept, the smaller its *Umfang*, and vice versa. This time-honoured "law" of traditional logic

2 In an approving comment on Helmholtz's *The Facts of Perception* Schlick explicitly characterized his *General Theory of Knowledge* as an attempt "to show that forming such a mapping of what is lawlike in the actual, with the help of a sign system, altogether constitutes the essence of all knowledge, and that therefore our cognitive process can only in this way fulfil its task and needs no other method for doing so." (Schlick and Hertz 1921, 166, endnote 15)

appears in various forms in virtually every logical treatise of the 19th and early 20th century. As will be discussed later in more detail, it is the target of Marc-Wogau's incisive criticism put forward in *Inhalt und Umfang*. More precisely, he contended that all accounts of *Begriffstheorie* that hold some version of the reciprocity law were doomed to be inconsistent.

Before we come to this, let us note that the project of defining concepts by characteristic features is threatened by two complementary dangers, either by infinite regress or ending up in some features that lack exact definitions but instead are grounded in some murky empirical intuition that undermined the exact character of concepts so defined. According to Schlick, it was Hilbert's account of implicitly defined concepts that provided a way out of this impasse. According to it the basic concepts of mathematical theories are just defined as entities that satisfy the axioms specified for them. Hence, there seem to be at least some concepts that can be defined in a completely precise and unambiguous way. Let us assume, for the sake of the argument, that we possess concepts in Schlick's sense that are coordinated in a 1-1-way with objects. What is the purpose in coordinating concepts to objects? Schlick's answer in *Allgemeine Erkenntnislehre* is that coordinations enable us to make judgments about objects, and only 1-1-coordinations enable us to make true judgments (cf. GTK, § 10). This answer is unsatisfying in that it still allows a "Lagadonian coordination"[3] of concepts and objects that coordinates each object with one concept in a 1-1-way in some arbitrary fashion. Such a Lagadonian conceptual system would allow us to make true judgments in a trivial manner. In order to exclude such undesired conceptual systems, Schlick hastened to add that the real aim of coordinating objects and concepts is not simply to enable us to formulate true judgments but to get knowledge, which depends on very special coordinations:

> Knowledge is more – much more – than mere truth. Truth requires nothing but uniqueness of coordination; as far as truth is concerned, it does not matter what sign is used for that purpose. Knowledge, on the other hand, means unique coordination with the help of certain definite symbols, namely, those that have already found applications elsewhere. … Hence if we were to coordinate a special sign to each fact and object in the world, we should have nothing but isolated truths, each of which would have to be learned separately. … Our truths would be nothing but discrete points, so to speak; they would not form a coherent system. Yet it is only in such a system that knowledge is possible, since the finding anew of one thing in another presupposes a pervasive interconnection. (GTK, 66, 67, dt. 97)

Thus, an essential point of a Schlickian *Begriffstheorie* would have been to distinguish between "good" and "not so good" conceptual coordinations. This issue, however, remained underdeveloped in *Allgemeine Erkenntnislehre*. Schlick was content to give some vague hints pointing at a sort of Machian thought economy

3 On the philosophical appeal of Lagadonian languages see D. Lewis *On the Plurality of Worlds* (Lewis 1986, p. 145).

by which we could single out "good" parsimonious from "bad" Lagadonian concepts.

Summarizing we may say that Schlick's coordinative account of knowledge is characterized by two complementary features: on the one hand it was based on a rich notion of structured reality that did not only recognize "simple" objects as real, but even the most "theoretical" relations; on the other hand, it ascribed a rather austere role to the conceptualizing activity of the subject: for Schlick, concepts were nothing but conventional 1-1 coordinations that allow easy "syntactical" manipulations. Thereby his account claimed a neat separation between factual and conventional components of knowledge. As we shall see, Cassirer's *Begriffstheorie* pulled in the opposite direction: although based on coordination as well, it emphasized the active role of the conceptualizing subject against that of reality "out there".

2. Conceptual Constitution: Cassirer's *Begriffstheorie*

Cassirer's philosophy of science is concept-oriented *par excellence*: "The theory of the concept becomes a cardinal problem of systematic philosophy. It becomes the nub around which logic, epistemology, philosophy of language and cognitive psychology are rotating" (Cassirer 1928, 163). Since *Substance and Function* (Cassirer 1910) he conceived philosophy of science as a theory of the formation of scientific concepts. His theory was naturalistic in the sense that according to him philosophy should not decree what scientific concepts were and how they worked. Rather, since scientific concepts evolved in the history of science, it was the task of philosophy of science to study this conceptual development of science and to make philosophical sense of it, not to legislate it according to some preconceived philosophical ideas.

The role concepts played in the evolution of scientific knowledge science according to Cassirer's *Begriffstheorie* may be described in telegram style as follows. Scientific knowledge does not cognize objects as ready-made entities. Rather, knowledge is organized objectually in the sense that in the continuous stream of experience invariant relations are fixated. The unity of a concept is not to be found in a fixed group of properties, but in a rule, which lawfully represents the mere diversity of experiences as a sequence of elements. The meaning of a concept depends on the system of concepts in which it occurs. It is not completely determined by one single system, but rather by the continuous series of systems unfolding in the course of history. Scientific concepts and conceptual systems do not yield pictures of reality, rather, they provide guide lines for the conceptualisation of the world. The fundamental concepts of theoretical physics are blueprints for possible experiences. Factual and theoretical components of scientific knowledge cannot be neatly separated. In a scientific theory „real" and „non-real" components

are inextricably interwoven. Not a single concept is confronted with reality but a whole system of concepts. Our experience is always conceptually structured. There is no non-conceptually structured „given". The „given" is an artifact of a bad metaphysics.

The concepts of mathematics and the concepts of the empirical sciences are essentially of the same kind. In a similar way, as the objects of a mathematical theory are constituted by a system of concepts, the objects of an empirical theory are constituted by theory's concepts. With this account of scientific concepts and their role in the ongoing evolution of science Cassirer goes beyond Helmholtz's and Schlick's structural realist accounts of knowledge and coordination. He emphasized the "constitutive" character of symbolic representation:

> … we do not know "objects" as if they were already independently determined and given *as objects*, – but we know *objectively*, by producing certain limitations and by fixating certain permanent elements and connections within the uniform flow of experience. The concept of the object in this sense constitutes no ultimate limit of knowledge, but is rather the fundamental instrument, by which all that has become its permanent possession is expressed and established. The object marks the logical possession of knowledge, and not a dark beyond forever removed from knowledge. "(SF, 303f)

Instead of conceiving knowledge as a structure-preserving map between a world of transcendent things on the one hand and a domain of sensory images (Helmholtz) or conventional symbols (Schlick), for Cassirer knowledge as coordination meant the coordination of different areas or stages of knowledge. This led to a new "internal" account of coordination or representation that described the coordination between thought and reality not as a relation between two ontologically different spheres but as a relation between different areas of knowledge. Thus, if one graphically represents Schlick's and Helmholtz's account by a simple relation $W \dashrightarrow S$ between a world W of transcendent things and a domain of internal symbols S, Cassirer's account could perhaps be represented by an unending chain of conceptualizations evolving in the history of science:

$$\ldots \dashrightarrow C_i \dashrightarrow C_{i+1} \dashrightarrow C_{i+2} \dashrightarrow \ldots$$

Here, the C_i should not be interpreted as "mere conceptualizations". Rather, the C_i are always thought to aim at empirical confirmation and corroboration. In modern terms, they may perhaps be conceived as interpreted models of reality. Thus, a scientific object is never "given as such", independently of all the C_i, it always appears in a lawful conceptual context by which it is constituted. In a similar way as a mathematical object such as a geometrical point cannot be thought outside a geometrical system, an object of physics or of any other science cannot be thought outside its theoretical context to which it belongs. Indeed, Cassirer considered it as the essential task of critical idealist philosophy of science to make clear "that the same foundational syntheses (Grundsynthesen) on which logic and mathematics rest also govern the scientific construction of experiential knowledge …" (Cassirer

1907, p. 44). This contention did not imply that empirical and mathematical objects and concepts are one and the same thing. In *Substance and Function* he pointed out there was an important difference between mathematics and empirical concepts:

> In contrast to the mathematical concept, however, in empirical science the characteristic difference emerges that the construction which within mathematics arrives at a fixed end, remains in principle *incompleteable* within experience. But no matter, how many „strata" of relations we may superimpose on each other, and however close we may come to all particular circumstances of the real process, nevertheless there is always the possibility that some co-operative factor in the total result has not been calculated and will only discovered with the further progress of experimental analysis. Cassirer (1910/1953, p. 254)

In a nutshell, then, for Cassirer the difference between mathematical and empirical concepts resided in the fact that the latter are open ("incompleteable") while the former are closed: the implicit definition of a point in Euclidean geometry fixes the meaning of this concept once and for all. In contrast, the meaning of a concept such as "atom" is never fixed by a single conceptual system. Cassirer claimed that the key concepts of empirical science had a "serial form" ("Reihenform") in that their meaning was not fixed once and for all by a single theoretical framework. Rather, it emerged in a series of theoretical stages in the ongoing evolution of scientific knowledge. Thus concepts comprise two complementary moments: on the one hand they are rules for further investigations, on the other hand they are devices for determining the objects of scientific knowledge. The feasibility of this complex relation of the two components is at stake in the debate between Cassirer and Marc-Wogau.

3. Marc-Wogau's Criticism and Cassirer's Defense

The aim of Marc-Wogau's treatise *Inhalt und Umfang des Begriffs. Beitrag zur Theorie des Begriffs* (1936) was to clarify the essence of the concept of concept. According to him, such a clarification was urgently needed, since virtually all extant accounts of *Begriffstheorie* were fatally flawed. As an expedient starting point for such a clarification he considered the problem of the relation between the *Inhalt* and the *Umfang* of a concept: "It seems to me that the nature of concept can best be clarified at this problem." (Marc-Wogau 1936, 5). He pointed out that in the theory of concepts one easily runs into logical difficulties. For instance, the "concept of concept" (*der Begriff des Begriffs*) immediately leads to well-known paradoxes of a class that contains itself as an element. Hence, in order to avoid such pitfalls one had to be extremely careful in the choice of the basic assumptions on which to build a consistent *Begriffstheorie*. Consequently, Marc-Wogau was prepared to recognize only those "determinations" (*Bestimmtheiten)* as concepts

which were non-contradictory (*widerspruchslos*) or unequivocal (*eindeutig*) and could be grasped by a uniform (*einheitlich*) thought (ibid., 7). Thereby he hoped to exclude inconsistent expressions such as "round square" from the realm of *Begriffstheorie*. Although it is rather plausible not to admit openly contradictory concepts such as "being red and non-red all over at the same time" Marc-Wogau's requirement of uniformity is more tricky, in particular, since he considered non-uniformity as the main source of "dialectical", i.e., inconsistent concepts. According to him, virtually all theories of concepts sinned against the command of uniformity and fell prey to inconsistency. The main entrance door for non-uniformity (and thence inconsistency) was that virtually all theories of concepts subscribed to a correlation between *Inhalt* and *Umfang* of a concept. Marc-Wogau claimed that this correlation could not be grasped in a "uniform thought" and therefore led to an inconsistent "double thought" (*Doppelgedanken*).

He attempted to show that all accounts of *Begriffstheorie* endorsed a very strong version of the reciprocity law according to which the *Inhalt* uniquely determined the *Umfang* and the *Umfang* uniquely determined the *Inhalt*. This claim may well be doubted. For instance, a non-extensional *Begriffstheorie* readily allows for the existence of concepts having the same *Umfang* but different *Inhalte*. Fortunately, we need not go into the details of these quibbles when we wish to grasp the essence of Cassirer's and Marc-Wogau's dispute. Cassirer intended to refute Marc-Wogau principally, i.e., he readily admitted that he did subscribe to a "double thought" approach of concept. But he denied that this led to contradiction. Hence he argued that even if *Inhalt* and *Umfang* determined each other in the strict way that Marc-Wogau assumed, even then this fact would not lead to inconsistency. In the following I want to show that Cassirer was right, even if the argument he presented for this thesis, was less than convincing.

In some sense, Marc-Wogau's arguments against the "double thought" hidden in the standard approaches of *Begriffstheorie* resemble those of the British idealists such as Bradley who claimed that the concept of relation was "unintelligible" and even "contradictory". As Marc-Wogau put it:

> If one relatum of this relation (between *Inhalt* and *Umfang*) is thought, thereby the other is thought as well. Consequently, the relata coincide. If A is to be related to B in such a way that A obtains its determination (or determinateness) only through B, then it is impossible to distinguish between A and B. They coincide." (Theoria 2, 291ff)

Let us call this thesis Marc-Wogau's identity thesis. Cassirer's counter-argument against the identity thesis was to give a counter-example, i.e., he presented a decent, scientifically recognized relation whose relata strictly determined each other but nevertheless were not identical. Thus his strategy was based on the naturalist assumption that it is not the task of philosophy to decree what is possible and what is not possible but to understand the conceptual evolution of the sciences. According to him, there was no reason to assume that "thinking together" *Inhalt* and

Umfang led to contradiction since the conceptual evolution of science had shown that relations whose relata are different but nevertheless strictly determine each other, do not lead to contradictions. In other words, he accused Marc-Wogau of being caught in the trap of some unfounded philosophical prejudice refuted by the conceptual evolution of science.

In order to refute Marc-Wogau's identity thesis Cassirer relied on Schlick's *Allgemeine Erkenntnislehre*, and pointed out that systems of implicitly defined concepts as considered by Schlick refute Marc-Wogau's thesis:

> In an implicitly defined conceptual system there is given a totality of concepts that stand in strict correlation to each other and have no independent content outside this correlation. None of them is meaningful "for itself", each is defined only with respect to the other, or, better said, with respect to the whole system. Nevertheless this mutual dependence cannot be considered as a flaw; rather it lays the foundation for a certain highly characteristic advantage. One cannot say that, due to the fact that none of the system's basic concepts can be explained or used meaningfully outside the system, their meaning disappears or becomes ambiguous. Each has its well-determined place in the system and thereby it distinguishes itself from any other concept of the system. (Cassirer 1938, 226)

He concluded that thereby Marc-Wogau's thesis was "directly refuted" (ibid.).

Even if from a formal point of view Cassirer's argument against Marc-Wogau seems flawless, one may consider it not as fully convincing: firstly, it is an abstract argument in the sense in that it has nothing to do with the specifics of the reciprocity law that correlates *Inhalt* and *Umfang*. It simply gives an example showing that there exist relata that strictly determine each other without being identical. Secondly, the argument based on implicitly defined concepts does not provide any positive evidence for Cassirer's own version of a constitutive *Begriffstheorie*. In the next section I want to show that today we have powerful formal tools that allow one to refute Marc-Wogau's thesis on his own ground. That is to say, there are consistent theories of concepts that satisfy a strong version of the reciprocity law.

4. Formal Theory of Concepts and Adjoint Situations

Cassirer's refutation of Marc-Wogau's identity thesis by invoking implicit definitions may not be considered as fully adequate, since the implicit definition of concepts has nothing to do with the problematic of the relation between *Inhalt* and *Umfang* that occupies centre stage in Marc-Wogau's *Begriffstheorie*. In other words, Cassirer's argument is too general than to be really convincing.

Fortunately, today better and more specific arguments are available to back up Cassirer's arguments against Marc-Wogau. I'd like to mention two different approaches. First, the so-called *Formal Concept Analysis* (FCA) inaugurated in the 1980s by the German mathematician Rudolf Wille and his collaborators. Sec-

ondly, on a more general level, the theory of adjoint situations that belongs to the core of the foundational discipline of category theory founded in the late 1940s by the American mathematicians Saunders Mac Lane and Samuel Eilenberg. Both approaches offer mathematical models of (generalized) concepts that are better suited to refute Marc-Wogau's identity thesis than Cassirer's vague allusion to Schlick's equally vague theory of implicit definitions in *Allgemeine Erkenntnislehre*.

FCA starts with the reciprocity law. A concept is determined by its *extent* ("Umfang") and its *intent* ("Inhalt"). The *extent* consists of all objects belonging to the concepts, while the *intent* is the collection of all attributes shared by the objects. As it is often difficult to list all the objects and usually impossible to list all its attributes, it is natural to work within a specific context in which the sets of objects and attributes are fixed.[4]

Then a context is defined as a triple (G, M, F) where G and M are sets and $F \subseteq G \times M$. The elements of G are called objects, and the elements of M are called attributes. If $(g, m) \in F$ this is to be interpreted as the fact that in M the object g has the attribute m, or, put it differently that the attribute m is instantiated by g. For $A \subseteq G$ and $B \subseteq M$ define

$$I(A) := \{m \in M; \text{ for all } g \in A\ (g, m) \in F\}$$
$$U(B) := \{g \in G; \text{ for all } m \in B\ (g, m) \in F\}$$

Informally, I(A) is the set of attributes common to all the objects in A, and U(B) is the set of objects having all the attributes in B. Denoting the power set of A and B by PA and PB, the operators I and U just defined above may be conceived as mappings

$$PA \dashrightarrow I \dashrightarrow\!\!> PB \quad \text{and} \quad PB \dashrightarrow U \dashrightarrow\!\!> PA$$

These mappings have some interesting properties. For instance, they satisfy the following requirements (cf. Ganter and Wille 1999, chapter 0.4, Definition 16, 11):

(1) $A_1 \subseteq A_2 \Rightarrow I(A_1) \supseteq I(A_2)$
(2) $B_1 \subseteq B_2 \Rightarrow U(A_1) \supseteq U(B_2)$
(3) $A \subseteq U(I(A))$ and $B \subseteq I(U(B))$

The pair (I, U) is called a Galois connection, and the maps I and U are called dually adjoint to each other. As is well known a pair of maps $PA \dashrightarrow I \dashrightarrow\!\!> PB$ and $PB \dashrightarrow U \dashrightarrow\!\!> PA$ is a Galois connection if and only if it satisfies the equivalence

(4) $A \subseteq U(B) \Leftrightarrow B \subseteq I(A)$

Now we are ready to define concepts of a context (G, M, F) as pairs $(A, B) \in PG \times PM$ that are "balanced" in the sense that I(A) = B and U(B) = A. The *Umfang*

4 If this is not done, one runs into difficulties, as is discussed in detail by Marc-Wogau (1936).

of the concept (A, B) is A while its *Inhalt* is B. The set of concepts C(G, M, F) := {(A, B); I(A) = B} has the structure of a complete lattice (cf. Theorem 3, p. 20, Ganter and Wille 1999).

By definition, *Umfang* and *Inhalt* of a concept strictly determine each other. Nevertheless they are different. But given the *Inhalt* I one can calculate the *Umfang* U, and, vice versa, given the *Umfang* U, one can calculate the *Inhalt* I. For A $\in$ PG and B $\in$ PM) one obtains:

$$U(B) = \cup\{A; B \leq I(A)\} \quad \text{and} \quad I(A) = \cup\{B; A \leq U(B)\}$$

In sum, the Galois connection (I, U) neatly disproves Marc-Wogau's identity thesis according to which strict mutual determination implies identity. Moreover, the refuting example is directly concerned with *Inhalt* and *Umfang* as key concepts of *Begriffstheorie*.

By briefly mentioning FCA and the theory of *Galois connections* I only scratched at the surface of what may be characterized as a modern version of traditional *Begriffstheorie*. In this direction much more has to be done in order to find out if traditional *Begriffstheorie* could indeed be fruitfully related to contemporary strands of research in category theory, computer science and cognitive science.

It would be a gross underestimation of the theory of Galois connections to take it just as an abstruse calculus that is useful for some special theory such as FCA. Rather, Galois connections are a very special case of so called Adjoint Situations. Adjoint situations are, according to the assessment of Saunders Mac Lane, one of the founding fathers of category theory, THE fundamental concept of category theory. There is no time to explain this contention in any detail. Be it sufficient just to state that in adjoint situations the rather austere structures PG and PM are replaced by appropriate, much more richly structures categories, and the role of the mappings I and U is taken over by appropriate functors these categories. Then one of the fundamental theorems of category theory, the so-called Adjoint Functor Theorem, ensures that under certain conditions something like a generalization of the reciprocity law holds.

Painting it with a broad brush we may contend that in this way the allegedly obsolete *Begriffstheorie* of the early 20th century, centering on the notorious "law of reciprocity", has found an unexpected comeback in the guise of category theory. The fact that adjoint situations are one of the core concepts of category theory, and the fact that category theory is one of the most successful contemporary foundational theories suggest that even today *Begriffstheorie* may deserve more than mere philosophico-historical interest.

5. Concluding Remarks

Begriffstheorie may be considered as hidden meeting point for a variety of philosophical currents more or less closely related to some sort of "scientific" philoso-

phy, in particular Logical Empiricism, among them Schlick's empirio-criticism of the early 20s, Cassirer's critical idealism, and the logical philosophy of the Uppsala School. More generally, as Marc-Wogau's treatise *Inhalt und* Umfang shows the issue of *Begriffstheorie* was a common ground for the various currents of analytic and continental philosophy that in the following decades became neatly separated. *Begriffstheorie* was a topic where philosophers of quite different orientations met. It exemplifies that once upon a time philosophers, who today are classified as belonging to allegedly quite different traditions, were engaged in discussing similar problems. *Begriffstheorie* shows in particular that it would be a serious distorsion to characterize the continental tradition as anti-logical, and the analytical tradition as pro-logical.

From Marc-Wogau's perspective the theories of concepts put forward by philosophers such as Cassirer, Frege, Husserl, Kant, Rickert, Russell, and others, all appeared to be rather similar, since they all suffered from similar defects. He treated them as united in the common endeavor of elucidating the nature of (scientific) concepts. Implicitly he thereby defied the sharp distinction between continental and analytic philosophy that later became current. This feature of *Begriffstheorie* would have deserved more attention as I could give to it in a short paper like this. Rather, I concentrated on *Begriffstheorie* as a common ground of the more closely related currents of Cassirer's critical idealism and Schlick's early logical Empiricism as presented in his *Allgemeine Erkenntnislehre*.

From Marc-Wogau's rigid conception of logic that did not allow one "to think together" the complementary aspects *Inhalt* and *Umfang*, both Cassirer's and Schlick's accounts of *Begriffstheorie* were untenable. Logically, Marc-Wogau's criticism is refuted by the existence of concepts that mutually determine each other without being identical. Pragmatically, Marc-Wogau's account of *Begriffstheorie* is unacceptable, since it is hard to see how scientific concepts could do the work they are designed to do without assuming the existence of some kind of relation between two components of concepts that more or less resemble the classical aspects of *Inhalt* and *Umfang*. Summing up one may say that Cassirer's *Begriffstheorie* survives Marc-Wogau's assault since his argument against "dual" accounts of concepts is fatally flawed. This is not to say that Cassirer's rich "constitutive" account of *Begriffstheorie* did not suffer from its own problems. But that is another story.

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Department of Logic and Philosophy of Science
University of the Basque Country (UPV/EHU)
P.O. Box 1249
20080 Donostia-San Sebastián
Spain
ylxmomot@sf.ehu.es

Thomas E. Uebel

The Nature and Status of Scientific Metatheory. The Debate between Otto Neurath and Åke Petzäll

Critics from the Nordic countries played a significant role in the development of the philosophies of the Vienna Circle. By the time the first English-language monograph-length critical study of Viennese neopositivism (Weinberg 1936) was published—as well as A. J. Ayer's *Language, Truth and Logic* (1936)—several critical studies by philosophers from Sweden and Finland were already gathering dust: Eino Kaila's *Der logische Neupositivismus* (1930), Åke Petzäll's *Logistischer Positivismus* (1931) and his *Zum Methodenproblem der Erkenntnislehre* (1935).

With their authors having participated in the meetings of the Circle, these early monographs mirror the development of Viennese neopositivism as few other critical monographs do. Kaila focussed on the philosophical methodology and the doctrines adopted in Carnap's *Der logische Aufbau der Welt* (1928). His arguments were discussed in the Circle's meetings and Carnap published a friendly review (1930) in which he nevertheless rejected Kaila's criticisms, in particular of the method of quasi-analysis. Petzäll also seems to have regarded an *Aufbau*-style *Konstitutionssystem* as the ideal type of Viennese neopositivism, but he rather focussed on the difficulties to find a version of the empiricist meaning criterion that not only all members of the Vienna Circle could agree to, but also was able to withstand critical attention. His first monograph reached the sceptical conclusion that verificationism was caught in a paradox.

> It is evident that, if we wish to adhere to that concept of a sentence that follows with necessity from the concept of meaning adopted, then we cannot call the instance of verification a sentence. … The analysis extends beyond language, but that is impossible in principle. (1931, 34)[1]

Focussing on Schlick's rendition of verificationism in "Die Wende der Philosophie" (1930), Petzäll already located a central aspect of the complex of issues that a few years later would be highlighted critically by Neurath's response to Schlick's "Über as Fundament der Erkenntnis" (1934). This was the tension between the finality that the immediacy of phenomenal experience seems to bestow on our interpretation of it and the fallibility that arises from the demand that the content of this experience be communicable and knowledge be of a propositional nature.

1 Translations from sources for which no translation is cited in the bibliography are by the present author.

Petzäll also noted that the notion of logical form as that which was, according to the *Tractatus*, shared by states of affairs and statements about them, and which, according to Schlick, alone made knowledge possible, likewise stood in need of further clarification (ibid., 35-6). Petzäll put his finger on allergic points of the new philosophy very early on.

When he returned to consider the state of Viennese neopositivism four years later in his *Zum Methodenproblem der Erkenntnislehre*, Petzäll was justified to note that

> The subsequent literature has shown that the difficulties within the 'scientific world-conception', which were demonstrated in my work, were working themselves out, already at the time when that little piece was written, in such a way that the splitting apart into different directions was imminent. (1935, 12)

This time Petzäll considered not only Schlick's then latest writings but also those of the physicalist wing of the Vienna Circle. And once again, as we shall see, he reached a negative conclusion. Both wings of the Circle, Petzäll argued, were caught in—as it happened, complementary—contradictions.

Petzäll second monograph is a good example of the difficulties of understanding fully the the efforts underway on the so-called left wing of the Vienna Circle to fashion an entirely new approach to the theory of scientific knowledge. The short debate between Otto Neurath and Petzäll, in the journal *Theoria* in 1936 can serve as our focus here: Neurath reviewed Petzäll's 1935 monograph, calling forth a response by Petzäll which was followed by a rejoinder by Neurath. What renders this debate valuable still today is that in his response to Petzäll, Neurath was forced to clarify the relation between his own naturalist approach to scientific knowledge and Carnap's more formalist logic of science. While Neurath's remarks left several loose ends, what emerged from them nevertheless was the outline programme for a bipartite metatheory of science comprising both formal investigations focussed on the logic of science as well as empirically informed analyses of scientific theorising (data acceptance, theory change etc.).[2]

1. Petzäll's *Zum Methodenproblem der Erkenntnisforschung*

Petzäll's *Zum Methodenproblem der Erkenntnisforschung* represents one of the very first accounts of what we now call the Vienna Circle's "protocol sentence debate" by a non-participant. (In this respect it may be compared with the introductory sections of Hempel's contemporaneous "On the Logical Positivists' Theory of Truth" (1935) which, however, also went on to contribute to that debate.)

2 Here I concentrate on the published Neurath-Petzall exchange and neglect their correspondence.

Petzäll's discussion in section 2 of his monograph covers, in this order, Carnap's "Überwindung der Metaphysik durch logische Analyse der Sprache" (1932a), Neurath's "Physikalismus" (1931) and "Soziologie Physikalismus"(1932a), Carnap's "Die physikalische Sprache als Universalsprache der Wissenschaft" (1932b), Neurath's "Protokollsätze" (1932b), Carnap's "Über Protokollsätze" (1932c) and *Logische Syntax der Sprache* (1934), Schlick's "Über das Fundament der Erkenntnis" and Neurath's "Radikaler Physikalismus und 'wirkliche Welt'" (1935). Petzäll clearly discerned that in the early 1930s Carnap moved from a position sympathetic to the psychologistic reading of the *Tractatus* that was then *en vogue* (with Wittgenstein's blessing) towards Neurath's more radical physicalism and that by 1934 the Circle's "splitting apart into different directions" was irreversible.

In Petzäll's monograph, however, this discussion of the protocol sentence debate, titled "The Problem of Knowledge Presented in a Concrete Example", is sandwiched between a short introductory section, headed "The Anarchy in Epistemology", and a long third section, titled "A Possible Starting Point for the Investigation of Knowledge" in which the author advanced his own suggestions for how the current impasse could be overcome. Petzäll's purpose emerges only gradually in this monograph. When in the first section Petzäll echoed the Vienna Circle's complaint about the current anarchy of philosophical systems and resolves to investigate their epistemological efforts due to their plea for clarity and intelligibility, one senses (as in his earlier book) a certain sympathy with his subjects. Early in section three, however, it becomes evident that Petzäll's own position is by no means so easily defined. Instead, Petzäll effects the stance of a cautious sceptic vis-à-vis epistemology, vaguely reminiscent of the Neo-Friesian Leonard Nelson's "Die Unmöglichkeit der Erkenntnistheorie" (1912). Petzäll claimed that his investigation of the epistemologies of the Vienna Circle demonstrated what holds for epistemology generally, namely, that "the time has not yet come for a *theory* of knowldge" (1935, 51, orig. emphasis).

Neurath's response engaged directly only with Petzäll's critique of physicalism. It must suffice here to note two things about Petzäll's alternative approach. First, it is notable that the issue over which, in Petzäll's view, the Viennese neopositivists came to grief, was no different from that which already afflicted Kant's and all epistemologies since, namely, the tension between genetic or causal inquiry and normative reasoning:

> Kant was unable to conduct his *quaestio juris*-investigation without falling back on the *de facto* obtainment, in the epistemic subject, of the condition that something is valid for somebody. The presupposition of validity as an a priori form in the transcendental apperception becomes a fact of the cognitive process in its actual course. ... But the tension between the descriptive genetic method on the one hand and the investigation of the grounds of validity on the other is not only characteristic for those researches that are based more or less on Kant. The contrast between pragmatism and fictionalism on the one and phenomenology on the other shows the two perspectives in sharp antithesis, but at the same time we

can observe a peculiar fusion of both elements in both of these tendencies. And finally we witness in logical positivism how in Schlick the psychological and physiological genesis of a certain cognition becomes the ground of the validity for the propositional system of the language of science that is amenable to formal analysis alone. (Ibid., 58-9)

For Petzäll, the frequent recurrence of this problem raises the question "whether the two points of view can be separated at all" (ibid., 59).

Yet Petzäll did not rest content with the fact that previous epistemologies were unable to bridge "contrast between genetic explanations and investigations of validity" (ibid., 58). The second point to note is that while he was happy to declare that there did exist a distinct "epistemological question" ("Erkenntnisfrage"), Petzäll conceded that its precise form is not yet known. "Its approximate meaning could be expressed by the question of what relation obtains between logical form and empirical content." (Ibid., 65) However, Petzäll also held that the pursuit of the epistemological question must not beg the sceptical question. Dogmatism in epistemology can be avoided, Petzäll argued, only by adopting a "provisional formula" that as yet avoids any characterization of what is known (and so stays clear of the ancient problem of the criterion): "We would formulate the provisional formula for the investigation of knowledge as follows: *'what is the meaning of validity claims?'*" (Ibid., 69) Investigating the question of the meaning of claims to epistemic validity was to cast light on the previously intractable issue of the relation of descriptive and normative questions and of that of empirical content and logical form.

Petzäll was aware that his diagnosis of the central issue of epistemology recalled and reopened the foundational debates around the previous turn of the century about the mutual relation of logic and psychology.

Our formula states with regard to the role of psychology and logic in the investigation of knowledge, that both methods complement each other in a field of which it is true to say in principle that it belongs neither to psychology nor to logic, but whose specific issues are only done justice to, if the methods of psychology and logic are combined. Claims to validity do not fall under any one of these sciences separately. They can only be comprehended by both together. (Ibid., 83)

Petzäll's diagnosis is not without its own obscurities. Clearly, his intention was to "inaugurate the investigations of all factors that enter as components into that which is in one or another way can be associated with the term 'knowledge'" (ibid., 76). (The distance from logical positivism that Petzäll sought to gain by this procedure, for instance, found expression in his hope that this procedure would "avoid all dogmatic couplings of 'thought' and language" (ibid., 78).) Yet his attempt to cover all bases seems beset by considerable tensions. Being naturalistic in some respects—precisely in wanting to investigate all factors and reject the sharp exclusion of causal considerations from normative investigations—yet traditionalist in others—in wishing not to beg the question of scepticism—it is difficult to see

how all his desiderata could be met. Still, Petzäll's plausible suggestion was this: before a "theory of knowledge" is attempted, we better become clearer about what the "problem of knowledge" is in the first place: to contribute to this alone was the professed point of his "reflections about the question of method in the investigation of knowledge" (ibid., 82-3).

2. Petzäll's Critique of Logical Positivism

Beginning his critique of logical positivism, Petzäll stressed that throughout he aimed for an immanent critique: the failings of the views analysed were to be shown by following the principles professed by the authors under investigation. In particular, Petzäll took very seriously the desideratum that philosophy should not transgress the bounds of language and he ascribed to Carnap and Neurath the aim to avoid the contradiction that he previously (in his first monograph) had diagnosed in Schlick. In doing so he certainly identified problematic points in Carnap's early physicalism.

Noting the central role of protocol statements in the elimination of metaphysics such that only empirical and logical statements remained, Petzäll began by asking of Carnap's position (in "Überwindung") whether the protocol sentences "were regarded as *empirical* statements on account of a formal, logical feature of theirs" (1935, 15, orig. emphasis). Relatedly, he asked of Neurath's position (in "Soziologie im Physikalismus") what distinguished the protocol sentences, especially so-called reality statements, from other statements featuring spatio-temporal determinants. In both cases Petzäll found that no answer was forthcoming and he voiced the suspicion that the relevant distinction could only be drawn by relying on resources that were not officially available: Carnap had limited philosophy to formal inquiries and Neurath had rejected the conception of (correspondence) truth.

Probing further the claim of physicalism that all meaningful statements can be expressed in the language of physics, Petzäll focussed on Carnap's admission (in "Universalsprache") that "in establishing the scientific system there is ... an element of convention, i.e. the form of the system is never completely settled by experience and is always partially determined by conventions" (1932b [1934, 49]). Since Carnap did not specify what these conventions were, the relation of protocol sentences to the other sentences of the scientific system was unclear. Moreover, in answering the question of how it became possible that the qualitative protocols of individuals were translatable into the quantitative language of physics, Carnap was forced, Petzäll argued, to "attribute to the protocol language a quality that has *nothing* to do with its logical nature" (1935, 24, orig. emphasis). Carnap held "that determinations of this kind are theoretically always possible is due to the fortunate circumstance (an empirical fact, not at all necessary in the logical sense) that /the

protocol//the content of experience/ has certain ordinal properties" (1932b [1934, 61]) and that this also holds for "the structural correspondences between /the protocols//the series of experiences/ of the various experimenters" (ibid., 64).[3] Carnap went on to claim that "these facts, though of an empirical nature, are of far wider range than single empirical facts or even specific natural laws. We are concerned here with a perfectly general structural property of experience which is the basis of the possibility of intersensory physics ... and intersubjective physics, respectively" (ibid., 65) Petzäll was surely right to comment that "these lines would fit better into the *Critique of Pure Reason* than where they are. For Kant they would not amount to a radical inconsistency." (1935, 25) He concluded:

> The result is thus reached that in proving the fundamental thesis of physicalism Carnap is forced to apply a procedure which he himself is forced to consider scientifically illegitimate, i.e. that belongs neither to the sciences nor to logic." (Ibid., 26-7)

Matters did not improve, so Petzäll, at the next stage of the debate. Noting Neurath's opposition (in "Protokollsätze") to Carnap's retention up until then of "original" protocol sentences that do not stand in need of justification, Petzäll remarked that "strangely enough" he did not criticise Carnap's "extra-logical speculations about the 'general structural property of experience'" (ibid., 29). Carnap's subsequent embrace (in "Über Protokollsätze") of a still more pronouced conventionalism concerning the language of science, in particular of the characterisation of protocol sentences, was likewise rejected by Petzäll. "If the form of these sentences is arbitrary, then one obviously cannot any longer speak of their special structural constitution." (Ibid., 32) This rendered Carnap's old method A invalid. Petzäll noted correctly that Carnap's new fallibilist method B had no need any more to invoke such an assumption, but there he found, unsurprisingly, that no good reason was provided to stop testing at one point rather than another.

Petzäll summarised the dilemma which he saw facing the physicalists as follows:

> If we limit, as Neurath wants to, the logic of science to the logical synatx of language, then language does not say anything; if we do not want to accept this consequence, then the logic of science must be more than the mere logical syntax of language. (Ibid., 35)

What prompted Petzäll were two things: first, Neurath's claim made in the course of rejecting the correspondence theory of truth that "statements can only be compared with statements" (1931 [1983, 53], 1932a [1983, 66]); second, Carnap's denial in *Logical Syntax* that "all logical investigations comprise two parts: a formal inquiry which is concerned only with the order and syntactical kind of the

3 The expressions on either side of the double forward slash belong to Carnap's formal mode and material mode of speech, respectively (appearing in parallel columns in the original).

linguistic expressions, and an inquiry of a material character, which has to do not merely with the formal design but, over and above that, with questions of meaning and sense" and his claim that "the formal method comprises all logical problems, if conducted sufficiently thoroughly, even the so-called material or meaning problems (as long as they are truly logical and not psychological problems)." (1934 [1937, §73]) Petzäll clearly took these remarks to mean that meaning had no role to play in physicalism and logical syntax, perhaps even that these doctrines denied the phenomena of meaning altogether. The alternative conception that logical syntax was meant to capture all those aspects of meaning that were relevant for epistemology (in parallel to the *Aufbau's* dismissal of the unfortunately named "epistemic content" in favour of "logical content") was not considered—nor that Neurath aimed to replace the theory of truth with a theory of protocol acceptance (as in his 1932b).

Turning to Schlick, Petzäll again found himself confronted with the unsolved problem of properly delimiting the special sentences "which Schlick does not want to call 'protocol sentences' but 'fundamental sentences'" (1935, 39). The "novelty" of Schlick's approach lay for Petzäll "mainly in the heavy stress on the psychological or 'physiological' character or, if you like, on the non-linguistic nature of a certain cognitions" (ibid., 43). Even though he criticised Neurath's response to Schlick as unhelpful, Petzäll remained unimpressed by Schlick's efforts.

Petzäll's overall conclusion was this:

> The debate we have focused upon for detailed consideration thus shows us that it contains an unsolved problem that makes itself felt against all resistance and that due to its own dynamic produces a sharp and principled opposition between epistemologists who jointly reject the previously accepted philosophical questions as pseudo-problems and aim to distance themselves from traditional epistemology. Carnap and Neurath persist against all consequences with trying to get away from the question, without success. Schlick 'solves' it, without success. (Ibid., 47)

Thus we reach the unsolved problem that Petzäll called "the question of knowledge":

> We thus come upon the question of knowledge ... in two guises: in Schlick, in the guise of the question of what the nature of the element of experience is that makes verification absolute; in Carnap and Neurath in the guise of the question of what the relation is between the form and the content of what is said. Both of these guises of the question of knowledge can be summarised in a more general formulation: what is the relation between pure logical form and experience? (Ibid., 50)

With that formulation in hand, Petzäll then turned to develop his alternative approach to the "question of knowledge" (which we briefly surveyed above).

3. Neurath's Response

It would be difficult to imagine Neurath responding to this monograph had it been written by a philosopher in Germany. Instead, his engagement seems to presage Philipp Frank's response to Cassirer's book on determinism two years later in the same journal (1938). The absence of undue polemics in Neurath's response suggests that the point was to build bridges to Cassirer's Göteborg.

Recognising that he and Petzäll were "adherents of different views", Neurath conceded right away that "the rapid development of modern logical empiricism had the effect that there are still all sorts of points in dispute, even within the Vienna Circle" (1936a [1983, 159]). Instead of carefully going through the criticisms point by point, however, Neurath proposed to "elucidate the problem of knowledge as formulated by [Petzäll]" from his own standpoint. His own central point he characterised as follows.

> A radical physicalism—this is to denote the total conception, not only a special tenet—does not lead to a theory of knowledge of its own, as Petzäll demands. If, e.g., within physicalism we use the term 'validity', we deprive it of any 'absolute' meaning and avoid what we call 'pseudo-problems' of the theory of knowledge. We best start from the operation of science and look at its procedure. (Ibid.)

This, as it were, anti-philosophical point of Neurath's stood in clear contrast to Petzäll's distinctly philosophical stance. Petzäll had not announced it as such, but it shines through clearly when he noted about Carnap's intention, largely due to Neurath's influence, to put increasing distance between his and Wittgenstein's conception:

> That he cannot stay within the limits of his own method is not due to his misunderstanding Neurath's opposition, but due to the impossibility in principle for physicalism to justify physicalism with its own methods. (1935, 27)

The force of Petzäll's criticism is clear: physicalism relies for its plausibility on assumptions which it is in no position to substantiate. For Neurath, however, this constituted "no reproach" (1936a [1983, 165]).

What was Neurath's meaning here? It was not that physicalism could make arbitrary assumptions, but that physicalism situates the investigation of knowledge entirely differently from traditional epistemology. The investigation of knowledge was not to be undertaken by a philosophy that aimed for autonomy from empirical science in proving valid its own foundations (as Petzäll seemed to presuppose). Instead, as an investigation of scientific knowledge, it was reflectively undertaken by science itself. For Neurath, the philosophy of science was part of science as its own metatheory.

Importantly, this metatheory came in two parts: what he called "the behaviouristics of scholars" and logical investigations (ibid., 160). Neurath gave the following examples. To the former belong statements like "'The scholars of a certain epoch made experiments, undertook voyages of exploration, formulated statements of a certain kind' or 'Scholars who are under the influence of great amounts of alcohol formulate different statements than scholars who have consumed no alcohol'"; to the latter belong statements like "'This group of statements is of equal content with a second group of of statements of the same language' or 'This statement is in contradiction with other statements within a certain system' or 'From the statements "Homer is a Negro", "all Negroes are poets" follows "Homer is a poet"'"(ibid.). While Neurath here spoke simply of "logic" he clearly meant what Carnap called "logic of science". As can be seen from his examples, Neurath expected the logic of science to deal with issues of logical relations like consistency and entailement and with issues of meaning in so far these did not concern psychology.[4] What Neurath called the "behaviouristics of scholars" I shall call—with Frank (1957, 360)—the "pragmatics of science": it concerned, to begin with, descriptive statements from the history, psychology and sociology of science. But note that for Neurath also "the term 'accept' belongs to behaviouristics. We can think of the mass of statements that we accept as being unified in an encyclopedia." (1936a [1983, 160]). This raises the question of whether and where or how normative questions concerning data and theory acceptance find a place in Neurath's scheme. (I will return to this.)

Now, since both the pragmatics and the logic of science are second-order inquiries it can hardly be demanded of them that they are autonomous in that they can in some sense prove their own presuppositions for their very object, first-order science, which is given to them in the form in which they find it. That Petzäll claimed "the impossibility in principle for physicalism to justify physicalism with its own methods" constituted "no reproach" therefore. But neither can we expect science as a whole, the combination of first- and higher-order inquiries, to be so autonomous, for first-order inquiries of an empirical nature typically depend on what is given to them in experience and experiment. The idea of philosophical self-sufficiency that radical scepticism trades on, simply had to be abandoned.

How then did Neurath propose to treat questions that Petzäll found troubling, for instance, how did he ensure that his collections of accepted statements were of an empirical nature? Neurath's answer was that "the reduction of testing with observation statements—protocol statements—would determine the empiricist character of the encyclopedia" (ibid., 161). But what ensured that protocol statements were of an empirical nature? This was to be answered not by the logic of science alone, but only in conjunction with the pragmatics of science. Very roughly, the logic of science determined protocol statements to contain a certain type of terms

4 Neurath simply disregarded Petzäll's overreaching criticism that logical syntax reduced all statements to tautologies.

the application of which the pragmatics of science showed to be fairly directly responsive to experience. The precise specification of the terms in question depended on the favoured form of observation statements: on this issue there obtained a difference between the physicalists Neurath and Carnap which Neurath chose not to discuss. Here it may be added, therefore, that it is characteristic that Carnap left it for psychology to determine which predicates should be considered observational ones (1936/37), while Neurath insisted on the use of perception terms in the formulation of protocols (1932b). Of course, Neurath's way of reasoning—nowadays we'd call it "naturalistic"—would not have satisfied a philosopher who sought to establish the possibility of knowledge against the threat of radical scepticism. But this was one ambition that neither Neurath nor Carnap shared.

It fits with this outlook that Neurath refused to attach to the term 'valid' an "'absolute' meaning". There are two aspects to this. First, there is Neurath's problematic rejection of any truth talk which he felt "leads to all kinds of difficulties" (1936a [1983, 161]). Believing truth talk somehow inherently to involve a metaphysical conception of correspondence between linguistic entities and the world (for the postulation of such a relation lay beyond empirical control), Neurath resolved to make do without truth and instead only use the concept of acceptance. We may leave open here the question whether this still allowed him to develop a somewhat minimalist epistemology, but it is clear that such fastidiousness precludes the appreciation of standard semantics and all that this entails. (I will not try to defend this strand of Neurath's thinking.)

A second aspect of Neurath's anti-absolutism is that he also rejected talk of "verification" or "falsification", "even of a 'limit' to which confirmation or shaking would approach". ("Shaking" was Neurath's term for disconfirmation.) His reason was that "verification and falsification need as premise the use only of precise terms", whereas "the total encyclopedia with all its observation statements necessarily also contains terms that are just precise enough to be used within certain boundaries" (ibid., 161-2) what elsewhere he called "Ballungen". What shines through here, but again was not designated as such by Neurath, is an important difference in the conception of the "universal language of science". The basic language of a Neurathian encyclopedia was distinct from the language that Carnap had claimed was universal in his "Universalsprache" of 1932, the precise and wholly quantitative language of mathematical physics. Neurath reasoned that, typically, protocol statements contain imprecise terms, so hypothesis testing in science is inevitably afflicted with imprecision. Ultimately this meant that, for Neurath, even protocol sentences were "not absolutely distinguished either by terms or validity" (ibid., 164) and were themselves accepted or rejected in the light of more or less theoretical considerations.

Neurath summed up his alternative conception of investigations into knowledge:

Physicalism uses the concept of 'validity' in a historical sense and with reference to a certain mass of statements, it does not arrive at formulations of 'dignity' … that should somehow lead us to 'the real world', to 'the one true world in itself'. (Ibid., 165)

"Metaphysical dignity" was Petzäll's descriptive term for the distinction that historically had been held to be enjoyed by universal truths (1935, 58); whether Petzäll himself actually hankered after such solutions of the "question of knowledge" is not entirely clear, but Neurath evidently sought to shut this door very firmly:

… one would never get to the confrontation of statement and reality; language and reality; thinking and being; knowledge and reality; subject and object; logical form and experience; etc.—all these are formulations that Petzäll employs as the starting point for considerations of epistemological considerations (cf. his pp. 7, 56, 60, 66)." (1936a, [1983, 163], trans. altered)

There were still other points on which Neurath sought to correct Petzäll—physicalism did not hold that all the statements or laws of the special sciences reduced to statements or laws in physics (ibid., 164) and it did not accept the narrow behaviorism of Watson (ibid.)—but it is fair to say that Neurath rested his case as follows.

Though much may still need clarification, the present state of research gives no cause for the assumption that we need specific terms and specific statements of a separate 'theory of knowledge' besides the statements of science as a whole (including the logical disciplines) for the building up of our science. (Ibid., 166)

Or, as Philipp Frank was to put it some fifteen years later, "the fact that no special science can … 'defend its own principles' does not lead to the conclusion that the system of all sciences cannot do so" (1951, 30). In their different contexts Neurath pointed to the combination of empirical and logical inquiries and Frank to that of the natural and the social sciences; shifting to the distinction between the logic and the pragmatics of science, we could in Neurath's spirit say with Frank that their cooperation "would reach the objectives that that were formerly reserved for philosophy" (ibid.).

4. Petzäll's Reply and Neurath's Rejoinder

Yet does the rejection of correspondence truth and realism not lead to relativism? This is a counter one could expect from a theorist of knowledge with traditional philosophical ambitions, but that was not how Petzäll did react. It seems Petzäll was genuinely surprised by the strategy that Neurath had chosen which he characterised as follows:

> According to Neurath, the task of the so-called theory of knowledge is taken over by two sciences, by the logic of language and the so-called behaviouristics of scholars. (1936 [1983, 166])

Petzäll's concern lay in learning "more about the way in which these two spheres of research participate in the construction of the encyclopedia" (ibid., 167). Since the precise wording of the questions he put to Neurath is less significant than the point of the answers received—Petzäll detected confusions between the logical and the behavioural spheres in certain formulations which Neurath then disambiguated—I turn straight to Neurath's response.

> The question which contradictions can just be tolerated, which not, how one behaves altogether in the development of the whole of science, is a question of behaviouristics, of history of science, of behavioristsics of scholars. But the discussion of contradictions, the discussion of the question which groups of statements are logically of equal content, belongs to the sphere of logic. If I am occupied with the behaviour of people who produce encyclopedias, I am concerned with behavioristics; if I am occupied with the logical interconnecting of the statements themselves, I am not concerned with behavioristics. (Ibid., 169).

As so often, Neurath's own clarifications are not fully self-explanatory. For instance, when Petzäll distinguished what I called the two "branches of metatheory" as "two sciences", he may have had it in mind that both make a different contribution to "the construction of the encyclopedia", one descriptive, the other normative. But precisely concerning the distinction between descriptive and normative inquiries, Neurath's remarks seem to remain studious silent.

On a second look, however, these remarks can also be read as studiously ambiguous. Neurath's quoted sentences can be read as themselves concerned with descriptive or with normative questions in mind: "can be tolerated", "how one behaves", "being occupied with"—of all of them we can ask "in what sense"? Moreover, this holds not only with regard to the questions in the pragmatics of science, but also in the logic of science itself given that "the" logic is no longer given and that therefore any descriptive judgement about the logical relation of two statements is dependent on the prior decision of which logic was or is to be adopted. Lastly, it must be recalled that for Neurath the distinction between descriptive and normative questions was not a fundamental and categorical one—given that kind of normativity was concerned that he recognised (as opposed to one he rejected as metaphysical). To be sure, Neurath had no time for deontological norms of a Kantian variety, but he agreed with Max Weber that it remained within the purview of scientific investigations to determine instrumental norms. These depended, after all, on observable means-ends relations: given a stated end, it was an empirical matter what type of course of action was more likely to lead to success than another. Such instrumental normativity Neurath had explicictly defended as legitimate in "Soziologie im Physikalismus" by comparing their status

to recipes for cooking, for instance, and it was this defense that he now happily fell back on.

So questions of instrumental normativity were not categorically distinguished from descriptive matters for Neurath and raised no special problems. Specify the parameters of which ends are pursued under what circumstances and descriptive inquiries will deliver answers to "which contradictions can just be tolerated, which not" in this normative sense. Just as the logic of science holds both a descriptive and a normative office—it describes what follows from what, given the rules of the logico-linguistic system under investigation, and so prescribes what someone who has adopted this system should deduce—so the pragmatics of science has both a descriptive and normative office: it can describe what are appropriate methodological means for given cognitive ends and thereby legitimate their adoption for these purposes. In a fuller discussion Neurath would perhaps have addressed this matter explicitly as well, but in this rejoinder he kept matters short.

In retrospect, of course, Neurath can be blamed for a certain blindness that also affected Carnap at the time: instrumental normativity is not as plain a concept as they apparently took it to be. Moreover, whether the aim of scientific theories should be successful prediction—"an occupation with predictions and their testing" is what "logical empiricism leads to" (ibid., 170)—or not is debatable. (Does the idea of empirical adequacy capture the nature of the scientific enterprise as a criterion of theory choice?) Yet one can admit this and with Neurath also ask whether such questions require a distinct, philosophical theory of scientific knowledge—or whether they are best understood as discussions within metatheory itself.

5. The Relevance of the Petzäll-Neurath Debate

Given that Neurath's conception of a bipartite metatheory as a replacement for philosophy has been widely disregarded in logical empiricism, it is doubtful whether the debate between Neurath and Petzäll was noted by fellow logical empiricists at all—apart from Hempel and Frank, who also published in *Theoria* in the following two years, and Carnap.

Hempel's paper "De la problème de la vérité", in fact, explicitly referred to this exchange and sought to clarify some issues raised about the notion of truth in the light of Tarski's theory. In doing so, he practically took Neurath's side in the debate with Petzäll, noting that "Tarski's theory [of truth] does not concern the criteria by which the system of the propositions of the empirical sciences is established" (1937 [2000, 54]). In his own work, however, Hempel soon abandoned any concern with the pragmatics of science for work on formal confirmation theory in the logic of science, only to return to the pragmatics of science late in his life (see Friedman 2000).

Frank's contribution to *Theoria* does not explicitly signal his adoption of the bipartite conception of metatheory, though he adopted, like Neurath, Karel Reach's convention of describing metaphysical statements as "isolated" ones (1938 [1949, 173]). But already in 1932 Frank had written:

> The events around Galileo make it clear that the passionate conflicts connected with a physical theory have nothing to do with its suitability to represent natural processes but much more with their relationships to the political and social events of the time. Therefore there is no need to amplify the positivist conception of science by a metaphysical concept of truth but only by a more comprehensive study of the connections that exist between the activity of the invention of theories and the other normal human activities. (1932 [1998, 14])

There Frank clearly suggested adding the sociological dimension to the theory of science that at the time was conducted mostly in terms of the analysis of the symbol system it used (and that in syntactic terms). This of course meant adding something like a behaviouristics of scholars to the logic of science. Frank can thus be seen to have anticipated the distinction which Neurath first drew explicitly in the debate with Petzäll. He can therefore be counted as a supporter of the conception of a bipartite metatheory. Indeed, in the 1950s Frank mainly worked on the pragmatics of science (1951, 1954-6, 1957), but this work was increasingly ignored in mainstream philosophy of science.

But what about Carnap? Here we come to one reason why Neurath may have chosen to respond to Petzäll in the way he did. Not only did Petzäll offer him an opening of sorts by himself suggesting that both causal and normative inquiries pertain to "the epistemological question" against which Neurath could show how this was really to be done, but he also afforded him an opportunity to address an issue that had arisen within the physicalist wing of the Vienna Circle. Just what was the relation between Carnap's purely formal logic of science and Neurath's naturalistic approach to theorising about science? Given that the protocol sentence debate had ended inconclusively with all of Schlick, Carnap and Neurath espousing different conceptions of their favoured form of protocols, it stood to reason to ask even whether Carnap's and Neurath's approaches were still compatible.

Consider what Carnap said about the logic of science as the successor to philosophy in §72 of *Logical Syntax*, appropriately entitled "Philosophy Replaced by the Logic of Science". Carnap began with a basic distinction of types of discourse. (Significantly enough, he allowed himself use of the material mode of speech, not the formal mode of speech which at the time would have constricted him to matters of syntax alone.)

The questions dealt with in any theoretical field ... can roughly be divided into object-questions and logical questions. ... By object-questions are to be understood those that have to do with the objects of the domain under consideration, such as inquiries regarding their properties and relations. The logical questions, on the other hand, do not refer directly to the objects, but to sentences, terms, theories, and so on, which themselves refer to the objects. (1934 [1937, 277])

With the basic distinction between first- and second-order (or generally higher-order) discourses in hand, Carnap turned to consider the proper domain of philosophy.

According to traditional usage, the name 'philosophy' serves as a collective designation for inquiries of very different kinds. Object-questions as well as logical questions are to be found amongst these inquiries. (Ibid., 276-7)

Carnap went on give examples of such supposedly philosophical object-questions. Some of them concerned

suppositious objects which are not to be found in the object domains of the sciences (for instance, the thing-in-itself, the absolute, the transcendental, the objective idea, the ultimate cause of the world, non-being, and such things as values, absolute norms, the categorical imperative, and so on) (ibid., 278),

others concerned "things which likewise occur in the empirical sciences" (ibid.). About the logical questions he remarked that they

occur principally in the logic (including applied logic), and also in the so-called theory of knowledge (or epistemology), where they are, however, for the most part entangled with psychological questions". (Ibid.)

Finally,

the problems of the so-called philosophical foundation of the various sciences ... include both object-questions and logical questions (ibid.)

Carnap's own findings stoods in opposition to this traditional understanding:

The logical analysis of philosophical questions shows them to vary greatly in character. As regards those object-questions whose objects do not occur in the exact sciences, critical analysis has revealed that they are pseudo-problems. The suppositious sentences of metaphysics, of the philosophy of values, of ethics (in so far as it is treated as a normative discipline and not as a psycho-social investigation of facts) are pseudo-sentences; they have no logical content, but are only expressions of feeling which which in their turn stimulate feelings and volitional tendencies on the part of the hearer. In the other departments of philosophy the psychological questions must first of all be eliminated; these belong to psy-

chology, which is one of the empirical sciences, and are to be handled by it with empirical methods. (Ibid.)

So much for idealist metaphysics and its relatives. But Carnap did not stop there.

The remaining questions, that is, in ordinary terminology, questions of logic, of the theory of knowledge (or epistemology), of natural philosophy, of the philosophy of history, etc. are sometimes designated by those who regard metaphysics as unscientific as questionsof scientific philosophy. As usually formulated, these questions are in part logical questions, but I part also object-questions which refer to objects of the special sciences. Philosophical questions, however, according to the view of philosophers, are supposedd to examine such objects as are also investigated by the special sciences from a quite different standpoint, anemly, from a purely philosophical one. (Ibid., 279)

Not surprisingly, Carnap had no patience with this supposedly "philosophical" perspective on object-questions.

As opposed to this, we shall here maintain that all these remaining philosophical questions are logical questions. Even the suppositious object-questions are logical questions in a misleading guise. The supposedly peculiarly philosophical point of view from which the objects of science are to be investigated proves to be illusory, just as, previously, the supposed peculiarly philosophical realm of objects proper to metaphysics disappeared under analysis. (Ibid.)

Thus:

Apart from the questions of the individual sciences, only the questions of the logical analysis of science, of its sentences, terms, concepts, theories, etc., are left as genuine scientific questions. We shall call this complex of questions the *logic of science*. (Ibid, orig. emphasis)

Accordingly, "once philosophy is purified of all unscientific elements, only the logic of science remains" (ibid.). Note that Carnap's analysis here is in fact not disabled by the fact that at the time he possessed a particularly narrow understanding of the logic of science, namely, as "the syntax of the language of science" (as §73 was to argue). His rejection of the traditional understandings of philosophy remained the same once the semantics of the language of science was admitted into the logic of science (as it was soon after).

Now it is clear there is one reading of these passages on which Carnap's delimitation of the successor discipline to traditional philosophy renders problematic the idea of a bipartite metatheory I attributed to Neurath and Frank: *legitimate philosophy comprised only the logic of science, nothing else*. It also seems to me that there is little doubt that Carnap was widely understood in this way and continues to be so. Understood in this way, of course, a sharp contrast opens up between his conception and Neurath's "behaviouristics of scholars". Given, moreover, a

comparison of the exemplary clarity with which Carnap's inquiries proceeded with Neurath's decidely less clear explorations, it is perhaps no surprise that not only did the view that their perspectives were irreconcilable won the day, but that Neurath's perspective was not taken up in the burgeoning movement of logical empiricism.

Yet this is not the only reading possible. Consider what philosophy is qua logic of science: an a priori, not an empirical inquiry. To designate the logic of science as the heir to traditional philosophy is thus to stress the one point of continuity that obtained between them: the presumed fact that philosophy was separate from science and possessed its own distinct methodology. Just this, of course, allows the easy assimilation of Carnap's "logic of science" to Reichenbach's "analysis of science" in his influential *Experience and Prediction* (1938). But it is also one of the differences between Carnap's logic of science and Reichenbach's analysis of science that points to an alternative reading of the relation of Carnap's logic of science to Neurath's naturalistic inquiries. Whereas Reichenbach allowed into the analysis of science not only the problems of logic, probability theory and, importantly, "all the basic problems of traditional epistemology" (1938, 8), Carnap stressed that to designate his logic of science as "theory of epistemology (or epistemology)" is

> not quite unobjectionable, since it misleadingly suggests a resemblance between the problems of of our logic of science and the problems of traditional epistemology; the latter, however, are always permeated by pseudo-concepts and pseudo-questions, and frequently in such a way that their disentanglement is impossible. (1934 [1937, 280])

Thus when Carnap declared that "the logic of science takes the place of the inextricable tangle of problems which is known as philosophy" (ibid., 279), he also announced a much sharper break between traditional philosophy and his logic of science than did Reichenbach for his analysis of science. It is just this difference from Reichenbach that places Carnap back in the company of Neurath.

To see this, note that his logic of science not only possesses, as befits any logic, an a priori methodology—it provides justifications for its pronouncements on the bais of reasoning on a priori grounds—but also that it is clearly designated as a second-order inquiry. Now consider what Carnap added (in square brackets) following his introduction of the designation "logic of science" (quoted above):

> We shall not here employ the expression 'theory of science' ['Wissenschaftslehre']; if it is to be used at all, it is more appropriate to the wider domain of questions which, in addition to the logic of science, includes also the empirical investigation of scientific activity, such as historical, sociological, and, above all, psychological inquiries. (Ibid., 279)

Carnap here recognised as perfectly legitimate other second-order inquiries but noted that these, unlike the logic of science, were of an empirical nature. Moreover, Carnap here grouped both of them together under the heading "theory of sci-

ence", which is fully coextensive with what above I called "scientific metatheory" which likewise divides in logical and empirically based inquiries. I conclude that Carnap's conception is fully commensurable with Neurath's as he outlined it in his response to Petzäll.

Historically speaking, of course, it is true to that Carnap, for his part, stuck with his focus on the logic of science. But mine is not just a reconstruction that is logically compatible with the material but was not as such recognised by the historical actors (as may perhaps still be claimed). In a Festschrift for Frank in 1965 he wrote:

> Frank recognized more clearly, I think, than most other philosophers and scientists that it is of greatest importance that those who work in theoretical fields be aware of the role of their work in the wider context of life, of the life of society and culture. Therefore, Frank, both in his own thought and in his teaching activities, paid close attention to the historical development in order to show how currents of thinking are motivated not only by striving for knowledge but to a great extent also by practical and emotional needs and social situations. He showed that this holds for theoretical work just as much as for work in other fields like art and religion. (1965, xi-xii)

Here Carnap recognised the idea of a bipartite metatheory not only as legitimate but also as useful. Elsewhere he endorsed the bipartite nature of metatheory by acknowledging the importance of the empirical aspects of the theory of science and noted that

> unfortunately a division of labor is necessary, and therefore I am compelled to leave the detailed work in this direction to philosophically interested sociologists and sociologically trained philosophers. (1963, 868)

In other words, despite his own concentration on the formalist logic of science, Carnap recognised the legitimacy and usefulness of the pragmatics of science and the incompleteness of a philosophy of science that only concentrates on the former.

6. Conclusion

I argued that in his debate with Petzäll, Neurath formulated a distinction between two types of metatheory that sought to codify not only conditions of peaceful coexistence, but also of fruitful cooperation between the logical and the empirical perspectives on the study of science. This distinction was already "in the air" but in light of recent developments increasingly needed to be made explicit. Though aiming in a different direction by using the Vienna Circle's protocol sentence debate as a mere foil for his discussion, Petzäll's efforts to delineate an approach to the epistemological question that also sought to find room for both logical and

empirical reasoning provided a welcome occasion for Neurath's intervention that, however, has been widely overlooked.

Let me close with a thought on the question to which my paper leads but which clearly goes far beyond it. The exclusive contraction of what we nowadays once again happily call "philosophy of science" to the formal logic of science—however widely understood (i.e. as also comprising semantics)—has been perceived to be under attack ever since Kuhn. We may note that the failings of, in a word, disembodying scientific thought by de-contextualising it both historically and socially are meant to be remedied precisely by widening the inquiries so as to include what here we called the pragmatics of science. Suppose this correction to be successful or, given its ongoing nature, to have hope of succeeding. What we also need to ask, however, is whether this correction goes far enough. What are we to make of the current fashion to rediscover the metaphysics of science? Can this be understood as a kind of Strawsonian exercise in descriptive metaphysics and then be assimilated to the explicatory project that the later Carnap saw himself himself to be engaged in? Or take the discussions about the choice of parameters with regard to which epistemologies establish their instrumental norms, discussions which Neurath allowed for but did not engage in himself. Could such discussions find a place in the conception of philosophy of science as a bipartite metatheory that we find shared by Carnap, Frank and Neurath? But whatever the answer to these questions, that they arise at all so very closely on the heel of revisiting the debate between Neurath and Ake Petzäll may surely serve as one example of the acuity and fruitfulness of the reception of logical empiricism in Scandinavia and Finland.

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Centre for Philosophy
Department of Government
University of Manchester
Manchester M13 9PL
United Kingdom
thomas.uebel@man.ac.uk

Michael von Boguslawski

Young Ketonen and his Supreme Logical Discovery

Introduction

Oiva Toivo Ketonen was born in Teuva January 21, 1913, into a family that all together raised 13 children. Oiva was child number eight. Based on his unfinished autobiography, there seems to have been much going on in the small village, Perälä, where he grew up.[1] The village saw some action during Ketonen's childhood despite its small size: during 1919–1932, the government passed a prohibition law on alcohol. This naturally led to a lot of illegal smuggling.[2] Perälä connected two important roads in the region, so it became something of a strategic hub for these local bandits. Ketonen still later remembered the village's law-enforcer roaming the roads on a sidecar-equipped Harley-Davidson motorcycle.

During his youth, Ketonen reveals in the autobiography, everyday experiences taught him the reality of life, in many respects. The law-governedness of nature etched itself deeply into his consciousness. "There were strange things, but also they are part of the natural order."[3] He recalls that these experiences proved to be extremely valuable: He noticed, for example, how "narrow-minded and strange conceptions some other students had" regarding theological questions and the individuals relationship with the church.

Ketonen graduated from Kristiinakaupungin Lukio (upper secondary school) in 1932, and enrolled in the Division of History and Philology (where philosophy in Helsinki was taught at that time). The current professor in philosophy in Helsinki was Eino Kaila, who was closely connected with the Vienna Circle. We can read in the autobiography, however, that Ketonen was not quite content with his studies. At that time, philosophy and psychology were not separate subjects, and Ketonen switched his main subject to mathematics. This should not be seen to suggest that he thought less of psychology – indeed, he reveals that Kaila's lectures on the psychology of personality made a deep impression on him. In his memoirs Ketonen also writes that, he suspected that mathematics and the natural sciences would be too "thin", that they would not contain the type of richness that would give life emotional and perspectival content. Ketonen began studying under the mathematician Rolf Nevanlinna, famous for his work on the theory of complex

1 The manuscript for the autobiography was kindly made available to the present writer by Oiva's son Timo Ketonen.

2 Products containing more than two percent alcohol were available only for medical, technical or scientific purposes.

3 Unfinished autobiography.

functions, and we can tell from preserved correspondence that Nevanlinna was extremely impressed by Ketonen's mathematical abilities.[4]

Regarding the teaching of logic at the University, Ketonen notes that the only textbook on logic available in 1932 was Thiodolf Rein's *Muodollinen Logiikka* (*Formal Logic*, my translation), which devotedly followed Aristotelian logic. There was, however a change in the university curriculum, and so new literature was introduced, including Bertrand Russell's *The Problems of Philosophy* and Kaila's *Nykyinen Maailmankäsitys (World Concepts of Today* – my translation*)*. Teaching in logic, Ketonen notes, was confined to the basics, and could not in such a form offer a subject of interest. Ketonen's study book reveals that he did not take a single course in logic.

According to discussions with Timo Ketonen, one of his sons, Oiva found himself interested in algebra and number theory. Ketonen's fellow student, Max Söderman, made Ketonen aware of Gödel's incompleteness theorem, and Nevanlinna later mentioned the theorem.[5] Gödel's fantastic result could be what sparked Ketonen's interest in formal logic. Ketonen began planning his Master's thesis fairly early in his studies, in 1935 at the latest (based on his study diary); this was after only two years of university studies. There is no doubt that the interest in logic was already serious: He writes in his study diary – cleverly entitled *Lahjomaton Tilintekijä* (*The Unbribable Accountant* – my translation) – that "Real mathematics begins with axioms and proceeds to prove from them, in the most direct way possible, the more complex propositions."[6] Ketonen writes in the autobiography that he frequently went to evening meetings of what he called "The philosophical club". These meetings seem to have been quite unofficial, usually the group gathered at the home of one of the professors, e.g. Kaila or prof. Yrjö Reenpää. They also gathered at least once at Söderman's home. In the study diary, we read that he later spent some evenings attending what he calls "mathematical-logical conferences". Where the members of these meetings the same? Logic was of course thoroughly discussed during these meetings, and Ketonen remembers a particular time (possibly May 5, 1936, based on an observation in the study diary) when he presented and defended one of his original ideas which will be expanded below:

In classical Hilbert-style propositional axiomatic logic, one has as the first axiom

$$A \supset (\neg A \supset B)$$

By this axiom, if A, and then from the negation $\neg A$, one can derive an arbitrary proposition B. The last instance in this derivation of B is thus intuitively modus ponens. Adapted to natural deduction it would, after suitable modification and

4 The correspondence between Ketonen and Nevanlinna was, once again very kindly, made available by Timo Ketonen.

5 How well Nevanlinna was acquainted with logic, and what he thought of the new discipline, remains debated.

6 My translation from Finnish.

with the addition of the rule *Ex falso quod libet* in the form of the axiom $\bot \supset B$ and the definition of $\neg A$ as $A \supset \bot$, look like:

$$\dfrac{\dfrac{\bot \supset B \qquad \overset{\vdots}{A \supset \bot} \quad \overset{\vdots}{A}}{B}\ \supset E}{B}\ \supset E$$

Ketonen argued that there is something not quite right with this principle, because, although the derivation is formally correct, in order to correctly use rule $\supset E$ for concluding B, both premisses must be true. But $\bot$ is *never* true. This caused a heated debate during one of the evening gatherings, and Ketonen won over some participants to his side, but that is all that ever came of it, although Ketonen thinks it would have been worth developing.[7]

That Nevanlinna was impressed by Ketonen's mathematical abilities is demonstrated yet again in the study diary. The notes show that Ketonen and Nevanlinna discussed the topic of the Master's thesis repeatedly during the latter's office hours, and that he wanted Ketonen to take up function theory. One can presume that Nevanlinna would not recommend his own field of expertise to a student he did not consider up for the task.

The original plan for the thesis was to write something on pure axiomatics and prove, for example, the fundamental theorem of algebra. This is noted on December 18, 1935. Later, on March 21, 1936, he writes: "The thesis is changing like protoplasm". Roughly a month later – April 18, 1936 – we learn that "I will probably write the thesis on the theory of functions after all". Ten days later, the subject is changed again, this time back towards axiomatics, specifically towards the foundations of mathematics. Nevanlinna commented thus: "Quite a rare subject, since these questions are very scientific, not really intended for a work by a student." Ketonen's first note in the study diary that he has been studying Gödel's famous proof is from the May 4, 1936, 8 p.m. to 9.30 p.m. Two days earlier, he had had discussions with both Kaila and Nevanlinna. Five days later, he has discussed again his master's thesis with Nevanlinna, and his decision to write on axiomatic logic is re-affirmed and final. In the autobiography, he remembers having viewed the work ahead as "extremely interesting". One could speculate that after studying Gödel's proof personally, it made such an impression that it was no longer possible for him to even consider working in another field of mathematics. Formal logic and Gödel's first incompleteness theorem thus became the subject of Ketonen's master thesis. Ultimately then, the choice to take up formal logic seems to have been independent,[8] there is nothing in the study diary along the lines of, say, "After discussions with Mr. X I will take up logic" which one could assume to have been

7 He notes in the autobiography that he suspects that they lacked the necessary logical-philosophical tools at the time, but that later others have written about the subject.

8 Independent in the sense that no one actually suggested the topic to him. One can assume that every professor leaves some mark on his students.

the case had such discussions taken place. The actual writing on the master's thesis *Tutkimuksia Formaalisen Todistamisen Ristiriidattomuudesta* (*Investigations in the Consistency of Formal Proving*, my translation) began in May 1936. To the present writer's (and many others') dismay, the last line in the study diary reads "May 23, 9.00 – 10.00. Work on Master's thesis. See second notebook". No such notebook has been found.

The master's thesis concentrates on two main topics, namely Hilbert-style axiomatic propositional and predicate logic, and arithmetic and Gödel's theorem. It is not known exactly what the thesis looked like, because only part of it has survived in original form. When work on the thesis was finished and graded (Ketonen received the highest possible grade for it, Laudatur), approximately half of the pages were (probably) torn out. The reason for this mutilation was that this first half was going to be published in the *Ajatus* series (yearbook of the Philosophical Society of Finland) but Ketonen apparently wanted to change some passages, and had to alter the order of others, because the observations on Gödel incompleteness that were at the end of the original thesis were included in this published version. From the published version, he omitted the sections on arithmetic. Hence, the published version contained axiomatic propositional and predicate logic, and discussions on Gödel's incompleteness theorem. The original handwritten thesis (the cover and the pages that are left) has survived. When comparing this with the table of contents for the article published in *Ajatus* one quickly spots the differences and gets a picture of what has been changed. The article published in *Ajatus* is titled "Todistusteorian Perusaatteet" – "The Main Ideas of Proof Theory".[9]

Ketonen had received the impression from Nevanlinna that some mathematicians suspected that there was some fault in Gödel's proof, and that this fault might be worth uncovering. Ketonen believes that as a result of this investigative work, he somewhat succeeded in streamlining Gödel's proof. In the autobiography, he laments that he was given the highest grade for the thesis. This might seem odd, but the explanation is sound: Since he was given the highest grade, he thought the work to be 'complete', and so just put it in the bookcase and never gave it a second thought. Had he been given any other grade, he would have reworked the problems, trying to find out what went 'wrong'. He realised later, he writes, that this way of thinking had not been rational. Thus, he may have continued to pursue the task of clarifying Gödel's proof, and develop the ideas that he came to think of during the writing of the thesis.

Ketonen kept himself occupied with Gödel's theorem also after he finished his master's thesis and the subsequent article for *Ajatus*. In 1941, Ketonen made a small improvement to Gödel's completeness theorem for the predicate calculus.[10] Gödel showed that that either a proposition A is provable, or it is impossible that there does not exist a counterexample. Ketonen improved this result so

9 Oiva Ketonen, "Todistusteorian perusaatteet", in: *Ajatus* 9, 1938, pp.28–108.

10 Oiva Ketonen, " Predikaattilogiikan täydellisyydestä", in: *Ajatus* 10, 1941, pp.77–92.

that this counter example can be found directly. Reportedly,[11] Söderman explained Ketonen's result to Gödel in Vienna, who admitted that it was indeed an improvement.

The Dissertation – Untersuchungen zum Prädikatenkalkül Studies in Göttingen

According to his autobiography, Ketonen had decided already in the spring of 1938 to go for a dissertation immediately. He went to the university of Göttingen to study under Gerhard Gentzen, most probably with the aid of Nevanlinna's contacts, who had worked at the University as a visiting professor in 1936–1937. Kaila had met Gentzen in 1936 in Münster. Some letters from Nevanlinna to Ketonen have survived[12] and they show conclusively in how high esteem the former held the latter (this respect of course also held in the other direction). Göttingen's mathematical 'omnipotence' had already somewhat diminished, in particular since several Jewish professors had already been expelled. The atmosphere was very 'mathematic-formalistic'. Morbidly, the very same night that Ketonen arrived in Göttingen, the night between December 9 and 10 in 1938, later became known as the infamous 'Kristallnacht' – 'crystal night', named after the shards of broken glass littering the streets of Germany the next morning after a horrific night of anti-semitist violence. The following remark is found in the 1989-presentation in connection with the subject of the Master's thesis: "[…] I did not for a moment think that I would try to proceed along that road". This is an extremely puzzling remark, since, he did indeed proceed along that road immediately; the *voyage* included visits to Göttingen and Münster, a meeting with Heinrich Scholz, and then studying under Gerhard Gentzen's supervision resulting in the dissertation *Untersuchungen zum Prädikatenkalkül* published in 1944.[13] Why did Ketonen make this remark in 1989 of not having planned to proceed with research on mathematical logic, but then in his autobiography state the complete opposite?

There were of course recognised mathematicians still present in Göttingen, for example C.L. Siegel. Surprisingly however, according to Ketonen, no lectures on mathematical logic were given.[14] Ketonen recalls in the autobiography (my translation):

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11 Jan von Plato, "Ein Leben, ein Werk – Gedanken über das wissenschaftliche Schaffen des finnischen Logikers Oiva Ketonen", in: Rudolf Seising (Ed.), *Form, Zahl, Ordnung: Studien zur Wissenschafts- und Technikgeschichte.* Stuttgart: Franz Steiner Verlag 2004, pp. 427-435.

12 I once again thank Timo Ketonen for providing copies of these letters.

13 Oiva Ketonen, *Untersuchungen zum Prädikatenkalkül*, Annales Acad. Sci. Fenn, Ser. A.I. 23 1944.

14 Note that Hilbert was retired, hence Ketonen's comment that Gentzen was the only logician *at the university*.

There were no lectures in mathematical logic. The field's only representative in the university was Gerhard Gentzen, a sympathetic relatively taciturn young man, who was Hilbert's personal assistant. He told me that his duties consisted mainly of the reading of popular scientific publications to him [Hilbert]. I saw Hilbert once when he was going, walking alone, to the city theatre to watch Cinderella and the Golden Slipper, where I was going myself.

The dissertation contains three parts. The first part presents and improves Gerhard Gentzen's sequent calculus, part two discusses a certain Skolem normal form, and the third part applies the results from parts one and two in order to produce a proof of the underivability of Euclid's parallel postulate from the Skolem-axioms for Euclidean geometry. Ketonen was the first to continue Skolem's work on geometry. The first part will be discussed in detail below.

Propositional Logic

Next, we will briefly discuss the notation for propositional logic and sequent calculus, so that the discussion on Ketonen's result is accessible also to the non-specialist.

We use the *capital Latin letters* A, B, C … to indicate *formulas* (either compound or atomic). We use the *connectives* &, ∨, ⊃, ¬ for conjunction, disjunction, implication, and negation, respectively. Use these and parentheses to form *propositions*, for example.

A&B	$C \vee (D \supset E)$	$\neg (A \vee B)$
'A and B'	'C or D implies E'	'not A or B'

Notice how the parentheses remove the ambiguity of natural language. Consider next the proposition $(A \supset B) \supset (\neg B \supset \neg A)$. It reads 'If A implies B, then B implies not-A'. This proposition is always valid, and we call such propositions *tautologies*. Before we can perform any derivations, however, we require formal *rules of inference*. For this, we introduce a Gentzen-style[15] *sequent calculus*. A *sequent* is of the form

$$A_1, A_2, \ldots, A_n \rightarrow B_1, B_2, \ldots, B_m$$

The formulas to the left of → make up the *antecedent*, those to the right the *succedent.* The formulas in the antecedent can be viewed as *assumptions*, those in the succedent as *possible cases*. Thus, 'A, B → C' reads 'from A and B together, C follows'. The sequent arrow can also be read as 'gives'. Greek capital letters Γ, Δ, Θ,

15 Gerhard Gentzen, "Investigations into Logical Deduction", in: Manfred Szabo (Ed.), *The Collected Papers of Gerhard Gentzen*. Amsterdam, London: North Holland Publishing Company 1969, pp. 68-131. The article was originally published in *Mathematische Zeitschrift* in 1934–1935 and accepted as Dissertation by the university of Göttingen.

… are lists of formulas, and can be interpreted as a *context* for the derivation. The only axiom is the *initial sequent* A → A, which states that from the assumption A, the case A follows. We can think of the sequent as a generalization of the concept of derivability. If we put n=1 in the sequent above, we get the standard case of a single conclusion as in natural deduction. Below are two examples of inference rules, along with an intuitive explanation of how they are applied.

$$\frac{\Gamma \to \Theta, A \quad \Delta \to \Lambda, B}{\Gamma, \Delta \to \Theta, \Lambda, A\&B}\ R\&$$

If something, call it Γ, gives Θ and A as possible cases, and something else, call it Δ, gives Λ and B as possible cases, then Γ and Δ together give Θ, Λ, and A&B as possible cases.

Another example:

$$\frac{A, \Gamma \to \Theta \quad B, \Delta \to \Lambda}{A \vee B, \Gamma, \Delta \to \Theta, \Lambda}\ L\vee$$

When the assumptions A and Γ give Θ and the assumptions B and Δ give Λ, then A∨B together with Γ∨ and Δ will give Θ and Λ as possible cases. The symbols R& and L indicate which rules has been applied.

The inference rules are divided into two groups, *logical rules* and *structural rules*. Roughly, the logical rules are applied on connectives, while the structural rules are applied on the formulas.

Logical Rules for Gentzen's Calculus LK

$$\frac{\Gamma \to \Theta, A \quad \Gamma \to \Theta, B}{\Gamma \to \Theta, A\&B}\ R\&$$

Right conjunction

$$\frac{A, \Gamma \to \Theta \quad B, \Gamma \to \Theta}{A \vee B, \Gamma \to \Theta}\ L\vee$$

Left disjunction

$$\frac{A, \Gamma \to \Theta}{A\&B, \Gamma \to \Theta}\ L\&_1$$

Left conjunction 1

$$\frac{B, \Gamma \to \Theta}{A\&B, \Gamma \to \Theta}\ L\&_2$$

Left conjunction 2

$$\frac{\Gamma \to \Theta, A}{\Gamma \to \Theta, A \vee B}\ R\vee_1$$

Right disjunction 1

$$\frac{\Gamma \to \Theta, B}{\Gamma \to \Theta, A \vee B}\ R\vee_2$$

Right disjunction 2

$$\frac{A, \Gamma \to \Theta}{\Gamma \to \Theta, \neg A}\ R\neg$$

Right negation

$$\frac{\Gamma \to \Theta, A}{\neg A, \Gamma \to \Theta}\ L\neg$$

Left negation

$$\frac{A, \Gamma \to \Theta, B}{\Gamma \to \Theta, A \supset B}\ R\supset$$

Right implication

$$\frac{\Gamma \to \Theta, A \quad B, \Delta \to \Lambda}{A \supset B, \Gamma, \Delta \to \Theta, \Lambda}\ L\supset$$

Left implication

Structural Rules for Gentzen's Calculus LK

$$\frac{\Gamma \to \Theta}{A, \Gamma \to \Theta}\ LW \qquad \frac{\Gamma \to \Theta}{\Gamma \to \Theta, A}\ RW$$

Left weakening *Right weakening*

$$\frac{A, A, \Gamma \to \Theta}{A, \Gamma \to \Theta}\ LC \qquad \frac{\Gamma \to \Theta, A, A}{\Gamma \to \Theta, A}\ RC$$

Left contraction *Right contraction*

$$\frac{\Delta, B, A, \Gamma \to \Theta}{\Delta, A, B, \Gamma \to \Theta}\ LE \qquad \frac{\Gamma \to \Theta, B, A, \Lambda}{\Gamma \to \Theta, A, B, \Lambda}\ RE$$

Left exchange *Right exchange*

$$\frac{\Gamma \to \Theta, B \quad B, \Delta \to \Lambda}{\Gamma, \Delta \to \Theta, \Lambda}\ Cut$$

Cut

In 1943, Ketonen discovered that all rules can be made **invertible**, i.e. such that, if a sequent matches the conclusion of a rule, and if it is derivable, then the corresponding premisses are derivable. Not all of Gentzen's rules are invertible, consider this counterexample. The sequent $A \to A \vee B$ is clearly derivable from the initial sequent $A \to A$. However, if the Gentzen's rule $R\vee_2$ were invertible, it would mean that also the sequent $A \to B$ is derivable. This cannot be: $A \to B$ is not at all an initial sequent if A and B are non-identical atomic formulas.

Gentzen's LK rules for left conjunction and right disjunction are not invertible, and Ketonen chose to simplify the rule for left implication so that it has shared contexts in the premiss. The modified rules receive the following form:

$$\frac{A, B, \Gamma \to \Delta}{A \& B, \Gamma \to \Delta}\ L\& \qquad \frac{\Gamma \to \Delta, A, B}{\Gamma \to \Delta, A \vee B}\ R\vee$$

$$\frac{\Gamma \to \Delta, A \quad B, \Gamma \to \Delta}{A \supset B, \Gamma \to \Delta}\ L\supset$$

Below two proofs of $\to (A \supset B) \supset (\neg B \supset \neg A)$ with Ketonen's invertible rules are given.

Example 1: Proof of $\to (A \supset B) \supset (\neg B \supset \neg A)$

$$\dfrac{\dfrac{\dfrac{\dfrac{A \to A}{\neg B, A \to A}\ LW}{\neg B \to \neg A, A}\ R\neg}{\to \neg B \supset \neg A, A}\ R\supset \qquad \dfrac{\dfrac{\dfrac{B \to B}{B \to B, \neg A}\ RW}{B, \neg B \to \neg A}\ L\neg}{B \to \neg B \supset \neg A}\ R\supset}{\dfrac{A \supset B \to \neg B \supset \neg A}{\to (A \supset B) \supset (\neg B \supset \neg A)}\ R\supset}\ L\supset$$

Example 2: Another proof of $\rightarrow (A \supset B) \supset (\neg B \supset \neg A)$

$$\dfrac{\dfrac{\dfrac{\dfrac{A \rightarrow A}{\neg B, A \rightarrow A}\,LW}{\neg B \rightarrow \neg A, A}\,R\neg \qquad \dfrac{\dfrac{B \rightarrow B}{B \rightarrow B, \neg A}\,RW}{B, \neg B \rightarrow \neg A}\,L\neg}{A \supset B, \neg B \supset \rightarrow A}\,L\supset}{A \supset B \rightarrow \neg B \supset \neg A}\,R\supset}{\rightarrow (A \supset B) \supset (\neg B \supset \neg A)}\,R\supset$$

With the invertible system established, one can now construct the proof of a proposition root-first, beginning from the conclusion (thought of as the root of the proof tree) and applying the rules 'upwards' until one reaches initial sequents. This proof search terminates trivially, because each rule, when applied in reverse direction, reduces the number of connectives in the proposition. Ketonen calls this kind of proof search decomposition.[16] We can now construct proofs mechanically; we don't need to think (that much) about what we are doing when constructing a proof. Another feature is, naturally, that the system permits us to investigate whether a proposition is provable or not. Since this is a terminating process, it can be done by a computer. The computer would have difficulties with Gentzen LK since it would have to 'guess' what is missing in the premiss *qua* the conclusion. The examples above show that the order in which the rules are applied in the decomposition does not matter. The calculus is neither deductive nor reductive, but *deduktionsgleich*.[17] In the thesis, as an example of the application of his invertible sequent calculus, Ketonen applies root-first proof search to axiomatic geometry (based on work by Skolem from the 1920s) in order to show the independence of the parallel postulate. The earliest reference to Ketonen's work internationally is probably in Karl Popper's "New Foundations for Logic" from 1947. Beth, in his work on the tableau method, cites Kleene, but not Ketonen, despite Ketonen's work being relevant (and cited by Kleene).

Ketonen received his Ph.D. in March 1944, in the middle of the bombing of Helsinki. Only the day before, the old part of the main building of the university had been hit, the main hall and the rooms nearby had been badly burnt and so the dissertation was moved to an auditorium on the 'new side' of the university where one could still sense the smoke. There is a peculiar statement in the autobiography concerning the dissertation: "I did not expect much from it, but it appears that someone actually read it". These "someones" included Bernays, Curry, Feys, Kleene etc. In any case, Ketonen's opinion of his dissertation was consistent with that of his master's thesis – he promptly put it in the bookcase. He notes that there were indeed some ideas that could have been developed further (we can read in the introduction to the dissertation that he at least at the time of publication intended to extend his results), but he says that they did no longer interest him.

16 Translated from the German word *Zerlegung*.

17 The conclusion of a rule is derivable if and only if the premiss is derivable

Ketonen's thesis originally became known through Bernays' favourable review from 1945.[18] Arend Heyting also wrote a review of Ketonen's thesis in 1947 for *Mathematical Reviews*.[19] One must wonder, however, whether Heyting studied the thesis thoroughly. The review is three sentences long (or short), and does not at all point out the fact that Ketonen's results amount to significant progress in proof theory, it more or less resembles a table of contents.

There is no evidence that Gentzen would have had any other students except Ketonen. Thus, he was one of the first to work with and extend Gentzen's calculus. Ingebrigt Johansson published a work related to Gentzen in 1937.[20] Kleene notes explicitly in 1952 that he knows of Ketonen's work only through Bernays' review.[21] Curry began using Ketonen's calculus by 1950, and the present writer has seen a letter[22] by Curry to Ketonen dated September 29, 1947, where the former asks for any material Ketonen might have written on logic in any language – "even in Finnish!". Curry reportedly[23] held Ketonen's work to be the best thing in proof theory since Gentzen.[24]

No More Logic? Lost Works

When one reads Ketonen's works published after the dissertation, one notes that no more original logical work is to be found. As stated earlier, Ketonen intended to continue along the logical path, but the plans changed. We will probably never know exactly why. As the story goes, whenever someone later asked him why he shifted his interests away from logic, the reply was "Logic gives me such headache". However, combining bits and pieces from survived correspondence, and notes in the autobiography, we can make some observations regarding what might have been the cause of this headache. First, Ketonen did not at all completely cease with research on logic and mathematics. Apart from giving lectures on mathematical logic in the 1950's and 1960's (attended by several professors), correspondence with his son Jussi Ketonen from the period 1969–1971 reveals that he

18 The review appeared in *The Journal of Symbolic Logic*, Vol. 10, No.4, Dec. 1945, pp.127-130.

19 Heyting's review is known to the writer via the American Mathematical Society's electronic database MathSciNet.

20 Ingebrigt Johansson, "Der Minimalkalkül, ein reduzierter intuitionistischer Formalismus", in: *Compositio Mathematic*, tome 4, 1937, pp.119-136.

21 See Stephen Kleene, "Permutability of inferences in Gentzen's calculi LK and LJ", in: *Memoirs of the American Mathematical Society*, vol. 10, 1952, pp. 1-26. See also (by the same author) *Introduction to Metamathematics*, Noordhoff, Groningen: North-Holland Publishing Co., Amsterdam, 1952

22 The letter is kept in Finland's National Archive in Helsinki.

23 See note 11 above.

24 Ibid.

has been working on the theory of numbers, on Kaila's work on the paradoxes of relativity theory, and on the logical concept of forcing. Especially forcing seems to have caught his interest: He writes that he has come up with some interesting distinctions and concepts, but suspects that they have probably been published elsewhere. We will never know, since none of this work has survived. A brief description of some post-thesis work has survived, however, in letters by Ketonen to Georg Henrik von Wright during the former's stay in the US 1949–1950 on a Rockefeller grant. Ketonen mentioned that he sent two works to Kaila for evaluation, and based on von Wright's expert opinion statement in connection with Ketonen's application for the professorship Kaila must have passed these works on to von Wright: One can compare the description of the works with each other and find that they converge.

What is then treated in these lost works? The works sent to Kaila were titled "On Analytic and *a Priori* Knowledge" and "On the possibility of a three-valued logic". The former comprises, according to von Wright, about 50 pages of material. The first two chapters discuss, in connection with C.I. Lewis' and Rudolf Carnap's work, basic concepts in the theory of meaning such as extension, intension, comprehension and significance. These are then in the third chapter employed to define analytic knowledge. The first three chapters serve as an introduction, the following two are more complex. In these the suggestion is made and argued for, that analytic knowledge is knowledge *a priori* and vice versa. Von Wright applauds the exposition for its comprehensiveness: Although it is a tad rough on the edges as a piece of research, it is most clear and readable due to Ketonen's ability to produce clear and concise formulations. Thus, Ketonen is able to link the work's main subject to related interesting questions such as the new nominalistic approach to knowledge analysis, the subjectivity of the concept of meaning etc.

In a letter dated April 22, 1950, (kept in the National Library in Helsinki) sent from the US, Ketonen makes his own summary of this manuscript: First, one establishes the transfiniteness of the definition of analytic knowledge, the equivalence analytic–*a priori*, that this equivalence is non-constructive and not suitable as a guideline for analysis and does not hold unless one considers meaning as intensional i.e. not valid on its own (context independent). Finally, if meaning is to be restricted to the extensional – the nominalists – then the whole concept of analytic knowledge changes so that the question disappears.

"On the Possibility of a Three-valued Logic" treats, according to von Wright, the works of Lukasiewicz and Post published in the 1920's. Ketonen has produced a commendable presentation of the formal aspects of the structure and interpretation of the calculi, and formulates a condition the calculi must fulfil in order to be applicable. This condition is constructive (exactly in which way 'constructive' is to be read is not made clear in the description), and Ketonen does not comment on the probability to actually realize it in a calculus. We find Ketonen's own description of this work in the same letter mentioned previously. He notes that three-valued logic should be reduced to the 'applicability' or 'non-applicability' of

certain concepts, and that there for this reason should exist some translation of a proposition in a three-valued logic into a two-valued logic, in order for it to be held as true. If it is true, it is not inconsistent. Post's translation, Ketonen notes, does not fulfil this requirement. Furthermore, Ketonen points out that these questions are of such a kind as to be solved *a priori*, so that we can say what it means to apply non-classical to logic to experience.

In the said letter, Ketonen also mentions a 12 page presentation on the philosophical interpretations of scientific disciplines, and, perhaps more interestingly, on the interpretations of consistency proofs. The main point in the work is to show that if logic is understood analytically (non-formally), then consistency proofs say something, namely, the same as all other proofs. Logic is then treated only through the interpretation of expressions and symbols. Ketonen notes that this would perhaps have turned out as a better piece of work, had he only used more pages for it.

One Recovered Manuscript

As discussed above, hints of some later on work on logic are to be found in various places. The present writer was happy to discover a manuscript, comprising 16 pages, titled "Tietomme apriorisista aineksista" ("Our Knowledge of *a Priori* Elements"[25]) in the National Archives in Helsinki. The contents of this manuscript closely resemble von Wright's description of chapters 4 and 5 of the 50-page manuscript "On Analytic and *a Priori* Knowledge" included in the application for the professorship, and thus obviously also match Ketonen's description of the work sent to Kaila from the US. Ketonen writes in the previously discussed letter dated April 22, 1950, that "these things have been lying around for a while" (referring to the work sent to Kaila), so we can assume that they have been written before he travelled to the US. The manuscript is in an extremely unfinished form, written on typewriter but containing several corrections by hand, especially towards the end. The changes and additions are quite clearly indicated however, so quite a high readability is preserved.

Ketonen begins the manuscript with the following question: Is everything that we prove [in mathematical logic] based on our modes of speech, that reflect, cleverly hidden, but without deeper connections, in a sense only by chance the invariance of reality? Or, Ketonen continues, does our knowledge include other, more higher elements, that are necessarily true in all experience, notwithstanding that we cannot at all analyse the nature of this knowledge? [26] These questions are, he notes, as old as philosophy itself. Ketonen proceeds to clarify, with the aid of

25 My translation from Finnish.

26 One of the main proposals of Kaila's logical empiricism, according to Ketonen, is that all *a priori* knowledge is analytic.

proof theory and axiomatic geometry, how one could interpret the equality of the analytic and the *a priori*. He constructs a model of *a priori* knowledge – a simple and idealized world containing only points and lines – with some initial configuration of these. He then invites us to assume that this world is governed by the laws of elementary geometry that allow to add to (or construct from) some arrangement of points and lines new points and intersecting lines. Think of the logical proposition

$$A_1 \& A_2 \& \ldots \& A_k \supset B_1 \vee B_2 \ldots \vee B_l$$

as in the antecedent describing a multitude of possible different initial configurations of points and lines, and in the consequent describing other, possibly more complex, configurations. Assume now that some of the configurations in the consequent are known to us to have been realized in our world. Let us also assume that we select, from the proposition above, those elements from the antecedent that together describe our assumed initial condition, i.e., what is true in our idealized world. Ketonen now gives us a process by which we, applying *all* the axioms on the initial configuration, and subsequently again on the resulting configuration, and so on, exhaustively can examine whether or not some configuration can be constructed from some initial one. If now one of the disjuncts on the right is also realized in our world, the proposition is true. Assume also this to be the case. Now, if we, by Ketonen's construction procedure, can reach from the initial configuration the configuration defined by the true disjunct in the consequent, the knowledge that the disjunct on the right is true is analytic. If the construction process goes on *ad. inf.,* this particular piece of knowledge could be called synthetic *a priori*: At least, Ketonen notes, this model would at least come close to a logical model of such a situation: "It is sufficient for most classical cases". If Kant was correct about the about the parallel postulate, Ketonen continues, it would be impossible for human beings to even imagine non-Euclidean geometry (how could we then imagine, say, a sphere?).[27] The parallel postulate would assume the position of some mysterious "property of nature as a whole".

Ketonen also presents C.I. Lewis' argument for this position, from his 'newest' book at the time, *An Analysis of Knowledge and Valuation* from 1946. It runs, simplified: Assume the concept B not to be deducible from the concept A, but when one experiences A, then B follows. The first mistake here is, then, that the meaning of A is extended to objects in general, which means that the impossibility of – in experience – presenting A together with the opposite of B does not say anything of the connection between A and B. Furthermore, A should be limited to A as 'phenomenon' in order to be *a priori*, in which case it is no longer synthetic since the phenomenon of A includes all relevant conditions in order for A to be identified in experience.

27 The surface of a sphere is an example of a model of non-Euclidean geometry.

We note that this argument goes through in a very simple manner when compared to the somewhat complex formal-logical analysis above. Its emphasis is only on realization in experience, there is no mention of 'finite steps' or 'properties of nature as a whole'. If we reconstruct this example in the form of a logical model, however, this difference can be spotted and we are able to see what makes the proof so simple. The bottom line of the argument seems to be that since there is only one type of knowledge by experience, which includes all 'layers' of knowledge simultaneously, logical distinctions disappear. The "sense meaning" of a concept is based solely on what is true in experience and what is not. Based on the disappearance of these distinctions, one may say that all *a priori* knowledge is analytic.[28] There is, Ketonen notes, one problematic consequence of such reasoning: We will get as result a model which is philosophically impeccable, but presents paradoxes for the exact sciences: We would be *forced* to accept as universal such laws of nature that we have recognised *only in experience*, although it may be the case that they are completely incomprehensible, i.e., we are unable to construct any type of theory for them. It may further be the case that we could not even imagine such a model in which these laws were not valid, i.e., we would be unable to negate these laws. This possibility is not excluded by the previous proof that all *a priori* knowledge is analytic.

War, Disappointment, and Ethics

In the letters from the US to von Wright, Ketonen is quite clear about the fact that he is broadening his philosophical horizon, and reconsidering the most important elements of philosophy. This is due to the fact he was extremely disappointed with the lectures on the philosophy of science given at Columbia University in the fall of 1949. He found it "hard to digest" on the whole, and on February 12, 1950, he actually writes that he has "had enough of it", and he feels that such a thing as philosophy of science does not exist. He writes, in the same letter, that there either has to exist a positive natural science, or a philosophy of science existing as just another practice, investigating one aspect side by side with other more current topics of interest. He still believes in logical empiricism, but sees it as being perhaps too limitative. He writes that philosophy does not exist, unless it practices and involves ethics and the life of man in general. He writes: "I don't mean that philosophy should present rules of life, I mean that ethics is more important than the philosophy of science". In the next letter, dated March 15, 1950, he writes, however, that logic and the foundations of logic are what he really respects in philosophy. He admits that the words in the previous letter were quite strong, but he insists that he "cannot consider as philosophical anything which explicitly forbids

28 Ketonen is not entirely clear on this point. With 'layers' is probably meant something like 'level of logical complexity'.

the study of ethics […]" He remembers that he, when mentioning to Kaila a discussion on sociology with a graduate student, felt as if he had been "down a dark alley looking for forbidden company". It is clear from letters to the Rockefeller foundation that Ketonen planned not only to go to the US, but also to visit Gödel in Princeton. This, at least to the writer, constitutes proof that Ketonen was serious about continuing his research in logic up until the visit to the States.

Ketonen was not at the frontline during the Finnish winter war, but later (briefly), in the continuation war (1941–1944) he served in the artillery, at the Ladoga archipelago, and at the ballistics office (which at the time was part of the air force). Recall that bombs were raining down on Helsinki in regular daily intervals during the days of Ketonen's defence of his doctoral thesis, and that the work on it began with the arrival in Göttingen during the Kristallnacht. One cannot even imagine how these events must have affected the young logician! He writes in the autobiography how the war and everything it brought with it had a profound effect on him. One can speculate that the horrors of the war combined with his dissatisfaction with the philosophy of science prompted a need for a turn towards a broader philosophy incorporating ethical studies, and logic became a spare time activity instead of an object of full-time academic research.

Department of Philosophy
P.O. box 9
(Siltavuorenpenger 20 A)
SF-00014 University of Helsinki
Finland
michael.vonboguslawski@helsinki.fi

Fredrik W. Thue

Empiricism, Pragmatism, Behaviorism: Arne Næss and the Growth of American-styled Social Research in Norway after World War II[1]

I

Arne Næss is conventionally portrayed as the seminal character of modern Norwegian philosophy. Equally important, however, is his status as a founding father of the social sciences as a distinct academic field in Norway. Shortly after the German invasion Næss gathered an interdisciplinary group of students and junior scholars to scrutinize the foundations of their respective fields of study. After the war the agenda of this group drifted from philosophy toward social research. To introduce a new interdisciplinary complex, known from the United States as the "behavioral sciences", into the national university system became its highest priority. In late 1949 these efforts led to the formation of the *Institute for Social Research*, which would prove seminal to the development of social psychology, sociology, and political science throughout the following decades.

It seems to be a common characteristic of the intellectual situation in all the Nordic countries that Vienna-style empiricist philosophy tended to operate as a gateway to American-style social science. In my master's thesis, now fifteen years old, I studied how this transition from philosophy to social research came about in the Norwegian setting.[2] My focal argument was that Næss' distinctive epistemological program and the social experience of Fascism and resistance both proved decisive, and that the group's intellectual development could be analyzed in terms of an intriguing dialectic between basic epistemological, ethical, and political attitudes. From 1943 Næss and his students increasingly addressed the practical and normative challenges of postwar society as a special responsibility of philosophers and social scholars. Similar to such proponents of unified science as John Dewey and Karl Popper, they came to see the ethos of empirical research as intrinsically relevant to the basic norms and methods of democratic politics.

This fascinating interplay of epistemological and political ideas will not be explored in much detail here. Instead I want to focus on a contribution by the young Stein Rokkan, one of Næss' most distinguished students. Rokkan's masters' thesis on David Hume (1948) was never published and exists merely as a rather

1 This article is based on my doctoral dissertation, *In Quest of a Democratic Social Order: The Americanization of Norwegian Social Scholarship 1918–1970*, Oslo 2006.

2 Later published as *Empirisme og demokrati*, Oslo 1997.

mistreated *Nachlass* in his archives. However, in what follows I would argue that it could be read as an attempt to explore the philosophical genealogy of Næss' radical empiricism. By constructing Hume as the philosophical father of radical empiricism, Rokkan indirectly challenged Popper's theory of piecemeal social engineering, which represented a competing interpretation of the ethical-political implications of unified science. In order to appreciate Rokkan's early work, I will first sketch some major features of Næss' program in the theory of science and his wartime attempts to extrapolate it into a program of ethical and political education.

II

Næss' distinctive approach to unified science was often referred to as "radical empiricism". This program was first presented, or rather demonstrated, in his doctoral dissertation *Erkenntnis und wissenschaftliches Verhalten* (Cognition and Scientific Behavior), written during his sojourn in Vienna in 1933-34.

Erkenntnis was a rather eccentric contribution to the discourse of the Vienna Circle, and to ascribe model status to it among his philosophical followers would be somewhat exaggerated. Still it expressed a view of the principles of unified science which helps explain why Næss became quite a gate-opener to the social sciences. What he set out to do was to replace "subjective" epistemology with an objective psychology of scientific cognition. The aim was to overcome what he saw as a fundamental inconsistence in the movement for unified science: When the logical empiricists drew their sharp line of demarcation between science and metaphysics, they applied epistemological doctrines which were themselves ultimately metaphysical rather than scientific in nature. Næss' alternative was a naturalistic and radically action-oriented model of human cognition. The behavioral sciences were here invoked to produce a characteristic alienation to the object of study and thereby facilitate the transition from philosophy to science. This ambition was typical of the movement for unified science. But while Carnap and Neurath based their theory of science on highly formalized disciplines such as physics and mathematics,[3] Næss was more inclined to look to such disciplines as biology, psychology, sociology, and cultural anthropology. This difference had far-reaching implications. The physicalism of Carnap and Neurath led to a model of unified science as a one-way avenue leading from the social and cultural sciences via psychology down to physiology, biochemistry, and ultimately physics. Næss' approach suggested a more flexible, non-hierarchical cooperation between various disciplines involved in the study of man, from biology and psychology to anthropology and sociology. While diverging from prevailing modes of thought within the Central-

3 Cf. Rudolf Carnap, Logical Foundations for the Unity of Science, *International Encyclopedia of the Unified Sciences* 1 (1), Chicago 1938.

European movement for unified science, this thinking had certain affinities with American pragmatism. Næss, like the pragmatists, conceived of science and the pursuit of knowledge in general as *activity* or as modes of behavior, and believed the growth of scientific and other forms of knowledge could be most adequately reconstructed by examining the evolution of these behavioral systems.

Næss' behavioral epistemology was an avant-garde experiment of thought, but impracticable as an empirical research program. His so-called empirical semantics was an attempt to translate his epistemological naturalism into a more applicable methodology. But empirical semantics also helped bridge the gap between Næss' radical empiricism and the more general cultural task he had been entrusted as Norway's only professor of philosophy: to introduce all university students to the ethos of academic scholarship. Næss' original contribution to the *examen philosophicum*, an introductory exam in psychology, logic, and the history of philosophy which was mandatory for all academic students in Norway, was to transform the curriculum in formal logic into a course in the use of natural language in everyday reasoning.[4] The aim of this course was to study how particular concepts and formulations were actually used as instruments in different kinds of communication. Næss held such investigations to be seminal to the growth of science, but he also found them valuable for general educational purposes. By sensitizing students to the pitfalls of communication he hoped to immunize them against demagoguery and manipulation. Scientific objectivity had a general cultural value, which expressed itself in an attitude that Næss, with a favorite term of his, called *saklighet*.[5] *Saklighet* could be described as the capacity to assume a distanced and disinterested perspective, even in matters where one's own interests or identity were at stake. It was fundamentally a question of decency in communication and thus involved ethical values.

Næss' epistemological naturalism and ethos of *saklighet* gave a clue to his wartime reflections on the problems of Fascism, resistance, and postwar democratic reconstruction. He thus sharply rejected the widespread view that democracy, in order to become resistant to totalitarian ideologies, had to be grounded on a set of absolute values, a strong unifying ideology, or firm communitarian solidarity. Næss instead defended an approach he called *ethical trivialism*: a conscious translation of pretentious moral ideals into norms which were closer to actual human behavior patterns and habits. This principle had, according to his argument, two great advantages vis-à-vis a high-flown ethical idealism: It reduced the psychological drives that led to moral hypocrisy and a distorted self-image, and it represented a more effective form of motivation and learning, since the reduced distance between behavior and ideals would make for less punishment and

4 Arne Næss, *Endel elementære logiske emner*, Oslo 1941 (and ten later editions), English version: *Communication and Argument: Elements of Applied Semantics*, Oslo 1966.

5 This term, which is untranslatable into English, is known to the English-speaking world in its German version: *Sachlichkeit*.

more reward. Where moral alarmists found abundant evidence of moral decay and confusion, Næss saw rather signs of moral *evolution*: an ongoing transition from a "moral preaching with major emphasis on duty and punishment, on relations of authority and submission, toward ethical clarification with primary regard to social attitude, welfare, and personal development". And this transition entailed a shift from simple, pointed imperatives to complex, enlightened moral supervision and education, in short, to a more conditional, scientifically and philosophically enlightened discourse.[6]

Trivialism, when extrapolated from ethics to politics, was tantamount to ideological secularization. It was therefore more than a coincidence that the seminar in which Næss first presented his ethical trivialism, autumn 1947, drifted into a discussion of Popper's recent magnum opus, *The Open Society and its Enemies*. Popper's piecemeal social engineering aimed precisely at "trivializing" contentious issues by translating unconditional normative claims into conditional hypotheses or means-ends clauses. But there were also striking differences between Næss and the position Popper outlined in his grandiloquent narrative of the eternal philosophical struggle between the principles of the open and the closed society. Some of the most basic principles of the open society, according to Popper, followed from the Kantian dichotomy between the sphere of necessity and the sphere of freedom. In Popper's formulation this amounted to a fundamental distinction between *facts* and *decisions*. To blur this distinction inevitably led to the suspension of enlightenment and reason, and was therefore a basic constituent of the philosophies of the closed society. This criticism did not only apply to the three great villains in Popper's narrative, Plato, Hegel, and Marx, but also to what he labeled sociological naturalism, represented, among others, by John Stuart Mill.[7]

Popper's harsh criticism of naturalism in epistemology, ethics, and sociology was a challenge to Næss, who never accepted a sharp logical dichotomy between facts and decisions. Contrary to the common wisdom of Popper, the logical empiricists, and the Uppsala school of legal realism, Næss argued that norms could be clarified by the same forms of linguistic analysis that applied to descriptions. Neither did Næss share Popper's pointed critique of ethical and sociological naturalism. Næss' ethical trivialism, itself a moral philosophy, was based on the view that spontaneous reactions of empathy between humans presented deeper and more universal moral wellsprings than philosophical dogmas. Friendliness and love, Næss stated, did not seem to rely on "beliefs to which we arrive by complicated reasoning or un-analyzable intuitions, but on deep-seated, biologically well-founded tendencies to ... arouse sympathy reactions vis-à-vis other sensitive beings".[8] The key to a better world thus lay not so much in the fixation of ultimate

6 Arne Næss, "Om noe vi kan tro på og leve for. Kulturkrisen og uinnskrenket verdipessimisme", *Samtiden* 1948, pp. 174–179.

7 Karl R. Popper, *The Open Society and its Enemies*, vol. 2: *The High Tide of Prophecy: Hegel, Marx, and the Aftermath*, London 1974, p. 88 f.

8 Næss 1948, p. 170.

ethical and political principles as in the gradual expansion of benevolence from one's closest circle to ever-wider circles of humanity.[9]

This discrepancy between Næss and Popper reflected a momentous tension in the movement for unified science as such. I would argue that it might partly be seen as a tension between European-style scientism and logical empiricism on the one hand, and American-style pragmatism, behaviorism, and functionalism on the other. In this conflict Næss seems to have been more of an "American" than a "European", which might help explain the easiness with which many of his students turned to the postwar social sciences as a continuation of ethics and political philosophy with new means.

III

The tension within the movement for unified science between naturalism and transcendental reasoning constituted the backdrop of Stein Rokkan's early work on the appeal to nature in David Hume's social and political thought. Rokkan would later become a leading political sociologist and is probably Norway's most important sociologist in the postwar era. But he started out as an intellectual historian. Unlike Næss and most of his students, Rokkan was a philologist with a special interest in the history of philosophy. Shortly after the liberation he got hold of Popper's *The Open Society and its Enemies*, which he consumed with enormous interest. What particularly fascinated him was the way in which Popper related epistemology to politics. Starting out as a "passionate student of the history of ideas", he explained in a letter to Popper in early 1947, he had gradually become "more and more absorbed in the general problems of the foundations of social and political theory".[10] Rokkan was particularly interested in exploring the tension he sensed between Næss' and Popper's philosophical attitudes. As a historian of philosophy, he also had some second thoughts about Popper's historiography. His thesis on Hume addressed both of these issues, albeit in a cunning, indirect manner.

In late 1943 Rokkan had presented a paper at Næss' seminar on the character and implications of Hume's alleged epistemological skepticism. Rokkan here argued that conventional philosophical accounts of Hume had tended to rely heavily on the Kantian reception. But since Kant could not read English, he had not been able to interpret the skeptical elements in Hume in the light of his ethical, sociological, economic, historical, and political contributions. When thus contextualized, Hume's skepticism appeared not as a fundamental threat to scientific rationalism, but rather as an integral part of his central philosophical concern: to

9 *Ibid.*, p. 181.

10 Letter from Stein Rokkan to Karl R. Popper, Paris, April 5th 1947, Stein Rokkan Archives (SRA), Ea:1.

establish a firm and endurable foundation for the sciences of "human nature".[11] Hume's skeptical dismantling of the logical basis of knowledge was but a starting-point for the foundation of an alternative epistemological paradigm, based on the "cognitive behavior of man".[12] A firm belief in the predictability of man's cognitive and social behavior in fact constituted a fundamental principle of Hume's entire philosophy. Hume's alleged skepticism, Rokkan concluded, applied only to ultimate philosophical problems, and was therefore fully compatible with scientific activity and a general science of human nature.[13]

Thus reinterpreted, Hume appeared as an obvious philosophical ancestor of Næss. Both endeavored to replace the *logic* of science as conventionally understood with an empirical science of cognitive behavior. Both sought to avoid the potentially self-defeating consequences of skepticism by maintaining the invariability of human nature as an alternative basis for epistemology, ethics, and politics. And both combined skepticism vis-à-vis philosophical doctrines and a priori knowledge with confidence in man's capacity to achieve consensus on ethical and political tenets.[14] As Rokkan construed him, Hume also appeared as a predecessor of the contemporary behavioral sciences: By founding his moral, social, and political philosophy on the invariability of human nature, he provided the basis of a science of human behavior.

Hume therefore seemed to present an anomaly to Popper's historiography of philosophy in *The Open Society and its Enemies*. As an epistemological and sociological naturalist, Hume would qualify as a philosopher of the closed society in Popper's sense, or at least as a skeptical dismantler of scientific and political rationalism. Rokkan's reading led to the opposite conclusion: Hume's epistemological naturalism was indeed a precondition for the scientific transformation of social and political theory. This conflict of interpretations set the stage for his analysis: How had Hume invoked naturalistic arguments in his social and political philosophy? How had he combined the appeal to nature with arguments about the social contract and consent? And how did this argumentative synthesis relate to

11 Stein Rokkan, Hume og skeptisismen. Et fragment av et fragment (November 1943), SRA: Hb:1, cf. Stein Rokkan, Philosophy and Ideology. Notes on the Politics of David Hume (MA thesis 1948), p. 12, SRA: Hb: 2.

12 Philosophy and Ideology, p. 11, 13.

13 *Ibid.*, p. 18.

14 Rokkan quoted Hume's argument that men would have a "natural" propensity to converge in their views, provided that their deliberations addressed "any subject of common life and experience" rather than those metaphysical speculations "which lie entirely beyond the reach of human capacity". Hume even tended to believe, according to Rokkan, that "the vast majority of [philosophical] controversies … have 'turned merely on words' and that all men are likely to agree if induced to clarify their language: controversies are chiefly upheld through the use of ambiguous terms and will tend to vanish if their different interpretations are kept apart." *Ibid*., p. 77 f. (Quotes from Hume, probably *Treatise*, reference missing in Rokkan's manuscript.)

his central concern: to transform moral and political philosophy into a science of human nature?[15]

The character of Hume's appeal to nature was quite different from the classic doctrine of natural law. While rejecting the notion of a natural moral law in the sense of an "eternal, immutable, and universally obligatory" principle, Hume "stressed the 'naturalness' of any social norms gradually developed through human conventions and hallowed by long traditions of acceptance". This argument was consistent with his general tendency to stress the empirical uniformities in human behavior. Society, Hume argued, was "in perpetual flux, one man every hour going out of the world, another coming into it". And this continuous binding of one generation to another, which was critical to the continued existence of society, could not be produced by an original social contract. Hume therefore became concerned with those "empirical relationships that accounted for people's actual obedience, their consciousness of an obligation to obey, and the arguments they accepted or were persuaded by in matters of obedience to the laws and order of government."[16] What Hume addressed in his moral and political philosophy, Rokkan concluded, was the general problem of *social order*. Hume's original contribution to socio-political theory was his "observance of the actual system of rules, whether strictly legal or customary, which bind together the members of any society into an organic whole".[17] This synthesis of conventionalism and naturalism expressed a "realistic", "behavioral", or "sociological" transformation of older contractual paradigms of political thought.

Rokkan's analysis confirmed his expectation that there were close logical and genealogical links between eighteenth-century British empiricism, the radical empiricism of his teacher, and the realistic, behavioral, or naturalistic approach to social and political theory that characterized the contemporary, American-styled social sciences. To the extent that Hume conceived of his theory as a basis for social engineering (which Rokkan in fact suggested that he did), it was a conservative, system-maintaining engineering similar to the variety that Popper had criticized in his analysis of Plato. But Hume was no utopian engineer aiming at controlling or arresting all social change. His view of the history of mankind as a gradual readjustment of human habits, attitudes, and values *from* the primary group level *to* more encompassing institutional contexts rather suggested an incremental, "piecemeal" perspective on the social process. Here was yet another fundamental affinity between Hume and the American social-scientific tradition.

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15 Philosophy and Ideology, p. 6 f.

16 *Ibid.*, p. 122.

17 Henry Sidgwick, *Methods of Ethics* (1874), London 1907, p. 440, quoted from Philosophy and Ideology, p. 86.

IV

Rokkan's thesis, while never published, presented an intellectual clarification with crucial implications for his and his fellow social researchers' scholarly project. Through an analytical effort at a highly strategic area of the contemporary discussions of scientific rationalism and its political implications, he explored a contrast between two models of social science and social engineering, each of which claimed to epitomize the true spirit of liberal-progressive democracy. By taking sides in this conflict, albeit implicitly, Rokkan indicated the direction of his own future scholarship in the sociology of politics, and substantiated the strong philosophical kinship between Næss' circle and the American-styled behavioral sciences.

Popper's critical rationalism shared three vital tenets with the American liberal-progressive tradition. First, both departed from an instrumental or technological interpretation of the natural sciences, claiming that the potential unity of science was rooted in a unity of scientific methodology. Second, both argued that scientific methodology and democratic politics constituted two potentially interrelated forms of creative-adaptive learning based on institutionalized processes of trial-and-error. The scientific spirit therefore had a general cultural and pedagogical value as a unifying, "*meta*-political*"* creed of the democratic polity.[18] Finally, by seeking to integrate and translate elements of the socialist political tradition into a distinctively liberal institutional framework and philosophical idiom, Popper placed himself close to central twentieth-century currents in American sociopolitical thought. In both cases, a new, "socialized liberalism" emerged from a fusion of politics and social science, which crystallized in a program of "piecemeal social engineering".[19] These three elements – unified science, a readiness to apply the social sciences in a gradual "rationalization" of politics, and a political creed that sought to reconcile the opposition between socialism and liberalism – were also shared by Næss and his students.

On three other, equally critical points Popper challenged the behavioral paradigm within American philosophy and social science. First, he sharply criticized any attempt to base the social sciences on assumptions about "human nature". The task of the social sciences, as Popper saw it, was rather one of explaining human actions as a function of the "logic of the situation", as economists did when they explained market behavior by means of their theory of demand and supply.[20] By

18 The concept of *metapolitics*, which I develop in my thesis (Thue 2006), alludes to those fundamental social notions and tenets which were supposed to unify the body politic across the cleavages between particular ideologies and social interests. I argue that the postwar, US-dominated social sciences incorporated such a meta-political ambition, which made them highly attractive to Næss and his students.

19 Cf. Dorothy Ross, *The Origins of American Social Science*, Cambridge 1991, p. 168

20 Popper 1974, p. 97. Popper here quoted Max Weber's argument that the psychological analysis of an action in terms of its (rational or irrational) motives presupposes that we

taking institutional economics rather than psychology as the paradigmatic social science, he distanced himself from the dominant American understanding of the social sciences as behavioral sciences.

Second, Popper defined social engineering in a manner that differed significantly from the common American usage, where the concept tended to merge with notions of democratic education and social control. As Popper saw it, the practical task of the explanatory social sciences was not to predict or control social behavior per se, but to analyze and explain the gap between our well-intentioned social actions and their objective repercussions on the fabric of society. He therefore rejected any attempt at fusing social engineering with democratic education or character-formation: Piecemeal social engineering should always mold and modify social institutions, and *never seek to re-form or re-socialize the individual*.[21] A democratic society would above all have to abstain from any attempt to "engineer" human happiness.[22] This reflected Popper's sharp distinction between the sphere of necessity and the sphere of freedom. The social engineer operated *on* society in a purely instrumental fashion, confronting the "natural" realities of social life with a set of normative standards which were seen to be the product of people's decisions qua ethically responsible subjects. By contrast, the American pragmatists tended to appreciate social engineering as a complement to "natural" processes of social integration and adaptation, and played down the Socratic-Kantian distinction between *poiesis* and *praxis* – between acting *on* and acting *in* society.

While partly attributable to conflicting epistemologies and theories of the social sciences, this difference could also be traced back to a third difference: the parties' underlying views of the "open society" or the modern social condition itself. For Popper, the open society was tantamount to an "abstract society", where interpersonal relations were predominantly rational and instrumental. Life in the open society therefore involved a loss of "tribal" collectivity, a loss which was only partly compensated for by those social affiliations that people entered into on a voluntary basis.[23] Man now found himself thrown back on his private sphere and intimate relations in his search for happiness and spiritual "meaning"; and the only decent way for him to cope with this "strain of civilization" was to endure it with-

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have previously developed some standard of what is to be considered rational in the situation in question.

21 It was a distinctive feature of the totalitarian regimes and their utopian mode of social engineering, Popper argued, that they were inclined to extend their agenda from the transformation of society to the transformation of man. If man did not "function" properly in the brave new world that the social engineer had brought about, he should be conditioned to do so. But this would remove any possibility of testing the success or failure of the new social structure, and therefore undermined the scientific attitude. Karl R. Popper, "The Poverty of Historicism II: A Criticism of Historicist Methods", *Economica* (August 1944), p. 124.

22 Popper 1974, p. 237.

23 Karl R. Popper, *The Open Society and its Enemies*, vol. 2: *The Spell of Plato*, London 1974, p. 174−75.

out escaping into any compensatory "tribalism". This argument, which echoed Max Weber, revealed Popper's kinship with the German sociological tradition and its somber reflections on the transition from *Gemeinschaft* to *Gesellschaft*. By contrast, American sociology and social engineering had been shaped by confidence in the possibility of mediating traditional community values into the fabric of modern, urban-industrial society. This faith had given rise to a social theory that sought to define modern society in terms of the individual's social mobility and multiple group affiliations, rather than as the inevitable "rationalization" of all human relations.

Through his theory of piecemeal social engineering, Popper stated, he sought to replace the lost faith in natural historical progress with a modern faith in rationalism and scientific progress. The American pragmatists and their inheritors in the social sciences were, by contrast, inclined to regard scientific and historical progress as organically interrelated. They conceived of society *both* as an "organic" conglomerate of concrete social groups *and* as an "artifact" in need for constant watchfulness and management. Social engineering was seen as a way of sustaining and modifying "natural" processes of social control and integration. In the vernacular of John Dewey, social engineering was tantamount to socializing people into a democratic way of life.

In his early paper on Hume's skepticism, Rokkan had drawn attention to a feature in the "shrewd Scot's" thinking which he shared with Næss: a tendency to *replace confidence in philosophical dogmas with confidence in the social process*. This was exactly where the paths between Popper and the American liberal-progressive tradition divided. This philosophical dividing line helps explain Næss and his students' attraction to the American social sciences and corresponding alienation from Popper.

Such fundamental confidence in the social process was, as Max Horkheimer dryly remarked to Næss at a UNESCO symposium in 1947, hard to reconcile with the world of concentration camps.[24] At historical hindsight one cannot help being struck by Næss' peculiarly inadequate understanding of the most sinister phenomena of his age: the worldwide "ideological" wars and the abysmal atrocities committed in their name. Some of these shortcomings were common to large parts of the American social sciences, such as the psychological trivialization of the problem of evil, or the implicit bracketing of fascism as but a transitory regression from a continuous "secular upward trend" in the history of mankind (Julian Huxley).[25] Næss and many of his students also grossly underestimated the iron-bound reality of Stalinism and the problem of totalitarian power. This liberal innocence was perhaps exactly what connected them to liberal American social scientists at

24 Max Horkheimer's comment to Næss, "The Functions of Ideological Convictions", in Hadley Cantril (ed.), *Tensions that Cause Wars*, Urbana, Ill. 1950, p. 296.

25 Julian Huxley, *UNESCO: its Purpose and its Philosophy*, Paris: Preparatory Commission of the United Nations Educational, Scientific and Cultural Organization 1946, p. 20.

a point in time when the hot war had ended and the cold war not yet stalemated. Unlike most countries in continental Europe, the United States and Norway both came out of the war with a consolidated national self-image and tended to conceive of themselves as democratic models to the postwar world. While highly critical of Norwegian as well as American national chauvinism, Næss and his students were formed by historical experiences which differed markedly from those of the Vienna Circle. It is tempting to suggest that this showed through in their philosophical thinking.

V

Based on his studies of the Swedish economist Gunnar Myrdal, the Finnish historian Pauli Kettunen has argued that the Nordic welfare states were underpinned by a conception which he terms "the society of virtuous circles". The essence of this mental image was the optimistic faith that society could be progressively improved in a mutually reinforcing interplay between economic growth, social welfare, and political democracy. This appears in Myrdal's writings to be a specifically modern conception, where the old faith in natural social harmony had been replaced by the idea that social order had to be *created* by social engineering. As Kettunen sees it this break-up from the past turned out to be incomplete: Myrdal's project was in fact based on an older, protestant, and specifically Nordic image of society. This linkage between social mentality, science, and welfare state appears to Kettunen as an historical paradox, inasmuch as the society of virtuous circles was promoted by social sciences brought in from the United States, where the ideology of the "strong society" found much less cultural support that in Scandinavia.[26]

What Kettunen sees as a paradox might seem somewhat less paradoxical in the light of those underlying liberal-progressive affinities highlighted in this article. The real historical irony is rather that philosophical impulses from Vienna and Berlin became filtrated through the American intellectual tradition before they made their greatest practical impact in the Nordic welfare states.

Forum for universitetshistorie
Postboks 1008 Blindern
0315 Oslo
Norway
f.w.thue@iakh.uio.no

26 Pauli Kettunen, "The Society of Virtuous Circles", in Kettunen and Hanna Eskola (eds.), *Models, Modernity, and the Myrdals*, Helsinki: The Renvall Institute for Area and Cultural Studies 1997, pp. 153–173.

Jaakko Hintikka in the *Library of Living Philosophers*: A Dialogue

An evening discussion between professors Jaakko Hintikka (Boston University) and Simo Knuuttila (Academy of Finland) on September 2, 2007, at the Helsinki conference:

Knuuttila: Ladies and gentlemen. Thank you for inviting us to this event and for this opportunity to discuss the latest volume of the Library of Living Philosophers, which was published last spring. The title of the volume is "The Philosophy of Jaakko Hintikka". Probably most people here know this series, which was founded in 1939 by P. A. Schilpp. The idea of the volumes is to invite some prominent philosophers to describe their lives and ideas and then ask other philosophers to write and comment on topics which the first author has written about. Then the philosopher to whom the book is dedicated answers and discusses these other people's papers about his or her philosophy. These volumes gradually became a very popular series which most philosophical libraries wanted to have. It was considered such a good idea, giving rise to one of the most prestigious series in philosophy. It is also considered as a kind of philosophical honor for the philosopher to be chosen to be among these living philosophers. The books always have the same structure; first, an intellectual autobiography, pretty extensive, then the papers on the basic author's philosophy and, last, the papers are answered separately. So there is a kind of discussion going on. The book on Jaakko Hintikka's philosophy consists of almost 1000 pages. It is also available as a paperback. Now, I have prepared some questions. There is pretty famous American TV series which you might have seen where a person interviews famous movie stars about their training and how they became actors and so on; then they go to their most important movies, their Oscar movies, etc. I am going to ask philosophical questions which are discussed in this volume. I have chosen some general topics which very much figure in this book and which, I think, are kind of key areas in Jaakko Hintikka's philosophy. Now, first question – this is actually taken from the TV series: How did you become a philosopher?

[Laughter]

Hintikka: Actually that is a very good question for this particular audience, which is connected very much to the persons I will be discussing. My first philosophical inspiration was the typical one at the time in Finland. It came from Eino Kaila's writings when I was still in the high school. This was my original philosophical inspiration. And then, the second part of the question is how I became a profes-

sional philosopher, and that has to do with, not Eino Kaila whom I only later came to know personally, but von Wright. He was the Swedish language professor of philosophy at the University of Helsinki. I began to listen to his lectures. Since he was lecturing in Swedish, he had a few students only. Only some three or four, and very soon his lectures became almost like seminar discussions. He got this audience involved in his own thinking, which was a marvelous experience. So that is how I came to involved in active philosophical philosophizing and not just being interested in philosophy. And I was very lucky, soon I had my first original idea and that came from von Wright's lectures. It was about the distributive normal forms that he had been using. So I got involved in active philosophical, in this case logical research.

Knuuttila: This was also your dissertation topic?

Hintikka: Oh yes, but it took me a long time. My philosophical and logical training was still going on and I had to work everything out myself, so it took a long time. But I was able to, yes, I was able to use this work as my dissertation topic.

Knuuttila: It could not have taken very long because you were extremely young when you published it in 1953 –

Hintikka: I was very young when I got the idea.

Knuuttila: OK. But then after the dissertation you were also in the USA and the United Kingdom?

Hintikka: Well, actually already before finishing it. It was very shortly after the war and I was very lucky. I think these were the first years when anybody could go from Europe to United States to study. And I received what was called a one-year exchange scholarship, to study in United States, when I was still an undergraduate in 1948–49. That was the first time, and then I kept visiting United States and studying and working there in other ways. If you want to hear about the later stages of my becoming professional, one crucial stage in my career was the good luck of being elected to Harvard's Society of Fellows for 3 years, which is of course a marvelous opportunity of not only doing research but coming to know and becoming a member of the philosophical community in United States and even in England. During my 3 years as a fellow (1956–59) I was also able to visit Oxford for a term and come to be involved in discussions there.

Knuuttila: Was the book *Knowledge and Belief* connected with your stay in Harvard?

Hintikka: That is where I did most of the work for it. The book was published a

couple of years after I got back from there.

Knuuttila: Let's go forward to some topics in this Library of Living Philosophers volume. One interesting feature of the book is that many papers are related to some central issues in the philosophy of the last century, such as early analytic philosophy, the Vienna Circle and Wittgenstein's philosophy, Frege, neo-Fregeans, Husserl and phenomenology, and so on. Let's speak a little about these. You make some critical points on the weaknesses in these traditions but also see some systematic ideas which make some of them interesting and valuable philosophical contributions. Perhaps we could first discuss the analytic tradition, the Vienna Circle and logical positivism. What is the condition on which we would all be logical positivists nowadays?

Hintikka: Well, I have my own take of the overall situation. When I read philosophical discussions, particularly in the United States, very often I got the impression that the author is thinking that we are now finally getting rid of the bad influence of the logical positivism, slowly overcoming it. I think this is a wrong historical perspective. What we are experiencing at this time is not the end of logical positivism or logical empiricism. This is the tail end of the reaction in the analytic tradition to logical positivism. Now we are seeing the end of the influence of people like Quine, Popper, Kuhn. It is being realized fully that their ideas are no longer leading to any further insights. I indicate briefly my reasons for saying this. I have written about Quine's presuppositions in his thinking. If I am halfway correct, the assumptions that he is making will not lead to any successful research program and no further development in the philosophy of language. I think that Quine's ideas of philosophy and language have not had any real applications to real linguistics. Nor have his ideas about philosophy and logic had any influence on the development of real logic. So I think this influence is justifiably coming to end. What about other critics? Well, I think that Popper, whom I knew personally, was extremely intelligent, quick and a really sharp thinker. But also I think his problem was, exaggerating perhaps, that on every topic he took up, he immediately had one extremely good idea and then he spent the rest of his life defending, not developing the idea but defending it and proclaiming that he was the only thinker in the history of the world to have put it forward. And that is why it is so easy to give labels of his views. You know how to list them, you have the third world, you have falsification, you have information, you have propensity and so on. I mean, that exhausts Popper's contribution. You cannot do that to Wittgenstein or to Kant that way. Or take Kuhn. He was an important figure, not only in history of science and sociology of science but also in philosophy. But if you look at his purely philosophical ideas, they do not really go methodologically far beyond the old-fashioned models. I have argued by means of specific examples that Kuhn could have improved his own work in history of science, I mean, in his own field, if he had taken a greater account of the logical and epistemological issues in science. To take one example, since I want

to be concrete here. Kuhn's most significant contribution as a historian of science was probably his work on the early history of the idea of quantum. He presented it in a very interesting way and documented how Planck, although he is usually thought of as the originator of the notion of quantum, never based any work on that notion, did not make any use of it. And this is interesting, because it took Einstein to start using the idea of quantum as a tool of actual physical explanations. Well, what is the explanation of this strange phenomenon? If you look at the history of the notion of induction and take induction in the old, pre-Humean sense, you can see that you can perfectly understand historically, methodologically why Planck did what he did. This is not a criticism on Kuhn, of course, but it shows he could have put his own discoveries in the extremely interesting philosophical and historical perspective. And I can give other examples. So I do not think this Second Wave is really the wave of the future any longer.

Knuuttila: You have written and edited works on Carnap. What do you think about Carnap's development from the Vienna Circle to his later philosophy?

Hintikka: Take his later ideas about semantics, which look very much contrary to what was said and perhaps also thought in the Vienna Circle in the early 30's. The big change is that Carnap gave up the idea of purely syntactical approach, a peculiarity of the Vienna Circle, involving the impossibility of semantics and a preference of purely syntactical approach. This was a dogma for Wittgenstein and his *Tractatus* as he says himself in his letter to Schlick in August 7, 1932. This was the view of Quine, as of pretty much everybody else in the Vienna Circle. Yet in some ways for Carnap it was always a much less important restriction. It was not really an integral part of his core ideas at any time. If you look at the *Logical Syntax of Language*, there is a lot of what we would call semantics there. So the move to an explicitly semantic approach was in a way a smaller step for Carnap than it would have been, for instance, for Quine. I mean that it would have been a complete change in Quine's view, for Carnap it was a much smaller step.

Knuuttila: You have also written lots about Wittgenstein's view of language and the absence or inaccessibility of semantics in Wittgenstein. How do you think this is related to Carnap?

Hintikka: Carnap in the early 30's adopted what he called a formal mode of speech which means wanting to do everything on a syntactical level. But as I said, this was more of a kind of choice of one approach than others. It was not based on a deep belief on the impossibility of doing semantics discursively, scientifically. Initially, in the early 30's, the basic philosophical differences of the questions Wittgenstein and Vienna Circle were asking were very small, even though their style of thinking and their style of expressing themselves were worlds apart. There is a proof of this which I have written about. The proof comes directly from Wittgenstein.

There was a very angry quarrel in the summer of 1932 which began when Wittgenstein received a kind of offprint from Carnap. He read it and flew into an absolute rage. Well, what was the problem? Is Carnap misunderstanding, distorting Wittgenstein's views? No, Carnap is borrowing from them, almost plagiarizing. It is a long story and I do not want to go into details. But that controversy is a very convincing proof of the basic similarity of Wittgenstein's ideas with those of the Vienna Circle, not only his early ideas but at least some of his middle period ideas and questions. Schlick tried to manage as a peacemaker. He reported Wittgenstein's objections to Carnap. I can see Carnap shrugging his shoulders, saying that he has never heard Wittgenstein explaining these things and that there is nothing in the *Tractatus* about them. Poor Schlick, I think on his own, reported this to Wittgenstein who got even angrier and said that Carnap was likewise using ideas from the *Tractatus* without mentioning their source. He lists half a dozen ideas that he claims Carnap got from *Tractatus*. Now everything Wittgenstein said about his own ideas is not always the last and final historical truth. But that at least shows that there was a great deal similarity. Wittgenstein, of course, drifted apart, away from the Vienna Circle. But I think this has less to do with the basic difference in their problems. It has more to do with how Wittgenstein developed his own ideas independently of anybody else.

Knuuttila: How do you see Wittgenstein's development in more general terms, especially its earlier phase? You have been pretty interested in this.

Hintikka: In order to understand Wittgenstein, you have to be aware of his ways of thinking and also ways of relating to different people. Wittgenstein at one time listed ten thinkers that he says influenced him. It is a very strange list, for in the case of about 5 or 6 of them you cannot find any traces of any philosophical influence whatsoever in his writings, I claim. Well, even if there is something, that is minimal. And the list omits two thinkers whose views Wittgenstein took over much more than perhaps anybody else's. G. E. Moore and Ernst Mach are not even mentioned by Wittgenstein. I think the reason is that when Wittgenstein spoke of influences or wrote about influences, he meant people who started him to think, who inspired him or made him think about something maybe in terms of objections or problems. Whereas he was not interested in highlighting ideas that he took to be obvious or commonplace, even though I do not think all of them were so obvious. I think Mach was one of these suppressed influences. However, there also was a curious personal idiosyncrasy. For whatever reason, maybe I can understand it but I do not think I can explain it, Wittgenstein despised Mach. He thought Mach was hopelessly simple-minded and vulgar in his thinking. He wrote to Russell that reading Mach makes him sick. But if you take for instance the views Wittgenstein expresses at the end of *Tractatus* on philosophy of science, there is a great deal of common ground with Mach there. Mach was one of the major figures in the intellectual life in Vienna and perhaps elsewhere too. That is why even Lenin wrote a

book against him. Lenin was not interested in philosophy but he was counteracting Mach's general intellectual influence. And what was the controversy between Boltzmann and Mach all about? It is whether our symbolic system, our language, influences our total structure of knowledge. The same problem comes even little bit less clearly around in Hertz. Now, of course, Mach denied any influence there. Scientists acknowledge the economic description of our experiences. Boltzmann raised the question about precisely that influence. And we know that Wittgenstein admired Boltzmann greatly. But if you look at his *Tractatus*, logical truths are there said to be tautologies. He thought that our language does not contribute at all to the structure of knowledge. This was Mach's answer. I suspect greatly that Wittgenstein would have said something like "oh yes, that is true, I've disagreed with Boltzmann but Mach was so stupid that he did not even see the real problem". That is why Wittgenstein never calls his own philosophy in the *Tractatus* phenomenological. It would have associated him in public consciousness with Mach's. Even though there was a verbal agreement, he did not want to be associated with Mach in any way. Only when he gave up this early phenomenological approach, in 1929, did he begin to refer to Mach because now he could criticize him. I am not claiming that there is any direct influence but, for instance, if you look at Mach's views on the self, on the ego, there are very striking similarities with Wittgenstein's comments in the *Tractatus* on solipsism.

Knuuttila: Wittgenstein thought that philosophers should somehow stop being philosophers now that most problems are solved. He did not encourage his students to become philosophers, but doing something more useful with their lives. Would Carnap think that philosophy is somehow useless in the long run, in the future? Or was it a common ideal in logical empiricism that they were, so to say, liberating philosophy from the wrong problems, misguided philosophical questions, and what philosophy was really needed for was a kind of methodology of correct thinking in the future?

Hintikka: I have not looked at the different answers. But I think there are some things one can say here. There are several quite different things going on. On one hand, the Vienna Circle attacked metaphysics and earlier philosophy in clarifying philosophy. They also wanted to solve the foundational problems of sciences. It looks somewhat like Wittgenstein. But Wittgenstein's attitude to explanations about his philosophy being purely descriptive, leaving everything as it is, therapeutic and all that, that is due to the particular dilemma that he was in. This is connected with Wittgenstein's views on the inexpressible at the end of the *Tractatus*. Wittgenstein was a member of a larger tradition, a very common way of thinking, according to which semantics is merely a way of looking at language. One part of this unorganized tradition was that semantics of a language cannot be expressed in the same language. Or if you absolutize it, we cannot speak discursively about semantics of language at all. But now, what is *Tractatus* all about? It is about the

relations of language to reality and language and thinking to reality, that is semantics. So Wittgenstein had to explain to himself what he was doing. And that is how he was led first of all to explanations about his own way of looking and his own thinking. This was based on, perhaps, his own attitude. He was not interested in science. Well, this should not be a sort of great stumbling block in reading Wittgenstein. Wittgenstein thought that his semantical views are inexpressible, but we need to see ourselves. This does not make his theory of the inexpressible a great dilemma at all. There are perhaps further things to be said but basically this is the story.

Knuuttila: When did you get your idea about the definability of truth? Was it somehow related to your interest in Wittgenstein's view on semantics or the other way round?

Hintikka: There was an earlier discussion on the technical level, there is Tarski's famous theorem of the indefinability of truth for a language in the same language. This discussion has been going on intensively. I think there are two things. What happened was a simply a result in the formal semantics. This is perhaps not the order of what happened in my thinking, but basically I realized the reason of Tarski's impossibility or why Tarski was able to prove that. Originally, I was very much puzzled by this impossibility. I knew that Tarski was right for the language he was considering. But if we look at the Gödel numbering method and all that, it seems that it should be extremely easy to define, turn Tarski's own T scheme into a truth predicate. If you are given a sentence, its Gödel number can be trivially calculated from that, so why cannot you turn the same process around and say that the truth predicate applies to this Gödel number and then, so to speak, calculate back and say, "if and only if the original sentence holds." Why is this impossible? That is an effective procedure! The answer is that in so doing you create dependency relations between the variables in a sentence that destroy the equivalence. When you see this, you realize that, then if you liberate the language by allowing the expressibility of different kinds of dependency relations and independence relations, then such a truth predicate is trivially possible. Thus, the first impression of easy definability turns out to be true. So, purely technically, it suddenly became obvious to me that in the whole issue there is no reason to believe in any sort of interesting impossibility. This is combined with the fact that the reason why truth is inexpressible in the languages that Tarski was dealing with, is that those languages are too strong. You could create a Liar Paradox in them. And therefore, since it was earlier believed that because natural language is even stronger than these languages, we cannot use – Tarski believed this himself – the notion of truth in our natural language, because it is even stronger than this language. This is not the right explanation. The problem was that that Tarski's languages were too weak. So there is no reason left to think that we cannot use the concept of truth in our colloquial language, to use Tarski's term. There are still some problems and

questions, but there is no reason to think that this is not possible, it can be done, I mean, you can give truth definitions way beyond first order languages. I once tried to express this by saying that "true" is no longer a four-letter word.

Knuuttila: At the same time as these things, you also developed this so-called independence friendly logic. It is considered really very important and a big event in the history of logic and discussed in many papers in the volume. I think that many people have difficulty in understanding why it is called independence friendly. You would yourself say in your autobiography that you are not quite happy with independence-friendliness terminology.

Hintikka: That was actually a bad terminological judgment. The reason I used the term was that in that kind of logic, that kind of language, you can express relations of independence that are not expressible in the ordinary old-fashioned first order languages. But it is not that there are deeper relations that also could not be now expressed. So it is both independence friendly and dependence friendly logic. The big mistake was to give it a special term for independence-friendly logic is in reality *the* logic, it is the true first order logic, while only liberated from the restrictions that were unfortunately put on that kind of language by the earlier logicians. It should be called simply the first order logic. That is what it is. Instead, we should give the old traditional first order logic a special name, maybe "dependence handicapped logic" or "independence challenged logic".

Knuuttila: OK, that's only name. Actually it is now called independence friendly logic in any case, so that you won't be able to stop the name any longer –

Hintikka: What I suggested once is calling it neo-classical or hyperclassical logic.

Knuuttila: Neo-classical may be nice. One more question about Wittgenstein. You are not happy with neo-Wittgensteinians. You are criticizing them in some of your answers and also in your autobiography. Why are you so critical of some philosophers who are doing Wittgensteinian philosophy, perhaps not the mainstream, but at least some Wittgensteinians?

Hintikka: First of all, the reason is that they are all wrong. I am not saying they are always wrong but often they are actually wrong and I can illustrate this by making some specific points. Unfortunately, I have to make them rather briefly. Let me take three different main lines of approaching Wittgenstein, interpreting Wittgenstein. First, I think one of them centers on the idea that for Wittgenstein language is in an everyday sense a social phenomenon. Well, in that sense language is a social phenomenon, but not for Wittgenstein in the specific sense that having a language should conceptually speaking presuppose a language community. Wittgenstein did not believe that. And the proof is that he says so. The proverbial

expression of a person without society, outside any linguistic community, is of course Robinson Crusoe. And Wittgenstein repeatedly says that Robinson could have a language. So that line of interpretation is off the mark. Well, what else? Take the Wittgenstein discussion of rules. What is his problem? He tells us in the *Blue Book*. To follow a rule is not just to act in accordance with it. In Wittgenstein's semantics, there cannot be any "action at distance". So how does the rule guide my action? Wittgenstein's problem is not following a rule but being guided by the rule. It is the same problem as how the blueprint of a machine determines the motions of the machine. And it has nothing to do with my knowing what the rule is. It has nothing to do with whether I know what the next step would be. It is not an epistemological problem. It is the problem about the mechanics of language, so to speak. This point I can illustrate with what Wittgenstein says about computers. Contemporary thinkers might ask, "Does a computer think?" Wittgenstein asks instead, "Does a computer *compute*?" So the whole line of interpreting the rule following discussion as a bunch of epistemological problems is completely off the mark. We already saw how to interpret the inexpressibility, which the so-called Neo-Wittgensteinians make so much about. I already referred to Wittgenstein's identification of his problem of inexpressibility with the general problem of the expressibility of semantics. It is maybe the real problem, and in Wittgenstein's negative view is very interesting and shared by many others. It is a very interesting philosophical view, historically and philosophically. It leads Wittgenstein into genuine, very interesting questions because then the question is "What are the basic semantic relations that we cannot express in language?" But it was not even an original question, in no sense original with Wittgenstein. In Russell's theory of acquaintance, when he used the theory of acquaintance as a basis of a language understanding, his theory came down to the idea that to understand a proposition, you had to have acquaintance with the ingredients of the proposition. But what about the objects of acquaintance? I have to have them before I understand anything. So you cannot say anything nontrivial about them, however they exist because that presupposes that you already have them, but then they exist automatically. You cannot define them because then you can say that they exist. So Wittgenstein simply takes this over from Russell. It is a very interesting view. But it does not by itself lead into any kind of unwritten philosophical truths the same way as for instance in Plato. Well, but as regards the New-Wittgensteinians, one objection is that their scholarship is simply unacceptably sloppy. James Conant has referred to the letter I mentioned claiming that it proves his point. Then he says that in this letter Wittgenstein claims that Carnap misunderstood him. If you read the letter, you find it a little terse, but if you see the context, the context of the controversy, and if I may make it a little bit more colourful than it is, you find that what Wittgenstein is saying, is that even poor stupid Carnap could not have misunderstood the *Tractatus* so badly as not to see that Wittgenstein's, theory is the same as Carnap's idea of the formal mode of speech. Hence Conant is making precisely the mistake that according to Wittgenstein even Carnap could not have

made. Perhaps I should not say this but I have somewhat uncharitable lines about the neo-Wittgensteinians. You may remember the Oscar Wilde's characterizations about the great British traditional foxhunt. Wilde's line is "the unspeakable pursuing the uneatable". Now I could say the New-Wittgensteinians are the unspeakable pursuing the unspeakable.

Knuuttila: Philosophers are pretty good at finding jokes about each others, even in ancient times. You are now also not on very friendly terms with Kripke's theory of names, meaning and reference. On the other hand, you were one among the main figures developing the new modal semantics and modal logic and the so-called possible worlds semantics, even though you did not like this name for it. But then you wrote several papers in which you wanted to distance yourself from Kripke's version of the possible world semantics. What was the big difference between Kripke's approach and yours in this context?

Hintikka: This question is very good because it enables me to make one point which is extremely important. I think the problem with Kripke is that he never internalized the idea of possible worlds in the sense of alternative realities, because this idea implies that you can only know the particular world you are in. And therefore all these semantical relations are subject to the same restriction. So, for instance, in Kripke's theory of naming by dubbing, dubbing is an event in our world. It does not carry to any other world, any other scenario. If you move Kripke to another world, neither he nor anyone else would then have known about the dubbing. The objective counterpart of this is that the relations of ordinary reference do not determine relations of identification. Kripke saw one very important thing, that we have in our language semantic relations that do not relate or reduce to precisely descriptive terms. And what they are, are precisely what are needed to carry the identification relations from one world to another. So, in our actual semantics, we have to have two systems, we have the reference system that works descriptively, we have the identification system that works in some other way. It is largely independent of the reference system. For instance quantifiers rely on the identification system, not on the reference system. Here the philosophical logic, philosophical logicians and philosophers, epistemologists, missed the tremendous opportunity, because this difference between the two systems plays a major role in the human information processing in the central nervous system. It is implemented by two different neural systems. The different manifestations of the damage to one system or to the other one are absolutely striking. This is a major aspect in neuroscience. I first became aware of that a long time ago when I was talking to a neuroscientist and she wanted to explain their problematic to me. She described this difference between two kinds of visual systems. After ten minutes I said, you are preaching to the converted, this is a special case of my theory of two modes of identification. This neuroscientist did not believe me at first. One of her objections was that how can it be, your distinction is merely logical, semantical, ours is a real distinction

in the central nervous system. It is not even merely functional. There are two different centers in the brain implementing the two systems. That is fine, I said, then I will be the first philosopher since Descartes proving his theories by anatomical evidence. Well, but this agreement is absolutely striking. There is a major opportunity here for philosophers to make an actual contribution to the methodology, to the foundations of an important science. So I think Kripke simply failed to see this, this difference among other things. This is missing from his semantics.

Knuuttila: Game-theoretical semantics was something you were developing in interesting works in the 80's. Some philosophers were nervous of this because they had just learned the new modal semantics, and it was not so easy. Some were disappointed that now one had to learn game-theoretical semantics, which was also applied to linguistics. It was a demanding task for philosophers who are often not all that eager to learn new things. After game-theoretical semantics, they should then start to read independence friendly logic and so on; you have kept them busy. But the game-theoretical semantics was also related to your views of the logic of questions and answers, which seems to be a pretty important part of your philosophy in general. You have written and continue to write and think about this. Do you think this topic is making progress in contemporary philosophy, or is there something preventing it?

Hintikka: The basic idea of game-theoretical semantics is very simple. I think this is also Wittgenstein's idea although he dealt with it in another way. The basic question here is, What do the semantical relations between language and reality consist in? Are they somehow intentional relations here or causal relations? I think the Wittgensteinian answer is the right one. They consist in certain rule governed human activities. The term "language game" was for Wittgenstein simply a word for these activities. What I did was to take Wittgenstein more literally than he intended himself and said, What happens if I apply some of the simplest basic ideas of mathematical game theory to these games? As it turns out, this is extremely useful way of dealing with semantics. I do not think that there are any limitations here. This has led to all sorts of developments even beyond what has been published about independence friendly logic or anything like that. There are further developments in the works, very important. However, I think the story is somewhat different with the questions and answers. There the right way of looking at the logic is rather epistemic logic whose semantics goes back to the possible world semantics. But I think the situation can be described very easily. What is going on, is simply the oldest approach to epistemology in the Western philosophy, the Socratic method of questioning. It has played tremendous role in the history of philosophy. Plato was so impressed he made the questioning games the cornerstone of his philosophic training in academy, Aristotle made it the universal way of finding out all the basic truths of science. It is even more important in Aristotle than people have realized anyway. Simo and his students have done pioneering work on the

important role of the questioning games in medieval philosophy. Gadamer has made what he called logic of questions and answers a kingpin of his hermeneutical approach. Thus questioning has been repeatedly emphasized very strongly, but it has never been fully systematized. There was never a really close theory about this, contrary to the expectation that surely the possibilities of this marvelous idea should have been exhausted long ago. What is the reason? The reason is that the logic of questions and answers was not really a logic. It was simply bunch of suggestive ideas. The basic concepts in that really give us a real logic of questions and answers have been adequately defined only very recently. So for the first time we have a way of really seeing what is involved in the Socratic method. Collingwood, for instance, talks a lot about presuppositions of questions. Yes, but what are they? How do you define the presuppositions of questions, or even the question-answer relation explain. What precisely is this relation? What is it for an utterance of a proposition to be answer to a given question in the sense of a full or conclusive answer? Now we have answered those questions. I think this opens tremendous possibilities of further development. I tried to do something myself and any day now there will be a book of essays on epistemology starting from these ideas coming out by Cambridge University Press. In this way, for instance the problem of induction is put to a new light, for instance, take experimental induction. The scientist varies the control variable and sees how the observed changes accordingly. And if it is a good method of measurement, he ends up with beautiful curve on a graph paper. Is this an answer to the question to how the one variable depends on the other? It is not fully, it contributes to an answer, but it is not a conclusive one before you know what mathematical function is represented by that curve. So the problem of experimental induction has two components. It is, so to speak, filling the curve of observed value more and more fully. There are techniques of curve fitting and so on, but it also involves the question, the problem of finding out what curve we are getting mathematically. And there are inevitably two components in actual work, sometimes one of these component problems is easier to solve than the other one. Sometimes you realize very quickly what the curve is like. Then the problem becomes simply estimating the parameters. But in some cases the physicist cannot tell what mathematical function he has uncovered, because mathematicians have not studied the function yet. So he so to speak goes to the mathematics department and tells the mathematicians to study these curves. This is actually one of the main ways in which mathematics has developed. Physicists have taken them the problems that involve new functions that they had not studied before. So now you can see this whole problematic in perspective. Then you see this is simply what follows from the logic of questions and answers. It inevitably leads to the trivialization of the difference between these two components of the task of induction.

Knuuttila: Thank you, those are very interesting ideas. Let's ask if people who are present would like to ask some questions. We do not have much time.

Stadler: I have one question relating to your answers during this fascinating talk: What do you see as the task of philosophy, the relation of philosophy and science? I understand that you are not really an admirer of Quine but he sometimes mentions that philosophy of science is enough philosophy. What would be your response?

Hintikka: I have two different answers. I think philosophy comprises a huge collection of different kinds of things. I am not ruling out anything. But I think this query is very closely related to our symposium. Sometimes I think that philosophy and the foundations of science are likely to play a crucial role in philosophy, without thereby excluding anything. Perhaps I can make this point by asking: Why did the influence of the Vienna Circle slowly come to an end? You can also point to all sorts of historical reasons, for instance that the Vienna Circle members had to emigrate and so on. But if we take simply the intellectual question and if I may oversimplify, caricature the situation, we can ask: What did the Vienna Circle people promise to do in the philosophy of science? They promised to solve all the problems in the foundations of mathematics and in the foundations of science by means of logical syntax of language or more generally by logical, semantical means. Did they? No. But, indulging in counterfactual speculation suppose that the Vienna Circle had carried out Hilbert's program and solved all the interpretational problems of quantum mechanics, what would have happened? I am tempted to say, we would all be logical positivists. Let me say one more thing. It is very important. Now, did they fail because they used too much logic? No, they used too little logic. So, I think, without putting philosophy or science on a pedestal in any way, that it may be very crucial that philosophers do not miss opportunities of solving these foundational problems of science. They are not the be-all, end-all of philosophy but they may be the test cases of what philosophy and what different methods can do.

Knuuttila: We opened this discussion with the question of what should have happened in the Vienna Circle to make us all logical positivists. Now you have answered this. Thank you very much for the great number of interesting philosophical ideas, as always, when you give lectures or discuss questions.

Gerald Holton

On Unity and Disunity in the Sciences: Variations of ancient Themata

I

I feel honored to be asked to speak at this university where so many ground-breaking scientists and philosophers were students or teaching, spreading their message world wide and I am especially glad to have been asked to come by the Institut Wiener Kreis, of which I am proud to be a member, and whose splendid work for two decades and to this day is being carried out vigorously under Professor Stadler and his colleagues. Through that, a bright flame is being kept shining. That has its own salience. But I firmly believe, as you will hear later, that at just this time such studies have additional purpose, force and inspiration, in academe and society, as well as in global policies that are now under our very eyes. All these contain an urge to bring about a new version of a unifying *Weltauffassung*. If that succeeds, historians of the future may well say that there was a certain pre-established harmony between the original Vienna Circle program, and what is now being done here, and a new, better world.

Let me add two remarks about why being invited to speak here today is special for me. You have often seen the large, elegant building at the corner of Schottengasse 10 and Schottenring. One of its high balconies were part of a *Kanzlei* of an attorney, specializing in international law, who had got his degree in jurisprudence right at this university, nearly a century ago. When his older boy visited there and looked out from that balcony, he could see the university where he hoped to study one day.

Secondly, I think I may be one of the last persons who had the privilege of being intellectually influenced to a large degree by the remnants of the Vienna Circle which came, as a result of the persecutions in the 1930s, as refugees to the United States and started there a new branch, pursuing the old dream. On this aspect of the *vertriebene Vernunft*, much has been written. But let me single out one of these displaced intellectuals, Philipp Frank. He had previously studied and taught right here, then was for 26 years successor to the Mach-Einstein chair in Prague, coming to the meetings of the Wiener Kreis as often as he could.

Of Philipp Frank, to whom I shall refer again, Herbert Feigl said that his work

combines informal logical analyses of the sciences with a vivid awareness of the psychological and social-cultural factors operating in the selection of problems and in the acceptance or rejection of hypotheses … In a sense, this is a genuine sequel to the work of Ernst Mach.

Professor Stadler has called Frank "one of the most important figures for the transfer, transformation and the further development of the Central European philosophy of science." Frank, a student of Boltzmann, who also knew and consulted for Ernst Mach as a young *Privatdozent* in Vienna, at that time participated, as you know well, in a series of generating events that eventually led to the formalization of the Vienna Circle, in which Frank became an active member.

In late 1938, Frank came to Harvard University at the invitation of its Physics Department, lead by P. W. Bridgman (who, from 1945 on, supervised my doctoral thesis on experimental high-pressure physics). Bridgman was a crucial initiator of the Wiener Kreis re-established on the East Coast of America. For about a dozen years, all three of us were in the same building. While still a graduate student, I was lucky to be asked by Frank to be his teaching assistant in his Physics and Philosophy courses, I shared his office, then became his colleague, and was asked by Frank to be the Secretary of his Inter-Scientific Discussion Group and of his Institute for the Unity of Science.

Let me give you a taste of that heady atmosphere, during those fourteen years, longer than the official years in Vienna. In the next building was Richard von Mises, who never forgave me for turning down his request to translate his Positivism book manuscript into English. Also nearby were other sympathizers such as Van Quine, S. S. Stevens, E. G. Boring, Joseph Schumpeter, Gottfried Haberler, Harlow Shapley, Gyorgy Kepes, Wassily Leontief, Norbert Wiener, etc., later B. F. Skinner, visitors such as Ernest Nagel, Charles Morris, and many others. With all their differences, they agreed with one another, and even with Plato, that clear ideas drive out fantastical ones.

I was happy to get to know all of these scholars during our monthly meetings of the informal group, devoted to "fruitful mutual inspiration." As the saying goes, I imbibed some of the Vienna Circle ideas with my mother's milk. And I was especially attracted to their program, continued from the original Wiener Kreis, of trying to bring harmony between the philosophical, historical, and sociological contexts of science. Indeed, their ambition was already to be found in Franz-Serafin Exner's famous *Rectoral Rede* of 1908. I mention this because that address had been of great interest to young Philipp Frank. He referred to it not only in his *Kausalgesetz* book, twice, but again in his late years in the unpublished Oral History interview, kept at the Center for History of Physics in America.

II

Now to the main body of my talk. But at this point you might expect me to clear up a seeming paradox. I have often written, in addition to the conventional parts of doing scientific research, also about the visual imagination in science, the metaphoric imagination, the aesthetic elements, and above all the thematic imagination, all of which are close to what Einstein once called his "irrational" parts in the pursuit of ideas, at least in the early stages of one's work. Put another way, at least the circle around Hans Reichenbach would not have approved of my interest in the context of discovery.

The seeming paradox I mentioned is that people in the transplanted Vienna Circle atmosphere on the East Coast of America did in fact not oppose my ideas, but rather supported them. Instead of looking for doctrinaire followers, they in fact, in their new environment, looked for extensions of ideas in new directions. They were generous and *neugierig*. And in any case, one had to accept the existence of the thematic analysis—which has some precursors such as Francis Bacon and passages in Karl Popper (Logic, 1959, 238)—since it springs out of the historical study of the documents and archives and publications of scientists in specific cases.

The hold of a thema or its antithemata, each with its saving flexibility, such as evolution or its opposites, devolution or steady state, or atomism and anti-atomism, discreteness or the continuum, and other such *Dauerthemata*, can have an iron grip on the imagination of the scientist, often without proof or even despite contrary experimental disproof. But without some necessary presupposition, scientists could often not even know how to start. Einstein called his persistent presuppositions "categories in the non-Kantian sense," since they are not a priori, but essentially subject to disproof or uselessness. In his Autobiographical Notes he celebrated the "constructive speculative character of … scientific thought;" but then he became a prime example of the hold, to the bitter end, of the thematic concepts of the continuum (the field) and of causality in the strict sense. One cannot imagine that a Parmenides could persuade a Heraclitus, or vice versa. Similarly, as Erwin Panofsky has pointed out, though Galileo of course knew of Kepler's laws, he insisted on the more clumsy and less accurate circular motions as explanatory of planetary motions—a thematic presupposition aided by his aesthetic sense.

Although originally little had been written or studied on thematic origins of scientific thought, we should not have been surprised to find their pervasiveness. Our daily life decisions are made frequently on the basis of presuppositions that may turn out to be useful or not, wise or not. Our very language is bifurcated, constructed to a large degree by synonyms and antonyms, as Peter Roget's *Thesaurus* shows (analysis vs. synthesis, etc.), in historic studies (cyclicists such as Spengler vs. linearists), in politics (nationalists vs. cosmopolitans), in religion (fundamentalists vs. secularists), eros vs. thánatos, in music (classical vs. romantic; but in

each of these cases, each portion is itself manifold—think of the vast differences between Beethoven, Schubert, Brahms and Mahler, yet all with underlying romantic structure, as even sometimes in the case of Mozart, e.g., K. 516, 3rd movement), in geography (even a *Großstadt* vs. its greatly varying *Bezirke*), and of course in science (Copenhagen interpretation vs. Schrödinger; causality vs. probability and the whole Exerei, including Philipp Frank's work on limits of causality; or Chicago-based Millikan's allegiance to unitary electricity versus Vienna-based Felix Ehrenhaft's variety of subelectrons)—or even what is thought the world is made of—particles and antiparticles (and eventually perhaps unparticles).

Rarely do scientists switch from major preconceptions to the opposite. But it happens. For example, Max Planck said in his 1910 Königsberg lecture, that to change from one working hypothesis to another, very different one, requires a change in one's whole world view, although he admitted in 1913 that this comment itself is only an unprovable hypothesis. No doubt Planck's opinion was colored by the fact that he himself famously switched around 1908 from positivism to metaphysics. Another very familiar case is that of Wilhelm Ostwald. As his biography stresses, he was not alone in being reluctant to accept Daltonian atomism—until his conversion toward it.

And I cannot help but return to one of the most profound changes of a fundamental presupposition, the one by Johannes Kepler. We can pinpoint it to February 10, 1605, with immense consequences for the physical sciences. There he writes to his friend Herwart von Hohenburg,

> I am much occupied with the investigation of the physical causes [of planetary motions]. My aim in this is to show that the celestial machine is to be likened not to a divine organism, but rather to a clockwork …, in so far as nearly all the manifold movements are carried out by means of a single, quite simple magnetic force, as in the case of a clockwork all motions (are caused) by a simple weight. Moreover, I show how this physical conception needs to be presented through calculation and geometry.

With this change of mind and of spirit, Kepler can be said really to have started the modern phase of physical science. We can forgive him that he thought gravity was caused by magnetism, a subject very fashionable at the time, and no great fault, the more so as we don't really know yet what causes gravity.

Here one may well ask how the relatively few and rather stable themata have survived in science from antiquity to this day, and are still such strong guides and motivators. That is a large subject of its own, but here I can only point to a possibility—that the themata and antithemata need one another, that they are in symbiosis. Something like it was already considered by the great *Naturphilosoph* Hans Christian Oersted, who wrote:

> [We] have always a tendency to combine the phenomena and to discover their analogies; another class, on the contrary, employ all their efforts in showing the disparities of things. Both tendencies are necessary for the perfection of science, the one for its progress, the

other for its correctness. The philosophers of the first of these classes are guided by the sense of unity throughout nature; the philosophers of the second have their minds more directed towards the certainty of our knowledge. This conflict of opinions keeps science alive, and promotes it by an oscillatory progress …

A final theoretical point about themata: Superficially they can change in time, but at base they do not. Change itself would serve as an example, as it is de-allegorized and re-allegorized over centuries and decades. If I had time, I would remind you of the various stages, from Aristotle onward, where the concepts of change and motion are still entangled with each other, motion being one change, equally positioned with alteration in quantity or quality, generation or corruption. There followed at a distance the geometrization of motion at Merton College and by Nicole Oresme, the arithmetization of acceleration by Galileo, and on and one through Minkowski and Feynman's virtual particle exchange. But also Ernst Mach's experiments on *Bewegungsemfindungen* (1875), which, to my surprise, has recently been translated into English in order to be used in the training of American Astronauts—thus taking Mach's ideas up into Heaven, and illustrating the manifold versions possible of an underlying ground motive. With this variety in the unity of a thema we have come a long way from its beginnings, and witnessed the alternatives within the underlying ground motive.

III

A similar fate befell two important thema-antithema couples that I wish to present here, as well as to illustrate the often violent opposition between the devotees of thematic alternates. First the Absolutism and its main alternative, Relativism, each with its own *Einheit und Vielheit*, and finally the old dreams of *Einheit und Vielheit* themselves—all these of great antiquity as well as current excitements.

When we mention the majestic concept of the Absolute, of course the figure of Newton arises, and also of Immanuel Kant, for whom it was the first of all categories. A nearly infinite amount has been written on this matter, but I want to draw attention in this setting only to Newton's choice of absolute space and time. For me, the briefest and truest observation on the subject was the observation of A. Rupert Hall and Marie Boas Hall, namely that just as Newton knew well that the cause of gravity was God, "preferring God to Leibniz," Newton also knew that absolutes of space, time and of motion referred to the properties of events in God's "Sensorium"—the cut-off point beyond it was not possible to ask further questions. Here one can't fail to recall Joseph Needham's opinion that the Scientific Revolution happened more easily in the Monotheistic West than in the East, to which Robert K. Merton's work added the influence of the pious British Puritans on the rise of 17th-century sciences.

Newton's very private theologizing of physics reaches of course back to the position of scholars in medieval times and before. But while Newton's silence helped in the mechanization of the word picture, Ernst Mach smelled out the metaphysics behind Newton's mechanics, when Mach famously called absolutes of space and time "monstrous conceptions". Mach's long attack on these conceptions brought his ideas into the cultural conversation of the time, and resonate to this day. In countering his opponents, Mach lauded what he called "relativists," from Stallo to Petzoldt and Pearson. As we know, we can include Einstein, who as late as August 1909 signed a letter to Mach with the words, "Ihr Sie verehrender Schüler".

Mach's influential relativism was to some degree a counterattack on one of the strands within early absolutism in physics, the theological and metaphysical aspects, as well as ideological elements that underlay the mechanical-physical one. And this combination of force, on each side, can be discerned in more recent debates on this dueling place, which, as you will see, extends even to the heights of church and of state.

The attacks of Einstein's ideas on relativity from 1920 on are familiar to you. One such dueling place was Germany, from about 1920. For example, a so-called Working Party of German Scientists for the Preservation of a Pure Science, held a raucous meeting in the Great Hall of the Philharmonic Society in Berlin on 24 August 1920, attacking Einstein and his theories. Einstein, calling it the "anti-relativistic association," was so appalled that he toyed with the idea of leaving Germany then, the more so as other such meetings soon followed. After 1933, this enmity to Einstein and his work became of course part of government policy. It was declared at the highest level that science must be understandable by the ordinary folk, because otherwise it "undermined people's instinct of nature." Most German scientists were of course appalled, but in teaching or in articles, whenever they had to refer to relativity theory, they had to deflect credit for it to ideologically more acceptable scientists such as H. A. Lorentz, Henri Poincaré, and Friedrich Hasenoehrl.

To be sure, politically and ideologically based opposition to relativity theory and to Einstein was also present in the Soviet Union, and in China during Mao's regime. Evidence of rebellion against modern science flares up also in the USA. As one example among many, a widely read journalist, John Horgan, prophesized and urged "The End of Science" in his book with that title in 1996. And since then, in a long essay on this theme in the *New York Times*, John Horgan celebrated "commonsense" as a desirable alternative to much of modern science. The unforgivable sin of relativity theory, he said, was that it "shattered one's common-sense notions about how the world works."

There are and have been more violent appearances against Anti-Absolutists in America. As the books by George Reisch and Deborah Coen document, ironically, Philipp Frank, a refugee from fascist totalitarianism, in his late years in America became the victim of J. Edgar Hoover's FBI, as did Rudolf Carnap, not least for

their liberal use of such terms as "Unity" and "International," terms suspected of being keywords of left-wing totalitarians.

Among scientists, antagonists against relativity theory have today shrunk to a mere handful. But there is now, and has been for a long time, a quite different set of disbelievers, not just in relativity but in the related thematic notion of *relativism*. I now turn to them.

IV

The complex responses of some theological authorities to new scientific thought is a well known story, starting long before the dispute between Galileo and the clergy. But when Einstein's relativity theories eventually became widely admired, not just by most scientists, but—to Einstein's ever-lasting puzzlement—also by the general public, various *theologians* became concerned to what extent that theory and its general point of view might infringe on the authority of the church. This came to Einstein's notice in an unexpected way in June 1921. When he was returning to Berlin from his first trip to America, he stopped in England. There, the Archbishop of Canterbury, the head of the Anglican Church, had been concerned about the possible consequences of relativity theory for religion. Dutifully, he had been trying to read several books on relativity. However, that conscientious task had, according to a friend of his, only driven him to "a state of intellectual desperation." So he asked to meet Einstein during his stay in London. A dinner was arranged. The Archbishop was placed to sit next to Einstein, and asked him quite bluntly what effect relativity would have on religion. Einstein answered simply, "None. Relativity is a purely scientific matter and has nothing to do with religion."

This response reassured that theologian. But others were not so easily satisfied. Among them was the prominent Cardinal O'Connell, Archbishop of Boston. In one of his speeches, he put his finger on an old, powerful and continuing accusation by some clergy that relativity, like evolution before it, was to be rejected because, as he put it, "they were mainly materialistic and therefore unable to stand the test of time." He added that Einstein's theory was a "befogged speculation producing universal doubt about God and His creation," implying "the ghastly apparition of atheism."

While Cardinal O'Connell's judgment was typical only of a small segment of the clergy, I want to look briefly at the severe unease felt by two other theologians who responded to relativity and its implications from a more sophisticated base.

The first of these is Paul Tillich, one of the most intellectually engaged and widely admired persons in his field in the twentieth century. In 1933, after having been dismissed from his position at the University of Frankfurt, he came as an exile to the United States, and for many years was a colleague of mine at Harvard University. He was a spellbinding teacher and extraordinarily productive writer.

In 1928, he had met Einstein and heard him lecture, and became interested in the very question that had puzzled the Archbishop of Canterbury, namely, what consequences Einstein's ideas and point of view might have for religious thought.

Tillich's early position was in fact along the lines of Einstein's reply to the Archbishop. Writing in his book *Dynamics of Faith*, Tillich stated

> Scientific truth and the truth of faith do not belong in the same dimension of meaning. Science has no right and no power to interfere with faith, and faith has no power to interfere with science. One dimension of meaning is not able to interfere with another dimension.

But as time went on, Tillich became more and more alarmed by what he thought Einstein was saying and writing. Between 1930 and 1948 Einstein had published several widely discussed articles on religion and science, a project by which Einstein was inventing his own religion, namely "cosmic religion," just as he had invented his science. In one of these essays, Einstein explained that the concept of a personal God was an anthropomorphic remnant of primitive times, of a "religion of fear." This primal urge, he advised, had to be abandoned in favor of a Spinozistic feeling of awe and sense of wonder at the rationality and beauty of the universe. Moreover, as a believer in strict causality throughout the universe, very much in the sense of Newton's physics, Einstein said he could not "entertain for a moment … the idea of a Being who interferes in the course of events," such as causing prayers to be answered and miracles to occur. Einstein concluded that "serious scientific workers are the only profoundly religious people."

By 1940, Paul Tillich had enough of that. While he did not mention relativity theory explicitly, Tillich thought, like the two Cardinals I mentioned before, that Einstein's ideas, especially because of his world-wide fame, might constitute a challenge to a key doctrine at the base of the authority of religion. Thus Tillich issued a manifesto in 1940 entitled "Science and Theology: A Discussion with Einstein." His sharp attack ended with the statement, "As the philosopher Schelling said: 'Only a person can heal a person.' This is the reason that the symbol of the Personal God is indispensable for a living religion."

So much for Einstein's cosmic religion. But Tillich also sensed that behind it there loomed a powerful viewpoint embodied in Einstein's relativity theory, and that had to be dealt with also. So toward the end of his life Tillich wrote a book with the straightforward title *My Search for Absolutes* (published in 1967). There he closed in on what was to him the most dangerous enemy. Tillich was writing, as he put it, "out of a feeling of uneasiness—uneasiness about the victory of relativism in all realms of thought and life today … a total victory. … The sea of relativities…threatens to overwhelm us." What Tillich called "the great spectacle of scientific relativism" was now, in his view, invading contemporary philosophy, ethics, and most vexing of all, religion itself through the "secularist criticism of religion."

Against all of this, Tillich said that he stood for "Absolutes." Absolutes make language possible, understanding possible, truth possible. They are at the bottom of the "moral imperative." Indeed, "The experience of the Absolute-itself is the experience of the holy, the sacred." I regard these declarations of Paul Tillich, with which Newton might have agreed in his heart of hearts, as the best succinct confrontation of Absolutism against Relativism.

V

In 1950, there appeared a book important for our story (a German edition appeared in 1952). The author was Philipp Frank. His book of 1950 has the challenging title, *Relativity: A Richer Truth*. Frank certainly knew about relativity. When still in Vienna after getting his doctorate in 1907, Frank wrote many of the earliest technical publications, explaining the special theory of relativity to physicists. I need not here refer to the relativism and pluralism of the other members of the early and later Vienna Circle, only to remind you of Professor Stadler's work on this, for example his remark (in his article, "History of the Philosophy of Science …" 2007) that "Neurath remained very skeptical of explanations on the basis of one method and one image of science without pragmatically relativizing the field of 'Prediction and Induction' (1946)." And again: Neurath defended Logical Empiricism as "Through and through 'Pluralist.'"

As to Frank's book of 1950, he distinguished in it between two opposites: on the one hand, "*relativism*," which he defined as a "common form of thought … frequently regarded as an effect of the allegedly exaggerated role science has played in modern thinking," and on the other hand, "*fundamentalism"*, or the belief in absolute values. He then made the important distinction between relativism *within* science, which he celebrated, and on the other side, relativism *outside* science, which Frank called a largely illegitimate translation of the concept from science to ethical or social beliefs—*with one exception*.

Indeed, the whole short but fervently written book by Frank is about that one exception, the one legitimate transfer from relativistic science to the other parts of culture and life. Thus, Frank argues, in science one learns that

> a statement of the type "this table is three feet long," without also mentioning a system of reference, is a statement too poor to describe adequately our experience about length. We need to use a richer language, one that contains statements of the type "this table is three feet long *relative to the coordinate system of this room*,"

and not with respect to some other, relatively moving coordinate system, because there the length measurement of the table would be different, perhaps only 2½ feet.

In the same way, Frank continues but somehow again hints at a theological subtext. He writes: "A man who declines to answer by a flat Yes or No the question: 'Do you believe in God?'" should not be called a skeptic or relativist or agnostic, because the question, Does a person believe in God, depends on what kind of life this person leads, so as to distinguish between the *effective* parts of his religion from the merely *verbal* ones. Similarly, terms such as freedom, democracy and the like make sense only in terms of their operational meaning, just as the length of a table depends on the specification of the reference system within which the length is measured. In short, Frank's book was yet another attempt to persuade, by skeptical rationalism and empiricism, those who preferred absolutism and relying on revelation.

Frank's book is also unusual in that it has a remarkable Foreword, an essay by Albert Einstein. Einstein's title is nothing less than "The laws of science and the laws of ethics." (Some books published by the Verein Ernst Mach had similar titles.) Science itself, Einstein reports, has properly nothing to say about values, purpose, emotions and the like. "It cannot produce ethical directives." However, he continues, there are two ways in which science is related to ethics. One is that the habit of logical thinking and the search for empirical facts are important and useful, both in science and in other fields. Frank's book was of course largely devoted to urging that point. But the second relationship, Einstein continues, is that the *mechanism* by which true and useful results may be obtained is the same, both in science and in ethics. That is, in both fields one should start with fundamental premises or propositions, and from these, other propositions can be derived by logic, both in physics and in ethics.

In fact, in this Foreword, Einstein was transferring to ethics his own favorite method of theory construction in science. For example, Einstein had jumped at the very start of his relativity theory paper to the two principles or axioms of relativity—generalizing Galilean relativity to all of physics, and the constancy of light postulate—and then deriving almost everything else from them.

Now, in his Foreword to Frank's book, Einstein advised the same method for finding answers to ethical problems, by setting up first a set of ethical axioms and deducing their consequences. But of course there is a difficulty, so Einstein asks, "what is the origin of such ethical axioms?" From the logical point of view, all axioms are arbitrary, and there are infinitely many of them. How to choose the right ones? Here, Einstein continues happily, there do exist "inspired individuals," who can be the source of "comprehensive" and "well founded" ethical axioms. Elsewhere Einstein named three such inspired individuals: Moses, Jesus and Buddha—all together.

In the last two sentences of his Foreword, Einstein subjects the supposedly inspired ethical axioms to *tests* for their actual effectiveness in practice. He writes: "Ethical axioms are … tested not very differently from the axioms of science … *Truth is what stands the test of experience.*" With this, Einstein essentially repeated Carnap's 1928 test of *Prüfbarkeit* of ethics, and also anticipated the chapter on

ethics in the book by von Mises. One may say that these declarations of Einstein are the best succinct confrontation of a form of the thema Relativism against a form of the antithema Absolutism.

Let us take a moment to look at the definition of *Relativism.* Since the early 20th century, relativism more often than not was held to be a point of view, both in science and outside, fostered by Relativity theory. But the word relativism, which came into the English language in mid-19th-century, connotes a much older point of view. As the *Oxford English Dictionary* puts it, in relativism knowledge is only of relations; therefore "truth, morality, etc., are relative to situation and are not absolute." Thus, meanings can fluctuate from place to place and time to time. The allegory can be reallergorized.

At the extreme, some non-scientific postmodernists hold that all beliefs are equally valid, since they are all "socially constructed." For example, the widely read Professor Stanley Fish, then at Duke University, famously wrote that the laws of nature which scientists deal with are just as arbitrary as the rules of a sport, of baseball. One might well say that this is one version of relativism run amok. Even Bruno Latour, once one of the icons of postmodernism, quoted this example in his famous mea culpa article (*Critical Inquiry*, 30, Winter 2004), in which he accused himself of having for a long time undermined the scientific *Weltauffassung*.

A far more profound and important indication of the differing claims of authority came to a head in Rome. According to the official Vatican transcript, on Monday, 18 April 2005, while delivering a Homily in the Vatican Basilica in St. Peter's cathedral, His Eminence Cardinal Joseph Ratzinger, the Dean of the College of Cardinals and for twenty-three years Prefect of the Congregation of the Doctrine of Faith, quoted St. Paul's warning not to allow oneself to be "tossed here and there, carried about by every wind …" The Cardinal gave examples of such tossings, but concluded the list with the one example that he evidently held to be the most dangerous one today: It is, in his words, "Relativism…the only attitude that [it is believed] can cope with modern times." He continued, "There has been building up [*Es ensteht*] a *dictatorship of relativism* that does not recognize anything as definite …."

It will obviously be fascinating to watch the consequences of these challenging views, the more so as on April 15, 2008, almost precisely three years after becoming Pope, Benedict XVI was greeted by President George W. Bush on arriving in the USA for his visit, with the President's speech warmly endorsing the Pope's view and repeating the phrase, "the dictatorship of relativism."

VI

I turn now to some recent and current philosophers who also participated in debates about relativism. (I leave aside non-philosophers such as Lenin, who launched in 1908, in *Materialism and Emperio-criticism*, his attack on relativity theory as Kantian idealism, and also Oswald Spengler who published in 1918 that the rise of relativity theory is part of the death of Western culture.) Thus, Hilary Putnam, in his book *Reason, Truth and History*, attacked some of his fellow scholars to be relativists, including Richard Rorty and Paul Feyerabend. Rorty responded in his book *Objectivity, Relativism, and Truth,* by dismissing the unacceptability of relativism in the following, somewhat contorted passage.

> "Evidence" is not a very useful notion when trying to decide what one thinks of the world as a whole. Such an admission only looks relativistic if one thinks that owing to the lack of general neutral antecedently formulable criteria for choosing between alternative, among equally coherent webs of belief there can be no 'rational' decision. Relativism seems a threat only to those who insist on quick fixes and knock-down arguments.

On the other hand, Paul Feyerabend openly flaunted his relativism, famously in his book *Against Method*, where he applied it to science itself. He wrote that there is no such thing as scientific method, a conclusion that has political implications. One of them is that in Feyerabend's opinion the activities of scientists should be subjected to what he called "democratic relativism," which means, for example, that lay people and "democratic councils" should be evaluating the work of the scientists. This is of course close to what has been happening in the United States since 2000, where Congress and the Administration have been trying either to censure or dismiss findings of scientists on such matters as climate change and energy conservation. Feyerabend wrote in the same vein that the belief in rationality "may … be nothing but a pious wish … There is not one rationality, there are many, and it is up to us to choose the one we like the best."

One could spend many weeks among the numerous books that deal, positively or negatively, with relativism. I shall just mention two serious ones. One is by Joseph Runzo, titled *Reason, Relativism and God* (1986). It systematically exposes scholars whom he regards to be relativists in various professions, such as the anthropologist Ruth Benedict, the sociologist Peter Berger, the psychologist Jean Piaget, even the novelist William Faulkner, and of course Thomas S. Kuhn, the inventor of the succession of revolutions in science, each revolution appearing equally plausible, and separated from the previous as well as the next ones by a barrier of incommensurability.

Perhaps a favorite among all these books is that of the venerable art historian Ernst Gombrich, titled *Topics of our Time: Twentieth-Century Issues in Learning and Art* (1991). Gombrich begins by reminding his readers of Goethe's belief in the *universality* of human nature and contrasts it with Hegel's philosophy of his-

tory. On the latter, Gombrich says, "Right at the beginning Hegel formulates the opposite view, which I should like briefly to characterize as 'cultural relativism.'" And Gombrich quotes Hegel as follows: "Every age has such peculiar circumstances, such individual conditions, that it must be interpreted … by reference to itself."

Of course, Gombrich does not deny that, as he puts it, "ages and people differ from each other. We all know that." But

> what makes the cultural historian into a cultural relativist is the conclusion which we saw Hegel draw, that cultures and styles of life are not only different but wholly incommensurable. In other words, that it is absurd to compare the peoples of a region or an age with human being of other zones or periods because there is no common denominator that would offer us a yardstick … [Cultural relativists] refuse to acknowledge any constancy that would enable us to recognize a permanent human nature behind all changing appearances.

Gombrich could have gone further back than Hegel to discover the ancient roots of relativism. Many scholars point to the implicit debate between the Sophist-Philosopher Protagoras and Plato. Robert W. Jordan put it in his book, *Plato's Revolt Against Relativism*, that Plato, especially in his Dialogue *Phaedo*, tried to identify what remains unchanged, universal, absolute, and constant despite all appearance of diversity, "all Becoming and Change, all birth and decay." That universal absolute was for Plato "the immortality of the reincarnated soul." By contrast, perhaps the earliest expression of relativism in philosophy was the famous dictum of Protagoras that "Man is the measure of all things."

Unlike philosophers, historians tend to put the beginning of relativism in history on the shoulders of Herodotus (484–425 BC). While Herodotus still reports myth and religious beliefs with relish, he gives, perhaps for the first time, a desacralized sort of history. The gods of Olympus may still be meddling with mankind, but that is something Herodotus avoids, preferring not to "fall into the traps of the supernatural."[1] Herodotus rather chronicles the "great men, great cities, and great deeds," and is intensely interested in the variety of human behavior. He delights in information such as this: "To some of the Egyptians, the crocodile is sacred, but for some it is not—in fact, they regard it as an enemy." Elsewhere: Among the Lydians, for a person "to be seen naked is an occasion of great shame;" but not so in Greece. In short, as his commentator David Grene puts it, Herodotus wrote, "a kind of universal history; it is the record of all the logical possibilities, political and human, that coexist in the human world." Nothing absolute there.

The important fight in our culture between the claims of relativism and absolutism has gone on for some 2500 years, since Protagoras versus Plato. It is very alive today, and it may never end. Einstein and the Vienna Circle found themselves

1 Quotations are from *History* by Herodotus. Translation by David Grene, University of California Press, 1987.

entangled in this fight, and the uses and abuses made of their ideas have become part of our intellectual history.

VII

When we finally come to speak of the antonyms of *Unity* versus *Disunity*, each shows again the inherent multi-allegorical substructure. The thema of Unity is perhaps historically the most ancient dream of nature philosophy, but also is said by some psychologists to be one of the earliest experiences of the child with respect to the mother. It would be presumptuous to lecture to a learned audience of the *Institut Wiener Kreis* about all the vagaries of the notion of *Einheit* vs. *Vielheit* in science and philosophy. But it is worthwhile to recall that the early decade or so of the 20th century was a great period of experimentation in this debate. Neurath, Hahn, Frank, von Mises, and students at the Vienna University were famously meeting in one of the old coffee houses—the embryonic or first form of the unity-seeking Vienna circle, at just that time when the American historian Henry Adams was writing his autobiography (*The Education of Henry Adams).* There he was fascinated by the recent discovery of radioactivity. The probabilistic, apparently a-causal radiations coming with violent force from those atoms indicated the coming of a vast change in world conception: away from the vestiges of cultural unity and continuity, represented by the grand cathedral of Chartres for previous centuries, and onward toward disunities, discontinuities, and fragmentations that would characterize a chaotic new 20th century. In the same spirit, the 1911 Solvay Conference on the new quantum physics signaled the ending of the Newtonian classical coherence and continuity, leading Henri Poincaré to exclaim in anguish: "Is discontinuity destined to rein over the physical universe, and will its triumph be final?"

It is not unconnected that elsewhere in European culture, during those volcanic years at the beginning of the new century, there were also extreme challenges to the reigning worldview. One need only mention Dada in the arts, Kokoschka and the Expressionists, Stravinsky's "Rite of Spring" and Diaghilev, the Viennese Schönberg, Webern, and Berg in atonal music, and the transforming new technologies.

VIII

One may perhaps say that those brave academics in Vienna, who found refuge in those coffeehouses, were re-asserting the capability of the clear mind to reach to basic unities. They had many enemies, but also some allies. One ally, of whom the Viennese may not have known then, was the Belgian mathematician George

Sarton, who later became known as the father of modern history of science. He founded the journal *Isis*, and launched it in 1913 with a 39-page manifesto. In that essay, he laid out a visionary program for the new profession. It has four components: to produce a complete and synthesizing manual of the history of science; to assure that the pedagogic presentations of science should be in historical sequence; to contribute to a *synthèse* of the study of mankind; and to rebuild, on solid scientific and historic knowledge, the philosophical work begun by Auguste Comte.

Sarton's project is dazzling in its missionary aim and far-reaching extent. A close reading of his essay adds even more tasks to be met. Thus Sarton deplores the danger of "disaggregation" and ever-more limiting subdivision of the work of scientists, which he says not only threatens any common understanding among scientists themselves, but endangers the sharing of a common viewpoint of mankind itself. The healing power here, too, Sarton hints repeatedly, will come from recognizing the need for synthesis. In the conclusion, Sarton summarizes that *Isis* will be "a critical review, an international one, in a certain manner a dogmatic one," but first of all it will be "*une revue de synthèse*."

That aim fitted well with one component of the beleaguered *Zeitgeist* of the time. Thus, one development that Sarton reported in his first issue of *Isis* was the formation of a new international positivistic society. He was referring to the ambitious if short-lived *Gesellschaft für positivistische Philosophie*, which came into being through a manifesto ("*Aufruf*"), published and circulated widely about a year before Number 1 of *Isis*. The *Aufruf* overlapped significantly with Sarton's own program, as indicated by the names of its thirty-three signers from a great variety of fields, such as Hilbert, Mach, Einstein, Freud, Helm, F. Klein, J. C. S. Schiller, Tönnies, and Jacques Loeb, a positivist of Mach's variety. And the audacious program of that new society, quite parallel to that of the first *Wiener Kreis*, was nothing less than this: [The purpose of the *Gesellschaft* is] *alle Wissenschaften untereinander in lebendige Verbindung zu setzen, überall die vereinheitlichenden Begriffe zu entwickeln und so zu einer widerspruchsfreien Gesamtauffassung vorzudringen.*

One can see the work being done, in the same decade or two, by physicists such as Boltzmann, Planck and Einstein, as crashing through current doctrines. Their thematic antithesis against the old order is part of the many-colored allegories, each with its long history, and is very much alive today. But those physicists and other scientists (not excluding Freud) as well as philosophers sympathetic to them in those early decades, were breaking away from the old unities—not to join the disunifiers, but in their different ways to forge new unities, often again severe internal and external obstacles. (The word "despair" occurs in the writings of many of these scientists.)

IX

The variously admired forebears of many in this *Gesellschaft* were the philosophers of the Enlightenment and Auguste Comte, Maxwell, Helmholtz, Mach, Pearson. However, the Canadian philosopher Ian Hacking thinks he can discern *Vielheiten* in the *Einheit* they and their predecessors clung to. He cites three main family varieties of unity in science (in *The Disunity of Science*, 1996). He cites as the first family the metaphysical one, "a collection of ideas about what there is." There he focuses on Salam, Weinberg, and Glashow's successful reunification of the two main forces in physics, praising their "interconnectedness."

His second family "is a collection of practical precepts about the sciences"--the method and the aims of the sciences. Here, he focuses on finding "connections between important phenomena", and praises Dirac for having united theories that were previously disjointed. He also singles out Adorno and Popper as representative of the trend to use "the same method (whatever it is) … to be used in all the sciences, natural, social and human."

His "third family forms a set of theses about scientific reasoning, and includes both logic and methodology." And under this heading he singles out the "unity of science manifesto issued by Helmholtz and others in 1847", as well as Darwin, Crick, and at last Ernst Mach.

I don't wish to imply that I accept all these divisions as my own, but they are characteristic of contemporary philosophers of science. I do find myself in full sympathy with Professor Hacking's remark that "many of the present youngest generation of disunifiers are quite cynical not only about established images of science but about the sciences themselves."

At this point, I should draw attention to one other, little acknowledged set of allies of the first Vienna Circle, one that happened also a century ago, also in Vienna, and which Eric Kandel has been studying in his remarkable way. I refer to a variant of *Einheit*, to the extraordinary subterranean integrations in the unique cultural life of Vienna in the two decades on either side of fin-de-siecle. Members of the artistic and intellectual elites not only met and learned from one another in the salons and coffee houses, but brought into their own respective studies and labors what they had learned from one another. One example among many was the influences of Freud and the inheritors of the school of Rokitansky on Austrian Expressionist artists such as Klimt, Kokoschka and Schiele.

For me the most striking fact about those unifiers is their sharing in that ancient dream, and their high and serious ambitions, especially when measured against the philosophical and political forces in charge in those days. Against those forces, the Vienna Circle's activities were also quite breathtaking.

X

In concluding, I remember that one must, even in theoretical discussion, return, as the Wiener Kreis 1929 Manifesto put it, to the questions of life, and to Otto Neurath's warning to attend, in his words, to "the great historic process going on in the world." That process is today still as it was then: the combat between the antithetical themata, eros and thánatos.

On the negative side, the historic process today is the possibility of our societies' spinning down further into the ever-increasing disunity and chaos of ethnic and religious wars, of millions of refugees, of financial ruin as one result of the globalization of unstable finance, of hunger, disease, and ignorance.

On the opposite side is the challenging possibility, as the Manifesto put it, of "returning, after a metaphysical interlude, to a unified picture of the world." The reason for my cautious optimism that the positive side may yet win, is something that those theoreticians of unification in Vienna, who started only with a unification of science and philosophy, could not have expected, except in their most utopian dreams. For there have been forming, during the last few decades, increasingly numerous *global, integrated* organizations. One thinks of institutions, with all their current deficiencies, such as the United Nations, UNESCO, the European Union, the World Trade Organization, the World Health and Food Organizations, the International Criminal Courts, the World Bank, the International Atomic Agency, and hundreds of others of that sort. Look around you—at the huge number of nongovernmental organizations housed in Austria.

Such experiments are presenting themselves as a source of hope, not only *institutionally* but also *intellectually*. A practical aspect of intellectual globalization is *interdisciplinarity*, especially in the sciences. A typical recent article in *Science* had researchers from a great variety of specializations, from over 100 institutions in 16 nations, working together on a genome project on drosophila, the fruit fly. At the LHC collider in CERN, over 2000 persons, most working at a distance, are involved in a new experiment. Mega-teams are busy on environmental science, and so forth.

Within contemporary natural sciences there is now actually a highly accelerating movement of integration, which earlier one could only have hoped for. The big new word is Integration. To give one parochial example, at my university a large new building is being finished, which has the astonishing name: Laboratory for Integrated Science and Engineering. All of natural science, all of engineering! At least, that is the invitation to scientists.

In addition, there is now worldwide a consciousness that there is some underlying unity in science, perhaps not of the Theory-of-Everything variety, but of a different, operational kind. That is exemplified in the ceaseless borrowing connecting diverse traditions and disciplines. In principle, any two research efforts, however removed in time, in subject or in purpose, may well turn out to be

genealogically connected. And in the limit, the whole of natural science may be represented as one thickly linked continuum, which can be divided into distinct disciplines and traditions only in an arbitrary way. While they may differ, the multitudinous projects of the sciences share in and emerge from a common history.

The increasing intellectual and institutional globalizations, together with the rapid integration among the separate sciences, allow me to conclude with a vision of a possible future and a sincere hope. Let me dare to say it here: Something new is trying to be born in our world, something on which we must place our bets and invest our intellect and energy. It is a new variation of an intellectual and living tendency toward the old thema of *Einheit*, of which the Wiener Kreis was an early voice of prophecy.

The last paragraph of the 1929 Manifesto began as follows: "So steht die wissenschaftliche Weltauffassung dem Leben der Gegenwart nahe … gibt es viele … die angesichts der soziologischen Lage der Gegenwart, hoffnungsfroh der weiteren Entwicklung entgengehen." We know all too well that this hope was not to be, in the tragic 20th century. But it can and must live again; and your studies here in the Institute are necessary sources of a better understanding toward the new *Auffassung der Welt*.

Harvard University
Jefferson Physics Lab
MA O2138 Cambridge MA
USA
holton@physics.harvard.edu

Thomas Mormann

Enlightenment and Formal Romanticism – Carnap's Account of Philosophy as Explication

A. W. Carus, *Carnap and Twentieth-Century Thought: Explication as Enlightenment*, Cambridge, Cambridge University Press, 2007, 346pp.

1. Introduction

Carnap and Twentieth-Century Thought: Explication as Enlightenment is the first book in the English language that seeks to place Carnap's philosophy in a broad cultural, political and intellectual context. According to the author, Carnap synthesized many different currents of thought and thereby arrived at a novel philosophical perspective that remains strikingly relevant today. Whether the reader agrees with Carus's bold theses on Carnap's place in the landscape of twentieth-century philosophy, and his even bolder claims concerning the role that philosophy in Carnap's style should play in the thought of our century, does not matter so much as the excellent opportunity Carus's book offers to thoroughly rethink one's ideas about Carnap's philosophy. One reason why *Carnap and Twentieth-Century Thought* (henceforth, CTT) might change one's ideas is that Carus has unearthed much hitherto unknown material from the archives that sheds new light on Carnap's early life and thought. Indeed, the many archival findings presented in CTT for the first time suffice to make the book rewarding reading for philosophers and historians of philosophy alike. CTT exhibits a high standard of historical scholarship, and the book itself is a beautiful example of high-quality academic publishing.

Up to now, Carnap has remained a controversial figure on the philosophical scene. On the one hand, he has a solid reputation as a leading figure of logical positivism (or logical empiricism). According to conventional wisdom, this was a school of thought characterized by its formal and technical philosophy, as well as being rather dismissive of other ways of doing philosophy, dogmatically sticking to its own theses. As a typical example of this arrogant logical empiricist attitude, one usually refers to Carnap's notorious *Overcoming Metaphysics by Logical Analysis of Language* (Carnap 1932), written when the Vienna Circle's Logical Empiricism had entered its most radical phase. Self-proclaimed postpositivist philosophers of science dismissed logical positivism, in particular Carnap's, as the dogmatic and orthodox "received view." The tendency to portray logical em-

piricism as an obsolete doctrine centering around certain "dogmas" started with Quine's *Two Dogmas of Empiricism* (1951) and reached its somewhat ridiculous culmination in the early 1980s when allegedly "six or seven dogmas" were discovered (cf. Stegmüller 1983). Thereby an allegedly unbridgeable gap between classical "dogmatic" logical empiricism and its modern "enlightened" successors was construed.

This standard view of logical empiricism has come under fire for some time now. In the last twenty years or so a growing number of scholars have been engaged in undermining this simplified picture. According to them, the more one engages with Carnap's real thought, the more one finds open-mindedness and pragmatism at its very core. As the revisionists claim, Carnap was a prodigy of tolerance, always engaged in the business of building bridges and finding ways of reconciling apparently irreconcilable philosophical positions. Pushing this interpretation even further, Carus proposes to consider Carnap 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 is allowed to flourish in its own right.

One may ask why such a profound misinterpretation has captivated so many philosophers for such a long time. Carnap himself might have answered that the misunderstanding was based on the fact that people were talking about two (or more) different things when they dealt with tolerance. In other words, he might have proposed that the explicandum "tolerance" is to be replaced by several different explicata, as happened in the case of "probability", which Carnap offered as his paradigmatic example. The partisans of the "new Carnapian tolerance" rely on a different strategy, however. According to them, the misunderstanding is caused by the fact that those who scorn Carnapian narrow-mindedness and intolerance simply have not read Carnap carefully. As soon as one engages in a detailed study of Carnap's writings, one will discover the true, tolerant Carnap.

More precisely, the main thesis of CTT is that in the early 1930s Carnap's philosophy underwent a thorough-going revolution that changed forever the direction of his thought. Conceptually, this revolution amounts to the replacement of the idea of rational reconstruction or logical analysis, which marked Carnap's early philosophy, by the idea of explication. The ideal of explication, Carus contends, set Carnap's philosophy on the secure path of tolerance, pluralism, and pragmatism. Moreover, it was not only the conceptual driving force of Carnap's mature philosophy, it is also uniquely able to provide the "Enlightenment tradition" (which Carus wholeheartedly endorses) with a program for the future, as it provides tools for breaking out of the "dialectic of enlightenment" (CTT, 32). This claim is, as the author readily admits, "rather startling".

The main historico-philosophical thesis Carus puts forward in CTT is "that the Vienna Circle's utopian aspirations [such as 'enlightenment', 'progress', 'socialism', T.M.] remained central throughout Carnap' s later years." In the case of Carnap, these utopian aspirations took a special form, namely, to overcome "the

gap that has split the thinking world since Newton (or before).” For the generation of the young Carnap this gap might be described as the “split between knowledge and life” (*Geist und Leben*), or the opposition between “Enlightenment” and “Romanticism.” Carus credits Carnap with having offered a promising way to bridge the gap by conceiving philosophy in a new key, namely, philosophy as explication.

2. The Principle of Tolerance

Carus locates the nucleus of Carnap’s novel account of philosophy as explication in the so-called “principle of tolerance” formulated for the first time in *Logical Syntax of Language* (1934). While it has often been noted that the principle of tolerance has deep philosophical implications, usually very little space has been devoted to explaining how it can be used to overcome traditional philosophy. Carus intends to do just this, and therefore is certainly to be applauded.

Some sort of tolerance principle can be traced back even to Carnap’s dissertation, *Der Raum*, where he argued for the acceptance of a variety of different conceptions of space in mathematics, physics, and philosophy – although with a clear preference for a formal conception. Later, this pluralism in geometry was extended to the natural sciences, allegedly extending Poincaré’s conventionalism, until even logic and language were claimed to belong to the field of conventions:

> *In logic there are no morals*. Everyone can construct his logic, i.e. his language form, however he wants. If he wants to discuss it with us, though, he will have to make precise how he wants to set things up. He has to give syntactic rules rather than philosophical considerations. (Carnap 1937, 52)

One should note that “tolerance” is only one side of the coin. The principle of tolerance also imposes a strong imperative on everyone who wants to discuss with “us”, namely, the requirement to make fully explicit the syntactical rules of his language. This is a formidable requirement. Even hard-boiled Carnapians almost never took it seriously, giving explicit syntactical rules of the language form they employed in their discourses, perhaps with the exception of Carnap himself in the *Aufbau* and in *Syntax*.

One may object that this reading of Carnap’s imperative is overly strong. It seems obvious that Carnap did not mean it literally. Making explicit the syntactical rules simply meant that one should express oneself as clearly as possible. This requirement is plausible enough, but is not much more than a vague commonsense rule.

Later, the requirement that one has to make explicit the syntactical or semantical rules of the language one is to use disappears from the surface. In “Empiricism,

Semantics and Ontology", which is considered as the mature formulation of the Carnapian principle of tolerance, the principle is stated as follows:

> The acceptance or rejection of … any … linguistic form 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 efforts required. … 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. (Carnap 1950, 221)

"Logicality" is taken for granted here, or so it seems. The most important role is played by a Darwinian principle of the survival of the conceptually fittest. This sounds very modern, even close to Feyerabend's notorious slogan "anything goes." Actually, a similar idea can already be found in Dilthey's *Lebensphilosophie*, which considered the struggle of *Weltanschauungen*: "Those *Weltanschauungen* that lead to useful conceptions of life and foster its understanding maintain themselves and supersede the lesser ones" (Dilthey VIII, 85). In this selection process, neither for Carnap nor for Dilthey did arguments and reasons play any role whatsoever.

3. From Rational Reconstruction to Explication

The key to understanding Carnap's novel approach is replacing the concept of rational reconstruction with the concept of explication. The term "explication" first appears in Carnap's work, in print, in June 1945, in a paper entitled, *The Two Concepts of Probability* (Carnap 1945). The most extensive discussion of explication by Carnap himself is the first section of his *Logical Foundations of Probability* (Carnap 1950). Another important text for the elucidation of the concept of explication is his reply to Strawson (Carnap 1963). Let us start with an innocent-looking version of the new key concept of his philosophy, found in *Meaning and Necessity* (§2, 7):

> The task of making more exact a vague or not quite exact concept used in everyday life or in an earlier stage of scientific or logical development, or rather of replacing it by a newly constructed, more exact concept, belongs among the most important tasks of logical analysis and logical construction. We call this the task of explicating, or of giving an explication for, the earlier concept …

The difference between rational reconstruction or logical analysis, on the one hand, and the concept of explication on the other, is explained by Carus as follows:

> Rational reconstruction was a one-way street; vernacular concepts were to be replaced, piece by piece, with more precise ones. It was assumed that there was a single, definitive logical language in which this reconstruction could be achieved. But under the new regime

of tolerance after 1932, there is no longer a single correct language. There is an infinity of possible languages, and the community must decide among them. Explication is therefore *dialectical*, as Howard Stein, … has pointed out (Stein 1992). (Carus 2007, 41, 42)

Carnap did not have much to say about the rules or principles according to which "the community decides." He simply assumed a strict division of labor. The philosophers, as language engineers, are engaged in constructing languages, the scientific communities test them in scientific practice. It goes without saying that this is a rather abstract and unrealistic account of how the collaboration between philosophy and science could be organized.

The move from unique rational reconstruction to explicitly pluralist explication implies a fundamental change in the very task of philosophy itself. It was traditionally understood that the business of philosophy was to make assertions about philosophical issues. As assertions, the assertions of philosophy could be true or false. As the history of philosophy shows, philosophers have usually argued that the assertions of their fellow-philosophers were false. This led to interminable fruitless disputes between the various philosophical schools. Carnap wanted to get out of this rut. In *The Character of Philosophical Problems* (Carnap 1934) and *Testability and Meaning* (1936/37), he put forward a novel interpretation of what philosophical statements really are. According to him, philosophical assertions, conceived as propositions about the syntax of scientific language, could be understood essentially in two different ways; as assertions that describe the language of science available today, or as proposals that propose that the language of science should be built up in such and such a way. The second interpretation became more and more important for Carnap. Finally, he came to characterize the task of philosophers as making proposals for the linguistic forms. As Carus puts it, philosophy for Carnap became "language engineering", i.e., the construction of languages that might be useful for science in general or, more specifically, for some branches of science, or some special purposes dealing with matters scientific. In this vein, the main aim of *Testability and Meaning* was to offer definitive solutions of problems treated:

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 discussions. 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. (Carnap 1936/37, 38).

This sounds very open-minded and tolerant, but one should not be too surprised at this new openness if one takes into account Carnap's situation in that time. In Vienna, the Vienna Circle no longer existed, he himself had just moved to the US, and the future of the empiricist movement was anything but secure. It was high time to look for allies and supporters wherever one could find them. As far as I

know, this is the only time when Carnap explicitly mentioned Morris's project of scientific empiricism (cf. Morris 1937).

Surprisingly (or not), this loudly proclaimed tolerance was accompanied by a considerable narrowing of the scope of his philosophical perspective. When Carnap began to work in philosophy in the 1920s, he started with a rather eclectic vision of a scientific philosophy that attempted to embrace virtually everything on the market; empiricism, critical idealism, various currents of neokantianism, phenomenology, energetism, or empiriocriticism. In the *Aufbau*, he sympathetically took into account even rare and obscure work such as Rehmke's *Grundwissenschaft* or Gätschenberger's *Symbola*. When he moved to Vienna, this comprehensive perspective was gradually replaced by a more restricted version of what scientific philosophy was to be, ending up with the conception that "Philosophy is logic of science", as he put it in *On the Character of Philosophical Problems*. In some sense then, after 1930, the "outer philosophical world" disappeared from Carnap's philosophical horizon. Instead, the richnesses of an inner domain of formal languages become more and more important. Thus, the newly propagated openness can hardly be conceived as the result of a great synthesis of all the enlightenment-oriented currents of twentieth-century philosophy. Rather, it seems to have emerged from the concentration on a rather small and austere conceptual basis and perhaps rather mundane political necessities.

Carus endorses an opposite interpretation. According to him, Carnap was a great "bridge-builder", who was always engaged in the task of finding ways to overcome the gaps between apparently irreconciliable positions. It may well be the case that, from Carnap's own perspective, things appeared in this way, but from outside a quite different assessment held. After the publication of the *Aufbau* in 1928, the then still existing bridges to traditional scientific philosophy were pulled down systematically. Philosophers such as Dingler, Cassirer, Rickert, Vaihinger, Husserl, Poincaré, and many others no longer played a role for him, at least officially. Even the relations with the American pragmatists, who may be considered as the other great movement of "scientific empiricism", always remained tense.

4. Carnapian Pragmatism

One of the main pillars of Carus's general contention that Carnap's explicative approach to philosophy is to be considered as the philosophy of the future is his claim that it is a pragmatic one. At first sight, the assumption that there was an affinity of Carnap's thought with pragmatist currents seems plausible. For instance, since its inception, the American pragmatist Charles Morris had urged the members of the Vienna Circle to engage in the common project of a "scientific empiricism" that combined the virtues of both the strands of logical empiricism in the Vienna style and American pragmatism:

> It would be possible to develop from the standpoint of scientific empiricism a modern form of the older systems of philosophy, in which the traditional philosophic fields of logic, cosmology, and value theory would find their empirical equivalent. Such an empirical synthesis must, like science itself, be a co-operative entreprise, and its erection will be the work of many generations. (Morris 1937, 5)

Indeed, Morris argued for a pragmatist scientific philosophy that comprised four different stages: (1) philosophy of science as logic of science, (2) philosophy as clarification of meaning, (3) philosophy as empirical axiology, and (4) philosophy as empirical cosmology (ibid. 8ff). For a closer collaboration between pragmatism and logical empiricism, he proposed that the empiricists should acknowledge that the envisaged scientific philosophy should not be restricted to philosophy of science in the narrow sense (1), but should recognize (2)–(4) as legitimate areas of a scientific philosophy as well, even if strictly logical methods could hardly applied to them. The reaction of the logical empiricists to this offer always remained lukewarm, although occasionally Carnap did make some gestures towards pragmatism (e.g., Carnap 1936/37). At the end of the day, however, Carnap offered nothing more to Morris than the flabby assertion "that the difference between my view and that of the pragmatists is not as large as it might appear at first glance" (Carnap 1963, 862). To put it bluntly, it seems doubtful whether Carnap was a good pragmatist. Admittedly, he paid lip-service to the pragmatist creed. Finally, however, he stubbornly stuck to his anti-pragmatist convictions, namely a strict separation between the theoretical and the practical, a clear separation between means and ends, and a neat distinction between internal and external questions. Nevertheless, some authors (e.g., Richardson 2007), claim that notwithstanding certain differences, Carnap should be considered as belonging to the pragmatist camp:

> From within Carnap's thought, then, we have have a view that stresses open-mindedness, tolerance, plurality, and a experimental spirit – all well-known hallmarks of philosophical pragmatism. (Richardson 2007, 296)

Carus is a vigorous supporter of this view. According to him: "Carnap's ideal was pragmatic to the core" (CTT, 302). I must confess that I am unable to see this. I think it is misleading to call someone a pragmatist who strictly separated the theoretical and the practical, and conceived the practical merely as instrumental. Carnap always insisted on clear-cut and neat dichotomies; "analytical vs. synthetic", "empirical vs. theoretical", "internal-external" etc. Perhaps this rigid Cartesian attitude was mitigated somewhat by his pluralism, but this should not be confused with a full-blooded pragmatism. Hence I rather think that the arch-pragmatist Dewey was right, when he diagnosed Carnap's persistent inclination of strictly separating the domains of the "emotive" and the "scientific" as evidence for a basically anti-pragmatic attitude that attempted to evade the real practical problems:

> The hard-and-fast impassible line which is supposed by some to exist between the "emotive" and "scientific" language is a reflex of the gap that exists between the intellectual and the emotional in human relations and activities. … The practical problem that has to be faced is the establishment of cultural conditions that will support the kinds of behavior in which emotion and ideas, desires and appraisals are integrated. (Dewey 1944, 444-445).

Some time ago, Stein pondered whether the later Carnap (the one of *Empiricism, Semantics, and Ontology*) might have been driven surreptitiously to blur the strict distinction between the theoretical and the practical, since theoretical frameworks themselves turned out to be constitutive of alternative notions of possible experiences, thereby localizing fundamental theory change in the framework. Somewhat reluctantly, then, he proposed that this development could be characterized as sort of dialectic (Stein 1992, 291). Carus warmly welcomes Stein's proposal and claims that the new explicative way of doing philosophy may be considered as a "dialectic in the sense of Stein" (CTT).

5. Carnap versus Rawls and Habermas

Now one may be inclined to consider the tensions between Carnap's logical empiricism and American pragmatism as an issue that is interesting for historians of philosophy only. The final proof of the viability of the new explicative philosophy would be in showing that it is able to do some real work. That is to say, Carus could make a convincing case for the new explicative approach if he offered a convincing example showing how the new explicative approach is able to overcome the deficiencies that plague the more traditional approaches. Carus accepts this challenge head-on. In chapter 11 of CTT he aims to show that a Carnapian *Ansatz* may be able to overcome the impasse between the rival accounts of Rawls and Habermas of how a just and reasonable order of society would look.

Carnap's own contributions to this field are rather meager. In his *Intellectual Autobiography* (Carnap 1963) he summed up his (and other members' of the Vienna Circle) political and societal convictions in the following three principles:
(i) Man has no supernatural protectors or enemies; therefore, whatever can be done to improve life is the task of man himself;
(ii) Mankind is able to change the conditions of life in such a way that many of the sufferings of today may be avoided for future generations;
(iii) Deliberate action presupposes knowledge of the world, and the best method of acquiring knowledge is the scientific method, therefore science must be regarded as one of the most valuable instruments for the improvement of life.
According to him, these general principles implied that the global political and economical problems of mankind could not be solved by "the free interplay of forces", but required rational planning: "For the organization of the economy this means socialism in some form; for the organization of the world it means a gradual

development toward a world government." As a brief designation of this *Weltanschauung* in "American terminology" he proposed the term "scientific humanism" (cf. Carnap 1963, 83). It goes without saying that this sketch of a "scientific humanism" hardly bears comparison with Rawls's and Habermas's elaborated conceptions of how a just and enlightened society may look.

If one could show that, notwithstanding the conceptual and empirical poverty of Carnap's "scientific humanism", his explicative approach could offer means to improve on Rawls's or Habermas's vastly more elaborated accounts, or to overcome some of their deficiencies, this would be an excellent argument for the fruitfulness of the explicative approach. Regrettably, CTT does not fulfil these expectations. Carus does not engage in any detailed scrutiny of Rawls's or Habermas's work. Instead he is content with some general criticism:

> Rawls and Habermas share many assumptions that a Carnapian perspective allow us to dispense with. One is their unquestioning acceptance of the ordinary natural language in which we find ourselves situated … as the canonical and ineluctable medium for all discourse. (CTT, 300)

Neither Habermas nor Rawls ever claimed that the "ordinary natural language" was the only medium that was apt to express their thoughts about justice, wealth distribution and related topics. Moreover, one may well deny that Habermas's jargon of the Frankfurt School still belongs to "ordinary natural language in which we find ourselves situated." For Carus, Habermas's and Rawls's usage of ordinary language is just a symptom of a more basic defect of their accounts. It evidences the tacit assumption "that any system of political order be rooted in some common substratum of untutored universal human nature… "(CTT, 306). In contrast,

> the Carnapian ideal regards human institutions (including languages) as the products of human constructive ingenuity, no less than science, technology, and other human tools. We *make* them, they are not out there to be found or revealed. (ibid.)

To me, this seems to be a somewhat naive instrumentalist account of language, science, and technology. It is hardly sufficient simply to state that we "make" language, science, and technology. They also "make" us, as mankind has been experiencing for some time. Technology is not a "tool" in the same sense as a screwdriver, to say nothing about science or language.

Probably a more promising feature than this radical instrumentalist interpretation of language, science, and technology is Carus's insight that to establish a common political framework it is not required that we base our considerations on some common ideas concerning reason, justice or non-coerced discourse. Following Carnap's advice, we should not start with discussing fundamental principles, rather, we should talk about language, i.e. we should attempt

to clarify what the abstract concepts in question actually mean (in practice) to the participants and then, on the basis of such clarifications, to negociate explicata (establish meanings) that can lay the groundwork for a practice recognisable to all concerned as instantiating those abstract concepts. (CTT, 303).

In CTT the prospects of the new Carnapian explicative approach are painted with a very broad brush. Perhaps it would be advisable to follow Carnap's guidelines more concretely. This was done, for instance, by Amartya Sen. On several occasions Sen gave a constructive criticism of the Rawlsian account of justice in the spirit of a Carnapian explicative approach (cf. 1970, 1975 and elsewhere). For instance, in Sen (1975) he is engaged in comparing Rawls's maximin concept of justice with the more common one favored by utilitarianism. For this purpose he proposes some axioms (e.g., the axiom of symmetric preference or the axiom of weak equality) that every plausible concept of justice should satisfy (at least, at first approximation). Then he shows that neither Rawls's maximin rule nor the traditional utilitarian account complies with all these requirements. Rather, both may be characterized as extreme in that each takes into account only one half the whole picture (see Sen 1975, 310). The elegance and undeniable success of Sen's axiomatizations show that at least Rawls's approach *is* susceptible of formal explications. Sen's example evidences that, without being conscious of it, some scientists and philosophers are already engaged in a sort of Carnapian explicative philosophy.

Carus's sweeping objection against both Rawls and Habermas (namely, that both uncritically accept ordinary language) leads him to a generalized suspicion against everybody who does not whole-heartedly support the program of formal explication. He contends that:

Behind the objections against Carnap's general approach lies a deeper and more persistent, more widespread attitude that is harder to answer head-on, as it is not usually made explicit. If it were, it might be expressed as the suspicion that some, perhaps most "folk" categories (including those within a scientific vernacular) are indispensable – to human emotional and practical needs, to the progress of science, or to something else. (CTT, 292)

For the last two hundred years or even longer there has been a variety of attempts to employ concepts and methods of the mathematized natural sciences to the realm of the *Geisteswissenschaften* and *Sozialwissenschaften*; e.g., Boscovitch's psychophysics, Quételet's social physics, extreme versions of behaviorism, strong AI, and many others. All these attempts have failed. This is not to say that there is a domain of knowledge (history, humanities, *Lebenswelt*) that for *a priori* reasons is exempt from the application of formal methods. However, the burden of proof of whether these methods are really useful or not is on the side of those who advertise them. As his fellow-empiricist Neurath admonished Carnap more than once, it may well be the case that the introduction of fancy concepts and methods from mathematics or logic amounts to nothing more than the re-introduction of a

glamorous but, in the end, misleading metaphysics. I think that Neurath made an important point here, although he may not have fully understood the fundamental reasons that attracted Carnap to the universe of formal metaphysics.

6. Romantic Constructivism

One major achievement of CTT is the attempt to contextualize Carnap's philosophy in the cultural and political landscape in which his hero grew up and took his first philosophical steps. The scene is presented in a neat and well-ordered manner: On the one hand we find the Enlightenment camp, on the other hand we find the forces of Romanticism, concentrated in Germany and other German-speaking countries. Arguably, only in German-speaking Central Europe did Romanticism have any appreciable influence in the public sphere during the nineteenth century (CTT, 2), while Western Europe and the US belonged to the realm in which Enlightenment traditions were firmly entrenched. The German inclination to Romantic irrationalism became even more dominant after the turn of the century, and in particular after Germany's defeat in the World War I, and reached its culmination, when in 1933 the National Socialists came to power. Like so many of his generation, young Carnap was exposed to both ideologies, to Romanticism *and* Enlightenment. In contrast to the majority of German intellectuals, however, Carnap managed to retain the best of both worlds, or so Carus argues.

Let us start with an overall picture of Carnap's political outlook when the war had come to an end. According to Carus, Carnap used the concept of politics in a very broad sense (cf. CTT, 63). For him, it meant everything that has some connection with the public social life of people which includes practically all human activities. In order that all these activities work smoothly together, it was essential to arrive at a "form of community" (*Gemeinschaftsgestalt*), that could serve to coordinate them so as "to remove these tasks from the realm of chaotic whim and subordinate them to goal oriented reason" (*der chaotischen Willkür zu entziehen und der zielbewußten Vernunft zu unterwerfen)*. It might be interesting to note that, a few years later, Carnap used the very same expression to describe the goal of philosophy of science when confronted with the task of establishing criteria for a reasonable choice between rival theories. More precisely, Carnap contended:

> Thus we have shown which decisions have to be made and which criteria have to be established in order to evaluate a physical theory and to decide between several competing theories, without appeal to scientific instincts that have so far reigned supreme in this area, and within the scope of conscious principles of the theory of science (*Wissenschaftslehre*). (Carnap 1923, 107)

For some, this may smack of an Enlightened scientistic absolutism of reason that did not distinguish clearly between science and politics. According to Carus, a

clear echo of the scientific positivist "engineering attitude" descended from the Enlightenment via Comte and Ostwald (ibid.). In the case of Carnap, however, this "engineering attitude" was combined with a voluntarist, Romantic, and utopian streak, in particular with the conviction that after the catastrophe of the German defeat in the Great War "the world" should be rebuilt from scratch. This attitude is most clearly evidenced by his magnum opus, *Der Logische Aufbau der Welt*. Although Carnap is directly concerned only with the lofty task of the logical *Aufbau*, a closer look reveals that for him much more was at stake than just a merely logical or epistemological issue (see Galison 1996). The concept *Aufbau* encapsulates one of the basic leitmotifs of Carnap's thought. As has been pointed out by Galison, in German *"Aufbau"* is a heavily loaded concept. Its meaning does certainly go beyond the pale English translations "construction" or "structure". It evinces a pronounced moment of a strong Romantic utopianism in Carnap's *Weltanschauung*.

I believe that Carnap's orientation to (Neo)Romanticism may have been even stronger than Carus has noticed in CTT. For instance, Carnap's "boundless ocean of unlimited possibilities" is nothing but a remake of a famous slogan of Nietzsche:

> The first attempts to cast the ship of logic off from the *terra firma* of the classical forms were certainly bold ones, considered from the historical point of view. But they were hampered by the striving after "correctness". Now, however, that impediment has been overcome, and before us lies the boundless ocean of unlimited possibilities. (Carnap 1937, xv)

Fifty years before, in *The Gay Science*, Nietzsche had launched the appeal:

> *Get on the Ships*! – … [We need] … new philosophers! The moral earth, too, is round! The moral earth, too, has its antipodes! The antipodes, too, have their right to exist! There is yet another world to be discovered – and more than one! On the ships, you philosophers! (Book IV, § 289)

> Indeed, at hearing the news that "the old god is dead", we philosophers and "free spirits" feel illuminated by a new dawn; our heart overflows with gratitude, amazement, forebodings, expectation – finally the horizon seems clear again, even if not bright; finally our ships may set out again, set out to face any danger; every daring of the lover of knowledge is allowed again; at long last the horizon appears free to us again, even if it should not be bright; the sea, *our* sea, lies open again; maybe there has never been such an "open sea." (Book V, §343)

Carnap shifted Nietzsche's radical Romanticism from the moral sphere to the theoretical domain, or, more precisely, to the realm of logic ("In logic there are no morals"). According to his interpretation, the recent achievements of logic and mathematics had opened up a new logical universe that contained an infinity of possible systems awaiting their exploration. At least temporarily, the real world and its real

problems were given up in favor of an infinity of imaginary possible worlds that could be invented at will. The relation between these possible worlds and the real world became tenuous and indirect. In this way, Carnap's philosophy may be seen as a sketch for a "science of possibilities" or *Möglichkeitswissenschaft*, i.e., as the elaboration of Musil's "sense of possibilities", which he had described in *The Man Without Qualities* (see CTT, 64, 242). Carnap's affinity for "possibilities" did not come out of the blue. I propose to conceive of it as a scientistic adaptation of the legacy of German Romanticism. In a modernist and scientistic garb it rehearsed the basically romantic thesis that the "I" is able to build the "world" according to his ideas, or, in an even more radical manner, that the "I" has the power to built up infinitely many different worlds. At first, this claim may sound a bit startling. But Dewey in his *German Philosophy and Politics* (Dewey 1915) had considered it "typically German" that "… Germans … can withdraw themselves from the exigencies and contingencies of life into a region of *Innerlichkeit* which at least *seems* boundless." Dewey asserted that "this region which at least seems boundless can rarely be successfully uttered save through music, and a frail and tender poetry …" (Dewey 1915, 45). Carnap's "boundless ocean of unlimited possibilities" indicates that the region of *Innerlichkeit* can be expressed otherwise – not only by music and poetry, as Dewey believed, but also by the construction of beautiful formal systems that describe fancy idealized worlds. Neurath often criticized this feature of Carnap's thought as an inclination to re-introduce metaphysics through the back door. Carnap's affinity for exploring formal possibilities was the mirror image of a remarkable absence, throughout his life, of any sense how messy the practical realm is. He never had any sympathy for matters of approximation, vagueness, and ambiguity. He never took seriously Neurath's fundamental idea that "*Ballungen*" were inevitable even in our best science.

In *The Man Without Qualities*, Musil clearly sympathized with the "possibility people", i.e., those that possess a refined *"Möglichkeitssinn."* Nevertheless he was well aware that a complementary "sense of reality" may be more important to come to terms with the real world: "If one wishes to pass well through open doors, one has to respect the fact that they have a fixed frame: this principle is just a requirement of the sense of reality." The Romantics will consider this remark as nothing but the expression of a boring bourgeois attitude, but there may be more in it. Without mentioning Musil, Isaiah Berlin characterized the sense of reality in the following way:

> The arts of life – not least of politics – as well as some among the human studies turn out to possess their own special methods and techniques, their own criteria of success and failure. … Bad judgment here consists not in failing to apply the methods of natural science, but, on the contrary, in over-applying them. … To be rational in any sphere, to apply good judgment to it, is to apply those methods which have turned out to work best … [To demand anything else] is mere irrationalism. (Berlin 1996, 40-41).

Certainly Berlin's remark was not meant as a novel or original contribution. It is hardly more than a paraphrase of an assertion that can already be found in Aristotle's *Nicomachian Ethics*. In any case, Berlin's sense of reality is directly opposed to the radical Romantic constructivism of Carnap which attempts to build up the world from scratch thereby overcoming the "Newtonian split" between "science" and "life."

It might be interesting briefly to compare Carus's proposal with a rival project undertaken by Stephen Toulmin some time ago, see his *Cosmopolis* (Toulmin 1990) or *The Return to Reason* (2002). In contrast to Carus, Toulmin bets on the "sense of reality." According to him, for the last four hundred years, the Western imagination has been captivated by the vision of "Cosmopolis", a society as rationally ordered as the Newtonian view of nature. The ideas of "reasonableness" and "rationality" – closely related in Antiquity – have been separated, as an outcome of the emphasis placed on formal deductive techniques. As is exemplified by the rise of the Vienna Circle's logical empiricism and similar philosophical currents, the stress on the rationality of formal theories or calculations had such prestige that they continued to entrench themselves well into the twentieth century. While fueling extraordinary advances in all fields of human endeavor, this vision perpetuated a hidden yet persistent agenda, the delusion that human nature and society could be fitted into precise and manageable rational categories. The liberation from the Cartesian/Newtonian straightjacket, i.e. the "return to reason", is taking place just now when at last we learn again to esteem the epistemological values of the humanistic Renaissance. Toulmin is painting this picture with a very broad brush, and he certainly oversimplifies matters, but he does, I think, make some valid points. The objections against Carnap's general approach cannot be dismissed as easily as Carus would have us believe.

7. Conclusion

Those who are engaged in the historico-philosophical project of investigating the origins and the evolution of the logical empiricism of the Vienna Circle like to characterize it as a current of Austrian "late Enlightenment" (*Spätaufklärung*) (cf. Stadler 2001, 180ff). In the case of Carnap at least, it may be expedient also to take into consideration motifs from German late Romanticism (*Spätromantik*). This would be in line with a remark that Gabriel made some time ago: "For [Carnap], Frege's *Begriffsschrift* lies on the desk, so to speak, and Nietzsche's *Zarathustra* on the bedside table" (Gabriel 2004, 12). In line with Gabriel, I would contend that Romantic motifs surreptitiously had a more profound influence on Carnap's philosophy than he himself would have admitted. In Carnap's approach we find a highly complex amalgam of motives taken from science, Enlightenment, and

Romanticism that is difficult to disentangle. It is an important achievement of CTT to have elucidated some aspects of this complex structure.

Theoretically Carnap's version of the Enlightenment project – as influenced by the German Neoromanticism and *Lebensphilosophie* of the early decades of the twentieth century – was radical and utopian, since it never came close to the point where it had to show that it could be realized. In my opinion, characterizing it as an "engineering approach", as Carus does, endows it with a much greater solidity than it actually had. Take, for instance, the hundreds of systems of modal logics that are available on the market. They can be considered rather directly as "proposals" in Carnap's sense. They are certainly nice gadgets logicians and mathematician can play around with, but it is not so clear if they have contributed very much to a better understanding of possibilia.

This is not to deny that we need the dimension of the possible to come to terms with reality. Without doubt it is important "to open conceptual possibilities", setting sails for exploring the "open ocean of unlimited possibilities." But perhaps one should realize that "exploring the open ocean of possibilities" fatally resembles a Romantic "flight into an interior domain (*Innerlichkeit*)", which Dewey considered as typical of German Romanticism, and that the sense of possibility needs to be complemented by a sense of reality – as was described in various ways by Dewey, Berlin, Toulmin and many others. Carus takes another path. Unabashedly he keeps on singing Carnap's remake of Nietzsche's romantic song, taking it as the anthem of a new Enlightenment:

> Sixty years after [Carnap] first set his sights on the open sea of free possibilities, it still lies before us, all but unexplored. … It is time we ventured forth again in the pioneering spirit of the original Enlightenment, emboldened by Carnap's example. (CTT, 309)

If Dewey et al. are correct, those of us who like doing philosophy in a Carnapian possibilist style would be well advised to take into account from time to time the advice of a friend whose philosophical outlook shares some features with that of Carnap's friend Neurath. My disagreement with some of Carus's "startling theses" should not deter the reader. CTT is a rich and important contribution to a better understanding of one of the most important philosophers of the last century. *Carnap and Twentieth-Century Thought: Explication and Enlightenment* is a fascinating book that breaks new ground for the further exploration of one of the most important philosophical currents of the last century and one that still has an important impact on the contemporary philosophical scene. CTT certainly is compulsory reading not only for those interested in matters Carnapian but more generally for everybody interested in related topics such as the history of logical empiricism of the Vienna Circle, the development of German philosophy in the twentieth century and the history of analytic philosophy in general.

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Elisabeth Nemeth

An Improbable Case of Philosophy: Arne Naess between Empiricism, Existentialism and Metaphysics.[1]

The Selected Works of Arne Naess, ed. by Harold Glasser and Alan Drengson in cooperation with the author, Dordrecht: Springer 2005. 10 volumes.

On January 12, 2009 Norwegian philosopher Arne Naess passed away at the age of 96. He was still actively involved in putting together the edition of the Selected Writings of Arne Naess (SWAN). He worte an introduction to the writings which is printed at the beginning of each volume together with the extensive introduction by the editor Harold Glasser. At first sight this seems strangely repetitive and superfluous but on closer scrutiny it certainly makes sense. In view of the large breadth of philosophical themes that are presented in the volumes it is quite likely that the readers interested in the writings will come from different areas. A number of these writings make high demands on the reader who is expected to be relatively versed in logical analysis and in the work and thought of Spinoza, Kierkegaard, Gandhi, Husserl, Carnap or even Sextus Empiricus. The reader who is interested in one of these heterogenous fields is encouraged by both introductory texts to reflect on a specific theme against the backdrop of the philosophers's entire oeuvre – and that's a good thing.

In the present review I will trace several lines leading through Naess' work which reflect the continuity of and differences to logical empiricism – on the basis of writings that have appeared in volumes I and VIII. In the following Thomas Seiler will review volume X which brings together Arne Naess' writings on "ecosophy". First, I will briefly describe the SWAN. The selection made by the editors gives the reader an impression of the diversity of philosophical themes that Naess was interested in: communication theory, empirical semantics and behaviorist epistemology (vols. I, VII, VIII), scepticism, scientific and cultural pluralism (vols. II, III, IV, IX), normative systems theory and the idea of what Naess called „total views" (vols. III, IV). There is an important amount of Gandhi und Spinoza scholarship (vols. V, VI), and last but not least ecology (vol. X).

The editors decided not to include writings that are relatively well known and still available. This refers primarily to writings on ecology. Naess coined the terms "deep ecology" and "shallow ecology" and made them the focus of his environmentalist ethics. Since the 1970s he was a prominent activist of the ecology move-

1 Thanks to Camilla Nielsen for translating parts of the text and revising the English of the whole essay.

ment. His thoughts and writings are widely read in the community of ecologists. In this edition the writings on ecological themes are thus limited to volume X. The collection does not include a number of writings that would be interesting from the perspective of 20th century history of philosophy – most notably Naess' dissertation "Knowledge and Scientific Behavior", completed in 1936 and submitted to the University of Oslo. This work is informed by the critical exploration of the ideas of logical empiricism. Naess had become familiar with these ideas during his sojourn in Vienna (1934 to 1935) when he attended the discussions at the Schlick Circle.

In volume VIII we find an article (published for the first time in 1993) in which Naess reports on his experiences at the Schlick seminar: "Logical Empiricism and the Uniqueness of the Schlick Seminar: A Personal Experience With Consequences." In this text Naess underlines how deeply influenced he was by the group (SWAN VII, 261). Naess saw his life-long interest in communication theory and the great attention he gave to the broad spectrum of logically possible linguistic "formulations" (SWAN VIII, 263, 280) as the direct legacy of the Vienna Circle. In addition to touching personal reminiscences the text also includes several central points on which Naess criticized logical empiricism - in particular the metaphyics-criticism in Carnap's famous Heidegger critique which Naess saw as being too restrictive (SWAN VIII, 268.) He appealed for taking philosophical texts seriously also when they are written in strange language. Moreover his reflections on the interpretation of philosophical texts also had certain commonalities with the those that Richard von Mises expressed in his *Kleines Lehrbuch des Positivismus* (published for the first time in 1939). But let's hear what Naess himself said:

> When I interpret a philosophical text, my point of view is that of a lawyer interpreting a will. Grammatical failures, strange uses of words, misspellings do not count when one tries to find out exactly what the author of the will wanted to convey in his will. If he calls his wine cellar the library, that is okay, if it can be established that this was the habitual way of talking in his family. Similarly, if a philosopher has strange ways of expressing certain opinions, one of the tasks of the historian is to try out re-formulations better suited to present his or her opinions. On the other hand, we may look upon the text as a musical or mathematical score and see which interpretation might be most interesting given certain purposes. The later texts of Heidegger, for example, have been freely interpreted by some environmentalists and found very useful. The logical empiricists, however, were too attracted to the exploration of one definite model of language, namely calculi with sets of formation and transformation rules, to be interested in the more empirical investigations of philosophical texts as presenting ordinary ways of talking. The ordinary ways are full of metaphors, pictures, unscientific phrases – as are those of philosophers through the ages. (SWAN VIII, p. 268)

This paragraph aptly expresses the tension that can be found throughout Naess' philosophical thought. Against Carnap's "calculus model of language" Naess ad-

vocated interpreting philosophical texts either hermeneutically with a view to the author's intention or making them the object of empirical study of language. As we know, each of these two approaches to the study of language was represented and developed by a large number of philosophers and linguists in the 20th century. There are, however, few philosophers who would grant both the hermeneutic and the radically empirical approach to philosophical issues the same legitimation. It is precisely here that the originality of Naess' philosophical approach lies. And it is also here that its provocation lies. It looks as if Naess wanted overtake the logical empiricists both the left and the right at the same time. He did not shirk from associating philosophies defined in strictly anti-naturalistic terms such as Heidegger's with a strictly naturalistic theory of meaning of truth. What was to be the outcome of this? – SWAN does not necessarily offer an answer to this question but it does provide highly interesting material on how Naess viewed this question and how he worked on coming up with an answer.

Volume I includes one of Naess' writings that can be ascribed to analytical philosophy and that documents the radically naturalistic side of this thought: "Interpretation and Preciseness. A Contribution to the Theory of Communication" (1953). Here questions are addressed that have interested the logical empiricists from the beginning. They have to do with the relation between uninterpreted linguistic basic terms and complex terms with which the language of science and everyday language work. Naess, however, points out right at the beginning of the book that his goal is "similar to, but slightly different from, the aim of various contemporary studies in logical analysis, theory of communication, conceptual analysis, and so forth." (SWAN I, 1) His goal was not

> to solve problems that philosophers down the ages have not succeeded in solving. What I have tried to do is to open up certain channels of research of a rather basic, but trivial kind. The research I have in mind can be varied out only step-by-step as a cooperative enterprise. [….] The immediate aim of this work is to contribute to the foundation of semantics and the theory of communication as an empirical science. (SWAN I, 1)

The theory of communication outlined in the book is based on the relation between what Naess called "intrapersonal synonymity" and "interpersonal synonymity". He developed several types of questionnaire procedures that can be used to turn "assertions about intrapersonal synonymity" into an "object of research rather than ingredients in intelligent conversation." (SWAN I, 1) In the next step he analyzed the structure of "interpersonal synonymity" which is closely related to communication procedures like agreement and pseudoagreement, interpretation and misinterpretation, description and normative definition. Although an important part of the study addresses the logical relations between sentences, the subject matter of the book is not purely logical.

This work concentrates on cognitive aspects of verbal communication – for example, the attempt to convey information – but spoken and written expressions are not abstracted from the context of individuals' speaking, writing, listening to, and reading those expressions, as is legitimately done in pure logical analysis. The basic materials for us are occurrences of utterances." (SWAN I, 2)

Naess hoped that this type of investigation would

be of help to philosophers with an analytical and an empirical bent […] and to those who are carrying out comprehensive studies of certain terms or phrases as they occur in politics, religion, and ethical or other kinds of indoctrination; or of terms in some of the sciences including history, theory of law, and other branches of humanities. (SWAN I, 3)

However, many philosophers would not be happy with the specific way that Naess combined logical analysis and social research. His studies on the concept of truth and other problems of semantics are the best-known examples of this type of logico-social research. Some of them have been published as the first part ("Empirical semantics") of Volume VIII of SWAN. The crucial point made in those writings is still inspiring and provocative. Naess wanted not only to analyze the "utterances" of individuals in certain communication contexts, but also to know more about what he called the "common-sense theories" on semantics held by these individuals. Naess thus asked "people who are not (supposed to be) philosophers" (SWAN VIII, p. 3) about their theories concerning the notions of truth and logical equivalence, and about their understanding of the logical term "or". One should not underestimate what Naess tried to do in these studies – he was not as naïve as some philosophers might be inclined to think.[2] He worked with highly sophisticated questionnaires, creating conditions in which semantically relevant questions would arise. Here are only a few examples of such questions concerning truth: Is there anything absolutely true? What is the common characteristic of that which is true? What is the common property of a true statement? (SWAN VIII, p. 9) In order to know more about the common understanding of the term "or", Naess asked questions such as the following. Peter made a bet that Volga is in Russia or in Romania. It is in Russia as well as in Romania. Did Peter win the bet? Can one infer that Jack is married to Joan if one knows that he is not married to Phyllis and that he is married to Joan or Phyllis? (SWAN VIII, p. 34)

Note that Naess did not claim that this type of social research could determine the meaning of truth, logical equivalence, logical inference, etc. He had a different aim. He wanted to know to what extent the ideas non-philosophers hold on philosophical issues differ from the ideas of professional philosophers. He also wanted to find out how the philosophical ideas of the respondents can be related to their

2 However, today's "Experimental Philosophy" seems to have a similar agenda. Thanks to C. Limbeck-Lilienau for calling our attention to J. Knobe, S. Nichols (eds.): *Experimental Philosophy*, Oxford 2008. See also F. Stadler in this volume.

social background, to different levels of education, etc. His research on the question produced the following results. First, there is no such thing as the one truth theory that is characteristic of common ordinary thought. Second, many of the truth theories developed by philosophers over the centuries have reappeared in the "common sense theories" of truth. Third, even people who have had no training in philosophy whatsoever articulate arguments similar to those classical philosophers have put forward. Fourth, the views held by philosophically trained respondents do not differ significantly from views of respondents without any philosophical training. From this it seems to be clear that Naess' "empirical semantics" does not make any philosophical claim on meaning and truth. Instead, it purports to produce empirical knowledge about common sense theories which philosophers are used to speculating about. Naess believed that such knowledge could be of great use to philosophers. Since many of them refer systematically – albeit in a critical way – to philosophical views allegedly held by most people, any misconception of those views may have a negative effect on the philosophical theories. Seen from this angle, Naess' empirical semantics aimed at creating new conditions of communication between philosophers and so-called non-philosophers.

Interestingly enough, it was in a similar spirit that already in the 1930s Naess criticized the logical empiricist program. His paper "How can the empirical movement be promoted today? A discussion of the empiricism of Otto Neurath and Rudolf Carnap" was written in German between 1937 and 1939. The English translation of the text and the commentaries which Naess in 1956 added to it (published for the first time in 1992) are included in SWAN VIII. This highly interesting paper shows how close many of Naess' ideas were to Neurath's. Naess argued for understanding Logical Empiricism to be not a set of theories but rather a set of proposals for bringing communication between different disciplines and theoretical approaches about. Naess thought that it was futile to search for definitively clear-cut demarcation criteria between empirical and non-empirical sentences. He thus warned against overstraining the "physicalist thesis" which claims that every term of the language of science is reducible to the terms of physical language. (SWAN VIII, 171) He pleaded for interpreting physicalism as an ongoing project which aims at formulating questions in an empirical way and making them sufficiently precise. Naess found it more desirable "to produce recommendations for expository and discussion practice" than to defend far-reaching theoretical theses about science in general. "One should not lose sight of the fact that the physicalist speaks of Science as a whole, and hence makes statements that I as an empiricist want to treat with the greatest caution." (SWAN VIII, 180)

Naess was aware that his conception of empiricism was very close to Neurath's. Yet as the present collection of writings shows, this affinity was surprisingly far-reaching. Some of the formulations in Naess' 1937–39 paper articulate almost exactly the same considerations Neurath brought forward in his paper "The Lost Wanderers of Cartesius". Remember a few of Neurath's sentences in 1913:

It was a fundamental error of Descartes that he believed that only in the practical field could he not dispense with provisional rules. Thinking, too, needs preliminary rules in more than one respect. The limited span of life already urges us ahead. … Whoever wants to create a world-view or a scientific system must operate with doubtful premises. ... The phenomena that we encounter are so much interconnected that they cannot be described by a one-dimensional chain of statements. The correctness of each statement is related to that of all others. … In order to make progress one very often finds oneself in the position of having to choose one of several hypotheses of equal probability … (Neurath [1913] 1983, p. 3)

Here is what Naess wrote in 1937–39:

I personally think that it is compatible with the empirical attitude to devote all one's energy to a certain research program, that is, in a certain direction. Absolutism in the choice of problems is caused by the fact that there are limits to what an individual can accomplish in science. This does not necessitate a categorical formulation of the results one hopes the program will yield. Absolutism of action in no way implies an absolutism of hypotheses. (SWAN VIII, p. 188)

And in 1956 Naess added the following to the last sentence:

When research is understood as a type of human activity, this sentence can be restated as follows: "Absolutism of action does not justify absolutism concerning hypotheses."
The investigator continually has to act on the basis of priority lists resting on conclusions drawn from dubious *pro-and-contra* deliberations. The time and the energy he has at his disposal are limited; the field of research is infinite in all directions. If he wants to obtain results, he will have to concentrate on definite tasks. This often requires a certain amount of painful resignation, which may lead him to overestimate the area he has chosen and the significance of his results. However, the fact that a certain investigator has made an absolute (final, unconditional) choice does not justify utterances about his choice being the only adequate one in the research situation in question. (SWAN VIII, 210)

It is interesting in itself that Neurath articulated in a very similar way the project "to understand research as a type of human activity". In addition to this, Naess' approach to science might be of some help in discovering some features of Neurath's concept of science which usually go unnoticed. Neurath's philosophical ideas on science are generally viewed as an early pragmatist version of Logical Empiricism. There is no doubt a lot of pragmatism in Neurath and, to a certain degree, even in Naess who was an admirer of William James. But Naess' philosophical works also allow us to look at Neurath from a different angle. It was not only pragmatists but also existentialists who emphasized that any human activity, including cognition, takes place in a situation in which we lack an overall picture and have to make "absolute (final, unconditional) choices". Existentialists, too, stressed that we cannot justify our decisions by referring them to unshakable principles. Naess explicitly pointed out that what he called "absolutism of action" had a strong existentialist background.

> Then you … feel like Kierkegaard, where you are always deep in the water – sixty thousand fathoms, in the sense that a decision *must* be made. That is existentialism at its best. No more talk now, you go left or right. And then you have a proclamation ending with an exclamation mark. You must act! You can't get away from it. That is Kierkegaard at his best. I have been very much influenced by Kierkegaard. …
> You are caught as a human being. In a certain sense it is dreadful, especially for people who are morally extremely serious – they cannot accept that you go right instead of left without a complete justification. You jump right, and there is a risk that it was completely wrong of you … That they cannot stand. They must have a complete reason and stick to that. (Naess 1993, 123)

Remember that Neurath's anti-foundationalism, too, was not restricted to philosophy of science. When he articulated for the first time, in 1913, his conception of science as a provisional human enterprise, he presented it primarily as a response to the cultural, social and moral challenges of his time. He criticized what he called "pseudo-rationalism" primarily for its destructive effects in the moral, social and political domain. Yet however close their anti-foundationalist views might have been, Naess' commitment to existentialism went much further than the allusions to it we can trace in Neurath. In his book *Four Great Philosophers* (Chicago 1968) Naess put Heidegger and Sartre side by side with Carnap and Wittgenstein, a gesture which Neurath would hardly have accepted. The book (regrettably not included in SWAN) is an impressive example of Naess' thoughtful and attentive way of interpreting philosophical texts of very different style.

Though we decided to restrict this review to some of the subjects in Naess' philosophy which are related to Logical Empiricism, we want to emphasize that it is exactly the connection between logico-analytical methods, radical empiricism and a deep-rooted interest in metaphysics which makes Naess' philosophy unique, fascinating, and – considering all its tensions – disturbing, too. He was deeply interested in the philosophical analysis of norms. He believed that investigating the basic norms of human behaviour is as much a logical enterprise as it is an empirical and a metaphysical one. In respect to his view of metaphysics, the studies of Spinoza are particularly rich. Some of them are published in vol. VI. But even volume VIII which puts together much of the writings related to Logical Empiricism, contains a part dedicated to "Metaphysics, Morals, and Gestalt Ontology". It includes writings on the cognitive status of norms and Gestalt Thinking. There is also a small article on "Kierkegaard and the Values of Education" from 1968. From our point of view, this is a particularly precious piece of writing, for it shows both, the continuity of Naess' thinking with some of the crucial aims of Logical Empiricism, and the new direction his philosophical thought took later on. Besides that, the article gives another example of Naess' sensitive, highly original and inspiring way to interpret philosophical texts. In this case, he looked closely at Kierkegaard's "Concluding Unscientific Postscript". For Naess the "delightful anti-Hegelian sayings of Johannes Climacus" are still a valuable criticism of any "nationalist, theological, historical 'scientific' dogmas and myths" (SWAN VIII,

344). Johannes Climacus' refusal of all system-building lead Naess to considerations very close to Neurath's, on the one hand, and to some critical reflections on the higher education system of his days, on the other. His plead for encouraging the individual development of students is not less important today than it was in the 1960ies.

> The system-building most dangerous to the inner, individual sources of belief, including valuation, is today the interpretations provided by popularizers of science and by "experts" in administration. We need a neo-Duhemian stress on the difference between more or less certain and indubitable results of scientific or technical research, on the one hand, and interpretations and interpolations, on the other. The latter can exhibit vast differences in direction, but owing to ideological and other idiosyncrasies of teachers and parents, the young are stuffed with one interpretation, to the accompaniment of a negative inducement to allow their imaginations to play with other possibilities. Consequently, the very sources of creative personal belief are apt to dry up, with resulting loss of individuality and interest in spiritual matters. The vast textbook systematizations foster the illusion of a pre-existing world common to all individuals in which they all live, one that is known in all important respects. We need to stress a plurality of world views, of historical interpretations, of views on human existence. (SWAN VIII, 344)

For sure, neither Neurath nor any other early Logical Empiricist commented on individual belief or spiritual matters in the way Naess did. They would probably have been rather suspicious of the existentialist notion of "deep choices" and of Naess' view that what counts "is the seriousness, pathos, energy, genuineness, enthusiasm, and depth of choice. A choice may be taken as deeper the more it touches the system of attitudes as a whole, that is, the more fundamental it is." (VIII, 345) Logical Empiricists would have been even more irritated with the idea of "being in the truth" which Naess took from Kierkegaard: "The deeper choices have a purely personal relation, an individual component: is the chooser in the truth?" (ibid.)

Yet his reflections deserve some more close reading. If we read Naess in the unbiased and attentive way that he read other philosophers, we might find an empiricist and naturalist as radical as Neurath, who, at the same time, did not limit himself to the third-person perspective which is so characteristic of the Neurathian approach. Seen from this angle, Naess rather followed Schlick's emphasis on subjective experience (which Schlick articulated – against Neurath – in the protocol sentence debate), but elaborated the subjective side of experience in a different way. Naess took up the radically individual perspective on one's own life articulated by existentialists and tried to make it compatible with uncompromising empiricism and naturalism. To articulate this improbable project, he used philosophical elements of such different nature as Phyrronian Scepticism, traditional Chinese and Indian philosophy, the teaching of Gandhi, phenomenology, Gestalt-psychology and – perhaps most importantly – Spinoza's metaphysics. This sounds very eclectic, and to a certain degree it is. But reading Naess on so highly different

subjects and authors is a fascinating experience in itself. One never gets the feeling of arbitrariness but rather of a particular type of development. In his comments on what higher education is all about, Naess explicated that there is no intellectual and personal development without discontinuity, created by choices.

> Every deep choice creates a discontinuity; the individual develops into something different from what he was before, and something more self-made, autonomous. Only through such choices can the youngster develop into a strong personality. Only if he is able to 'go into himself', concentrate and listen to more or less immature impulses, and have the courage to follow them, only then can the growth of the personality withstand the external pressures of parents and teachers trying directly to influence choice. (SWAN VIII, 345)

For Naess, there was no contrast or tension between "creative personal belief" and "inwardness", on the one hand, and the scientific attitude, on the other. On the contrary. He conceived of research as an excellent example of human activity in which personal development towards more autonomy can and should take place.

> The authority of Kierkegaard is sometimes used to belittle scientific research and objectivity. Seen another way, however, the researcher tries to be intellectually honest and open-minded in his choices, and the dedicated researcher requires his own kind of endurance and faithfulness as he proceeds (like the historians of the Bible) along the infinite "road of approximations". Research, therefore, is one of the professions admirably adapted to test inwardness and ethical stamina. (SWAN VIII, 346)

It is not only for the richness and thoughtfulness of Naess' writings that the SWAN edition deserves much more attention than Naess' philosophy receives nowadays. The ten volumes can also be read as a testimony of a philosophical journey which was motivated and shaped by two orientations that we have become used to thinking of as incompatible: the pluralism of world-views and the search for objectivity and truth. Remarkably enough, Naess saw himself as an uncompromising pluralist as well as a uncompromising searcher for truth. No wonder that such a project leaves more questions open than it can answer. However, open questions do not prove that the project was futile. Questions are open to further investigation. The new edition has created very good conditions for this pursuit.

Department of Philosophy
University of Vienna
A-1010 Vienna
Austria
elisabeth.nemeth@univie.ac.at

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Review of Arne Naess, *Deep Ecology of Wisdom.* 687 p. (SWAN, vol. X)

The recently published survey volume "Deep Ecology of Wisdom. Explorations in Unities of Nature and Cultures. Selected Papers" is the 10th volume in the series "The Selected Works of Arne Naess". This volume was edited by Harold Glasser together with the author and the well-known theorists of the Deep Ecology movement Alan Drengson, Bill Devall and George Sessions. A number of important essays, to date unpublished or published a long time ago in for the most part difficult to access journals or books, are united here for the first time in one volume.

Of the introductory articles of the editors I would like to refer in particular to Harold Glasser's remarks (especially pages xl-xlv). Ever since his article "On Warwick Fox's Assessment of Deep Ecology" was published, he is seen as the leading expert on Arne Naess's work (see: Journal Environmental Ethics, 1997, online: www.umweltethik.at).

Most of the 57 articles contained in the 9 chapters of the volume were written in 1973 to 2000. Altogether, the volume has 600 pages including introductory articles, footnotes, an extensive bibliography of Naess' work as well as a keynote index. This is the most comprehensive collection of articles on Arne Naess and Deep Ecology. Indeed it is a treasure trove for the philosopher and the environmentalist alike. The collection is an ideal complement to the monograph with the title "Ecology, Community and Lifestyle" (1889) which was not taken into account in the "Selected Works of Arne Naess".

The eco-philosophical aspects of Deep Ecology are addressed in section I (The Long-Range Deep Ecology Movement; 12 contributions) and to a certain degree in section VIII (Theoretical Dimensions of Deep Ecology and Ecosophy T; altogether 9 contributions).

Accounts of the Deep Ecology movement can be found in sections II (Values, Lifestyle and Sustainability; 7 essays), III (Deep Ecology and Politics; 3 texts), IV (Deep Ecology Practices: Integrating Cultural and Biological Diversity; 10 texts) and IX (Deep Ecology and the Future; 4 texts).

The following sections are dedicated to Arne Naess' own 'Ecosophy': section V (The Significance of Place: At Home in the Mountains; 4 texts), VI (Spinoza and Gandhi as and also parts of section VIII (Theoretical Dimensions of Deep Ecology and Ecosophy T; a total of 9 texts).

In "Nature Ebbing Out", published in 1965, the main theme of Deep Ecology already emerges: "The spectacular, free, beautiful and 'dangerous' nature is about to disappear. Our children will live in a domesticated world …" The disappearance of a 'free' or wild nature entails a reflection on man's relation to nature. The analysis of the growingly inconsiderate approach of man to nature takes place primarily in epistemological and ontological 'problematizations'. Which value structures, which understanding of the world, which self-understanding contributes to the part of nature that has been transformed by human intervention at a speed that makes the 'end of (wild) nature' seem almost inevitable in a couple of decades? While socio-economic structures in the broadest sense, as for instance population size, techniques, economic modes, power structures are addressed, and actually play an important role in the deep ecology platform – they are not analyzed in-depth.

The concepts that are central for Deep Ecology are addressed in almost all articles. These mainly include: a) the distinction in a far-reaching remodeling of man's relation to nature as opposed to a superficial environmental protection reform movement (Deep vs. Shallow Ecology); b) the thematic convergences of a 'radical' environmental protection movement with the goal of an ecologically sustainable approach to nature (Deep Ecology Platform); c) an 'ecologically' inspired value structure (ecosophy); d) the instrinsic value vs. instrumental value of nature ('right' to life and the right of all forms of life to thrive); e) identification with non-human nature (identification); f) a hierarchical-deductive system on four levels so as to move from basic values to norms of action (apriori) on four levels (apriori); g) an unbiased as possible clarification of basic valuative ideas and value priorities (Deep Questioning). In certain passages one would, however, have wished that the account was more systematic, comprehensive and more in-depth. Naess describes the philosophical underpinnings of his own ecosophy with the difficult concepts of Gestalt-ontology and Gestalt-experience. The former relates to the claim that reality does not consist of separate but of a network of related elements that in part constitute the others, while the latter refers to the distinction between the 'concrete contents' of reality and its 'abstract structures' and to the fact that in spontaneous experience there is no split in 'facts' and 'value' experiences. The central claim for Naess, namely that an identification with nature entails a transformed understanding of self and nature (self-realization) is also allotted ample space by the editors.

The reader, however, who is less interested in natural philosophical positions and more interested in intuitions and reflections related to ecology and the 'practice' of environmental protection will be satisfied. Arne Naess knows exactly how to convey his intuitions on the basis of own experiences with nature. These

descriptions offer consolation to all environmentalists who occasionally lose heart.

The ‘simple’ or ‘nature-oriented’ life is recommended to the reader not with false romanticism or normative gesture but as an option on the search for the meaning of life, life quality and rewarding experiences. In the article “An Example of Place: Tvergastein”, we witness Naess as a ‘naturalist’ in the sense of wilderness explorer of nature who stands out for his receptivity, attention, consideration, identification with his ‘object of study’ and devotion to even the smallest detail. Naess feels part of nature and neither inferior nor superior to it, and he respects the ‘right to life and thriving’. In the vicinity of his ‘Tvergastein’ hut, located at 1,500 meters above sea level, he spent a total of ten years, pursuing botanical, zoological, mineralogical, meteorological and other scientific studies. His approach is remarkable: “The meaningfulness inherent in even the tiniest living beings makes the amateur naturalist quiver with emotion. There is communication: the ‘things’ express, talk, proclaim – without words. Within a few yards from the gnarled wooden walls of the cottage Tvergastein there are rich and diverse changing worlds big enough to be entirely unsurveyable.”

This anthology is warmly recommended to anyone who seeks answers to basic questions, how we can relate as responsible members of larger – ‘more than human’ – world.

Thomas Seiler (www.umweltethik.at)

Reviews

G.E. Rosado Haddock, 2008, *The Young Carnap's Unknown Master: Husserl's Influence on "Der Raum" and "Der Logische Aufbau der Welt"*, Aldershot (England), Ashgate, 150pp.

Rosado Haddock's *The Young Carnap's Unknown Master* is a slim book that contains quite a few strong theses. Probably the reader will not agree with all of them. Rather, this booklet offers a good opportunity to think over some well-entrenched ideas the reader may habour about Carnap and his philosophy. To put it bluntly, the author's main thesis reads that Husserl was the young Carnap's "philosophical master", who exerted a profound influence on Carnap's early thought. What is more, Rosado Haddock contends that the later Carnap intentionally masked Husserl's influence on his early work. Against the mainstream of analytic philosophers, Rosado Haddock launches the criticism that they have ignored or played down the importance of Husserl not only for Carnap's thought, but more generally, that they have underestimated, ignored, and distorted Husserl's philosophical achievements. I think Rosado Haddock has a point here, although, in my opinion, he overstates his point. *The Young Carnap's Unknown Master* (henceforth YC) consists of four chapters:

Chapter 1:	Carnap's First Husserlian Book: *Der Raum* (1 – 34)
Chapter 2:	On *Der logische Aufbau der Welt* (35 - 73)
Chapter 3:	Carnap and Husserl on Intersubjectivity (75 - 98)
Chapter 4:	Carnap, Husserl and Analytic Philosophy (99 - 124)

Today, many philosophers and historians of philosophy assume that Carnap began his philosophical career as a neo-Kantian. But, as can be gleaned from Carnap's early writings, he was also influenced by scientists and philosophers such as Dingler, Driesch, Frege, Helmholtz, Hilbert, Husserl, or Poincaré, who did not belong to the Kantian tradition in the strict sense. The more we come to know about Carnap's philosophical beginnings, the more clearly emerges the picture of the young Carnap as a philosophical eclectic. The early Carnap turns out to have been some sort of a philosophical chameleon, a philosophical "Zelig" in Woody Allen's sense, who adapted to various philosophical surroundings. Rosado Haddock would not agree with this pluralist description. He resolutely bets on early Carnap as the student of Husserl and seeks to convince the reader that Carnap began his philosophical career under the spell of Husserl. The overwhelming importance of Husserl is evidenced, according to him, not only by Carnap's earliest philosophi-

cal publication, the dissertation *Der Raum* (1922), but also by his later works, in particular by his *opus magnum Der logische Aufbau der* Welt (1928) and even some subsequent works. There might have been other minor philosophers that had an influence on Carnap's thought, but they were of secondary importance at most.

Although "space" had been a problem for philosophy since its inception, the discoveries of non-Euclidean geometries and Einstein's relativitist theories in the early 20th century dramatically changed the agenda of philosophy of geometry and endowed this classical topic with new momentum. *Der Raum* was Carnap's dissertation written under the direction of his neo-Kantian supervisor Bruno Bauch. It was the most Husserlian piece Carnap ever produced. Rosado Haddock characterizes it as Carnap's "first Husserlian book". Even if one agrees with him that many authors have played down or even ignored Husserl's influence on it, this characterization certainly needs some qualification: *Der Raum* is hardly an orthodox phenomenological work, many other philosophical currents make their appearance in this booklet of just over 60 pages, whose bibliography comprises not less than 275 items.

Carnap's principal aim in *Der Raum* was to dissolve the conceptual difficulties into which philosophy of geometry had been led by the then recent physical and mathematical achievements. To set the stage, he distinguished between three different concepts of space used by scientists, philosophers and mathematicians often without clearly distinguishing between them: space as formal space, space as intuitive space, and space as empirical space. A specifically Husserlian ingredient of *Der Raum* is the concept of intuitive space (*Anschauungsraum*). The concept of intuitive space in Carnap's sense combines elements of Kantian and Husserlian intuition in a peculiar blend. On the one hand, intuitive space is a structure whose particular nature we cannot fully specify. It can only be pointed at by certain contents of experience such as spatial forms and relations like points, linear segments, surface elements etc. On the other hand, the basic properties and relations of intuitive space are independent of experience in the sense that their cognition is not, as with experiential propositions, made ever more secure by often repeated experience. Following Husserl, Carnap maintained that in intuitive space we are not dealing with facts in the sense of experiential reality, but rather with the essence („eidos") of certain data which can already be grasped in its particular nature by being given in a single instance. It is the task of philosophy (phenomenology) to show „which axioms about spatiality can be established by appeal to intuition". Carnap's general answer is that intuition can only relate to the properties and relations of limited spatial regions. More specifically, he claimed that only the local topological properties of space can be intuited, while „we have complete freedom with respect to the total structure we construct from these basic forms."

The concept of *Anschauungsraum* is not the only Husserlian feature in *Der Raum*, also the relations between the formal, the intuitive and the empirical concepts of space are conceived by Carnap in Husserlian terms as corresponding to

the relations between formal ontology, regional ontology, and factual science. On the other hand, the last part of *Der Raum* aims at an, at least *prima facie* justification of a Kantian account of space in that the underlying topological structure is claimed to be the spatial synthetic a priori.

Whether Carnap's Kantian-Husserlian account is really feasible or not, is hard to say. One may well doubt that Kant's *reine Anschauung* can be equated meaningfully with Husserl's *Wesenserschauung,* as Carnap does. Rosado Haddock suggests that Carnap dealt with Kant only to please his (neo)Kantian *Doktorvater* Bauch. I think this is too short-sighted an interpretation. Carnap was then a philosophical eclectic for whom it really did not matter too much whether the "intuition" he dealt with was Kant's or Husserl's. In any case, this question is hard to answer since soon after 1922 Carnap abandoned many of the Husserlian positions of *Raum* and fell back on neo-Kantian or empiricist lines for which intuitions, be they Kantian or Husserlian ones, no longer played a role.

Carnap´s *Der Logische Aufbau der Welt* (in the following *Aufbau*) is a complex work that has found many different interpretations. Traditionally, it the has been read as an empiricist work, more recent interpretations use to emphasize the Kantian or neo-Kantian heritage of this work. Rosado Haddock challenges the traditional empiricist and the more recent (neo)Kantian interpretations. For him, the main debt of the *Aufbau* is to Husserl. More precisely, he considers YC as a "complete refutation" of the presently fashionable (neo)Kantian interpretations. This is a strong claim indeed. But Rosado Haddock goes even further. According to him, the later Carnap intentionally masked Husserl's influence on his early thought, in particular on the *Aufbau*:

> The present work offers a more or less complete view of a forbidden chapter in the history of analytic philosophy, namely, the much more than casual influence exerted by Edmund Husserl on the young Rudolf Carnap's writings. It is a forbidden chapter, since Carnap never acknowledged such an influence, though the influence was overwhelming and decisive, especially in Carnap's *Der Raum* and *Der logische Aufbau der Welt*. (YC, vii)

The reader may raise his eyebrows at this conspiratorial hypothesis, but I think that Rosado Haddock touches upon some important issue.The intellectual biography of Carnap is still less clear than it should be for one of the important figures of 20th century philosophy. In particular, his *Intellectual Autobiography* certainly does not tell the whole story. This is evidenced by the fact that Carnap originally prepared a much more detailed manuscript than what appeared in the Schilpp volume. Whether the "full" version will shed new light on his difficult relation with Husserl remains to be seen. At least sometimes, it seems, also Carnap indulged in a sort of "programmatic history" that eliminated undesired philosophical figures from the scene or reduced their role to a minimum. This may not only hold for Husserl, but also for Driesch, Rickert and others.

In any case, the following facts seem to be certain: After having finished

his dissertation Carnap left Jena and moved to Buchenbach, a small village near Freiburg. In his autobiography he fails to give any convincing reasons for this. Rosado Haddock suggests that he moved to Buchenbach in order to study under Husserl in Freiburg. This may have been a reason, but certainly not the only one. Carnap had a more mundane motif. His affluent parents-in-law happened to possess a large estate in Buchenbach, Carnap had not yet obtained any academic position and hence lived as a *Privatgelehrter* in Buchenbach, before he moved to Vienna in 1925. There are reliable witnesses that in the early 1920s, Carnap attended some of Husserl's seminars in Freiburg. It is not clear, however, whether he had any closer contact with the master himself. After Carnap went to Vienna, Husserl's influence faded away. For the last time, Carnap mentioned phenomenology somewhat positively in his otherwise extremely critical *Überwindung der Metaphysik* (1932), when he discussed several criteria of meaningfulness, among them that of "philosophy (phenomenology)".

Although Rosado Haddock admits that the *Aufbau* is certainly "not simply a Husserlian book like *Der Raum*", he contends that "it is still much more Husserlian than Kantian … and also more Husserlian than Fregean, Russellian or Machian, as some others have believed." (34) His main argument for this contention is based on the similarities between the notions of constitution that both authors use. In line with Verena Mayer's approach[1] he points out that there are important similarities between the concepts of "constitution" in *Der Aufbau* and in Husserl's *Ideen* II (*Phänomenologische Untersuchungen zur Konstitution*).

Rosado Haddock starts with the observation that both authors employ a very general notion of "object" which for Carnap includes "everything about which a statement can be made." Indeed, Carnap even went so far to state „that the object and its concept are one and the same." (*Aufbau* §5). Somewhat cryptically he continued: „This identification does not amount to a reification of the concept, but, on the contrary, is a „functionalization" of the object. (*Aufbau* §5) Apparently, this was meant as an allusion to Cassirer's *„functional concept"* which has, as far as I know, no analogue in Husserl's system. As we shall see, this kind of conceptual hybridization is typical for the *Aufbau* approach but usually ignored by Rosado Haddock.

A constitutional system (in Husserl's and Carnap's sense) is essentially a hierarchy of concepts (or objects) of different levels, in which the concepts (or objects) of higher levels are constituted by those of the lower levels, and, since the relation of constitution is a transitive relation, all concepts are constituted on the basis of a few primitive concepts at the most basic level. Rosado Haddock correctly emphasizes that „constitution" in Carnap's sense cannot be equated with orthodox Kantian constitution. But I am less sure that it can be identified with Husserl's constitution either. For instance, Carnap characterized the *Aufbau's* constitutional

1 V. Mayer, 1991, Die Konstruktion der Erfahrungswelt: Carnap und Husserl, *Erkenntnis* 35, 287-303.

method of quasi-analysis as being closely related to the neo-Kantian distinction between „*Sein*“ and „*Gelten*“. More precisely, the quasi-analytically constituted objects were to be conceived as a kind of valuation of the lower-level objects from which they were constituted. This idea has no analogue in Husserl's system. Rather, it is to be characterized as an originally Carnapian, conceptual hybridization that has no counterpart in any other constitutional system. For Husserl, the intuitive grounding of the constitution was of primordial importance, while Carnap unambigously stated that for him the specific features of the constitutional system mainly dealt with in the *Aufbau,* were of secondary importance. The aim of his constitution theory was not the detailed elaboration of a specific constitutional system but the outline of a general theory of constitutional systems. In particular, Carnap did not subscribe to a Husserlian system based on certain "intuitions". Rather, for him, the basic entities of a constitutional system could be freely chosen according to the preferences of the *Aufbauer*. Hence, Carnap was prepared to accept constitutional systems of various kinds based on Machian elements, *Elementarerlebnisse*, physical objects or whatever entities, since his constitutional theory was designed as a theory of constitutional system*s*, not as a theory of one specific constitutional system. On the other hand, there is, as Verena Mayer pointed out already some 15 years ago, a certain similarity between Husserl's and Carnap's constitutional systems in so far as both are based on "Erlebnisse" from which the higher levels are constituted. As Rosado Haddock rightly emphasizes, "Erlebnisse" cannot be straightforwardly equated with "experiences" as is often done in the English literature. On the other hand, I am not so sure whether "Erlebnisse" can be considered as belonging to the ken of phenomenology: for instance, they also appear in Rickert's writings and in the works of many other authors more or less closely related to the current of "Lebensphilosophie".

Summarizing his efforts to render plausible the Husserlian origins of the *Aufbau*, Rosado Haddock puts forward a really astonishing interpretation of Carnap's *opus magnum*. According to him, the *Aufbau* is plagiarized from Husserl's *Ideen* 2, i.e., his *Studies in the Phenomenology of Constitution*, i.e. the *Second Book of Ideas Pertaining to a Pure Phenomenology and to a Phenomenologicial Philosophy* (1912, 1952). A first draft of this work was written in 1912, but Husserl was dissatified with it and did not publish it. Later he turned it over for editing to his assistant Edith Stein. Husserl still did not like the result. Around 1923 - 1925 his assistant Ludwig Landgrebe worked on the draft. Husserl was still not satisfied and thereby *Ideen 2* were eventually published posthumously in 1952. In the early 1920s, Landgrebe and Carnap befriended each other and used to discuss philosophical issues. According to Rosado Haddock Carnap drew heavy (and illegitimate) advantage from these discussions:

> … It is very probable that Carnap simply obtained his views on [constitutional theory] from Husserl via the indiscrete young Landgrebe. … [This interpretation] is much nearer to the truth than all other interpretations of *Aufbau* ever made. (YC, 48)

Rosado Haddock's only arguments for his "interpretation" are the similarities betwen the constitutional systems of Husserl and Carnap that he exhibits in chapters 2 and 3 of YC, to wit the affinity between the constitutional systems of both authors, and the similar treatment of the intersubjectivity problem. Without doubt, there are important similarities between the two constitutional accounts. It is quite another question, however, whether these similarities allow the strong speculative conclusion Rosado Haddock draws from them.

Later in the book, Rosado Haddock expresses a certain indulgence for the young Carnap's intellectual misdemeanor explaining it by the pressure exerted upon him by his Viennese fellow logical empiricists:

> … the young Carnap probably would have had no misgivings in giving Husserl the proper acknowledgment. But … the hostility of Schlick and Neurath to Husserl's views … made Carnap "forget" his especially important debt to Husserl, … Nonetheless, after Neurath's death in 1945, Carnap still had some twenty-five years to acknowledge Husserl's influence …, but opted to remain silent. (YC, 95)

To me, this conjecture sounds highly implausible for a variety of reasons. For instance, the final version of the *Aufbau*, published in 1928, contains a lot of stuff that Neurath and/or Schlick did not like, e.g. rather positive assessments of a variety of neo-Kantian theses. Carnap would have had plenty of time to eliminate it, but he did not. Moreover, the kind of implicit censorship that allegedly made Carnap "forget" about Husserl, was quite alien to the Vienna Circle's style of discussion.

For a long time Husserl's influence on the early Carnap, and more generally on the emerging analytical philosophy of the first decades of the last century, has been underestimated. This is a state of scholarship less than optimal. On the other hand, Rosado Haddock's extreme theses of the towering role of Husserl for Carnap's thought and (analytical) philosophy in general leads him to some sweeping assessments that are less than compelling. Moreover, he is sometimes expressing himself in a rather polemical style that may distract some of the more tender minds from the contents of what he has to say. For instance, the relation between Carnap and Heidegger as "anti-metaphysicians" is presently the topic of a vigorous debate between philosophers of quite different orientations which may lead to a fruitful exchange between scholars who otherwise would hardly have taken notice of one another. Rosado Haddock makes short shrift with the whole issue:

> Some would say that [Carnap and Heidegger] … had in common being opponents of traditional metaphysics. Such a contention, however, does not deserve any consideration, since the anti-metaphysician Carnap and the ultra-metaphysician Heidegger were opposed to traditional metaphysics on the basis of diametrically different reasons. (70)

For him, the common trait of Carnap and Heidegger – beside the fact that both had studied with neo-Kantian professors and had been Husserl's postgraduate students – is that both plagiarized their common master (70). Such a characterization is cer-

tainly not helpful to overcome the still existing gap between analytical philosophy and phenomenology that Rosado Haddock rightly deplores.

It is surprising that YC does not consider any other early work of Carnap – except *Raum* (1922) and *Aufbau* (1928). Otherwise, the author might have noticed more clearly the young Carnap's astonishing ability of doing philosophy of science in many different keys – Husserl's was only one of them.

Thomas Mormann (Donostia-San Sebastián)

Thomas Ryckman, *The Reign of Relativity. Philosophy in Physics 1915–1925*. Oxford Studies in Philosophy of Science. Oxford–New York etc.: Oxford University Press 2005, 317+ ix pp., 45.00 , ISBN 13 978-0-19-517717-6.

This book presents a clearly written philosophical evaluation of Hermann Weyl's gauge geometry of 1918 and its subsequent mutation in Arthur Stanley Eddington's geometrization based on an affine connection as the sole fundamental structure. We find a huge amount of historically rich insights into motivations and perspectives of two major figures of mathematical physics in a period when (general) relativity stood at centerstage, i.e., between its inauguration in 1915 and the rise of the (then) "new" quantum mechanics in 1925. The story can be read from the overlapping, but never identical, perspectives of history of science and the philosophical study of science.

T. Ryckman starts his study with a characterization of the situation for the philosophy of science brought about by the final formulation of Einstein's general theory of relativity around 1915. He sees it characterized by "two roads from Kant". One was pioneered by Moritz Schlick and Hans Reichenbach. Both started off from a Kantian perspective on the epistemology of natural science and established the road toward *logical empiricism* which later turned into a contributing lane to the broader highway of the analytic theory of science. Crucial intermediate steps on this road were marked by the specific way, not just any, to talk about a "relativized apriori" (Schlick) and "coordinative definitions" of key terms, forming the pillars of a bridge to experimental practice or observational evidence. In this way they intended to complement Hilbert's mathematical axiomatics for the sake of physical theory building; but I guess that they broke away from Hilbert's more holistic view of the role of the axiomatic method in natural science. A similar view was shared by Weyl, Hilbert's famous opponent in so many points, in the latter's reflections on the epistemology of mathematized natural science after the mid 1920s. The reviewer proposes to call this Weyl's *mature view*.[1]

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The second road from Kant, *transcendental idealism*, was exemplified among philosophers by Ernst Cassirer and Edmund Husserl whose differences are, of course, taken full account of in the book (but may be neglected for the sake of this rather general review presentation). The charm of the book lies in its argument that also the scientific proposals of Weyl and Eddington to generalize Einstein's theory of gravitation were strongly motivated by versions of such a "transcendental idealist" point of view.

In order to bring this point home, Ryckman gives a fine an exposition of the Husserlian traits in Weyl's early thought (between roughly 1916 and 1923) and of Husserl's early "transcendental phenomenalism" in the first years after the latter's "transcendental turn" (1906) until about 1913, when the *Ideen zu einer reinen phänomenologischen Philosophie* were published. During these years Weyl was close to Husserl, first as a student then as young researcher and *Privatdozent* at Göttingen, and participated in seminars of lectures of the latter.

Close to the end of this period, Weyl got to know and to love his later wife Helene who was formed by Husserlian ideas during the same years of early phenomenalism and became a strong representative of Husserlian ideas in the communicative microcosmos of Weyl, even after both moved to Zürich (in 1913). In the respective chapters of his book (chap. 5, 6) Ryckman convincingly reestablishes that Weyl not only used Husserlian language to express some of his philosophical reflections on scientific knowledge, but also took up some of the phenomenological motivations in his scientific work. That has long been claimed by other Husserl experts in the philosophical history of mathematical science, but never before the claim was presented so clearly and well documented by a variety of textual records.[2]

On the other hand, the author passes over certain nuances of distantiation in Weyl's reference to phenomenological topics. In this way, Weyl appears in this book streamlined as a true Husserlian. That seems doubtful to the reviewer. To give just one example: The famous Husserlian figure of putting the cognizing subject out of action (*epoché*) leading to an "absolute being of pure consciousness" as kind of "residuum of world's annihilation" (*Residuum der Weltvernichtung*), before any reconstruction of the world can take place in a phenomenologically satisfying way, was taken up by Weyl in a rather specific modification characteristic for his look at phenomenology. Weyl liked to talk of the coordinate system in differentiable manifolds as the "residuum of the Ego's annihilation" (*Residuum der Ichvernichtung*). This is very difficult to decode from the point of view of classical history of mathematics. Ryckman convincingly deciphers it as mirroring Husserl's epoché. In his expression, Weyl indicated the role of the coordi-

2 This convinces the reviewer that an argument presented in the history of mathematics some years ago by him, that Weyl used Husserlian language rather more than he adhered to philosophical motivations of Husserlian phenomenology, was too cavalier and has to be refined (chap. 5, fn. 29).

nate system as a kind of apriori structure of the subject, necessary before any empirically related knowledge in (relativistic) physics can be acquired (or even meaningfully be talked about). But that is all: The role of the Ego is downplayed ("annihilated"); then the structures which symbolically represent some otherwise unknowable "transcendent" (objective) world can be formulated and come into the center of investigation. In this sense Weyl's "annnihilation of the Ego" turned Husserl's *epoché* upside down (or downside up, depending on the reader's view), an orientation reversing rhetorical procedure which indicates the different roles of the subject in Husserl's phenomenology and in Weyl's discussion of the space concept in mathematical physics. T. Ryckman owes the merit for having directed our attention to this specific Weylian figure to Husserl's *epoché*; but he does not discuss the orientation reversion and its meaning; he rather talks of a "parallelism" of the two (p. 131ff.).

This is more than hairsplitting with regard to modes of description. By other formulations (e.g., in the often quoted passage on Husserlian analysis of essences at the end of the passage on the analysis of space in the 4th edition of *Raum Zeit Materie*) Weyl definitely insisted on counter-positions to true Husserlian phenomenology, as much as the epistemology of natural sciences is concerned. In fact, Weyl drew strongly upon other philosophical resources at the same time, mainly Fichte's *Wissenschaftslehre*, brought close to him by Fritz Medicus just like Husserl's philosophy was brought close to him by his wife Helene in the years under discussion. Surprisingly – to those who have not yet dealt with these questions – Weyl, in these years, considered certain aspects of Fichte's philosophy as a guide for what he started to call symbolic construction of the external world [Scholz 2005a, Sieroka 2007, Sieroka 2009a].

This aspect is not T. Ryckman's subject; it is even completely shaded off in this otherwise so meritorious book. This should not be understood as a reproach. Our author is primarily a philosopher, rather than a historian of science – although one with tremendously broad and detailed knowledge of the history of his subject. It cannot surprise that he succumbs to the temptation to stylize his historical main figures (Weyl and Eddington) to philosophical witnesses for what seems to be his own agenda in the present discourse on the philosophy of science: reestablishment of the position of an (enlightened) transcendental idealism against an analytical outlook on the philosophy of science. Analyzing the Fichte connection would deflect from this main line of argument of the book. It has to be added that most of the literature mentioned in this respect was not yet publicly available at the time of writing the book.

For the reviewer (coming from the background of history of mathematics and physics) it is a joy to find here a book large parts of which are devoted to a serious and up to date philosophical study of H. Weyl's scientific and philosophical contributions. Their content and role for the first phase of the rise of modern physics has still been sifted only partially for the present discussion on the philosophy of physics and the role of mathematics in it. Tom Ryckman makes a huge step in

this direction; and he does so with a clear philosophical agenda, a rehabilitation of *transcendental idealism* (in the sense of Kant and Husserl and followers). We, as readers, need not necessarily identify it with the mature perspective of H. Weyl, in which the transcendental role of concepts became relativized by an increasing sensibility for constitutive parts of scientific knowledge, which are rooted in genuinely empirical practices.

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Erhard Scholz (Wuppertal)

Erhard Oeser, *Popper, der Wiener Kreis und die Folgen. Die Grundlagendebatte der Wissenschaftstheorie,* WUV: Wien 2003.

As Michael Friedman and Alfred Nordmann recently put it, "the time is ripe for fruitful collaboration of historians of philosophy, historians of science, and historians of mathematics in developing a richer and more nuanced picture of the Kantian legacy in the nineteenth century and beyond. […] Its our conviction, in addition, that the development of modern scientific thought more generally […] can also be greatly illuminated when viewed as an evolution from Kant, through Poincaré, to Einstein"[1].

At first sight, the monograph under review "Popper, der Wiener Kreis und die

1 Friedman, M./Nordmann, A., eds.: *The Kantian Legacy in Nineteenth-century Science*. Cambridge [Mass.] 2006, 1.

this direction; and he does so with a clear philosophical agenda, a rehabilitation of *transcendental idealism* (in the sense of Kant and Husserl and followers). We, as readers, need not necessarily identify it with the mature perspective of H. Weyl, in which the transcendental role of concepts became relativized by an increasing sensibility for constitutive parts of scientific knowledge, which are rooted in genuinely empirical practices.

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Folgen" seems to advance such a collaborative project. In his book, Oeser intends to give a large-scale diachronic analysis of the formation and transformation of the philosophy of science as a scientific discipline in its own right. Explicitly with regard to Eduard Dijksterhuis' frequently cited, and certainly penetrating, dictum that "the history of science forms not only the memory of science, but also its epistemological laboratory"[2] (cf. p. 145), he initially reconstructs the conversion of the aim and structure of methodological theory given by William Whewell, Ludwig Boltzmann, Ernst Mach, Albert Einstein and Erwin Schrödinger (chapter 1). Subsequent to this pretty narrow view on the debates among philosopher-scientists in the late nineteenth and early twentieth century, Oeser outlines their specific impact on Logical Empiricism (including pre-history and peripherals; chapters 2 & 3) and the further transformation of the methodological claims[3] of Popper, Lakatos and the so called 'historical school' of the New Philosophy of Science (chapters 4 & 5).

In the final Chapter 6, Oeser elaborates on his conception of a general philosophy of science as a technology of scientific progress (in the natural sciences as well as in the humanities) that depends heavily on a naturalistic interpretation of an integrative functional interaction between inductive and deductive reasoning ("Funktionszusammenhang von Induktion und Deduktion", p. 230). This specific kind of functional interaction is traced back primarily to Popper's early account of hypothesis generation, which is, according to Oeser, ultimately based on a natural selection interpretation of Kantian epistemology (cf. p. 213). While this coherent and very interesting historico-systematic approach is thoroughly corroborated by recent scholarship[4], Oeser's philosophical analysis seems to be a bit self-absorbed. His systematic claim – for instance – that within an advanced methodological theory a logic of discovery (in the literal sense), a 'heuristic of induction', is necessarily intertwined with an overarching axiomatic-deductive theory ("Prinzipientheorie", cf. pp. 227f.) is nowadays a very controversial issue. So Oeser's arguments certainly should have been related to contemporary debates, perhaps to the recent 'unity/disunity debate' or at least to some recent work done within the scope of 'inductive reasoning' in general[5].

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2 Dijksterhuis, E. E.: *The Origins of Classical Mechanics. Critical Problems in the History of Science.* Madison 1959.

3 For a recent analysis see Carrier, M.: "The Aim and Structure of Methodological Theory", in: L. Soler, H. Sankey, P. Hoyningen-Huene (eds.): *Rethinking Scientific Change and Theory Comparison: Stabilities, Ruptures, Incommensurabilities*. New York 2008.

4 For a recent analysis of Popper's early heuristic see Shah, M.: "The Logics of Discovery in Poppers's Evolutionary Epistemology", in: *Journal for General Philosophy Science* 39/2 (2008).

5 See for instance Morrison, M.: *Unifying Scientific Theories. Physical Concepts and Mathematical Structures*. Cambridge 2000, esp. chapters 1, 2 & 6 and Cartwright, N.: *The Dappled World. A Study of the Boundaries of Science*. Cambridge 1999, esp.

In addition to this rather idiosyncratic flavour and in contrast, firstly, to the well-written standard history of Popper, Kuhn, Lakatos and Feyerabend, and, secondly, to the merit of bringing a few interesting aspects of William Whewell's and Victor Kraft's work to a wider audience, there are unfortunately some significant lapses in the diachronic analysis given in the first three chapters.

Even though the author makes it quite clear in the preface that he intends to confine himself to a Viennese and a natural sciences perspective, it is irritating to read virtually nothing about the neokantian tradition both in the early 20th century and in the recent philosophy of science. In the light of this neglect it is not surprising – for instance – that within the discussion of Moritz Schlick's early notion of probability Johannes von Kries is not mentioned, even though his 'Spielraum'-conception of probability was highly influential for Schlick as well as for the later Vienna Circle (VC) and Karl Popper.[6] But it is indeed surprising – keeping in mind the Viennese focus of the analysis – that we read nothing about Edgar Zilsel's monograph "Das Anwendungsproblem"[7]. A closer look to the first edition of the "Allgemeine Erkenntnislehre"[8] would have shown that Schlick refers primarily to Zilsel, and not so much to Richard von Mises, as Oeser pointed out (cf. pp. 67f.). It is another historiographic inaccurateness to overemphasize a one-sided relation between Einstein and the philosophers around the VC (cf. pp. 48f.). As recent scholarship on the history and prehistory of the VC exhibited, we should (particularly with respect to Moritz Schlick) rather assume a fruitful interaction[9].

Given such lapses in accurateness one could suppose that the historical equipment of Oeser's epistemological laboratory is possibly not well adjusted and therefore some errors of measurement are to be expected. In sum: Oeser's methodological claims seem to be built on shaky historiographic ground.

Carsten Seck

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part I.

6 See for instance Kries, J.: *Ueber den Begriff der objectiven Möglichkeit und einige Anwendungen derselben*. Leipzig 1888. Cf. Heidelberger, M.: "Origins of the logical theory of probability: von Kries, Wittgenstein, Waismann", in: *International Studies in the Philosophy of Science* 15 (2001) and Seck, C.: *Theorien und Tatsachen*. Paderborn 2008, esp. 117-132.

7 Zilsel, E.: *Das Anwendungsproblem. Ein philosophischer Versuch über das Gesetz der großen Zahlen und die Induktion*. Leipzig 1916.

8 Schlick, M.: *Allgemeine Erkenntnislehre.* Berlin 1918.

9 See for instance Engler, O.: "Moritz Schlick und Albert Einstein", in: *Schriftenreihe des Max-Planck-Instituts für Wissenschaftsgeschichte.* Berlin 2006.

Deborah R. Coen, *Vienna in the Age of Uncertainty. Science, Liberalism and Private Life*. The University of Chicago Press, Chicago and London, 2007, xi + 380 pp., ISBN-13: 978-0-226-11172-8, ISBN-10: 0-226-11172-5

Coen's book tells the story of the Exner family and of their contributions to the Viennese political, scientific and artistic life over three generations, beginning from the philosopher Franz Exner (1802–53) and ending with the dispersion and different fates of members of the family with the rise of fascism, and then of Nazism, in Austria.

As the book's title suggests, this story is only a means to reflect on much more general issues, both politico-historical and related to the history of ideas and of science. With respect to the former, Coen focuses in particular on the fate of Austrian liberalism and on the origins of modernism: she intends to challenge the widespread thesis (originally due to Schorske) which relates the political failure of Viennese liberalism at the turn of the 20th century to the bourgeoisies' giving up their ideal of objective (rational, scientific) knowledge and their political commitment in favour of subjectivity and the retirement in the private sphere.

In rethinking the dichotomies reason/uncertainty and public/private, Coen deals with the history of science and of ideas as well. With regard to the first dichotomy, Coen's main tenet is that probabilistic reasoning, since it admits uncertainty and at the same time "tames" it through mathematical means, played a central role in the Austrian liberal's attempts to refuse dogmatism without giving way to relativism. With regard to the second dichotomy, Coen aims to show how a continuity between the private and public sphere was characteristic of the Exners' way of life, and how such a continuity characterized both the ideals of Austrian liberals on education and the way Austrian science was performed.

Coen devotes particular attention to the bourgeoisie's custom of family retirement in the Sommerfrische (summer resort): a second home far away from the city and immersed in nature, where the children could learn to be autonomous (according to the liberal ideals on education) and where observation and scientific experiments could be performed in an unconstrained environment. The tradition of the Sommerfrische is indeed a pivot in Coen's effort to characterize a 'typically Austrian' way of pursuing science, exhibiting an empirical approach to scientific inquiry, a favourable disposition to deal with uncertainty without dismissing it as due to human ignorance or "error", and the custom to work in a family or a family-like group in which a subjective point of view could be progressively 'objectified' through communication.

In general, it could be argued that Coen identifies and analyses some 'third ways' and tries to relate them with each other: family, as a 'third way' between individualism and conformity; probabilistic reasoning, as a 'third way' between uncertainty and determinism; liberalism, as a 'third way' between conservative dogmatism and what she calls (p. 349) "revolutionary relativism". In this 'third space', and with the fundamental contributions of the Exners, Austrian science and

arts could flourish and give rise to the fin-de-siècle Viennese "Golden Age" and to Viennese modernism.

To keep together all these themes and dimensions is indeed a hard task. Sometimes the reader may have the impression of an 'underdetermination' of the author's theses by the historical evidence given to support them, as if some supplementary deal of interpretation or a particular perspective were needed in order to bring back all single pieces into the global picture. Of course, this is to some extent implied in the historians' work (unless we want them to be determinist!). Still, the possibility of keeping all pieces of the story together seems sometimes to rely – for example – on a conflation of the concepts uncertainty/scepticism/probability/induction/chance (not always sufficient evidence is given, that this conflation is an historical matter of fact, rather than the result of an a posteriori reconstruction), or, with respect to politics, of relativism/particularism (when Coen talks about "revolutionary relativism", she refers in fact to those allegedly particularistic ideologies, nationalism and socialism, against which the liberals presented themselves transcending nations or class divisions. However, to define nationalism or socialism either relativistic or particularistic ideologies seems a quite disputable choice).

These kinds of problems seem to be a by-product of the challenge at the core of the book: to move beyond the Exners' story either to generalize or to typify some features of the Exners' work and thought (it is significant that the Exners are not even mentioned in the book's title). These moves not always succeed straightforwardly. With respect to generalization, Coen shows very well how relevant were the Exners within Austrian liberalism, Austrian science and within Austrian academic life, but sometimes the question remains open about their being representative.

With respect to typification, Coen joins some recent efforts to identify 'typical Austrian' traits in Austrian epistemology, science, art and culture in general, with particular attention to distinguish them from German ones. Very interesting indeed is her attempt to develop Stöltzner's concept of "Vienna indeterminism" by linking it to Austrian liberalism in the (specifically) Austrian political situation between the 19th and the 20th century. But, of course, to support theses about national characters and differences a lot of evidence is needed (and beyond the inquiries on 'famous' personalities), including counterfactual evidence (the tradition of the Sommerfrische, for example, was common in Germany and in Switzerland too: why should it not have given rise to the same 'Austrian' traits there?).

From a historical point of view, Coen's book has the unquestionable merit of collecting, achieving and organizing into a coherent, sometimes illuminating story a huge amount of knowledge about the Exners, their scientific work and their fundamental role in Viennese life. Coen's patient and passionate reconstruction relies on a great deal of primary sources, which includes both Exners' scientific publications in a wide range of scientific fields and autobiographical writings by the Exners' themselves as well as some of their correspondence. In 19th–20th century Austria the Exners played a really key role in the public sphere as well as in

scientific life: several of them had a very high institutional position (membership of the Parliament, rectorship of the University, collaboration with Ministries), ten of them (within three generations) were university professors, and among Exners' students or friends were scientific personalities like Loschmidt, Freud, Boltzmann, Schrödinger, Brahms, Smoluchowski, to mention just some. Furthermore, over the course of three generations the Exners produced very relevant scientific achievements in a wide range of disciplines, from law to physics, from biology to meteorology, to physiology, and they participated (in particular some women of the family) in the flourishing artistic and cultural life of the Vienna fin-de-siècle. Not only provides Coen accurate insights in the very different fields of the Exners' work, but she also allows us to see some illuminating continuities between these fields, continuities which can emerge only in considering family and scientific life as intertwined. Furthermore, following the Exners over three generations, Coen comes to deal with very significant issues in the history of science, shining light on many neglected ones or providing new perspectives on 'classical' ones (most interesting is, for example, her view on Schrödinger's work).

In this book Coen provides a great deal of new important knowledge in the history of science and of ideas thanks to her original way to approach different themes and relate them to each other, although some of her most 'audacious' theses are still in need to be supported by new evidence.

Donata Romizi (Wien)

Esther Ramharter (ed.). *Prosa oder Beweis? Wittgensteins ‚berüchtigte' Bemerkungen zu Gödel. Texte und Dokumente.* First edition. Parerga Verlag, Berlin, 2008. 176pp.

The volume presents assorted texts regarding comments by Ludwig Wittgenstein concerning Kurt Gödel's first incompleteness theorem. Its editor, who also contributed a lengthy introductory essay to the volume and translated many of its texts into German, divides the material into two groups: the first group comprises Gödel's 1931 paper "Über formal unentscheidbare Sätze der *Principia mathematica* und verwandter Systeme I",[1] given here in facsimile, containing the proof of the theorem, swaths of text from Wittgenstein's posthumously published *Bemerkungen über die Grundlagen der Mathematik* (henceforth, RFM),[2] a compilation of passages taken from several of his philosophical notebooks and diaries belonging

1 First published in: *Monatshefte für Mathematik und Physik*, 38, 1931, pp. 173-198.

2 First published as *Remarks on the Foundations of Mathematics / Bemerkungen über die Grundlagen der Mathematik*. Oxford: Basil Blackwell 1956.

scientific life: several of them had a very high institutional position (membership of the Parliament, rectorship of the University, collaboration with Ministries), ten of them (within three generations) were university professors, and among Exners' students or friends were scientific personalities like Loschmidt, Freud, Boltzmann, Schrödinger, Brahms, Smoluchowski, to mention just some. Furthermore, over the course of three generations the Exners produced very relevant scientific achievements in a wide range of disciplines, from law to physics, from biology to meteorology, to physiology, and they participated (in particular some women of the family) in the flourishing artistic and cultural life of the Vienna fin-de-siècle. Not only provides Coen accurate insights in the very different fields of the Exners' work, but she also allows us to see some illuminating continuities between these fields, continuities which can emerge only in considering family and scientific life as intertwined. Furthermore, following the Exners over three generations, Coen comes to deal with very significant issues in the history of science, shining light on many neglected ones or providing new perspectives on 'classical' ones (most interesting is, for example, her view on Schrödinger's work).

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to his literary estate (henceforth, *Nachlass*),[3] and excerpts from various sources testifying to the initial negative response Wittgenstein's comments on Gödel's theorem drew from philosophers and mathematicians alike, including Gödel himself. The editor's effort to make remarks on Gödel contained in Wittgenstein's *Nachlass* writings even more easily available by including them in the present volume is certainly commendable. It would have been desirable if the publication of these passages in hard copy form included those phrases deleted by Wittgenstein in the autograph documents as well as those phrases he inserted and viewed as variants of existing phrases, as is customary in major critical-genetic editions and in OUP's edition of the *Nachlass*. Also, the lack of a minimal scholarly apparatus assigning probable dates of composition to individual passages whenever possible is regrettable and renders recourse to OUP's edition necessary for effective scholarly work. The second group consists of the reworked version of a paper by Juliet Floyd and Hilary Putnam,[4] a text by Jacques Bouveresse,[5] an original contribution by Richard Heinrich, and two excerpts from a translation into German of Douglas Hofstadter's popular 1979 book *Gödel, Escher, Bach: an Eternal Golden Braid*, whose inclusion in Ramharter's volume puzzled this reader into momentary silence and which shall be excluded from the following discussion. In their "Wittgensteins 'berüchtigter' Paragraph über das Gödel-Theorem. Neuere Diskussionen", Floyd and Putnam defend claims originally set forth by them elsewhere concerning two consecutive passages in RFM against criticism by others. Floyd, whose earlier work helped re-open the debate on the question of the philosophical significance of Wittgenstein's remarks on Gödel, and Putnam recapitulate their claims that what Wittgenstein is concerned here with is a hypothetical scenario in which the Gödel sentence has been refuted in the system of PM, and that the consequence Wittgenstein draws from this supposed derivation of the sentence's negation essentially amounts to the conclusion to be drawn from the ω-inconsistency of the system, provided the latter is consistent. Their first claim runs counter to established readings which view the passages in RFM as a bungled attempt to refute the first incompleteness theorem. The issue of ω-inconsistency, or rather, the failure to address it, lies at the heart of their discussion of a reading proposed by Mark Steiner in a 2001 paper. The authors maintain that Steiner's reading is anachronistic for its reliance on subsequent proofs of closely related theorems, such as the strengthening of Gödel's proof by J.B. Rosser, which replaces the assumption of ω-consistency by that of consistency of the system of PM. The issue returns in the final section of their paper where they defend their reading against criticisms made by Timothy Bays. In his "Non ignoramus nec ignorabimus",

3 Previously published in electronic form on CD-ROM by Oxford University Press as part of their Wittgenstein's *Nachlass: The Bergen Electronic Edition*.

4 "Bays, Steiner and Wittgenstein's 'Notorious' Paragraph About the Gödel Theorem", in: *The Journal of Philosophy*, 103, 2, 2006, pp. 101-110.

5 Chapter 10 of his *Le Pays des possibles: Wittgenstein, les mathématiques et le monde réel*. Paris: Les Éditions de Minuit 1988.

Jacques Bouveresse does not discuss Wittgenstein's remarks on Gödel. Instead, he maintains that Wittgenstein sides with David Hilbert against L.E.J. Brouwer on the question as to whether there are undecidable propositions in mathematics, that is, open mathematical problems which, by some kind of necessity, are not amenable to a solution. Drawing on a narrow range of passages almost all of which were composed in the late 1920s and early 1930s, Bouveresse argues that Wittgenstein's reasons for dismissing the "ignorabimus" in mathematics, unlike Hilbert's, are not epistemological but "logical" or "grammatical". For Wittgenstein, on his view, the question really presents a false dichotomy rooted in a failure to grasp some essential grammatical difference between mathematical propositions and statements of the empirical sciences. Bouveresse's attempt to characterize this supposed difference turns on expansions of Wittgenstein's frequent comparisons between mathematical propositions and games. He takes these comparisons to encapsulate the idea that, characteristically, a necessary condition for the meaningfulness of a mathematical proposition is the existence of a "calculus" (some unspecified sort of usage of mathematical signs) part of which is the proposition's proof, rather than the existence of special kinds of objects or the existence of special sorts of facts. Bouveresse believes that, for Wittgenstein, there is no "ignoramibus" in mathematics on account of the meaninglessness of the notion of a mathematical proposition without a proof. In his "Bedeutungslose Offenbarung. Philosophiegeschichtliche Anmerkungen zu Wittgensteins Gödel-Notizen", Richard Heinrich explores various similarities of views he attributes to Wittgenstein, Descartes, Pascal, and Kant on the nature of mathematical propositions. He goes on to bring the results of these investigations to bear on several of Wittgenstein's remarks on Gödel. Towards the end of his paper, Heinrich puts forward several intriguing interpretive suggestions. Among them is the idea that Wittgenstein took the demand for the consistency of axiom systems to be proved prior to their employment as formalizations of bodies of knowledge to depend for its justification on a fixed notion of the reliability of axiom systems. It is the assumption of the fixity of the notion of reliability that, according to Heinrich, Wittgenstein critically examines in exchanges with Alan Turing in 1939 as well as in remarks on machines and proofs in RFM.

Enzo De Pellegrin (Wien)

Ortrud Lessmann, *Konzeption und Erfassung von Armut. Vergleich des Lebenslage-Ansatzes mit Sens „Capability"-Ansatz.* Volkswirtschaftliche Schriften, Heft 552, Berlin: Duncker & Humblot 2007, 359p.[1]

Behind the unpretentious and somewhat unwieldy title of this book lurks a question which, in recent decades, has received growing attention in economics and philosophy thanks to the works of Amartya Sen. Can economic theories make the wealth of individuals and groups the subject of their studies in a different way than interpreting it in terms of income? Into what theoretical and methodological components does this question have to be broken down for it to be answered in a serious way? This question is motivated by the insight that it often leads to unsatisfactory results when one tries to calculate the life standard of a population on the basis of monetary income. This monetary calculation only partially grasps the reality of wealth and poverty and also somewhat obscures the real conditions. For economist and sociologist Otto Neurath this had a radical consequence in terms of philosophy of science. He believed that the focus of economic theories on the study of market relations and monetary economics could result in economists losing sight of their actual subject: the wealth of nations. According to Neurath wealth had to be conceived as a multi-dimensional totality which could not be calculated by reducing it to one unit of measurement (be it money or utility). It can, however, be studied scientifically on the basis of higher-developed mathematical methods. The application of relational calculi in the social sciences was at that time still in an embryonic stage, which explains why Neurath's methodological suggestions remained rudimentary. Since the 1980s, Amartya Sen has developed a multi-dimensional approach which is based in part on game theory and is formalized in its basic features.

The present study focuses on a specific aspect of this issue, namely the theoretical analysis of poverty. It sees it as a part of a more general theoretical problem, namely how the well-being – or lack of well-being – of individuals and groups can be properly grasped and studied. Lessmann reconstructs a field of conceptual and methodological problems that can be identified historically between Neurath and Sen. In the history of theory that spans almost the entire 20th century Lessmann describes a number of highly interesting models that show how complex theoretical developments in this area were.

The book begins with a differentiated introduction to the present state of assessing poverty and focuses on several basic issues with which all theories to be studied in the following are confronted with. Is it possible to set an absolute boundary by which poverty can be qualitatively delineated from non-poverty and which is valid for all times and all places and all individuals? Or can poverty only

1 The text of this review including the quotations from the book has been translated by Camilla Nielsen.

be defined in relation to the social environment? (23 p.[2]) Which methods can be used to define the boundary of poverty (conceived as absolute or as relative)? Should one make a list of things that someone must have to not be poor? Or would it be better to calculate the income that is required to buy the things that this list names? Can "experts" even say what belongs to the most necessary things or isn't this a very personal matter? Lessmann describes the classical measures of poverty that only consider the differences in income between individuals and shows the limits of several of these measures of poverty. Lessmann's real interest are, however, the approaches of multi-dimensional poverty measurement that assume that not all dimensions can be converted into monetary units. "The comparability of incomes in relation to life standard is not immediately given for two reasons: First, people are different and thus have different needs and second, incomes are not directly assigned to individuals but usually to households. Children, for instance, do not have any income so that there is then the question of what income can be assigned to them." (p. 49) It quickly becomes clear how difficult it is to consider the large number of issues that emerge when one considers the differences between individuals and the various dimensions of poverty and wealth. One of the greatest problems always appears where dimensions that cannot be measured cardinally are to be included such as health or education. "In multi-dimensional measurement of poverty two problems emerge which the one-dimensional measurement of poverty does not have: On the one hand, the identification of the poor is (…) no longer clear, since a person can be poor in one dimension and non-poor in another dimension. The question arises whether it is possible to offset the various dimensions. On the other hand, in a multi-dimensional spread it has to be defined when a distribution is more equal than another one, i.e., here, too, it must be clarified whether it is possible to balance the various dimensions." (p. 55)

The theoretical history reconstructed by Lessmann in the following starts in the years before World War 1, at the time when Neurath began developing his multi-dimensional conception of wealth and poverty. Recent research has spawned a number of publications on Neurath's economic theories. They drew attention to ecological arguments in his writings and to the relations between his economic and his ideas on theory of science.[3] An important subject of research has been Neurath's position on the economic theory of his time and his contributions to the planning debates of the 1920s and 1930s. Whereas earlier studies focused on philosophical and historical issues, Lessmann's turns her gaze to Neurath's

2 All page numbers refer to O. Lessmann's book.

3 Here are a few examples: Martinez-Alier, J. *Ecological Economics. Energy, Environment and Society*, Blackwell, London 1987. O'Neill, J.: *The Market. Ethics, Knowledge and Politics, Routledge*, London 1998. Thomas E. Uebel: "Neurath's Economics in Critical Context", in: *Otto Neurath: Economic Writings. Selections 1904–1945*, Vienna Circle Collection Vol 23, Dordrecht: Kluwer 2004, pp.1-108. Elisabeth Nemeth, Stefan W. Schmitz, Thomas E. Uebel (Eds.): *Otto Neurath's Economics in Context*. IVC Yearbook 13/2007.

economic writings with the eyes of an economist who has an interest in the issues of today's poverty research. She takes Neurath's approach to life situation as a theoretical suggestion seriously and analyzes its structure. Neurath had taken the "Lebenslage" (life-situation) concept from Engels and used it to refer to the living conditions of individuals and groups of people. The terminology in which he tried to systematically develop his theory of life conditions sounds strange even today and may be one reason why the term has hardly been taken seriously. Neurath distinguished between life conditions, life moods, life ground, life order, economic order, to name just some of the central systematic terms. Lessmann succeeds in showing that these terms were introduced not only for good objective reasons and clearly relate to each other. She also shows that in this theoretical framework Neurath already identified a large number of problems with which economists seeking to understand wealth and poverty as a multidimensional phenomenon still grapple today. The most important themes can only be cited here: comparability of orders of poverty / orders of wealth, cardinal / ordinal ordering, completeness / incompleteness of order of elements of affluence, selection of the dimensions of poverty / wealth, selection of indicators, etc. Lessmann also shows that theoretical elements can be found in Neurath that are lacking in later theories of life-situations and which make his approach appealing to the present day. It is significant, for instance, that Neurath introduces the aspect of time by not just speaking about "life situations" but also about "life trajectories" (*Lebensläufe*) (see, e.g., p. 69). A further original element can be found in the form of exposition of the multidimensional life situations. Neurath found "geometric" renderings (he was referring to Pareto's indifference curves) to be unsuited for multidimensional relations since only two to three variables could be related to each other with them. This resulted, according to Neurath, in a problematic limitation of studies. He thus advocated a form of representation that normally originates in geography and that Lessmann thus calls "geographic". (p. 73) A direct path leads from the "life-situation reliefs" and "life-situation silhouettes" to Neurath's ISOTYPE method by means of which economic and social relations were to be visualized. In the German-speaking world Erich Weisser's teaching of social politics has popularized for the term "life situation". He used this term for the first time in 1921 (p. 93) after having attended one of Kurt Grelling's seminars on social policy in Göttingen in 1919/20. In this seminar Grelling had introduced and reinterpreted Neurath's term of life situation (p. 86). An individual's "life situation" is, according to Grelling "the totality of possible life attitudes from which he can select at the beginning of the period (…)" (p. 90). Grelling's definition of "life situation" thus accentuates that individuals can select from possible life attitudes. With this use of the term the term "life situation" began its trajectory of reinterpretation and transformation. Lessmann analyzes this transformation with great care and in so doing keeps discovering surprising connections to modern economic theory (e.g., between Nelson – Grelling – Samuelson, p. 90). Here the acting individual who stands in a field of possibilities assumes a systematic position in the life-situation theory. After all,

Weisser did not succeed in making full use of the theoretical potential lying in the idea of a scope of possibilities of action. He wanted to posit a normative theory of human basic concerns which separated his approach from the purely empirical study of life situations. This delineation led him, as Lessmann argues, into a theoretical dead-end (p. 108). In any case both, Grelling and Weisser, wanted to give a systematic status in their theories to the field of action of individuals. Both had been influenced by Leonard Nelson's teachings, according to which man is endowed with an innate interest in reason-based self-determination (p. 86). From here there is a direct line leading to Amartya Sen whose economic theory concedes a priority place for individuals' fields of action. His "capability approach" has been much further elaborated than the "life-situation" approaches of Neurath, Grelling and Weisser and parts of the theory are presented in formal terms. Lessmann gives an excellent description of Sen's theory. She analyzes the underlying image of man and draws attention to the fact that Sen's approach differs more radically from Martha Nussbaum's than is often assumed. She studies the philosophical underpinnings – in particular Sen's understanding of freedom as opposed to Rawls and Nozick. She gives special attention to the question as to how Sen operationalized his approach with a view to related theoretical approaches and a wealth of empirical studies. Lessmann examines these studies taking into account above all methodological aspects: how are the lists of wealth dimensions compiled? Which indicators are seen as expressive? What are the possible data sources? And what methods of comparison are used? Lessmann thus gives an overview of the broad range of studies that were inspired by Sen's "capability approach". The thoughtful systematic study is also of enormous value to philosophers. In recent decades Sen and Nussbaum have received great attention in philosophical debates. Lessmann's differentiated account of the methodic issues shows how far back the traces of epistemological and anthropological basic assumptions can be traced in the actual work of economists.

The book derives its specific profile from the tension between systematic and historical perspectives. Lessmann describes the problems of multidimensional measurement of affluence and assesses its value for today's study of poverty. Here she includes historical positions that in comparison to present-day theories may, at first glance, appear obsolete. She draws the historical background and the steps that led from Neurath via Grelling to Weisser, but also takes their approaches seriously in a systematic sense. After reconstructing the historical theories with great care se devotes the most extensive chapter of the book (pp. 204-314) to a systematic comparison. This chapter deals with the theoretical structure of the various approaches and the problems of operationalization and concrete issues of poverty measurement. Here there are inevitably overlaps in subject matter between the chapters. This may prove tedious for someone reading the book from cover to cover. The structure, however, has the big advantage that in each individual chapter the perspective remains focused on both the systematic and the historical context. In this way the author is able to create an expanded context for examining

and assessing the approaches that are now the subject of discussion.

From the perspective of Neurath research the book is extremely useful. It is the first work that confronts Neurath's early writings with modern approaches of the measurement of affluence. The theoretical elements that Lessmann uncovers are not just an important complement of existing studies in philosophy and economics. Her study also provides valuable material for a deeper understanding of the visualization method that Neurath developed together with Gerd Arntz and which his colleage and later wife Marie Reidemeister-Neurath dubbed ISOTYPE. Lessmann refers explicitly to this connection and thus gives those who are interested in Neurath's pictorial language an important reference for further research.

Elisabeth Nemeth (Wien)

Activities of the Institue Vienna Circle

Activities 2008

8th Vienna International Summer University
Scientific World Conceptions (VISU/SWC)
"History and Philosophy of the Medical Sciences"
Main Lecturers: Rachel Ankeny (University of Adelaide, Australia), Bernardino Fantini, (University of Geneva, Switzerland), David Wootton (University of York, UK)
Guest Lecturer: Keith Wailoo (Rutgers University, US)
Date: Jun 30 – July 11, 2008
Venue: Kapelle and Department of Contemporary History, University Campus, Court 1
Together with: University of Vienna / Department of Contemporary History
http://www.univie.ac.at/ivc/VISU
VISU-SWC is part of the doctoral program "The Sciences in Historical Context" of the University of Vienna, 2006–2009. www.univie.ac.at/HPS/

International Symposium
The Philosophy of Science in a European Perspective
Opening conference of the 5-year programme of the Europan Science Foundation (ESF) with 13 countries participating
Date: December 18-20, 2008
Venue: University Campus, Vienna
www.esf.org

Internationaler Workshop
Wiener Methode der Bildstatistik und ISOTYPE –
Forschung und Dokumentation
Date: April 11, 2008
Venue: Institut Wiener Kreis

International exchange programme as part of the ÖAD – Stiftung Aktion Österreich-Ungarn
Wissenschaftsgeschichte und Wissenschaftsphilosophie –
Der Wiener Kreis in Ungarn
Date: 4 bilateral workshops in Vienna and Budapest during the academic year 2007/08

Venue: Eötvös Loránd Universität Budapest (ELTE) und Universität Wien, Institut für Zeitgeschichte
International closing conference:
Date: May 18-21, 2008
Venue: Collegium Hungaricum Wien

Trilateral programme de formation-recherche du CIERA (Centre interdisciplinaire d'etudes et de recherches sur L'Allemagne)
La Philosophie des Sciences en Autriche et en France au XXéme siécle: histoires croisées, héritages, réceptions et influences réciproques
Together with: Université Paris 1 et IHPST/UMR und Institute of Philosophy (University of Vienna)
Date: 3 Journées et 1 Colloque in Vienna and Paris, 2007–2009
Venue: Vienna and Paris

Workshop
Moritz Schlick Project: Critical Edition of the Complete Works and Intellectual Biography
Phase 2: 2006–2008
Zusammen mit: Universität Graz und Universität Rostock
Date: February 15, 2008
Venue: Institut Wiener Kreis
http://www.univie.ac.at/ivc/Schlick-Projekt/

Vertreibung und Rückkehr der Wissenschaftstheorie: Rudolf Carnap und Wolfgang Stegmüller
Internationales FWF-Forschungsprojekt, 2005-2007
Together with: University of Innsbruck, Forschungsinstitut Brenner-Archiv
Closing International Workshop
Date: Spring 2008
Venue: Institut Wiener Kreis
http://www.univie.ac.at/ivc/stegmueller

Wissenschaftliche Weltauffassung und Kunst: Kunsttheorie und Kunstforschung im wissenschaftlichen Diskurs / Scientific World Conception and Art: Art, Theory of Art and Studies in Art in the Scientific Discourse
Kurt Blaukopf and his project "Wissenschaftliche Weltauffassung und Kunst", 1992–2004.
Scientific direction: Martin Seiler
www.univie.ac.at/ivc/wwuk/

16th Vienna Circle Lecture / 16. Wiener Kreis Vorlesung
Gerald Holton (Harvard University, Cambridge, Mass.)
Einheit und Vielheit der Wissenschaften – Variationen eines Dauerthemas
Date: June 23, 2008
Venue: Universitätscampus, Aula

Book presentation featuring the authors
Gerhard Sonnert / Gerald Holton
Was geschah mit den Kindern? Erfolg und Trauma junger Flüchtlinge vor den Nationalsozialisten (Münster: LIT Verlag 2008. German edition of: *What happened to the Children who fled Nazi Persecution?*, Palgrave 2007)
Date: June 24, 2008
Venue: Universitätscampus, Aula

Wissenschaftsphilosophisches Kolloquium
Weekly lectures on the philosophy and theory of science given by scholars from Austria and abroad
http://www.univie.ac.at/ivc/koll/

Publications

Moritz Schlick Gesamtausgabe, Hrsg. von Friedrich Stadler und Hans-Jürgen Wendel. Wien–New York: Springer Verlag.
Band 1: *Allgemeine Erkenntnislehre.* Hrsg. von Fynn Ole Engler / Hans Jürgen Wendel. 2008
Band 6: *Die Wiener Zeit. Aufsätze, Beiträge, Rezensionen 1926–1936.* Hrsg. von Johannes Friedl und Heiner Rutte. 2008

Activities 2009

9th Vienna International Summer University
Scientific World Conceptions (VISU/SWC)
"The Culture of Science and Its Philosophy"
Main lecturers: Ronald Giere (University of Minnesota, USA), Mary Jo Nye (Oregon State University, USA), Alan Richardson (University of British Columbia, Canada)
Venue: University Campus
Date: July 13–24, 2009
Together with: University of Vienna / Department of Contemporary History
www.univie.ac.at/ivc/VISU
VISU-SWC is part of the doctoral program "The Sciences in Historical Context" of the University of Vienna, 2006–2009. www.univie.ac.at/HPS/

17th Vienna Circle Lecture / 17. Wiener Kreis Vorlesung
(as part of the Summer University)
Peter Galison (Harvard University)
The Assassin of Relativity. Friedrich Adler and Albert Einstein
Venue: University Campus
Date: July 16, 2009

Closing workshop
Moritz Schlick Project: Critical Edition of the Complete Works and Intellectual Biography
Phase 2: 2006–2009
In co-operation with *Universität Graz* and *Universität Rostock*
Venue: Institut Wiener Kreis
Date: Spring 2009
www.univie.ac.at/ivc/Schlick-Projekt

International workshops
The Philosophy of Science in a European Perspective
Continuation of the 5-year programme of the European Science Foundation (ESF) with 13 countries participating
www.esf.org/pse

Trilateral programme de formation-recherche du CIERA (Centre interdisciplinaire d'études et de recherches sur L'Allemagne)
La philosophie des sciences en Autriche et en France au XXème siècle: histoires croisées, héritages, réceptions et influences réciproques

Together with Université Paris 1 and IHPST/UMR and the Institute of Philosophy (University of Vienna)
3 journées et 1 colloque 2007–2009
Venue: Vienna and Paris
Date: May 29 and 30 2009, Vienna

Programmatik und Engagement. Transdisziplinäre Untersuchungen zum Verhältnis von Wissenschaft und Politik am Beispiel des Wiener Kreises
Venue: Vienna, Institute Vienna Circle
Date: November 20 and 21, 2009

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

Vienna Circle Institute Yearbook, vol. 14
The Vienna Circle in the Nordic Countries. Networks and Transformations of Logical Empiricism. Ed. by Juha Manninen and Friedrich Stadler. Dordrecht: Springer 2009

Vienna Circle Institute Library, vol. 3
Philosophy of Science before and after World War II: Banishment and Return. Ed. by Allan Janik, Friedrich Stadler et al. Wien–New York: Springer 2009

Veröffentlichungen des Instituts Wiener Kreis, Bd. 15
Logischer Empirismus, Werte und Moral. Hrsg. von Anne Siegetsleitner. Wien–New York: Springer 2009

Veröffentlichungen des Instituts Wiener Kreis, Bd. 16
Juha Manninen, *The Vienna Circle from Inside.* Wien–New York: Springer 2009

Moritz Schlick Gesamtausgabe. Hrsg. von Friedrich Stadler und Hans-Jürgen Wendel. Wien–New York: Springer Verlag
Bd. 5: *Rostock – Kiel – Wien. Aufsätze, Beiträge, Rezensionen 1919–1925.* Hrsg. von Edwin Glassner und Adelheid König-Porstner. 2009
Stationen. Dem Philosophen und Physiker Moritz Schlick zum 125. Geburtstag (= Schlick-Studien, Bd. 1) Hrsg. von Friedrich Stadler und Hans-Jürgen Wendel. Wien–New York: Springer 2009

Ernst Mach Studienausgabe in 9 Bänden. Berlin: xenomoi Verlag 2008ff.
Band 1: *Die Analyse der Empfindungen*
Band 2: *Die Mechanik in ihrer Entwicklung*
www.xenomoi.de/mach.html

Index of Names

Not included are: Footnotes, Tables, References and Figures