John Symons
Olga Pombo
Juan Manuel Torres
Editors

LOGIC, EPISTEMOLOGY, AND THE UNITY OF SCIENCE 18

Otto Neurath and the Unity of Science

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VOLUME 18

Logic, Epistemology, and the Unity of Science aims to reconsider the question of the unity of science in light of recent developments in logic. At present, no single logical, semantical or methodological framework dominates the philosophy of science. However, the editors of this series believe that formal techniques like, for example, independence friendly logic, dialogical logics, multimodal logics, game theoretic semantics and linear logics, have the potential to cast new light no basic issues in the discussion of the unity of science.

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Otto Neurath and the Unity of Science

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ISBN 978-94-007-0142-7 e-ISBN 978-94-007-0143-4
DOI 10.1007/978-94-007-0143-4
Springer Dordrecht Heidelberg London New York

Printed on acid-free paper

Springer is part of Springer Science+Business Media (www.springer.com)

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Chapter 1
Neurath and the Unity of Science: An Introduction

Olga Pombo, John Symons, and Juan Manuel Torres

> *Imagine sailors who, far out at sea, transform the shape of their clumsy vessel from a more circular to a more fishlike one. They make use of some drifting timber, besides the timber of the old structure, to modify the skeleton and the hull of their vessel. But they cannot put the ship in dock in order to start from scratch [. . .] That is our fate (1944, 47).*

Otto Neurath was one of the central figures in the Unity of Science movement in the twentieth century. We believe that reconsidering Neurath's views on the possibility and the desirability of what he sometimes called a unitary science is not only historically interesting, but that it is also directly relevant to contemporary philosophy of science. Thanks, in large part to Quine's influential promotion of some Neurathian themes, Neurath's fallibilist view of science has played a central role in the formation of modern naturalism. Furthermore, as one of the originators of the notion of physicalism, Neurath has a special place as one of the sources of recent philosophy of mind and metaphysics. Neurath's work has influenced the development of many of the central themes in contemporary philosophy of science. His views combine sensitivity to the diversity of the scientific enterprise, an anti-foundationalist approach to inquiry, and a refusal of the misology and obscurantism promoted by anti-modernist and romantic thinkers. The special focus of this volume is his view of the unity of science. Neurath is devoted to the scientific enterprise without being crudely scientistic, just as he adopts a form of physicalism without being crudely materialistic. Understanding whether this delicate, perhaps unstable, balance can be maintained is critical to our current philosophical predicament.

The present volume is divided into two parts. The first is devoted to primarily historical and philosophical studies of Neurath and the Vienna Circle. This includes a previously unpublished essay by Neurath in response to Max Horkheimer's criticisms of the Unity of Science movement. The second part is composed of contemporary philosophical reflection on the idea of the unity of science. By way of

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J. Symons et al. (eds.), *Otto Neurath and the Unity of Science*, Logic, Epistemology, and the Unity of Science 18, DOI 10.1007/978-94-007-0143-4_1,

introducing both Neurath's philosophy and the contents of this book, it is useful to provide some context for Neurath's response to Horkheimer. Specifically we discuss Neurath's view of scientific progress. From there, we discuss some of the specific contributions to this volume and describe their connection to the project of the unity of science

One way of thinking about Neurath's views on scientific progress and the possibility of a unitary science involves beginning with a relatively unphilosophical venue: In his *Modern Man in the Making* (1939) illustrates his view of social progress using his trademark *isotype*[1] graphs and charts. In this book, Neurath is addressing a non-specialist audience. His graphical presentation of statistical information concerning, for example, the decline of infant mortality, the increasing participation of women in political power, and the spread of literacy is simple, colourful and unambiguous. Isotype was intended to serve as a kind of graphical Esperanto; a means of communicating scientific information across language barriers and without the need for any specific scientific background. In the text accompanying his graphics, Neurath argues that modern progress is directly associated with the growing influence of what he called the scientific attitude. The message of *Modern Man in the Making* is strikingly simple. In spite of the threat of a world war, which others saw as evidence of the failure of the enlightenment, Neurath's book expresses his confidence that the scientific attitude is an ideal to which we should aspire. Understanding what this ideal is, how it is related to the unity of science project, and how it can be defended against obvious criticisms is one of the principal goals of the present volume.

In *The Dialectic of Enlightenment*, Horkheimer and Adorno claim that the kind of scientific enlightenment that is defended by the Vienna Circle led us to the barbarism of the Second World War, to the triumph of technological and bureaucratic reason, and to the disenchantment of nature. For Neurath, by contrast, the virtues of the scientific attitude were self-evident and were by no means implicated in the rise of twentieth century fascism. So, for example, *Modern Man in the Making* represents precisely the kind of view which Horkheimer and Adorno criticized. In that book, Neurath presents a portrait of what he saw as a trend towards modernization, internationalization and the elimination of superstition. To contemporary readers, the use of data and diagrams in this book can seem crudely didactic; obscuring the complexity of development in the twentieth century. While he frequently acknowledges the incompleteness of available evidence, Neurath regarded the trend towards modernization and social progress as obvious. Modernization, internationalization, and industrialization were developments which he regarded as broadly positive and which he supported. More importantly, developments in the twentieth century reflected the salutary influence of the scientific attitude as he understood it. For Neurath, it seemed clear that the growing influence of the scientific attitude leads to a steady increase in human happiness.

The present volume includes (for the first time in English) Neurath's response to Horkheimer's charge that the unity of science movement is based on a naive "harmonistic" fantasy and that it unreasonably privileges logic in philosophy. It is worth recalling the general position of the Frankfurt school with respect to modernity and the enlightenment. For Horkheimer and Adorno, what Neurath would have called the scientific attitude is a self-undermining delusion, as mythical as any of the superstitions that modern science has tried to overcome. There is a central skeptical assumption at work in the thought of the Frankfurt school, insofar as they deny the possibility of progress of the kind that modernists hoped for. In his "Unity of Science and Logical Empiricism: A Reply." Neurath defends the idea that it is worth seeking a unitary science which can enable conversations and interaction across existing domains, but he admits that this is an assumption (or perhaps better that it serves as an ideal) underlying the aspiration towards unity.

> The Unity of Science Movement rests on the assumption that one can find within unitary science as much as within physics, for instance, a form of expression that enables scientists to express their views in a common language, however contrary their views may be.

For the Frankfurt school, such ideals would be considered delusional.

Neurath does not provide an argument for the existence of such a common language. Instead, his work is motivated by the ideal and the goal of universal communication. While most myths and mysteries are barriers to communication and intellectual progress, if unitary science is a myth, it is the myth of the possibility of communication. The alternative to the myth of a unitary science in Neurath's sense, would be the kind of despair and anti-modernism we see in the philosophy of Horkheimer and Adorno. Following Neurath's essay in this volume, Karlheinz Bark provides some background to the origins of Neurath's manuscript and provides some important historical context concerning Neurath's frustration with what he saw as Horkheimer's misunderstanding of his views. Jan Sebestik's essay provides more historical discussion of Neurath's epistemological views. Sebestik situates Neurath's physicalism and holism in relation to the work of his Vienna Circle colleagues as well as within the broader historical context. He defends the view that the kind of common language that Neurath had in mind was ultimately pluralistic in nature. This essay is especially important in light of current debates concerning the nature of physicalism. Neurath's physicalism cannot be identified in any simple fashion with materialism or with other kinds of foundationalist metaphysics.

Throughout his work, Neurath often notes gaps in the available data, especially in the historical and sociological data, as well as the vast extent of our ignorance with respect to the natural world. Nevertheless, he emphasizes that one of the important features of scientific inquiry is the accumulation and transposition of available facts from a variety of distinct sources. He regarded all scientific projects as works in progress; partial, but progressive efforts to grapple with problems in a variety of domains. On Neurath's view, scientific progress is directly related to political and social progress, for Neurath an increasingly scientific attitude in political and social realms contributes to an increase in happiness. These features of Neurath's work are worth emphasizing in light of the tendency among some readers to see Neurath's anti-foundationalist epistemology as foreshadowing anti-scientific currents in later philosophical thought. It would be a misunderstanding to read Neurath as aligned with postmodern critics of scientific rationality since he clearly believed that the unavoidable starting point for all responsible inquiry is the facts as provided by our best available science.

According to Neurath, a scientific attitude involves the recognition of the tragic side of human life and specifically the recognition that our inquiry is unavoidably subject to constraints. Neurath's views on social and political engagement are similar to his views on theoretical questions. In both domains he emphasizes the inevitably provisional status of our work. However, unlike later critics of science who would despair of making any kind of scientific or social progress, Neurath highlights the fruitfulness of scientific inquiry.

Neurath's own encyclopedic conception of the unity of science is built on the notion of cooperative action in the scientific community and the accumulation of available results. As mentioned above, at the heart of the project is the goal of providing a universal medium for communicating across disciplines and languages. The project of the encyclopedia is meant, in a way, to show us how much we have already achieved; to present in the available resources from a variety of sources in a way which facilitates future investigation. His view of the unity of science, as Olga Pombo explains in her essay in this volume, is an extension of the traditional idea of the encyclopedia. Rather than imposing some pre-existing philosophical principle or methodological prescription on scientific investigation, the encyclopedia serves as a repository for what has been achieved by a diverse range of scientific projects.

Among philosophers, Neurath's anti-foundational approach to epistemic and metaphysical questions and specifically, his rejection of aprioristic reasoning is well known. However, it is worth remembering that the kinds of constraints that were most pressing for Neurath were social, political and economic rather than philosophical. So, for example, *Modern Man in the Making* was written during the economic depression of the 1930s and repeatedly mentions the looming threats of war, authoritarianism, and economic crisis. Neurath was not thinking of purely epistemic considerations when he argued that we sailors cannot put our ship in dock in order to start from scratch, but must "deal with heavy gales and thundering waves" (1944, 47). He reminds us that we face economic and political turmoil, war and the baggage of religious superstition: "[t]he life of modern man is not wholly modern. . . life is shot through and through with inherited ways of thinking and behaving that are centuries old." (1939, 131) In spite of the turmoil which surrounded him, Neurath's commitment to the project of modernisation and to the idea of progress was unwavering.

1.1 Reconsidering Neurath

This book is divided into two parts, the first more or less directly addresses Neurath's views or the views of other members of the Vienna Circle. Part Two contains contemporary reflections on the idea of the unity of science. Given that contemporary philosophy of science has a rather negative view of most prominent theses of the Vienna Circle why should twenty-first century philosophers turn their attention back to Neurath? Contemporary scientific inquiry presents a multiplicity of completely or partially divergent theories, methodologies, and domains of inquiry. Nancy Cartwright has famously characterized the scientific landscape as dappled. On her view, science is a set of sometimes overlapping patches of inquiry, which has no single coherent theoretical principle of organization. Physicists aspire to provide a foundational account of reality, but struggle to settle on a theory capable of unifying quantum mechanics and general relativity. The behavioural and social sciences also lack unifying models and the prospect for a single non-trivial account of the unity of science seems a distant dream.

In the face of the obvious diversity of scientific theories and practices, it is worth exploring Neurath's view of the Encyclopedia before discussing the essays collected in this volume. For Neurath, "[t]he task of the encyclopedia is to represent the present state of science and not to anticipate a unanimity which does not exist". There is no notion of reduction (in the contemporary sense) implicit in Neurath's view of unity. As Creath notes "Neurath never speaks of reducing one science to another but talks instead of symmetrical relations such as connecting, building bridges between, and filling gaps between various branches of science" (1996, 161). As such, Neurath's encyclopedia was close to the ideals of his French predecessors, Diderot and D'Alambert. Neurath's goal with regard to the encyclopedia was relatively simple: to build a useful tool for reciprocal cooperation and understanding among scientists. Therefore, the encyclopedia should be an organized formulation of all scientific research, but not – as mentioned above – a super-system of theories and laws. Neurath understood that in order to carry out his project, the creation of a language common to the scientific community was a necessary condition; i.e. a kind of lingua franca for scientists. As Ulisses Moulines notes in his contribution to this volume "Neurath's long-term aim was a universal jargon, a sort of scientific Esperanto, by means of which all kinds of scientific ideas and results could be expressed."

Four of Neurath's key words related to scientific activities help to clarify the goals of his encyclopedia and lingua franca project: "communication", "cooperation", "interdisciplinarity", and "interaction". For us it is crucial to observe that, when Neurath speaks of a language that makes it possible to optimize the activities meant by those terms, he does not think only in terms of nouns, e.g. "structure" should have the same sense for all scientists all over the world and, therefore, the same reference. He also notes that there are logical questions involved. The following quote is very revealing in this regard:

"It is not within the scope of this work to describe the achievements of particular disciplines. Instead, to the extent possible, it will present the many branches for science as a whole. *In particular, it must be seen to what extent logic-scientific analysis can be put in the service of the unification of science*" (italics are ours). Here where we can recognize the deep connection between Neurath's thought and tangible and present progress in philosophy, especially in post-Tarskian logic. In his essay in this volume, Ahti Pietarinen describes Neurath's view of language and logic, emphasizing the role of meta – linguistic reasoning. Pietarinen uses Hintikka and van Heijenoort's distinction between language as calculus and language as universal medium to elucidate Neurath's differences with Carnap over the nature of scientific language. Neurath falls squarely on the side of the language as calculus approach. Carnap, by contrast, according to Pietarinen, advocated a general position which is closer to the view of language is universal medium. Pietarinen describes how this divergence is reflected in Neurath's advocacy of ISOPTYPE

Thomas Bonck's paper adds an additional dimension to our understanding of the differences between Neurath's views and the views of the other members of the Vienna Circle. Specifically, he focuses on Moritz Schlick's defense of the view that ascriptions of reality to an object or event involve commitment to the idea that this object or event can be localized in space-time. On Schlick's view, existence is a univocal concept. By contrast, Neurath has no such restriction and may even be viewed as adopting a broadly antirealist approach to questions of existence.

In their essay, Gideon Freudenthal and Tatiana Karachentsev discussed the influence of Gregorius Itelson on Neurath's philosophical development. Freudenthal and Karachentsev describe how Itelson's idea of a "universal science" (*Universalwissenschaft*) may have influenced the development of Neurath's views. Itelson's universal science was envisioned as providing an account of the most general characteristics of all objects and events. As Freudenthal and Karachentsev explain, there is reason to believe that Itelson had conceived a philosophy similar to Logical Empirism a generation before the establishment of the Vienna Circle. They note for instance that Neurath points out that what he and others in the Vienna Circle called "logical empiricism" was earlier named by Itelson "empirical rationalism.“ Freudenthal and Karachentsev discuss the origins of the idea of a "Universal science" in Itelson's work and explain its emergence in the context of an argument against Neo-Kantianism.

1.2 Contemporary Reflections on the Unity of Science

In the second part of this book eight essays present contemporary reflections on Neurath's unity a science project. Each essay approaches the challenge of developing a coherent understanding of the notion of unity. For the most part, the essays are broadly sympathetic to Neurath's project. However, there is considerable disagreement with respect to the details of, for example, achieving anything like that goal of unifying the sciences.

Daniel Andler begins with an examination of the prospects for unification in science. He defends a moderate form of unity which he argues is compatible with the view espoused by Neurath. Andler uses the term "federalism" to denote this distinctive kind of unity characterized by moderation plurality and the construction of an epistemic common area. Also useful is his analysis of the range of distinct approaches to the unity of science found in the literature.

Mario Bunge argues that those who proclaim the disunity of science have understood the multiplicity of disciplines in a superficial manner. He defends the view that there is a strong movement of convergence and integration in and among the sciences. He further argues that the tendency towards integration has its roots in both ontological and methodological features of scientific practice. "[T]he world is buried but one" he argues "and the successful study he argues presupposes its reality as well as the plurality of human viewpoints and interests." Robert Causey makes a similar case but by very different means. In his essay, he makes the case that unification is an important component of scientific investigation insofar as unification is connected to comprehension. Juan Manuel Torres' essay adds an additional dimension to the discussion of unification and comprehension by exploring the distinction between unification and unity in the context of biological research.

Jan Wolenski's article makes the case for strong connection between naturalism and the unity of science project. He argues that naturalism requires a commitment to the unity of science. He then responds to the well-known challenge to naturalism which derives from the difficulties that naturalistic accounts have had normative notions. Next, we examine a relatively skeptical approach to Neurath's project. In their essay, Sheldon Steed, Gabriele Contessa and Nancy Cartwright argue that it is unlikely that we will find some set of laws which will unify the diverse kinds of inquiry and results which characterize modern science.

Ulises Moulines unpacks the various senses that the phrase "unification of science" can be given. He provides a detailed account of what he takes to be the most philosophically significant sense of the notion of unification, namely theoretical unification. He provides a formal explication of theoretical unification before examining the prospects for achieving unity in the sciences. Like many of the contributors to this volume, Moulines acknowledges that the ideal of unification is quite vague. Furthermore, even among members of the Vienna Circle, we find considerable diversity of opinion concerning the ideal of unified science. Moulines' contribution brings much-needed formal precision to the discussion of unity.

1.3 Neurath's Unity of Science Program

This book was organized in honour of Otto Neurath and it is about some themes and problems that constitute what we may call "his legacy". Why go back to Neurath? This is not a rhetorical question because present-day philosophy – especially philosophy of science – has a rather negative view about the most prominent theses of the Vienna Circle. As modern thinkers like Bacon and Descartes often began their essays with severe criticisms to scholastic essentialism, almost all contemporary introductions to the philosophy of science begin by condemning logical positivism and its theses. Reading surveys and introductory discussions we often encounter the following, rough characterization of the kind of philosophy of science which is associated with the Vienna Circle:

(i) Every scientific enterprise aims at the construction of true theories;
(ii) Scientific laws can be parsed as formulas of classical first-order logic;

(iii) Scientific theories will be finally gathered – *via reductionism* – in a super theory;
(iv) In principle, we will always be able to derive all knowledge from protocol sentences.

It would be very hard to find an advocate for these four theses today. But it should also be clear from many of the contributions to this volume that Neurath's views certainly do not match cleanly with these four stereotypical theses. The contributions to this volume demonstrate that Neurath's philosophy of science cannot be dismissed by conflating it with the simplistic caricature of the thought of the Vienna Circle.

When we consider, more specifically, Nuerath's views of the unity of science, one might wonder whether there are additional, far stronger reasons to think that he was mistaken. How does the landscape of scientific theorizing appear today? At first sight, we find a multiplicity of completely or partially divergent theories and methodologies. The theoretical landscape is dappled indeed! Physicists struggle to settle on a theory capable of unifying quantum theory and general relativity. We find highly speculative hypotheses – many of them quasi-philosophical – proliferating in the foundations of physics. Biological inquiry exhibits a vast range of diversity and divergent approaches, while at the same time contributing enormously to our understanding. Less obviously successful, the social sciences lack unifying models and the relationships among all of them could be described by the Latin adage *bellum omnium contra omnes*!

So, why should we go back to Neurath's vision of the *Encyclopedia* and the idea of unity of science? Most historians and philosophers of science have observed the scientific landscape described above and – presumably under the influence perhaps of 1960s-style sociological and historical analyses – have assumed that there is an irreducible diversity of sciences. By the 1990s the rejection of Neurath's project and the tradition behind it seemed complete. In this sense, Peter Galison and David Stump's 1996 *The Disunity of Science* can be mentioned as an expression of the consensus among philosophers and historians of science at the time.

There is no doubt that many cases of struggle and opposition among theories, methodologies, and styles exist in scientific practice. But it would be an exaggeration to think that everything aims in that direction in science. Clearly scientific inquiry is a dynamic process, whereby many new phenomena – or new characteristics of the phenomena already known – are identified and described on a daily basis in major fields of research. Nevertheless, the final assimilation of these materials and their subsequent organization – so to speak – in the scientific corpus takes place slowly. Thus, using the language of biology, it is unsurprising that disunity is dominant and unity is recessive in the population of scientific results. In other words, the accretion of data proliferates at a faster rate than their subsequent incorporation into new or pre-existing theories.

In addition, an exaggerated view of disunity overlooks the many disciplines and theories that have included knowledge from other fields, and, thanks to this incorporation, they have reached an enormous success in basic and applied sciences.

Two examples which are close to the hearts of the present authors are game theory and information theory. By means of the first, social sciences and the ethology of social animals can predict actions and explain behavior that would otherwise be paradoxical without such theory. By means of the second, biologists can measure the exact complexity contained in biomolecules – proteins and nucleic acids – a fact that allows knowing whether the evolutionary mechanisms proposed are enough to explain the order found in living nature. It is true that disunity looms large in the landscape; however, there are also many areas of successful integration. Remember, for example, the crucial role played in physics by Pierre Fermat's principle of minimum time and Maupertuis' principle of least action. Unity or disunity in science depends fundamentally on where you focus your attention.

Finally, and before making some brief comments on Neurath's idea of Encyclopedia, we should emphasize that, although disunity seems to be a reality because science is essentially an active process, it cannot be understood to be the permanent condition of any particular region of scientific inquiry. As recognized by Kant, human reason has the unavoidable tendency to unify knowledge. At present we witness the efforts of scientists as well as philosophers in pursuit of unification insofar as they aim to generate theories capable of previously independent domains, such as the classical and quantum realms. Perhaps it might sound rather awkward to hear that there are "philosophers working for the unity of science", because it seems that the construction of theories and hypotheses is a job for scientists. However, many historical cases, such as Leibnitz' or von Neumann's – who noticed that matrix and wave mechanics were equivalent formulations for quantum theory – contradict the presumption of the irrelevance of purely conceptual work for unity.

In mathematics, the development of category theory provides direct evidence of the unifying directions which are possible in the purely conceptual domain. Category theory is a branch of abstract algebra devoted to investigating transformations in a highly abstract form. In his excellent recent textbook, Steve Awodey characterizes category theory as the "mathematical study of (abstract) algebras of functions. Just as group theory is the abstraction of the idea of a system of permutations of a set of symmetries of a geometric object, category theory arises from the idea of a system of functions among some objects." (2006, 1) While there is obviously a long history of reflection on the idea of transformation in geometry and algebra, the development of abstract algebra in the 1930s permitted the study of transformations and compositions of transformations in the most general form possible.

Category theory can be understood as an abstract algebra of relations, mappings, or functions. Having something like this is interesting to philosophers and mathematicians for a number of reasons. It provides a toolbox of techniques whereby relationships between distinct domains of mathematical investigation can be illuminated. The first presentations of category theory arose out of algebraic topology and specifically with Samuel Eilenberg's observation that Sanders MacLane's calculations on a specific case of a group extension coincided precisely with Norman Steenrod's calculation of the homology of a solenoid. Eilenberg and MacLane's effort to make sense of this coincidence across apparently distinct areas of mathematical inquiry gave rise to their development of category theory. It should be of obvious interest to philosophers that there are direct connections between fields like set theory and proof or logic and geometry which have been described in categorial terms. One striking example, as mentioned by Awodey in his textbook, involves the categorial notion of an adjoint factor which occurs in logic as the existential quantifier and in topology as the image operation along a continuous function (2006, 2). Category theory offers all kinds of deep and surprising insight into the shared features of a wide variety of phenomena.

There are an increasing number of introductory texts which can provide philosophers some technical acquaintance with category theory (See for example Awodey 2006; Lawvere and Schanuel 1997). Furthermore, Awodey and Reck provide an excellent historical account of the place of category theory in the development of formal philosophy in the twentieth century in their 2002 articles. Given the increasing accessibility of category theory to philosophers it is likely that there will be more interesting engagement with the field in the years ahead.

Given its generality and applicability, category theory is widely thought to serve as a viable alternative to set theory as a foundation for mathematics. Not surprisingly, most of the work on category theory that has been done by philosophers has focused relatively narrowly on questions pertaining to the role of category theory in the foundations of mathematics and its relationship to set theory. This focus comes largely in response to the claims of category theorists to have identified the elementary topoi that are equivalent to the category Set, thereby effectively axiomatizing set theory in categorial terms (see Mitchell 1972; Cole 1973).

The development of category theory, the proliferation of non-classical logics, the increasing ease of electronic access to information all cast Neurath's project in a new light. However, his own views must be considered apart from these more recent developments. So, by way of conclusion, what did Neurath have in mind with regard to the encyclopedia? The answer is relatively simple: to build a useful tool for reciprocal cooperation and understanding among scientists. Therefore, the encyclopedia should be an organized formulation of all scientific research, but not – as expressed above – a super-system of theories and laws.

Note

1. The figurative language called ISOTYPE (International System Of Typographic Pictorial Education), was developed by Neurath and his collaborators at the *Mundaneum Institute* of the Hague.

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Part I
On Neurath

Chapter 2
Unity of Science and Logical Empiricism: A Reply

Otto Neurath
Translated by Thomas Bonk

Every scientific movement can be considered from many different points of view. For example one can characterize the works of various researchers with respect to scientific achievement without taking their chronological order into account, which does not even need to be known for this purpose. One can show to what extent Oresme's writings on money as a means of exchange touches on Knapp's theory, and how Grimaldi's ideas on the inner polymorphy of light originated in his refraction and interference experiments, while Huygens moved the mathematical treatment of light into the foreground and neglected the colorfulness and blurring of certain phenomena of light, which others eventually treated mathematically. Alternatively one may connect theories chronologically and conceptually and thus trace the development toward a comprehensive set of doctrines. Many ideas are forgotten, others connect with elements from other sources. Ideas of ancient thinkers combine with scientific works of the scholastics, with speculations of astrologists, with theories by scholars of the Renaissance, to form a mosaic whose composition is continually shifting and perpetually revealing new features. Additionally one can ask how individual thinkers arrived at their views, which individual experiences and fortunes were decisive, and thus make progress toward a "behavioural study of scholars". Yet another question concerns the problem of how the overall states of affair of an epoch are linked to the appearance of certain points of view, in particular the problem to specify the connection between the situation of society and scientific

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O. Neurath (✉)

See Max Horkheimer. Der neueste Angriff auf die Metaphysik. *Zeitschrift für Sozialforschung* VI, Heft I. p. 4 ff.

Editorial Note. All underlines, including those in quotations taken from Horkheimer's essay, are by Neurath. Square brackets enclose materials inserted into the text by the editor/translator. The translation of some of Neurath's "technical terms" is followed on their first occurrence by the original German expression in square brackets. Page numbers in square brackets refer to the English translation of the quotation.

The quotes from Horkheimer are taken from M. Horkheimer, *Critical Theory. Selected Essays* (Transl. M. J. O'Connell) New York, 2002, with kind permission by the publishers.

J. Symons et al. (eds.), *Otto Neurath and the Unity of Science*, Logic, Epistemology, and the Unity of Science 18, DOI 10.1007/978-94-007-0143-4_2,

praxis [Wissenschaftsbetrieb]. Thus we arrive at the "sociology of science" and the "history of science".

Historical transformations not only change what we call theoretical expressions or constructions but the stock of protocol sentences too. That which has gone completely unnoticed at one time becomes conspicuous in another, what has been noticed but judged as unimportant, may become the center of important considerations in another time. One has to take into account here that "constructions" and "raw materials" are difficult to separate. Some of our observation sentences and perceptions [Anschauungen] turn out to be very stable, but – principally speaking – nothing is certain, everything flows. Very likely a rigorous thinker applies all those considerations, based on experience, to his own life and asks himself, how he would behave, how he would argue, if he were in another position. He recognizes that decisive transformations of scientific praxis are not only determined by the intense thought of a generation of scholars, but in addition by what happens in the life of society, of which scholars form a part. If he considers the totality of this behaviour, in which his scientific work is interwoven with everything else, it may well happen that a social process (continuation or reorganization), which he finds desirable for one reason or another, is detrimental to the scientific praxis he is fond of. How much more pleased he will be when the social process, he had wished for other reasons, actually seems to advance scientific practice. Suppose he detects a "conflict", then it is a matter of personal decision where he wants to take a stand.

Someone may at first believe based on certain experiences that only calm contemplation guarantees sound scientific judgment, particularly in the field of social sciences, where today, to be sure, emotional elements are at home, only to discover subsequently on the basis of historical cases that love and hate can be excellent masters as well. Whether the one or the other is the case cannot be established a priori in general, and especially not in a case that is currently under discussion. We see the manifold combinations, which may exist: Kepler, a groundbreaking astronomer, started out from mystical considerations on the harmony of the spheres and regular Platonic polyhedra, apart from casting horoscopes in the service of the monarch, whereas Galileo had to struggle in the defense of his theory. Astronomical, geological, biological, social theories arise in complex ways. That which is elaborated today by people with conservative outlook, may become tomorrow the lever of transformation, and vice versa, what appears today as enormously transforming may hinder tomorrow certain developments of science.

The hope to locate by foresight the position of one's activities and scientific attitude [Wissenschaftlichkeit] in historical dimensions is obviously the more limited, the more one feels that great transformations are immediate, since how can the as yet unaltered way of thinking foresee what the future altered way of thinking is like and predict how one is going to evaluate the contemporary situation retrospectively.

A great many people are not sufficiently concerned with comprehensive problems of experience to arrive at considerations like these. Many of those who do, and who choose not to ignore their conclusions, feel greatly hampered in their activities by all these restrictions. This may well be the cause why others by any means, so to speak, strive to get hold of any view, which promises the resemblance of foresight, since only then they are able to make a decision. Yet, there are scientifically minded people who can act energetically and persistently without deluding themselves with an insufficiently grounded unique insight into their historical situation. It is upon behaviourists, sociologists and historians to investigate whether this combination of critical scientific attitude and uninhibited decisiveness can and will spread itself more universally.

He who has decided himself to adopt a scientific attitude is therefore active in a social sense as well, without necessarily knowing what consequences such a form of behaviour has. It is obvious that many people, those in particular who are far removed from having any aprioristic point of view, who do not concern themselves with metaphysical constructions, and who view life from an empirical point of view, are struck by the problems and considerations that have just been sketched. One sees on first sight that all these inquiries are very much conducted in an empirical vein.

Similar considerations may have stimulated the objections to logical empiricism and unitary science [Einheitswissenschaft] in an article by Max Horkheimer, but Horkheimer searches for a vaguely supra-empirical formulation of the question at hand and for a unique answer. His principal thesis, which is the main subject of the following, says roughly: "there is an extra-scientific method capable of criticizing Underlinethe sciences mainly by way of exhibiting their historical position in a way alien to the sciences themselves, although based on everything scientifically determinable." This point of view, Horkheimer calls it "dialectical" or "critical", allegedly goes beyond both the metaphysical and the scientific one; it is, he claims, the follower of scientivism's [Szientivisten] core mistake to be "opposed to thought, whether it tend forward with reason, or backward with metaphysics." (p. 51 [p. 186]) He himself thinks he is defending reason against empiricism, where he follows traditional German philosophy in distinguishing between "understanding" [Verstand] – which he does not deny to empiricism – and "reason" [Vernunft].

This is not the place to show how Horkheimer's arguments in the final analysis trace back to German idealism, since that would exceed the scope of the present reply, whose main objective is to indicate how one may respond to Horkheimer's general considerations from the point of view of logical empiricism. In this manner misunderstandings and distortions are to be corrected, which make it difficult for the general reader, who is not well read in the literature of the movement under attack, to get an accurate picture of the movement. Horkheimer's style of writing and his overly rarefied similes reveal perhaps his emotional involvement with those problems, but they do not help with the task of clarification.

That the Unity of Science Movement[1] to which more and more scientists worldwide become conscious of belonging to, aims to pose a comprehensive frame for inquiry and is open-minded with respect to all kinds of questions, no reader will suspect who finds Horkheimer writing that "[members of this school] claim that fruitful discussion can begin only when the limited problems of logistics, the logical syntax of speech, or the calculation of probabilities are the subjects." (p. 49 [p. 184]). Over many years a cooperation of scientists has taken shape, which finds a highly visible expression in yearly held "International Congresses for the Unity of Science". The "International Encyclopedia of Unified Science",[2] in preparation (edited by Neurath, Carnap and Morris; to be published by the University of Chicago Press), has set itself the task to show how one can examine quite diverse questions scientifically and connect them with each other, and how diverging scientific methods (presented by their proponents in their own words) can be applied to the same problem. So as not to let the idea of a "science an sich" arise, particular attention will be paid to the methods used in the "sociology of science", in order to reveal the place of the sciences within the body of society. A glance at the table of contents of the two introductory volumes "Foundations of the Unity of Science"[3] shows the range of questions: (1) The Unity of Science, (2) Theory of Signs, (3) Mathematics and Logic, (4) Procedure of Empirical Science, (5) Probability, (6) Physics, (7) Cosmology, (8) Biology, (9) Formal Biology, (10) Behavioristics, (11) Social Sciences, (12) Empirical Axiology, (13) Sociology of Science, (14) General Linguistics, (15) History of Science, (16) History of Logic, (17) From Rationalism a priori to Empiricism, (18) Problems of Empiricism and Empirical Rationalism, (19) Logical Empiricism, (20) Bibliography. Unitary science on the one hand wants to evade metaphysical speculation; on the other it wants to create a basis for scientific discourse. In this way one wants to render possible an encyclopedic synthesis without overlooking that on the basis of logical empiricism in principle encyclopedias of very different kinds can arise, which may contradict each other. The equivalence of scientific methods and logical constructions may lead to the design of a logical framework – with gaps, imprecisions and other kinds of vagueness – that comprises all sciences.

The very idea of a unitary science does not presuppose that the researchers, who work towards its completion, agree on its construction or even on particular statements in the areas of geology, biology or sociology. But even if all researchers on earth were to agree by and large on the logical scaffolding, on theories, hypotheses, single statements – a very strange assumption – then such a unity would not be necessarily associated with socio-political unity. Horkheimer however assumes that a "naïve harmonistic belief [...] underlies his [the empiricist's] ideal conception of the unity of science and, in the last analysis, the entire system of modern empiricism" (p. 17 [p. 147]). One can easily imagine, that in an isolated region, in which only 200,000 individuals can earn their living, two groups, one of 150,000 and the other of 170,000 individuals fight with all their might, although both have the same theories at their disposal and know equally well the impossibility of 200,000 people to share that space.

If someone has reasons drawn from history to suppose that we move towards a peaceful organization of mankind, he will view the unification of the techniques of production, of the arts of war, of the means of communication, of the methods of organized crime [Gangstertechnik], of the methods of the police, of the methods of science [Wissenschaftstechnik] and many other techniques as a kind of preparation of that future unity, although some of these techniques are used as means in war and strive nowadays. A conspicuous empiricist however would judge it premature to infer from the unification of those techniques the future peaceful organization of mankind. But he would equally judge as premature Horkheimer's professed opinion that where presently is war between groups of humans there can be no mention of unification of science in any way, not even in the carefully expressed way of the Unity of Science Movement; he thinks that the struggle between groups generates opposition even within the sphere of perception and protocol sentences. This possibility cannot be excluded. Furthermore frequently a theoretical difference will be invented once the fighting has begun. In particular those parts of the theories that are not as directly testable as the theory of poisonous gases and machine guns are likely to be emphasized and used as "banners" to group the fighters; yet, oppositions like these hardly prevent the combatants of two fighting groups to agree on their immediate experiences. It would be very interesting to see some examples and Horkheimer will surely communicate what material he relies on (p. 17 [p. 147]), 19, 27) when he concludes that it is very misleading for empiricism to maintain that within the scope of objective experience, one "can come to an understanding with everybody on every subject".

By representing this agreement within the scope of everyday experience as a mere historical fact, Carnap, whom Horkheimer quotes, as well as other proponents of logical empiricism, imply that deviation is possible. As a matter of fact the greatest measure of constancy show the protocol sentences (Neurath, Pseudorationalismus der Falsifikation. Erkenntnis 1935, p. 353f.), while theories are essentially more unstable. The constancy of those forecasts, on which one actually relies, is lower than that of protocol sentences and higher than that of theoretical formulations, which help to support those forecasts.

The comprehensive scientific attitude that plays such a prominent role in the Unity of Science Movement rests on the readiness to subject in principle everything to scientific criticism, always aware of the possibility that one is deceived or observes with a certain bias. How many scientists and laymen alike are critically disposed in one part of their life, for instance in their profession, while in others they are conspicuously uncritical, be it because they accept and use metaphysical speculations as reports of fact, or because they accept theories and observation reports that are everything but certain without much ado, thus behaving quite differently here as compared to the "critical area" of their life. From the viewpoint of the scientific attitude one seeks to build up a comprehensive empiricist conception, yet scientific criticism is more than fighting non-empirical expressions and contradictions, as Horkheimer suggests (p. 28 and [rest is missing]) – these are just "minimal requirements".

As important as the development of modern logic [Logistik] has been to the rise of logical empiricism, Horkheimer exaggerates when he thinks the Unity of Science Movement requires every philosopher to study logic (p. 44). In the passage Horkheimer that quotes Carnap, who is after all himself a logician, merely suggests that the cause of the philosophers' resistance against logic may be that they feel threatened by the critical potential of modern logic. Besides it is by no means excluded that 1 day they will use modern logic in the construction of metaphysics (see Neurath, Le dèvelopment du Cercle de Vienne et l'avenir de l'empiricism logique. Herman and Cie. Paris 1935). Ernst Mach, whose paradigmatic meta-scientific analyses prepared the way for Einstein's work, did not use logical formulae. Horkheimer, on the other hand, seems to require that one should study "dialectics"; it would be helpful to know which textbook teaches the "dialectic" he advocates (the term is very ambiguous), as one can learn modern logic from Carnap's well-known "Grundriss [der Logistik]".

The comprehensive attitude of empiricism developed out of interactions between innumerable individuals in all walks of life; the scientists among them play a predominant role. This broad stratum of active or contemplative thinkers, this by and large anonymous mass has, as Schlick has strongly emphasized, done most to advance scientific enlightenment, more than "the sensational philosophical systems, which follow and contradict each other in an endless series". The idea that it is just a matter of finding the "key" (p. 28 [p. 159]) that opens the gate, is alien to the Unity of Science Movement. It believes that the development of the unitary science may follow along lines similar to the development of the special sciences.

Why do we speak of "logical empiricism"? Since the development of modern logic supports all kinds of logical analyses now is the first time in the history of scientific empiricism that certain tendencies can be merged: namely those tendencies that rationalism had cultivated (for instance, the idea of a thorough, universal application of logic and total comprehension) and those that empiricism, more devoted as it is to the special branches of science, has developed to perfection. Galilei recognized mathematics as an important ancillary discipline to physics (although it was revered mainly by idealistic philosophers, like the Platonists, and not by the more empirically minded Epicureans, who were nevertheless somewhat inimical to science), yet he rejected logic along with scholastic doctrine as a barren sub-discipline of the latter. Kant, who sought to give philosophical support to the century old scientific view [of logic], contributed to the petrifaction of the rejection of logic. Horkheimer does not seem to regret that Kant thus hampered the development of modern logic (p. 42). For this reason, the great idea of Leibniz to aid our research and construction by means of a universal calculus could only be taken up at the end of the nineteenth and the beginning of the twentieth century in modified form. Bertrand Russell, who does not get away well with Horkheimer (p. 19, 35, 41), once was the focus of research in modern logic on the one hand, and on the other he had a great interest in the development of empiricism. Therefore he may be regarded as an example of the development of logical empiricism. The works of the Vienna Circle with Schlick at the center, of the Berlin group with Reichenbach, of the Polish group, of the French, who tied up with Duhem, Poincaré, Abel Rey and others, the British and American, who had in Peirce, James, Dewey and others pioneers of the new movement, led to a unique scientific cooperation, the main lines of which we have sketched by way of introduction.

The representatives of the movement emphazise the point that for them there is no philosophy as a higher court that issues ultimate judgments. Work on the unitary science takes up the position of an all-embracing philosophy so to speak, in so far as work in the logic of science (Carnap) is an extension of analyses that formerly were the task of philosophers. Whatever is asserted in science can be criticized from a more comprehensive scientific point of view without regard to disciplinary boundaries, but we do not recognize a tribunal beyond science, with sits in judgment of science and investigates its foundations. The most recent developments of physics in particular have demonstrated that scientists themselves are competent to investigate the efficacy of foundational concepts like "space" and "time", while the philosophers of the nineteenth century not only did not prepare the way for the great transformation in the conception of space and time measurement that Mach and Einstein initiated, they later either carped at it or very slowly sought to build it in.

Horkheimer makes it sound as if in the Unity of Science Movement the special branches of sciences have special authority, while the basic idea is that the comprehensive system of science has singular significance and the traditional subdivision of science into branches is of less importance. When Horkheimer maintains "No criticism can be brought against a branch of technical science from outside; no thought fitted out with the knowledge of a period and setting its course by definite historical aims could have anything to say to the specialist." (p. 16 [p. 145] also 45), a proponent of logical empiricism could reply that if someone who is standing outside a particular branch of knowledge has something scientifically testable to contribute, he is only welcome; but possibly Horkheimer has his meta-scientific method in mind: "In the dialectical theory, the fact that subjective interests in the unfolding of society as a whole changes continuously in history is not regarded as a sign of error, but as an inherent factor of knowledge. All basic conceptions of the dialectical theory of society, class, economy, value, knowledge, and culture are part and parcel of a theoretical context dominated throughout by subjective interests." (p. 31 [p. 163]). If one were to understand this passage as a scientific one, one would have to ask how one can distinguish an "unfolding of society as a whole" from a "non-unfolding", what is to be understood by a "self-modifying moment" and so on. In these and other passages Horkheimer seems to suggest that these terms cannot be determined by definition or reduction in the usual manner. It is hard to see however why terms like "economy" or "society" should not be taken simply as scientific terms, which everybody can employ. It is certainly not clear how one should express the thought that the terms belong to a theoretical whole "governed by subjective interest". Empiricists have often emphasized in plain words that any theory can be traced back to the subjective interests of groups of humans. John Stuart Mill had always praised Bentham for directing attention to connections like that and pointed them out himself: "MILL QUOTE1" Mill, like all empiricists, does not need a special terminology to build a meta-discipline on the basis of which everything can be criticized, instead he uses the expressions of everyday psychology to characterize a certain kind of behavior that gives rise to moral doctrines. He could equally well attempt to trace back any scientific doctrine to its sources in society.

Horkheimer's article returns again and again to this problem. The empiricist's belief that science is not something "external to life", that doing science is, like art or philosophy, something closely connected with all activities of life, is not enough for him as long as judgments like these are not expressed in the non-scientific manner he advocates. He holds that empiricism hides something, which could be discovered by other means, that it somehow does not take action sufficiently into consideration and does not get clear about how closely action is connected with thought. For him the passage by Mill, an empiricist, quoted above is no more disproof than the following assertion Mill's: "MILL QUOTE2". Obviously, Horkheimer assumes a position "outside" science (which only makes use of the faculty of "understanding") to analyze the whole of the scientific enterprise from the point of view of "reason" and to show thus in the "correct" and explicitly non-scientific manner what is behind it all. Surely he has to shrink back from subjecting those "correct" theses to scientific test, because then he would enter the domain of science, which he wants to criticize after all. So it is only consistent when he says in a different paper ("Traditionelle und kritische Theorie". Zeitschrift für Sozialforschung 1937 Heft 2): "The critical theory of society is as a whole one unfolded existential judgment." and "General criteria for the critical theory do not exist [. . .]". The proponent of logical empiricism would suppose that these are "isolated sentences", which have no use in science for him, as he is not clear about what court would decide which are "right". "When an active individual of sound common sense perceives the sordid state of the world, desire to change it becomes the guiding principle by which he organizes given facts and shapes them into theory. The methods and categories as well as the transformations of the theory can be understood only in connection with his taking of sides. This, in turn, discloses both his sound common sense and the character of the world. Right thinking depends as much on right willing as right willing on right thinking." (p. 4 [p. 162]) "The facts of science and science itself are but segments of the life process of society, and in order to understand the significance of facts or of science generally one must possess the key to the historical, the right social theory." (p. 26 [p. 159] and p. 48 as well). But Horkheimer nowhere indicates by means of which kind of test one determines if a view is "right" or is "not right".

Horkheimer's extra-scientific method cannot, so it seems, dispense with certain terms, which trace back to German metaphysics and for which he professes sympathies although not without some reservations. He mentions in particular Hegel, Kant, the neo-Kantians and Husserl (p. 26, 27, 17, 41 etc). Horkheimer disparages empiricism for claiming: "Neither the inexpressible nor the unexpressed may play a role in thinking; they may not even be inferred." (p. 13 [p. 143]) Even putting aside any question regarding the rules of usage for the terms employed, one must notice that something is declared "inexpressible" which according to Horkheimer's view can be "inferred". That what can be inferred can surely be said! When Horkheimer criticizes empiricism for rejecting "to speak of a subject or of a reality that could not be given, but lay before or behind individual facts and their interrelations" (p. 24 [p. 154]), his charge can only be properly discussed when formulated without the metaphors of traditional metaphysics.

When Horkheimer publishes scientific papers in his specialty, he employs a language the sentences of which are empirically testable. The extensive introduction to the research report "Authority and Family" employs a language which proponents of logical empiricism may well find intelligible (this is not true of other papers in this volume of about 900 pages). In the Unity of Science Movement one aims to talk as scientifically about science as one talks about plants, animal or humans in the special sciences themselves. It is true that in earlier times the inclination towards comprehensive syntheses was at home in the great works of the philosophers. Hegel's "Encyclopädie [der philosophischen Wissenschaften im Grundrisse]" is a good example. He starts with entirely metaphysical reflections, moves on to empirical expressions mixed up with metaphysical ones and concludes with metaphysical reflections. A scientist who is inclined to empiricism may find here some stimulation. For instance, Hegel has developed wide-ranging historical lines of thought, which had influence on the empirical explanations of Marx and Engels. The bulk of his arguments however are not scientifically testable.

Today an encyclopedic summarization can be tackled on account of preparatory, logical meta-scientific analyses, and probably one will get a step ahead of Comte's positive philosophy and Spencer's synthetic philosophy, the torso of which lacks an account of physics; this needs emphasis, since Horkheimer, like some other critics of scientific empiricism, stresses its narrow limitation to physics. Horkheimer's attack on "physicalism" [Physikalismus] mainly moves into this direction. One overlooks thereby that John Stuart Mill's "[A] System of Logic, Ratiocinative and Inductive" – which is in many details objectionable from the point of view of logical empiricism – deals with all sciences including the social sciences, and that it was not written by a physicist but by a political economist concerned with the method of the sciences. It is characteristic of him that he hails from an utilitarianism aligned to empiricism and that he gathered genuine experiences in business administration and in the workings of Parliament. Jevons, the author of "The Principles of Science", a book, which helped prepare the way for logical empiricism as well, was an economist too, who in addition contributed to the development of logic. Karl Pearson, the author of "The Grammar of Science", was, like his teacher Galton, mainly interested in socio-biology, besides occupying himself with physics. For these forerunners of logical empiricism all special branches of science together make "the science", the completion of which they aim at. This historical development explains how latter day logical empiricism connects with the scientific activity of past centuries, with the concern of Comte, Spencer, with science taken as a whole, and with the preparatory meta-scientific analyses of Mill, Jevons, Pearson and many others.

Insofar as in metaphysically tinted books there is something empirical useful to be discovered, a proponent of the unitary science will certainly aim to recover by way of a suitable interpretation the empirical content for the purpose of the further development of science. He may well agree with Horkheimer in that "a great many of the writings of the metaphysicians contain a more profound insight into reality that can be found in the works of the special sciences, no matter how well the latter are adapted to the needs of the present." (p. 50 [p. 185]) But Horkheimer opposes the translation of anything an empiricist regards as barely intelligible and metaphysical, and he gives importance precisely to those terms that frequently appear in pseudo-problems. For instance, Horkheimer wishes that one takes into account "the distinction between essence and appearance, identity in change, and rationality of ends" (p. 16 [p. 145] and 37). Well, one can define "essence" and "appearance" in an empirical manner, although that will probably not satisfy him, no more than the great attention that has been devoted to the concept of "gen-identity" (Kurt Lewin) in the framework of logical empiricism, or for that matter that one can introduce the term "rationality of ends" relative to a set of principles, for example those that govern a group of individuals. Horkheimer's above mentioned tendency to introduce a kind of "esoteric" use of words shows itself in the following statement, which so to speak blocks every way to establish a common platform of discourse by means of definitions that are acceptable to both discussants: "indeed, the concept of man, of personhood, even of society and class taken in the sense, that presupposes specific viewpoints and directions of interests [does not exist for empiricism]. In exceptional cases, when the empiricist does employ such concepts, he restricts them to a purely classificatory function as if they were zoological genera. For this very reason, the structure of knowledge and consequently of reality – as far as the latter can be known – is as rigid for him as it is for any dogmatist." (p. 16 [p. 145–146], see also p. 31) It is difficult to say how concepts that apparently refer to groups of individuals, for example human beings etc, should be used in any other than "classificatory" way. And it is not plain for an empiricist why "zoological kind" marks a sort of concept that is unsuitable to be used in the sphere of problems that concern human beings. What does it mean to say that some concepts are to be employed in a sense, which "presupposes specific viewpoints and directions of interests"? It is easy to explain from an empiricist's point of view why some people, characterized by a certain common attitude, introduce a group of new concepts and expressions for the first time historically speaking, but then one can define those concepts in ways such as to take their points of view and directions of interests into account. Horkheimer apparently wants to indicate that one cannot employ those concepts without sharing those points of view. We lack, however, examples from the history of mankind to support this assumption. Anybody can, acting as "advocatus diaboli", imitate any kind of discourse. The concepts "human being", "class", "society" and many others appear to be on first sight very much definable or reducible (Carnap) in empirically acceptable ways.

We know from previous studies that some terms mislead one into using metaphysical expressions, and for that reason it has been proposed to dispense with them as far as possible, to create an Index Verborum Prohibitorum so to speak. The usefulness of this procedure has already become plain and should be evaluated on the basis of case studies. In any case the procedure is not as Horkheimer describes it, namely that all those terms are assigned to the Index which "some noted specialist has pronounced useless" (p. 49 [p. 184]). Sometimes one may introduce new terms because the traditional ones are freight with too many associations. Horkheimer objects: "They hold that concepts like man or capitalism – provided they are not on the Index Verborum Prohibitorum – could just as well be rendered "larifari" or "ruarua"; in fact, it would be preferable to choose such "neutral" expressions because once correctly defined, neutral expressions would prevent misunderstandings." (p. 45 [p. 179–180]) An empiricist finds it quite difficult to understand why one should not use arbitrary sequences of letters as names, provided they are properly defined. In fact new words, like "gas", have been introduced into the language. It is self-evident that "neutral" terms are particularly well suited for scientific discussions. Of course, occasions may arise in which scientific discussion is not desirable, and then one will prefer words that act as banners and arouse enthusiasm or annoyance. The unification of the scientific language within unitary science has been proposed mainly for the benefit of scientific practice, and this is the main concern of the Unity of Science Movement as well. It is a sorrow state of affairs, for instance, if one cannot find out without much trouble whether two psychologists speaking different scientific languages are of the same opinion or not with regard to the same question, or address different matters and talk past each other. The situation in the field of sociology is frequently like that. Certainly, the methods to carry out the unification of the language of science in the spirit of empiricism would lead to the removal of a large chunk of metaphysical terms, which are dear to Horkheimer. From this perspective Horkheimer's attack becomes slightly more understandable.

On the whole the linguistic proposals of the unitary science are relatively simple in character. The only requirement is that in the end all scientific theories are testable on the basis of sentences which contain only space-time related expressions, like "The table in this room is round.", "On this marketplace people have been killed just now." und so on. Now someone could object that this requirement eliminates otherwise meaningful propositions, like "Enthusiasm is an important element in comprehension.", because neither "enthusiasm" nor "element" nor "comprehension" are spatiotemporally given. Yet, inquiring into the reasons for the claim one will eventually come up with a proposition of the following kind: "If some children are more enthusiastic than others (it is inessential how this is determined, by observation or interview) they will be better at recalling certain sentences they have been previously presented with." Doubtful alone are sentences like: "Psychic phenomena are in time, but not in space." as long as no way is specified for how to test the sentence. This requirement to test all theories on the basis of sentences drawn from ordinary language carefully employed (Carnap's "thing language"), is essential for "physicalism", which Horkheimer and many others condemn. The best test of physicalism is scientific practice. On the one hand Horkheimer thinks that the proponents of physicalism aim to make ultra-exact prognoses: "The ideal it pursues is knowledge in the form of a mathematically formulated universal science, deducible from the smallest number of axioms, a system which assures the calculation of the probable occurrence of all events [. . .]. Ultimately, according to positivism, the events of the human world will be predicted with the same degree of probability as all other events." (p. 10 [p. 138–139]) But then again he believes they lack the courage to make prognoses: "The task of the scientist is to find facts, and not to indulge in prophetic insights." (p. 29 [p. 161]) In fact, the stance of a proponent of the unitary science with respect to the present question is attuned to the scientific practice in a given branch of science; as far as possible one makes highly accurate prognoses, for example in determining an eclipse of the sun, then again in geology or history one is satisfied with much less accurate forecasts. Sometimes crises and revolutions are perhaps better predictable than occurrences of earthquakes or hurricanes in the realm of inanimate nature. In other cases regarding society at large one can only make unreliable forecasts or none at all. The method advanced by Horkheimer would be superior if it were to permit systematic prognoses in circumstances where similar prognoses are unattainable on the basis of the unitary science.

If one interprets Horkheimer's metaphysical expressions from an empiricist's viewpoint, he seems to believe that one can scarcely predict anything entirely novel if one is restricted to collecting experiences and to theories resting on generalizations. This is correct and this is why the strict empiricist holds that many new, successful and useful beliefs do not arise by way of deduction, as was mentioned above. Yet Horkheimer thinks logical empiricism robs us of the ability to turn to what is novel: "[Empiricism] assigns supreme intellectual authority to the accredited science, the given structure and methods of which are reconciled to existing conditions." (p. 15 [p. 144]), without even so much as indicating the claims from which he has drawn this conclusion. There is nothing in empiricism's scientific stance – open as it is for the discussion of any form of expression and making room for any kind of skepsis but also for every new idea, provided they comprise testable theses – which could support the following judgment Horkheimer's: "It is, of course, true that every event is resolvable into facts – and facts, varying in widely different ways according to the situation, play a decisive part in any proof. Nevertheless, it seems to us rather out of place to form a new school of empiricism on this circumstance alone. It looks too much like a promise that knowledge will keep to the narrow path of certainties and not deal with historical controversies at all or only in some indefinite future. "The view that thought is a means of knowing more about the world than may be directly observed... seems to us entirely mysterious," is the conviction expressed in a work of the Vienna Circle.[4] This principle is particularly significant in a world whose magnificent exterior radiates complete unity and order while panic and distress prevail beneath. Autocrats, cruel colonial governors, and sadistic prison wardens have always whished for visitors with this positivistic mentality. If science as a whole follows the lead of empiricism and the intellect renounces its insistent and confident probing of the tangled brush of observations in order to unearth more about the world than even our well-meaning daily press, it will be participating passively in the maintenance of universal injustice." (p. 21 [p. 151], see 17, 28, 29 and so on) Stripped of its emotional dressing this means: the principal stance of empiricism prevents it from observing certain connections and processes, from looking behind the scenes, while the one advocated by Horkheimer permits all this. Horkheimer's claim would have to be supported by a wide-ranging historical investigation into the scientific working method of empiricism. General metaphysical and emotional phrases carry no argumentative force to the empiricist. Horkheimer's statement "[Empiricism renounces] constructive thought which evaluates facts and discriminates between surface and pith" (p. 21 [p. 152]) is nothing more than a metaphorical expression, since what counts as "pith" and what as "surface" in the analysis of a given historical case? By way of example Horkheimer demonstrates how by removing a tortured animal's capacity to scream through surgery poorly observing members of the vivisection movement may be deceived. "The pleasure which the younger Vogt derived from the gullibility of those good people is a perfect example of the pleasure to be derived from naïve empiricism in a world in which everything is attuned to deception." (p. 22 [p. 152]) Let us take up this example – one of the few given at all: does Horkheimer believe that an empiricist, trained in biology or sociology, and hence immodest, who would even be somewhat skeptical in this case, will have more trouble discovering the act of surgery than a critic schooled in Horkheimer's dialectic? Yet on account of general considerations Horkheimer has established: "[Hume] felt that the elimination of constructive thinking, the obliteration of the opposition between subject and object, theory and practice, thinking and willing [. . .] had a disturbing, negative effect. His followers no longer share this feeling; one would look in vain for any sign of sorrow on their part over the impotence of reason. Modern empiricism is silent on this point, that is, unless it unsuspectingly adopts a Hegelian term and declares that "the mystical" enters with the problems of life." (p. 26 [p. 157]) To what extent logical empiricism holds that at life's problems the "mystical" begins the encyclopedia will surely reveal. Now, Horkheimer feels that one somehow has to make plausible how it is possible that empiricists, who are critical and always hungry for facts, do on principle overlook certain facts. He imagines "[a struggle between] resolute groups who are no longer able to bear life under that oppressive order [. . .] – a struggle which the impassive "fact-finding" mechanism of science does not see" (p. 29 [p. 160–161]). "Fastening their eyes on a better life they were able to see through the deceit of the established order [. . .]. None of the trumpery recorded in protocols had escaped their penetrating attention [. . .]. Dialectical thought integrates the empirical constituents into structures of experience which are important not only for the limited purposes served by science, but also for the historical interests with which dialectic thought is connected." (p. 30 [p.161–162]) The empiricist would tend to think that science would eventually be adapted to the one as to the other purpose, as was always the case, and furthermore, that it may well come to pass that people, who with extraordinary energy aim to change the order of society, reorient their attention and change their theories. But as has been observed at the beginning, it is hard to understand why such a shift must always promote the scientific attitude; sometimes it may reduce the sum total of insight. Occasionally those who aim for something turn out to be right in their prediction of success, although such prophecying does not rest on an argument that could be used further on as a method. By the way, it is difficult to see why empiricists could not be among the people who want to change the organization of a society, and do not make all the errors of observation, which Horkheimer otherwise attributes to the followers of "modest empiricism". The assumption, incidentally, that this group of people remains entirely unnoticed must almost be labeled "mythical"; it expresses belief in some wise powers, dwelling in the hidden, which secretly shape events. Supposing that other people too get to know about the existence of these groups, it becomes possible that disinterested onlookers are in the best position to make predictions, or that the deeply pessimistic defeated prove to be reliable prophets. In whatever way one interprets Horkheimer's assumptions it is difficult to bring to light how one can deduce a method from them. Perhaps this will be shown at a later point. In the meantime Horkheimer distances himself from the Unity of Science Movement without further argument in favor of his method: "[The] formulation of a unitary language and a unitary science, even if their specific usefulness were conceded, do not in any case belong to a science that desires the respect of philosophical thought." (p. 49 [p. 184]) Since his own method is an extra-scientific one, the question arises what kind of science it is, which ought to be respected in his view.

If one interprets as far as possible in an empiricist manner Horkheimer's emotional phrases, his metaphorical and simile-ridden accounts as well as his metaphysical expressions one faces serious problems, as has been described in the beginning employing empiricist language. It would certainly be valuable to further analyze these problems that also concern the situation of empiricism, based on facts and cases from the last 100 years. An historico-sociological account of the history of scientific empiricism remains a scientific desideratum; if it existed one could find out easier whether one or another of Horkheimer's metaphysical formulated theses point to certain connections, which so far have escaped many empiricists. Yet, even if such an examination were to confirm this, that would not prove that Horkheimer's method is useful and his metaphysical style of writing necessary, no more than the astrologists' onetime advantage over the astronomers in accounting for high and low tides proves that their method is superior to the astronomical method. To this end a comprehensive scientific study would be necessary.

The Unity of Science Movement rests on the assumption that one can find within unitary science as much as within physics, for instance, a form of expression that enables scientists to express their views in a common language, however contrary their views may be. Certainly, a critical, non-dogmatic empiricist cannot exclude the possibility that the urge to express oneself in ways that hinder universal communication will remain alive, to the effect that the Unity of Science Movement can only capture part of the total scientific activity. As soon as the Unity of Science Movement has shown to a greater extent what she is capable of achieving, one will be better able to estimate its historical chances, which of course depend on social and other kinds of factors. An empiricist can engage himself in the unitary science with unwavering determination without need for some kind of "reason" to proclaim that one and only one way is correct, and he is the one who knows which one it is.

Notes

1. English in the original.
2. English in the original.
3. Title and table of contents reproduced in English by Neurath.
4. Hahn, H. (1933). Logik, Mathematik und Naturerkennen, Otto Neurath et al. (eds), *Einheitswissenschaft,* Heft 2, p. 9. Vienna. M. J. O'Connell.

K. 63

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EINHEITSWISSENSCHAFT UND LOGISCHER EMPIRISMUS
Eine Erwiderung 1)
von Otto Neurath .

(03)

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1) vgl. Max Horkheimer . Der neueste Angriff auf die Metaphysik . Zeitschrift für Sozial~~XXXXXXXXXX~~forschung VI, Heft I. S 4 ff

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Man kann jede wissenschaftliche Bewegung unter vielerlei Gesichtspunkten betrachten ; man kann z.B. die wissenschaftlichen Arbeiten verschiedener Forscher in Hinblick auf ihre Leistung hin kennzeichnen , ohne sich um die chronologische Folge zu kümmern, die nicht einmal bekannt sein muss. Man kann zeigen ,/wie weit das was Oresmius ~~XXX~~ über ~~XXX~~ das Geld als Tauschmittel sagte , sich etwa mit der ~~XXXXX~~ Theorie Knapps berührte ; man kann zeigen, wie bei Grimaldi Vorstellungen von der inneren Vielgestaltigkeit der Lichtvorgänge auftreten als er seine Beugungs-und Interferenzversuche machte , während Huyghens die mathematische Behandlung der Lichtausbreitung in den Vordergrund rückte und sich wenig um die Buntheit und Unschärfe gewisser Lichterscheinungen kümmert, die erst andere mathematisch behandelten . Man kann aber auch ~~die einzelnen XXXXXXXXXXXXXXXX~~ Lehren chronologisch und gedanklich so ~~XXXXXXXXXX~~ miteinander verknüpfen ~~, dass man~~ und so die Entstehung eines umfassenden Lehrgebäudes verfolgen ~~XXXX~~ . Manches, was einer ausgedacht wird vergessen , anderes verbindet sich mit Elementen aus ganz anderen Quellen; Ideen antiker Denker verknüpfen sich mit wissenschaftlichen Arbeiten der Scholastiker , Spekulationen der Astrologen, Theorien der Renaissancegelehrten , um so ein ~~XXXXXXXX~~ Mosaik zu bilden, dessen Zusammensetzung sich ständig ändert , immer neue Züge zeigend . Man kann sich ~~XXXX~~ aber auch fragen, wie denn die einzelnen Forscher zu ihren Anschauungen gekommen sind, welche persönlichen Eindrücke und Schicksale wesentlich waren ~~XXXXXXXXXXXXXXXXXXXXXXX~~ um so zu einer " Behavioristik der Gelehrten " vordringen . Eine andere Fragestellung beschäftigt sich mit dem Problem , wie die Gesamtzustände eines Zeitalters mit dem Auftreten bestimmter Anschauungsweisen verknüpft sind , insbesondere welcher Zusammenhang zwischen gesellschaftlicher Situation und Wissenschaftsbetrieb besteht ~~XXXXXXXXXX~~

- so kommen wir zur " Wissenschaftssoziologie" und " Wissenschaftsgeschichte" .

Durch historische Wandlungen wird aber nicht nur das geändert, was wir theoretische Formulierungen oder Konstruktionen nennen , sondern auch der Bestand an Protokollen ; was in einer Zeit überhaupt nicht bemerkt wird, fällt einer anderen auf , was in einer Zeit, zwar bemerkt, aber nicht für wichtig gehalte n wird, kann in einer anderen Mittelpunkt wichtiger Erwägungen werden . Dabei muss man beachten , dass " Konstruktionen " und " Rohmaterial " sich nicht recht trennen lassen, ~~wie insbesondere Duhem sogar im Gebiet der Physik hervorhob~~ . Manche unserer Beobachtungsaussagen und Anschauungen erweisen sich als sehr stabil , aber grundsätzlich ists nichts gesichert - alles fliesst . Es liegt XXXXX nahe, dass ein konsequenter Denker , XXXXXX all diese auf Erfahrungen gestützten Betrachtungen auch auf sein eigenes Leben an - wendet und sich abfragt , wie er wohl sich verhalten, wie er wohl argumentieren würde, wenn er andere r Stelle stünde. Es wird ihm klar , dass entscheidende Wandlungen im Wissenschaftsbetrieb nicht nur durch intensives XXXXXXXXXX Nachdenken einer Gelehrtengeneration bestimmt wird, ~~abhängen,~~ sondern auch durch das, ~~von dem,~~ was im gesellschaftlichen Leben geschieht , von dem die Gelehrten einen Teil ausmachen. XXXXXXXXXXXXXXXXXXXX Betrachtet er sein XXXXXXX Gesamtverhalten XXXXXXXXXXXXXXX , in dem ja wissenschaftliches XXXXXXX Tun mit allem anderen aufs innigste verwoben ist ~~- es fragt sich ob bei solcher Betrachtung es sogar zweckmässig ist so etwas, wie das " wissenschaftliche Tun" abgesondert zu betrachten und zu benennen -~~ dann kann XXXXXXXXXXXXXXXXXXXXXXXXXXXXXXXXXXX es passieren , dass ein gesellschaftlicher Vorgang (Beharren oder Neugestaltung), den er aus irgend einem Grund ersehnt , etwa den Wissenschaftsbetrieb schädigt, der ihm auch lieb ist ; wie viel erfreulicher für ihn, wenn der ihm aus anderen Gründen erwünschte soziale Vorgang auch XXXXXXXXXXXXXXXXXXXXX den Wissenschaftsbetrieb zu XXXXXXXXXXXXXXXXX fördern scheint . Wenn er einen " Konflikt" vor sich sieht , ist es Sache seines persönlichen Entschlusses , XXXXXXX XXXXXXX an welcher Stelle er wirken will.

Einer mag zunächst auf Grund einiger Erfahrungen ~~meinen~~, dass ruhige Beschaulichkeit allein gutes wissenschaftliches Urteil sichere , besonders im Gebiet der Sozialwissenschaften , wo ja emotionelle Elemente heute zuhause sind , dann aber an historischen Beispielen feststellen, XXXXXXXX dass auch Liebe und Hass treffliche Lehrmeister zu ermöglichen (sein ~~können~~. Das kann nicht a priori ausgefunden werden ; erst recht nicht, ob in einem

Chapter 3
The Neurath–Horkeimer-Controversy Reconsidered: Otto Neurath's *Erwiderung* to Max Horkheimer's Attack Against the Vienna Circle

Karlheinz Barck

3.1 Hors d'œuvre

The history of the relations between the Frankfurt School and the Vienna Circle during the period between the world wars is now generally well known. Since the eighties and nineties historians and historians of science like Friedrich Stadler, Elisabeth Nemeth, Rainer Hegselmann, George Albert Reich and Hans-Joachim Dahms described the "Exodus of scientific reason"[1] under the rule of National Socialists in detail. They also provided critical examination of the polemic prejudice of a "Herrschadtsphilosophie" of the Vienna Circle coined and put in circulation by Max Horkheimer.[2] In Germany it was namely Hans-Joachim Dahms who analyzed the relations between Critical Theory and Vienna Circle as an evolution "from cooperation to confrontation"[3] putting into question the philosophical leadership role of Critical Theory in Western Germany after 1945 such as it was reclaimed by Max Horkheimer. In the nineties the controversy of the thirties gained also some new interest and attention in the context of the ideas and concepts which Otto Neurath had developed in favour of *Unified Science* and *Encyclopedism* as a model and a medium for connecting (vernetzen) knowledge and science.[4] „Neurath's conception of science and its metatheory places us squarely, if oddly, in the field of ‚postmodern' debate. Yet in the respect Neurath remained resolutely modernist. Is the convergence of his reasoning with ideas forwarded by contemporary theorists of science undercut by his Enlightenment orientation? Did he not 'buy into' the metanarrative of emancipation over the course of history?"[5] To those questions the authors of their study of Otto Neurath's concept of science and politics gave an answer that looks at Neurath's position as a radicalization of the *Dialectics of enlightenment* (*Dialektik der Aufklärung*). "But recall Neurath's long-held view: for reason to fulfill its Enlightenment promise it has to be *reconceptualised*. Moreover, if 'post-modernism' represents not a blanket denouncement of rationality but rather

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J. Symons et al. (eds.), *Otto Neurath and the Unity of Science*, Logic, Epistemology, and the Unity of Science 18, DOI 10.1007/978-94-007-0143-4_3,

a programme to seek a new and better understanding of what reason is and can do, then Neurath's drive for rationality without foundations is not at odds with it either."[6]

3.2 Neurath's Typescript Found in the Haarlem Archive

Now: the document that is published here for the first time, thanks to Olga Pombo who motivated me and proposed to include it in this book about the actuality of Otto Neurath's intellectual universe, is a *trouvaille* and can be taken as one further mosaic stone to complete our knowledge and our understanding of those historical polemics and controversies between the Frankfurt School and the Vienna Circle whose protagonists were Max Horkheimer and Otto Neurath and whose waves reached also Walter Benjamin and Bertolt Brecht.[7]

Horkheimer's well known attack against the Vienna Circle, published in his typical polemic style of the epoch and written with programmatic intentions as the opening essay of number one of its sixth year 1937 in the *Zeitschrift für Sozialforschung*, under the title *Der neueste Angriff auf die Metaphysik* (*The latest Attack on Metaphysics*). This journal edited "im Auftrag des Instituts für Sozialforschung" by Max Horkheimer in Paris by the *Librairie Félix Alcan.*[8]

With his text Horkheimer started a huge confrontation with the members of the Vienna Circle. Otto Neurath as the black sheep was the most concerned person in place because it was he who tried to develop contacts and discussions with Horkheimer and other members of the Frankfurt School exiled in the US. As an exile himself, Neurath lived from 1935 to 1936 in Den Hague and didn't return to Vienna except during a short visit after a brief visit to Moscow in 1934, being on the watch list of the austrian police. When the Horkheimer-Neurath-Correspondence became public and accessible in the Frankfurt Max-Horkheimer-Archive, along with the publication of other materials and documents, it became clear that Neurath wrote a substantial answer to Horkheimer's attack. But the *documentary evidence* was missing and couldn't be found and seemed to be lost. So Hans-Joachim Dahms wrote in his detailed reconstruction of the Horkheimer-Neurath-Controversy, published in 1990, that by the letters exchanged between the protagonists there can be no doubt that Neurath sent on december 8, 1937 his response with the title *Erwiderung* to Horkheimer in New York. Dahms writes that Neurath's text "must have been a small article. I couldn't find this article anyhere. For reasons that will be explained, it doesn't even exist in Horkheimer's estate.[9] The reason may be, suggests Dahms, that Horkheimer sent back the typescript to Neurath, who asked for it after Horkheimer had refused to publish his response in the Zeitschrift für Sozialforschung. Dahms continued "The original or even a copy neither exists in Neurath's estate. This is not astononishing because Neurath's whole property, which he left at home after an adventurous fleeing from the german troops to England, had been plundered by a task force command of the 'Rosenberg Office' which reported to their office that <we succeeded to get hold of the private library of Neurath, former minister of the Eisner government_."."[10]

This was written in 1990. At this time the Otto-Neurath-Heritage, after being established by Henk Mulder in 1978 in the *Wiener-Kreis-Stichting* at Amsterdam, had been transferred 10 years later to the Royal Netherlands Academy of Science. Because of space problems it was moved in february 1992 to Haarlem and integrated in the *Riksarchief in Noord-Holland* where it is since then accessible. In 1996 Reinhard Fabian published a complete inventory of the Otto-Neurath-Heritage at Haarlem which "lists all the items which can be found on mircrofilm."[11] It is there that Neurath's *Erwiderung* is listed under the inventory number *203* and the identification number *K.63*.

3.3 Einheitswissenschaft und logischer Empirismus: Eine Erwiderung (Unified Science and Logical Empirism: A Response)

This is the title under which Otto Neurath put his response. When I read the book mentioned and Fabian's répertoire in 2002, I was working with a project on Neurath's encyclopedism-concept. I decided to go to Haarlem in order to consult the Neurath papers in September 2004. During my work at the Haarlem archive I found the 24-page-typescript which I could read and copy from its original paper form. The DIN A 4 format is apparently a first version of Neurath's text which he corrected and worked through (*durcharbeiten*) by pencil and by felt tip (*Filzstift*) before sending it in *Reinschrift* to Horkheimer. The original (or last hand) version up to now continues to be missing.

Our text contains several corrections made by type writer (mainly cancels) and additions made by fountain pen or *Filzstift*. On the basis of Neurath's corrections to the first type-writer-version the fair copy which he send to Horkheimer in New York on December 8, 1937 must have been realized. As we know by the Horkheimer-Neurath-Correspondence, the Zeitschrift für Sozialforschung by Horkheimer's apodictic decision didn't publish Neurath's *Erwiderung*. When Neurath had read Horkheimer's attack in the June-1937-number of the Zeitschrift für Sozialforschung, he wrote a letter to Horkheimer on June 21, 1937: "Now I have read your article. First it struck me like a shock. Then I read it again and I saw that you give all your crushing blows soft soaped with loving encouragements."[12] In another letter Neurath was more intransigent and insisted that the Zeitschrift für Sozialforschung ought to publish his *Erwiderung*: "You really try, after exact observation and personal investigation, to send us to scaffold. Because we keep quite nasty (. . .) As sociologist of our Circle who is taken personally under gunfire (. . .), I must briefly reply to this article, because only those who don't want to get in contact with us will better have no reply".[13] But Horkheimer gave Neurath no chance so that Neurath on February 21, 1938 considered the controversy closed: "Dear Mister Horkheimer! Please send back my manuscript to me. With kind greetings yours Otto Neurath."[14]

3.4 The <différend> Between Horkheimer and Neurath

At a first glance on Horkheimer's and Neurath's text there is a difference in style and form, Horkheimer's attack being highly polemical, whereas Neurath's response doesn't pick up those polemic points, tries to be more emotionsless, more objective, more *sober* as it were. In this respect Horkheimer even didn't recoil to compare his understanding of the scientific praxis of the logical empirism as *clean* (*sauber*) with the nazi racial politics of *Säuberung* (*cleansing, purge*). "Because its barbarous attitude to language causes it to miss the actual meanings inherent in words, logical empiricism must fail to see the deeper connection between the glorification of the isolated quality of purity (*Sauberkeit*) and the need for a purge (*Säuberung*) to which authoritarian states make the most appalling concessions."[15] It is surprising that somebody like Walter Benjamin, one of the finest *Sprachkritiker* we have in Germany, welcomed Horkheimer's essay almost enthusiastically. In the first of his *Literaturbriefe* from Paris which he wrote for Horkheimer, he celebrated Horkheimer's essay as a model for a new style of philosophical critique. "The justification of the ruling class by undisguised market positions made so big progress that every criticism can't ignore it. The form of such criticism is then becoming a polemical one. The German philosophical production would à *la longue* not be sufficient in order to present our own position *per contrarium*. Your essay against the Vienna School represents a first attack-operation with larger dimension. It would be good if resolute criticism of schools in other countries could be published (...). Do you think it possible that the Review by chance give some space to a criticism of the leading american schools?"[16] This is rather astonishing when we remember the fact that Benjamin was involved in Brecht's project for the foundation of a *Gesellschaft für materialistische Dialektik*, which later was conceived as a Diderot-Society. Brecht who read in the beginning of 1933 Otto Neurath's "Empirical Sociology" nearly with enthousiasm, tried to win Otto Neurath for cooperation. In a letter from Thurø (Denmark) he wrote in the midst of 1933: „Dear Mr. Neurath. I read with great profit your <Empirische Soziologie>, I wrote down some remarks and questions that I would like to discuss with you orally. By now I like to inform you about a project which keeps us busy, some friends and myself. This autumn we will start, probably at Paris, a little society which in close cooperation will tackle a catalogue of concerned statements. By <concerned> we mean 'concerned by the configuration of social life' and the cooperation ought to be realized in the line of new methods of collective thinking in a way that those methods must at every moment keep subjects/objects of the working process."[17] Contrary to Brecht, Benjamin didn't give up his irreconcilable position. In his *Tagebuchnotizen* concerning his conversations with Brecht, he wrote in 1938: "Then we came to the topic <logical positivism>. I was rather intransigent and so our conversation threatened to a nasty turn. We could avoid this because Brecht for the first time confessed a somehow superficiality of his own formulations."[18]

Neurath's *Erwiderung* doesn't improve our knowledge about the controversies and the *différends* between Francfort School and Vienna Circle. It rather confirms what we learned from different research, especially by Hans-Joachim Dahms, Karl Müller, Rainer Hegselmann and others. It must be emphasized however that in a certain sense this controversy has been a special german affair because of the *Postivismusstreit* in Western Germany around the sixties.[19] In this context the members of the Frankfurt School didn't make any auto-critique of their positions proclaimed during the thirties. On the contrary, as Hans-Joachim Dahms, points out, we may understand „that the Frankfurt School during the 'Positivism-Quarrel' (Positivismusstreit) and in view of its strange opposition and the recent changes of the 'scientific worldview' developed rather totalizing judgments that are historically erroneous. But it remains, up to know, a problem to be explained in view of the failure of a cooperation between 'Critical Theory' and the 'Vienna Circle' how it came that one of the leading figures of the 'Critical Theory', Max Horkheimer, already at times of the Nazis in power proceeds in a rather short circuit way in his critique of the logical positivism, in a way which is an 'inexcusable false one' (R. Hegselmann 1983).“[20] I would imagine that Neurath's *Erwiderung* can give us an answer. It is really striking to see that the two concepts of philosophy and of science used by both are incompatible. Horkheimer didn't (and couldn't) understand the Vienna-Circle-Concept of science. "Dialectical logic", writes Horkheimer, "has reference to thought involved in the interpretations of living reality, to thought in process, and not merely to static expression."[21] "*Thought in process*" ("*Denken im Prozeß*") means for Horkheimer *thinking a posteriori* (*Nach-Denken*) as it were, whereas for Neurath it means *thinking in advance* (*Vor-Denken*), thinking as prediction and control. The whole of his *Erwiderung*, the logic of his argumentation, is organized around this main point, putting aside all the ideological attacks against the so-called affirmative theories of the Vienna Circle which weakens, in Horkheimers view, every anti-fascist activities. "These latter-day apologists for freedom from value judgments (*Wertfreiheit*) glorify the fact that thought has a subordinate role, that it has fallen to the level of a handmaiden to the prevailing objectives of industrial society with its extremely dubious future. The ruling powers can use thought that has renounced every determinative function. And the scientists, whose disparaging interpretation of values expresses just such a renunciation, help them along."[22] Neurath's claim, on the contrary, is for a *new thinking* confronting a new and dangerous historical thinking. "The hope to get orientated in advance about the standing of our own activity and scientificity in a historical manner seems to be the more restricted the more one is convinced that big changes are coming. How can unchanged thinking forsee what the future of changed thinking will look like and how can one work with it explaining our present historical configuration?"[23] The main point which Neurath discusses in his *Erwiderung* in order to make clear the fundamental *différand* between Horkheimer's and his own position, is what Horkheimer describes as "richtiges Denken" (right thinking): "Right thinking depends as much on right willing as right willing on right thinking."[24] Horkheimer's statement that "rational knowledge does not controvert the tested findings of science; unlike empiricist philosophy, however, it refuses to terminate them,"[25] marks for Neurath the blindness of Horkheimer's text. All over the 24 pages his *Erwiderung* is motivated by the intention to make this clear, hoping that further discussions might be developed on better than idological grounds. On page 4 Neurath writes: "Seine Hypothese, von der im folgenden vor allem die Rede sein soll, ist etwa: “this hypothesis which will be discussed above all in my text is like this: ‘We have an outer-scientific method which, based on everything that can be verified scientifically, is able to critique the sciences, especially by showing its historical situation in a manner completely unfamiliar to the sciences themselves’. This position which Horkheimer calls <dialectical> or <critical> is one transcending the metaphysical view as well as the scientific one. It is just the failure of the <scieentivists> to oppose the thinking, going by reason forward or by metaphysics backwards”.[26]

Neurath’s critique of Horkheimer’s claim for something like a universal *Deutungsmacht* of the critical philosophy in front of the sciences was perhaps the reason for the refusal to publish Neurath’s *Erwiderung* in the Zeitschrift für Sozialforschung. The displacement of philosophy by Unified Science as a *cosmic aggregation* in Neurath’s understanding,[27] the “key” metaphor which Horkheimer uses in his auto-description of critical thinking, is onconceivable in Neurath’s unified-science-thinking: “The idea that it could be a problem to find the <key> which opens every door, is unknown to the unity-of-science-movement. It is our opinion that the construction of the unified science can proceed in an analogous way like the particular sciences (Einzelwissenschaften) (. . .) But we don’t know any authority beyond science which may sit in judgment on science, analysing its fundamentals.”[28]

This is a programmatic statement that gives Neurath’s *Erwiderung* a special significance as a document for the theoretical battlefields in the european thirties where National Socialism and fascism, as well as stalinism worked as *enjeu* for the intellectual debates. The affinities and the differences between the Vienna Circle and the Critical Theory of the Frankfurt School, the french group of *Synthèses* and the Bachelard-Canguilhem-Tradition of epistemology as well, with the Collège de Sociologie around Georges Bataille and Roger Caillois, were s*urdéterminés* by the fact that they all were looking forward for a comparative historical view. For the case in point and for the time being, there was no way out. Horkheimer’s attack closed the door, and Herbert Marcuse joined him immediately when he wrote one year later in his review of the *International Encyclopedia of Unified Science*: – “The modern encyclopedists won’t proclaim any philosophy or worldview (. . .). The danger, however, lies in the motives that bear the project and that are not available out of it. What is bad and not true are those preconditions that orient the work – be they as fine and as exact as they may be”[29]

3.5 Colofón

Otto Neurath as a man of solidarity in fighting fascism was disappointed. The 1936 plans for discussion with the Frankfurters in New York had failed. By an indirect reference to the Horkheimer attack, we can estimate the degree of bitterness in Neurath’s feeling. In an unpublished 18-pages long “stenographic transcription of an address given by Dr. Otto Neurath at the Informal Seminar of Professor Clark

C. Hull, November 1, 1937", Neurath said what I read as an echo of Horkheimer's attack and his rejection to publish the *Erwiderung*: "The one point is that we are against dogmatic formulations. That means we are for the attitude which gives a great many possibilities to say, perhaps in a discussion: <We do not know this and this exactly, we can change tomorrow this theory; we have no absolute point for our science.> The scientific statements, together – that is the point; here we are at this moment. We have no a priori, no absolute point."[30]

Notes

1. Friedrich Stadler, Studie zum Wiener Kreis. Ursprung, Entwicklung und Wirkung des logischen Empirismus im Kontext, Frankfurt/M. Chapter 13 (pp. 607–622).
2. Cf. Hegselmann, R. (1983). Empirischer Antifaschismus. Das Beispiel Otto Neurath. In: Dialektik, 7, pp. 65–75. München; Müller, K. H. (1985). Die verspätete Aufklärung. Wiener Kreis und Kritische Theorie in der Epoche des Faschismus. In: Dahms, H.-J. (ed), Philosophie, Wissenschaft, Aufklärung, pp. 291–306. Beiträge zur Geschichte und Wirkung des Wiener Kreises, Berlin_de Gruyter.
3. Dahms, H.-J. (1990). Die Vorgeschichte des Positivismus-Streites von der Kooperation zur Konfrontation. Die Beziehungen zwischen Frankfurter Schule und Wiener Kreis 1936–1942. In: Jahrbuch für Sozialgeschichte, pp. 9–78. Opladen: Leske & Budrich.
4. Cf. the two-volume edition edited by Rudolf Haller and Heiner Rutt of Neurath's Gesammelte philosophische und methodologische Schriften, Wien: Verlag Hölder-Pichler-Tempsky 1999; George Albert Reisch, A History of the "International Encyclopaedia of Unified Science", Chicago Phil. Diss. 1995; Jürgen Mittelstraß (Ed.), Einheit der Wissenschaft. Internationales Kolloquium der Akademie der Wissenschaften zu Berlin, Berlin-New York: Waltert de Gruyter 1991; Elisabeth Nemeth/Friedrich Stadler (Ed.), Encyclopedia and Utopia. The Life and Work of Otto Neurath (1882–1945), Dordrecht-Boston-London: Kluwer Academic Publishers 1996; Nancy Cartwright/Jordi Cat/Lola Fleck/Thomas E. Uebel (Ed.), Otto Neurath: Philosophy between Science and Politics, Cambridge University Press 1996.
5. Cartwhright, N., Cat, J., Fleck, L. and Uebel, T. E. (eds) (1996). *Otto Neurath: Philosophy between Science and Politics*, p. 254. Cambridge University Press.
6. Ib. p. 254.
7. It was only after having written my commentary that I knew thanks to Thomas Uebel about and could read the seminal essay that he and John O'Neill dedicated 3 years ago *en connaissance de cause* (i.e. Neurath's *Erwiderung*) to the controversy, situating the unpublished Neurath-Text for the first time in its broader historical, theoretical and political context. In their conclusion they make the very interesting point for a "partials reconciliation" between the two schools (or movements): "However, we believe to have shown that there are systematic grounds for at least a partial reconciliation. And whatever advance this reconciliation will afford in the end, it also provides an occasion to end the odd estrangement and misunderstandings instigated by the 'Latest Attack on Metaphysics' and the failure of dialogue that followed in its wake." (John O'Neill and Thomas Uebel, Horkheimer and Neurath: Restarting a Disrupted Debate, in: European Journal of Philosophy, vol. 12, No 1 (April 2004), p. 98).
8. There is a complete reprint in 9 volumes by Kösel-Vberlag Munich 1970. An english translation of Horkheimer's text ahs been published in a volume of selected essays: Critical Theory. Selected Essays. Max Horkheimer. Translated by Matthew J. O'Connell and others, New York: The Continuum Publishing Company 1982. – Horkheimer's text has also been included in the edition of his writings: M. H., Schgriften, ed. By Alfred Schmidt und Gunzelin Schmid-Noerr. Vol. 4: Schriften 1936–1941, Frankfurt/M.: Fischer Taschenbuch Verlag 1988, pp. 108–161. – In retroview it is somewhat astonishing that in the same number was published a text by Otto Neurath *Inventory of the Standard of Living,* pp. 140–151.

9. (…) “muß den Umfang eines kleineren Artikels gehabt haben. Es war mir unmöglich, diesen Text aufzufinden. Er ist aus Gründen, die weiter unten geschildert werden, nicht in Horkheimers Nachlaß.”, Hans-Joachim Dahms, see No. 4, p. 56.
10. “Aber das Original oder wenigstens eine Abschrift befindet sich auch nicht unter den Hinterlassenschaften Neuraths. Das ist vielleicht auch kein Wunder, da sein Eigentum nach abenteuerlicher Flucht vor den deutschen Truppen nach England von einem Greifkommando des Amtes Rosenberg geplündert wurde, das der vorgesetzten Dienststelle darüber schrieb, ihm sei <die Privatbibliothek des früheren Ministers der Eisner-Regierung, Neurath> in die Hände gefallen”, Ibid. – Cf. Léon Poliakov/Josef Wulf, Das Dritte Reich und seine Denker. Dokumente. Berlin: Verlags-GMBH 1985, pp. 156–160.
11. Reinhard Fabian, „The Otto-Neurath-Nachlaß in Haarlem (NL). In: Elisbateh Nemeth and Friedrich Stadler (Ed.), Encyclopedia and Utopia. The Life and Work of Otto Neurath (1882–1945). With the first publication of Otto Neurath’s full manuscript on “Visual Education” and the documentation of the *Otto Neurath Nachlaß*, Dordrecht-Boston-London 1996, pp. 337–355.
12. “Nun habe ich Ihren Artikel gelesen. Erst hats mir die Stimme verschlagen vor Schreck. Dann habe ich ihn nochmals gelesen. Da habe ich denn doch gesehen, wie Sie alle Keulenschläge sozusagen unter liebevollem Zuspruch austeilen”, Cit. In Hans-Joachim Dahms, No. 3, p. 55.
13. “Sie sind ja wirklich bestrebt, uns nach genauer Betrachtung und persönlicher Befragung aufs Schaffott zu senden. Denn wir kommen übel weg […]. Als Soziologe unseres Kreises, der überdies persönlich unter Maschinengewehrfeuer genommen ist […] muß ich natürlich auf diesen Artikel kurz antworten, denn nur denen, die mit uns keinen Kontakt suchen, antwortet man besser nicht”, Ibid.
14. „Lieber Herr Horkheimer! Bitte lassen Sie mir mein Manuskript zusenden. Mit bestem Gruß Ihr Otto Neurath. Ibid. p. 60.
15. Max Horkheimer, The latest attack on metaphysics, loc. cit., p. 179 (German oiriginal in t. VI of the reprint of the Zeitschrift für Sozialforschung, p. 45).
16. “Die Befestigung der herrschenden Klasse in unverkleideten Marktpositionen hat so große Fortschritte gemacht, daß sie sich jeder Kritik aufnötigt. Deren Form wird damit zur polemischen. Die deutsche philosophische Produktion wird freilich à la longue nicht ausreichen, unsere eigene Position per contrarium zu umreißen. Ihr Aufsatz gegen die Wiener Schule stellt eine erste Angriffsoperation weiteren Ausmaßes dar. Es wäre gut, wenn entschlossene Auseinandersetzungen mit Schulen anderer Länder erscheinen könnten […]. Könnte die Zeitschrift gelegentlich eine kritische Auseinandersetzung mit den maßgebenden amerikanischen Schulen geben?”, Walter Benjamin an Max Horkheimer 3. November 1937. In: W. B., Gesammelte Briefe. Bd. V 1935–1937. Ed. By Crhistoph Gödden and Jenri Lönitz, Frankfurt/M.:Suhrkamp Verlag 1999, p. 600. – This is, by the way, the only remark by Benjamin about the Vienna Circle.
17. “Lieber Herr Neurath, ich habe Ihre <Empirische Soziologie> mit großem Gewinn gelesen, mir auch Anmerkungen gemacht und Fragen notiert, möchte diese aber gern einmal mündlich vorbringen. Sogleich aber möchte ich Ihnen von einem Projekt berichten, das einige Freunde und mich selber beschäftigt. Wir wollen diesen Herbst, wahrscheinlich in Paris, eine kleine Gesellschaft starten, die in enger Zusammenarbeit einen *Katalog eingreifender Sätze* in Angriff nehmen soll. Unter <eingreifend> ist natürlich gemeint >in die Gestaltung des gesellschaftlichen Lebens eingreifend> und die Zusammenarbeit soll nach neuen Methoden kollektiven Denkens erfolgen, und zwar dergestalt, daß auch diese Methoden immer Gegenstand der Arbeit bleiben sollen”, Bertolt Brecht Werke, Vol. 28: Briefe I, Berlin u. Weimar/Frankfurt/M. 1998, p. 366. – It is obvious that Brecht’s formulation >A catalogue of intervening sentences> refers to Neurath’s concept of “Protokollsätze”. We don’t know (up to now) if O. N. did respond to this letter. For Brecht’s relation with the Vienna Circle which during long time has been ignored by the Brecht-Forschung, see now the book by Erdmut Wizisla, the director of the Berlin Brecht and Benjamin Archives: Benjamin und Brecht. Die Geschichte einer Freundschaft. Mit einer Chronik und den Gesprächsprotokollen

des Zeitschriftenprojekts <Krise und Kritik>. Frankfurt/M. 2004 and the seminal study by Ulrich Sautter: „Ich selbst nehme kaum noch an einer Diskussion teil, die ich nicht sogleich in eine Diskussion über Logik verwandeln möchte." Der Logisch Empirismus Bertolt Brechts. In: Deutsche Zeitschrift für Philosophie (Berlin), 43 (1995), 4, pp. 687–70.

18. „Kurz darauf erschien das alte Thema <Logischer Positivismus>. Ich erwies mich ziemlich intransigent und das Gespräch drohte eine unangenehme Wendung zu nehmen. Sie wurde dadurhc verhütet, daß Brecht zum ersten Male die Oberflächlichkeit seiner Formulierungen eingestand.", Walter Benjamin, Gesammelte Schriften Vol. VI. Fragmente vermischten Inhalts. Autobiographische Schriften. Frankfurt/M. 1985, p. 535.
19. Cf. T. W. Adorno et al., Der Positivismusstreit in der deutschen Soziologie, Neuwied 1969.
20. "daß die Frankfurter Schule im <Positivsmusstreit> angesichts seiner merkwürdigen Frontstellung und auch angesichts der inzwischen eingetretenen Wandlungen der 'wissenschaftlichen Weltauffassung' zu ziemlich pauschalen Urteilen kam, die zumindest historisch irreführen, mag man vielleicht noch verstehen. Daß aber einer der Wortführer der Kritischen Theorie', Max Horkheimer, schon zu Zeiten der Naziherrschaft in seiner Kritik des logischen Positivismus eher kurzschlüssig argumentiert, seine Sicherheit sehr stark sachlicher Ignoranz verdankt, kurz, <in unentschuldbarer Weise fehlerhaft> (R. Hegselmann 1983) verfahren war, wird zumal angesichts der damals angebahnten aber schließlich nicht zustande gekommenen Kooperation erklärungsbedürftig bleiben.", Hans-Joachim Dahms 1985 (See note no. 3, p. 360).
21. Max Horkheimer, The latest attack. . ., loc. cit, p. 177 (german original loc. Cit, p. 43).
22. Ib. p. 164s (german original, p. 32).
23. "Die Hoffnung, sich vorausschauend über die Stellung der eigenen Aktivität und Wissenschaftlichkeit in historischen Dimensionen orientieren zu können, ist offenbar umso begrenzter, je mehr man der Ansicht ist, dass eben grosse Umwandlungen bevorstehen, denn wie soll das noch ungeänderte Denken voraussehen, wie das in Zukunft geänderte Denken aussehn und wie man von ihm aus unsere heutige historische Situation bearbeiten wird" (*Erwiderung* p. 3).
24. Ib. p. 162.
25. Ib. p. 164 (german original p. 32).
26. (. . .) es gibt eine ausserwissenschaftliche Methode, die gestützt auf alles, was wissenschaftlich festgestellt wird, die Wissenschaften kritisieren kann, insbesondere auch dadurch, dass in einer den Wissenschaften fremden Weise ihre historische Situation aufgezeigt wird. Dieser von Horkheimer dialektisch' oder kritisch' genannte Standpunkt gehe sowohl über den metaphysischen, als auch über den wissenschaftlichen hinaus, es sei gerade der Fehler der Szientivisten', dass sie gegen das Denken sind, ob es mit der Vernunft nach vorwärts, oder mit der Metaphysik nach rückwärts will.' (Ibid, p. 51)
27. Cf. Otto Neurath, „Universal Jargon and Terminology (1941). In: Philosophical Papers 1913–1946, ed. and translated by Robert S. Cohen and Marie Neurath, Dordrecht: Reichel 1983, p. 218.
28. Die Vorstellung, es könne sich darum handeln, den "Schlüssel" (p. 28) zu finden, der die Pforte öffnet, ist der Unity of Science Movement fremd, die sich denkt, dass der Ausbau der Einheitswissenschaft ähnlich vor sich gehen könne, wie der Ausbau der Einzelwissenschaften [. . .]. Aber wir kennen keine Instanz jenseits der Wissenschaft, die über die Wissenschaft zu Gericht sitzt und ihre Grundlagen untersucht (p. 10).
29. "Die modernen Enzyklopädisten wollen keine Philosophie und keine Weltanschauung proklamieren [. . .]. Die Gefahr liegt in den Motiven, die das Unternehmen tragen, und diese sind nicht aus ihm heraus greifbar. Schlecht und unwahr sind die Voraussetzungen, auf Grund deren gearbeitet wird, – die Arbeit selbst mag sehr fein und sehr exakt sein"., Herbert Marcuse in: Zeitschrift für Sozialforschung, vol. VII (1938), pp. 228–232.
30. Otto Neurath, Problems and Methods involved in the Unification of the Sciences. In: Otto-Neurath-Nachlaß (Haarlem Archives, NL), Inventory number 201, identification number K 52, p. 15.

Acknowledgement I thank the stuff of the librarians in the Haarlem-Archive of the Rijksarchief in Noord-Holland for their invaluable help and my colleague Karsten Strathausen for reviewing this english version of my text. – Thanks also to Thomas Bonk for his english translation of Neurath's *Erwiderung*.

Chapter 4
Otto Neurath's Epistemology and Its Paradoxes

Jan Sebestik

4.1 Otto Neurath's Holism

Otto Neurath occupies a special place inside the Vienna Circle. His activities during the revolutionary Republic of Councils in Bavaria blocked all possibility of an academic career and obliged him to look after for other sources of income. The variety of subjects touched upon in his work, and his often neglected and sometimes irritating and aggressive style, together with his radical and militant opposition to metaphysics and to philosophy in general, prevented the spread of his ideas. His scientism and the naïveté of his social engineering was the target of constant criticism and mockery. More to the point, his holism represents a distinct current within the Vienna Circle, especially when compared to the general analytical trend represented by Carnap who became its leading figure. Neurath's first masters were Mach, Duhem and Marx – and also Josef Popper-Lynkeus – rather than Bertrand Russell, Einstein and Wittgenstein, who were claimed to be the main inspiration for the Vienna Circle in the Manifesto *The Scientific Conception of the World*: *The Vienna Circle* as its instigators. In spite of his radical opposition to the Tractarian metaphysics of the unsayable, his reflections on language parallel those of Wittgenstein. Contrary to Carnap and many other members of the Circle, he was keenly interested in history of all sorts: social, religious, and economic history, history of religions, history of science – his dissertation dealt with economics, commerce and agriculture in Antiquity and another book, *Economic History of Antiquity*, became a standard work in its field. He also contributed to the history of science with two fundamental articles on history of optics.[1]

His holism, thinking in terms of systems, of global structures, of wholes, is already present in his first works. These wholes are not necessarily found in the real world or in nature; they are parts, which are "cut out" from different sectors of

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J. Symons et al. (eds.), *Otto Neurath and the Unity of Science*, Logic, Epistemology, and the Unity of Science 18, DOI 10.1007/978-94-007-0143-4_4,

reality and combined with other parts or elements in view of the solution of a particular problem. Let us take consider a few some examples. Suppose we are studying the transfer of material goods in some economical system.

> It seems that all goods behave mutually like complementary goods in such a way that only the total picture of the system of goods can serve as the basis of comparison. This manner of considering the things leads to a conception of political economy and of the social sciences that very much resembles to an organicist theory.[2]

Suppose we wish to compare two political systems, e. g. two states. We should not proceed in by comparing particular aspects, for instance, first the constitutions, then the climate and so on. "We must first conceive each state as a whole".[3] Do we intend to describe the customs and habits of a society? We have to ask which system they form: "In a sense, we have always to discuss the *totality of the customs* of a society, which are often interconnected in very different ways".[4] "It is necessary to catch the totality of thought."[5] This effort to derive the particular aspects from the whole makes Neurath conspicuous stand out in the Vienna Circle. Neurath He interprets all phenomena in their global context. It is impossible to understand them independently of the circumstances, of the whole context in which they appear and eventually ultimately of the cosmic context itself. Moreover, for Neurath, this global conception is closely connected with the rational activity of man, aiming at planned reorganization of the society through education and social engineering inspired by Marxism. The education is to be the global formation having its aim in the development of all the capacities of an individual. "Our ideal is again the complete man".[6] Neurath accepts specialists, "partial men", only as a necessary evil. The utopian character of the idea of a universal individual, advocated already by Engels in the *Anti-Dühring*, is particularly striking when compared with the development of science and technology in the last 100 years, which has required specialization demanding an increasing competence in relatively narrow fields. This does not mean, of course, that a future scientist or engineer should not first have a thorough general instruction in his discipline as well as an informed introduction into general culture.

Is Neurath a holist? He surely is if we follow the contemporary usage of the word by Quine and other epistemologists. But he is not, if we adhere to the original meaning of the term as defined by General Smuts, for whom: holist metaphysics was, a kind of mysticism of universal harmony, similar to Neoplatonism. Neurath opposes the global thinking of science to the mystical holism which he finds in the works of his contemporaries Karl Mannheim, Othmar Spann and of course Oswald Spengler, the target of his *Anti-Spengler*. Surprisingly, he combines his scientifically oriented holism with nominalism.

Holism, then, but without mysteries, without mysticism and metaphysics. In spite of the preceding quotation of the young Neurath, his global thinking is not an organistic theory like that of Hans Driesch, which admits a *force vitale* and works with the Aristotelian concept of entelechy. In Neurath's theory of science, no other concepts are admitted than those which can be expressed in terms of time and space.

His holism consists in the necessity of integrating all events and phenomena into structures of ever higher order and increasing complexity and eventually into cosmic structures. This unexpected combination of global thinking and nominalism does not have its roots in biology, but rather in physics and in social sciences: on one hand, his inspiration comes from Mach and Duhem, on the other hand Marx. In spite of all the differences among these three authors, they agree in the following substantial points: (1) our concepts depend both on the system to which they belong, and on historical circumstances which gave birth to them, (2) all research develops on the background of wholes which are not directly given, but whose global properties are locally reflected in objects in such a way that through the objects directly aimed at, our concepts reveal the global structures, whether this be social or even cosmic. Even when we verify the statements describing the behavior of levers or of rigid bodies, we verify in fact "the behavior of our cosmic aggregation. We cannot get rid of it and, at least in principle, we are obliged to take it into account. On one hand, this kind of reasoning agrees with some of Mach's arguments, on the other hand, with some arguments of Marx".[7]

Our theories and observations are embedded in a double context: that of synchrony, the context of actual statements about reality, that of diachrony, in the web of our traditions, of our past. Let us look first on at *synchrony*. Neurath's *Ganzheitsdenken,* the global thinking, is revealed in the interdependence of all our statements about the world. Contrary to the largely widespread image of the positivist collecting isolated facts and believing that the theory will simply follow from them – which is not true even of Auguste Comte – Neurath stresses that all observation, all experiments are saturated by theory. This "positivist" speaks about the relationship between theory and observation like Alexandre Koyré or Gaston Bachelard: "It is necessary to have an approximate theory already in order to ask the good questions concerning measure".[8] Even if it is possible to separate the statements of a science into theoretical and protocol statements, it is impossible to isolate the protocol statements and make of them the immutable basis of the whole theory. We do not have on one side the ordinary language full of impurities, on the other side the purified language of science. The language of science is embedded into the ordinary, natural language; it contains "verbal clusters", *Ballungen*, containing both ordinary words and the expressions of pure science. It is precisely these verbal clusters which make possible the communication possible between the scientists and the engineers who design, produce, install and use the instruments for observation, experiments and measure. If we want to reconstruct the science from the first elements, we cannot but operate with existing theories and material we have at our disposal. This is the meaning of the famous metaphor of Neurath's ship, borrowed perhaps from Maxwell, that appears three times in his texts.

> We possess no fixed point which may be made the fulcrum for moving the earth; and in like manner we have no absolutely firm ground upon which to establish the sciences. Our actual situation is as if we were on board ship on an open sea and were required to change various parts of the ship during the voyage. We cannot find an absolute immutable basis for science.[9]

Let us turn now to consider diachrony. Our language and our theories are rooted in the past, grow out of it. We cannot simply erase our past: "we cannot simply discard at once the whole conceptual construction that we have received from the tradition,"[10] because all change of our concepts and words is accomplished by means of concepts and words. No scientific language is created *ex nihilo*, and an uninterrupted chain connects the new concepts with the old words and with the words of the common language. Our actual statements about the world are thus related by multiple links to the science of the past. We have inherited our language from the past generations and the scientific activity develops against the background of ancient habits and usage.

Neurath is attentive to global aspects and interconnections in studying historical phenomena: specific forms of behavior, of habits, ideas and convictions characterize each society as well as each person. These global phenomena are nevertheless not isolated. Neurath stresses the function of each phenomenon and can thus escape to the objection of historicism. His interpretation of the relationship between Protestantism and capitalism is not too far from the functionalism of Malinowski. Contrary to the idea of Max Weber, it is not the Protestantism that created the class of merchants and entrepreneurs in England; in reality, the British businessmen adopted and adapted the religion that teaches that the success in enterprising is the sign of election by God, and that condemns the King's intervention in these matters. Linking the present to the past, Neurath's historical functionalism avoids the pitfalls of both organicist conceptions of history and of historicism.

For Neurath, scientific knowledge takes place against the background of common knowledge; it is its systematical conceptual elaboration. On this point, in opposition to the positivist and neopositivist stress on continuity, Gaston Bachelard and Georges Canguilhem both emphasize the phenomena of rupture and oppose to positivist and neopositivist stress on continuity the sudden systematical reorganization of scientific theories, which makes the ontological import and the normative character of scientific knowledge conspicuous. Neurath would probably not object to the discontinuist picture of the scientific revolutions, but he always insists on the necessity of using more or less the same language for the needs of communication. Even a revolutionary scientist like Gallileo must speak a language apt to be understood by his colleagues if he wants to persuade them. Moreover, even in the case of the twentieth-century science, a common if perhaps only fragmentary language must be found for the communication between scientists, designers of instruments and heavy equipment, and the laboratory technicians who use them. In any case, Neurath's continuism is a bit shaky, as he reminds us the constant use of *quaternio terminorum* in the development of scientific theories. Old words acquire a slightly different meaning and new words cannot but be explained by means of old ones. It is as if a continuous series of slight misunderstandings connected old languages and theories to the new ones.

Is there a place for ontological questions in Neurath's philosophy? Which are the objects of science?

For Neurath, a theory is nothing more than an instrument for systematically producing predictions, a prognostication machine. Does it mean that Neurath's epistemology is purely operational, pragmatic, without ontological import?

His nominalism, as well as his concern about coherence, should lead to such a conclusion, as well as his concern about coherence. The question of the alleged "reality" to which refer scientific statements is meaningless, *sinnlos*; it is a typical metaphysical question to which no answer can be given. There are no points of contact between a theory and "reality" external to the theory; no points of anchoring our theories that could smoothly develop until their completion in the total system of knowledge. The only warrant of our predictions which remains is the coherence of our statements. Neurath draws the conclusion from his refusal of all metaphysics and ontology: the truth of a scientific theory can be measured only by itself. One thinks on Spinoza's dictum taken literally: *veritas est index sui et falsi.*

The result of all this is Neurath's thesis about the scientific progress in its totality, concerning the past, the present as well as the future of science: the theories that follow one another do not converge towards an ultimate theory as a kind of that would be their limit. Even if there is continuity in their succession, one cannot say that they approach more and more precisely the "true" theory, contrary to what Peirce had thought. Science is an adventure of the human species; the phantom of "reality" or of the "world" cannot support it.

Nevertheless, in the *Anti-Spengler*, Neurath rejects the view held by Mach, according to which the theoretical parts of science are mere auxiliary constructions whose only purpose is just to classify and to connect the data in order to predict future data. The relations between concepts are not only the "means for grasping" the data; they constitute also a "knowledge in itself". Physical theories are theories structured by the mathematics and behind mathematics, there is logic and "even a God 'would see' the logical connections" by intuition.[11]

4.2 The Theory of Truth and the Controversy About Protocol Statements

Scientific knowledge, being by its essence systematic, rules out the existence of *isolated* statements. This is perhaps the basis of Neurath's holism. For him, to set out a scientific statement means tacitly to use countless other statements as more or less remote premises. As Duhem taught, the fate of a theory never depends on the approval or rejection of a single statement. How can we then explain innovation, the discovery of a new phenomenon, of a new property? How these can be integrated into actual theories?

One of the consequences of Neurath's *Ganzheitsdenken* is the possibility of subjecting to submit *any* scientific statements to revision. They all defend themselves jointly against the intruder or the troublemaker, because no statement is shielded against the revision: neither the fundamental theoretical principles, nor scientific laws, nor the observational protocol statements. Descartes erred precisely in believing that there is a fixed line that separates the field of theoretical knowledge, the field of certainty, from the domain of practical action governed by rules that are only provisional. According to Neurath, even in a scientific theory, we are forced to work with premises that are as dubious as the premises of Cartesian morals. In science, we ought to follow the behavior of wanderers of Descartes lost in a forest: we are obliged to take decisions even if our reasons are not sufficient to determine our action. What is even more important, we must not delude ourselves into thinking that a decision is an inference, as it happens with the so-called rules of induction. Such rules have only local and limited validity: the essence of induction consists precisely in a *decision*. As we see, Neurath is strongly opposed to all foundationalist enterprises, even to that of Carnap's. In this sense, the logician Quine is closer to Neurath than to Carnap.

The possibility of revising protocol sentences is closely connected with Neurath's doctrine of truth. First, Neurath puts forward pertinent arguments against the traditional theory of truth as a correspondence with reality. Its weak point consists in the impossibility of finding the second term of this correspondence. To what sort of entity is a true sentence supposed to correspond? To what kind of entity can correspond a true sentence? To things, facts, states of affairs, parts of reality or even to reality as a whole? If we try to determine precisely this second term theory, we always arrive at a paraphrase of the sentence itself, which is just another sentence. Neurath and Schlick agree with Wittgenstein's maxim according to which we cannot step out of the language. For Neurath, the words like "reality", "thing", "fact" and similar do not mean anything; they are just "meaningless reduplications", i.e. they are metaphysical terms. When Wittgenstein and Schlick try to replace the correspondence between a sentence and a fact by the correspondence between the totality of language and the "world" or the totality of experience, they again are immersed in metaphysics. A sentence can only be confronted with another sentence, with nothing else.

Because all statements of a science are interdependent, a new statement must be confronted with all of them. If it is compatible with other statements of the system, it is accepted and declared by definition as true. If it is not compatible with them, we can either declare it false, i.e. reject it, or modify the whole system in order to make it compatible with the new statements. We can also put off our decision for further scrutiny. Thus, truth and falsity consist in the integration of a statement into science or its rejection, and Neurath adds that "in science, there could not be another 'concept of truth.' "[12] Not all the members of the Vienna Circle did agree with such a radical conception, one that seemed to undermine the empiricist basis of knowledge. Without entering into the details of the controversy between Neurath and Schlick, we just recall their respective lines of argumentation of both of them. The divide consists in the possibility of founding a science on an indubitable empirical basis. Schlick upholds a version of the theory of truth-correspondence, but agrees with Neurath that all statements of sciences, even the protocol sentences are hypothetical and revisable. But then what is the role of experience, how can we avoid relativism if we can conceive several consistent, but mutually incompatible systems based on the same protocol statements and nevertheless mutually incompatible?

For Schlick, behind the protocol statements are what he calls *Konstatierungen*, i.e. affirmations of immediate experience like "pain here now pain" or "here now coincidence of two strokes". Such affirmations are on the limit of the language, not really sentences; they are sudden illuminations, properly speaking inexpressible, and nevertheless, they are absolutely certain and yield the foundation of all knowledge.

One understands Neurath's repugnance for such a solution. To try to found knowledge on the individual experience, which cannot be adequately expressed in a language, means to open the door to all sorts of metaphysics. Science needs authentic observational sentences as a starting point of its conceptual constructions; it cannot work with the fleeting impressions like "red here now".

One might have expected the Viennese empiricists to look at scientists at work in their laboratories in order to find out how they state their protocols and how they work with them. Instead, both Schlick and Neurath prescribe to the scientists how they should formulate the protocol sentences. Schlick imposes a foundational role on protocols while Neurath and Carnap prescribe their form and even the particular syntax for their expression. For Neurath, protocols must contain an indication of space and time of an event and the name (or description) of the author of the protocol. Moreover, a system of parentheses allows us to accept the testimony of an author and at the same time to reject his interpretation. Due to the imbedded statements, we may for instance accept as a protocol the following as a protocol: "In the sixteenth century, people saw fiery swords in the sky".[13]

If the complexity of protocol sentences leaves the door open to error, the presence of perceptual terms guarantees their stability. That is why they can take over the function of control. Nevertheless, as regards certainty, they do not enjoy any special privilege in comparison with regard to other statements of the theory. Given their complex syntax, they cannot even be called elementary or basic sentences. They are "impure" because they contain verbal clusters, which cannot be analyzed in the terms of the theory (such is e.g. the verbal cluster "the dynamo in a certain laboratory"). We accept, for example, the statement "there is an elephant here" if it can be integrated into the class of statements already accepted, be they protocol or not. If this statement contradicts them, we either reject it or else it will be rejected, or we modify the class of already accepted statements. "No statement knows *Noli me tangere*."[14] There is no supreme instance of verification or of rejection; no supreme judge, and even if we accept a judge for a time or occasion, he is removable.

In fact, each of the protagonists of the controversy introduced some of his opponent's ideas into his own reasoning. If Neurath's empiricism risks to loosing experience in favor of coherence and systematicity, and Schlick's, risks loosing communicability in favor of evidence, one must not forget that for Neurath, each new statement has to be confronted with protocols which provide the empirical content to the theory, and that for Schlick, coherence is sufficient to define truth of the statements of sciences, because they take their empirical content from the *Konstatierungen*, i.e. from experience. Against Schlick, Russell and Ayer, Neurath himself stresses that his theory should not be identified with the theory of coherence, which he claims almost no one holds. It is, instead, rather a theory of the *acceptability* of statements. Should we believe Hempel saying when he says that Neurath confuses both and merges them into one? I prefer to say that Neurath replaces a *semantic* theory of truth, which he did not like even in Tarski's formalized version, with an *epistemic* theory of the acceptability of statements. Throughout his life, Neurath has remained faithful to this conception of truth, which corresponds to the "syntactical" period of the Vienna Circle.

4.3 Physicalism

Even if Neurath's holism was toned down during the first period of the Vienna Circle – that of Carnap's *Aufbau* (*The Logical Construction of the World*) and of the *Manifesto* of the Vienna Circle – it manifested itself in nourished critical remarks against the subjective constructivist variety of empiricism. Against the *Aufbau*, Neurath stresses the impossibility of separating the "pure" and "impure" ways of thinking. He also reminds us of the polysemy of theoretical statements and their underdetermination by observation. In such cases, their choice and interpretation depends on particular ideologies linked to historical conditions and to the interests of social classes (it is a piquant paradox that the disciples of Althusser largely used this argument of Neurath who himself had adapted an argument of Duhem). Further, Neurath questions the existence and unity of an ultimate basis composed of elementary experiences. Eventually, he examines the principal difficulty of the *Aufbau*, namely the existence of two fundamentally different languages: on one hand the private phenomenal language of personal experiences, on the other hand the public intersubjective physicalist language. Personal experiences cannot be communicated; one can speak only about structures. Nevertheless, it is from such personal experiences that Carnap tried to reconstruct the world. According to Neurath, Carnap breaks the unity of the world. A gulf divides the pretheoretical world of human experience, called the "natural world" by Avenarius and "lifeworld" (*Lebenswelt*) by Husserl, from the world of science constructed on the basis of Carnap's *Aufbau*.

In this conflict, in spite of everything that separates them, Neurath is on the side of Husserl and Patočka: the world of human experience cannot be sacrificed to the advantage of logical constructions which are rooted in it. The difference between Neurath and the phenomenologists consists in the way in which science *extends* the "natural world" (Neurath) or is constructed *in opposition* to it (phenomenology).

All human experience is impregnated with language and Neurath's physicalist language takes its starting point in ordinary language. The latter is peppered with impurities; it contains verbal clusters, which no analysis can dissolve. Ordinary or "trivial" language sometimes uses even suspect and metaphysical terms, but it contains the basic matter of all sciences. All men speak and understand the trivial language and "almost all – even the Kantians and the phenomenologists – have one part of this language in common with us and other simple people".[15]

The language of science is an extension of this trivial language, enriched and purified. An extension, not a break with it:

> We logical empiricists want to show people that what physicists and astronomers do is only on a grand scale what Charles and Jane are doing every day in the garden and the kitchen.[16]

In fact, Carnap already mentions the possibility of a physicalist language in his *Aufbau* when he considers a physicalist basis as an alternative to the autopsychical basis built up from elementary experiences. But contrary to Carnap, Neurath does not look for a basis to reconstruct the world; he wants to unify all sciences by means of a common language. The goal of his physicalism is to yield a language for the unified science. This is also why he is indifferent concerning the nature of the elements; the important thing is that they can be described in terms of space and time.

One can also apply Neurath's the term "cluster" to his physicalism. The word "cluster" covers a conglomerate of theses which Neurath does not always clearly separate. At least two meanings of physicalism should be distinguished: the methodological and the ontological. Neurath favors the first one. For him, physicalism is not a doctrine that explains the ultimate nature of objects – that would be metaphysics. It is a *rule* saying that all objects and natural processes, mental and social processes included, should be described only in terms of space and time. In his sense, Neurath's physicalism plays a role analogous to that played by the mechanist and materialist thesis in the eighteenth and nineteenth centuries.

Let us dispel some possible misunderstandings. Physicalism does not advocate the reducibility of all sciences to physics It commends the reducibility of all *objects* of the sciences to objects and processes situated in space and/or time, more exactly, the reducibility of the *terminology* of all sciences to the spatio-temporal terminology shaped according to the terminology of physics. Only the *terms* of a science can be reduced to physicalist terms; on the other hand, each science contains its own statements and laws. It is therefore impossible to identify the laws of biology and even less those of sociology with the laws of physics. The only thing to do is to deal with human society in the same way as we deal with a star cluster, namely as a certain spatio-temporal structure.

Why physicalism? Due to the unification of terminology, the physicalist language allows to connect the statements of all sciences and to combine, if necessary, the statements of several disciplines in order to predict complex phenomena. Neurath puts forward the example of forest fire: in order to predict its evolution, we must combine the statements of geography, meteorology and of botany. If we want to predict the behavior of an exotic tribe facing the fire, we also must resort to statements of sociology and anthropology. In the making predictions, we must be able to combine the different statements; that is why they must use the same universal physicalist language.

How to treat mental phenomena, for example perception? Perceptual terms should be used only in physicalist sense. A statement like "I see something blue" can belong to three categories: it is a statement about reality (better, to avoid the metaphysical, "isolated" term "reality", a "statement-about-reality") if some modifications of the brain state correspond to the spatio-temporal modifications outside the perceiving person. It is a hallucinatory statement if the observed modifications concern only the body. Finally, it is a lie if the modifications take place only in the language center of the brain. Can we then say "I see the same red as my friend"? Here, we do not compare different perceptive states, but only the *use* of the term "red". "It is the structure of the expressions of my friend that instructs me how he combines the symbol 'red' with other symbols. Science cannot do more."[17] Let's us raise an objection: should not a physicalist rather compare two *physical* states? A latent conflict comes up to light between two ways of treating perception and in general the contents of consciousness: on one hand by physics, on the other hand by grammar.

In any case, according to the physicalist thesis, the psychological terminology using the words like "consciousness" must be replaced by the description of neuro-physiological processes and observable events, gestures and speech acts. For Neurath, consciousness is rejected along with the soul of traditional metaphysics. All perceptual terms are submitted to a transformation similar to that which led to the creation of geometrical terms: the statements about the order of visual or tactile terms were replaced by statements containing only the geometrical terms like "cube" or "sphere". Similarly, the statements containing the perceptual terms ("hard", "blue", "clear", etc.) have to be replaced by the statements about certain periodical oscillations or other physical phenomena. We can nevertheless accept their use in the ordinary language and even use the old terms in the new purified sense.

Physicalism is the heir of materialism; it is "the modern form of materialism."[18] Nevertheless, the terms like "spirit" or "matter" are inadequate, linked to metaphysics, and should be avoided.

> The question of 'spirit' or 'matter' is resolved by the disappearance of the doctrine of spirit; what remains is only the doctrine of the 'matter', i.e. physics. What is given as the science of reality cannot be other thing than physics.[19]

Physics in the large broad sense becomes an all-embracing discipline, a transdiscipline whose terminology claims universality. It is not the least paradox that at the very moment when physicists begin to question the possibility of a unitary language of physics, when mutual incomprehension arose between macrophysics and microphysics, Neurath puts his money on the language of physics as a unitary language of *all* sciences and at the same time links his physicalist language to the latest state of physics: "It is essential that the concepts of the unified science share all the time the destiny of the fundamental concepts of physics in the cases where we look at its last subtleties as well as where the description remains only approximate."[20]

4.4 Elimination of Metaphysics?

Physicalism is the positive side of a doctrine whose negative side is expressed by the slogan *elimination of metaphysics*, which contributed to the bad reputation of the Vienna Circle among traditional philosophers. Two major reasons guided the Viennese in their opposition to metaphysics: its historical and social role and the absence of cognitive content.

In the first place, Neurath fights metaphysics under the banner of Enlightenment. Heir of theology, metaphysics was on the side of obscurantist and oppressive forces. The Marxist militant Neurath follows the line of Voltaire and Diderot. By contrast, logical empiricism can never become an instrument of oppression. Cognitive reasons are equally compelling. The parallel and complementary doctrines of Wittgenstein, Schlick, Carnap, Neurath, Hahn, but also of the poet Paul Valéry coincide in this point: metaphysical statements are meaningless, they are but pseudo-statements, sequences of words that do not designate anything and do not mean anything. It is impossible to connect them with an object, an observation, a phenomenon or an act. They can have an emotional and perhaps a poetical effect, but no cognitive value. Nevertheless, as Eckehard Köhler has reminded, the Viennese have always revised or toned down their radical critique of metaphysics "when it became clear that actual practice of science required a tolerant attitude. The Vienna Circle did not share the limitations of the set theory, of logic and of classical mathematics by effective constructive processes imposed by Brower; neither did it share Mach's rejection of atomism, nor Watson's ban of psychological concepts like introspection or unconsciousness."[21].

An attentive ear can discern other tones beneath the call of the trumpets in favor of the antimetaphysical crusade. As Carnap pointed out in a note joint to the English translation of the *Overcoming of metaphysics by the logical analysis of language*, the targets of the Vienna Circle in the 1930 ties are principally the speculative doctrines of Fichte, Schelling, Hegel, Bergson and Heidegger, but not the effort aiming at a synthesis of different scientific disciplines. Neurath's opinion is even more qualified:

> One understands very well that a Frenchman begins to be astonished when he hears the hard talk of the Viennese school that wants to stand aloof from 'philosophers'; maybe he thinks of Descartes and Comte while others think of Fichte and Heidegger.[22]

Neurath has always recognized the merits of the metaphysics, at least of a certain metaphysics, for the establishment of modern science. Scholasticism and the Talmud favored methodical and scientific thinking. The difference in intellectual climate between Austria and Germany was important here, as Neurath explained in several articles. German philosophy is permeated with Protestant metaphysics that is almost impossible to eliminate, while the Austrian Catholic and scholastic metaphysics can easily be separated from statements about reality and eliminated. Due to Bolzano and Brentano, Austrian philosophy was able to avoid the "Kantian interlude" and to practice a philosophy in close collaboration with the sciences. The Vienna Circle has received and further pursued this Austrian heritage. Finally, Neurath is more severe with Wittgenstein's metaphysics of the "unsayable" – where meaningless statements play a preliminary elucidatory role but shall be eventually rejected, – than with Catholic or Jewish scholasticism. Under the heading "pseudorationalism", he even denounces the remnants of metaphysics in Popper and in

Carnap. He sensed the close relationship between the logical empiricism and the scholasticism and warned Carnap against the danger of the a new scholasticism.

What the metaphysicians desire and hope, says Neurath, is "to find a methodical way by which one can learn more than by the scientific means of empiricism."[23] They want to short-circuit science, to arrive at truth without having gone through the hard apprenticeship and without participating to in the patient work of science. There is a criterion of successful knowledge: the ability to predict. Therefore, if metaphysics delivers better predictions than ordinary science, "the advocates of logical empiricism should immediately begin to study this method."[24]

How are we to eliminate metaphysics from "infected" statements? According to the account of Marie Neurath in a personal conversation with Antonia Soulez and me in 1983, the severe discipline of Neurath's friend Gregorius Itelson who became friend of Neurath inspired his extreme mistrust regarding metaphysical terms. Perhaps it was Itelson who first had the idea of compiling an *Index verborum prohibitorum* containing suspect and vague terms. But Neurath's Index is not the *Decalogue*. Paul Valéry put together a similar Index; in both cases, it is intended principally for personal use (Neurath's behavior during the sessions of the Vienna Circle nevertheless shows that his Index was also intended for his colleagues). In principle, neither Neurath nor Valéry formulate interdiction; they content themselves to renounce to the use of certain terms.

The content of Neurath's Index varies slightly with the time. Belonging to it are such words as "reality", "transcendent", "existence", "me", "concept", "thing", "fact", "reality" and of course the terms of traditional metaphysics such as "soul", "substance", "essence" and many others. In actual practice, Neurath is nevertheless less dogmatic than in his theory and often uses suspect terms, trying to give them a precise meaning.

Progressively, other arguments, more in agreement with Neurath's holist orientation, support his instinctive repudiation of metaphysics. During the first period of the Vienna Circle, metaphysical statements were declared meaningless because it was impossible to reduce them to elementary personal experiences. They remain meaningless even after the abandonment of Carnap's methodical solipsism, but for other reasons. They cannot be integrated into science, because they have no links to protocol statements and they do not contribute to the formulation of predictions. They do not contain terms connected with the concepts and statements of science. They are epistemologically inert, thence superfluous. For these reasons, Neurath calls them, borrowing the word from Reach, isolated statements. *Metaphysical statements are isolated statements*.

The change of their status indicates a re-evaluation of the role of metaphysics. Imperceptibly, for those who are able to discern the harmonic tones of Neurath's discourse, other feelings filter through the invectives against metaphysics. The imposing Hegelian deductions, do they not facilitate the description of social changes on a large scale?"[25] The systems of great metaphysicians exert a great fascination even today. The Kantian architecture of categories, the successive development of the points of view on history in Hegel, the whole-embracing system of Schelling contain panoramic visions of nature and history that one cannot find in the materialists like Helvétius. The writings of Hegel are full of empirical details and his power of synthesis inspired Feuerbach, Marx, and Engels. Psychoanalysis and other disciplines of the "modern behavioristic" owe many insights to Schelling and Nietzsche. In Germany, it was Schelling and not the physicists who first reported on the discoveries of Faraday. Thus, at least in the past, metaphysics took on a function that no particular science can fulfill: the function of synthesizing human knowledge. Speculative intuition often anticipated scientific discovery and "there were metaphysicians of the highest standing who attempted imposing syntheses, with long-lasting and produced effects that lasted a long time".[26]

How, then, can Neurath hold the metaphysical statements to be isolated? He eventually realized that we are not able to decide "which statements should be considered as metaphysical" because we do not posses a "magic riddle that can filter and so to speak automatically eliminate the metaphysical components from science."[27] Isolated statements that we are not able to integrate today into science can 1 day reveal themselves as useful and fruitful.

In fact, the notion of an isolated statement runs counter to Neurath's holism and to his remarks on the synthetic function of metaphysics. He eventually realized that the metaphysics against which he declared the crusade is embedded in our language, that it belongs to our tradition and to our science, the unified science included. After the paradox of physicalism, we face the Neurathian paradox of metaphysics: made up of so-called isolated statements, it expresses in fact a huge effort of integration and synthesis of all our knowledge. But was not this paradox already in the cards as the ultimate consequence of his holism?

4.5 Unified Science and Encyclopedia

Physicalism finds its fulfillment in the unified science, which is "the most general physics, a web of laws that express the spatio-temporal connections".[28] From his first articles, the idea is present in Neurath's works: the science forms a unity; it is therefore necessary to formulate "what is common to all sciences".[29] This goal can only be obtained by a the collective work of scientists of different fields. We should arrive at a global description of the world, which would not be a simple juxtaposition of special sciences.

The unified science therefore assumes a function that was formerly incumbent on philosophy: the function of synthesis of knowledge. But once philosophy itself is eliminated, no philosophical statements remain and the task that Schlick and Wittgenstein prescribe to philosophy, namely the clarification of concepts, "must not be separated from scientific work".[30] Unified science is science without *Weltanschauung*; it does not propose a new doctrine, new dogmas, it must not become a new deity. The philosopher becomes the organizer of unified science and social engineer.

For Neurath, science always is a system of statements aiming at predictions. Now, prediction means control, arguments and action. These three modalities break up the old frontiers of disciplines. The unified science has to overcome their compartmentalization, the irreducible specificity of their objects, methods, and languages. Due to physicalism, Neurath is able to abolish the division between the exact sciences (*Naturwisenschaften*) and the sciences of mind (*Geisteswissenschaften*). For him, "it is essential that the basis of all laws consists in a kind of order, be it the laws of geology, of chemistry or of sociology".[31] In this sense, Neurath's unified science paves the way for the interdisciplinary research so characteristic of the science of the last 50 years.

He quotes geology as an example of interdisciplinary naïveté, which should inspire sociology. "If we consult a book of geology, we notice that somebody who works in the sciences which deal with the real world ingenuously collects all sorts of knowledge necessary to deduce certain processes."[32] The sociologist should do the same: in his work he ought to use laws of different disciplines, from meteorology and linguistics to biology and chemistry. And Neurath recalls that the goal of unified science does not consist in the establishment of general laws valid for all disciplines, but in developing a unitary physicalist language that makes possible the interchange and interrelations between the disciplines.

In fact, the scientific language is an inextricable mixture of the ordinary and the physicalist language. According to Neurath, it is a slang or rather a *universal jargon*, the daily language of the artisans of the unified science. "Our modern folklore called scientific language" strongly contrasts with the pedantic linguistic constructions of Carnap built up from their initial stock of statements by means of formal rules. For Neurath, such languages, useful in their proper field, have only a limited and local value. To mark its the scattered, synthetic and incomplete character, Neurath finds another name for the "jargon" of sciences: it is a *lingua franca*, a composite language which combines elements of unified science of varied provenance.

The unified science written in the physicalist language finds its concrete expression in the *Encyclopedia*. Neurath liked all kinds of encyclopedias, in particular those accompanied by drawings and illustrations, such as Comenius' *Orbis Pictus* or Diderot's and d'Alembert's *Grande Encyclopédie*. From Comenius and Leibniz, he took the idea of universal science; from Marx, the conception of knowledge as a historical process and the idea of social engineering; finally, Neurath's Encyclopedia reminds recalls Bolzano's theory of science which teaches how to write an encyclopedic series of scientific treatises.

How to reconcile the unified science with Neurath's hostility towards the a single system of knowledge? "*The system is a great scientific lie*".[33] The true model of the totality of knowledge is not a system, but the encyclopedia. Contrary to a system, the encyclopedia has no fixed stock of initial statements; it is obliged to work with verbal clusters, which cannot be strictly analyzed or defined. With the progress of science, its observational basis changes; moreover, the words change their meaning when they enter into new configurations. Strictly speaking, it is not even possible to speak of *the* physicalist language and of *the* language of the unified science. "We could start with different unified languages that could not be simply translated one into another."[34] What is given is always a plurality: a plurality of objects, of languages, of interpretations. Neurath notes that even the statements put at the beginning of a book and those put at its end often belong to slightly different languages. Polysemy and indetermination belong to the essence of language.

Reflecting on these different languages that do not always admit a translation, Neurath introduces a new problem in philosophy, namely that of *translation*. He thus transforms a question that was only of interest of the linguists and the professional translators and interpreters into a problem concerning the expressive power of different languages. The mutual translatability of languages containing a high proportion of empirical statements becomes one of the main arguments in favor of empiricism. "This translatability is the basis of our scientific undertakings,"[35] because the only difficulty of a translation is linked to the fact that the reference of the terms belonging to different languages does not exactly coincide. Once again, Neurath illustrates the situation of communication by the wreck of his ship. The survivors found refuge on an unknown island and are obliged to learn how to understand the natives if they want to survive. They will have no difficulty of communicating by gestures, images and by words that can be translated from one language into another, when they will speak of fish, trees, of eating and drinking, and of pleasure and pain. But some statements will resist to all tentatives attempts at translation: those that contain the terms like "law", "soul", "cause", etc. Heidegger cannot be translated into the Bantu language. Neurath then formulates another criterion of metaphysical statements: they are statements which are impossible to translate, they are enclosed in the limits of one language. Again, one could say in a slightly different sense that they are isolated statements, isolated within the frontiers of their own language.

Although the Encyclopedia is written in the unified language, it is pluralist. It is the expression of pluralism, because instead of expounding *the* system of knowledge, it sets out partial and local systematizations, sometimes even in competition with another. A large place will be reserved for the visual communication: for images, reproductions, schemes, figures and the pictorial display of statistics in image (*isotype*, invented by Neurath). Neurath's pluralistic encyclopedism then becomes perspectivist and polysemic. Following James, Neurath replaces the *universum* by a *pluri-versum* and imagines the multiplicity of possible descriptions of the same object, person or event: a *pluri-moon* and a *pluri-table*, and even a *Pluri-Newton* and a *Pluri-Cromwell*, and a *Pluri-Cromwell* of a certain day. His Encyclopedia is a Pluri-Encyclopedia.

The pluralism of the unified science may threaten the unity of knowledge. As we have seen, even a simple verification of an elementary theorem of physics is affected by "our cosmic aggregation". Such an aggregation is an undetermined mixture containing everything: stars, plants, animals, men and women, the atmosphere, clocks... All our laws, those of mechanics but also those of biology and sociology, are in reality the laws of our cosmic aggregation. The same thing happens in chemistry, geology and history: "each quantity is in its way an element of a cosmic aggregation" because each statement, each event "implies the connection of a certain quantity with the cosmic aggregation to which it belongs".[36] Such a conglomerate of things, also called *synousia* by Neurath, exhibits a sort of internal cohesion and stability, and is governed by surprisingly simple laws. Because all statements of science are historical statements, the ultimate goal of the unified science is to set up a vast history of the cosmos. “The language of our Encyclopedia can be conceived as a typical language of history”.[37]

A unified science is nevertheless multiple. Its language remains unified: it is the language of a historical and cosmic physicalism, but the organization of knowledge proceeds by partial, sometimes even contradictory systematizations, by the choice of hypotheses that cannot be simply deduced from observational and experimental data. “I always supported the monism as a means of empirical communication, and the pluralism as an attitude when conceiving hypotheses”.[38]

Neurath’s work was dispersed in innumerable articles and small booklets and only the recent edition of his writings has revealed all its richness. Others have taken and developed some of his ideas, first of all Carnap who owes to Neurath the idea of physicalism and (along with both to Neurath and Wittgenstein), his interest in the syntax. Quine, too, learned as much from Neurath as he had from Carnap. Even if the *Encyclopedia of Unified Science* did not induce the deep change in the new organization of sciences, other Neurathian themes enjoy a new life in our days: the sociology of science or, according to his expression, the “social behavioristic” of scholars, as well as many ideas of the evolutionary and anarchist epistemology: naturalism and integration of the human thought into the evolution of life and of the cosmos, links of scientific theories with the social context and the ideology of a given historical period, the pluralism of methods and even the rehabilitation and recourse to “impure” ways of thinking and revival of abandoned theories. Physicalism remains a possible direction of research and the idea of unified science, confronted with scattered state actual dispersion of knowledge, has not lost its appeal. In spite of the many points of opposition between all what can oppose the thinkers like Gaston Bachelard and Neurath, or the late Wittgenstein and Neurath, I find a similar freedom of thinking off the beaten track in all of them.

Notes

1. Cf. Sebestik (1999). I am greatly indebted to Paul Rusnock for his help in the revision of my text.
2. Neurath (1981, p. 39). s.
3. Ibid.
4. Neurath (1981, p. 484).
5. Neurath (1981, p. 198).
6. Neurath (1981, p. 197).
7. Neurath (1981, p. 947).
8. Neurath (1981, p. 500).
9. Neurath (1983, pp. 180–181). Philosophers (Kant) and scientists often used navigation metaphors. Maxwell thus illustrates the fact that our knowledge of time and space is essentially relative: “There are no landmarks in space [. . .] We are, as it were, on an unruffled sea, without stars, compass, soundings, wind, or tide, and cannot tell in what direction we are going.” Maxwell (1991, p. 81).
10. Neurath (1983, p. 127).
11. Neurath (1981, p. 184).

12. Neurath (1981, p. 419).
13. Neurath (1983, p. 129).
14. Neurath (1981, p. 581).
15. Neurath (1983, 1008).
16. Neurath (1983, p. 1007).
17. Neurath (1981, p. 415).
18. Neurath (1981, p. 466).
19. Ibid.
20. Neurath (1981, p. 537).
21. Köhler (1985, p. 190).
22. Neurath (1981, p. 743).
23. Neurath (1981, p. 841).
24. Neurath (1981, p. 841).
25. Neurath (1981, p. 451).
26. Neurath (1981, p. 729).
27. Neurath (1981, p. 746).
28. Neurath (1981, p. 415).
29. Neurath (1981, p. 45).
30. Neurath (1981, p. 534).
31. Neurath (1981, p. 419).
32. Neurath (1981, p. 473).
33. Neurath (1981, p. 626).
34. Neurath (1981, p. 627).
35. Neurath (1981, p. 932).
36. Neurath (1981, p. 999).
37. Neurath (1981, p. 935).
38. Neurath (1981, p. 1011).

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Chapter 5
Neurath and the Encyclopaedic Project of Unity of Science

Olga Pombo

5.1 Encyclopaedia as Metaphor

Neurath says repeatedly that Encyclopaedia is the model of unity of science, "the model of men's knowledge (. . .), the genuine model of science as a whole" (Neurath 1938a, p. 20), "the soil in which science lives" (Neurath 1936, p. 201), "the symbol of the developed scientific cooperation and unity of science" (ibid), an image of "our knowledge taken in it totality" (Neurath 1936, p. 199).

In a first sense, encyclopaedia is thus for Neurath a *metaphor* of unity of science. A metaphor like many others Neurath likes to use: mosaic, orchestration, sailor, onion. Something which is surprising in an author like Neurath who looks for univocal designation. We know that a metaphor is an expression which, behind its common sense, is able to analogically designate a plurality of other meanings. How to understand its importance for an author like Neurath who is a reformer of language, someone who even proposes an *index verborum* in order to inhibit and even to forbid the use of dangerous words. Of course we know that by dangerous words Neurath meant just those words which avoid communication. And that is not the case of metaphor which, on the contrary, is a strong communicative linguistic procedure able to make us *see* what the concept means, in this case, the concept of unity of science. Almost an *Isotype*, a pictorial sign, a hieroglyphic which condenses dense information and exhibits it through its spatial form. We must remember that Neurath is also a constructor of a new language endowed with common intelligibility and great communicative capacity.

In this respect, it is interesting to make two observations:

1. Unity of science always gave rise to strong metaphors. That is the case of the *circle*. From Antiquity (from Cicero' s circle of liberal arts to Martianus Capella's *Disciplinae Ciclicae*) until Hegel, Adler or Piaget, the circle has been the metaphor of eternity, divine perfection, stability, systematicity, immobility,

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J. Symons et al. (eds.), *Otto Neurath and the Unity of Science*, Logic, Epistemology, and the Unity of Science 18, DOI 10.1007/978-94-007-0143-4_5,

closeness, no beginning, no end, no hierarchy, a metaphor by which unity is theologically thought out. That is also the case of the *tree* which, from Rámon Lull to Bacon and Descartes represented a dynamic form, a living being, an organic development, historicity, generation, multiplicity, vertical subdivision, mostly dichotomical. Unity is there thought out as hierarchical. That is also the case of Diderot and D'Alembert's metaphor of the *Mapamundis* by which unity of science is thought as a topographical, topological, cartographic, territorial, juxtaposition. Unity is the horizontal result of the complementary work of different sciences, of its ordered evolution and cumulativity, or yet Leibniz's metaphor of the *Oceanus* as permanent connection, fluidity. That is also the case of Couvier, Comte, Oppenheimer's metaphor of the *House* by which unity of science is thought out as architectonic, previously planned assemblage of elements, growing by savage proliferation.[1]

2. Neurath never uses any of the metaphors proposed by his antecessors. He, always so devoted to praise the achievements of the past, he proposes a new metaphor – encyclopaedia – a word which, in itself, is already a metaphor coming from the Greek *eu kuklios paideia*, the perfect circle or complete course of knowledge and education.

What does it mean that metaphor? That is what we will try to understand. However, before that, let us just remember that encyclopaedia is not a metaphor as many others Neurath uses to mean unity of science. Encyclopaedia is a Neurath's *idea* in origin,[2] a *project* first conceived by Neurath and in which he works since 1920 up until the end of his life, a project afterwards proposed, discussed and approved in the First International Congress for the Unity of Science in 1935 but of which Neurath seems to have first talked with Einstein and Hans Hann and only afterwards with Carnap and Philipe Frank; it is an *attitude*, an open, co-operative and anti-dogmatic "attitude" (cf. Neurath 1937a, p. 141); it is "*plataform* which makes it possible to find out how much cooperation is in fact possible" (Neurath 1937a, p. 137); it is the "*simbol* of a developed scientific co-operation, of the unity of sciences and of the fraternity among the new encyclopaedits" (Neurath 1936, p. 201); it is a *program's life* for "men of good will" (Neurath 1936, p. 200). As Neurath says, "it is the very practice of life which imposes encyclopaedic task" (Neurath 1936, p. 199). Encyclopaedia is also a *movement*, a *cooperative endeavor* by which Neurath tried to bring "together scientists in different fields and in different countries, as well as persons who have some interest in science or hope that science will help to ameliorate personal and social life" (Neurath 1938a, p. 1). Practical thinker par excellence, Neurath was aware of the need of overcoming "dreams by acts" (Neurath 1936, p. 200), of giving institutional form to theoretical ideas. With all his energy and perseverance, he dedicated himself to the social engineering of science organizer, building the necessary instruments – congresses, institutes, museums, series of books, associations, educational institutions, journals and – of course – that powerful mechanism of unification of knowledge which is encyclopaedia.

So, encyclopaedia is in Neurath not only a *metaphor* but also a *realization*, a concrete, *material work* which, even if far from the first ambitious plan, he succeeded to put forward, overcoming a set of big difficulties.

A work which Neurath puts in the line of a long history of similar endeavors: the cosmic poems of antiquity, the *Sumae* of Thomas Aquinas, the *Ars Magna* and *Ars Generali* (1308) of Ramon Lull, the *Instauratio Magna* (1620) of Bacon, Comenius's *Pansophia*, *De Rerum Humanorum Emendatione Consultatio Catholica* (1642–1670), Leibniz's *Encyclopaedia sive Sciencia Universalis and Atlas Universali*s, Hegel's *Encyclopädie der Philosophischen Wissenschaften* (1817), Schelling's *Naturphilosophie*, A. Comte's *Cours de Philosophie Positive* (1830–1842), Spencer's *Synthetic Philosophy* and, of course, the French *Encyclopédie* of Diderot and d'Alembert (1751–1765).

Three remarks must be made.

First, Neurath only refers philosophical encyclopaedias. He could have mentioned the *Encyclopaedia Britannica* (1st edition 1768–1771), the *Encyclopaedia Metropolitane* by Coleridge (1817–1845), *Le Grand Dictionnaire Universel de* Larousse (1866–1890) or the monumental *Allgemeine Encyclopädie der Wissenschaft* (1818–1889) of Ersch and Grüber (167 volumes). But he did not. And he did not because he knew that the aim of a philosophical encyclopaedia is less to give an exhaustive exposition of the totality of knowledge (empirical, scientific and technical) than to stress the articulation, the integration, the connections, the universal relations of the various kinds of knowledge. In a word, to make us remember that knowledge has a unity.

Secondly, the list of philosophic encyclopedias which Neurath gives is quite exhaustive. It includes inclusively some attempts previous to the seventeenth century, the time in which encyclopaedia reaches its very identity. Of course he forgets some: he forgets Alsted's *Encyclopaedia Omnium Scientiarum* of 1630; he forgets Novalis's *Das Allgemeine Brouillon*, a posthumous publication of 1802; he forgets St. Simon, whose *Prospectus à une Nouvelle Encyclopédie* (1809) constitutes an important formulation of a theoretical program stressing the need to articulate all the domains (science, politics, social life) according to the scientific attitude and methodology proposed by Bacon and realized by Newton in natural sciences. Something which Neurath would appreciate.

In third place, of those philosophic encyclopedias he quotes, some are just labels, references, celebrated ancestors (like Lull, Comenius, Spencer), others are enemies, adversaries, projects with which Neurath wants to establish strong opposition (Thomas Aquinas, Hegel).[3] Others are recognized like major predecessors (Bacon, Leibniz). Finally, it is the *Encyclopédie* of Diderot and D'Alembert which is claimed to be the great inspiration. The very introduction Neurath writes for the new *Encyclopaedia of Unified Science* – the text untitled "*Unified science as Encyclopaedic Integration*" (1938a) – is clearly marked by such illustrious ancestor which is the *Discours Préliminaire* (1751) of D'Alembert. As Neurath writes: "Our encyclopaedia continues the famous French *Encyclopédie*" (Neurath 1938a, p. 2). And further, "About 190 years ago, D'Alembert wrote a *Discours Préliminaire* for the French *Encyclopédie*, a gigantic work achieved by the co-operation of a great many specialists (. . .). One must carefully look at their work as an important example of organized co-operation" (ibid)

5.2 The Main Determinations of the Encyclopedia

Let us now see what is common to all these projects and how is Neurath positioned face to the main determinations of the encyclopaedia. We will try to briefly characterize encyclopaedic project in eight points.

1. Encyclopaedia aims to become a complete, impartial and objective reflection of all knowledge conquered by mankind and available at a certain historical moment. This vertigo towards exhaustivity can lead encyclopaedia to a teratological dimension – the case of the immense Chinese encyclopaedias which Neurath refers is eloquent – the *Yung-loh*, fifteenth century, 11.995 volumes (never ended), the *Tu shu chi ch'eng* published in Shanghai, 1726, with its 5.020 volumes and of which there is a complete exemplar at the British Museum.

However, in the Western world, encyclopaedia is touched by the law of constant innovation that characterizes our civilization. It is thus always designed, not as a *complete* but as a *compact* library (the aim of encyclopaedia is to put library inside the book), an economic work forced to combine exhaustivity with selectivity. In the line of Bacon's indications, encyclopaedia is assumed as an historical production, always unfinished, incomplete, precarious, and condemned to the voracity of knowledge progress.[4]

This is what Neurath recognizes when he stresses that "encyclopaedia is a provisional assemblage of knowledge, not something incomplete but the reunion of scientific knowledge which we possess at present" (Neurath 1936, p. 188). I quote again: "The future will produce new encyclopaedias (. . .) and it is senseless to speak about a complete encyclopaedia" (ibid). And further, "the progress of science goes from one encyclopaedia to another one" (ibid). Like science, encyclopaedia is something to go on doing "little by little" (Neurath 1938a, p. 3). Sciences are living realities. Reductionism is an infinite task.

2. Encyclopaedia is not a dictionary. Dictionaries aim to be a complete codification of language, even if they can never realize such a project and thus they all suppose some encyclopaedic openness to the world. On the contrary, encyclopaedia is a semantically opened structure, a representation referring the world of things and events which are to be spoken, that is, to be known. Even if some encyclopaedias may have been designated as dictionaries (the most celebrated example if the *Encyclopédie ou Dictionnaire Raisonné des Sciences, des Arts et des Métiers*, by Diderot and d'Alembert or Larrousse's *Grand Dictionnaire*); even if some have in common with the dictionaries the alphabetic presentation of its elements (the case again of Diderot and d'Alembert's *Encyclopédie* or of Coleridge's *Encyclopaedia Metropolitan*a) – encyclopaedia is never a dictionary. Encyclopaedia is not interested in words but in what words mean and refer – the world behind the words.

That is why encyclopaedia always reflects the cultural and scientific situation in which it is created. That is why encyclopaedia needs constant actualizations. As it is said in the *Preface* of the 1974 edition of the *Britannica*, "if we want to seriously reflect the knowledge situation of nowadays, we cannot dedicate 30 pages to chivalry and 31 to legal status concerning pornography" (Adler 1974a, vol I: XIII). And to actualize it is not just to add new entries (for instance, bioethics) but to diminish the importance of some (for instance, flogistus) and to grow up others (for instance, atom). As Neurath says: "the encyclopaedia which we aim is a given historical formation to which any extra-historical ideal can be opposed" (Neurath 1936, p. 200). And he adds: "Our attempt can obviously be rejected later. That is something we must be aware. Nevertheless, the new generation will probably improve the work with enthusiasm and success towards unitary science." (Neurath 1936, pp. 200–201)

3. If encyclopaedia is never a dictionary, yet they have one point in common. Like dictionary, encyclopaedia is a discontinuous text made of independent segments or *entries*, either alphabetically organized or structured in larger conceptual, thematic or disciplinary frameworks.

Those semantic fields never present well-defined borders. Each entry opens (explicitly or implicitly) to other entries which, in turn, open to others, in such a way that each entry is virtually connected with all others. In other words, encyclopaedia is not so much a monumental reunion of all knowledge in one closed place, but the free circulation of unity throughout the dense and sensual effectivity of its volumes and pages. It is not a static totality but a dynamic entity, "a living being and not a phantom (. . .), not a mausoleum or an herbarium, but a living intellectual force" as Otto Neurath said in his famous *Unified Science and Encyclopaedic Integration* (1938a, pp. 25–26).

4. The material objectivity of encyclopaedia has thus an unlimited condition. The finite member of its pages contains a net of discrete elements which can be articulated according to multiple relations in an undetermined number of combinations, a kind of combinatory without rule.[5]

That is to say, behind the additive synthesis of all its entries, encyclopaedia does point to the exhaustion of all the possible combinations of its entries. That is the point in which encyclopaedia makes unity of science appear more like an infinite task. As Neurath recognizes, the experience of infinite gives great pleasure: "many young people, to whom sciences appear cold and distant in their isolation, will surely be attracted to unified science because of the possibility of connecting everything with everything" (Neurath 1937a, p. 140).

That is why encyclopaedia offers its readers the possibility of making their own journey of reading according to their interests and preferences. In fact, encyclopaedia not only offers that possibility but also suggests it, promotes it, invites the reader to take his own course, proposing a set of resources (for instance, indexes, thesaurus) by which he chooses, by successive extension, which semantic fields he should read after another. That is why the *International Encyclopaedia of Unified Science* "will not be", as Neurath says, "a series of alphabetically arranged articles but rather a series of monographs with a highly analytical index" (Neurath 1938a, p. 24). And, in the very heart of the onion, there will be "two volumes which will deal with the problems of systematization in special sciences and in unified science" (ibid).

5. That is why encyclopaedia always has a strong hope in its cultural, educative role.

It is true that, inviting the reader to follow his own *cursus*, the encyclopaedia is not didactical, not a student's manual. The reader is never a student, never a pupil, someone who intends to follow a pre-determined *curriculum* in order to obtain a systematized knowledge. Neither is he an autodidactic – caricature and victim who tries to substitute school by encyclopaedia.[6] The reader of an encyclopaedia is always an already lettered public – "un publique éclairé" as D'Alembert says,[7] a "curious and intelligent reader", as stated in the Preface of the *Britannica* (Adler, 1973–1974b, vol I: XV).

However, encyclopaedia always supposes the constitution of a new knowledge community whose sociological limits ideally coincide with the entire humanity. So, in order to reinforce its cultural, educative and even ideological role, encyclopaedia points to the semantic exploration of the diagrammatic resources of language putting them at the service of the iconic and imagetic description of the world. That is why encyclopaedia frequently includes non-linear materials such as pictures, drawings, diagrams, illustrations, maps, statistic lists, plans, and tables of all types (see the case of the more than 600 pictures of the *Encyclopédie* and of the 11 complementary volumes of pictures Diderot published in 1762–1772).

We know that Neurath stressed, in theory and in practice, the need of democratization and popularization of knowledge. See his many activities in terms of social and political education, as *Museum* director in Vienna (1925–1934), school reformer (in the line of Otto Glöckel social democratic school reformer movement,[8]) militantly working in adult education at the *Mundanaum Institute*, and, above all, inventor of the Vienna Method of Picture Statistics and International System of Typographic Picture Education or Isotype. By creating Isotype, Neurath said, "I was thinking mainly of the masses who could now grasp something more than before of the present knowledge of mankind" (Neurath 1946, p. 502). It is thus quite understandable that, in what concerns encyclopaedia, Neurath pointed to the construction of a 10 volumes *Isotype Thesaurus* including all kind of pictorial representations able of "showing important facts by means of unified visual aids" (Neurath 1938a, p. 25) and that he places such a project in the line of Leibniz *Atlas Universalis* and Comenius *Orbis Pictus* (cf. Neurath 1938a, p. 16). That is to say, Neurath understood well the close connection between *Museum* and Encyclopaedia – *Museum* is a material encyclopaedia. Encyclopaedia tends to recover the idea of *museum*. The both are seeing machines.

6. Further, encyclopaedia is a collective work. It is true that some works today can be included in the gender of encyclopaedia were made by one author only. That is the case of Varro (116–1127 b.C.) *Rerum Divinorum et Humanorum*, of Plinius (23–79) *Historia Naturalis*, of medieval work by Isidorus of Sevilla (cf. 560–636) *Etimologies,* Vincent de Beauvais (c. 1190–1264) *Speculum Majus* and those many Renaissance encyclopaedia like Giorgio Valla (*De expetendis et fugiendis rebus*, 1501), Rafaele Maffei (*Commentarium*, 1506), Domenico Delfini

(*Summario di Tutta Scienza*, 1556), Luis Vives (*Tradentis Disciplinis*, 1531), Comenius (*De rerum humanorum emendatione consultatio catholica*, 1662–1664), Alsted (*Encyclopaedia Omnium Scientiarum*, 1630) or Pierre Bayle (*Dictionaire Historique et Critique*, 1697).

But, from eighteenth century on, encyclopaedia supposes the collaboration of different competencies: half a dozen of celebrated science men like John Ray and Newton as in the case of John Harris *Lexicon Thecnicon* (1704); many unknown, unidentified, even anonymous collaborators (like in the case of Diderot's *Encyclopédie*[9]); various identified authors presenting their controversial perspectives as put in practice in the twentieth century.[10] As Neurath says, "In these volumes, scientists with different opinions will be given an opportunity to explain their individual ideals in their own formulation" (Neurath 1938a, p. 25); "The collaborators will certainly learn from their encyclopaedical work. Suggestions from different sources will stimulate this activity so that this Encyclopaedia will become a platform for the discussion of all aspects of scientific enterprise" (Neurath 1938a, p. 26). As he writes: encyclopaedia "would always be open to questions and give rise to innumerable controversies" (Neurath 1937a, p. 140). That is, from one's voice discourse, encyclopaedia becomes a plural, pluralistic, polymorphic, democratic, international "orchestra" (cf. Neurath 1946).

Like all orchestras, it needs a maestro – to do what? To bring together the various and diverse instruments, to coordinate the differences, to support a "working community" (Neurath 1937a, p. 137), in a word, to "harmonize" the multiplicity (cf. Neurath 1946, p. 498). The fundamental aim is: "The maximum of co-operation. That is the program!" (Neurath 1938a, p. 24).

7. Collective work, encyclopaedia is never an amount of discontinuous elements coming from different sources. It is never a miscellany, never an inventory, but an ordered presentation. As Leibniz said, "l' encyclopédie est un corps oú les connaissances humanise les plus importants sont rangées par ordre" (Leibniz, Gerhard (ed.), 1960, vol7, p. 40). It always supposes a "systhème figuré des connaissances humaines", a *mapamundus* where the *order* and *connection* of human knowledge can be discovered, as stressed by Diderot and D'Alembert.

Let us say it clearly: encyclopaedia always supposes, implicitly or explicitly, a system of organization of knowledge. This systematization can be disturbed by the thematic or disciplinary order or even concealed (hidden) by the alphabetic presentation of entries. But the systematic structure is there and it is that systematic structure which determines both the quantity and quality of the entries, the inclusion or exclusion of certain topics, the settling, the articulation, the ordering, the relative status and importance of some entries towards other entries.

However, that does not mean that encyclopaedia should be endowed with a systematic perspective, a constraining point of view. Neurath is very strict concerning this point: "any kind of intellectual absolutism should be avoided as not being in harmony with our scientific practice which is of the same type as our everyday life" (Neurath 1947, p. 80). "We must, of course, avoid the error of trying to anticipate *the* system as our model of science. Our model is encyclopaedia itself" (Neurath 1937a, p. 136). "The task of encyclopaedia is to represent the present state of science and not to *anticipate* an unanimity which does not yet exist" (Neurath 1937a, p. 139). In a much clear formulation, Neurath asserts: "The anticipated completeness of *the* system is opposed to the stressed incompleteness of encyclopaedia" (Neurath 1938a, p. 21).

Neurath's inheritance from the French *Encyclopédie* comes precisely from the fact that Encyclopaedia should constitute a true "alternative to systems" (Neurath 1938a, p. 7). French *Encyclopédie* had not the aim, typical of metaphysics, of gaining an absolute point of view, of starting on the most general propositions in order to deduce the particular sciences. Similarly, the Encyclopaedia of logical empiricism should refuse any systematic totalization. It cannot be a unique, definitive whole. It must renounce foundationalism. It must accept the historical, provisory character of all synthesis. That is why it "should continue in some way the work which d'Alembert put forward with his extreme dislike by systems (Neurath 1936, p. 201). That is to say, the synthesis Neurath aims is of Baconian nature, a reunion always provisory and opened of empirically grounded knowledge.

8. Last point: If it is true that encyclopaedia reflects the knowledge situation of its time and, in what concerns the organization of knowledge, encyclopaedia has also a prospective role, both in its practical, ideological, political, educative aims and in its high heuristic value. This is the point in which Neurath comes close to Leibniz encyclopaedism, namely to its most meaningful feature: the heuristic value of encyclopaedia.[11] For Neurath as for Leibniz, encyclopaedia is a kind of an *organon* at the service of science progress and search for the truth.

By establishing cross-connections, by doing "local systematizations" (Neurath 1946, p. 498), by promoting "terminological unifications" (Neurath 1936, p. 196), by advancing "aggregations" (Neurath 1936, p. 188), by developing "transversal connexions" (Neurath 1936, pp. 197, 198), by showing "the gaps in our present knowledge and the difficulties and discrepancies which are found at present in the various fields of science" (Neurath 1938a, p. 25), encyclopaedia reveals itself clearly as an *organon* at the service of science progress and search for truth. By overcoming the "speculative juxtaposition" (Neurath 1938a, p. 20), by putting together several disciplines, by taking into practice a co-operative articulation, encyclopaedia, as Neurath says, allows scientists to build up "systematic bridges from science to science, analyzing concepts which are used in different sciences, considering all questions dealing with classification, order, etc" (Neurath 1938a, p. 18), to establish a "comparison of the argumentation in cosmology, geology, physics, biology, behavioristics ("psychology"), history and social sciences" (Neurath 1938a, p. 14), in such a way that "advances in one will bring about advances in the others" (Neurath 1938a, p. 24).

In a word, by synthesizing the already known, by giving to know what is known, encyclopaedia constitutes a kind of artificial protesis which liberates natural memory for what really matters – the unknown. Encyclopaedia – we could say – empties the opposition between memory and invention. An opposition which can only be thought out upon the disregard of the Leibnizian intimate connection between the *ars judicandi* and the *ars inveniendi*.

5.3 Concluding Remarks

We can now approach a modest conclusion. We did recognize Neurath's project of encyclopaedia in all those 8 issues. And we have to give him reason. We have to recognize that Neurath's encyclopaedism is of great significance for the formulation of a contemporary conception of unity of science. Maybe we are now in a better position to understand why encyclopaedia is, in fact, the metaphor of unity of science.

Unlike the circle, encyclopaedia does not need divine perfection. "It would of course be nice to harmonize the demonstrations in all areas, but in the meantime, scientific research must proceed" (Neurath 1946, p. 498). Encyclopaedia does not need eternity, stability, and complete systematicity. We can start with what we have. "If we don't have a system by the top we can build a system by down" (Neurath 1936, p. 196). Utopia in Neurath is not an ideal located far in an impossible future but an active attitude.

Unlike the tree, encyclopaedia does not necessitate hierarchy. Encyclopaedia does not believe in "leader's intuition" (Neurath 1946, p. 504), it "challenges any intellectual authority which pretends to preach the truth" (ibid), it does not need "transcendental credos" (Neurath 1946, p. 505), nor "centralized and dominating zeal which always lead to self-sacrifice and sacrifice of others" (ibid). Encyclopaedia demands pluralism, tolerance, perspectivism. It accepts arguing, it works with a great many scientific units of varying magnitude and differing provenance. It starts from everyday language; it rejects absolutism, pyramidism, foundationalism, and substitutes that by fraternity. Unity does not imply the exclusion of variety. On the contrary, it demands it.

Unlike the house, encyclopaedia does not need previous planification, no superscience or pseudo-nationalistic anticipation of the system of science. But it must fight against savage proliferation, disciplinary terrorism, closeness of specialties. Encyclopaedia needs organized cooperation, socialist planification. Encyclopaedia is an *orchestra* (also a good metaphor which Neurath uses for encyclopaedia), plural, controversial, cross connected, a potential multiplicity able to overcome its own limits and capacities. The maestro must be democratic (giving voice to all instruments). That is why encyclopaedia is a so deeply anti-Cartesian endeavor. It is not the work of a meditative singularity. Nor is it grounded in any indisputable truth. It does not make *tabua rasa* of the competencies and virtuosim of the members of orchestra. The task of the maestro is mostly to harmonize.

Unlike mapa mundus (Diderot and d'Alembert preferred metaphor), encyclopaedia does not need previous cartography, no previous classification of sciences. A mere librarian classification is enough.[12] Above all, encyclopaedia does not have any territorial, colonialist, imperialist conception of knowledge. To progress in knowledge, to know more, is not to conquer another foreign country. To know more is to establish new fraternities, new interdisciplinary forum.

That is to say, Neurath's encyclopaedia is close to the celebrated Lebnizian oceanic metaphor[13] for the unity of science: "The entire body of science can be considered as the ocean since it is continuous and without interruption or separation even if men consider in it different parts giving them names according to their commodity" (Leibniz 1903, pp. 530–531).

Apart from arbitrary, institutional borders, encyclopaedia points to a fluid, infinite, combinatory regime, aiming to promote the free circulation – something which clearly announces the curiosity of navigation[14] in the electronic encyclopaedia and Internet – in the interior body of the encyclopaedia.

That is why "we are like sailors who have to rebuild their boat at open sea, without ever coming to a safe and dry coast and rebuild it on the basis of the best materials"

Notes

1. We have studied elsewhere the main metaphors of Unity of Science which have been proposed since the Hellenistic circle up until the electronic net (cf. Pombo 2006a)
2. As Charles Morris says (1969: IX), "Encyclopedia is, in its origin, a Neurath's idea". See also the testimony Carnap gives in his *Intellectual Autobiography* (Carnap 1963, p. 23)
3. In this respect, it is interesting to note that Neurath praises A. Comte *Philosophie Positive* (cf. Neurath 1938a, p. 8), even if he should blame him almost for the same anti-fundamentalist reasons he rejects Hegel's piramidism.
4. As Bacon writes in the Preface to the *Instauratio Magna*: "it does not suppose that the work can be altogether completed within one generation, but provides for its being taken up by another" (Bacon (1620), vol IV, p. 21).
5. This is one of the major points in which encyclopaedia shows its close relationship with Internet. Cf. Pombo (2006b)
6. That is the tragedy presented by the extraordinary work of Flaubert *Bouvard et Pécouchet* (1880), the unwise adventures of two heroes taken by encyclopaedist passion who succumb the labyrinth of knowledge mostly, we would say, by absence of an ordered plan of studies.
7. "It can work as a library in all subjects for a man of the world and in all subjects except his own, for a science professional" (D'Alembert, 1751, p. 143).
8. On the practical, militant and educational philosophical and encyclopaedical activity of Neurath, cf. Haller (1991) and Stadler (1991).
9. The *Encyclopédie* had in fact the colaboration of first level science men, artists, musicians, writers like Quesnay, Rousseau, Voltaire, Du Marsais, Turgot, Montesquieu, Grimm or Duclos, side by side with craftsman, agricultures, gardners, weavers, etc. and even many spontaneous and sometimes anonimous "coleagues", all united by a militant "intéret général du genre humain et par un sentiment de solidarité reciproque" as Diderot says (1994, p. 368).
10. Around 4.000 in the case of the 15th edition of the *Britannica* (cf. Adler (1974a), Preface, vol I: XVIII).
11. As Leibniz states, "le principal est que la reveue exacte de ce que nous avons acquis faciliteroit merveilleusement des nouveaux acquest" (GP 7: 159). For further developments on Leibniz encyclopeadism, cf. Pombo (2002)
12. In this respect, see a curious passage by Neurath in his article *The Departmentalization of Unified Science* (1937b) in which the opposition to what he calls there as "pyramidism" is enlarged to the anticipative models of science classification. As Neurath writes: "Pyramidism (...) intends to built a symmetrical and complete edifice of the sciences by means of main divisions, subdivisions, subsubdivisions, etc" (Neurath 1937b, p. 245). And he adds: "Encyclopaedism is satisfied with a rough bibliographic order for an initial orientation, made by librarians" (ibid).
13. Neurath himself recognizes the oceanic character of encyclopaedism even if, quite surprisingly, he relates it not with Leibniz but with Freud. As he asks: "Is such a pure scientific

Encyclopaedism in a position to satisfy human yearnings and to create 'oceanic feeling' – if we may use Freud's term in this case? This question will be answered by the Man of the Future" (Neurath 1938b, p. 484).

14. The concept of "navigation", appears explictly at the *Organon* of the *Enciclopaedia Universalis*, vol. XVII: 595. *Encyclopaedia Universalis, Symposium*, Paris: Encyclopaedia Universalis France S.A.

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Chapter 6
Principles and Practices of Neurath's Picture Language

Ahti -Veikko Pietarinen

Otto Neurath (1882–1945) might never have been the pet philosopher of the logical empiricists' influential movement. For instance, he appears never to have assumed any distinctively genial and agreeable position in the mainstreams of the movement.[1] His clash with Rudolf Carnap is especially poignant.[2] Concerning the publication of the Encyclopedia series that was to promote the Unity of Science movement, Neurath feared that Carnap was not "only irritated by [his] formulations", but that between them "there are much more deeper [sic] differences".[3] In his 1944 letter to Charles Morris, Neurath identifies these differences in his attempt, already there in the *Antispengler*,[4] to present empiristically his Protokollsätze, which are "statements about statements". His attempts to convince Carnap or the mathematician Hans Hahn on the value and worthiness of his ideas were nevertheless frustrated, Neurath recalls, because "in the Wittgenstein period the statements of statements seemed to be taboo". Neurath recollects the Wiener Kreis readings of Wittgenstein, in which he frequently remarked how metaphysical some statements were, only to be advised by Hahn that Neurath should make a comment only when he "is satisfied by saying 'NM' ['No Metaphysics']".

The subsequent emergence of Tarski semantics was "praised" by Neurath, as it indeed was by Carnap and Karl Menger. Neurath applauded its meta-level way of arguing but did not accept the "comparison of thing and statement". Because by the mid-1930s Carnap had accepted Tarski's point of view pretty much wholesale, which Neurath thought then implied a commitment to obsolete Aristotelian metaphysics, "certain differences" between him and Carnap were unavoidable.

I suggest that the "certain differences" leading to a conflict between Neurath and Carnap can be explained, at least partly, in terms of the van Heijernoort–Hintikka dichotomy of language as calculus vs. language as the universal media.[5] Neurath promoted meta-theoretical systematisations early on, while for Carnap, the adoption of that point of view required a long incubation period.[6] I shall argue, furthermore,

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J. Symons et al. (eds.), *Otto Neurath and the Unity of Science*, Logic, Epistemology, and the Unity of Science 18, DOI 10.1007/978-94-007-0143-4_6,

that Neurath's calculistic position shows up in his attempts to establish a standard for a pictorial language of communication, the Isotype system.[7]

The Neurath–Carnap controversy is instructive also in the sense that Tarski's curious and somewhat fluctuating occupation of the middle ground between the universalists and the calculists can now be better identified: he accepted meta-level languages in order to be able to discuss the fundamental properties of object languages while at the same time being committed to what according to Neurath is bound to be "unfashionable" metaphysics.[8]

This clash between two absolute presuppositions seems to have played a part in Carnap's reluctance to give a final go ahead to the publication of Neurath's monograph *Foundations of the Social Sciences* in the *International Encyclopedia of Unified Science* series (Neurath 1944). Carnap, who was the editor of the series together with Neurath and Morris, found the final proofs that he received to be "in a rather unsatisfactory state" both from the "linguistic-stylistic" and the "representation of the whole" points of view.[9] He moreover held the central tenets of the *Foundations* to have been "formulated in a very hasty and careless way".[10] Terminology had been left unexplained, including the key term of "Terminological Empiricism". "Not even I have been able to find out what is meant by it," Carnap avers, and continues: "Some sentences are incomprehensible to me, and how many more will be so for the average reader.... The monograph seems to me to be below the level which so far we have succeeded in maintaining the Encyclopedia, and the weakest thing N[eurath] has ever written." Carnap presumes that "Neurath the Volcanic['s]... violent emotional reactions" and "obstinacy and unwillingness to accept suggestions for improvements from anybody" might have restrained Morris from communicating to Neurath his possibly likeminded reactions. Neurath had sent the final version of the manuscript to Morris for printing in the Encyclopedia on 11 September 1943.

The more mundane explanations, including that Neurath's manuscript was indeed written in a hurry for fear of the publisher's discontinuation of the entire series, do explain some of Carnap's unsavoury reactions. The lack of that painstaking rigour emblematic to any material that was hoped to receive Carnap's undisputed approval certainly had a part to play.[11] Yet Carnap nowhere states that he would fundamentally disagree with Neurath's philosophical point of view. Nothing directly suggests that the opposing philosophical presuppositions were decisive in Carnap's disapproving assessment of Neurath's work.

Neurath's *Foundations* appeared in the Encyclopedia without Carnap's editorial consent. Neurath was disappointed in both "Carnap's making a declaration" which "many people will regard as an insult" as well as in Morris's "not induc[ing] him to drop his decision, which in no case supports the movement".[12] However, Neurath reveals here how he, in turn, "very often could hardly agree with papers [Carnap] accepted without protest". This is an apt recognition of the philosophical differences the two men had. That Neurath and Carnap had very different editorial approaches is just grist to the mill: Neurath explains having assumed no editorial responsibility for the content of the material published in the series, while Carnap was much more sensitive to such matters. Interestingly enough, it is here that Neurath attempts to identify the deep differences between him and Carnap over the directions into which the research on protocol statements was heading, in terms of Carnap's reluctance to accept the meta-systematic role of language in science.[13]

In any event, Neurath had a vision. The value of his most lasting contribution has been redeemed only very recently. That vision was an establishment of a standard for a pictorial method of communication, known as the Isotype (International System Of Typographic Picture Education). In Neurath's own words in his 1936 book *International Picture Language*, Isotype was a picture language for "an education in clear thought – by reason of its limits".[14]

Originally a Viennese invention, Isotype was calculated to be the educational medium targeting *hoi polloi* on economic and social issues facing the interwar nation. There were as many as 25 people working on it in Vienna (Twyman 1975). During the war, Neurath re-established Isotype as a British institute. Many charts were produced in books of science as well as in some books of fiction. Most of them were published in the late 1940s soon after Otto's death, with the aid of his wife Marie Neurath. Marie Neurath continued working on Isotype, and published close to a hundred articles, books and pamphlets illustrated by the Isotype system between the late 1940s and early 1970s.[15]

Most of the research on Isotype has focused on the issues from the point of view of design sciences, such as graphic design, picture design and typography (Hartmann and Bauer 2006; Lupton 1986; Neurath and Kinross 2008). In addition, Neurath worked on and promoted a development of an international language of urban planning and design (Vossoughian 2008). The connections between the socio-economic data that supplied the Isotype charts and Neurath's overall political views have also been discussed to a considerable extent. Nevertheless, the purpose of the Isotype system was to establish and promote some much more general and deeper perspectives in the philosophy of language and the philosophy of education than these practice-oriented endeavours would have us believe. Isotype has some wider and lasting significance that has not sufficiently been acknowledged before.

I choose to highlight here six items illustrating Neurath's vision, adjoined with brief comments[16]:

> 1. To make a picture is a more responsible work than to make a statement, because pictures make a greater effect and have a longer existence (p. 15).

We can interpret this statement in the following way. Designing, drawing or drafting a picture is an assertion in a similar sense as putting forth a statement in a written or spoken language is. Asserting something is to assume responsibility for what is being asserted. Assertions are made by someone, and they are about something: they may be true or false. Likewise, pictures and other visual representations are assertions and can well have truth-values. What is more, however, is that pictures possess special *iconic* qualities that linguistic assertions by and large lack, which shows up in the "greater effect" of the former by virtue of them being closely related to our actual cognitive structures and processes of thinking and reasoning. Such close relations with cognition are exemplified by the information contained in the Isotypes that according to Neurath has "a longer existence" in the human or societal memory than what can happen with linguistic assertions.

> 2. The ISOTYPE picture language is not a sign-for-sign parallel of a word language. It is a language which may be put into words in very different ways. The units of the picture language have different senses when they are in different positions (p. 18).

Neurath accentuates the important fact that his picture language is not a translation of some well-formed fragments of spoken or written languages. On the other hand, pictorial assertions may be verbalised in many different, non-equivalent ways. Unlike with most words and in most natural languages, the meanings of the iconic image-like constituents of a picture can change when they are moved across to different locations within the picture as their contexts will change accordingly.[17]

The broader point that statements such as (2) imply is that the specific kinds of pictures Neurath was striving at do have a clear, defined meaning. They are assertoric and have propositional content. They achieve what they do not simply because they are visual and hence "catch the eye", but because of the specific iconic modalities that are more directly linked with cognition than the conventional symbols of natural language are. Language pictures are "living signs" (Neurath 1936), which is a phrase interestingly reminiscent of what Charles Peirce had asserted iconic signs to be in his theory of semiotics.[18]

> 3. It is not possible to give a word for every part of such picture or a statement for every group of parts. The parallel in a normal language of a complete "language picture" is a complex group of statements; and an account in words of what is in a group of language pictures would make a book. The sense of every part of these pictures is dependent on the sense of the complete picture and on its relation to the other parts of the picture (p. 20).

In contemporary terms, Neurath is stating here that pictures are not compositional.[19] We do not derive the meaning of a picture by contemplating first the meanings or senses of its constituents and then assembling these constituent meanings together according to some appropriate functional method, in view of thus arriving at a comprehensive grasp of the meaning of the whole. The meanings or senses of the parts of the picture hinge on to the whole so much so that the translation of these constituents to any counterpart assertions given in the symbolic form of natural language is liable to fail.[20]

> 4. But the uses of a picture language are much more limited than those of normal languages. It has no qualities for the purpose of exchanging views, of giving signs of feeling, orders etc. It is not in competition with the normal languages; it is a help inside its narrow limits (p. 20).

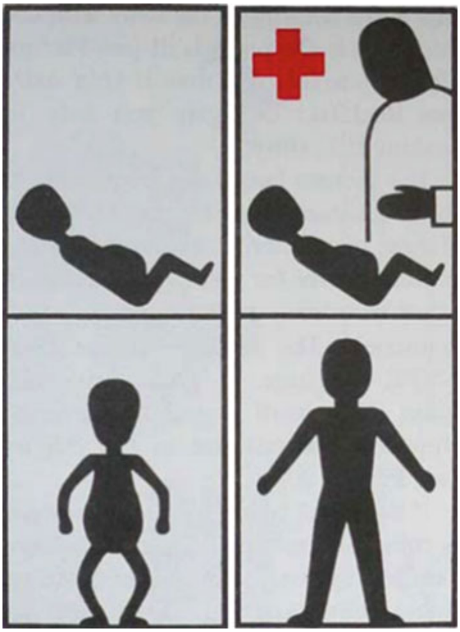
Neurath is making a valid point that a picture language is not a substitute for a spoken or written language. Pictorial expressions are not used and applied in the same way or in the same circumstances as natural languages are used and applied. Rather, Isotype pictures are complementary to natural languages. They do not supplant what can be stated with natural language. Neurath acknowledges in addition that picture languages will not be well geared for reciprocal communication in actual communicative situations, nor do they facilitate assertions of non-declarative statements.[21]

> 5. The effect of pictures is frequently greater than the effect of words, specially [sic] at the first stage of getting new knowledge. The number of pictures which come before everybody's eyes in newspapers, in motion pictures, in the street advertisements, is getting greater and greater, and today it is most necessary for the schools to take note of this fact, if the effect of the streets is not to be greater than that of the school (pp. 20–22).

This is a most acute observation on the sustaining effects of information and data visualisation and its educational value. Neurath emphasised the value of pictures purged of any unnecessary detail when displayed within the public sphere. The increasing information flood was evident in his time just as it has been in our ongoing digital era. Contained in Neurath's remarks is also the idea that the economy of expression is much greater with thoughtfully crafted pictures than what can be accomplished with the limited linear structures of symbolic languages. Cognitive economy is a common quality of any iconic representation, of which Isotype pictures are subspecies. Moreover, picture languages have educational value in raising general awareness and in portraying introductory material to the masses. However, we must learn to read and use such pictures correctly to avoid social chaos. It is apparent that any schooling must take a note of this fact today just as they needed to do so in Neurath's time. Yet he did not envision picture languages in order to attract the widest possible audiences or to enable adjustment of teaching or curricula according to the weakest of learners. Pictures are not rivals to, but harmonious and complementary with texts, lectures, and the rest of the entire spectre of pedagogy.[22]

> 6. Every process, however simple, has to be in harmony with the rules of logic and mathematics. No process, however clear-cut, and however well based on science and delicate thought, will have any value for science or for education if it is not in harmony with the rules of this poor logic and mathematics. . .. It has never come to our knowledge that experts in mathematics or logic have come across such errors in the ISOTYPE. This system does not take for its field the more complex part of science; its field is only the teaching of the very first stage (pp. 103–104).

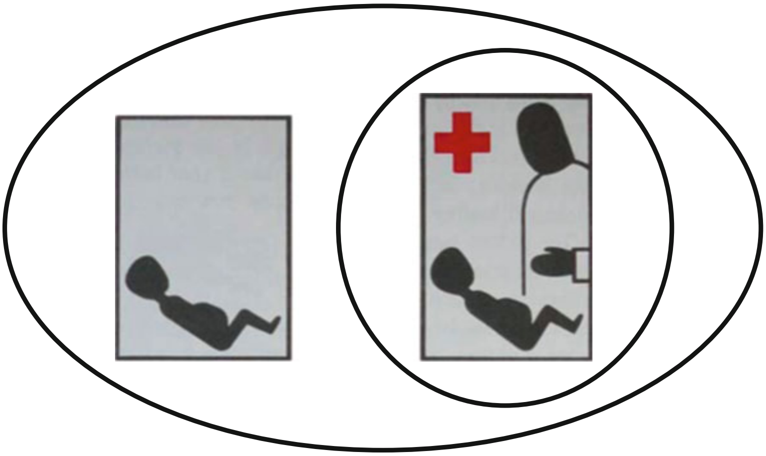
The final remark extracted from Neurath's *International Picture Language* is at the same time the most enigmatic and visionary. For Neurath, all real thought processes and reasonings are rational activities not in conflict with the rules of logic or the rules of mathematics. But he does not make clear what the rules are that he intends here. Nevertheless, we may conjecture that Isotype, in its fully developed form, would not only be a carefully crafted pictorial language of some graphical information or representation of useful facts but also an elementary *logic*. But how could that be accomplished? Unlike Peirce before him, Neurath and his followers did not attempt merely to build a pictorial logic. However, we would be well advised to look further and add some logical characteristics to the Isotype system. Taking cues from Peirce's iconic and diagrammatic logic is helpful towards this goal. Just to give a simple example how that can be done, let us remark that Neurath wanted to express with Isotype pictures complex assertions. But we can observe that they will contain some basic logical information such as connectives. For example, Neurath grafted a picture for the assertion "Go to the medical man, if your baby has Rachitis" (Neurath 1936, 55) as follows[23]:



The sentence contains an indicative conditional. But apart from the left to right ordering of images, the picture does not contain anything corresponding to the conditional. In Peirce's diagrammatic logic (Peirce 1931, 4.400, 1903),[24] however, the conditional is represented with a nested structure in which the antecedent lies within the outer nest and the consequent within the inner nest. He thought that such iconic forms are directly apprehended in cognition, and even without prior knowledge of how the system as a whole is supposed to work, because icons are capable of showing their own meaning.

Now it is perhaps more commonplace to use an arrow as a symbol for the conditional, but let us assume that such a convention has not been established in the community of picture users. Then the iconically communicated information concerning the nested structure of images is how the untrained eye is able to get the meaning even when the conventional meaning is yet to be established. The picture with the added logical structure of connectives may be drawn thus:

We read this picture as if we were now to assert the following indicative conditional: "If you have entered the area of situation depicted by the pictures in the outer nest, then you are capable of entering the area of situation depicted in the inner nest enclosed within the outer." It naturally remains to be seen what the logic of pictures



that is emerging here would look like in the end, including the semantics and the inferential relationships that are needed to describe various kinds of situations and to perform reasoning upon picture-like assertions.[25]

Be this as it may, what is evident is that for the overall idea of Isotype to be viable the presupposition of language as calculus has to be assumed. A picture language is a language among many. It is not a universal medium of expression, since to understand it other methods and languages must be augmented into its expressions. We are not "prisoners of our pictures": we can discuss their meanings using other means than those provided by the picture language in question. Pictures are closely related to the processes of human thinking and reasoning and not to the structures of the world our concepts talk about. The holistic and non-compositional nature of pictures means that there is little hope of formulating semantics in an inductive fashion. Assuming that comparable impossibility results arise in the logic of pictures as in symbolic logic, such semantics is effable though it cannot be defined in the same language of pictures.

Moreover, we should note that the meanings of pictures are reinterpretable. For instance, by varying the locations of some of its subcomponents, the meaning of the picture as a whole can change. Since pictures – qua icons – so to speak show their own meaning, syntax is secondary to semantic and pragmatic concerns of pictorial meaning. But iconic meaning is possible only because icons are closely linked with contextual and collateral information in the communities of interpreters. Because such information and observation of how it is used is required for the interpretation of pictures, semantics and pragmatics are not separable components of picture-like languages such as the Isotype.[26]

Otto Neurath has a good claim to having been a pioneer in information visualisation. Image-like iconic representations allied to his Isotype conceptualisations are today in global use throughout the public sphere as well as on the Internet. Computer technology has greatly expanded the development of such representations, originally static pictures, towards picture languages that enable movement and animation with notably amplified educational value. Nyíri (2003) has gone as far as to claim that, with Neurath's vision of an international standard for information visualisation, a new "unity of science" lurks around the corner after all. There is a certain respect in which this bold claim is nevertheless to be taken quite seriously, however, as how the mechanisms of pictures work is quite different from the logical empiricists' – first and foremost Carnap's – largely failed attempts to create a unifying language of science. Thus an appropriate and expressive enough picture language might accomplish in the philosophy of science more than Carnap, but also bring about new modes of thinking and meaning that excel the mere educational and pedagogical merits of the particular system of Isotype emphasised by Otto and Marie Neurath.

Information visualisation of the broadly same kind that was envisioned by the Neuraths will play an ever-more important role in areas from public education to the software development for a broad spectrum of web-based statistics and presentations. But this role will be more than just raising general awareness on emerging global issues. Information visualisation is likely to change the very fundamentals of how human beings can think and communicate. Inventions of new communication methods have done precisely that from the time immemorial. A new "major transition" in the evolution of language may thus await us here: from static symbols, speech and text we are moving towards the era of iconic signs and societies, such as using dynamic images, pictures, charts, diagrams, emoticons and metaphors to communicate our fundamental meanings.[27] Maynard Smith and Szathmáry (1995) identify eight major transitions in the evolution of complexity, the last of which is from primate societies to human societies, mediated by the emergence of language. Now that the human species has learned and mastered how to communicate with the conventional language and speech, why wouldn't the next major transition be from humans as a "symbolic species" to the "iconic" societies partly human (natural) and partly computerised, automatised and agent oriented (artificial, online), with visual and other diagrammatic means of communication as the emerging standard?[28]

Notes

1. Curiously enough, in his own words he was the one "promoting the term LOGICAL EMPIRICISM", which was his "expression-baby". But he "did not succeed", and the term logical positivism was to prevail. Neurath writes to Morris (18 November 1944, 4): "I remember hwo [sic] I tried to convince Schlick of the usefulness of this term, he tried to promote consistent empiricism or something like that. I think always with a look at JAMES (all other proposals, as radical empiricism, scientific empiricism etc are all of the same brand) and just that seemed to me dangerous, since James, who is to a certain extent somebody who helps us, is on the other hand full of metaphysical trends, liking Bergson, etc the same is right of Peirce. Therefore we should avoid to be in too close a contact with these fine people, who neverthelss [sic] do not belong to our movement as such." Quotations are from letters

deposited in the *Charles S. Morris Archives* at the Institute for American Thought, IUPUI (Indiana University–Purdue University Indianapolis), Indianapolis.

2. The contrast in the historiographies of logical empiricism has commonly been that of between Neurath and Carnap on the one side, and Schlick and Waismann on the other.
3. Quotations in the following two paragraphs are from Neurath's nine-page letter to Morris, 18 November 1944, 7.
4. Neurath's date for the *Antispengler* (his spelling) is 1919; however, the book was published for the first time in 1921 (Neurath 1921). His early book was a critique of Spengler's ambiguously titled *Der Untergang des Abendlandes*.
5. See van Heijernoort (1967) and Hintikka (1997).
6. Just how long has been given some elucidations in Hintikka (1992). Hintikka takes Neurath to be a member of the universalist camp, too, but evidence from Neurath's correspondence with Morris contradicts that claim.
7. Though communicating Neurath's political and military outcry, His remark to Morris on Heidegger – who could certainly be regarded as a staunch representative of the universalist position – is instructive in suggesting that Neurath is indeed an opponent of that philosophical view: "I did not see the Journal of Phenomenology. It is a sad thing, that now the German illness enters the States. . .. but that is the same in many other cases. In history etc. The Nazidom of Haidegger [sic] did not sufficiently shock people on the other side of the Atlantic. I bought his Nazideclarations, they are TERRIBLE, of the worst Goebbels type, full of distorted details, freely told lies etc but you feel through all these sentences the HIGHER language of his philosophy" (Neurath to Morris, 18 November 1944).
8. Let us be wary of the fact that both Tarski's and Carnap's viewpoints kept evolving throughout their careers and thus their positioning in the calculist-universalist axis should likewise reflect those changes.
9. Quotations in this paragraph are from Carnap's two-page letter to Morris, 19 June 1944.
10. "It seems that the whole has been formulated in a very hasty and careless way; no care and time has been taken to work it over, make things clearer, and give it some coherence. It jumps from one idea to another, while the poor reader looks in vain for a connecting thread" (Carnap to Morris, 19 June 1944).
11. Neurath admits these to Morris by confessing that "impatience is disturbing all things" and suggesting that his "always overstimulated activity has to be tamed by patience" (Neurath to Morris, 4 September 1942).
12. From Neurath to Morris, 18 November 1944.
13. According to Hintikka's study (Hintikka 1997), in the time close to Neurath's death, Carnap was giving up universalistic tenets and fitting better in with the calculistic position.
14. Neurath (1936), see also Neurath (1937).
15. See Twyman (1975) for a bibliography. Isotype has been continued, both in the name and in the signs, for instance in an Italian profit-making information design project of http://isotype.org/. This initiative fails to give credit to Otto Neurath as the originator and the innovator, however. The site http://www.fulltable.com/iso/ is an Internet gallery of the material possessed by the Isotype Institute. The University of Reading Department of Typography has an archive of the Otto and Marie Neurath Isotype Collection holding material relating to the Isotype movement (http://www.reading.ac.uk/typography/collectionsandarchives/typ-collections.asp).

    The site http://www.vknn.at/neurath/ is a web page on Neurath's visual education project. It contains an online version of his *International Picture Language*. http://www.dada-companion.com/neurath/ possesses material and further information on Neurath's pictorial statistics and details the extant archives and collections of Otto and Marie Neurath. Related ideas have emerged recurrently – see, for instance, the broadcastings in YouTube about *Ecolanguage*, which is a graphic language designed for biology showing the interactions of ecology and economics. Its opening statement is very much reminiscent of the Isotype programme: "We lack the ability to see everything, but we need to show many basic facts, so we must use as few symbols as possible" (http://www.youtube.com/watch?v=GrVsLdTtepM). Similar motivations were behind the development of Energy Systems Language by Howard T. Odum (Odum 1994). Von Engelhardt (2002) is a detailed study on language-like characters in graphical representations, such as maps, charts and diagrams.

16. The quoted six statements are all from Neurath's *International Picture Language* (1936).
17. "Visual aids do not lead to only one statement in words and that there are several different ways of handling the same visual material. ... This possibility of various ways suggests looking at pictures again and again to deliberate how to proceed" (Otto Neurath in Nemeth and Stadler 1996, 267).
18. See Pietarinen (2006, Chapters 4 and 5: *Moving Pictures of Thought I* and *II*), Peirce (1931–1958, 4.531, 1906).
19. Whether natural languages are compositional is a matter of ongoing debate (see e.g. Lee 2008; Sandu and Hintikka 2001).
20. Neurath's term "sense" is not a Fregean one, and it is therefore fine to talk of meanings interchangeably.
21. In the late nineteenth century, Peirce developed a graphical, diagrammatic logic of Existential Graphs, which he suggests are in fact not limited to representations of declarative expressions only (Pietarinen 2006; 2008a). Peirce claimed that "there are countless Objects of consciousness that words cannot express; such as the feelings a symphony inspires or that which is in the soul of a furiously angry man in [the] presence of his enemy" (Manuscript 499, 1906, *On the System of Existential Graphs Considered as an Instrument for the Investigation of Logic*). Neurath believed pictures to be more neutral and charged with less emotional intensity than language, which of course does not contradict Peirce's attempt to *represent* "non-propositional content" with diagrammatic icons. Neurath seems to be right in at least one respect: witness the various contemporary phenomena found, say, in the ease of the use of all kinds of emoticons in online chats, which interestingly are much more neutral and weaker in their locutionary effects than any corresponding verbalisations. That said, the rapid increase in today's visual communication methods may fundamentally change the ways we are able to grasp and assign significance to these suppressed emotive meanings in the long run.
22. "It is unnecessary to say in words what we are able to make clear by pictures. And on the other hand, it is frequently hard to make a picture of a simple statement. Education has to put the two together, and a system of education has to see which language is best for which purposes" (Neurath 1936, 26–27).
23. This statement is continued "; he will give you help in making it healthy." This latter clause is omitted for simplicity.
24. As likewise is the case in some expanded versions of the discourse-representation theory (Kamp and Reyle 1993).
25. Pietarinen (2010a) studies the characteristics of the logic of images from the Peircean perspective, with some suggestions as to the logical aspects in such non-diagrammatic images as constituents of diagrams.
26. Pietarinen (2007) suggests that from the game-theoretic point of view, in which strategic habits of action are constitutive of meaning, the alleged semantics/pragmatics distinction turns out to be moonshine.
27. See Pietarinen (2008b) as to the possibilities of placing metaphors within the framework of pictorial logic of diagrams.
28. Pietarinen (2010b) develops upon the possibility of a "sonorisation of logic", namely to use non-visual diagrammatic representations such as auditory diagrams for the purposes of logical representation and reasoning.

Acknowledgments Supported by the University of Helsinki Excellence in Research Grant (2006–2008) and the Jenny and Antti Wihuri Foundation (2009).

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Chapter 7
Conceptions of Reality – Schlick, Carnap, Neurath

Thomas Bonk

I examine a simple, intuitively powerful idea regarding the ontology of the world – an idea that seems now out of fashion. It's the idea that to say of an object or event that it is real is to say that it can be localized uniquely in the space-time frame. One thinker who has made a strong case for this idea is Moritz Schlick. One who has opposed it is Quine. Quine argued that any such criterion would be superfluous at best since what there captured by looking at the quantifiers of properly "regimented" theories. Moreover, a spatial-temporal criterion aims to draw a principled distinction where there is none: between the existence of chunks of matter, say, and existence claims in set-theory and mathematics. Existence is a univocal concept.

I focus on Schlick's version of the "Maxwellian Criterion", as it is also known, presented in his acclaimed *Allgemeine Erkenntnislehre*, and ignore for my purpose Schlick's transformation into a follower of Wittgenstein and the associated point of view that the realism-positivism issue is a pseudo-problem.[1] After clarifications and comparisons of Schlick's criterion I conclude with remarks on Neurath's anti-realism. Although what I have to say does not directly deal with Neurath's point of view, I hope the discussion does shed some light – however indirectly – on an important issue that had shaped the discussions of Neurath and other members of the Vienna Circle. It is somewhat surprising that three radically different conceptions of "what there is" emerged from the Circle.

1. In his attempt to establish realism in his *Allgemeine Erkenntnislehre* Schlick opposes at times the Machian empiricist, the skeptic, the Kantian (respectively neo-Kantian) and the believer in universals.[2] This constellation indicates a considerable potential for confusion. However, judging from the space allotted to its discussion in *Erkenntnislehre*, the main target is an earlier *positivism*, associated with the names of E. Mach and R. Avenarius (and J. S. Mill in some respects), both with respect to scientific discourse and with respect to knowledge of the external world. Schlick defines his task as, on the one hand, defending the possibility of extending existence

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J. Symons et al. (eds.), *Otto Neurath and the Unity of Science*, Logic, Epistemology, and the Unity of Science 18, DOI 10.1007/978-94-007-0143-4_7,

claims beyond what is directly observable, and on the other hand, as defending the restriction of existence claims to events or objects (localizable) in space and time. His aim is to draw a line between scientifically respectable objects, mental states open to introspection, etc, and abstract entities or fictive events (dreams), where the problem of the reality of future and past events take on a special significance.[3] That he intends to include mental states into what to count as "wirklich" besides things may appear somewhat unusual. But it is part of his overall argument that mental states have the same ontological status as common sense material bodies. When he claims – in accord with common usage he thinks – that both kinds of entities are "wirklich", he does not mean to imply however, I believe that mental states are themselves material or reducible to physical processes.[4]

The meta-philosophy is explicitly anti-Cartesian: the philosopher initially accepts an entirely unrestricted ontology, and step by step critically weeds out kinds of entities, that do not belong to the "Wirklichkeit". In expounding his criterion of existence, or rather of an object "being real", Schlick understands himself as explicating a common sense concept of reality and existence that dominates scientific practice as well.[5] He formulates a general methodological requirement for any explication of the notion of being a real object: the explication must maximize agreement with common usage. Any concept of being real that fails this test has to be rejected. I find two reasons for this requirement (1) Schlick seems to think that if a universal concept, criterion or belief is strongly anchored in everyday or scientific practice, than it is default a priori in the sense that it is meaningfully applied or reasonably believed prior to any metaphysical or empirical investigation. Any criterion of existence or reality, for instance, Schlick maintains, has to be "connected" to the immediately Given[6]

> weil in ihm der Begriff der Wirklichkeit wurzelt.[7]

Only specific reasons for doubt can warrant revising or abandoning a widely held belief or commonly used concept or criterion. He examines and rejects various proposals for such reasons because they rest on "dogmatic" premises since they turn out to be neither a priori nor self-evident nor rooted in practice (2). The second reason advanced by Schlick, though in passing, has a strong linguistic flavor. It says that any conception of reality that departs from the common sense one runs counter to our ordinary use of the word "real" or "reality".[8] The deviant alternative thus borders on non-sense.

2. Before stating Schlick's criterion "der Zeitlichkeit" for being a real object or event, I need to address briefly the notion of the "Given". Any criterion for being real, after all, has to be (1) be purely formal or content-neutral, and (2) "rooted" in the immediately Given, as stated above. This notion has been sufficiently criticized, by Neurath among others, to warrant a few words. There are at least two potential ways of understanding the Given. First, the concept of the Given comprises what we all are disposed to accept without doubt on the basis of the senses or testimony: common sense truths, and perhaps a smattering of scientific "truths". Second, the Given refers to what is individually, subjectively and immediately present in the experience of the individual: sense data, "Erlebnisse", the contents of the "Bewußtsein". Schlick

makes it clear that he takes the Given in the second sense; he thereby chooses a traditional epistemological starting point.[9] But it is not the (alleged) foundational character and epistemic authority of the "Erlebnisse" for inferences about the external world that is of interest to him. Rather (1) mental events are exemplars of what is unquestionable "real", and (2) they serve to interpret abstract schemes of order. For instance, ascriptions of dates to events are – according to Schlick – only possible because the time-scale and hence the perceived "distance" of the event is anchored in the specious presence, the "Gegenwart" as its zero-point.[10] The objective time order of events would be "inhaltslos" and "sinnlos" if it were not anchored in the individually and directly experienced Now. I think this example illustrates one important way of how Schlick intends to make philosophical use of the Given. Equally problematic is the first point since it does not seem to be common usage to describe a subjective experience as something real.

Does Schlick regard the time order itself as an element of the Given? *Prima facie* time, durations, moments, are not the kind of items that are given in experience. It is things in time of which an individual has experiences of. Objects, states of objects, changes in those states can be experienced – but not, it appears, ethereal instants and time itself. In fact, Leibniz and others thought of time as a function or construct of such unproblematic events as observable changes in a state of an object.[11] Schlick distinguishes between actually experienced sequences of events and an abstract, relational time-order in which elements of the Given are placed.[12] The former is a purely subjective order-property without transcendental significance. Time as an abstract time-order, for him, is not element of the Given, and is not a "real" entity.[13] (Similar considerations apply to the notion of space.)

3. I turn to the characterization of the ontological criterion that Schlick finds operative in everyday and scientific practice. He states it in a number of versions, not all of which are fully equivalent. Here are key passages:

> (A) Niemand bestreitet, dass alles Wirkliche für uns in der Zeit ist [. . .] Hier können wir uns einfach auf den consensus omnium stützen [. . .] Es ist an diesem Punkt keine ausdrückliche Rechtfertigung und Begründung, sondern nur Erläuterung und Verdeutlichung erforderlich.[14]

> (B) Jedem Wirklichkeitselement kommt ein und nur ein Platz in der Zeit zu, der völlig bestimmt ist, sobald nur eine Masseinheit und ein Bezugsystem der Zeit gewählt sind.[15]

> (C) Wir behaupten also: Wirklich ist alles, was zu einer bestimmten Zeit als seiend gedacht werden muß.[16]

> (D) Sobald für irgendeinen Gegenstand sich ergibt, dass die Regeln der einzelwissenschaftlichen Forschung dazu zwingen, ihm einen bestimmten Ort und eine bestimmte Zeit zuzuerkennen, so ist seine reale Existenz auch im philosophischen Sinne gesichert; [. . .] [17]

Schlick formulates a necessary criterion for something being real. Since he takes mental events or states besides ordinary events and objects as something real, only "being in time" is a candidate characteristic these heterogeneous types of events share. Such a criterion also succeeds in excluding "Platonic" objects like numbers, functions, concepts, and the like from the realm of what is real. Schlick's criterion is hardly novel, and perhaps it verges on the tautological, but Schlick would not claim otherwise. He sees himself explicating what nearly everybody accepts anyway.

A first stab at what Schlick is driving at is (compare quote C):

> If x is an entity or event such that temporal expressions like "x and y are simultaneous", "x began at t" etc (where y is an element of the Given, t refers to a point in time) cannot meaningfully be applied to x, then x is not real.

When combined with its spatial analogue one arrives at a sufficient criterion for something (possibly or "formally") being real. Formulated this way, mental states are not excluded from the ontology of our world. Schlick's proposal for the sufficient criterion for something being ("formally") real may be phrased in the following manner:

> If x is an entity or event such that scientific methods allow in principle to uniquely date x in the time-order and locate it within the spatial order among elements of the Given, then x is real (a genuine, mind – and language independent object or event).

Thus the paradigm of the unobservable – the atom – is real, since physicists successfully describe phenomena based on the (statistical) behavior of (classically conceived) atoms in space and time, and devise experiments to locate them individually or manipulate their physical states.[18] Schlick is not claiming here that scientists have thereby proven the existence of atoms (once and for all). Rather he wants to say that scientists have the license to take atoms to be as real as any mid-size body.[19]

Since objects are frequently only known by way of theoretical descriptions the following formulation recommends itself:

> If a description of (entity, event) x is essentially incomplete if it does not contain reference to a point or duration in space and time then x is real (is a genuine, mind – and language independent entity or event).

The claim aims to capture the conceptual necessity to which Schlick appears to appeal in quote (C). The last quote on the other hand, with its appeal to scientific method, seems to require that science is able to actually locate and date a hypothetical object.[20] His formulations frequently sound as if being a real entity or event requires evidence for its actual existence at a particular spatial-temporal location. There is then a tension in *Allgemeine Erkenntnislehre* between the purely ontological component of a criterion of existence and the epistemological component.

The examples for how he wants his criterion to be understood illustrate the tension between the ontological and the epistemological, for instance the difference between an event in a dream, say my eating an apple, and its real counterpart. Although the act of my dreaming and its stages can be objectively localized and dated, the act of my eating the apple in the dream cannot. There is no correlation between time sequences in the dream and objective time, since there is no way to coordinate real, actually occurring events with the dreamt events. They leave "no traces" as Schlick wrote.[21]

The analysis of the question of the ontological status of future events follows similar lines. Can't we, he objects dialectically, imagine the precise date for some future event – without it therefore being real? If the event-to-be is going to happen at a certain point in space and time with "absolute necessity" (physical necessity, I assume), then the event is to count as real as any common object around us – although it has not taken place and will take place in a million years from now, say. Thus, in a deterministic Laplacian universe everything future is real and exists, at least for the mind who knows the current state of matter in the universe and its true laws. The physical laws in such circumstances allow us to predict and locate the event with certainty (neglecting measurement errors in the initial data, mathematical limits, etc). Schlick is quick to add that those ideal conditions for prognosis never obtain in practice. With regard to the ontology of past events he wrote: "Niemals wird sich mit schlechthin vollkommener Gewissheit ermitteln lassen, ob das vorgestellte Gewesene auch in der Weise wirklich war, wie es vorgestellt wird; je genauer aber wir es räumlich und zeitlich lokalisieren können, desto sicherer sind wir, die Wirklichkeit getroffen zu haben."[22] Although it is true that our certainty increases or decreases in the way Schlick describes here – reality does not come in degrees to objects. This kind of argument is a sign of the tension between the ontological aspect and the epistemic aspect of existence in *Allgemeine Erkenntnislehre*.

5. I turn to a difficulty for the ontological criterion that he addressed in *Allgemeine Erkenntnislehre*.[23] Space and time provide the ultimate order or structure for our experiences. The objective, universal order is critical for existence claims, according to Schlick. The spatial-temporal order, although objective, is however not itself "real" in the sense of the criterion. There is a long tradition, which regards time or some aspects of time as being in some way mind-dependent or anthropomorphic. Thus, propositions about time or temporal aspects of objects are such that they would be wrong or meaningless in a world devoid of conscious beings.[24] For instance, Kant had claimed that space and time as forms of our sensibility are subjective. Empirical objects cannot be experienced or described independently of these sensible conditions.

Schlick replies that this kind of view rests on an equivocation in the notion of time. Subjectivity, indeed, pertains to the individual experience of the "flow of time", ordering elements of the Given. Yet, the time order as a universal (conceptual) structure is perfectly objective and has "transsubjektive Bedeutung". Schlick's argument is contained in one sentence:

> [das eindimensionale Schema der Zahlenreihe] ist vielmehr dadurch begründet, daß eine ganz bestimmte Art, eine derartige Ordnung zu vollziehen, vor allen übrigen ausgezeichnet ist. das wir auf sie zwangsläufig hingeführt werden durch die Prinzipien, mit deren Hilfe überhaupt das Begriffssystem konstruiert wird, durch welches wir die Tatsachen der Welt bezeichnen.[25]

The linear order (relation) is said to follow from meta-principles that the guide the (conventional) construction of any conceptual system to describe the world, and it is in some way unique and even necessary according to Schlick. As mentioned in Section 2 above, the subjective experience of a sequence of mental states is thought to distinguish or interpret the linear order we associate with the “flow of time” from other, formally similar linear orderings.

The reply runs into a number of difficulties, two of which should be mentioned. First, time need not have the topology of a line, as Schlick assumes throughout. A number of thinkers (see Newton-Smith) have aimed to show that it is conceivable for the time order to have the topology of a circle, with history caught in eternal repetition. G. Nerlich has investigated a discrete time structure (others have considered a 2-dimensional time-“plane”). Quantum physics knows of a branching universe each time a measurement or observation is performed. Provided the interpretation is correct, there is no reason to believe that there is one absolute time order for all the multiple universes instead of multiple-time streams. S. Shoemaker and others have – against Leibniz and Kant – contemplated and re-opened the case for the existence of “gaps” in time, that is empty periods of time. The phenomenal aspects of time may well be compatible with non-standard topologies of time. There is little reason then to hold that the “Ordnungstypus des eindimensionalen Kontinuums” is “zwangsläufig” or necessary, as Schlick has suggested.

Second, general relativity theory and its more recent descendants imply that the space and time manifold is not a passive background for the motion and interaction of matter. Space, time and matter (energy) are interacting. In fact, the distinction between matter and space-time has seized to be fundamental and principled. This development is incompatible with a consequence of Schlick’s account, i.e. that time and space themselves are not real. If the time-order – in the final analysis – is as much a physical entity as any matter field or the next apple, why take it as basis for an explication of existence? The “elite” status Schlick has assigned to the time order of events as a basis for a criterion of existence appears to be unwarranted.

6. Another kind of objection is best introduced by way of a quote:

> The concept of reality occurring in these [...] questions is an empirical, scientific, non-metaphysical concept. To recognize something as a real thing or event means to succeed in incorporating it into the system of things at a particular space-time position so that it fits together with other things recognized as real, according to the rules of the framework.

This passage could have been written by Schlick, but it is – as the reader will have recognized – taken from Carnap’s “Empiricism, Semantics, and Ontology”.[26] Being real in this account is a relative concept – relative to the system of particulars in space and time, i.e. the thing language framework. The second statement is a bit puzzling, since due to the double mention of “real” in the explication of what it means to be real, it appears to be circular (Carnap, like Quine, uses the concepts real and existence interchangeably, real is what exists and vice versa). The thing language framework is one among other potential frameworks, or so Carnap claimed. Multiplicity of empirically equivalent systems and relativism aside, Carnap basically affirms Schlick’s criterion. Carnap’s account, though, seems more motivated by determining conditions for when a new hypothetical entity is to be counted as real among already accepted “real” entities. Moreover, Schlick thought he had identified the empirical, scientific and metaphysical concept of reality.

The multiplicity of linguistic frameworks is of course decisive in Carnap's conception, and Schlick did not properly take the multiplicity into account. For Carnap, the time order is a syntactic convention of the thing language on par with other conventions. This is one facet where the deep differences between Carnap, the Circle, and the Schlick of *Allgemeine Erkenntnislehre* come to the fore. Carnap's point here is a refined form of the position of *Scheinprobleme der Philosophie*: that the metaphysical "exists" of the realist is meaningless (because it is unverifiable.) However, the thesis of the conventionalism and relativism of existence claims is predicated on there being strong alternatives to the framework of particulars in space and time. Much of "Empiricism, Semantics, and Ontology" is dealing with the existence of abstract entities. There is an evident multiplicity here, as seen in the many ways arithmetic can be represented in set-theory. But the thing language is different in this respect. If the premise is wrong, and many doubt it for want of good examples, if there is only one such system – up to isomorphic copies – than Carnap's position here becomes less plausible. Moreover, to claim that the structure of time is a matter of setting up a framework, i.e. a matter of volitional choice in ordering our experiences, is a more dubious claim. It seems vastly more natural to take the apparent structure of time as a meta-principle for constructing acceptable "frameworks" or perhaps as a theoretical hypothesis or scientific posit.

7. Next, I turn to an objection against Schlick's ontological criterion, which follows from Quine's view. The claim is that existence is a univocal concept, covering material objects as much as universals. Whatever gulf separates the two kinds of objects it is vain and artificial to seek to reflect the discrepancies in a principled difference of two mode or senses of existence. We use one notion of existence, which is applied both to particulars in space-time and to abstract objects – not an ambiguous term "existence" with two senses or two usages, one good – one bad, an ambiguous term that is correctly applied to matter and somehow metaphorical extended to abstract objects. Quine held that a principled distinction between two senses of existence makes no sense.[27] Carnap essentially agreed on this point.[28] (They do not disagree over how to determine the existential import of a theory, rather over what goes into accepting a theory in the first place.)

Recall that to find out what a speaker is committed to ontologically, in using a language or theory, Quine suggested first, the reconstruction or paraphrasis of the theory in appropriate vocabulary and "quantifier theory", i.e. canonical predicate logic, and second, to look for what entities have to exist to make the predicates and statements of the theory true. The existence and universal operator indicate what exists (according to the speaker). Hence the slogan, to be is to be the value of a variable. Existence is relative to the speaker's theory (like in Carnap's account). This relativization comes to the surface most forcefully in the radical translation scenario from jungle language to home language. Quine subscribed to a general structuralist view of what there is, where we are barred from knowledge of objects and instead gain knowledge of permutation "invariant" relations and structures.[29]

This line of argument generates difficulties for Schlick's view of ontology. He insisted, as I pointed out, that his conception of what is "real" matches our everyday and scientific usage[30] and consequently Quine's "univocalism" does not. He seems to be broadly right here, the use of “exist” with respect to abstract objects appears to be without genuine ontic import, and “derived” or “deviant”. Schlick, I suspect, held that usage of exist in this case reflects the nature of arithmetic as an axiomatic system based on implicit definitions and logical and analytic truths. To claim that a mathematical object “exists” is a useful shorthand, legitimate whenever the formal system, whose axioms govern the object in question, is consistent, or perhaps if a constructive proof for the object in question is known. The reply is problematic, to say the least, due to the difficulties of logicism and the notion of analyticity. Small consolation that Quine’s celebrated and controversial argument from the indispensability of abstract objects in the natural sciences to the existence of such entities may not be valid, since – as H. Field (1980) has made plausible – physics can be done without numbers (Ironically, considering Schlick’s point of view, Field treats space-time points of the manifold as physical entities, hence as real).

Schlick’s rather consequent usage of real and “wirklich” (at the expense of “exist”) reflects perhaps a considered distinction between “existence” as a general term and the adjective “wirklich” (real) which is reserved for spatial-temporal forms of existence.

Before Quine’s ontological criterion there were others of course, for instance, the traditional claim that a universal exists, if a things exists of which it is true of.[31] Schlick did not spend much effort discussing the criterion. If Schlick had adopted a linguistic, conventionalistic view of concepts from the outset in *Allgemeine Erkenntnislehre*, then the traditional criterion presents no serious alternative account. Yet, if he started out with such a deflationary view of the nature of concepts and language, there would be no reason for him to investigate criteria that rule universals, propositions and the like out of existence.

8. Given the well-publicized dispute between Schlick and Neurath on the nature of observation sentences, facts and the justification of empirical knowledge, one would perhaps be led to expect similar discrepancies over what there is. One would be in for a mild disappointment. Witness a remark by Neurath in a letter to Carnap (1939):

> Dewey ist (. . .) einig mit uns, daß man nur von Dingen redet, die ein “Wo” und “Wann” haben (Physikalismus).[32]

Although this remark is somewhat cryptic (is this a convention for the usage of “Ding”?) Schlick seemed to be right when he claimed widespread support for his ontological criterion in the sufficient modus (“no one doubts it”). But it would be wrong to describe Schlick’s investigation as formulating and defending physicalism. Physicalism, as the term was used at the time, meant the restriction (of the scientific language) to common sense bodies and their observable characteristics, the “Ding-Sprache” (the linguistic framework for the Given in the first sense). Schlick aimed simultaneously to rule out, indeed, universals and rule-in scientifically respectable un-observables, and to give mental states their due. Schlick’s odd stance on the latter apart, one issue that seems to divide him here from the views of Neurath (and Carnap at this time), is that on his account atoms, electrical fields, and the like are as (potentially) real as any middle-sized, “Ding”. He thus appears as a forerunner of scientific realism, and more in agreement with scientific practice than Neurath (he was not alone in that, see Reichenbach's attempt to replace what he took to be a narrow positivistic criterion of meaning by a probabilistic criterion).

While Schlick is happy to accord "Wirklichkeit" to objects that satisfy the sufficient ontological criterion, like Zebras and electrons, Neurath famously protests. Two characteristic quotes from his programmatic "Radikaler Physikalismus und 'Wirkliche Welt'" (1934), the first explicitly aimed against Schlick's view, indicate why:

> Man kann die Wendung 'Übereinstimmung mit der Wirklichkeit' nicht mal als Metapher verwenden, da ja in sich widerspruchlsose Satzgesamtheiten zur Debatte stehen, die zusammen gewissermassen das Loch in unserem Denken ausfüllen müssen, das dadurch entstanden ist, dass wir auf 'die Wirklichkeit', auf 'die wahre Welt' und andere Termini dieser Art verzichten.[33]

And

> So reduziert sich für uns das Streben nach Wirklichkeitserkenntnis auf das Streben, die Sätze der Wissenschaft in Übereinstimmung zu bringen mit möglichst vielen Protokollaussagen.[34]

Yet Schlick did not understand the problem of "Wirklichkeitserkenntnis" as showing the possibility of having accurate representations of absolute, objective reality. Rather he framed the problem as overcoming the supposed primacy of the phenomenologically Given. Solving the "Erkenntnisproblem" means dissolving the traditional "gap" (the "veil") between appearance and essence ("Wesen") or Ding-an-sich. Phenomenalism, presented as the view that we have direct knowledge of immediate experiences only – the Given – not of the transcendent sources of those experiences, is a contradictory position or so he argues.[35] The subjectively perceived arrangement or order of sensible qualities (in subjective experience) must be "matched" (or caused) by some arrangement or order among the things-in-themselves and their properties on pain of collapsing in subjective idealism (solipsism). Hence from direct knowledge of relations between elements of the Given (or between observables) we can make inferences to the relations between characteristics of the unobserved and unobservable. Thus we can have knowledge of what there is and empirical science shows the way.

> Wenn wir unter dem "Wesen" der Dinge überhaupt etwas Erkennbares verstehen, so liefert uns die empirische Wissenschaft durchaus Erkenntnis des Wesens der Objekte. In der Physik z. B. erschließen uns die Gleichungen Maxwells das "Wesen" der Elektrizität (. . .) denn mit ihrer Hilfe können wir eben im Prinzip alle Fragen beantworten, die sich in Bezug auf diese Naturgegenstände stellen lassen. Gibt man dies zu, so sind wir nach dem Gesagten damit zugleich im Besitze der Erkenntnis des Wesens der Dinge an sich. Und nur der kann es nicht zugeben, der unter dem Wesen eines Realen nichts anderes verstehen will als ein schlechthin Gegebenes, eine unmittelbar erlebte Qualität; (. . .)[36]

In *Allgemeine Erkenntnislehre* Schlick is razing a distinction between beliefs that are distinguished by different kinds of content. Much later, in "Über das Fundament der Erkenntnis" (1934), he was engaged in defending a special, foundational relationship between the content of certain kinds of belief ("Konstatierungen") and its epistemic status (infallible, certain). It is against this later claim that Neurath, Hempel and others raised well-known objections (leading to various forms of coherentism, like the "Duhemian" variant in Neurath's case). In other words: nothing said in this debate shows that Schlick's dissolution of the "gap" is mistaken. Clearly these later developments do not address the original "Wirklichkeitsproblem". Indeed, in his post-critical-realism phase Schlick moved even farther away from the original problem and declared both the assertion of the existence of an external world and its denial, as meaningless utterances ("sinnleer").

With these remarks I conclude my preliminary investigation of Schlick's ontological criteria. They are flawed in some respects, but perhaps not beyond the pale.

Notes

1. Compare Carnap's remark on Schlick in Carnap's "Empiricism, Semantics and Ontology" in: *Meaning and Necessity*, Chicago: University of Chicago [1947] 1988, p. 215.
2. See his remarks on philosophical method pp. 215, 216 in: Moritz Schlick [1925] 1976. *Allgemeine Erkenntnislehre* (2nd ed). Frankfurt a. M.: Suhrkamp (abbreviated as AE).
3. AE pp. 218–219
4. This is one more critical difference to the received view of the Circle, where the mental was conceived behavioristically or as a system of dispositions to verbal or non-verbal behavior; compare Section 8 below.
5. AE pp. 213, 214
6. AE pp. 214, 221
7. AE p. 221: "(. . .) because the concept of reality is rooted in the [Given]."
8. AE p. 222
9. AE pp. 163, 446 fn. 39
10. AE pp. 221, 222, cp. 281
11. W. H. Newton-Smith, 1980, *The Structure of Time*, London: Routledge, p. 13.
12. AE p. 280 ff.
13. AE p. 224
14. AE p. 217, similarly p. 265: "[Es liegt kein Anlaß vor] das Wirklichkeitskriterium aufzugeben oder zu verändern, das sich aus den Verfahrensweisen des Lebens und der Wissenschaft abstrahieren läßt (. . .)".
15. AE p. 219
16. AE p. 224
17. AE p. 224. "If for any object it turns out that the rules of the individual sciences require to assign to the object a specific time and a specific place, then its real existence, in the philosophical sense as well, is certain." [translation T. B.]
18. AE p. 224
19. AE p. 265
20. For Schlick scientific methodology is by and large hypothetico-deductivism, with simplicity considerations thrown in, compare pp. 215, 265.
21. AE p. 220
22. AE p. 220
23. AE p. 276
24. See Newton-Smith, 1980, p. 12.
25. AE p. 279
26. See R. Carnap, 1988, p. 207.

27. See W. V. Quine, *Word and Object,* Harvard: Harvard University Press, 1960, §§ 27, 49.
28. See his “On the Logic of Modalities”, § 44, in Carnap, 1988.
29. For more on the relationship between Quine’s naturalism, his view of ontology and his professed (scientific) realism see my “Quine und der Realismus”, *Zeitschrift f. Philosophische Forschung*, 2006 2: 23–44.
30. AE p. 222
31. Schlick may have known that Russell claimed that the sentence “*a* exists”, where *a* is a particular, makes literally no sense (in “On denoting” 1905).
32. Quoted after: F. Hofmann-Grüneberg, *Radikal empiristische Wahrheitstheorie*, Wien: Verlag Hölder, 1986, p. 102. Neurath was skeptical of the concept of “existence”, calling it a “dangerous terminus”, see Hofmann-Grüneberg, p. 102.
33. In: *Wissenschaftliche Weltauffassung, Sozialismus, und Logischer Empirismus* (R. Hegselmann, ed.) Frankfurt a. M.: Suhrkamp. p. 110.
34. Ibid., pp. 112–113 “Thus the pursuit of knowledge of the real for us is reduced to the pursuit of bringing the sentences of science into agreement with as many protocol sentences as possible.” [translation T. B.]
35. AE p. 272
36. AE p. 274

Chapter 8
Keeping Track of Neurath's Bill: Abstract Concepts, Stock Models and the Unity of Classical Physics

Sheldon Steed, Gabriele Contessa, and Nancy Cartwright

> *We do* not *arrive at* 'one' *system of science that could take the place of the 'real world' so to speak; everything remains ambiguous and in many ways uncertain.*[1]
>
> Otto Neurath

8.1 Introduction

In 1935 Otto Neurath penned these comments in his paper "Unity of science as a task". A passage introducing the paper remarks that *scientific* people aim at a common procedure of inquiry by which to better understand the ambiguities and uncertainties of our world. But he asks, "Is this uniformity the logical consequence of our program? It is not; I stress again and again; I see it as a *historical fact* in a sociological sense" (Neurath 1935, p. 115). For Neurath unity of science was indeed a task, a goal. Unity in procedure of inquiry is crucial for understanding one another – and for making sense of the very uncertain world in which we live. But we do not arrive at it by discovering some small range of fundamental underlying principles of the world. It is this view of unity that resonates with the focus of this paper, a view that attempts to make sense of the domain of scientific inquiry while doing justice to the ambiguities and uncertainties that science necessarily leaves untreated.

As a means to this general end, this paper takes its cue from Nancy Cartwright's (1999) *The Dappled World*. It presents a defence of the view that the world in which we live – or rather the world given to us through scientific investigation – is probably at best described by a patchwork of laws with domains of limited range. Given this conception, the hope of uncovering some unifying set of characteristics of the world could be dramatically misplaced: for all we know there is no simple set of

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J. Symons et al. (eds.), *Otto Neurath and the Unity of Science*, Logic, Epistemology, and the Unity of Science 18, DOI 10.1007/978-94-007-0143-4_8,

underlying laws that describe these different domains. And we are better commissioned to examine the world as it is revealed to us – via a (monumental) set of often disjointed, sometimes converging, pockets of understanding. The world revealed to us through scientific investigation is a dappled one. We may elect to bring this world together under a unifying umbrella of scientific activity, but any such unity results from deliberative actions of scientists themselves. As *The Dappled World* suggests,

> What happens is more like an outcome of negotiation between domains than the logical consequence of a system of order. The dappled world is what, for the most part, comes naturally: regimented behaviour results from good engineering (Cartwright 1999, p. 1).

Many reject this point of view, claiming that the vast number of successful applications of scientific inquiry point in the direction not of some messy patchwork, but to a much more pristine and elegant set of fundamental laws applicable across the multifarious domains of scientific inquiry. These domains may not be unified in contemporary science, but we are warranted in holding out for the belief that they are in fact describable by a simple set of laws.

The present paper considers criticism from Sheldon Smith.[2] He locates his discussion in classical physics, forgoing the quantum domain. This restriction of scope suits our purposes just fine: if science provides a patchwork of local domains of analysis treatable locally then it seems we had best consider problems that arise within individual, narrowly defined domains one by one rather than assuming one sweeping philosophical account.

Smith argues that classical physics exhibits an element of unity which the *dappled view* – the view defended here – overlooks. Thus rather than challenge the dappled view in one global swipe, he attempts to "chip away" at it by indicating where it fails to account for important unified relations among, what he takes to be, sub-theories of classical mechanics. Moreover, he draws attention to a potential problem for the role stock models play in Cartwright's defence of dappling. In this paper we address the unity Smith claims is overlooked and take an opportunity to clarify certain features about stock models.

8.2 Warrant and the Stretch of Laws

The great virtue of our best physics theories lies in their claims to truth via the success of a huge number of precise empirical predictions. However, the cost of this virtue is the limitations on the domain we are warranted to assign them. Let us consider.

The no-miracles argument numbers among the most prevalent arguments in favour of the truth of scientific theories. The empirical success of our best scientific theories, the argument goes, would appear to be a miracle unless we assume that those theories are true. If we are to take this argument seriously, then we only have grounds for believing that our best theories are true within those domains in which they are empirically successful. Even the best of our scientific theories are successfully applied only to relatively rare situations in the real world. And many of these are situations that we devise within the walls of our laboratories or within the casings of our technological artifacts. The situations that occur spontaneously in nature and in which we have been able to make precise, successful predictions are few and far between. This gives rise to the view that the world could well be described at best by a patchwork of laws.

To illustrate this point, *The Dappled World* borrows an example from Neurath – a thousand-dollar bill swept by the wind in Saint Stephen's Square.[3] Can we model the trajectory of the bill dropped from the cathedral? Not likely. Could we model it *in principle*? The argument defended here is that empirical successes in classical mechanics at predicting the motion of an object cannot warrant the belief that these same laws of classical mechanics – or more precisely, point particle mechanics – govern the behaviour of the bill. If the theory does not provide the resources to construct a predicatively successful model of this particular situation then we are not warranted in claiming that it falls under the laws as they are specified. No model. No laws.[4] It is blind faith to say that we can model it in principle based on the scope of confirmed assignable force functions in classical mechanics.

According to Newton's second law the total force on the bill is equal to its mass times its acceleration. But to say merely that there are forces acting on the bill is too abstract to tell us about the situation. Any such description will apply only with more concrete statements about the particular forces at work. And these descriptions are provided by the stock models of the theory. For example, one of the basic stock models of point-particle mechanics tells us that an unsupported object of mass m in the vicinity of the earth will experience a force of magnitude mg towards the centre of the earth. So these models provide us with bridge principles to apply abstract theoretical concepts like "force" to concrete situations. Since the bill is an unsupported object in the vicinity of the earth, the bridge principle tells us that there is a gravitational force on it.

However, point particle mechanics does not seem to have a bridge principle that allows us to associate a force with another crucial cause acting upon the bill: the wind. What point particle mechanics provides us then is a partial model requiring supplementary information describing the effect of the wind as a force. This information could perhaps be provided by fluid dynamics, given answers to the right sorts of questions about the conditions within which the bill was dropped, what shape or state the bill was in, etc. We should however be wary of assuming that we can always describe the conditions of the bill in the right way – a way that allows us to assign a force function to the wind via a bridge principle. That's because the floppiness of the bill and the irregularities of its surface would make the distribution of lift around its surface at each moment practically impossible to describe with the set of stock models given in fluid dynamics. Alternatively we could construct a model in which the force that the wind exerts on the bill at each instant is set equal to the total force on the bill minus the force due to gravity so that the direction and magnitude of the total force on the bill at t is deduced, via Newton's second law, from the direction and magnitude of acceleration of the bill at t. This model might describe the motion of the bill quite accurately. Its descriptive success, however, cannot be evidence for

Newtonian mechanics because the assignment of the force function to the wind is purely ad hoc.

What is true of the bill is true in physics in general. It is usually only in very special circumstances that predicatively successful models can be constructed in a principled way from the resources of a single theory. These are cases where all the factors relevant to the targeted features of the phenomenon fall under the concepts of the theory, and of the same theory. In general no one theory contains enough stock models to go beyond simplified situations. Models may of course be fine-tuned to represent particular circumstances, but such corrections are generally ad hoc, often not given by any theory let alone one single theory.

Smith's paper argues that Neurath's bill can, in principle, be treated in a single theory. That is because it can be properly treated with the combined resources of point-particle physics and fluid dynamics, and these two are not really separate theories. They are actually instances of a more general super theory: classical continuum mechanics (CCM). Moreover, CCM utilizes not a limited number of stock models with a limited scope but a huge (perhaps infinite) number of principled models that are not ad hoc. This is because it provides models only in a trivial sense.

8.3 Unification: The Success Story

As Smith describes it, CCM contains two main kinds of equations. First, there are general principles, which describe abstract physical laws and apply to all materials. Second, there are constitutive equations, which describe the mechanical behaviour of specific classes of materials. As an example of a general principle of CCM, Smith mentions Cauchy's first law of motion:

$$\partial T_{ij} I \partial x_j + \rho b_i = \rho \left(dv_i / dt \right)$$

Smith notes that T_{ij} represents the stress tensor, while b_i represents body forces acting on the bill. This is crucial because locating both the contact forces and the gravitational forces in one equation precludes the notion that one must draw on models from different theories. Smith takes it that both the stress tensor and gravitational forces are utilized, but he argues, "it does not thereby follow that we are bringing to bear two different theories. All of this takes place within the framework of CCM centred around Cauchy's laws" (Smith 2001, p. 463).

This unification is nice. As Smith claims, CCM provides an abstract framework in which to bring together causes of motion otherwise separately treated in point-particle mechanics and in fluid dynamics. And it provides a way to show how to calculate what happens when both kinds of causes act at once. Moreover it is well confirmed across a variety of cases where both kinds of causes act simultaneously. This last feature is important because it provides warrant for the formula "as a whole" and hence for deductive predictions that follow from the formula when both kinds of causes are at work at once.

Perhaps it is well to explain this last virtue in more detail. In thinking about the travel of warrant from past predictive successes to hypotheses about new applications, we might face a version of an old problem that frequently besets theories of confirmation. Cauchy's formula reduces essentially to $f = ma$ in point-particle mechanics whenever T_{ij} is zero. It hence makes a huge number of accurate deductive predictions for these cases. Similarly it reduces to an analogous formula in fluid dynamics when b_i is zero and hence can count a huge number of these kinds of cases as predictive successes. Nevertheless we would not want to allow warrant to travel from the union of these two sets of successes to new cases where neither quantity was zero without a large number of additional successes for cases where both kinds of causes are at work at once. Happily these are available and where available we have warrant for ascribing successful application of the unified theory.

8.4 The Open Question

The question then remains: can CCM, as characterized by Smith, treat Neurath's bill? CCM, as Smith portrays it, provides a theory that describes the accelerations of bodies that are subject both to stresses – the causes studied in fluid dynamics – and to bodily forces – the causes studied in classical particle mechanics. Does that mean that (pace relativistic and quantum considerations) at least as far as accelerations are concerned we have a single theory that will cover every case, including Neurath's bill? That depends.

What is important for this question is that, as Smith points out, Cauchy's laws themselves "are not sufficient for tracking the motion of any system because one does not know anything about what the stress-tensor needs to be or what body forces might be acting on the medium" (Smith 2001, p. 462). That is, it is not until the constitutive assumptions are added to describe how to assign functional forms to the relevant stress tensor or body forces that the laws give a particular domain of application. These principles in CCM are in our vocabulary the "bridge principles" of the theory, connecting specific descriptions of situations and materials (the stock interpretive modes of CCM) with the functional forms of T_{ij} and b_i that are supposed to apply when the descriptions obtain.

The constitutive principles matter: the strong warrant that can be accorded the predictions of CCM depends on them. They fix the domain in which Cauchy's laws have the kind of strong warrant that is demanded in physics and they thus fix the domain of application for these laws. Can the stock models associated with these constitutive principles describe all the causes of acceleration that occur?

Phenomenologically the causes of acceleration are indefinitely various. Prima facie it seems unlikely they can all be properly described by the very restricted set of stock models available. The past history of successes and failures at bringing real situations under the purview of these models can hardly decide: there are notable successes and there are hosts of failures. The problem is that even when the resources of those two theories are successfully pulled together, we may not be able to construct a predicatively successful model of the situation and, therefore, we would have no evidence to believe that the motion of the dollar bill in Saint Stephen's Square falls under the jurisdiction of CCM.

Smith knows this. He suggests the initial conditions would make it unlikely that it would, in fact, be possible to model the event. Nevertheless he wants to argue, along with others, that this would still be possible in principle because Cauchy's law with the relevant constitutive equations contains terms accounting for both gravitational and contact forces. Smith wants to suggest that by subsuming fluid dynamics and point particle mechanics under CCM there is a unity there. We do not wish to deny this. Our worry is that this unity is not warranted for application beyond the domain in which its stock models have proven themselves. Locating individual theories under a more abstract notion can be fruitful in unifying distinct efforts in scientific inquiry, but it does not warrant the application of that abstract beyond the stretch of its bridge principles, or in the case of CCM, beyond the stretch of its constitutive principles. We are consequently left with nothing like the unity suggested by Smith, but rather with a unified theory that is warranted only over a much smaller domain than he supposes.

8.5 Stock Models and the *Dappled View*

Smith's second concern is over the distinction between ad hoc models and principled ones. According to Smith, there is no uncontroversial way to single out some of the models of CCM as its stock models. If science gives a patchwork of developed theories each with a limited number of stock models, then we need an account of why certain models get to count as "stock". Smith suggests that Cartwright (1999) leaves this account unclear, but that it potentially relies on three bases. First, one can look to something like a canonical list, for instance a list constructed from a text such as R. B. Lindsay's, which Cartwright herself cites as a source of stock models.[5] Second, one could consider scientific use, wherein past modelling successes give warrant for adopting specific bridge principles. Third, one can consider non-phenomenological models, which suggests that all legitimate scientific models are given directly by theory. He finds all three of these potential bases problematic. We consider the first two proposals in this section and the third in the next section.

Smith argues that looking to a textbook is clearly not a principled way to single out some models as stock models. We agree. We go to a textbook to learn what the principles of a theory are, but that does not show us why those *are* the principles of the theory. A model is not a stock model because it is included in Lindsay's textbook. Rather, a model is included in Lindsay's textbook because it is taken to be a stock model.

Smith also notes that, in the Section "Forced Oscillations of a Dissipative System", Lindsay introduces as an external force the force $F_0 e^{i\omega_0 t}$, which Smith calls the "Lindsay equation". If all models included in Lindsay's text were stock models, then, Smith argues, since F_0 and ω_0 are arbitrary constants, we would allow: "[. . .] any (odd) piecewise-continuous function of time (on the interval from $-\pi$ to π) to count as a principled force function derived in a principled way from this stock model" (Smith 2001, p. 467).

It may look, at first sight, as if a bridge principle is presented here: "when a dissipative system is subject to a forced oscillation it is subject to a force $F_0 e^{i\omega_0 t}$", but this is not correct. The Lindsay equation represents a generic force that drives the oscillatory system, not any specific force. In other words, the Lindsay equation represents a wide range of forces that might be driving the oscillations. We are not told *what* the force is. We are simply told its abstract form and, as Smith stresses, this is not to be told very much at all, since almost any concrete function can be cast into this form. So, in fact, no bridge principle is given here for assigning a concrete force function to a dissipative system subject to a forced oscillation. We are hardly even given a constraint on what any such force function must look like.

Second, Smith argues that if one relies on scientific use to fix what stock models are, then since Lindsay's text provides a canonical list of usage, use as a basis for selecting stock models falls to the same objection as the first. Scientific use does not limit the range of stock models because those models allow an unlimited range of force functions, and thus any cause that contributes to acceleration can be modelled within the theory. As we noted above, in classical particle mechanics, for example, if x represents the total force function that can be assigned to causes using other bridge principles of the theory, the remaining causes can be represented by the function $f = ma - x$.

But the lessons of successful prediction – the kinds of predictions that speak for the truth of the theory – point in the opposite direction. In order to develop a new model for classical mechanics, it is not sufficient to put forward one of the infinitely many possible force functions that are compatible with Newton's laws. We also need to associate with that force function a more concrete description of the circumstances under which a body is subjected to that kind of force. The role of stock models in classical mechanics is exactly that of providing us with a more concrete description of the circumstances under which a body is subjected to a force given by a specific function.

Stock models are well established when they have been successfully applied to concrete situations via bridge principles time and time again. The current set of the stock models of classical mechanics may not (and probably does not) exhaust the set of all possible stock models. In principle, we might be able to develop new bridge principles associated with new "stock" models that tell us what the force on a certain body is when the new stock models apply, but until we do so we have to resort to the set of well-established stock models. On our view then the only way to warrant the claim that it is possible to construct a principled model for the force of the wind on the bill is to show how such model can be constructed in a principled way and showing it to be successful. Anything short of this would seem to be nothing more than a promissory note that for all we know is likely to be void.

Smith, however, follows a different strategy. Rather than try to show that CCM provides us with the resources to construct a principled model of the wind-swept bill, Smith argues that no model of CCM is principled in our sense. This, according to Smith, is for two reasons. The first is that CCM does not operate with stock models. There is only a set of general principles with which any constitutive equations must comply. Even though most continuum mechanics texts treat a few tractable examples in detail, the standard practice in CCM is that any equation that adheres to these principles is an acceptable constitutive equation for use in modelling. But there will be infinitely many such equations. So, there are bound to be the ones needed for Neurath's bill (Smith 2001, p. 471).

This fits very nicely with his treatment of the Lindsay equation in classical particle mechanics. We have argued that the Lindsay equation does not figure a proper bridge principle since it does not specify a concrete functional form for the force causing the oscillation. But it does constrain the form of this function to some extent, and hence fits Smith's conception of a general constraining principle. The second objection that Smith makes to taking models in CCM as principled in our sense is that, according to him, in CCM there are no guides for applying constitutive equations, except for completely trivial ones such as "if the material is a Hookean elastic solid, then apply Hooke's law".

Smith's first claim seems mistaken. Classical continuum mechanics has a set of favourite constitutive equations that regularly can be counted as confirmed bridge principles. Textbooks in continuum mechanics usually provide their readers with constitutive equations for a variety of classes of materials, which typically include non-viscous fluids, Newtonian viscous fluids and Hookean elastic solids (cf. Fung 1969, Chapter 7 and Spencer Chapters. 8 and 10). And each of these has been repeatedly used with successful prediction and hence has claim to be included among the principles of the theory.

In fact, the development of what we call stock models for specific classes of materials seems to be one of the main aims of continuum mechanics. As one textbook puts it:

> The problems of continuum mechanics are [...] of two main kinds. The first is the formulation of constitutive equations which are adequate to describe the behaviour of various particular materials or classes of materials [...]. The second problem is to solve the constitutive equations, in conjunction with the general equations of continuum mechanics, and subject to appropriate boundary conditions, to confirm the validity of the constitutive equations and to predict and describe the behaviour of materials in situations which are of engineering, physical or mathematical interest (Spencer 1980, pp. 2–3).

Admittedly the first of these two tasks is not easily accomplished, so bridge principles are hard to come by. If there are few real bridge principles in CCM this is due to the fact that finding the right constitutive equation for a certain kind of material in certain kinds of circumstance is a formidable task, not to the fact that the theory can do without them and still have within it the resources to provide principled predictions for new cases.

To get a sense of the difficulties in devising bridge principles in CCM, consider the case of Newtonian fluids. The stress-strain relationship of a Newtonian fluid is specified by the equation: $\sigma_{ij} = -p\delta_{ij} + D_{ijkl}\, V_{kl}$, where D_{ijkl} is a tensor of the viscosity coefficient of the fluid and V_{kl} is the rate-of-deformation tensor (Note that when $V_{kl} = 0$ the constitutive equation reduces to the one for a non-viscous fluid considered above). As another textbook of continuum mechanics notes:

> For Newtonian fluids we assume that the elements of the tensor D_{ijkl} may depend on the temperature and density of the fluid, but not on the stress or the rate of deformation. The tensor D_{ijkl} [...] has [...] 81 elements. Not all these constants are independent. A study of

> the theoretically possible number of independent elements can be made by examining the symmetry properties of the tensors σ_{ij}, V_{kl}, and the symmetry that may exist in the atomic constitution of the fluid. We shall not pursue it here because we know of no fluid that has been examined in such details as to have all the constants in the tensor D_{ijkl} determined (Fung 1969, p. 129).

In most cases, a highly simplified version of the above constitutive equation is actually used. The rationale for using this equation is the assumption that most fluids are isotropic. For a fluid that is not isotropic the theory does not have the resources within itself to predict its behaviour. The shortage of bridge principles is a severe handicap in applying the theory.

Consider now the second objection. Even if in CCM there is a constitutive equation for a Newtonian viscous fluid, Smith maintains, there seems to be no bridge principles except for trivial principles such as "use the constitutive equation $\sigma_{ij} = -p\delta_{ij} + D_{ijkl} V_{kl}$, for a Newtonian viscous fluid". However, even this claim is not entirely correct. In introducing Newtonian viscous fluids, the Spencer textbook tells us:

> In experiments on water, air and many other fluids, it is observed that in a simple shearing flow [...] the shearing stress on the shear planes is proportional to the shear rate s, to an extremely good approximation and over a very wide range of shear rates. This behaviour is characteristic of a Newtonian viscous fluid [...]. This model of fluid behaviour describes the mechanical properties of many fluids, including the commonest fluids, air and water, very well indeed (Spencer 1980, p. 116).

Moreover the Fung textbook tells us:

> Air and water can be treated as nonviscous in many problems. For example, in the problems of tides around the earth, waves in the ocean, flight of an aeroplane flow in a jet, combustion in an automobile engine, etc., excellent results can be obtained by ignoring the viscosity of the media and treating them as a nonviscous fluid. On the other hand, there are important problems in which the viscosity of the media, though small must not be neglected. Such are the problems of determining the drag force acting on an airplane, whether a flow is turbulent or laminar, the heating of a re-entering spacecraft, the cooling of an automobile engine, etc. (Fung 1969, p. 129).

So if in CCM there are no strict bridge principles that associate a certain constitutive equation to a certain specific material, one reason is because in different situations we may use different equations for the same material. Air and water for example can be represented as Newtonian viscous fluids as well as non-viscous fluids depending on the problem at hand. But this is not to say that two completely different constitutive equations are assigned to the same material in different circumstances. As we have remarked above, the constitutive equation for a Newtonian viscous fluid reduces to the one for non-viscous fluid when the viscosity of the material in question is negligible.

The mechanical behaviour of real materials is diverse and complex and it would be impossible, even if it were desirable, to formulate equations which are capable of determining the stress in a body under all circumstances. Rather, we seek to establish equations which describe the most important feature of the behaviour of a material in a given situation. Such equations can be regarded as defining ideal materials. It is unlikely that any real material will conform exactly to any such mathematical model, but if the ideal material is well chosen its behaviour may give an excellent approximation to that of the real material which it models. The model should be selected with the application as well as the material in mind, and the same real material may be represented by different ideal materials in different circumstances. For example the theory of incompressible fluids gives an excellent description of the behaviour of water flowing through pipes, but it is useless for the study of the propagation of sound waves through water, because for the sound-wave propagation a model that takes into account the compressibility of water is essential (Spencer 1980, pp. 104–105).

Smith's denial of the existence of stock models and constitutive equations in CCM may derive from the fact that the Lindsay text is an advanced and highly abstract textbook. It can thus assume the reader is already familiar with the stock models of continuum mechanics. It can also assume that the reader is familiar with the stock models of both particle and fluid mechanics, which it encompasses.

Let us be clear in closing this section exactly what we take to be the selection criteria for stock models. Bridge principles associate a stock model with a theoretical description, so the stock models are the ones that appear in bridge principles of the theory. Their admissibility of the bridge principles is determined in the same way as that of any theoretical principle. Different methodologists have different views about what makes a principle admissible and we would like to stay neutral about that for our purposes here. We do at least though want to stress that empirical confirmation is crucial (with all the usual caveats that no principle is confirmed in isolation, etc.).

Beyond admissibility, we can enquire about *usefulness*. Here it is important to consider both ends of the bridge principle. The principle will be of little use if we do not have some independent ways of deciding if the model in it fits a given situation. Think back to our discussion of air and water. If we have no idea when air or water can be described as a "Newtonian viscous fluid" then having a bridge principle that tells us that $\sigma_{ij} = -p\delta_{ij} + D_{ijkl}\,V_{kl}$ is a Newtonian viscous fluid will not be of much use. At the other end, the principle will be of little use if the theoretical description is not specific enough to allow us to do calculations. This happens for instance if the theoretical description has system specific constants in it that we do not know how to evaluate, or if the functional form is not specified but only loosely constrained.

The two features, admissibility and usability, are not unrelated though. For if a principle is not very usable, either because we do not have good cues about where it applies in the world or because it does not give a specific enough theoretical description of the situations to which it applies, it will be equally difficult to confirm. And we reiterate: in our view theories can only be taken to stretch as far as their well-confirmed principles can take them.

8.6 Phenomenological Models

Finally, we should like to address a possible misunderstanding – Smith's discussion suggests the demand that the domain of a theory be determined by the range of the stock models may put a stranglehold on development of theory.

Smith's construction of Cartwright's argument seems to be the following,

- All proper science appeals to a small set of stock models, which provides the pool of legitimate models that fix the domain of the theory.
- We can find these models in a canonical text like Lindsay's or look to scientific use.
- Phenomenological models cannot number among the stock pool, which means that all legitimate science is given by pre-articulated theories.

These features allow Smith to conclude,

- If no legitimate models come via phenomenological considerations, then none of science can count as legitimate since, "Every model used by classical mechanics is merely phenomenological" (Smith 2001, p. 469).
- The dappled view then is too restrictive.

Smith's criticisms are helpful in that they direct attention to the need to explain what makes a model legitimate. However Smith's supposition that phenomenological models cannot be – or cannot become – stock models for our arguments to succeed seems mistaken.

Few philosophers of science would be willing to suggest that all legitimate scientific modelling proceeds only from established theories. Certainly this is no part of the view defended here. Cartwright et al. (1995), for example, argue explicitly that, "a theory-driven view of models can not account for common procedures used by scientists to model phenomena" (p. 142). Using the example of the London brothers' pre-quantum model of superconductivity in the 1930s, they argue that phenomenological considerations generate important instances of scientific model construction. That model provided an equation defining the domain of superconductivity that, "greatly influenced the development of theoretical treatments of superconductivity for very many years afterwards" (ibid). Existing theory had not been able to account for the Meissner effect, which is "the sudden expulsion of magnetic flux from a superconductor when cooled below its transition temperature" (ibid, p. 144). The London brothers made dramatic ad hoc corrections to the existing electromagnetic model to account for this phenomenon. The resulting model came to be a stock model for a superconducting material in electromagnetic theory at the time (defining the domain of superconductivity), and it was generated by phenomenological considerations.

The distinction between phenomenological and theoretical models is intended to indicate that stock models do not simply derive from existing scientific theory: they are also legitimated by phenomenological considerations. However presenting the distinction this way is too strong to account for the development of actual model construction in science. As Margaret Morrison (1999) has suggested, there are never strictly theoretical or phenomenological models, but all have elements of both. This seems to be in line with the intent of Cartwright et al. (2005), who write,

> Our scientific understanding and its corresponding image of the world is encoded as much in our instruments, our mathematical techniques, our methods of approximation, the shape of our laboratories, and the pattern of industrial developments as in our scientific theories (p. 138).

Cartwright et al. certainly do not advocate a view of science that holds all legitimate modelling derives from established theory. Successful models often incorporate both theoretical and phenomenological considerations. We must be careful, though, about what "success" means in these claims and what follows from it. The London model was successful in that it accounted for the phenomena – and for a while it was the standard model. But it involved the use of functional forms for the electromagnetic field that were not at the time licensed by bridge principles from a description of a superconducting material that could be assigned by independent means. The description of superconductors as ferramagnets, as the functional forms suggested, were ad hoc. So in this case the theory was shown at best to accommodate superconductivity, not to "predict" it. Correlatively, the "success" of the model did not count for much in defending the truth of the theory. So models involving phenomenological elements can be very successful at accommodating phenomena, as well as at a variety of other tasks. But these successes are no indication of the extent of the warranted claims of the theory.

Nor does the view here imply that theory sets its stock models as static. Sometimes we learn that the same model works repeatedly for the same kind of situation. In that case a new bridge principle can be added to the theory, thus expanding the set of "stock models". The London model seems a good example in that it became included among the stock models of condensed matter physics. Thus through successful application a phenomenological model can come to be principled. There is nothing about the view here suggesting that theories are not revisable through scientific practice and the success of a phenomenological model can be a good source of suggestion for changes to the theory.

8.7 Conclusion

Smith proposes three bases for stock models. Considering his third suggestion first, one need not hold that stock models must be derived from existing theory.[6] This contradicts Smith's claim that according to the view defended here legitimate science proceeds exclusively from a set of well-articulated theories. Second, Smith is right to suggest that the proper place to look is to scientific use: there is no guide to principle except successful practice. Stock models are those that appear in the bridge (or "constitutive") principles of the theory. So the question of what legitimates a stock model is really a question of what legitimates a bridge principle. And we have stressed one sine qua non: bridge principles (constitutive principles) are principles of theory. They at least must be empirically well confirmed by the repeated success of predictions from the theory that use the principle in an essential way. Successful modelling provides a pool of stock models. These can be generated through a mix of phenomenological and theoretical considerations, but once a model becomes stock, it gives principled applications of relevant functions of a specified scope. It can affect, moreover, the structure of the theoretical backdrop itself, which is not unrevisable. Third, any reference to a source like Lindsay's text merely indicates one place we might look to find what our stock models are.

Our argument does not deny that CCM can encompass point-particle mechanics and fluid dynamics. Rather, we wish to point out that in moving to the super-theory, the situation does not change substantially. In both cases, we have laws that involve abstract concepts. In the case of Newtonian mechanics the concept in question is force; in the case of CCM, the concepts are those of stress and contact force. The situation with respect to these abstract concepts is not substantially different in CCM than in Newtonian mechanics. In CCM, as in Newtonian mechanics, we apply the abstract concepts of the theory (stress tensor, contact force) by means of stock models (Newtonian viscous fluid, non-viscous fluid) and the associated bridge principles. The range of its stock models used in proper successful prediction fixes the domain over which the theory describes. And so far as we can see there is little positive evidence that the stock models of any one theory can cover all the causes that make an object move.

Smith takes himself to be showing that classical physics has a much larger domain than our view suggests. But his worry is misplaced: the domain of physics is as wide as the successful application of its models demonstrates. This will remain the same in the picture we defend here as it does in Smith's unity picture. An important difference however is that the *dappled view* avoids any pious hope that the successes of the scientific enterprise can somehow warrant claims beyond the scope determined by those successes.

We began this paper noting that Neurath proposed a conception of science that resonates with the view defended here. Of course, Neurath dealt with questions that were pressing in his own time. His target, in general, was metaphysics, understood by him to be unexplicated notions that misleadingly were taken to provide a scientific understanding of our world. Our focus is distinct though connected. The unity Smith argues for indicates a successful unification of scientific endeavours under the abstract principle of a single theory, CCM. However, that practical unification does not warrant application beyond the domains in which we have had empirical success. Like Neurath, we argue that unity does not give us a nice single set of principles from which to interpret the ambiguity of our world. Unification comes from the empirical successes of science. And the warrant we are accorded does not go beyond the domains of that success. Neurath's bill blows in the wind and it may or may not be governed by CCM or by any future theory that we have strong empirical reason to hold true.

Notes

1. Neurath (1935/1983).
2. Smith, S. R. (2001).
3. Cartwright (1999, p. 27). Originally from Neurath, O. (1933/1987).
4. Of there are laws functioning, but we have neither specified them, nor therefore, located the theory that gives the form of their application.
5. Lindsay, R. B. (1961 [1950]). *Physical Mechanics*, 3rd ed, London: D. Van Nostrand Co.
6. Though they of course become part of the theory since the constitutive and bridge principles are essential to it.

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Chapter 9
G. Itelson – A Socratic Philosopher

Gideon Freudenthal and Tatiana Karachentsev

What would we know of Socrates without Plato's work? What would we know of Socrates if Plato had chosen to incorporate what he learned from him into a presentation of his own views and had never named Socrates? What would we know of Socrates if Plato had merely named him in his "acknowledgments"?

These questions are not specific to the Socrates of the fourth century B.C., when the means of preserving texts were as yet rudimentary, for they are relevant to Socrates-like philosophers in the age of the printing press, and even today.

At least three philosophic scholars characterized G. Itelson as "Socrates": Franz Oppenheimer, Otto Neurath, and Otto Buek. The denomination "Socrates" stands for a philosopher whose life and philosophy are one. Socrates is permanently engaged in dialogues, he philosophises. And, conversely, Socrates does not write anything. To Socrates, philosophy is a form of life: it is not only the content of his dialogues, but his life exemplifies his philosophy. At any point, Socrates can fulfil the basic and most important philosophical requirement: *reddere rationem*. In the famous words of Platos's *Apology*, "The unexamined life is not worth living!" "Socrates" devotes his life entirely to philosophy. The historical Socrates cared little for his family, denied honours, and was poor. Moreover, in the moment of truth he chose rather to die than to betray his principles. Similar things – with the exception of this latter decision – will be observed below about Itelson. It was not the case that he chose to die for his principles, He was murdered because he was Jewish.

We shall return to Itelson's life further on in our remarks, but first we should like to think about why, with Itelson's philosophy, we are in a similar situation to the one in which we would have found ourselves in Socrates' case if it had not been for Plato.

Marie Neurath, Otto Neurath's widow, wrote that when he was studying in Berlin, at the beginning of the twentieth century, he took a friend to visit Itelson, "but after several visits the friend stayed away saying that if he went on listening to

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J. Symons et al. (eds.), *Otto Neurath and the Unity of Science*, Logic, Epistemology, and the Unity of Science 18, DOI 10.1007/978-94-007-0143-4_9,

Itelson's devastating criticism he could not go on studying his subject."[1] Ott Neurath himself recollected Itelson saying "What one cannot explain in principle to a taxi driver in his language must be somewhat twisted."[2]

Itelson was evidently at least as strict with himself and although he often gave papers at congresses, he published only two very short, ostensibly historical papers, nothing of his own philosophy, and apparently did not complete any of the studies he embarked upon.

The "Socratic" character of Itelson's philosophising was known to his contemporaries. Aaron Shteynberg writes in his memoirs[3] that for Itelson a philosophical congress was "the realization of democracy in philosophy" and that he "organically" disliked writing, always extemporizing his lectures. Schteynberg reports of a meeting with Itelson and Schestov at the Fourth International Congress of Philosophy in Bologna in 1911. On this occasion, Itelson asked Schteynberg to take notes on his lecture so that it could be included in the proceedings of the conference, for otherwise it would be lost. This is, however, what indeed happened. In fact, almost nothing of Itelson's philosophical work was published. In the proceedings of the various congresses in which he participated, we regularly find the remark that the text of his lecture had not been submitted and thus could not be published. Erich Mühsam writes in his memoirs of "latent talents" of would-be artists who do not create a work of art and defame those who do, and quotes what Itelson, "the witty philosopher," once said to such a person: "Did you ever think about why your plays and novels are unsuccessful? – The only reason is that you never wrote them!"[4] This also applies to Itelson himself[7].

We do not know whether Itelson wrote, whether his literary estate contained completed works or not. Otto Buek reports that at the time of his death Itelson was working on an autobiography and on a series of papers for the Kant-Studien in which he intended to present the sum of his life-long work in philosophy. Oppenheimer says in his obituary (*Frankfurter Zeitung*, 18.6.1926) that Itelson left nothing publishable, but three years after Itelson's death he himself belonged to a committee that was formed in order to publish Itelson's literary estate. It may therefore have indeed contained something worth publishing. However, it is certain that Itelson hardly published anything and no one has yet been able to locate his papers.

Buek's characterisation of his Socratic personality is telling:

> His life had a Socratic style. He had gregarious inclinations, and he maintained contact with the outer world from his small, sparsely-appointed apartment. He sought out other people in their own homes, in philosophical and literary circles, and this familiar figure with his learned, spiritually refined visage enjoyed great popularity, wherever he turned up.[5]

Basically the same picture although in very different colors, was drawn by George Steiner. He portraits Itelson as a "brilliant dialectician," who won arguments against renowned philosophers, but never presented his own view. When asked, he replied that since his youth he has been working on a "revision" of logic. This revision, however, could be written on two pages, which, to Steiner's knowledge, he never wrote. To Steiner, Itelson was no Socrates but an "idler" (Bummler) who spent his days in coffee shops.[6]

Nevertheless, we know the main outlines of Iteslon's philosophy. Already early in his life, when still in Russia, he conceived a "universal science" (*Universalwissenschaft*), and called it "Pantik" or "Panthologie," or "Nomologismus," etc. This universal science was supposed to deal with the most general characteristics of all objects whatever. Soon, it is said, Itelson realized that it is but what Aristotle and medieval philosophers called "logic." In fact, in all likelihood, Itelson conceived a philosophy similar to Logical Empirism a whole generation before the establishment of the Vienna Circle. Neurath says that what he and others in the Vienna Circle called "logical empiricism" was earlier named by Itelson "empirial rationalism." We will discuss below the traces of this "Universal science" in Itelson's few writing and argue that it was directed in the first place against Neo-Kantian dualism.[8]

9.1 Gregorij, Gregor, Gregorius Itelson[9]

The name on Itelson's tombstone and in cemetery records[10] is "Gregorius", and so is also the signature on some extant letters.[11] His two German publications, which we discuss below, are signed "Gregor" Itelson. The preface that he wrote to Felix Eberty's book, *Die Gestirne und die Weltgeschichte* (1923), is signed "Gregorius" Itelson. Oppenheimer uses "Gregor" in his obituary in the "Frankfurter Zeitung" of June 18, 1926. The Russian Newspaper "Rul" and the "Wiener Tageszeitung" of May 6, 1926 use "Gregorij". From "Rul" we also learn the patronymic: Borisovitch. Itelson's personal copy of Salomon Maimon's book *Philosophisches Wörterbuch oder Beleuchtung der wichtigsten Gegenstände der Philosophie in alphabetischer Ordnung* (1791)[12] bears the handwritten signature "G. Jtelson". "Gregorij" was hence presumably the name given to him at birth and "Gregor" is a "Germanizing" form, whereas the Latin "Gregorius" expresses a reservation with respect to belonging to either Germany or Russia.

9.1.1 Life in Russia and Russian-Jewish Circles in Berlin

We know next to nothing about Itelson's life in Russia. Let us first quickly summarise what we *do* know about him. Gregorij Borisovitch Itelson was apparently born in 1852 in Zytomir in the Ukraine, which was at that time Russian Not even this detail is certain.[13] Zytomir was then a small town with a large, liberal Jewish community. Itelson is said to have studied natural sciences in St. Petersburg, and, like many others, emigrated around 1884, following the pogroms and the discriminatory policy of the Russian government towards Jews. He went to Berlin, where he stayed until his death in 1926.[14]

We hear that he graduated from the faculty of natural sciences in St. Petersburg and was a student of Mendleev, but not even these details could be confirmed.[15] From a letter that Betrand Russell wrote to his wife in 1908, we learn that in Russia he must have been involved in oppositional political activities of some sort. Itelson told Russell that he lived in Berlin "because he found that if he stayed in Russia any longer he would be sent to Siberia, which would put an end to his pursuit of logic".[16] More is not known.

Of his political views in his years in Berlin, we have only one testimony, apparently from the 1890s, and a more general second statement. From this, it emerges that he was involved in discussions between Russian revolutionary socialists and Zionist students – and was opposed to Zionism. In an unpublished memorandum on the Zionist leader, Nachman Syrkin, Eliyahu Davidzon reports of public discussions in a "Russian-Jewish Scientific Association" "between Zionists and socialists, both Marxist and Narodniki." Davidzon singles out Itelson among the anti-Zionist discussants:

> A man of encyclopaedic knowledge, gifted with an acute and brilliant intelligence. He did not belong to our generation [Syrkin was born in 1868, Itelson in 1852]. He had graduated already in the [eighteen] seventies from the faculty of natural science in [St.] Petersburg, came to Berlin, and settled there. I remember his contribution to the discussion of the question of whether the Jews are a nation or a religious community.
>
> In the course of the discussion the attempt was made to answer the question according to the criteria defining nations that we learned in academia, and to determine whether they are fulfilled by the Jews. We believed that a lot depended on the answer to this question. For, if the Jews are a nation, then they should strive for national and political independence, i.e. for a Zionist solution, but if they are not a nation, then...
>
> Itelson pointed out the futile casuistry of the very posing of the question.[17] The Jews are a nation and also not a nation, depending on what is included in the definition of the concept. In zoological books, the chicken is defined as belonging to the family of birds, but the Russian folk proverb does not admit this definition. [The reference is to the Russian folk proverb: A chicken is not a bird and a woman is not a human being.] Here N. Syrkin jumped up and declared: "While the stupid and arrogant cocks loudly announce that the chicken is not a bird, it will fall prey to the eagle".[18]

As far as we know, Itelson never held a regular position. He must have lived off private lessons and translations. We know that he translated into Russian three books by Einstein, with whom he was acquainted.[19] Itelson also translated at least one play into German.[20] Buek writes in his obituary that Itelson translated for the "Patent-Amt" (Patent Office), but this cannot be verified.[21] In his last years (and perhaps earlier, too), he also lectured on philosophy at the Jüdische Volkshochschule and to Russian immigrants in Berlin.[22] Not only in obituaries, but also in memoirs of the time, Itelson is mentioned with great respect as an important philosopher, despite the fact that he published almost nothing.[23]

9.1.2 The Socratic Life

Franz Oppenheimer, the renowned sociologist, who published an article on Itelson immediately after his death and gave a speech commemorating him on the third anniversary of his death, also compared him to Socrates.[24]

In this commemorative speech Oppenheimer says:

> When we talk of him, we, who loved him, then the lament is heard that he died without writing his "Logic", that he did not fulfil the task which he had set himself, the lament, that nothing has remained of him besides the two famous formulae [i.e. his definitions of logic and of mathematics; see below] with which he had inscribed his name into the book of eternity.
>
> It seems to me – writes Oppenheimer – that this lament is justified in one sense and entirely unjustified in another. It is justified if we think of ourselves; if we consider how much richer we would have been if this book had been written as the fruit of a long life dedicated exclusively to thinking on these highest issues. But the lament is unjustified if we do not think of us but of him. It is unjustified if it should mean that this man, this Gregor Itelson, would have been more perfect if he had written the book of his dreams. He could not have been more perfect.
>
> We easily forget these days, when only externally visible, measurable achievement counts, that there are values of a different kind than achievements, values that are just as high: the values of Personality. A work of art can be formed not only out of stone or metal; we can form ourselves into works of art (pp. 1–2)

So far Oppenheimer. Indeed, these words are backed up by numerous stories and anecdotes from Iteslon's life.

Oppenheimer describes Itelson's impoverished apartment in which there was a bed, a chair, a table – and 5,000 of the finest philosophical books. He tells us that when Itelson wanted to buy an expensive book he would fast half-days for a fortnight in order to save the necessary money.[25] The financial arrangement the Berlin friends of the National Library Jerusalem proposed to Itelson for his library testifies to its high value.[26] To Russell, Itelson spoke of his "noble passion for old books."[27]

Buek says that Itelson's style of life "was without ornament and simple" and determined by his "renunciation of all external splendour, of earthly success and honours".

No doubt, here is somebody who gave the people who knew him the impression that he formed his life and his thought according to his principles as an integral whole.

In Oppenheimer's words:

> He was a free man, who freed himself also from himself, who commanded his animal nature and hardly noticed that the beast obeyed, so much did "disciplining his drives" become his nature.

In 1906, Otto Neurath, who had just written his PhD dissertation, wrote a long letter to the renowned sociologist Ferdinand Tönnies in which he described the four men who had the greatest influence upon him. The second was Itelson: "This man", Neurath writes, "did not so much direct me to intensify my thinking, but rather to let my thinking be active whatever else I do, to take everything seriously, etc., he is in a certain sense a second Socrates, and yet an outstanding scholar in different sciences. He is a bachelor, too, and I believe that his being single has a fatal influence on the man, it isolates him internally, he is used to isolating himself inwardly."[28]

9.1.3 A Socratic Argument

Let us first quote an argument of Itelson's which reflects his Socratic nature and his way of philosophising in dialogues: Itelson once discussed with a solipsist, i.e. somebody who maintains that his existence is evident but that all his ideas of external objects, including humans – and including of course his interlocutor – are to be considered as merely his ideas and without objective reference, i.e. as not objectively real. After a long debate, Itelson concluded:

> At most you will succeed in convincing me that you are my idea, but never that I am merely an idea of yours.[29]

This certainly is a witty answer, but it is also an excellent argument. The lesson is that solipsism depends on philosophising in monologues and that it is self-refuting if presented in a dialogue. This is so not only because it makes little sense to attempt to convince someone who is one's mere idea, but also because if one succeeds in convincing "him" (or "her"), then he/she must doubt one's own existence, yet the certainty of one's existence was the starting point and presupposition of the entire move. The argument of solipsism therefore makes sense only in the style of a monologue. It does not seem far-fetched to suggest that Itelson's argument is rooted in his dialogical way of doing philosophy, in his Socratic form of life.

9.1.4 Death and Bequest

The Wiener Tageszeitung of May 6, 1926 reported:

> Professor Itelson who in spite of his age had enjoyed what seemed to be unshakable health until a few months ago and had demonstrated unflagging energy for work, had suffered since December 1925 from the consequences of an anti-Semitic attack in front of the Nazi pub "Wilhelma" on the Kurfürstendamm. On Christmas Eve, severe anti-Semitic riots took place there during which the aristocrat Baron v. Engelhardt attacked Itelson with the words "Beat the Jews to death!" and hit him until the 74 years old scholar lay severely injured on the street and had to be brought to hospital and treated there. The old man has now died in consequence of the direct and indirect results of his injuries.[30]

It does not lack a touch of bitter irony that Itelson, who had left Russia following the pogroms of the 1880s, was ultimately beaten to death by an anti-Semite (a "völkischer Rohling", as Oppenheimer wrote in 1926) in Berlin. Moreover, the reaction of Jewish friends of Itelson's was very typical of the Jewish mentality in Weimar Germany. Franz Oppenheimer said in 1929: "We do not want to let our high spirits today be spoiled by recalling the disgraceful events which accelerated if not also caused Itelson's death by insulting his human dignity."

At the same commemorative event in 1929, a committee was appointed which undertook the task of erecting a tombstone on Itelson's grave and publishing his papers. Most members of this committee belonged to the Jewish-Russian-German community in Berlin: Jacob Teitel, Franz Oppenheimer, Emmanuel Lasker, David M. Koigen, M. N. Schwarz, Aaron E. Shteynberg, A. A. Goldenweiser, and B. N. Elkin.[31] The tombstone was indeed placed on the grave. The inscription reads: Gregorius Jtelson. Dem Denker und Lehrer. A few years later, the members of this committee were dispersed all over the world or murdered. To this day we have not succeeded in securing even one page of Itelson's literary estate. His marvellous library was donated to the University and National Library of Jerusalem. Two catalogues were prepared of it, one in Germany before shipping and one in Jerusalem upon arrival, but they are not extant and it has proved impossible to reconstruct the inventory of the library.[32]

9.2 Philosophical Work

Itelson may have dedicated his life to philosophical work, but he published no philosophical works. We owe the little we know to two lectures given at a conference, two predominantly historical papers in a journal, and an essentially historical preface to a book he re-published. We shall go through these sources briefly and show that they do cohere in a general picture or orientation of his philosophy, although, of course, lacking specifications.

9.2.1 The Definitions of Logic and Mathematics

Franz Oppenheimer said that Itelson "inscribed his name into the book of eternity" with his definitions of logic and mathematics. However, Itelson never published these formulae himself. They are quoted in a *compte rendu*, written by Louis Couturat, the Chairman of the session "Logic and Philosophy of Science" on September 5, 1904 at the International Congress of Philosophy, in Geneva. Couturat summarises two lectures by Itelson: "The Reform of Logic" and "Logic and Mathematics". As always, Itelson gave a quick survey of the history of his topic, "displaying great erudition", as Couturat says. Then Itelson addressed the question of the definition of logic. He rejected the widespread definition of logic as the science of the laws of thought, since this may mean the "natural," empirically given laws of thought (which often produce errors) or "normative" laws, according to which one ought think. However, the definition of normative laws requires the notion of "truth" and this would have to be defined independently of logic. Therefore, logic should not be defined as the science of the laws of thought. It should be defined not as pertaining to thought, but to its proper object. "Logic is the science of objects in general." But isn't this ontology? No, says Itelson. Ontology is the science of beings, of existent objects, whereas logic is "the science of all objects, existent or not, possible or impossible."[33] The wording of the definition, the reference to possible and impossible objects, suggests an affinity to Meinong's conception, and indeed, at least one reviewer of Meinong's relevant book and Meinong himself also acknowledged the fact.[34] Itelson proceeds with the observation that the hold of "psychologistic logic" is so strong that even Husserl, who dedicated the first volume of his *Logical Investigations* to a critique of psychologism, filled the second volume with psychologistic considerations. He was not the only one to think so. Louis Couturat reported Husserl of Itelson's critique – and reinforced it. It is not surprising that these remarks annoyed Husserl.[35] Itelson also took care to distinguish logic from epistemology. This was of course an attack on the Neo-Kantian concept of "transcendental logic" in which logic and epistemology converged and "general" or "pure" logic was practically neglected. "Pure logic must of course precede transcendental logic", says Itelson in the discussion.[36] In the next session, Wilhelm Windelband delivered his lecture on "The Present Problem of Logic and Epistemology in relation to the Sciences of Nature and of Culture". In the discussion, Itelson criticized the fact that Windelband had not said anything about the relation between logic and mathematics and Itelson repeated that logic may not be confounded with epistemology.[37] If Windelband replied to the criticism, it was not mentioned in Couturat's report. In the same year, however, Windelband had an opportunity to retaliate. In the Festschrift for Kuno Fischer, he wrote a lengthy paper entitled "Logik". There he ridicules "mathematical logic" as a "logical sport" that knows nothing of the real work of science.[38] Itelson owned a copy of this book and marked the relevant sentences with two exclamation marks and a "NB" (nota bene!).[39]

The anti-psychologistic tendency of which Itelson was part, conceived of logic and mathematics as entirely independent of human thinking. At the beginning of the twentieth century, this was an influential trend and Itelson belonged to its first and clearest advocates. The alternative, often associated with Christoph Sigwart's *Logik* (1873 and many more editions), considered logic as studying the "laws of thought". Understood in this way, so the critics said, logic becomes a scientific discipline describing empirical matters of fact, and its distinction from psychology is then unclear. If there are no people who think, logic loses its subject matter.[40] Moreover, as a discipline dealing with empirical matters of fact, i.e. human beings and their thinking, logic may be true or not, but it is certainly bereft of the apodictic nature which we usually ascribe it, the character of necessary, logical truth. If, however, logic is conceived as a discipline that formulates the rules of how people should think, then it also loses its claim to truth. Rules cannot be either true or false.

It is important to remember that these lectures were part of Itelson's life-long work on logic, which was intended to result in a comprehensive systematic account of the topic and presumably also a history of this discipline.[41]

In his second lecture, Itelson ventured to define mathematics in contradistinction both to previous definitions and also to logic. Mathematics was traditionally defined as the science of magnitude, but the objects of modern mathematics are not only magnitudes. They are also not objects as such. The objects of modern mathematics form "sets governed by certain laws", thus allowing this discipline to be defined as the "science of ordered objects".[42]

A final point should not be forgotten. Itelson remarked that if logic be named after its object, i.e. objects as such, then it should be called "Pantik" or "Pantology". But if it should be named after its method, then "Logistic" is the term to be used. In his *compte rendu*, Couturat remarks that it is remarkable that three scholars of three different tongues independently suggested the same word: Itelson, Lalande, and Couturat himself.[43] Itelson was evidently not convinced of this independence and coincidence. In his conversation with Russell in 1908, he kept complaining that Couturat "had bagged from him without due acknowledgment the word 'logistic'. . .. As long as he kept off questions of priority, he was charming", concludes Russell.[44]

9.2.2 Psychophysics

In 1890, Itelson published in the "Archiv für Geschichte der Philosophie" one of the two articles that appeared during his lifetime: "Zur Geschichte des psychophysischen Problems."[45] The paper seems of little philosophical value. Itelson himself refers to it as "notes" (Notizen). It begins with a paragraph of a few lines stating that the arguments raised against Fechner's measurement of the intensity of sensation have a pre-history, as the following "notes" set out to show. The notes refer to Malebranche, Ploucquet, Pasquale, Galluppi, Joh. Aug. Eberhard, and close with a remark about August De Morgan.

Yet the paper is indeed of interest, since the question of what "Fechner's Law" measures was considered crucial to whether the "mind-body-problem" had been solved or not. The re-emergence of "Idealism" associated with Neo-Kantianism in Germany was closely connected to the criticism directed at Fechner. Itelson mentions in his opening paragraph the critiques on the part of Jules Tannery, J.v. Kries, "and others" whom he does not name. However, the bulk of the paper discusses Kant's position on psychology, and the paper closes with the following promise:

> I shall extensively discuss all the points touched on here in a work (Schrift) entitled: "Kant's mathematical principles and the psychophysical problem" (1890: 290)

Needless to say, this work never appeared. However, it shows that Itelson wished to contribute to contemporary philosophical discussion and that, like his Neo-Kantian contemporaries, he used Kant to discuss systematic questions. It is not difficult to see that the discussion of "Kant" was actually a critique aimed at Hermann Cohen who, for his part, criticized Fechner as part of the project of re-establishing (Neo-) Kantian Idealism.[46] Itelson hence takes the opposite position. Itelson opposes the attempt to sever the "mind" from nature and thus exempt it from scientific study and reserve it for philosophy. His interest in psycho-physics is also apparent in his first paper, "Leibniz und Montaigne",[47] where he traces the notion of "petites perceptions" back to Montaigne. Of course, "petites perceptions" belong to the pre-history of psycho-physics. Also secondary perceptions (e.g. hearing colors) belongs to this area. In a short review of four publications on this phenomenon, Itelson corrects the bibliographical references concerning Fechner (!) and closes with the remark that in his view the whole topic is less important than the authors of the works under review believe. Itelson promises to present the reasons for this judgment in a paper to appear soon in the same journal. Of course, no such article ever appeared, although apparently Itelson studied this phenomenon.[48]

Also his next and last "publication" shows the wish to solve philosophical questions on a unified scientific basis that does not recognize the division between "Naturwissenschaften" (natural sciences) and "Geisteswissenscahften" (humanities).

9.2.3 Empirical Rationalism (or: Eberty's Thoughts on Space, Time and Eternity)

In 1923 and again in 1925, Itelson published a new edition of Felix Eberty's booklet *The Stars and World History. Thoughts on Space, Time and Eternity* that had first appeared in 1846, then again in 1874. In the interim, the book had appeared in an English translation, which was then re-translated into German.[49] In a style that had not changed since his paper on psychophysics, Itelson characterised the issue in question in one passage and then added three pages of references to predecessors of Eberty's ideas.

Eberty proceeded from the idea that since light travels with finite velocity, an event and its perception are not simultaneous. The time gap depends on the spatial distance. At every point in time all events of history can be simultaneously perceived in different points of space. Space can be seen as an "archive" of all events. Since God is omnipresent, He sees all events of history as simultaneously present. Moreover, since the future is determined by the past, God also sees all future events. This illustrates His omniscience. Now, time and space are measured against standard measures. Measured against infinity, the entire world and its history are reduced to a point and an instant without changing the ratios between distances and temporal intervals within this world. From within we would not know the difference. Space and time are hence human forms of intuition, and although we cannot dispense with them, we should seek the perspective from which the entire world can be conceived *uno intuitu* as God sees it, as emerging from one creative thought.

As in the paper on the pre-history of psychophysics, Itelson establishes a current implication:

> The problem of space and time has occupied the thoughts of scientists and philosophers since ancient times. Since the conception of the theory of relativity, this problem has moved into the foreground of general interest. However, for the lay person, comprehending the problems involves difficulties, and therefore any successful attempt to help understand it should be welcome. From this perspective, the new edition of the present small treatise, which sheds light on the problem mentioned in a unique and stimulating way, is justified.[50]

The connection with the theory of relativity was emphasized by a very short preface by Einstein who did not suppress his reservations with regard to the book:

> This booklet, written by a person of esprit who thinks originally, is not without current interest. For it shows on the one hand a critical attitude with respect to the traditional concept of time, and it shows on the other hand from what peculiar conclusions the theory of relativity saves us, which of all things is reproached by many because of *its* bizarre conclusions (A. Einstein, 5 June, 1923).[51]

Two central ideas seem to have interested Itelson. First, that allegedly philosophical questions can be conceived and answered in the framework of science. This project presupposes that “dualisms” of realms, of mind and body, of understanding and explanation, of freedom and necessity, are not accepted. Second, that this scientific framework can itself be reduced to logic, such that space and time are reduced to a point, causal relations to implications: The world proceeds from one creative thought. We remember that Itelson named his philosophy “Panthik”, “Panthologie”, “Nomologismus”, and “Empirical Rationalism”. It seems to us that these thoughts of Eberty show how all these ideas cohere in a philosophical programme. Itelson did not work out this programme himself, however, but rather published Eberty’s booklet and added historical “notes” on predecessors of these ideas.

Otto Neurath, who names Itelson as the strongest influence on his “intellectual development towards a comprehensive world-view”, refers to Eberty (in Itelson’s edition) as providing an example of “inspired analysis” and as being a “precursor of the logical-empirical attitude” that had contributed more to scientific and philosophical progress than systematic philosophers had done.[52] Neurath certainly correctly characterised both the tendency of Eberty’s book and Itelson’s intentions in re-publishing it. This also shows in Paul Feldkeller’s review of the booklet that appeared in the “Kant-Studien”. Feldkeller, who also indignantly wrote to Einstein about the re-publishing of the book, emphasised in his review the “sensualistic and materialistic” character of the work. The author’s “concept of history as well as of God originate in a bygone world.”[53] Itelson and Neurath sought to resurrect this world.

> My intellectual development towards a comprehensive world-view”, says Neurath, “was influenced by Mach, Poincaré, and other modern thinkers, and especially by Gregorius Itelson. My central conviction became that the elaboration of the differences between the various sciences is unessential talk, but that, on the contrary, it was especially important to develop an account of all the sciences using only one kind of scientific “style.” That is to say, I became convinced of the possibility of speaking about the stars and about men with the same logical techniques and with the same scientific dispassionateness.[54]

In fact, it seems that Itelson conceived a philosophy similar to logical empiricism in that it combined empiricism with the extensive application of logic and mathematics. He named “empirical rationalism” that which Neurath and others in the Vienna Circle called “logical empiricism” a generation later. Indeed, Neurath explicitly characterizes Logical Empiricism as essentially the same as Itelson’s approach to philosophy:

> The “rationalism” that we reject as a metaphysical principle, as a supreme judge in Leibniz, is descending as it were to the level of science. The extent to which the auxiliary means of logic and mathematics are applicable when we wish to make predictions is shown to us precisely by experience. “Formal logic,” which is mocked so much, will now become a major tool of committed empiricists who, what is more, are setting out to conquer the whole domain of science and reserve no propositions for that which one once called “metaphysics”. Gregorius Itelson aptly named this attitude “Empirical Rationalism” in contradistinction to former “Metaphysical Rationalism.”[55]

Looking back at the very few philosophical traces Itelson left, we nevertheless recognize the major tenets of Logical Empiricism: On the one hand, the emphasis on strict definitions of terms proceeding from actual experience and critique of metaphysical parlance, on the other hand, the adaptation of sound insights of metaphysics to scientific discourse. We also recognize the emphasis on logic and mathematics and, finally, the stress on the unity of science. This is the philosophical program underlying Itelson's interest in Fechner's Psycho-Physics. Psycho-physics unifies the natural sciences with philosophy and opposes the renewal of Idealism and metaphysics in general and in its Neo-Kantian garb in particular. Otto Neurath certainly did not exaggerate the influence of Gregorius Itelson on his thought.

Notes

1. Otto Neurath: Empiricism and Sociology, Marie Neurath and Robert S. Cohen, eds., Dordrecht (Reidel), 1973, p. 7.
2. "The Orchestration of the Sciences by the Encyclopedism of Logical Empiricism," *Philosophy and Phenomenological Research*, Vol. 6, No. 4 (June, 1946), pp. 496–508, 500.
3. The Friends of my Early Years (1911–1928) (Russian), by Aaron Shteynberg, Paris (syntaxis) 1991, Chapter 9, http://nivat.free.fr/livres/stein/09.htm

   Aaron Zakharovich Shteynberg (1891–1975) was born in Daugavpils (Dvinsk), Latvia. He graduated in law and philosophy from Heidelberg University. From 1918 to 1923 he was Professor of Philosophy at the St Petersburg Institute of Philosophy. He moved to Berlin in 1922, where he lived for 10 years. In 1934 he moved to England and worked for the World Jewish Congress, acting as head of the Cultural Department from 1948 until his retirement in 1971.

   Leo Schestov (original name: Leo Isaak Schwarzmann) 1866–1938, was born in Kiev and studied in Moscow. In 1919 he emigrated to Geneva. From 1920 he was Professor at the Russian Institute of the Sorbonne.

   Shteynberg relates that on the same occasion Schestov teased Itelson and asked: "Why don't you write yourself? Presumably the thread [of thoughts] would be so thin, that it would break. With Kant and Hegel the threads are so thick and strong that you cannot cut them even with a knife, but yours..." Itelson took the posture of a male cat ready for fight, but he felt guilty. After all, he was the author of a joke at the expense of Schestov: Pyat' durakov slusha'ut shestogo (shestogo = sixth in Russian; it is pronounced like Shestova) – Five fools listen to the sixth."
4. Erich Mühsam, *Unpolitische Erinnerungen*, Düsseldorf (Brück-Verlag) 1961, S. 18, 26. The Italian ring of the German title of this chapter in Mühsam's book „Latente Talente" is lost in English. Mühsam also mentions that Itelson "died recently from the consequences of a brutal attack on the part of Hakenkreuzler" (loc. cit. S. 18).
5. Buek, p. 429. In a letter to Gottfried Salomon Delatour (1892–1964) dated April 18, 1919, Itelson typically writes that Salomon Delatour may come to him for a „disputation. " („... gegen Ende der kommenden Woche können Sie einmal bei mir „disputieren"). Literary estate of Gottfried Salomon Delatour, No. 547, Internationaal Instituut voor Sociale Geschiedenes, Amsterdam.
6. See George Steiner, *Fachwissenschaften und Anthroposophie* (8 Vorträge, 11 Fragenbeantwortungen, ein Diskussionsbeitrag und ein Schlusswort, Dornach und Stuttgart 1920 und 1921), 435–436. This is No. 73a in the bibliography of George Steiner's writings: Christian Karl, *Handbuch zum Werk Rudolf Steiners*, zweite, elektronisch Auflage 2007–2010: http://www.rudolf-steiner-hand-buch.de
7. See e.g. the obituary by Otto Buek, Gregorius Itelson, Kant-Studien, vol. 31 (1926), 428–430.

8. See Otto Neurath, "Die neue Enzyklopädie des wissenschaftlichen Empirismus (1937), Gesammelte Schriften II, 801–811, 803. See also his "The Orchestration of the Sciences by the Encyclopedism of Logical Empiricism," *Philosophy and Phenomenological Research*, Vol. 6, No. 4 (June, 1946), pp. 496–508, 500.

   Bueck mentions Itelson's idea of a universal science which Itelson called "Pantik" or "Panthologie" and of which he later realized that it was traditional logic. Alfred Klee, a reknown zionist leader, spoke on the commemoration of 1929 of Itelson's philosophy of the all-embracing and omnipresent („Allumfassenden, Allgegenwärtigen"). See Juedische Rundschau, Nr. 37, May 14, 1929. D. M. Koigen spoke on the same occasion of Itelson's „Universalwissenschaft" (See Rul (Russ.) No. 2572, of May 15, 1929, p. 4.) On the IVth International Congress of Philosophy in April 1911 in Bologna, Itelson delivered (on April 10) a lecture with the title "Grundzüge des Nomologismus". We take „Nomologismus" to be yet another name for his Universal Science.
9. There are two "grey papers" on Itelson. The first is by Christian Thiel "Gregorius Itelson," the second by Volker Peckhaus, "Ergänzungen zu Gregorius Itelson". Arbeitsberichte aus dem DFG-Projekt *Fallstudien zur Begründung einer Sozialgeschichte der Logik* Nr. 3 (resp. 10) (Juni 1986) (resp. Mai 1987), Kolloquium v. April 14, 1986 (resp. Kolloquium v. June 25, 1986).
10. The Jewish cemetery of Berlin Weissensee, grave number 71009 in G6.
11. To Ferdinand Tönnies, October 1 and 26, 1908. Landesbibliothek Kiel, Nachlass Tönnies. To Gottfried Salomon – Delatour, April 18, 1919. Literary estate of Gottfried Salomon Delatour, No. 547, Internationaal Instituut voor Sociale Geschiedenes, Amsterdam.
12. Jewish National and University Library, Jerusalem.
13. One document of the Jewish Cemetery in Berlin has first January 27, 1852 as his day of birth, which is corrected to January 18, 1852. Another document, "Beerdigungs-Anmeldung für Friedhof Weissensee", has January 27, 1851 in "Kovno", corrected to "Zytomir/Wollonien". The "Bescheinigung über Eintragung eines Sterbefalls" has April 1, 1852 in Zytomir and so does the document which states that the police was notified of the funeral. However, Mr. Efim Melamed of Zytomir states (e-mail from August 29, 2000) that the name of Gregorij Itelson could not be found in the town records of 1852.
14. A rather surprising connection with the pogroms is shown in the following detail from Itelson's life. In response to a request published in the newspapers by the composer Anton Grigorievitch Rubinstein (1829–1894) to submit a libretto on a modern Jewish theme, Itelson, who was then a young student, presented a libretto on the persecution of Jews at the time of the crusades in the Middle Ages. It says that this was "in the last years of Rubinstein's life", hence rather soon after the pogroms. The allusion to the pogroms must have been more than clear. Rubinstein rejected the libretto and suggested that Itelson write a new one in which the hero would be a Jew similar to Figaro. "No more laments," said Rubinstein. "Give us a cheerful Jew, who is full of the joy of life and mocks them [the non-Jews]. See G[ershon?] M. Svet: "Jews in Russian Music", in: "A Book on The Russian Jewry. From the 1860s to the Revolution 1917. A Collection of Essays. Union of Soviet Jews, New York 1960 (Russian), p. 457.
15. Otto Buek reports that Itelson first studied "theology." In the German Encyclopedia Judaica, vol. 8, Berlin 1931, S. 706, M. Schwarz (who later belonged to the committee which undertook the publication of Itelson's literary estate) writes that Itelson studied "Jewish theology" in his home town. In Zytomir there was a seminary for teachers and rabbis. Itelson's name does not appear in the list of the graduated rabbis. The list of graduated teachers is not extant. He may have studied there and left before graduation (e-mail from Verena Dohrn to Gideon Freudenthal, January 9, 1999.) On his studies in Petersburg, see Shteynberg Chapter 9. In April 1999, Prof. Kolchinsky Eduard Izrailevich (Director of the Institute for the History of Science and Technology, Universitetskaya naberezhnaya 5, 199164 S.-Petersburg, Russia) informed Gideon Freudenthal that he could not verify this information.

16. The Selected Letters of Bertrand Russell, Volume I. The Private Years, 1884–1914. Edited by Nicholas Griffin. London: A. Lane, Penguin Press, 1992, p. 308.
17. It is therefore surprising to read that Hessenberg writes to Nelson in 1908 of Itelson's "talmudische Definitionssophistik". Hassenberg to Nelson (15.5.1908; Geheimes Staatsarchiv Preussischer Kulturbesitz, Berlin, Nachlass Nelson, 1/LN AA000271-1/LN AA000273). We owe this reference to Volker Peckhaus.
18. Eliyahu Davidzon, From my Memoirs of Nachman Syrkin, Appendix to Varda Pilovski, *Nachman Syrkin*, PhD Dissertation, Hebrew University Jerusalem 1974, pp. 165–174, 170–171. Translated from the Hebrew.
    On January 21, 1920, Professor Heinrich Loewe began a letter to Itelson with the following words: "I suppose, that you still remember me. You know me from the early times of the – National Jewish Student Movement, from the debates (Redekämpfe) in the "Russian-Jewish Scientific Association."Archive of the National Library Jerusalem, 4°793 275/III.
19. Itelson translated several works by Einstein into Russian. These were published in three volumes with the Russian publishing house "Slowo" in Berlin. Some details concerning these translations can be found in the "Einstein-Archive" in the Jewish National and University Library of Jerusalem. We are grateful to the Archive's staff for their kind help. Of special interest are first Einstein's remarks concerning Itelson, then his financial generosity towards him. Einstein writes in a short preface to Itelson's translation of his "Über die spezielle und die allgemeine Relativitätstheorie", 1917, that "der von uns hoch geschätzte Herr Itelson fuer eine ausgezeichnete Übersetzung buergt." (Mr. Itelson, whom we regard highly, is a guarantee for an excellent translation.) (Draft in Einstein's handwriting, November 9, 1920.) A letter from the publisher of the original German edition, Friedrich Vieweg & Son, to the Russian publisher Slowo (May 1921) shows that Einstein was concerned about Itelson's income: "Herr Professor Einstein macht ferner zur Bedingung, daß Herr Itelson mindestens mit 3% vom Verkaufspreise für seine Übersetzung von Ihnen entschädigt werde." (Professor Einstein further makes it a condition that Mr. Itelson receive at least 3% of the retail price of his translation as remuneration). This was in addition to Itelson's honorarium. In a letter to Dr. Paul Feldkeller, who wrote to Einstein with indignation concerning a mistake in a question of astronomy in Eberty's book, which was republished by Itelson in 1923 and 1925 with a forward by Einstein (See letter of Feldkeller to Einstein, March 3, 1927; Einstein Archive 15-054), Einstein concedes that the book contains grave mistakes but adds: "Ich schrieb das kleine Vorwort dazu nur, um dem von mir geschätzten alten Itelson einen Herzenswunsch zu erfüllen." (I wrote the short preface only in order to fulfil a dearly-held wish for old Itelson, whom I highly regard.) (March 27, 1927 Einstein Archive, 25-056.) Einstein repeats the expression „der alte Itelson" in a letter to Betty Neumann of November 27, 1923, No. 120/897 The Einstein-Archives. Einstein was also present on Itelson's funeral. See Rul no. 2563 of May 3, 1929.
20. We are grateful to Renate Heuer (Archiv Bibliographica Judaica, Frankfurt/M) for the following information. In the "Collection Steiniger" (Steininger Sammlung), Abt. IV, Publizisten und Geisteswissenschaftler, 90/4, Itelson is mentioned as the translator of [N. V. Gogol?] "Die Spieler". Deutsch von G. I'. o.O. o. J. o.V. (II) + 301-357 S. – Motto: "Geschichten aus alten Zeiten. . ." – Den Bühnen gegenüber Manuskript. Stempel: Genehmigt für das Neue Volks-Theater, Bln, d.11.7.1912. [Theaterzensur-Exemplar, Slg Bln. The copy was apparently extracted from a book and bound. The author is not mentioned.]
21. Otto Buek, 428–430. Peckhaus, "Ergänzungen zu Gregorius Itelson", of June 25, 1986.
22. See the index to *Chronik russischen Lebens in Deutschland 1918–1941*, Karl Schlögel, Katharina Kucher, Bernhard Suchy und Gregur Thum (eds.), Berlin (Akademie Verlag) 1999.
23. We will quote some of the references to Itelson. The list of his publications is indeed short:

    1. Leibniz und Montaigne, Archiv für Geschichte der Philosophie, Bd. II (1889), 471–472
    2. Zur Geschichte des psychophysischen Problems, Archiv für Geschichte der Philosophie 3 (1890), 282–290

This paper was reviewed by Hermann Ebbinghaus in: Zeitschrift für Psychologie und Physiologie der Sinnesorgane, Bd. I (1890), 127–128

The paper appeared also in French: "Sur l'histoire du problème psychophysique", Revue philosophique de la France et de l'étranger, 36 (1893), 224.

24. The commemoration took place on May 5, 1929 in the Gutmann Hall (Bülow Str. 101) in Berlin. Neither this speech nor the others were ever published. The text we quote is a typescript of seven pages and bears the title "Gregor Itelson". It is kept in the Central Zionist Archives, under # A 161/107. It is our conjecture that this is the text of the commemoration.
25. Franz Oppenheimer, "Gregor Itelson", Frankfurter Zeitung June 18, 1926, Nr. 445, S. 1–2.
26. See the details below.
27. Whereas Itelson's friend, Oppenheimer, gives a rather realistic picture of his poverty, Russell's report to his wife reveals more about himself in this respect than about Itelson: "He is very poor, and lives in an apartment without any servant [!]. . . I gave him a good dinner, which I imagine he had not had for a long time."
28. Otto Neurath to Ferdinand Tönnies, June 25, 1906. Gideon Freudenthal adds: Otto Neurath's widow, Marie Neurath, showed me this correspondence some 20 years ago in her home in London. I cherish the memory of this encounter.

    There are two letters of Itelson to Tönnies in the Schleswig-Holsteinische Landesbibliothek in Kiel (of October 1 and 26, 1908). Itelson mentions in these letters their personal encounter in Heidelberg. However, Neurath had left Berlin already 3 years earlier.
29. Quoted in: Franz Oppenheimer, *System der Soziologie*, 1923, Bd. I/1. Reprinted: Stuttgart (Fischer) 1964, p. 196.
30. The document containing the police permission to bury Itelson, dating from May 3, 1926, certainly supports the assumption that he died in sequel to his injuries. It is stated here that he died in consequence of an embolism in his lungs ("Lungenembolie").
31. The members of this committee are named in Rul no. 2563 of May 3, 1929, p. 5.
32. The correspondence concerning Itelson's library between Prof. Julius Citron, of the "Deutscher Verband zur Förderung der Universität Jerusalem" in Berlin and Dr. Shmuel Hugo Bergman, then head of the Jewish National and University Library Jerusalem, begins on June 27, 1926 and ends on June 22, 1927. It can be found in the Archive of the Jewish National and University Library Jerusalem in two files with the correspondence between the library and the Verband in Berlin. The inscription says "Berlin vom 1.7.1926 bis. . ." Itelson's sister, Marie Ratner, donated the library to the "Verband" (letter of June 27, 1926; see also "Tätigkeitsbericht der jüdischen National- und Universitäts-Bibliothek in Jerusalem. September 1926–April 1927; Arc. 4_ 973 213 [I]). Itelson's adopted daughter, Sonja, received a modest sum in compensation for her claims. We have not succeeded in finding any further details about her. The collection consisted of ca. 5,000 books and especially the works of scholastic philosophers were praised by Bergman. On September 18, 1926, Citron informs Bergman that "die Bibliothek ist zum grossen Teil bereits katalogisiert. Der Katalog gelangt in ca. 14 Tagen (. . .) an Sie zum Versand." (The library has been to a large extent already catalogued. The catalogue will be sent to you in approximately 14 days [. . .]). The books arrived in Jerusalem in October 1926 (see letter of October 26, 1926) and Bergmann says, as he was to repeat several times in the future: "Es ist eine der schönsten Sammlungen, die wir während der letzten Jahre erhalten haben." (It is one of the most beautiful collections that we have received in recent years). In fact, Bergmann knew the collection before it was donated to the Jewish National and University Library Jerusalem. In a letter to Dr. Eder in London (July 18, 1926), Bergmann writes: "Ich kenne die Bibliothek sehr gut, da ich den verstorbenen Philosophen besucht habe. Es ist eine für uns sehr wertvolle Erwerbung. – " (I know the library very well, since I visited the philosopher, who is now deceased. It is a very valuable acquisition for us.) (Archive of the Jewish National and University Library Jerusalem, Arc 4_ 793, 137 [III]). In November 1926 Bergmann informs Citron that "Die Kalogisierung der Bibliothek Itelson geht langsam vorwärts." (The cataloguing of Itelson's library is progressing slowly.) (Letter of November 10, 1926). These two inventory lists could not be found.

We did, however, find a document testifying that Itelson himself had considered the possibility of selling his collection to the Jewish National and University Library in Jerusalem. It is a memorandum of one page titled “Bibliothek Gregorij Itelson” with no date or signature, but it mentions that Itelson is 68 years old. Therefore it must have been written about 1920. The document mentions that the collection consists of 4,500–5,000 volumes, many of them rare and expensive, among them many first editions of important books. “Der Bücherschatz besteht zum grössten Teile aus philosophischen und mathematischen Werken. Besonders Logik und Geschichte der Mathematik sind sehr gut vertreten.” (The valuable collection consists mainly of philosophical and mathematical works. Particularly logic and the history of mathematics are very well represented.) The memorandum can be found in the Archive of the Jewish National and University Library Jerusalem, archive of Professor Loewe 4°793 275/II. See also the letters of Loewe to Itelson between January 21, 1920 and May 14, 1920 and Loewe’s letter to professor Otto Warburg of June 16, 1920; all in the Jewish National and University Library Jerusalem, archive of Professor Loewe 4°793 275/III. In these letters Itelson’s collection is estimated at 50,000 Marks. The proposed agreement was in an attachment to the letter to Otto Warburg of June 16, 1920. The letter is not in Warburg’s literary estate in the archive of the Max-Planck-Gesellschaft.

33. Congrès International de Philosophie, Genève, September 4–8, 1904. Louis Couturat: “Logique et Philosophie des Sciences. Séances de section et séances générales”. Revue de Métaphysique et de Morale, T. XII (1904), pp. 1037–1077, 1038. There are reasons to believe that Couturat had the text of both lectures. First, his detailed report suggests that he had the text in front of him, second he mentions explicitly that Wilhelm Windelband did not distribute the text of his lecture “comme les autres rapports” (p. 1061, note). We could not find a copy of this manuscript.

    Itelson’s definitions of logic and mathematics are quoted by various contemporary authors. It also seems that he intended to develop further activities in this field. We are indebted to Volker Peckhaus for the following information.

    In a letter to Nelson (May 15, 1908; Geheimes Staatsarchiv Preussischer Kulturbesitz, Berlin, Nachlass Nelson, 1/LN AA000271-1/LN AA000273) Hessenberg writes:

    “Von Itelson hat Dir wohl Grelling schon erzählt und von seinem Plan, eine Zeitschrift “‘Logos”‘ für talmudische Definitionssophistik herauszugeben; er hat Dir wohl auch erzählt, dass Zermelo, Dingler und ich mit einem “gleichen” Plan beinahe post factum gekommen wären, glücklicherweise aber – wenigstens anscheinend, — Itelson noch abgesägt haben. Ich unterhandle gegenwärtig mit Teubner über die Gründung einer “Vierteljahrsschrift für die Grundlagen der gesamten Mathematik” (Grelling has probably already told you about Itelson and his plan to found a journal for Talmudic sophistry of definitions. He will also have told you that Zermelo, Dingler, and I would almost have come with the “same” plan post factum, but that we, at least apparently, managed to dispose of Itelson).

34. H. J. Watt wrote in a review of Meinong’s “Über Gegenstandstheorie” (1904) (Archiv für die geamte Psychologie, Bd. VII (1906), p. 263) that it was akin to Itelson’s conception. Meinong quoted this verdict in his new book, *Über die Stellung der Gegenstandstheorie im System der Wissenschaften*, Leipzig (R. Voigtländers Verlag), 1907, p. 5, and interpreted it – and the wide approval of Itelson’s lecture of 1904 – as a sign that his endeavours expressed a generally felt need. Meinong mentioned Itelson approvingly in the introduction (p. 6) and also quoted Itelson’s lecture rather extensively (128–129) and spoke of the “ebenso beachtens- als dankenswerten Aufstellungen Itelsons” (the propositions put forward by Itelson were both noteworthy and praiseworthy) (129).

    On April 8, 1907 Meinong wrote to Itelson, sent him his aforementioned book and expressed the hope that Itelson would soon present his views to the public. (The draft of the letter is kept in the Meinong Collection in the Universitätsbibliothek Graz, box LXVIII.) We could not find Itelson’s copy of Meinong’s book in the Jewish National and University Library Jerusalem.

35. See Louis Couturat's letter to Husserl of November 7, 1904. After summarizing Itelson's critique, Couturat writes: „J'avoue que ce reproche ne m'a pas paru sans fondement, et qu'il correspond à mon impression personelle." Edmund Husserl, Briefwechsel, Bd. VI „Philosophenbriefe", hrgg. von Karl Schuhmann in Verbindung mit Elisabeth Schuhmann, Kluwer Academic Publishers 1993, pp. 33–34. For Husserl's reaction, see *Husserliana*, vol. 20,1, Dordrecht (Nijhoff), 2002, pp. 302, 429–430. Husserl also met personally with Itelson. See the draft of his letter to Meiner of May 4, 1914, in: Edmund Husserl, *Briefwechsel*, Vol. 8 (Husseriliana 3,8), Dordrecht (Kluwer) 1994, pp. 245–246.
36. Couturat, op. cit., 1039, 1041.
37. Couturat, op. cit., 1061–1063.
38. *Die Philosophie im Beginn des zwanzigsten Jahrhunderts. Festschrift für Kuno Fischer*, herausgegeben von W. Windelband. I. Band. Heidelberg (Carl Winter), 1904, pp. 166–167.
39. Itelson's copy is in the possession of Gideon Freudenthal.
40. See E. Husserl, *Logische Untersuchungen* I (1900), § 39 („The Anthropologism in Sigwart's Logic"), and see the answer of Heinrich Maier in the fourth edition of Sigwart's Logik (1911), pp. 24–25.
41. Franz Oppenheimer, Gregor Itelson (obituary, 1929), p. 1 believed that this book would have contained a history of philosophy in general: "... seine 'Logik', die ja wahrscheinlich auch eine ganze Geschichte der Philosophie enthalten hätte – er meisterte sie wie kaum ein anderer..." (his „Logic", which would probably have contained a history of philosophy. In this, he has an unmatched mastery).
42. "La science des objets ordonés". Couturat, op. cit., pp. 1039–1040. This definition was accepted by many. For two examples from different view points, see A. Voss: *Über das Wesen der Mathematik* (1908), Leipzig (Teubner) 1922, S. 26–27; and E. Cassirer: Kant und die moderne Mathematik, in: Kritizismus, Fr. Myrho, ed., Berlin (Pan) 1926, S. 101–102.
43. Couturat, op. cit., p. 1042.
44. Russell, Letters, p. 308.
45. Vol. III, pp. 282–290.
46. On the Neo-Kantian critique of Fechner (Adolf Stadler used also J. Tannery's arguments) see Michael Heidelberger, *Die innere Seite der Natur. Gustav Fechners wissenschaftlich-philosophische Weltauffassung*, Frankfurt/M (Klostermann), 1993, S. 237–243. See especially Hermann Cohen, *Das Prinzip der Infinitesimal-Methode und seine Geschichte*, 1883, § 106–111. Itelson did not comment on these paragraphs in his copy of this book. However, Itelson commented on one sentence in Cohen's introduction: „"It was after all Hegel's critique of this concept [the infinitesimal – GF] in which he exposed his shipwreck." In the margin Itelson noted: "Cohen too!" [Cohen auch!]. (Itelson's copy in the National Library of Jerusalem, S93 B3940).

    In a review of this paper of Itelson, Hermann Ebbinghaus makes the classical point of the scientist against the philosopher: What science can or will never be able to do (this alludes to the "ignorabimus" of Emil du Bois-Reymond) should not left to "the most subtle deductions" of philosophy but simply to the future. See Zeitschrift für Psychologie und Physiologie der Sinnesorgane, Vol. I (1890), pp. 127–128.
47. Archiv für Geschichte der Philosophie, Bd. II (1889), 471–472.
48. Review of Suarez de Mendoza, *L'audition colorée* etc (1890); H. Beaunis et A. Binet, *Sur deux cas d'audition colorée* (1892), Binet et Philippe, *Étude sur un nouveaux cas d'audition colorée* (1892), Binet, *La problème de l'audition colorée* (1892) in: Zeitschrift für Psychologie und Physiologie der Sinnesorgane, Bd. 4 (1893), 418–419.

    The evidence for Itelson's studies of this phenomenon is his lecture on the International congress of psychologie, München: „ Ueber paradoxe Nebenvorstellungen," See „Internationaler Congress fuer Psychologie", Heft: 3 (1896), p. 476.
49. *Die Gestirne und die Weltgeschichte. Gedanken Über Raum, Zeit und Ewigkeit*, Breslau 1846. *The Stars and the Earth; or, thoughts upon Space, Time, and Eternity*. London, 1846. Many more additions appeared in English. The translation into German appeared in Leipzig in

1860. The second edition of the authentic publication appeared in 1874. The German editions of 1846 and 1847 as well as the English editions of 1846 and 1847 are reprinted in Karl Clausberg, *Zwischen den Sternen: Lichtbildarchive. Was Einstein und Uexküll, Benjamin und das Kino der Astronomie des 19. Jahrhunderts verdanken.* Berlin (Akademie Verlag) 2006.

50. Eberty, vii.
51. Eberty, unpaginated preface following title page.
52. "Unified Science as Encyclopedic Integration", International Encylopedia of Unified Science, Vol. I, No. 1, Chicago 1938, S. 1–27. A German translation appeared in: *Gesammelte philosophische und methodologische Schriften*, R. Haller und H. Rutte Hrgs., Wien (Hšlder-Pichler-Tempsky) 1981, II, 873–894, 882–883.
53. See Kant-Studien 34 (1929), S. 198–199. There and in his letter to Einstein, Feldkeller makes much of a mistake on the part of Eberty concerning a question in astronomy. However, this mistake has no effect on Eberty's argument.
54. "Unified Science and its Encyclopaedia" (1937), Reprinted in Otto Neurath, *Philosophical Papers 1913–1946* (Robert S. Cohen and M. Neurath eds), Dortdrecht (Reidel), 1983, pp. 172–182, 178–179. A German translation appeared in: *Gesammelte philosophische und methodologische Schriften*, R. Haller und H. Rutte Hrgs., Wien (Hšlder-Pichler-Tempsky) 1981, II, 777–786.

    Neurath acknowledged his debt to Itelson already in the curriculum vitae attached to his doctoral dissertation. See Otto Neurath, Zur Anschauung der Antike *Über Handel, Gewerbe und Landwirtschaft*, 1906.
55. "Le dévelopment du Cercle de Vienne et l'avenir de l'empirisme logique. Acutalités Scientifiques et Industrielles, No. 290. Paris 1936. We translate from the original German typescript, p. 50. See also "Die neue Enzyklopädie des wissenschaftlichen Empirismus (1937), *Gesammelte Schriften II*, 801–811, 803.

Part II
On the Unity of Science

Chapter 10
Unity Without Myths

Daniel Andler

We seem to suffer from a case of cognitive dissonance. On the one hand, we seem to have almost unanimously rejected as hopeless or incoherent the aim of a unified science. On the other, we passionately debate about the prospects of research programs which, if successful, would considerably enhance the prospects of unification: from particle physics to cognitive neuroscience, from evolutionary theory to logical modeling or dynamic systems, a common motivation seems to be the quest for unity. The purpose of this paper is to relieve the dissonance. I will defend a moderate form of unity, one which is compatible with the diversity and open-endedness of science, for which I can think of no better name than federalism, as it combines plurality and the construction of a common epistemic area. This view is not original: Otto Neurath himself espoused it, albeit in a context which is in certain respects quite unlike ours.

10.1 Varieties of Unitarian Doctrines

Let us locate the notion of unity of science in logical space. As a matter of terminology, I will use "unitarian" and "unitarianism" to refer to the doctrine, principle or thesis of the unity of science (rather than the better-formed "monism" and its cognates, which need to be qualified, confusingly, as "methodological" to distinguish epistemic unity of knowledge from ontological unity of nature).

First, we should distinguish reductive unity from organic unity. A strong form of physicalism implies reductive unity (it is in fact the only active metaphysical program in mainstream philosophy of science which does, so that in practice though not in principle the two are often taken to be equivalent). Organic unity is weaker: it maintains the autonomy of a variety of sciences (or disciplines), but insists on an intelligible articulation between them. A living body has distinguishable parts which are held together by joints which are themselves body parts and are as open to full investigation than the parts themselves; and so, for the organic unitarian,

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J. Symons et al. (eds.), *Otto Neurath and the Unity of Science*, Logic, Epistemology, and the Unity of Science 18, DOI 10.1007/978-94-007-0143-4_10,

the corpus of science is, or will eventually appear as an organic whole composed of parts held together by theoretically accountable bonds. The sophisticated physicalist might argue that in the last analysis, organic and reductive unity are, or end up being the same. But not everybody will be convinced. Some French philosophers of science of the recent past, for example, have made much of the grammatical plural of “the sciences” (in French, *les* sciences), with the intent to reject any form of unity which would imply the elimination in principle and in the long run of the plurality of sciences; so they would of course reject reductive unity, while accepting organic unity. In fact, they may not have seen any coherent possibility of rejecting the latter. Perhaps this remains the case for some philosophers today: to them, the issue is between physicalism (or reductionism) *vs* what I call organic unity, which to them is the default, “antireductionist” position. While to me, and many others in our post-positivist age, the main contrast is between unity, whether reductive or organic, on the one hand, and disunity, a view which is assumed to go clearly beyond the mere admission of the plurality of sciences.

So we already have, at this early stage, a partial resolution of the initial dissonance. The impression that hardly anyone today defends unity is misleading: many of these alleged anti-unitarians may turn out to be organic nonreductive unitarians and thus feel comfortable about unification research programs. What does remain is, first, a sharp disagreement between the group formed by both kinds of unitarians and the radical, “anarchistic” anti-unitarians, and second, for the latter group, the problem of finding value or sense in the pursuit of unification programs.

But there is a second dimension to the issue. Are we seeking a correct description of science as it is, or are we assigning norms to the scientific enterprise, saying what it should aim for, or assessing how distant or close it is in fact from what it should be in principle? The difficulty here is that there is no unique correct description of anything as complex as “science as it is”, so that the description we seek is itself heavily dependent on norms. The description we seek should (or so it is often assumed) show the “deep structure” of science (whether logical, more broadly conceptual, or historical), rather than consist in an endless litany of observations, and the search for such a deep structure is perforce going to engage some of our deep theoretical commitments, amongst which those regarding unity. The unitarian will tend to seek a unitarian description, which might require quite a bit of theoretical work carried out at the meta level, though actually in some cases it may be indistinguishable from first-order scientific work. The anti-unitarian will tend to favor, in the name of faithfulness or “realism”, features which raise apparently insurmountable obstacles for a unitarian account; his is a “lazy” strategy, requiring as it does no feat of scientific or philosophical imagination.

It thus appears that mocking an earlier generation of philosophers for offering a “Legend” as an account of science, rather than aiming for the “real thing” is slightly disingenuous.[1] On the other hand, arguments can be sought either in a critical examination of the idealizations which were necessary to the construction of the view under scrutiny, or in the transformations which science has undergone in the last three or four decades: perhaps a theoretical account of science which was plausible, all things considered, at a previous stage can no longer be taken seriously.

But these higher-level assessments are themselves entirely open to discussion. The upshot is that there is no such thing as descriptive philosophy of science as opposed to normative philosophy of science: there are only better and worse ways of going the bootstrapping route, from our budding theories of science to science itself, which is never quite itself but always on the go.

Lastly, there is the question of the structure of nature. Is the unity of science a corollary of the unity of nature? Conversely, does the disorder of nature imply or explain the disunity of science? Poincaré thought that the unity of nature could not be regarded as a serious question, "for if the various parts of the universe were not like the organs of one body, they would not act upon one another, they would ignore each other, and in particular we would only know one of them".[2] (Poincaré would thus qualify as an "ontological organic unitarian"). Symmetrically, John Dupré argues that the "disorder of things" provides the "metaphysical foundations of the disunity of science".[3] Certainly the two claims, ontological and epistemic, sit well together, as do their opposites. But in fact the two issues are orthogonal: nature could be one and our science divided as well as united; and nature could be divided yet our science be united as well as divided. In both cases the possibility arises because we may be epistemically limited: nature could be one and we could be incapable of finding out; or nature could be divided yet our science limited to just one region of it and thus possibly united.

At any rate, as Poincaré points out, unity is not a given: it is conquered. Unification is achieved via two kinds of processes (a third will be added later). The first is the discovery of a common principle, a structuring hypothesis which reveals the essential kinship between two orders of nature initially conceived as separate. The best-known examples are provided by the developments of physics, chemistry and biology in the nineteenth century (Faraday then Maxwell incorporating electricity, magnetism and optics into a single realm; Berthelot doing the same for organic and mineral chemistry; cellular biology connecting zoology and botany, etc.). The second unifying process is reduction: an area is reduced to another after the model provided by Boltzmann's statistical thermodynamics, bringing heat under the theoretical control of mechanics. Reduction is in principle absolute: nothing remains of the initial ontological autonomy of the reduced realm, although practical considerations will often justify holding on to its vocabulary and to some of its methods: who would want to resort to Schrodinger's equations to write an oxidation-reduction reaction, or apply the Maxwell-Boltzmann distribution law to compute the maximal efficiency of a steam engine, when well-tested macro-level empirical laws provide the answers in minutes? Besides these two constructive forms of unification, there is the case of outright elimination, when an entity is ruled out of the ontology, leaving no traces in further developments of science: Galenic humors, natural places and imponderable fluids have simply vanished (but then it can be argued that they were mere accidents along science's journey towards truth).

Unitarian views are traditionally contrasted with regionalism, which contends that there is a natural division of mutually autonomous disciplines corresponding to natural divisions of nature. As we saw, regionalism can be no other than organic unitarianism viewed, so to speak, from the angle of the "organs" rather than the “organism”. There is a stronger form of regionalism which denies the existence of (non-trivial) connections between the sciences, at least at a mature stage of development. The main example is bifurcationism to which we now turn.

When Neurath introduced unity of science as a thesis and as a defining concept of the Vienna Circle, his target was the bifurcation principle defended by Droysen, Dilthey, Jaspers, Weber and others, making the *de facto* separation between the human sciences and the physical or natural sciences a *de jure* unbridgeable gap. Unity was to be defended as a matter of principle, and hardly concerned the natural sciences as a disorderly collection: the aim was to put an end to an exorbitant right, used and misused by the *Geisteswissenschaften*, to conduct their business as they please, disowning in advance any critical examination from other sectors of science among the *Naturwissenschaften.* The bifurcation thesis is of course still widely held.[4] Perhaps surprisingly, it tends to support a restricted principle of unity, one restricted, on the one hand, to the natural sciences, on the other, to the human sciences. Indeed, it presupposes such a principle: for only if the two groups are in some sense homogeneous does the issue of their essential difference take on any real importance.

Here we can identify, and thus suppress, another source of the cognitive dissonance we aim to dissolve. For philosophers of science who have in the last two decades inflicted damaging criticism to the unitarian conception, the problem concerns disciplines which were until recently thought to raise *no* obstacle for unification. Their contribution to the anti-unity near-consensus is only indirectly related to the most ambitious unifying program underway, *viz.* the naturalization of the human sciences by cognitive science and evolutionary biology. That program *is* directly relevant to the initial concerns of unitarians. Among its defenders and practitioners, the more sophisticated are not alarmed by the ongoing anti-unity arguments, in fact they can welcome them. As was just suggested, a loosening of the ties between the natural sciences delivers slack for a possible tightening of the bonds between *some* human and *some* natural disciplines or sub-disciplines. Nonetheless, they often continue to lean on orthodox unitarian views when it comes to justifying their project in general terms: they claim to merely be carrying out, so to speak, historical necessity, or obeying the prescriptions of a sound conception of science. Thus Neurath’s natural allies today do not all belong to any one camp: as we shall see, he has foes as well as friends in both. Some of those who are trying to substantiate the anti-bifurcation thesis which is, as we just recalled, at the heart of Neurath’s and his colleagues’ concern, also defend a rigid unitarianism which he rejects. By the same token, contemporary anti-unitarians are actually posthumously providing Neurath with arguments in favor of his non-standard version of unitarianism.

Contemporary naturalists defend yet another position, which partakes of both unitarianism and regionalism. They rely on a different sort of unification, compatible with a certain form of anti-unitarianism. This is a achieved by a third kind of process, which combines ontological monism with epistemic pluralism by introducing filters or screens between realms of nature (levels of organization, levels of aggregation . . . . the issue of what it is that filters both unite and separate is thorny and one which I will not discuss here). A filter is in place whenever an entity (object, state, process) can be fully characterized, at the level where it is initially individuated (conventionally called the "upper" level), by the role it plays in relation to other entities on that same level, without recourse to its "nature" nor any property it might possess on the other level (the "lower" level). Thus the theory of levers and pulleys is insulated from other areas of physics or chemistry by the fact that levers and pulleys are entities which are exhaustively characterized by the role they play in mechanical systems. The stuff they are made of, for example, plays no role at all in the theory (under appropriate idealization). A filter thus appears as a kind of antidote to reduction: whether reduction is possible or not (the issue is emergence, another topic which I will not discuss) becomes irrelevant. A proper filter, like a proper reduction, is perfect: it lets nothing from the "lower" level contaminate the "upper" level, save perhaps some fairly uninteresting constraints of a broad quantitative character. A theory can thus be developed for the higher level, a theory of roles or functions, with no significant contribution from lower-level facts or generalizations. Functionalism, concerning two realms of nature, is the thesis according to which a filter can be inserted between them. In its present form, functionalism is the center piece of mainstream philosophy of mind: the "upper" level is the realm of mental, or psychological, or again informational entities, the "lower" level that of physical (specifically, biological, or electronic) entities. But functionalism is a familiar strategy in many other fields, as can be seen by a cursory inspection of biology, economics, sociology, demography, geology, etc. In fact, it is available in principle to any discipline except the most basic one, fundamental physics. Its net effect, for the issue of bifurcation, is to make it no more interesting than the relation of say geology to basic physics: all "special sciences" in Fodor's terminology,[5] stand with respect to (fundamental) physics in the same kind of relation. Ontology is irrelevant, and one can freely choose one's camp; what matters is that explanatory dualism is seen to be no less respectable for psychology than it is for geology. Fodor's is thus a hybrid form of anti-unitarianism: disunity of science is claimed as a "working hypothesis" compatible with physicalism. Functionalism in philosophy of mind is the target of increasingly severe criticism, and may need to be reformulated in order to preserve its intuitive core (after all, there *is* a science of levers and pulleys, so there must be something right about the very idea of functionalism). But I will leave this issue on the side, as I want to focus on what appears to me to be more central concerns, and which will also bring us closer to Neurath's opposition to unitarianism.

First however we need to ask, in the light of the preceding discussion, whether unitarians have enough common ground to make an assault on unitarianism worth the effort. I think there is: unitarians share a common ground, which is a stance they hold above and beyond any specific set of theses, and this stance involves one particular assumption which remains usually unstated yet plays a key role. The generic Unitarian I have in mind assents to a significant proportion of the following complex thesis:

> Nature being one, and science, whose purpose is to provide objective knowledge of nature, having a near perfect record of success, the horizon common to all ongoing research programs in the various disciplines is a unified and complete body of knowledge about nature.

> The divisions which exist today are of no theoretical import, being due to mere historical factors, some involving methodologies or instruments, many having to do with the dynamics of academic institutions. These divisions are bound to disappear, leaving at most intelligible articulations between natural domains. Their final elimination will mark the end of a long series of abolitions of borders, achieved by unifying enterprises whose success are among the highest accomplishments of science.

The crucial assumption is that science is moving towards completeness: scientific knowledge is admittedly incomplete, but it aims for completeness in the limit. This goal is not only reasonable, from the unitarian standpoint: it is constitutive of the Unitarian view.

10.2 What Stands in the Way of Unity

I will now argue that all forms of unitarianism are based on a misleading picture of science. The same can be said in fact also about classical regionalism, so that my opponents include reductive and organic unitarians, as well as classical regionalists and functionalists, while my allies include Neurath, and I suspect several leading contemporary philosophers who I will not call on in this paper for fear of making it too long.

10.2.1 The Myth of Purity

The first problem with all of these doctrines is that they rest on an assumption of purity, which comprises two complementary parts.

On the one hand, it is assumed that a mature science only employs representations which are purified of any intuitive content, whether originating in commonsense, subjective perception, or ordinary language. These resources play only at the boundaries of the discipline, where they allow scientists to reach an intersubjective consensus on the validity, justification, degree of empirical support, of the theories which they are considering. This requirement may be regarded as a form of epistemic purity.

On the other hand, every real, existing entity (again: thing, state, process...) is assumed to belong to exactly one natural kind. Natural kinds are the domains of natural laws, and the trajectory of any entity is entirely determined (be it probabilistically) by the laws relative to the kind to which the entity belongs (of course, in combination with laws pertaining to other entities involved and initial conditions). This may be regarded as a form of ontological purity.

These two assumptions, which have been defended and attacked independently, jointly imply that for every realm of nature, there exists an essentially unique formal language which affords reference to the elements of the local ontology, expression of its fundamental laws, and formal inferences necessary for explanation and prediction. Uniqueness results from the requirement of ontological purity, and formality from epistemic purity.

Both assumptions have been challenged. In chronological order, Neurath first emphasized that science can never be rid of what he called *Ballungen*, translated as "clusters", which are hybrid concepts or terms with both a theoretical or scientific and a lay or intuitive component. These cluster concepts play in role in the theory in which they occur, and are scientific to that extent; but their reference is partly determined by commonsense and social or intersubjective practices which remain external to the theory. To take an example in contemporary science, the concepts of cognitive psychology, as Fodor notices, are intentional, or folk-theoretic, through and through. Yet inside the theory, they are deployed as natural-scientific concepts. In other words, they are typical *Ballungen* which both play a role in the theory and refer to phenomena which can only be identified with the help of our commonsense intentional psychology. This *matters:* as Fodor points out, as long as this will be the case, cognitive psychology will not deserve the title of natural science of the mind; and he bemoans the fact that the prospects of a change are slim. Neurath would not only concur, but stress the fact that all of science is in the same boat. The physicalism which he recommends (he is the inventor of the term) is a plea to all of the sciences to use a common language, one which is "nothing new as it were; it is the language familiar to certain 'naïve' children and peoples".[6] As Haller comments, the physicalist language is nothing but "the everyday language of the 'natural conception of the world'", the last expression being due to Avenarius, the founder of empiriocriticism, a major influence on Neurath.[7] No doubt Neurath would find the naturalistic worries of Fodor and his contemporaries misplaced, but this is another story. The main point is this: if Neurath's insight is correct, as I believe it to be, then the epistemic purity requirement can never be met, except perhaps in very special cases, which are at any rate unlikely to be encountered in the context of the sciences of man.

John Dupré has mounted a book-length attack on the other requirement.[8] Of course, it was never a secret that some phenomena occur at the intersection or the interface of different realms. Neurath's own preferred example was that of the forest fire: how can we predict when a given fire will be extinguished? Neurath plausibly argued that an answer would require the resources of botanies, meteorology, geography, sociology, various technologies, and today we might add ecology. It has never been a mystery that the same fragment of stuff can be a piece of clay, a statue, a religious symbol, an architectural support, a tool for crushing an infidel, etc. However, the thought was, physics has managed to ignore these cases: it does not deal with forests or statues, fires or religious wars. Pure science is precisely dependent for its existence on the exclusion of hybrid entities, which are the business of engineering and applied science. What Dupré argues is that far from being marginal, membership in a plurality of natural kinds is typical; this is the view he defends under the label of "promiscuous realism" a doctrine which combines an ontological commitment to natural kinds and a rejection of the uniqueness condition. There *are* natural, agent-independent boundaries in nature, but these boundaries criss-cross, and there is no fact of the matter as to which is *the* correct affiliation of a given entity. Although there is a fact of the matter as to which affiliations are *incorrect*, the choice of *a* correct affiliation is context-and purpose-dependent. No doubt this is not easily acceptable from a classical standpoint, whether in philosophy of science or metaphysics, or in science itself. *Hybrid entities* went unrecognized for a long time in the "noble" sciences. Take culture: surely its elements are collective representations, a self-standing ontological category? That is certainly what we were taught to believe. But if instead one considers something like Dan Sperber's epidemiological theory,[9] one sees culture as a collection of distributions of mental and material representations in a human population, just as an epidemic is a distribution, causally produced by contagion or other replicating mechanisms, of clinical states induced in human or other animal populations by micro-organisms or other pathogens. Given a mental representation in one particular human mind (John thinking today about whether to vote for Sarkozy), it naturally belongs to a kind of brain state, to a kind of psychological state, to a kind of cultural phenomenon. Ecology, meso-physics and countless other recently developed research programs provide further examples, but one also realizes, in retrospect, that past scientific developments are no less rich a source. Once one starts to look for cases of multiple affiliation, one finds so many that one gets to wonder if single affiliation is anywhere to be found, except in particle physics.

But if something like Dupré's account is true, how can we account for the late realization of this state of affairs, The answer, I submit, is this. First, experience teaches us a number of reasonable taxonomic choices; faced with a theoretical problem, we are guided by previous situations which are similar in appropriate respects. This is not a failsafe strategy: it works, well, exactly in those cases where it works; experience insures that such cases are frequent – how this is achieved is a notoriously difficult question, but one which is not specific to our present concern and which is not generally regarded as entirely hopeless. The second part of the answer lies in a cultural strategy which is the cognitive version of niche construction, a well-documented phenomenon related to co-evolution of species, recently extended to the study of interactions between biological and cultural evolution.[10] The conjecture is that we tend to collectively create a world, part epistemic, part institutional, part material, in which our scientific tasks are on average less awesome than they would otherwise be; this, if correct, would amount to a process of unsupervised collective learning. The better we become, as individuals and as members of scientific communities, at handling taxonomic underdetermination, the less we see that there is an underdetermination in the first place.

Hybrid concepts and sortal pluralism are the first potential objection to classical unitarian or regionalist views.

10.2.2 The Myth of Completeness

I now come to a second objection, which is less discussed perhaps because it is too obvious, or, more likely, because it runs counter received opinion. Neurath, as I discovered recently, had put his finger on it already. The starting point is a trite observation: at any given moment of time, the knowledge which any knower, any community of knowers has of a chosen realm is incomplete. It is usually tacitly assumed that this incomplete body of knowledge is bound for completion, be it in the indefinite future: let us call this the *completeness in the limit assumption.* It is also thought that that there comes, in any science, a time of maturity in which our knowledge far exceeds our ignorance, so that we can assume in such cases that we have achieved a state close to a full knowledge of the topic at hand. This latter, *near-completeness-in-practice assumption* is prevalent and it bolsters the case of the former, completeness-in-the-limit assumption. Even in our post-positivist age, one hears references to an "ideally completed physics", or "neuroscience" philosophers talk of counterfactual situations where someone would know "all there is to know" about some area or system. Commonsense and scientific practice, as well as history of science, seem to endorse both assumptions. And indeed, isn't it obvious that in more than a few areas we do possess all the knowledge worth having? Take just about any branch of physics or chemistry which is taught in schools and used in technology and engineering, with the notable exception of quantum mechanics: aren't those instances of near-complete knowledge? What about large parts of medicine, such as anatomy, physiology, ophthalmology? And our practical knowledge of, say, everyday economics (how do deal with money, banks, checks, bills. . .) or public transportation in the city where we live is also clearly complete or very nearly so.

Although a full discussion can't be offered here, it is important to see why these intuitions are misleading. Neurath had a general argument against what we could name the Myth of Completeness. Throughout his career, he was scathing in his criticism of what he called "pseudorationalism", the overconfidence in rationality as a kind of universal insurance policy against uncertainty and error. He writes for example:

> Pseudorationalism will time and again try to reach, in roundabout ways, the 'one *real* world' ('the *one* mass of statements distinguished by certain characteristics'), for example, by putting forward the doctrine of a perfection, perhaps 'infinitely far away' to which science gets closer and closer.[11]

I won't attempt an analysis of the Neurathian theme of pseudorationalism. I will instead suggest, in what I hope to be the same spirit, an answer of my own. It might be thought that the problem is the fallibility of knowledge, at least in scientific theorizing. But we have I think learnt not to overextend Popper' conjecturalism or Laudan's pessimistic induction: in many fields, we are in fact not expecting that our current theories will one day be proved dramatically wrong; we do not expect that a new fact will force us to simply discard them; at worst, they might require a reformulation. The problem lies elsewhere and is directly connected to the issue of boundaries which is at the heart of our present concern. What we can *not* be confident about are the boundaries of the domain of which we feel confident that we have complete or near complete knowledge. These boundaries may well suddenly burst open under the pressure of a new discovery, or a theoretical insight such as Maxwell's, when he noticed that the velocity of electromagnetic waves as he had just computed it came very close to the velocity of light. Light in fact provides an amusing example of the false security we sometimes feel regarding our state of knowledge. This is what Priestley wrote in 1772:

> . . .the nature of vision, in general, seems to be very well understood; and there are few phenomena belonging to it that have not been satisfactorily explained.[12]

lt is not necessary to be informed of the latest advances in vision science in order to realize the depth of Priestley's ignorance. What he did not know, for the most part, does not pertain to aspects of vision which he and his contemporaries were aware of. We can even safely assume, for the sake of the argument, that what they knew at the time has been on the whole confirmed rather than disconfirmed by later developments. Rather, what Priestley did not know regards what was later shown to be *relevant* to a fuller understanding of vision (such as the complex cognitive operations performed by the visual cortex on the deliverances of the optic nerve). In other words, what Priestley did not know is what one should have knowledge *about*, beyond the domain about which he did have considerable knowledge. A similar example is making the headlines as I write: the discovery of the role of RNA is shattering the illusory near-completeness which molecular biology was claiming until recently as regards the genetic line of command to the cell. What I am arguing for here is a form of conjecturalism, but applied to questions rather than answers as in Popper's doctrine.

We are now in a position to see why the near-completeness that we are prone to grant to certain bodies of scientific knowledge is either illusory or trivial. Yes indeed, in such cases we have the answers to all the questions on a list which we have made up; we have become unsurpassable experts of the domain *as we have defined it*, as the domain of which we are unsurpassable experts. Unfortunately for the pseudorationalists among us, it may well turn out that tomorrow's list of questions will go far beyond, or indeed be quite different from our present list.

This leaves us, as for the Myth of Purity, with a puzzle: Why is the Myth of Completeness so persistent? The answer may lie in a residual Cartesianism which still forces on us the mathematical ideal. In mathematics, there is such a thing as complete knowledge, or so at least it may be argued. Once a domain is fully axiomatized, the set of consequences of the axioms constitutes the complete knowledge of the domain, whether we are now, tomorrow or never able to determine all of these consequences. Unpurged Cartesianism may lead us to extend this view to empirical knowledge.

To conclude, let us make sure we understand why the necessarily partial character of our knowledge undermines the classical unitarian or regionalist views of science. For regionalism, either traditional or functionalist, the case is clear: on the partial knowledge view, there are no stable boundaries in science. For unitarianism, the argument is less direct. There is moderate form of unitarianism which is not only compatible with, but encouraged by the partial knowledge view, and which I will in fact defend below: if there are no non-arbitrary boundaries to be found, the default assumption is that of science as one large set of variously connected, diversely entrenched bodies of knowledge, forming perpetually changing configurations. On the other hand, the classical form of unitarianism seems to be wedded to the metaphor of the "complete picture" of an organism-like nature as described in the Poincaré quote above, a picture of which we would gradually uncover bits and pieces, as if we were involved in doing a gigantic puzzle, in reconstituting a whole from fragments, in putting Humpty Dumpty together again. This entire scheme, which has been challenged by several authors,[13] is wedded to the Myth of Completeness, and thus ceases in the partial knowledge perspective to be coherent or useful.

10.2.3 Generalized Complementarity

I now come to my third and final attack on (strong) unitarianism, based on a recent proposal by Rom Harré.[14] We may perhaps take again Neurath as starting point. He writes: "Very often scientists know perfectly well that certain principles applied to a certain area are very fruitful, while contradictory principles applied to a different area also appear to be fruitful".[15] Neurath has in mind such examples as Newtonian vs relativist dynamics, and he believes this situation to be frequent rather than exceptional. This is Harré's strategy as well, although he probably was not inspired by Neurath. More radically than him, he considers the case of a single domain. Bohr's complementarity principle applies to the domain constituted by a single electron, which can be subjected to two kinds of measurements. One kind of measurement will yield a precise value of its velocity, but make it definitively impossible to find out its momentum, and another kind of measurement will do the reverse. Harré claims that this is not restricted to the quantum world, but extends to large areas of knowledge.

The wave/particle dual nature of light provides another arcane example: when we submit a ray of light to a certain type of experimental set up, it reveals its corpuscular nature while its ondulatory potential is definitively lost; in a different type of set up, the situation will be reversed. Harré offers examples of a very different sort. The same individual, in a psychiatric ward, will turn out as a mental patient; in the dark of night, as a sadistic brute; in court, as a criminal in the legal sense. A given passing thought, from a phenomenological or introspective perspective, is revealed to the agent as a manifestation of her self; it is shown to be a feature of a cerebral state to the operator of an fMRI scanner. The crucial point is that these pairs of traits cannot be revealed simultaneously: any procedure needed to reveal one forbids the deployment of a procedure which would reveal the other. Harré's formulation is as follows. A *determinable* is a predicate variable, which can take on one of a number of values when applied to given entity; so that, for example, *color* is a determinable which, for a cherry, takes on the value Red. Complementarity arises when two distinct determinables cannot be attributed to the same object at the same moment. Cherries do not generally give rise to complementarity: their color and shape, for example, can be attributed to them simultaneously; nothing in an operation required to ascertain their color will compromise a simultaneous attempt to determine their shape. By contrast, a given game of rugby, when played by the players, makes accessible to them some features which are forever inaccessible to the physiologist; the measurements which the physiologist will make to determine various parameters of the bodies in play require a set-up which is incompatible with the lived experience of the game. A given moment of spiritual bliss can either be known as such to the enlightened person, or known as a neurophysiological event, but cannot be known as a phenomenon which is both spiritual and neurophysiological.

The intuition which Harré wants to tap, I think, is that consilience, "the linking together of principles from different disciplines, esp. when forming a comprehensive theory",[16] sometime runs into the impenetrable wall of complementarity: no theory can be expected to actually link organically, intelligibly certain kinds of properties concerning one and the same entity. This may be brute fact, or one may perhaps connect it with certain features of our epistemic situation, or with the grammar of our language. Probing Harré's idea and the more general idea of the limits of consilience will have to wait for another occasion. But *if* there is such a principle as Complementarity or Limits to Consilience, *then* clearly the classical, strong Unitarian conception is bankrupt, for it rests, as we saw, on the notion of a self-interpreting picture, one with a single, transparent, immediately accessible meaning. Harré's principle implies that even at the most local scale, at least in some (non exceptional) situations, the "image" provided by science is never at once the whole picture: rather like a hologram, it changes according to the angle of view.

10.3 Combining Unity and Plurality: Federalism as Description and Prescription

Problems such as these, adding to enduring divided condition of science, have encouraged the swing of the pendulum towards a new orthodoxy: the program now is the Disunity of Science. The problem of course is that it's not much of a program. The world is "dappled", all coherence is gone, and the most we can do is let scientists do their job, which is to sort things out locally as best they can.

There are two things lacking in this "realistic" perspective. One is a proper consideration for the awesome progress which by universal agreement is accomplished every time a genuine unification is offered. The idea of biological evolution conferred to the life sciences a degree of unity, or cohesiveness, which was simply inthinkable after the demise of the mechanistic program and before Darwin. The microbial hypothesis united pathology to very large extent. The Faraday-Maxwell theory of fields brought together an enormous part of the physical sciences. The utility-maximizing hypothesis (regardless of its imperfections) bas brought unity to micro-economics. Group theory has unified not only parts of mathematics, but of chemistry as well, and morphogenesis. Particle physics may be on the eve of a grand unification which some of its promoters see as nothing short of miraculous. Now the real difficulty lies surely not in noticing that science is in fact a rather unruly mass, but in accounting for these transformations, as well as the smaller-scale unifications which occur all the time, increasing the density of the links between the countless branches of science.

The second shortcoming of the Disunity position is that it can encourage a form of separatism which is the natural trend imposed by scientific institutions and other social, economic and psychological factors. Let us recall that Neurath's main motivation for promoting Unity of Science as a program was to prevent the sciences of man from claiming immunity from the critical look of other fields. His proposed cure was *not* to promote an unrealistic and dangerous blanket unification, but to make it possible for all fields to enter in a critical dialogue by providing them with a common language.

In a similar spirit, I would like to promote a federalist conception of the Republic of Science. My starting point is a minimal form of naturalism – nothing more than an acknowledgment of our epistemic engagement in nature, by means of common experience as well as the scientific enterprise, the two modalities being deeply intertwined. This naturalism is realistic in spirit: our engagement is both a cause and a symptom of some rough fit between nature and our ways of thinking about it. Therefore, the fact that nature still looks to us, not at first sight, but after more than three millenia of persistent inquiry, as orderly and disorderly at once, as somewhat homogeneous and somewhat heterogeneous, is to be taken as sufficient reason to discard both unitarianism, whether reductive or organic and its kin, classical and functionalist regionalism. Both camps suffer from inductive delusion: they extrapolate past experience to the day of the Last Judgement, when the Picture will be complete, and exhibit one or the other structure which they see emerging at present, disregarding the historical fact that the partial pictures at successive stages do not simply get gradually filled in, but sometimes undergo drastic restructuring. A second mistake is to ignore the imperfect character of two of the key operations at work in unification, reduction and filtering. A model of the first is provided by statistical mechanics, which is alleged to reduce thermodynamics to microphysics (another model is the reduction of chemistry to physics by way of quantum mechanics). There are two kinds of difficulty in such cases. First, they do not obviously fill the bill to perfection: the debates are still going on, and turn on the question of a remainder – does the reducing theory account for absolutely everything the reduced theory explains? if so, does it not help itself to exogenous hypotheses? But second, these cases of reduction, whether perfect or nearly so, are at any rate the exception rather than the rule among reductions. The average reduction leaves a quite visible remainder. Filters have problems of their own: the "lower", screened-off level 'shows' through the screen; more often than not, it turns out to be impossible to leave the filter in place all the time – it has to be removed once in a while.

All of which, of course, is grist to the mill of the "anarchist" anti-unitarians. But they in turn tend to exaggerate the chaos and recurring upheavals in science, and much recent work tends to show that appearances notwithstanding, important cores – structural or other – in established scientific knowledge resist theory change.[17]

By proposing federalism as a metaphor for the overall structure of science, I try to accommodate the core intuitions of all sides. Unitarians are right to regard unification, by whatever means, as an essential goal of science: it is a permanent feature of science on the move, and an irreplaceable source of progress. Regionalists are correct in detecting some permanence in the division of explanatory labor (whether or not this division is based in some boundaries in nature itself). Anti-unitarians rightly stress the disorderly, profuse and ever changing connectivity between the countless areas and subareas of science. Federalism grants some unity, some stable division, and some permanent reconfiguration at the local and global levels, and in the relations between the two.

As a descriptive stance, federalism can thus perhaps claim to be the most realistic of the contenders. But it also has merits as a prescriptive stance. Neurath borrowed an expression used by H.M. Kallen's at the Fifth Congress for the Unity of Science, which was held at Harvard in 1939: the "orchestration of the sciences".[18] He meant it first in an "anti-totalitarian" sense. To him, the attempt, by philosophers past or present, to erect a "system" of science, was both misguided and sinister. What was called for, by contrast, was the free circulation of ideas between the disciplines. Better, they should be put in mutual resonance, without in any way one imposing its order on the others: the diversity of the sciences was to be preserved above all. Orchestration was needed, though, to overcome the state of dispersion caused by specialization (Comte had a similar worry, but sought the cure in universal education by "scientific generalities"). Dispersion soon leads to separate development, each science becoming an autonomous fiefdom accountable only to its own subjects. The resulting situation, in Neurath's eyes, ran clearly against the higher goals of human learning, and encouraged instead noxious forms of irrationality. Orchestration was meant as a way of inviting a fruitful dialogue.

But who, we may ask, stands at the pulpit? Not physics (the tempting answer in Neurath's time), not biology (the temptation today), nor philosophy, or history. The metaphor, I contend, holds water only if subverted. Nature takes the pulpit, and the musician-scientists oscillate between their score (standing for the tradition and problem situation of their discipline) and nature, which distributes the emphasis and tempo to the orchestra. The scientist (or the scientific community she belongs to) sees Nature with one eye, its partial representation with the other, and the fusion of the two affords an ever-changing three-dimensional image of the object of inquiry. Each scientist simultaneously perceives, through her peripheral audition, the parts of (some of) the other scientists, and modulates accordingly her own play, while interpreting the conductor's "indications".

This admittedly stretched metaphor is only meant to convey the idea that scientific knowledge really emerges not from the mechanical adjustment of the pieces provided by the various sciences (although such adjustments do happen), but from a polyphony of inquiries disciplined by critical debate.

Notes

1. I am not suggesting for a moment that this is what Kitcher is doing in his 1993 book. But it is a trend in some post-positivist literature.
2. Poincaré (1902, Chapter IX).
3. Dupré (1993).

4. Note that there are in fact many distinct ways of construing bifurcation, some of which are in fact quite compatible with the sort of moderate unitarianism which is defended in the present paper. But I concentrate here on an strongly separatist version of bifurcation which is not compatible.
5. Fodor (1974).
6. Neurath (1932, 66) quoted by R. Haller, in Uebel (1991, 195).
7. Haller, ibid.
8. Dupré (1993).
9. Sperber (1996).
10. Laland (2000) and Sterelny (2003); for a related approach, Richerson and Boyd (2005).
11. Neurath (1936) in Neurath (1983, Chapter 11, p. 137).
12. Joseph Priestley, *History and Present State of Discovery Relating to Vision, Light and Colours* (1772).
13. See in particular van Fraassen (1999), which I discovered unfortunately after preparing the present paper.
14. Harré (2006), Andler (2006).
15. Neurath 1946: 498.
16. Merriam-Webster Dictionary.
17. Poincaré was the first to call attention to structural invariance, and to use it as an antidote to what was not yet known as the pessimistic induction (Poincaré 1902, Chapter X). For another kind of invariance, see Mayo (1996).
18. Neurath (1946), in Neurath (1983, Chapter 22).

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Chapter 11
Two Unification Strategies: Analysis or Reduction, and Synthesis or Integration

Mario Bunge

It is well known that, whereas two centuries ago there were only a handful of sciences, nowadays there are thousands. Just half a century ago, most physicists needed to consult only *Physical Review*. Today this journal has split into half a dozen publications, each of them devoted to a single subdiscipline, and read only by specialists in that branch. A similar branching out has been occurring in all the other research fields. In short, the branching out of science has been proceeded relentlessly. Something similar has been happening in technology and the humanities. On the surface, then, the collection of sciences look like a multicolored quilt.

In view of the multiplicity of disciplines, is it not foolhardy to proclaim the unity of the sciences rather than their disunity? I will argue that the disunity thesis is superficial, because actually every factual science studies selected features of a single reality, and it does so with a single general method. Furthermore, I will also claim that there is an equally strong movement of integration or convergence of scientific fields along with the well-known fragmentation or specialization. Such integration occurs when two or more branches of science coalesce, as in the cases of the recent emergence of developmental evolutionary biology (or evodevo), physiological psychology (or congnitive and affective neuroscience), neurolinguistics, social psychology, sociolinguistics, socioeconomics, political sociology, social archaeology, and even neuroeconomics.

I will also argue that both specialization and integration have ontological and methodological roots: the world is varied but one, and its successful study presupposes its reality, as well as the plurality of human viewpoints and interests. For instance, cognition and emotion are mental processes, but both of them occur in one and the same concrete system, namely the socially embedded brain.

Moreover, there is a two-way traffic between the organ of cognition, namely the cerebral cortex, and the organ of emotion, namely the limbic system. Besides, every human being is in direct or indirect contact with thousands of conspecifics. Therefore those mental processes are bound to affect one another both directly and

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J. Symons et al. (eds.), *Otto Neurath and the Unity of Science*, Logic, Epistemology, and the Unity of Science 18, DOI 10.1007/978-94-007-0143-4_11,

through social networks. Thus, one feels pleasure every time one meets a friend, does a good deed, or understands an argument.

Unsurprisingly, the cognitive and affective mechanisms are being disclosed by the greatest epistemological integration of ever: The fusion of psychology and sociology with human biology, in particular neuroscience, endocrinology, immunology, and pharmacology. Note that the merit is imputed to *integration*, not to *reduction*.

11.1 From the Enlightenment to Mid-Twentieth Century

The earliest and best-known attempt to integrate the various branches of knowledge – humanistic, scientific, and technological – seems to have been that of the French Enlightenment. This was the collective work designed and directed by the writer-philosopher Denis Diderot and the mathematician Jean le Rond d'Alembert. Their *Encyclopédie* (1751–1772) was to be both a sort of summary of human knowledge and a weapon to attack the two bulwarks of conservatism, the Crown and the Catholic Church.

What is common to all the branches of authentic knowledge, from mathematics and history to mechanical engineering and medicine, according to the Encyclopedists? That they are secular rather than supernaturalist; rational rather than revealed, intuitive, or purely empirical; they offer evidence instead of dogma; and hey are progressive rather than stagnant. Consequently, all those disciplines are the product of praxis, in particular research – invention, discovery, proof, and criticism – rather than divine inspiration or uncritically accepted tradition. Mark: the accent is on original research, not on dogmatic repetition.

All of the Encyclopedists were keenly interested in social and political issues: they took sides for liberty and progress. Voltaire, Rousseau and Montesquieu were particularly outspoken critics of the *ancien régime*, that was eventually demolished by the 1789 Revolution. Some of those *philosophes*, particularly d'Alembert, emphasized the role of science in promoting economic growth, which had been stifled by the feudal regulations. Others, particularly Holbach, Helvétius and La Mettrie, were outspoken materialists. Holbach was also systemist rather than individualist.

Still other Encyclopedists, particularly the politologist Marquis de Condorcet, professed scientism in addition to secularism, naturalism and rationalism. That is, they claimed that whatever can be known is best known through the scientific method. This feature, scientism, was inherited by the positivists, logical positivists, and Marxists, even though none of these schools practiced it consistently.

How did the *Encyclopédie* fare? It is generally agreed that both the American Revolution of 1776 and the French Revolution of 1789 vindicated the libertarian and progressive ideals of the Encyclopedists. Moreover, it continued to inspire thousands of progressive intellectuals down the centuries.

Two centuries later, the sociologist, social activist and philosopher Otto Neurath, undoubtedly inspired in the old *Encyclopédie*, organized what is perhaps the only collective project in the history of philosophy: the *Encyclopedia of Unified Science* (1939–1962). Some of the original master ideas of this project were the same as those of its predecessor: secularism, progressivism, rationalism, and trust in the scientific approach.

But, whereas the original Encyclopedia had been philosophically pluralistic, the new one was philosophically narrow: it promoted logical empiricism, which adopted the new (mathematical or symbolic) logic as the supreme tool of philosophical analysis, but also the old phenomenalism of Ptolemy, Hume, Kant, Comte, and Mach.

The founders of the new encyclopedia did not realize that phenomenalism is incompatible with scientism, and this for two reasons. First, Neurath's and Carnap's demand, that all the sciences adopt the "language of physics", in the sense that they should start with "protocol sentences", and reduce all the theoretical terms to those occurring in such sentences, does not fit physics or any other science. For instance, the protocol sentence "This ammeter pointer reads 10 amp" makes no sense unless one knows the ABC of the theory of electric currents. And this theory happens to describe an invisible process, namely the circulation of electrons accompanied by their electromagnetic fields.

Moreover, to design and read an electric meter one needs the fragment of electrodynamics that explains the way the instrument works. This theory is also indispensable to derive the formula that relates the current intensity to the angle of deviation of the needle. This formula (an indicator hypothesis) is used to calibrate the instrument and even to draw the figures on its dial. The protocol sentences are part of the empirical evidence, which lies outside the theories. Were it not so, it would be impossible to contrast theory to experiment.

Second, every science proper goes far beyond phenomena (appearances). Moreover, science attempts to explain the latter in noumenal terms, as when cognitive neuroscience explains the secondary properties, such as color, taste and smell, in terms of the primary properties of the sense organs. Ironically, such explanation had already been sketched by Galileo in *II saggiatore* and by Descartes in his *Traité de l'homme.* Thus, in this respect the logical positivists were three centuries behind.[1]

Moreover, as it advanced in years, the unified science project became increasingly eclectic, it failed to attract new adepts, and it declined in rigor. Suffice it to compare the early brilliant fascicules, by the positivists Neurath, Carnap and Frank, with the last one, namely Thomas S. Kuhn's best-seller, *The Structure of Scientific Revolutions*.

Kuhn's work, the only fascicle of the Encyclopedia to attain popularity, was soon to become the manifesto of the constructivist-relativist school in the philosophy and sociology of science. This school was characterized and anti-empiricist thesis that scientific changes, in particular theory changes, are just as irrational as religious conversions. The fact that Kuhn's book was included in the *Encyclopedia* suggests that the unified-science movement was already spent. It failed to attract new collaborators, and thus to generate new ideas. Worse, it ended up by opening its pages to the postmodern reaction to the modernism it had intended to advance.

In short, the *Encyclopédie* was a resounding success even if, like the Bible, it was more praised than read. By contrast, the neo-positivist *Encyclopedia* was a failure. So much so, that I am likely to be one of the very few living philosophers to have read it from start to finish, initially to learn from it, and even to use some of its fascicles (in particular Carnap's and Frank's) in some of my seminars, and later on to criticize it.

Why did this philosphical encyclopedia fail? Most philosophers are likely to respond: because the unity of science is a mirage. I happen to believe that, as sketched above, the encyclopedia failed for a different reason, namely, because it embraced positivism, which was a deeply flawed philosophy. By contrast, the unity of science is not just an ideal: it is also fact. Let me make briefly the case for this thesis.[2]

11.2 Reduction: Infrequent and Mostly Partial

Reduction would seem to be the most effective unification strategy. Indeed, if everything in the world is composed of elementary particles and fields, then physics would seem to be the all-encompassing science – hence physicalism or vulgar materialism. And if, as Wittgenstein believed, there is nothing behind language, then the languages of the various sciences should be translatable into the language of physics – as Neurath and Carnap thought.

However, reduction is not as straightforward as it looks. To begin with, there are two kinds of reduction: downwards and upwards, or microreduction and macroreduction respectively. Whereas microreduction is analysis or decomposition of wholes into their parts, macroreduction is synthesis or aggregation of individuals into wholes. And reductionism is of course the methodological doctrine that recommends reduction as the only way to understanding. We shall focus on the former, because it is the most popular strategy of the two, despite which it is often misunderstood: its practitioners exaggerate its power, whereas its opponents denounce it angrily for evoking holism.

If everything is either an individual or a collection of individuals (rather than a system), then the understanding of a whole can only be brought about by diving down to the very bottom of things – that is, by identifying the (putative) ultimate constituents. Thus, light beams will be understood it terms of photons; atoms in terms of elementary particles; cells in terms of organelles and their components; multicellular organisms in terms of cells; social groups in terms of persons; propositions in terms of concepts; texts in terms of sentences – and so on. In short, micro would explain macro without further ado.

The success of microreduction in physics and chemistry has given the impression that the concepts of scientific method and reduction are coextensive: that to conduct scientific research is basically to try and reduce wholes to their parts. Not surprisingly, the enemies of science – such as the New Age followers and the postmodernists – are vehement antireductionists.

That success has obscured the fact that, in most cases, microreduction has been partial rather than total. There are two main reasons for such limitation. The first is that a system, such as an atom, a cell, or a family, has a structure as well as a composition. In other words, an integrated whole is not just a collection of basic entities: it is a new entity with (emergent) properties of its own, such as dissociation energy and valence in the case of a molecule, and replicability and self-repairability in that of a cell.

The second reason for the limitation of microreduction is that reference to the environment of the thing o interest is unavoidable, and the environment belongs to a higher-order level than the thing in question. This holds for physical atoms as well as for social atoms. Indeed, as well-posed problem in atomic physics or in field physics includes the boundary conditions, which constitute an abbreviated description of the macrophysical environment. Likewise, a well-posed problem in psychology or in social science includes explicit reference to the macrosocial environment, in particular the embedding system or supersystem. In other words, what passes for reduction is often a far more complex operation.

Undoubtedly, the most sensational advances in contemporary biology have been inspired by the thesis that organisms are nothing but chemical reactors, whence biology is just extremely complex chemistry. But this thesis, though heuristically very powerful, is only partially true. We shall show this in the cases of genetics and of the vary definition of the concept of life.

At first sight, the discovery that the genetic material is composed of DNA molecules proves that genetics has been reduced to chemistry. Indeed, we look like out ancestors because we inherit DNA molecules from them, and these molecules control the development of phenotypic traits. In short, at first sight, Genotype→Phenotype.

However, chemistry only accounts for DNA chemistry: It tells us nothing about the biological functions of DNA – e.g., that it controls morphogenesis and protein synthesis. In other words, DNA does not perform any such functions when outside of a cell, anymore than a stray screw holds a machine together. (Besides, DNA does nothing by itself: It is at the mercy of the enzymes and RNAs that determine which genes are to be expressed or silenced. In other words, the genetic code is not the prime motor it was once believed to be. This is what epigenesis is all about.)

The reason for the insufficiency of biochemistry to explain life is of course that the very concept of a living cell is alien to chemistry. True, the cell components are physical or chemical entities, but these are organized in a cell in characteristically biological ways. It is also true that every known property of a cell, except for that of being alive, is shared by some physical or chemical systems. But only living cells possess jointly the dozen or so properties that characterize organisms, among them metabolism and self-repair. Consequently biology, though certainly based on physics and chemistry, is not fully reducible to the latter. For example, any system of biological classification based exclusively on the degree of similarity in DNA – as, e.g., between us and chimps – is bound to fail, for missing the supramolecular features of organisms.

What holds for cell biology holds a *fortiori* for organismic biology. Indeed. All the organs of a multicellular organism must work not only individually but also in concert for it to be fit. And a healthy organism engages in whole-organism processes, such as metabolism, motion, interaction with other organisms, and reproduction. The radical reductionist cannot explain such higher-level processes, and the holist denies the need for such explanation. Only the systemist will endeavor to explain the whole by its parts and their interactions. This is what the physician does, for instance, when diagnosing anemia of a certain type as a result of iron deficiency, and hemochromatosis as a consequence of iron excess.

Reductionists are stubbornly opposed to the very concepts of emergence and levels. To them, every property is a property of a basic constituent or, at most, a statistical average; and only the bottom level matters. Yet, even reductionists value organisms, particularly their own, more than their elementary constituents. And everyone knows that ordinarily parts come cheaper than wholes. For example, the total worth of the atoms in a human body is about one dollar; that of the usable tissues (excluding organs) is more than $200,000; and that of a young unskilled worker is more than $1,000,000.

Ecology too is the subject of spirited controversies between reductionists and non-reductionists. The former claim that ecosystems are nothing but accidental collections of populations, and they concentrate on the binary competitions among them represented by the famous Lotka-Volterra equations. They refuse to acknowledge that communities and ecosystems are characterized by such emergent as niche, food web, species diversity, equilibrium, productivity, and sustainability.

The holists (or "functional ecologists"), such as Eugene Odum and the believers in the Gaia fantasy, take advantage of this weakness of the reductionist camp. The scientists among them study whole-system properties and processes, and regard ecosystems as self-regulating systems aiming for stability. A critical result of this approach is the hypothesis that the more diverse ecosystems are the more stable. But a recent experiment has falsified this hypothesis, showing instead that maxikmal biospecies diversity is not optimal. This and other results have vindicated the analytic method.

Mainstream ecology is systemist rather than either holist or individualist, for it adopts "a moderate or mixed approach in which it is acknowledged that communities and ecosystems are discrete higher-level entities with their own emergent properties, but which seeks the explanation of these properties especially in interactions between the component species [populations]".[3]

What about psychology: is it reducible to biology? Assume, for the sake of argument, that all mental processes are brain processes (ontological reduction). Does this entail that psychology is a branch of biology and, in particular, of neuroscience (epistemological reduction)? Not quite, and this for the following reasons. First, because brain processes are influenced by social stimuli, such as words and encounters with friends or foes. Now, such psychosocial processes are studied by social psychology, which employs sociological categories, such as those of social group, occupation, income bracket, and place in a social network, neither of which is reducible to neuroscience. A second reason is that psychology employs concepts of its own, such as those of emotion, consciousness, personality, and social inadequacy, as well as peculiar techniques, such as self-reporting, that go beyond biology.

We conclude then that, even though the psychoneural identity hypothesis is a clear case of ontological reduction, and sensationally fertile to boot, psychology is not reducible to neuroscience even though it has a large overlap with it.[4] Shorter: ontological reduction does not imply epistemological reduction.

11.3 Convergence or Integration: Fact, not Distant Goal

The history of human knowledge is that of the search for truth (science and the humanities) or efficiency (technology). This search is punctuated by events of two types: Branching out or specialization (or divergence), and branching in or integration (or convergence). Specialization is required by the diversity of the world and the increasing richness of our mental tools, whereas integration is called for by the contrast between the fragmentation of knowledge and the unity of the world.

By definition, all of the factual sciences study facts, whether actual or really possible. And all of them, even the social sciences, are expected to study them in a scientific manner, that is, in accordance with the scientific method rather than by navel contemplation, crystal-ball gazing, trial and error, or discourse analysis. That is, underneath appearances, the sciences are ontologically and methodologically one: All of them study putatively real things and their changes, in a distinctive manner that is quite different from the way theologians, literary critics, shamans, or even craftsmen proceed. This is why philosophers, from Whewell (1847) until recently, have praised all efforts to unify theories and even whole sciences. They used to praise such great unifiers as lbn Khaldûn, Newton, Maxwell, Darwin, and Weber. No longer: unification is now out of fashion among philosophers.

Still, regardless of the fears and hopes of philosophers, the borders between the sciences are becoming increasingly blurred. In the words of a Nobel laureate, the "sciences are becoming more unified, not less" (Medawar 1984, 72). In fact, unification has been proceeding in the sciences and technologies along with diversification. Witness the flourishing of crossdisciplinary research in all the major fields.

One of the best-known cases is the synthesis of Darwinism and classical genetics effected in the 1930s and 1940s. However, there have been many other cases of integration, if less glamorous. One of them is contemporary geology, a synthesis of petrology, mineralogy, stratigraphy, orography, and seismology. Another is toxicology, the confluence of biochemistry, pathology, epidemiology, and pharmacology – and moreover, a field that lies in the intersection of basic science and technology.

More recent and equally outstanding cases of convergence were the emergence of cybernetics and operations research in the 1940s, and of molecular biology in the 1950s. Cybernetics arose from the need to understand, design, and manage communication and control systems of all kinds, whether physical, biological, or social. Operations research arose from the need to manage large sociotechnical systems, such as naval convoys and corporations.

As for molecular biology, it emerged as a synthesis of genetics and biochemistry, from the wish to understand the composition, spatial structure, and functions of the hereditary material. In all three cases a number of previously disconnected disciplines converged to tackle such emergents as feedback loops and the coordination of a large number of units, that are characteristic of multi-component systems, such as cells and factories.

An even more recent case of hybridization is the emergence of evolutionary developmental biology, or evo-devo. This new science, with its own journal – *Evolution and Development* – is currently all the rage.[5] Evo-devo emerged from the wish to account for both the evolution and the conservation of developmental pathways (mechanisms) and, in particular, the origin of both speciation and stasis. Since evolutionary novelties emerge in the course of individual development, phylogeny must somehow emerge from ontogeny – even if not exactly the way Ernst Haeckel had imagined in the 1860s. And because the nascent science seeks to uncover the mechanisms underlying the patterns of phylogenetic branching, it constitutes a missing link between genetics and evolutionary biology. Another such missing link is the study of phenotypic plasticity, or the ability of a genome to sculpt different phenotypes under distinct environmental conditions. This study too requires the cooperation of several disciplines, mainly genetics, organismic biology, systematics, evolutionary biology, and ecology.

As a consequence of the evo-devo synthesis, the traditional picture of evolution as only a sieve that eliminates the unfit genic variations is seen to have two flaws.[6] It is one-sided, because it stresses destruction (death and extinction) at the expense of construction (emergence); and it is simplistic, because selection acts directly on whole organisms, not on genes – Richard Dawkins's "selfish gene" dogma notwithstanding.

Indeed, the traditional schema, "Mutation → New genes → New species," is being completed nowadays to yield "Mutation → New genes → New proteins → New developmental patterns → New phenotypes → New organism-environment relations → New species". Evo-devo studies the intermediate process leading from new genes (that emerge mainly through mutation and recombination), and the corresponding new proteins, to new phenotypes. And the study of phenotypic plasticity emphasizes the role of the environment in gene expression and suppression.

Thus, the new picture puts the individual organism back at the center of biology between molecule and population, as well as in the midst of its habitat. It also replaces the instant-adult fiction of classical evolutionary biology and population genetics with the real developing organism, which it regards as the site of qualitative novelty. In sum, the emergence of evo-devo has supplied not just a host of new important findings, but also a novel approach to both development and evolution. And it has reinforced the links between the two parent disciplines, as well as with these and genetics, ecology, and paleontology.

In general, the blurring of interdisciplinary borders occurs as the result of either of two moves: Hybridization or phagocytation. The outcome of the first process is the emergence of a new interdiscipline, such as political geography, evolutionary developmental biology, and cognitive neuroscience. The second process, phagocytation, ensues in the inclusion of one discipline into another. Such inclusion can be legitimate, as in the case of the reduction of optics to electromagnetism; or illegitimate, as in the cases of molecular and structural biology.

The reduction of optics to electromagnetic theory, which occurred towards the end of the nineteenth century, was legitimate because it was found that light waves are electromagnetic waves. By contrast, molecular and structural biology do not belong in biology but in biochemistry and biophysics respectively. This is because biology proper, like life, only starts at the cell level. Indeed, molecular and structural biology study molecules, not living beings. In particular, structural biology investigates the folding of proteins, a physical process. The name "biology" seems to be only a gimmick designed to attract students and research funds at a time when biology is the star science, and physics an impoverished Brahmin. This case should alert us to the importance of the philosophic analysis of crossdisciplinarity.

A crossdiscipline is built when two disciplines are bridged by one or more propositions containing concepts of the two founding disciplines.[7] For instance, two of the newest interdisciplines, neuroeconomics and neuroaesthetics, emerged overnight when it was discovered that both economic calculation and aesthetic valuation are functions of the prefrontal cortex.

11.4 Why Both Strategies are Required

There are two main reasons for splitting disciplines. One is epistemological: As the knowledge of a subject increases in coverage and depth, it requires increasing specialization and therefore it becomes less accessible to students in other fields. For instance, although a chemical reaction is both a physical and a chemical process, it may be tackled either by chemists or by physicists. (However, only chemical physicists, or physical chemists, will produce the most complete account.)

A second reason, or rather cause, is social: Every specialization involves the formation of a community with resources and interests that may conflict with those of other turfs. In some cases, this social, division masks an underlying ontological unity. This seems to be the case with chemical physics and physical chemistry.

Some fields of inquiry have split, only to reunite later on, when the existence of crossdisciplinary problems was realized. Well-known examples are evolutionary and developmental biology, psychology and biology, and economic and sociology. These were cases of reversible divergence. The cases of irreversible divergence are far less numerous. In fact, I know of only one such case: that of mathematics and physics, which diverged in the nineteenth century with the emergence of abstract algebra and set theory. Even as recently as one century ago, rational mechanics was taught by mathematicians. (Many of them rejected relativistic mechanics for believing that mechanics, being rational, is an a priori science and thus impregnable to experiment.)

The reason for convergence is less obvious but no less compelling. The real world is one, and many processes, though different in some features, share others.

Thus, the study of spontaneous self-organization processes, such as the emergence of macrophysical disorder – such as solidification, the onset of ferromagnetism, and the clumping of cells that occurs in morphogenesis – have revealed some common general principles.

Just as the explanation of emergence often calls for the convergence of two or more disciplines, so convergence may in turn explain or even predict emergence. For example, Alan Turing, of Turing machine fame, predicted in 1952 the existence of chemical waves. He did so by studying chemical reactions diffusing through a medium, such as a liquid. That is, he coupled the equations of chemical kinematics with Fourier's diffusion equation. Quite independently, B. P. Belousov and A. M. Zhabotinsky produced such waves in the laboratory. The study of reaction-diffusion systems is now a thriving chapter of chemistry.

At other times, fragmentation results from shallowness. Suffice it to recall three historical episodes. Until the seventeenth century, mechanics was divided into two disconnected branches: terrestrial and celestial (or astronomy). Only the work of Galileo suggested that the matter involved was one. And Newton proved this point when he built the first successful scientific theory that could legitimately claim to embrace the whole universe – though not the totality of processes. It took another two centuries to join mechanics to thermology, a fusion that produced the thermodynamic laws met by all macrophysical systems, regardless of their composition. Third example: Before the acceptance of the materialist thesis that mental events are brain events, the study of the mind and that of the brain ran parallel courses. Cognitive neuroscience is only now firmly established.[8]

As for the epistemological reason for convergence, it was given by David Hilbert (1935 [1918], 151). He stated that it is not enough that the formulas of a physical theory be mutually consistent: besides, they must not contradict the propositions of a neighboring field. For example, mechanics must be compatible with electromagnetic theory. (This was the clue to Einstein's special relativity theory.)

I have called this the condition of *external consistency*, and have extended it to all the scientific theories (Bunge 1967). For example, a psychological theory should cohere with the relevant neuroscience; and a linguistic theory should jibe with the bulk of psychology, neuroscience, and sociology. Like wise, an economic theory should match the relevant sociology; thus, a theory of economic transactions should take into account that they occur among members of a social network rather than among mutually independent agents. Perhaps it should also take into account that pleasure, fear, trust, and other emotions involved in economic activity are functions of brain subsystems, such as the reward system, the amygdala, and the caudate nucleus – as the neuroeconomists are learning.

External consistency may be regarded as a condition of scientificity. The isolation of a discipline, as is the case with parapsychology and psychoanalysis, is a mark of pseudoscience (Bunge 1983). In turn, the reason for this requirement is that the parceling of the system of human knowledge is partly conventional. Consequently this system should not be pictured in analogy with a map of sovereign nations, but rather as a rosette of partially overlapping petals.

The preceding considerations have an important bearing on the question of the relation between the depth and the breadth of a study. The received view on this matter is that these features of knowledge are inversely related to one another.[9] However, the clear lead of crossdisciplinary over unidisciplinary strategies in tackling multifaceted and multilevel problems challenges the received view. We have seen, indeed, that at least in these cases breadth is necessary to attain depth.

Let us finally see how the information revolution is affecting the existing division of scientific labor. At first sight, the widening and tightening of the global communication network should strengthen interdisciplinary bonds. In fact, Internet has facilitated enormously the formation of international "collaboratories," as well as the search for information that used to be conceptually and geographically distant from the specialist's topics. The world of knowledge is thus a small world – at least potentially.

However, it has been argued that the very same mechanism can also help balkanize science, in strengthening the ties among investigators within highly specialized fields, thus corralling them rather than prompting them to jump over the fence. In other words, conceptual convergence would be masked or even frustrated by excessive social cohesion. However, whether the www promotes insularity or universality depends largely on individual interests, which are partly shaped by one's philosophical perspective. Whence the potential of philosophy to either promote or hinder the integration of science. Which suggests one more test for evaluating philosophies: Do they favor or obstruct the unification of knowledge and thereby its advancement?

11.5 Concluding Remarks

The qualitative richness of the universe is so patent that, a. first sight, the unity of the sciences looks like an impossible dream – as Dupré (1993) and others have claimed. But of course such diversity has always spurred philosophers and scientists to searching for underlying unities, both ontological and epistemological.

For example, far from being dualistic, cognitive and affective neuroscience conceives of mental processes as brain processes; and physiological social psychology supplements the biological account of the mental with a consideration of the social inputs and outputs of cognition and emotion. Besides, the scientific method is being practiced in both research fields, even if there is no consensus concerning its nature. In sum, the multiplication of the sciences of the mind has been accompanied by their increasing convergence. So much so, that all of them, even social psychology, use tools such as electrophysiological recording and functional magnetic resonance imaging.

Arguably, the ultimate root of the unification movements is the materiality, systemicity, lawfulness, and knowability of the universe. Thus, diversity in the details is consistent with overall unity, just as an account of particulars is complementary to the search for pattern. Contrary to what the hermeneuticists contend, all of the sciences are both idiographic and nomothetic, and use the scientific method (see Bunge 1996) in addition to techniques (special methods) of their own.

However, the basic unity of both the world and science does not entail the success of radical reductionism. For example, the reduction of chemistry to atomic physics via quantum mechanics is so far only partial, if only because it requires additional concepts, such as that of covalent bond, as well as additional assumptions, such as the basic rate equation of classical chemical kinetics. Likewise, the reduction of psychology to neuroscience is ontological (in identifying the mental and the neural) but not epistemological, since cognitive and affective neuroscience is a typical interdiscipline that employs such psychological concepts unknown to biology as those of attention and perception, pleasure and fear, empathy and intention, personality and depression.

Another example is the recent completion of the sequencing of the human genome, hyped as the deciphering of the Book of Life, and hailed as a victory of radical reductionism. Although this is undoubtedly a remarkable achievement of Big Science, it was no such philosophy victory, because the gap between genotype and phenotype is still to be filled. Indeed, the participation of proteins in intracellular processes the intercellular interactions that make organs and the whole organism work, are still far from being well known.

In short, here as elsewhere, knowledge of the parts is necessary insufficient to explain how they combine into a whole with (emergent) properties of its own. This is why reduction, though certainly necessary whenever feasible, must be supplemented with integration: Because the world is both one in some respects and diverse in others, and because it is not a pile of simple individuals but a system of systems (Bunge 1979). Everywhere, epistemology and methodology must fit ontology, and the latter must cohere with science. No ontology, no consistent philosophy.

To conclude, modern science is characterized as much by convergence or integration as by divergence or specialization. And convergence happens to be a unification process, just as much as its complement, reduction. Moreover, whereas the cases of strong reduction (or deduction) are few and far between, those of convergence are becoming increasingly frequent.

The reason for the paucity of reduction cases, and the proliferation disciplinary convergence, is the occurrence of emergence. Indeed, if items A and B synthesize into a C with properties that neither A nor B possess, then the disciplines that study A and B prove to be necessary but insufficient to understand C.

So, as soon as qualitatively new things either emerge or are studied for the first time, we need the collaboration of disciplines to understand them. Shorter: Emergence *de re* calls for convergence *de dicto*. And the convergence of disciplines illustrates and vindicates the unified science project, though without the ontological and methodological strictures imposed by neo-positivism.

In other words, unified science, yes – but on a realist basis rather than on phenomenalism. The reason is that reality contains much more than phenomena (appearances). Reality, the universe, is the totality of things-in-themselves on all levels – physical, chemical, biological, social, technological, and semiotic – and every one of them is characterized by properties of its own.

Notes

1. More on phenomenalism and realism in Bunge 2005.
2. More in Bungee 2003.
3. Looijen 2000: 153.
4. For details see Bunge 1990.
5. See, e.g., Wilkins 2002.
6. Arthur 1997.
7. More in Bunge 2003.
8. See, e.g., Gazzaniga 2004.
9. More precisely, the relation holds between the intension or sense of a predicate and its extension or coverage: see, e.g., Bunge 1974.

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Chapter 12
A Comprehensible World

Robert L. Causey

12.1 Introduction

The world is everything there was, is, and will be. The world has particular things, kinds, attributes, states, events, processes, functions, and other stuff I do not now imagine. In order to comprehend phenomena, we need to know *what* facts are true and *why* they are true. We need *knowledge* and *understanding*. I do not believe that we will ever completely comprehend the world, but we can achieve ever-increasing partial comprehension. In order to do this, we need to know what we are seeking – what kind of science must we develop in order to say, justifiably, that we have a comprehensible world? This raises the further question: What are the ingredients of comprehension? Since we are concerned with the present, the past, and the future, we need to be able to describe present, past, and future conditions. If an event surprises us, it may demonstrate some limitation in our understanding. Thus, comprehension also requires the ability to make reliable predictions, when the relevant information, and the necessary reasoning and calculating abilities are available. Of course, this also requires understanding what this relevant information is. Similarly, comprehension requires the ability to make reliable inferences about current or past events that may or may not be known. Yet, it is well-known that such predictive and inferential capabilities are not in themselves sufficient for understanding. Usually, we want these capabilities to be based on some kind of causal model or general causal theory. There is an enormous literature on causal explanation and I shall assume that the reader is generally familiar with this literature and some of the more successful models of causal explanation.

As science develops we discover many facts and limited generalities, e.g., about the acceleration of falling bodies or the refraction of light by a prism. We may also develop explanations of some of these facts and generalities. This development leads to ever-increasing bodies of knowledge and particular explanations. But a large collection of specific explanations should make us wonder how they might be related.

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J. Symons et al. (eds.), *Otto Neurath and the Unity of Science*, Logic, Epistemology, and the Unity of Science 18, DOI 10.1007/978-94-007-0143-4_12,

This can lead to the development of more general theories. A particular explanation of a particular kind of phenomena increases our understanding of the world. A systematic theory that explains an entire domain of phenomena increases our understanding to a greater extent. But if we have several such theories that are successful, we should wonder how they might be related. Just as a particular event may be surprising and demand an explanation, the existence of a set of theories about apparently different types of phenomena may be surprising and demand some kind of explanation. This is especially true if we have some reason to believe that there are important relationships between the apparently different types of phenomena. One way to reduce this surprise, and thereby increase our comprehension, is by a suitable unification of the theories. But unification of theories is difficult, and the goal of unifying all, or most, theories is highly controversial. There are people who maintain that achieving such unification is impossible, or extremely improbable, and that seeking it is a hopeless goal. *Complete unification probably cannot be achieved because we are unlikely to complete all scientific investigations. Yet, I believe that we can make progress towards unification and that we should seek this progress in order to increase our comprehension of the world. In this article I shall argue that unification is an important ingredient of comprehension, and that it is therefore an important scientific goal for guiding research.*

12.2 Some Background on Laws and Theories

The last chapter of my *Unity of Science*, Causey (1977) (henceforth abbreviated as *UofS*), is titled, "Scientific Progress and the Unity of Science." That chapter discusses various aspects of scientific research and states that *basic research* is an inquiry to acquire knowledge and understanding for no specified purpose other than to increase our knowledge and understanding. This contrasts with applied research which has primary goals other than acquisition of knowledge and understanding. Scientific knowledge can pertain to particular events, descriptions of structures, general laws (including probabilistic laws), etc. It is possible to have a body of knowledge about some set of phenomena without having much understanding of the phenomena. Understanding of particular events, or of general laws, results largely from adequate explanations based on general laws other than the ones, if any, that are being explained. Eventually scientific theories are developed. In *UofS* (p. 2), I distinguished *dynamic*, *developmental*, and *evolutionary theories*. Dynamic theories state and explain the general laws governing the attributes and behavior of the various kinds of things in a specified domain. In order to do this, a dynamic theory, **T**, includes some *fundamental* (*or basic*) laws that are used to explain other, *derivative* laws within the theory **T**. The explanations within dynamic theories will be assumed to be causal explanations, broadly interpreted. The fundamental laws function as the axioms of **T** and are not themselves explained within the theory **T**, although they may be explained, at least in part, with the help of some other theory. I shall say more about this in Sections 3 and 5. *Developmental theories* describe and explain the general types of changes which take place over time in certain kinds of things, while *evolutionary theories* are concerned with actual changes that occur in some particular thing or class of things. For the sake of brevity, they will not be discussed here in any detail. For details on their relationships to dynamic theories, please refer to *UofS*, especially Chapter 8.

For the purposes of this article, let us briefly review salient features of dynamic theories. This is a highly abstract sketch; much more detail can be found in *UofS*. A mature dynamic theory, **T**, is concerned with some domain, *Dom*, of objects, and is formulated in terms of logical and mathematical symbols plus a set, L, of nonlogical predicates and function symbols. We assume that $L = T \cup A$, where T is a set of thing-predicates denoting kinds of things, and A is a set of attribute-predicates denoting attributes, i.e., properties, relations, and quantitative terms. In general, **T** will contain two kinds of sentences: law-sentences, which represent or state empirical laws, and identity-sentences which state identities. Identity-sentences may be thing-identities between two thing-predicates, or attribute-identities between two attribute-predicates. I will usually just write "identity" for "identity-sentence". As was argued at length in *UofS* and elsewhere, the identities of a theory are not laws and are not subject to causal explanation. Yet, the empirical (synthetic) identities of a theory must, like laws, be justified or confirmed by empirical data. Most of this confirming data will be indirect; thing-identities, and especially attribute-identities, are often highly removed from direct observation. Thus, law-sentences must be empirically justified and they are also subject to causal explanation, whereas synthetic identities must be empirically justified, but are not subject to causal explanation. More generally, *UofS* (p. 32) introduces the term *noncausal sentence*, which is any sentence that is not subject to a causal explanation. An analytic sentence, such as "All bachelors are unmarried," is noncausal. Also, both analytic and synthetic identities are noncausal, and there are other types of noncausal sentences.

Much more can be said about these concepts but, for our present purposes, we can rely on a highly restricted consideration. Suppose that $L(\alpha)$ and $L(\beta)$ are law-sentences containing the predicates α and β, respectively. Suppose that these two sentences have the same general form, except that $L(\beta)$ has occurrences of β in exactly the same syntactic places where $L(\alpha)$ has occurrences of α. In other words, they are substitution instances of each other. Then, if $\alpha = \beta$ is a synthetically or analytically true identity-sentence, then $L(\alpha)$ and $L(\beta)$ state the same law and may be called *nomologically equivalent* law sentences. Stated roughly, the law that a law-sentence states is invariant under substitution of identical terms into that law-sentence. Moreover, if we have an explanatory derivation and an identity $\alpha = \beta$, we can uniformly substitute β for α in this derivation and the result is a new explanatory derivation that represents the same explanation as the original derivation. Derivations related in this manner may be called explanatorily equivalent. These substitution principles for law-sentences and explanatory derivations were first stated in Causey (1972); they were developed further in other publications, especially *UofS*. Thing- and attribute-identities, and these substitution principles, play an essential role in analyses of unified theories and of unification through intertheoretic reductions. *UofS* describes their roles in microreductions in great detail. In particular, an adequate reduction of $\mathbf{T}_2$ to $\mathbf{T}_1$ requires the identification of the ontology (both kinds and attributes) of $\mathbf{T}_2$ with that of $\mathbf{T}_1$ by means of both thing- and attribute-identities. When this is suitably accomplished, the fundamental law-sentences of $\mathbf{T}_2$ turn out to be nomologically equivalent to (either fundamental or derivative) law-sentences of $\mathbf{T}_1$. I shall assume general familiarity with these concepts in the following discussions.

Since a theory may contain identity-sentences in addition to law-sentences, we need to expand the logical model of a theory. In particular, a dynamic theory is considered to be a set of sentences, $\mathbf{F} \cup \mathbf{I}$, where **F** is the set of fundamental (or basic) law-sentences of **T**, **I** is a set of identities, and **D** is the set of derivative law-sentences of **T**. Explanations in **T** are represented by suitable derivations from premises (explanans) that may contain fundamental law-sentences and identities. Each law-sentence in **D** is explainable in this manner from subsets of $\mathbf{F} \cup \mathbf{I}$. No law sentence in **F** is so explainable within **T**, although it is possible that the law-sentences in **F** might be explainable from the fundamental law-sentences of some other theory. Thus, "fundamental" is not intended to be absolute, but instead is relative to a specified theory.

12.3 Theory Unification and Comprehension

There have been many different concepts of the unity of science and of unified science. Most of these are well-known. For instance, Carnap (1955) discussed both the unity of a language of science and the unity of scientific laws. Some inquirers have emphasized unification of scientific theories according to various conceptual models of intertheoretic reduction. Others have proposed what they consider to be non-reductionistic models of intertheoretic or interfield unification. I will not undertake a review of all of the various concepts of unification that have been considered; a recent article, Grantham (2004), reviews some of these and has references to many additional sources. Instead of considering the details of particular models of unification, I shall focus on some very broad issues that I have always considered to be the most fundamental aspects of the unity of science. In particular, I shall focus on the unification of a theory (making it unified) or theories (combining them into a unified theory), and show why these kinds of unification are so important for scientific comprehension of the world. Unless otherwise indicated, the terms *unity of science* and *unification of science* will be used here to refer to the unification of a theory or theories. Since a completed unification of science is an ideal limit point, I will frame the following discussion in terms of *means* towards this ideal limit point, and discuss *ways of increasing* the unity of science.

The unity of science (in the sense just mentioned) can increase in three principal ways:

(a) One can increase the unity of a dynamic theory. Suppose that a theory makes use of many specialized laws about phenomena that might be related, but such that the possible relations are not known. Such a theory lacks a common core of a few coherent fundamental laws that explain the other laws of the theory. If the theory is now reformulated in terms of a coherent, preferably small, set of fundamental laws, this will increase our comprehension of the theory's domain and increase unity. The classic example is Newton's formulation of mechanics and gravity to explain myriads of more specialized terrestrial and astronomical laws. It is extremely difficult to formulate general, domain-independent criteria for this type of unification of a theory. *UofS*, Chapter 6, Section B, constitutes an attempt to do this, but the attempt is very preliminary. I believe that the formulation of suitable criteria for the unification of a theory is still an important area of investigation, but it is also a very difficult endeavor involving simplicity, coherence, explanatory adequacy, and other matters. This endeavor is beyond the scope of this article.

(b) One can unify together two theories that were previously separate, or that are being separately developed (perhaps by different scientific research groups). In order to accomplish this fully, one would need to satisfy the still under-developed unification criteria sketched in the previous paragraph. But even without such criteria, two theories can be combined into one theory in such a way that the result is clearly more unified than the original two are separately. It is not necessary to solve completely the problem of unification criteria (a) in order to see that such results can occur. Most of the rest of this section is devoted to showing a few ways in which this can occur, and to arguing that such combinations can increase unity and increase our comprehension of the world.

(c) Science is not limited to dynamic theories, and unification is also not limited to them. A unified science requires that developmental laws (e.g., for chemical or biological changes) be explainable in terms of relevant dynamic theories. If the dynamic theory of a given domain is adequate, then it should be able to do this. Also, in an adequate unified science, actual historical or evolutionary change should be explainable in terms of dynamic theories and relevant boundary conditions. For example, if we have an adequate understanding of chemical and physical processes, and sufficient factual background information, then we should be able to explain why a certain malfunctioning chemical factory produced the wrong proportions of output products. Of course, evolutionary explanations are often crippled by a lack of adequate factual information, but this is a practical, not a theoretical, problem. The basic point here is this: The dynamic theories should be adequate for evolutionary explanations within their domains of phenomena. If they are not, then we lack a unified science and our comprehension of the world is correspondingly limited.

The various forms of explanation discussed earlier are most often applied in connection with some particular scientific problem, or in some domain of related problems. Yet, there are many different branches of science and specialized fields within these branches. This leads to the construction of a variety of different systems of description and classification, i.e., ontologies, and theories using these ontologies.

Henceforth, when I refer to a theory, I shall consider its ontology to be included in the reference. Let us now return to the type of unifications mentioned in the earlier paragraph (b). Naturally, if we have two different theories, say $\mathbf{T}_1$ and $\mathbf{T}_2$, with sets of fundamental laws and identities, $\mathbf{F}_1$, $\mathbf{I}_1$, $\mathbf{F}_2$, $\mathbf{I}_2$, respectively, we will want to understand how these theories may be related. For now let us assume that the two theories are dynamic theories. Here are just some of the possible ways they may be related; it is not an exhaustive list.

1. The fundamental laws of each of these theories may be explainable in terms of the fundamental laws of the other theory without any additional empirical assumptions except, possibly, synthetic identities. In this case the two theories are empirically equivalent.
2. The fundamental laws of one of the theories may be explainable by the fundamental laws of the other with the help of additional assumptions that include new law-sentences.
3. There may be some set of assumptions, say $\mathbf{F} \cup \mathbf{I}$, such that both $\mathbf{F}_1$ and $\mathbf{F}_2$ are explainable by $\mathbf{F} \cup \mathbf{I}$. Although the theoretical vocabularies will overlap, this set of assumptions, $\mathbf{F} \cup \mathbf{I}$, may be totally disjoint from $\mathbf{F_1} \cup \mathbf{F_2}$, or it may have some, but not all, elements in common with $\mathbf{F_1} \cup \mathbf{F_2}$.
4. Since it is well known that some deductions are not adequate explanations (for examples and references, see *UofS*, pp. 23–25), for each of the explanatory possibilities described in 1–3, there is a corresponding possibility in which "explainable" is replaced by "derivable". Moreover, if the theories are explanatorily or derivationally related, then their ontologies may or may not be related in some ways.
5. It is also possible that their ontologies are related in some ways, but that $\mathbf{T}_1$ and $\mathbf{T}_2$ are not explanatorily or derivationally related.

As previously mentioned, this is not an exhaustive list, but it is sufficient to show that there are many ways in which two theories may be related. Some of these relationships may lead to greater comprehension, others may have the opposite effect. Of course, in an extreme case, it is logically possible that the two theories may not be related in any interesting way at all.

A curious person reflecting on the multitude of possible relationships between theories $\mathbf{T}_1$ and $\mathbf{T}_2$ will want to know whether and, if so, how these theories are related. For example, consider a situation in which the two theories derivationally predict some of the same outcomes, but are otherwise different. In particular, suppose that the two theories use *prima facie* different ontologies and law-sentences, yet apply to the same domain of phenomena and make the same predictions within some current observational realm (e.g., some wave and particle theories of visible light). In such a case, we would hope to demonstrate that they are two explanatorily equivalent theories and that their ontologies can somehow be unified. If this is not done, we will be left wondering which, if either, of the two theories truly represents the world, and this amounts to some lack of understanding. In saying this, I am adhering to a realistic interpretation of scientific theories. An anti-realist would probably say that it makes no sense to talk of the truth of the theories, or at least that there is no rational foundation to justify such truth claims; thus, one can use whichever theory better fits one's purposes. The anti-realist can maintain such a position, but I believe that science will progress better if we seek to find a comprehensible world. In order to do this, we need to know what we are seeking – what kind of science must we develop in order to say, justifiably, that we have a comprehensible world?

In the case of $\mathbf{T}_1$ and $\mathbf{T}_2$, a realist would try to determine which is true, or more likely to be true. Eventually some evidence, perhaps including compatibility with other theories, might favor one of the theories much more than the other, eventually resulting in the elimination of the losing theory. Another possibility is that one is reductively explained by the other (with the help of some connecting (bridging) identities), so that their *prima facie* differences actually disappear. It is also possible that both $\mathbf{T}_1$ and $\mathbf{T}_2$ may be reduced to some other theory, $\mathbf{T}_3$, judged scientifically to be better than either $\mathbf{T}_1$ or $\mathbf{T}_2$. Yet another possibility is that both $\mathbf{T}_1$ and $\mathbf{T}_2$ may be totally rejected, after being replaced by some other theory, $\mathbf{T}_3$, judged scientifically to be better than either $\mathbf{T}_1$ or $\mathbf{T}_2$. In all of these outcomes, there are at least three significant results: the two different ontologies of $\mathbf{T}_1$ and $\mathbf{T}_2$ are replaced by one ontological system, the two different sets of laws of $\mathbf{T}_1$ and $\mathbf{T}_2$ are replaced by one set of laws, and the original two sets of explanations are replaced by one set of explanations (exclusively in terms of $\mathbf{T}_1$ or in terms of $\mathbf{T}_2$ or in terms of $\mathbf{T}_3$). Each of these results amounts to a kind of simplification and unification. Of course, if the result is the use of $\mathbf{T}_3$, it is possible that this theory will itself have a more complex ontology and set of laws than either $\mathbf{T}_1$ or $\mathbf{T}_2$, but this result is still a simplification and unification in the sense that two different ontological and theoretical systems are replaced by one. *Explanations of events or laws can increase understanding by reducing surprise or confusion. Correspondingly, ontological simplification and unification can increase our overall comprehension of the world by reducing perplexities resulting from the existence of multiple theories.*

Let us consider another hypothetical situation. Suppose that $\mathbf{T}_1$ is about the domain Dom_1, and that $\mathbf{T}_2$ is about the domain Dom_2. Also, $\mathbf{T}_1$ is formulated in terms of a set of nonlogical, descriptive predicates, L_1, and similarly $\mathbf{T}_2$ is formulated in terms of the set of predicates L_2. Also, suppose that Dom_1 and Dom_2 are not identical but also not disjoint, so they are partially overlapping. Correspondingly, the descriptive languages and the two theories are partially overlapping. Let $\mathbf{F}_1$ and $\mathbf{F}_2$, be the sets of fundamental law-sentences of $\mathbf{T}_1$ and $\mathbf{T}_2$, respectively, and let $\mathbf{D}_1$ and $\mathbf{D}_2$ be the derivative law-sentences of $\mathbf{T}_1$ and $\mathbf{T}_2$, respectively. Let the nonempty intersection, $\mathbf{D} = \mathbf{D}_1 \cap \mathbf{D}_2$, be the set of derivative law-sentences common to both theories. Also assume that $\mathbf{D}_1$ has law-sentences not in $\mathbf{D}_2$, and that $\mathbf{D}_2$ has law-sentences not in $\mathbf{D}_1$. Finally, to keep this example relatively simple, suppose that $\mathbf{F}_1$ and $\mathbf{F}_2$ are disjoint. We now have an interesting situation: $\mathbf{F}_1$ explains $\mathbf{D}_1$, $\mathbf{F}_2$ explains $\mathbf{D}_2$, each of $\mathbf{F}_1$ and $\mathbf{F}_2$ explain $\mathbf{D}$. It is clear that the law-sentences in $\mathbf{D}$ are formulated in terms of $L_1 \cap L_2$ and state laws of phenomena common to the two domains. Yet, we have distinct explanatory derivations of these laws. This is puzzling. Many different situations could occur. I shall now discuss one, very simple case. Suppose that we have the following law-sentences. In $\mathbf{F}_1$ there is

$$(\forall x)(Ax \rightarrow C_1x), \tag{1}$$

and

$$(\forall x)(C_1x \rightarrow Bx). \tag{2}$$

In $\mathbf{F}_2$ there is

$$(\forall x)(Ax \rightarrow C_2x), \tag{3}$$

and

$$(\forall x)(C_2x \rightarrow Bx). \tag{4}$$

In **D** we have

$$(\forall x)(Ax \rightarrow Bx). \tag{5}$$

We have A and B in $\mathrm{L}_1 \cap \mathrm{L}_2$, and for the sake of this example, assume that C_1 is only in L_1, and that C_2 is only in L_2. This situation is to be considered highly schematic and abbreviated. In a more realistic example, instead of just having (1) and (2), we would have additional premises and derivations that eventually result in (1) and (2). Similarly, (3) and (4) would be the result of additional premises and derivations. These derivations are assumed to be explanatory in the sense that they represent relevant causal relationships.

Using our schematic example, it is clear that (5) results from (1) and (2) and it also results from (3) and (4). This is what is puzzling; how can there be two different causal explanations of the same derivative law? A realist wants a deeper comprehension of such a situation. Here are some possibilities: (i) C_1 and C_2 may denote the same kind or attribute, i.e., they are identical; (ii) $(\forall x)(C_1x \rightarrow C_2x)$ may be true, $(\forall x)(C_2x \rightarrow C_1x)$ may be true, or

$$(\forall x)(C_1x \leftrightarrow C_2x)$$

may be true; (iii) All of the possibilities included in (i) or in (ii) may be false.

If (i) holds, then the law-sentences (1) and (3) state the same law, and the sentences (2) and (4) state the same law. In this case, we really have only one explanation, although it is expressed in two different formulations. We have achieved a limited unification.

Suppose that we have the case (ii) in which $(\forall x)(C_1x \rightarrow C_2x)$ holds. Call this law-sentence L. From L and (1) we can derive (3), and from L and (4) we can derive (2). Thus, we can replace the assumptions (1) through (4) by the set of assumptions $\{L, (1), (4)\}$. The latter set implies (2) and (3), as well as (5). Making this replacement achieves at least partial unification of $\mathbf{T}_1$ with $\mathbf{T}_2$. The other cases included in (ii) also yield partial unifications of the two theories. Note that the biconditional

$$(\forall x)(C_1 x \leftrightarrow C_2 x)$$

is not the same as a thing- or attribute-identity, so the last case of (ii) is different from case (i).

Finally, suppose that (iii) is the case. At first glance, this situation may appear to be a severe challenge to any unification, but it deserves closer examination. Since (1)–(5) are all universally quantified, I shall simplify the notation in the following discussion by omitting the quantifiers. Then, by conjoining (1) with (3), we obtain

$$(Ax \rightarrow C_1 x) \wedge (Ax \rightarrow C_2 x). \tag{6}$$

Of course, in general, a mere conjunction of two law-sentences is not by itself sufficient for a meaningful unification; such a conjunction may not even be a law-sentence. Also, it is well known that conjunctions can lead to derivations that do not represent any useful causal explanation. Yet, as will be seen, the current situation is rather special. Note that (6) is logically equivalent to

$$Ax \rightarrow (C_1 x \wedge C_2 x). \tag{7}$$

Also, by conjoining (2) with (4), we obtain

$$(C_1 x \rightarrow Bx) \wedge (C_2 x \rightarrow Bx), \tag{8}$$

which is logically equivalent to

$$(C_1 x \vee C_2 x) \rightarrow Bx. \tag{9}$$

Although no new empirical information has been added to the example, (7) and (9) can be considered to be law-sentences in a new, unified theory (which presumably will eventually contain additional law-sentences). Clearly, (7) and (9) imply the derivative law-sentence (5), that is, $(Ax \rightarrow Bx)$. I maintain that (7) and (9) increase our overall comprehension of the situation at hand, and that they point towards additional scientific investigation. Instead of explaining (5) in two different ways within two different fundamental theories, we now have one explanation of (5). Formula (7) at least strongly suggests that each of the conditions C_1 and C_2 is a causal consequence of A. Formula (9) tells us that B is a causal consequence of either C_1 or of C_2. (I am bypassing here further questions about the nature of disjunctive attributes.) Awareness of these relationships provides a more coherent and unified understanding of the phenomena under consideration, and it provides guidance for further research.

I realize that this is a very abstract and general discussion, so let us look at a particular, concrete example. It is a hypothetical example, but it illustrates a type of situation that could arise in actual scientific research. In this example, suppose that Dom_1 and Dom_2 are distinct sets of animals of different kinds, and that they also have a nonempty intersection that includes human beings. Also assume that *philosophine* is a hypothetical drug similar to morphine. We now interpret the previously mentioned predicates as follows:

Ax denotes: x is human and x has philosophine in its blood.
C_1x denotes: x has decreased awareness of painful stimuli.
C_2x denotes: x has suppression of anxiety.
Bx denotes: x has decreased responsiveness to painful stimuli.

If one scientist confirms (1) and (2) under this interpretation, it will appear that philosophine in the blood causes decreased awareness of painful stimuli, which in turn causes decreased responsiveness to painful stimuli. If another scientist confirms (3) and (4), it will appear that philosophine suppresses anxiety, which in turn causes decreased responsiveness to painful stimuli. Yet, by conjoining and unifying the law-sentences as previously described, we obtain a more general understanding of the neurological effects of philosophine. This broader understanding also suggests that there may be additional causal paths to investigate, and detailed biochemical mechanisms to study.

I concede that these additional paths of investigation could take place within the separate theories, $\mathbf{T}_1$ and $\mathbf{T}_2$. Yet, suppose that these investigations are successful and we learn a great deal about the biochemical mechanisms working through C_1, C_2, and additional pathways. Since all of these start with one kind of molecule, that of philosophine, it is natural to look for similarities in the pathways. It is possible that the pathways may turn out to be different, especially if the philosophine decomposes into different kinds of product molecules. On the other hand, there could be similarities. For instance, the philosophine molecule may become attached to different kinds of receptors in the nervous system. This is a recent hypothesis for the actions of morphine and some other drugs. In whatever way all of this might turn out, our comprehension of the world will increase to the extent that we can find similar explanations, within a coherent context, of similar types of phenomena.

It has been proposed by some that the key to most (if not all) successful scientific explanations is some form of unification. There are difficulties with this thesis, and I do not endorse it. Yet, certain forms of theoretical unification can enhance our understanding and provide general formats for a class of explanations. This idea is difficult to make precise, but it deserves more investigation by philosophers of science. Kitcher (1999) is one attempt along these lines. The technical details of his approach are complex and qualified in various ways. Yet, the basic idea is very clear. As he says (p. 185, his italics), "*Science advances our understanding of nature by showing us how to derive descriptions of many phenomena, using the same patterns of derivation again and again, and, in demonstrating this, it teaches us how to reduce the number of types of facts we have to accept as ultimate* (or *brute*)." Notice that the repeated use of the same patterns of derivation would play an important role in the unification of a dynamic theory, which is the first form of unification that I previously sketched in paragraph (a) of the list of three principal methods of increasing unity of science at the beginning of this section.

On the other hand, theories about very different domains are likely to use different patterns of explanation, for instance, classical geometrical optics compared to Newtonian mechanics. Unifying two such theories together can be extremely difficult, as the history of science shows for optics and mechanics. But when the two theories have some overlap, as in the hypothetical example about philosophine, there will be a chance of unifying them. The more that we can achieve this kind of unification of explanations and of theories, the greater will be our comprehension of the world. *Thus, ceterus paribus, unification of theories generally increases comprehension of the world.* I shall now compare this result to some different views about unification.

12.4 Two Views About Disunity

There have been many arguments for the disunity of science. One well-known example is Fodor (1974). *UofS*, pp. 142–151, presents a critical reply to Fodor's arguments for disunity. Probably the most discussed of these arguments is based on the claim that higher-lever properties (e.g., mental states) can be produced by, or correlated with, many different underlying lower-level states or systems (e.g., different brain states in different kinds of organisms). This type of argument is said to depend on *multiple realizability*, an issue that has generated a large and, in places, polemical literature. Since it has already been treated in *UofS* and elsewhere, for example Kim (1989), it will not be discussed here. Instead, this section will examine two, much more general doubts regarding unification of science.

12.4.1 Neurath's Encyclopedia

Neurath (1955) is titled, "Unified Science as Encyclopedic Integration." This section of the *International Encyclopedia of Unified Science* states Neurath's interpretation of the history of the unity of science movement and his formulation of the goals of the encyclopedia he helped to create and co-edit. Among other things, he wrote (p. 2) that the *Encyclopedia* ". . .aims to show how various scientific activities such as observation, experimentation, and reasoning can be synthesized, and how all these together help to evolve unified science. These efforts to synthesize and systematize wherever possible are not directed at creating *the* system of science. . ." (his italics). Neurath thus describes the goals in both positive and negative terms. Efforts devoted to the positive goal of synthesizing scientific activities could produce useful and interesting results. What concerns me here is Neurath's statement that the efforts to synthesize and systematize are not directed towards *the* system of science. What he means is clarified later (p. 20) where he refers to ". . .a 'super science' which is to legislate for the special scientific activities." He then asks, "If one rejects the idea of such a super science as well as the idea of a pseudo-rationalistic anticipation of *the* system of science, what is the maximum of scientific co-ordination which remains possible? The answer given by the unity of science movement is: an encyclopedia of unified science." He goes on to say that this encyclopedia may contain contradictory suppositions, and that "One may try to eliminate such contradictions, but in the historically given science, and so in a real encyclopedia, these and other difficulties always appear." Moreover, "An encyclopedic integration of scientific statements, with all the discrepancies and difficulties which appear, is the maximum of integration which we can achieve." Neurath contrasts his view of "encyclopedism" with a Laplacian view of completed science. It is not entirely clear here whether Neurath believes that a completed science is impossible. He does say (p. 21) that "Such encyclopedism is the expression of a certain skepticism which objects not only to metaphysical speculations but also to overstatements within the field of empirical sentences."

The actual state of science *at any given time* is likely to contain some discrepancies and contradictions, but that is not a justification for concluding that such a state of affairs is the "…maximum of integration which we can achieve." Neurath appears too willing to accept these contradictions, since his view can be interpreted to mean that there are (or will be) particular contradictions that will *never* be eliminated. On the other hand, perhaps all he meant is that contradictions will occur, then they will be eliminated, but then new contradictions will occur and be eliminated, etc. One can accept the latter interpretation, but I find the former interpretation to be too complacent, for it may discourage scientists from trying to eliminate discrepancies and contradictions. Indeed, a serious contradiction within a major theory, or between major theories, demonstrates a deficiency in our comprehension. Such contradictions should be, and often have been, motivations for important new investigations and theoretical developments. When successful, these developments can enhance comprehension of the world. The term "super science" can mean different things to different people, and Neurath is wise to be skeptical of some of these meanings. *Yet, the ideal of unifying theories in order to achieve greater comprehension is a useful guiding principle for research even if an ultimate "super science" may not be achievable.* Although Neurath's project of building an encyclopedia of unified science is a worthy endeavor, it should not divert us from other goals.

12.4.2 Dupré's Disorder of Things

Many of the arguments for the disunity of science claim that some particular intertheoretic reduction is not feasible, and such arguments often assume that some current empirical or conceptual obstacle to a reduction will be a permanent obstacle. In this article I want to address broader issues pertaining to the general unification project. In recent years John Dupré (1993, 1996) has attempted to develop a broad critique of this project. I cannot review his extensive work here; instead, I shall briefly describe what I take to be his central thesis. Dupré has a far more favorable attitude than Neurath towards metaphysical arguments. He writes (1993, 1), "It is now widely understood that science itself cannot progress without powerful assumptions about the world it is trying to investigate, without, that is to say, a prior metaphysics." After mentioning some scientifically fruitful, past metaphysical assumptions, he asserts (1993, 2) that

> The metaphysics of modern science, as also of much of modern Western philosophy, has generally been taken to posit a deterministic, fully law-governed, and potentially fully intelligible structure that pervades the material universe. The rejection of this set of assumptions is what I mean by 'The Disorder of Things.'

Dupré then proceeds to argue against what he calls essentialism, reductionism, and determinism, and these arguments presumably support the thesis that things really are disordered. Most of his book is devoted to these arguments, but I shall not discuss them here. It should be mentioned, however, that I have never considered any kind of strict determinism to be a requirement of the unity of science, and it is likely that many others interested in unification do not require determinism. It is quite possible that the universe is law-governed, but that some laws are probabilistic rather than strictly deterministic. I consider this possibility to be compatible with a unified science. Dupré has lengthy discussions of determinism in his book, and it is possible that he agrees that it is not an essential part of the unity of science. Be that as it may, if things are indeed disordered in the way that Dupré claims, then we must have a world (or material universe) that is not fully law-governed and not even potentially fully intelligible. What could justify such a proposition? Dupré is aware that it might be difficult to establish such a complex proposition about the world. Specifically, he writes (1993, 2):

> . . .whereas I accept the dependence of scientific inquiry on a complex body of fundamental presuppositions, I also claim that empirical inquiry (which I do not limit to scientific inquiry) provides the evidence on which such assumptions must ultimately rest. Thus I claim that founding metaphysical assumptions of modern Western science, most notably those that contribute to the picture of a profoundly orderly universe, have been shown, in large part by the results of that very science, to be untenable. And this, in turn, shows the impossibility of a unified science.

This is a very broad and rather vague assertion, but it is fair to interpret it to mean at least the following: The development of modern science depends, at least in part, on some metaphysical presuppositions. These presuppositions lead to certain kinds of expectations about how science should develop. In particular, some of these presuppositions lead to the expectation that science can find intelligible orderliness in the world. Yet, the actual results of modern science conflict with these expectations. Therefore, we cannot defend, and should give up, the relevant metaphysical presuppositions. Furthermore, according to Dupré, giving up these presuppositions implies that a unified science is impossible. Yet, it is still not clear how Dupré could justify such strong assertions.

Dupré's book on the disorder of things presents extensive discussions of essentialism, reductionism, and determinism, with considerable focus on the philosophy of biology. These are important and interesting subjects, but I am not convinced that his treatments of them must lead to the very general, skeptical position conveyed by the previous quotations. Fortunately, Dupré (1996) clarifies his position by way of a very general argument about classification systems. He writes (p. 105), "I suggest that many individual things are objectively members of many individual kinds." He says, for example, that he is a human, a primate, a male, a philosophy professor, and many other things, and that "All, or at least many, of these are perfectly real kinds; but none of them is *the* kind to which I belong." (his italics). He adds, "But I see no reason why many overlapping and intersecting kinds might not be equally and genuinely real." A little later he states what appears to be his main thesis (pp. 105–106):

> This combination of pluralism and realism, what I have sometimes referred to as promiscuous realism, provides the starting point for seeing the robust metaphysical basis that I suggest underlies disunified science. For if there are numerous distinct ways of classifying objects into real kinds, any one of which schemes of classification could provide the basis for a properly grounded project of scientific inquiry, then there can be no reason to expect a convergence of these projects of inquiry onto one grand theoretical system.

My first response to this argument is that I am perfectly willing to allow multiple classification systems. For instance, we can classify the elements in terms of their macroscopic physical and chemical attributes, and we can also classify them in terms of their atomic structures. These classification systems are related through the kind of reductive explanations that are examined in great detail in *UofS*. They are compatible classifications and each is scientifically acceptable. Therefore, the existence of multiple classification systems applying to the same objects does not, *per se*, provide for a robust metaphysical basis for disunified science. I agree that the existence of multiple classification schemes does not provide a reason for a convergence into one grand theoretical system. Yet, as just exhibited, it also does not provide any robust basis to the contrary. Hence, promiscuous realism may have a seductive voice, but if all it says is that there can be multiple, compatible classification systems, then it is itself compatible with a unified science, and it is not very exciting in spite of its name.

The previous paragraph discussed a benign form of promiscuous realism in which there are different, but compatible classification systems and theories. It seems clear, however, that Dupré does not discern a benign world. Recall that he says that one feature of the metaphysics of modern science (as he understands it) is that the world has a potentially fully intelligible structure. But he rejects this and claims that a unified science is impossible. He also claims that promiscuous realism is the metaphysical basis for disunified science. Therefore, his promiscuous realism is powerful and not benign. But in order to have this power, it must involve more than merely compatible classification systems. To understand his view, we must consider his words more closely. As previously quoted, Dupré (1996, 105) states, "But I see no reason why many overlapping and intersecting kinds might not be equally and genuinely real." The next two sentences continue, "This would preclude the general possibility of answering one kind of question to which a theory of kinds has traditionally seemed relevant, questions as to what (unique) kind a particular individual belongs to. But I see no reason why there should be any answers to such questions, any more than there need be an answer to the question what color something is (think of rainbows or peacocks).” Unfortunately, Dupré’s writing often relies on negative characterizations of what he does not accept, rather than clear, positive statements of what he does accept. Nevertheless, from these quotations, it is fair to interpret his promiscuous realism as allowing some incompatible, or perhaps “incommensurable”, classification systems that are considered to be scientifically equally well confirmed. Moreover, he believes that such incompatible or incommensurable systems exist in actual science (1993, 36, 58, 104–105, 262–263). Yet, even if there are such systems, basing a strong doctrine such as promiscuous realism on the present state of science is simply not justified, since the future state of science may be quite different from its present state and our views about what is scientifically feasible (or not) may be correspondingly different. Moreover, Dupré’s promiscuous realism is not only not benign, it is actually pernicious because it implies that the world is to a large degree unintelligible. This “realism” tells us that the world is disorderly, and that it contains things that can be classified in more than one incompatible or incommensurable way. Since classification depends on lawful behavior, such promiscuous classification implies that there are things in the world that can be “scientifically and legitimately” described as following incompatible or incommensurable laws. Such a world would be largely incomprehensible, and this is not surprising, given the arguments in the previous section that theory unification is an ingredient in our comprehension of the world.

Yet, as odd as it sounds, could *believing in* pernicious promiscuous realism perhaps promote the advance of science and therefore have a pragmatic justification? The answer to this question depends on what practices one believes follow from promiscuous realism. If one sees it as freedom to pursue a variety of different approaches to theory construction, that is fine, and may actually encourage scientific creativity. But we do not need a *pernicious* promiscuous realism for this; benign promiscuous realism suffices. On the other hand, if one sees the pernicious form as prohibiting or discouraging attempts at theoretical unification, then the effect could be deleterious. Recall the previous example about philosophine. In that example a successful unification of $\mathbf{T}_1$ and $\mathbf{T}_2$ could lead to a more general theory that increases our comprehension of the world. There is no guarantee that this would be the outcome, but the benefits of achieving it are worth some cost in trying to achieve it. Here is another, different kind of example. Suppose that in the future we explore an extrasolar planet orbiting a distant star and we discover that in that extrasolar planetary system the law of gravity is a little different from the law in our solar system. In the spirit of promiscuous realism, one could simply define new categories of planetary systems that obey differing laws of gravity. It could then be said that there is no need to explain the differing laws, because they apply to different kinds of things. Yet, I am confident that most scientists would consider such moves to be *ad hoc*. Instead, they would seek a more general theory that makes the gravitational differences understandable. Pernicious promiscuous realism would not encourage, and perhaps would discourage, research efforts towards unification. It could thereby be an impediment to increasing our comprehension of the world. I conclude that a benign promiscuous realism does not imply that scientific unification is impossible or infeasible, and that it does not support an anti-unification methodology. On the other hand, pernicious promiscuous realism is contrary to unification, and adopting it would promote inexpedient scientific methodology. It is also not justified.

12.5 Significance of Structure and Reduction

Most discussions about the unity of science have centered on issues regarding the reducibility of one theory to some other theory. This is not surprising because one way to unify two theories is by reducing one of them to the other, or by reducing both of them to a third, more general, theory. But the significance of reductions goes beyond this local interest to a more global concern. It has been proposed to unify all dynamic scientific theories by reducing all but one of them to one most general and fundamental theory. Furthermore, since there is strong empirical evidence that all matter is composed of some kind of fundamental particles, it is often assumed that the most general and fundamental theory should be the best theory about such fundamental particles. The main type, but not necessarily the only type, of reductions that would be used in this research program would be microreductions. Unfortunately, the nature of microreductions and role of structures in them often seems to be poorly understood. In this section I will point out some misunderstandings and briefly indicate how to avoid them. First we should briefly review the essentials of an adequate microreduction as developed in *UofS*.

As in Section 12.3, suppose that we have two theories, $\mathbf{T}_1$ about the domain, Dom_1, and $\mathbf{T}_2$ about the domain, Dom_2. $\mathbf{T}_1$ is formulated in terms of a set of nonlogical, descriptive predicates, L_1, and $\mathbf{T}_2$ is formulated in terms of the set of predicates L_2. Further assume that $L_i = T_i \cup A_i$, for $i = 1, 2$, where T_i is a set of thing-predicates and A_i is a set of attribute-predicates (for properties, relations, and quantities). From the point of view of $\mathbf{T}_2$, the elements in Dom_2 are considered indecomposable, *basic elements*. From the point of view of $\mathbf{T}_1$, $Dom_1 = Bas_1 \cup Comp_1$, where Bas_1 is a set of *basic elements* and $Comp_1$ is a set of *compound elements*. The basic elements of a theory are treated as indecomposable from within that theory, while the compound elements are structured wholes composed of two or more basic elements of the theory. The concepts of *basic* and *compound* are always used relative to a theory. Moreover, the predicates in T_1 refer only to the elements of Bas_1 and the predicates of A_1 refer only to attributes of these basic elements. Thus, predicates for compound elements and attributes of these compound elements must be definable within L_1.

The concept of *structured whole* is roughly that of a set of objects, the *parts* of the whole, combined, or *bonded*, together into a relatively stable configuration that constitutes a new object, the *whole*. *UofS* has extensive analysis of the concept of structured whole, additional analysis for social structures is in Causey (1980), and a much more detailed analysis of bonding and structured wholes is in Causey (2005). I can only remark on a few salient points here. In L_1, a kind of structured whole is described by a *structural description*, which describes its parts and their structural arrangement. This description must be stated within L_1 in order for the kind of structure to be definable in terms of its parts and relations between these parts. Each kind of element in $Comp_1$ must be a structured whole and the attributes of these compound elements are *compound attributes*. Predicates for these compound attributes must also be definable with L_1. In addition to all of this, an adequate microreduction of $\mathbf{T}_2$ to $\mathbf{T}_1$ at least requires the following: Each kind of element in Dom_2 must be identified with some kind of element in either Bas_1 or $Comp_1$, every attribute denoted by a predicate in A_2 must be identified with an attribute in, or definable in, L_1, and all of the fundamental laws of $\mathbf{T}_2$ must be explainable in terms of the fundamental laws of $\mathbf{T}_1$ plus identity sentences identifying the kinds of elements of Dom_2 and their attributes to kinds of elements and attributes of Dom_1. This synopsis should be adequate background for the following remarks.

It is an undeniable, although empirical, truth that structures (i.e., structured wholes) exist in the world: atoms, molecules, cells, organisms, planetary systems, ant colonies, bridges, airplanes, families, and so on. Both reductionists and antireductionists agree on the existence of at least some of the kinds of structures listed here. Yet, reductionists and antireductionists disagree over the extent to which such structures can be comprehended, and this disagreement leads to other disputes about the scientific methodologies that should be used in investigating structured wholes. My view is that we should try to develop adequate understanding of structures. This involves a number of factors. Perhaps the most basic issue about structures is their existence. More precisely, certain types of parts (in a Bas_1 of some theory $\mathbf{T}_1$) can form a relatively stable structure with a specified description in a specified environment. Yet, in the same kind of environment, under a different structural description the same kinds of parts may not form a stable structure. If $\mathbf{T}_1$ is an adequate theory of Bas_1, then it should be able to explain why the elements of $Comp_1$ are stable structured wholes in specified environmental boundary conditions. It should also explain why other possible structures, with descriptions different from those corresponding to $Comp_1$ elements, are not stable under any environmental conditions, i.e., they do not exist. A corresponding principle applies to attributes of structured wholes; they should be explainable within $\mathbf{T}_1$ with the help of the definitions mentioned in the previous paragraph of these compound attributes.

It should be noted that laws about the behavior of a kind of Bas_1 element will, of course, make reference to the environmental boundary conditions in which the element is situated, and the behavior may change in different environments. But if some Bas_1 elements are bonded together into a structured whole, then the environment of these elements will be somewhat determined by the fact that they are constrained within this structure. Thus, elements bonded into structured wholes will be somewhat affected by a kind of causal feedback resulting from the existence of the whole which they compose. An adequate $\mathbf{T}_1$ theory should also account for this feedback phenomena. The calculations required for this may be complicated and may require successive approximations, depending on the magnitude of the causal feedback. In some structures this feedback effect may be nearly negligible; in others it may be highly significant.

I am aware that all of these adequacy conditions (and other details not mentioned here) can be very difficult to satisfy. I do not claim that, at a particular stage of scientific progress, we can always construct theories that satisfy these adequacy conditions, and correspondingly we may not, at a particular stage of scientific progress, be able to develop desired theory reductions. In fact, in many cases the development of the reducing theory and the reduced theory may proceed more or less simultaneously. These conditions for microreductions are not intended to apply to actual theories in the history of science (although they may in some cases). They are intended to describe an ideal result for the explanation of wholes in terms of parts. There has been considerable progress in developing such microreductions, and this progress continues. *Anyone who claims that these adequacy conditions are impossible to satisfy for some domain of phenomena is simply denying that we can adequately comprehend that phenomena. He or she is hindering, not aiding, scientific progress.*

Unfortunately, the well-known article of Oppenheim and Putnam (1958) proposed an oversimplified model of the world in which structures are portrayed as a stack of layers, from elementary particles through atoms up to social groups. Although I am confident that they realized the simplifications, it must be emphasized that unification through microreductions does not depend on such a simple model of the world. Already in *UofS*, pp. 133–138, it is pointed out that the relational network between different branches of science is not a linear order, but much more complex. The domains of a set of theories to be subjected to microreductions could very well be represented by a complex network of part-whole relationships (see *UofS*, p. 137, Fig. 3), yet the basic requirements for microreductions might still be satisfied.

Because of the complex part-whole relationships that can exist, the model of microreductions sketched above must be treated as the simplest such model, where it is understood that this model is subject to elaboration in more complex situations. For example, a human body contains many things: hair, skin, blood, internal organs, cells, molecules, etc., that have complex structural relationships. Suppose that one wants a microreductive explanation of some property of the body, say, how its blood pressure varies under differing conditions. Such an explanation will depend on many different factors: the heart, kidneys, structure of blood vessels, chemical state of the blood, the nervous system, hormones, etc. An explanation of this form will involve structures and states on several "levels", so that the simple model of microreductions described here will need to be elaborated in various ways. Yet, most of the fundamental ideas of this model of microreductions should apply to these *hierarchical structures*, except in more complicated forms. In particular, parts inside of a complex hierarchical structure may be especially susceptible to the type of internal environmental (causal feedback) constraints previously described. This can severely complicate explanatory theories about such objects. There have been many examinations of these issues: an old one is *UofS*, pp. 138–142; an interesting, recent analysis is Emmeche et al. (2000).

Here is one final, but important, comment about structures. As already mentioned, structured wholes play an essential role in microreductions, and I consider a structured whole to consist of a set of parts bonded together. There are many kinds of bonds: chemical bonds, rivets and welds that hold together the parts of a metal bridge, ligaments and tendons in the human body, etc. Bonds can be flexible, e.g., two rubber balls held together by a rubber band. The atoms in a molecule are generally in motion and chemical bonds have flexibility. Structures can be highly movable, e.g., gravitational bonds hold together the parts of a planetary system in which the planets orbit a star and satellites orbit planets. But a structured whole must exhibit bonds and these bonds, in a specified external environment, must hold the parts in a relatively stable configuration with respect to each other. For example, the links of a chain are bonded together by their own interconnections and are movable within certain bounds, but these interconnections maintain the parts of the chain into a relatively stable configuration with respect to each other.

For contrast, consider this example from Causey (2005, 445). Suppose there are several balloons inflated with air. In a still room, each of these balloons would, if unsupported, slowly fall to the floor. Suppose, however, that a number of streams of air are directed towards the center of the room above the floor from several different strategically placed blowers. Imagine that a clump of several balloons is positioned above the floor where the air streams converge. The balloons are not attached to each other, but each one is either barely touching one or more neighboring balloons, or is close by and not touching. Thus, there is no significant frictional, or other connection between the balloons. Finally, suppose that the balloons and the airstreams are so arranged and balanced that the clump of balloons remains suspended above the floor in a fixed configuration. In this highly unlikely situation the balloons have a stable configuration with respect to each other, but the stability is not a causal result of bonds between the balloons. Instead it arises from environmental constraints. This is not a structured whole.

Thus, conceptually, a structured whole has parts in a stable configuration where the stability is a causal result of bonds. We still need an analysis of what a bond is. This is complicated, so I shall leave that notion vague and intuitive here. Details about bonding relations, and the effects of environmental conditions on structures, are developed in detail in Causey (2005). For the present purposes I want to stress one very important point: The concept of a bond between two objects, say *a* and *b*, requires, *inter alia*, that the degree of freedom of *a* and *b* with respect to each other is causally constrained by the interaction of *a* and *b* within a specified environment. This is what is lacking in the balloon example and is what is present in the other examples of structured wholes. But what is degree of freedom? The intuitive idea is this: In an ideal, totally unconstrained situation, the objects *a* and *b* can “move” around in a *configuration space* in all directions and dimensions of the space. This is their maximal degree of freedom. If the ability of *a* (or *b*) to move is bounded in some way, this is a limitation of its degree of freedom. Such limitations can be caused by purely external boundary conditions (the balloon example) or by bonds (the structured whole examples). In the latter cases, the degrees of freedom of both *a* and *b* with respect to each other are limited as a causal result of their interaction, and this is what constitutes a bond. Of course, environmental conditions also play a role, so the entire analysis in Causey (2005) is complex.

The salient point here is this: A configuration space may be a standard physical space, but it also may be multidimensional and very abstract. I believe this point is especially important when considering social structures. My concept of a social structure involves agents (animals, people, robots, corporations, political parties, etc.) that are able to exhibit certain types of behavior in a configuration space of behavior. Agents often are abstract types, such as mother, son, police officer, etc., characterized in terms of social roles they play. Agents may also be social structures themselves, such as corporations in an economic system, and individual teams in a sports league. The configuration spaces of behavior vary depending on what kind of agents are under consideration. The space of behavior of bees in a hive is very different from that of humans in, e.g., a band of Karankawa Indians in 1530 in the territory that is now called "Texas". A social structure, such as a family, involves a set of agents whose relative behavior is constrained by social bonds. Causey (2005) describes a hypothetical social structure that comprises three robots bonded together by bonds in an abstract space of behavior. *Since role playing involves aspects of agent behavior, any microreduction of a social structure presupposes some adequate theory of agent psychology.* In the case of conscious role playing, agent cognitive psychology will be required. Thus, any significant discussion of social structures and their reducibility must take into account the highly abstract concepts of agent, configuration space of behavior, social bond, and role playing. Doing all of this for any moderately complex animal or human social system is likely to be very complicated. There are many discussions of these issues in the social science literature, and Ruben (1985) contains useful philosophical analyses along with a critical review of much of the relevant philosophical literature. In the future, I believe that our understanding of social structures and other complex systems, including the kind of internal causal feedback already mentioned, will be enhanced by the use of computer calculations and simulations. There are now several research programs and institutes devoted to the study of complex systems. A few examples are: The Santa Fe Institute (www.santafe.edu), The Lagrange Interdisciplinary Laboratory for Excellence in Complexity (www.complexity-research.org), The Center for Study of Complex Systems (www.cscs.umich.edu); and a general directory is available at The CNA Corporation (www.cna.org/isaac/Complex.htm).

12.6 Concluding Remarks

I hope that these arguments and examples demonstrate that the unity of science, as described here, is a valuable part of the scientific comprehension of the world. Of course, some have claimed that unification is impossible. I hope that this paper shows that these claims are not justified. In fact, there has been enormous progress towards unification and enormous increase in our understanding of the world, especially during the past four centuries. There are no guarantees of the extent to which progress can continue, and there are certainly some large obstacles, but I do not believe that most of these obstacles are impossible barriers; they are challenges. Needless to say, the nature of the mind is still very much a mystery, and the mind-body problem will persist for a long time, perhaps as long as there are beings who can think about it. In spite of this, our understanding of special kinds of mind-body relationships will increase. Yet, I do wonder whether there may some kinds of conceptual or logical limits in our ability to understand our own minds; but this is an issue for a different occasion.

Many challenges result from complexity, and science needs to study increasingly complex systems if we are to comprehend the mind, ecological and economic systems, climate changes, and other phenomena. It is often said that laws about such phenomena will have exceptions, for instance, Fodor (1974) claims this about psychological and social laws. *UofS* (pp. 142–151) provides direct arguments against his reasons. However, I am now more inclined to believe that we probably will use laws with exceptions in some areas where great complexity is involved. This is because the complexity will result in some anomalies that are, in practice, uncontrollable and unexplainable. Fortunately, research on defeasible reasoning during the past three decades can help us to reason with defeasible conditionals, and law-sentences formulated using them; see Ginsburg (1987) for important early sources, Causey (1991) for a foundational theory, and (1994) for a computer implementation that may have scientifically useful applications. I hope that the use of defeasible law-sentences can be avoided, but if we do use them, it will be interesting to investigate how such laws could be reduced, at least approximately, to strict, non-defeasible laws.

More generally, the scientific use of complex computer simulations, and of defeasible reasoning, will raise important questions about exactly what counts as an adequate explanation. In the future our concept of a comprehensible world will probably include the use of computers as a method to expand our own thinking. This should not be a surprise or a concern. The invention of new methods for representing, processing, and storing information can enhance our powers of reasoning and comprehension. This has happened with the inventions of writing, axiomatic geometry, number systems, perspective drawing, algebraic notation, analytic geometry, formal logic, sound and motion picture recording, and many other representational and processing methods. Use of computers can also help to enhance our comprehension in many different directions. If not used wisely, this use could lead to increasing specialization within science, and threaten to break it into many independent fragments. But a breakup of this kind is not inevitable; by using computations to manage huge complexities, I believe that computers will help us to move in the direction of a unified science.

Acknowledgment As always, I thank my wife, Sandy, for helpful comments on an earlier draft of this paper.

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Chapter 13
The Role of Biology in the Unity of Science Program

Juan Manuel Torres

13.1 Introduction

First of all, it would be advisable to keep in mind a very valuable distinction, one that is frequently forgotten in the usual discussions about the unity of science. This distinction states that one thing is the unity of science and another very different one is its unification. In other words, we are talking about the difference between the notions of *unity* and *unification*. In order to achieve unity, the identification of some element that is common to all scientific disciplines – for which we could say that they have unity in connection with that element – would be enough. An example of this would be the classical doctrine assessing that all sciences share the characteristic of being founded knowledge. Another example would be the use of some general method, which would transcend specific methodologies used in the diverse fields of knowledge. During the nineteenth century, the inductive method was considered by many the common denominator in sciences, whereas the hypothetic deductivist methodology had this honor for many decades in the last century. In this sense, it is interesting to remember that the hermeneutic stream originated precisely in refusing to accept a common methodology for the natural sciences and the humanities, as well as for the social disciplines.

In order to assess the unity of science, it is not necessary for the common factor to be intrinsic to the sciences themselves. Strategies used for discovering or the purposes of the scientific community would be enough for this. Today, there is a strong tendency in favor of the disunity of science in view of the variety of scientific languages, practices, strategies, and epistemological perspectives that we can observe in academic life. The well-known book by Galison and Stump (1996): *The Disunity of Science: Boundaries, Contexts, and Power* relieves us from enumerating the arguments against the unity of science. However, many criticisms against the unity of science should be understood as criticisms against its unification when properly analyzed. However, it is not necessary to consider unity as something given.

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J. Symons et al. (eds.), *Otto Neurath and the Unity of Science*, Logic, Epistemology, and the Unity of Science 18, DOI 10.1007/978-94-007-0143-4_13,

A common factor can always be constructed *ex professo* for the benefit of science. Such would be the case of the construction of a common language, a kind of scientific *Esperanto*, as pursued by Otto Neurath. Certainly, such a common language would not ensure agreement among scientists, but at least it would make possible to establish a dialog among them. When Alfred Tarski occasionally defined the Philosophy of Science as the branch of Philosophy that analyzed what is common in sciences, he thought that terms such as "hypothesis", "law", "confirmation", etc. should have the same meaning for all scientists.

On its side, the unification of science entails much more than its unity; it entails that the scientific laws of the different disciplines can be integrated in a harmonic and continuous whole. This integration might take place in two senses. In the strong sense, unification would consist in a system of fundamental laws – traditionally, physical laws- from which the laws of another knowledge can be deduced, not only laws for natural sciences, but for social sciences as well. In the weak sense, unification would only require to demonstrate the mere consistency of scientific laws. Obviously, when we mention these two ways of unification, we have in mind the famous article by Rudolf Carnap "Logical Foundations of the Unity of Science" (1938). This contribution is important in two aspects: the historical and the conceptual one.

From the point of view of the history of ideas, it can be assessed that Carnap's contribution settled the topic of unification among the usual discussions in Philosophy. However, we should better say *re-settled*, because the thesis of the unification of laws in a unique system, a harmonic and continuous system, had explicit formulations in the scholastic philosophy, particularly in Raymond Lull and later in Leibniz, just to mention two classical thinkers. In this regard, let us quote two brief texts, which are particularly significant. The first belongs to the philosopher of Coimbra, Francisco Suarez (1960), who describes the view of Aegidio, Miranda, and other scholastics, who defended the thesis of unification based on some unusual interpretations of Aristotle's texts (mainly *Metaphysics* I, 1 and VI 1, and *Posterior Analytics* I, 23). For these scholastic philosophers, there was only one science: Metaphysics. Within Metaphysics, we may distinguish Mathematics, Physics and Astronomy for teaching and learning reasons only.

> Qui consequenter affirmat caeteras scientiea non esse a metaphysica totaliter diversas, sed esse partes eius, seu potious omnnes esse partes unius scientiae; communi autem usu distingui et numerari ut plures propter commoditatem et usum earum in addiscendo, quia ita docentur et addiscuntur, ac si essent distinctae, idque propter rerum varietatem (*D.M.* I, S. 2)[1]

The quotation of texts in a contribution that is not addressed at enriching the history of philosophy might seem unsuitable. Nevertheless, the quotation is introduced for two reasons. First, it makes evident that the program for the unification of knowledge originated many centuries before the modern age and that has its roots in some interpretations of Aristotle. Second, the text is an opportunity to remind us that Aristotle rejected such unification of sciences based on the irreducible diversity of their objects, among other reasons. However, it must be noted the he affirmed their unity by postulating the same model of inquiry and causal explanation, but without ignoring the differences that emerge in different disciplines (Lennox 2001).

Let us now quote a text from Leibniz (1966), so as to show the clarity with which he announced the roots for unification of the program of science:

> Le corps entier des sciences peut être considéré comme l'océan, qui est continué partout, et sens interruption ou partage, bien que les hommes y conçoivent des parties, leur donnent des noms selon leur commodité (530–531).

Now, leaving historical questions aside and adopting the conceptual view, many interesting aspects of Carnap's contribution should be distinguished. So, the above-mentioned dual meaning of the expression "unification of science": strong or weak unification. As it is well known, Carnap was not biased towards the fact that unification in the strong sense can be achieved. At the end of his article he settled a question that motivates our article. He says briefly:

> This aim [the strong unification of science] cannot be shown to be unattainable. But we do not, of course, know whether it will ever be reached.

What should we say 70 years after? Is it possible to reach some kind of unification of natural and social laws? We mean, have we encountered some reasons for thinking on it positively? Or, are we with the same uncertainty that Carnap was?

13.2 The Emergence of Normic Laws

The 1960s witnessed a revolution in the philosophy of science, headed by T. Kuhn, P. Feyerabend, and I. Lakatos, among others – to this group the so-called "precursors of the revolution", such as G. Bachelard, N. Hanson, W. Dray and many more who paved the way should be added. Among the causes that converged in this movement against the standard views of science – by that time logical empiricism and Popperian falsationism, the unattainable job of matching typical explanations used in history and social sciences to the model created by Hempel, Carnap, and Nagel should be mentioned. This structure – in any of its versions- required *stricto sensu* laws as its most important elements, a demand that could not be satisfied, as many noticed. The problem was evident in the enunciation of the supposed laws because such were always accompanied by adverbial expressions such as "usually", "normally", "generally", etc. The obvious purpose was to leave room for exceptions. However, if scientific laws should be true, then they cannot admit exceptions – at least according to Hempel & Oppenheim's model of explanation. Certainly, those who did not adhere so strictly to this model could choose another alternative: to present the law together with the list of exceptions to it. But this is impractical because of the heterogeneity and potentially infinite amount.

In spite of the fact that Carnap and other philosophers of the *Wiener Kreiss* never risked a positive answer to the possibility of unification in the strong sense, they did suppose that social sciences – just arrived to the academic world at the beginnings of the twentieth century – would be able to state their own laws, at least the basic ones. Biology, Economics, Sociology, Psychology and their related specialties would finally arrive – they thought – to the laws that the standard model of explanation required. Of course, also positivist philosophers had noticed the problem for obtaining genuine scientific laws in these fields. For many, as Hempel himself, the escape to the probability was an option and, therefore, the resource to statistical laws (1966), especially chapter 5. However setting aside Popper's criticisms, it was evident that among statistic laws there were laws of very different nature.

(a) "Birds normally can fly"
(b) "Those who have electricity normally use artificial light"
(c) "Most Swedes are Protestant"

The three statements are highly probable, though the first one seems to assess something stronger than a mere coincidence of characteristics.

Gerhard Schurz's merit is to go back to the problem of the nature of biological and social laws. Through an extensive analysis of normic laws, Schurz has offered a set of interesting views and responses, many of which seem to us to be satisfactory (Schurz 2001, 2004, 2005). However, as we will see, his philosophy precludes a negative answer to the unity of science program (in the strong sense, i.e. the unification). Probably, one of the most significant characteristics of Schurz's analysis is that he embraces the logical, epistemological, and ontological sides of the problem. Before we continue, a controversial issue should be explained: when we talk about the problem of social laws as an issue that is up to now unsolved, we make this assumption from the classical point of view of science. Such framework differs from historical, postmodern or sociological treatments, and considers that logic and general statements are essential tools for scientific knowledge and its expressions.

Whereas the logical expression of a law is (x) (Ax→ Bx), normic laws should be represented by formulas such as (x) Ax => Bx, which should be read as "As are normally Bs". Here "Ax" and "Bx" denote open formulas in the individual variable "x" and "=>" is a variable-binding conditional. Normic laws are dominant in biological and social sciences and also in the humanities and in technology. In Schurz's analysis, it is of crucial importance to take into account that normic laws do not have exceptions *stricto sensu* because they are not falsifiable. They only have *loose exceptions* that just indicate an abnormal L-instance with regard to a determined prototype and, therefore, a prototypical normality. The following statements express known examples of normic laws.

(d) "The division of genetic material normally occurs according to Mendel's laws"
(e) "People actions are usually goal-oriented"
(f) "All other factors being equal, as the price of a good or service increases, consumer demand for the good or service will normally decrease and vice versa"

As it was said, normic laws shouldn't be identified with the so called "statistical laws", which is a crucial point. However, normic laws usually imply statistical normality by indicating certain normality in connection with a given prototype, but the opposite is not necessarily true. It means that, for example, Ax => Bx, implies that the conditional statistical law of Bx given Ax (p(Bx/Ax)) is high. We should emphasize once again that normic laws cannot be reduced to statistical normality, though they may imply it (this is what Schurz calls "the statistical consequence thesis"). The essential difference between statistical and normic laws rests on the fact that the latter do not derive from accidental generalizations but from the reference to an existing prototype. The expression "normic law" was coined by W. Scriven (1959) in connection to the debate held in the 1950s on the status of explanations in history and the attack of W. Dray (1957) to the positivist philosophy of science, in particular to Hempel's model of explanation (see also von Wright 1971, Chapter 1).

When the revolution in philosophy of science broke out, the tools to face the reasonable criticism to the dominant views were still being forged. Among these *in status nascendi* tools there were some logic systems suitable for the formalization of normic reasoning, systems that today are called "default logics" or "non-monotonic logics". Though these logics were mainly developed in the field of artificial intelligence, they have been used for solving philosophical problems, such as those related to the systems of beliefs and, now, to the scientific explanation of biological and social phenomena by normic laws. Among the systems developed in the last years, conditional P-entailment allows for the treatment of exceptions. The analysis of systems for normic reasoning has been extensively addressed by Schurz (2004, 2005) and it allows for a suitable handling of exceptions *vis a vis* general laws. The use of these default systems for understanding the logic and nature of normic laws was a very important step for philosophy – a remarkable merit of G. Schulz. It becomes evident, as soon as we take this into account, that by the time of the discussions on the nature of social sciences and historical explanations, the normic reasoning was set aside because philosophers of science – namely Hempel – considered that a formalization of normic reasoning was impossible. Even more, many thought that normic laws were pseudo laws because they were not falsifiable.

It is difficult to exaggerate the importance of having incorporated default systems for understanding the logics of normic laws because in this way it is possible to surmount – at least partially – the traditional criticism originated in hermeneutic streams. This was a condemnation of classical philosophy of science for setting aside the abnormal and singular cases in favour of universal laws. From a purely methodological view and taking into account the extensions of classical logics that we have nowadays, especially non-monotonic reasoning, we think that there is no place for divisions – e.g. into nomotetic *versus* ideographic sciences – as those raised by W. Dilthey, M. Weber, and other eminent thinkers of the nineteenth century.

13.3 Ontological Foundation of Normic Laws, Unity of Science and Neo-Darwinian Evolutionary Theory

According to Schurz, normic laws do not originate in our limited abilities to know the world. On the contrary, they represent features of reality since they are based on objective entities, a fact that constitutes both their ontological justification and our reliability on them. The entities on which normic laws rest are *open systems* – in the thermodynamic sense of the expression – and final products of an evolutionary Darwinian process. Unlike sociobiology and similar views, Schurz does not limit the evolutionary process to biological entities. Cultural, social and technological beings, which exist in our minds, are also the results of a Darwinian process, whose known scheme is: *variation – selection – reproduction.* All these are open systems, for example, a biological species contains self regulation mechanisms and its identity is given by normal prototypical states. Since they are open systems, they receive external influences – sometimes disturbing, sometimes beneficial – which are compensated by those mechanisms. Certainly, when disturbing pressures exceed compensatory mechanisms for adaptation, the result is extinction or death. Vanished species give testimony of this kind of phenomena, recurrent in the history of life.

Of course, if the evolutionary process goes beyond the biological world, then it is not enough to speak of genes and genotypes because they are DNA entities. Thus, Schurz introduces the notions of *repro* and *reprotypes* that, in the social and cultural world, should be assimilated to *memes* and *memotypes* respectively. Unlike genes and genotypes, they are transferred through information-transmission mechanisms that, in turn, flow in all directions and not only from parent to offspring. The key point in order to understand the relations between biological and cultural beings – open systems with identity and regulatory mechanisms – and normic laws is the following: the former have a natural plasticity that allows them to move away from the prototype up to a certain point. These are the exceptional cases covered by normic laws when using the adverb "normally". With the statement "Birds can normally fly", we are assessing – implicitly and among other things – that some birds could have lost their capacity to fly in order to survive and, nevertheless, keep other characteristics that are typical of birds.

Henceforth, we only speak of Schurz's philosophy with regard to the biological world, setting aside questions related to the nature of social and cultural entities and the processes that generate and change them. A Darwinian evolutionary process – as that assumed by Schurz – is something that requires the random confluence of two causal lines: on the one hand, biological entities and, on the other, a set of influences and circumstances – favourable or unfavourable. Again we say that this convergence is totally random and Schurz assumes its consequences. The outcome is that regularities found in biological entities – on which normic laws rest – are unpredictable from the basic laws governing matter, particularly physical laws. For the programme of the unity of science, in the strong sense, i.e. the unification of science, this represents a lethal stroke because it is not possible to predict regularities (such as Mendel's laws) from the most fundamental laws. Schurz accepts this negative result:

> Unlike laws of nature, normic laws are not *physically necessary*. Because of their dependence on the accidental circumstances of evolution, normic laws involve a considerable portion of *contingency*'. If evolution has taken place in another part of the universe, it has probably produced species which are rather different from those on earth.

Despite the many merits of Schurz contributions, his proposal has a very serious problem when examined from a scientific and methodological view. His philosophy rests on a highly controversial theory: the Darwinian Theory. Today, this theory is the target of many reasonable criticisms in the scientific community, mainly among molecular biologists and biophysical researchers. A century ago, in the preface to his *Logic Investigations*, Husserl described the state of Logic by the following Latin adagio: "Bellum omnium contra omnes", that is, "the war of all against all". We think that the same adagio would be adequate for describing the sate of evolutionary studies nowadays. Perhaps, this can surprise many unfamiliar with evolutionary studies. In addition, Husserl's adagio could be applied to those processes that Kuhn called "crisis periods". The crisis about the evolutionary theory began in the 1960s – at the very beginning in an imperceptible way – to culminate 30 years after with the arrival of four alternative theories, which today divide the scientific community of biologists.

According to Lovtrupt (1987) and many others, the expression "evolutionary theory" is dangerously ambiguous. On the one hand, it means *the hypotheses* on the mechanisms through which living species evolved, but the same expression may mean *the fact* that living species descend from others that were very different and much simpler. Whereas the contemporary scientific community agrees with *the fact evolution*, this is not the case with regard to *the hypotheses on mechanisms of evolutionary processes*. Even more, today we have four very different and opposite theories about these mechanisms. They are: (i) the neo-Darwinian Theory (Stebbins and Ayala 1981); (ii) the Self-organization Theory (Kaufmann 1995); (iii) the Symbiotic Theory (Margulis 1991; Saap 1994, 2003) and (iii) the Structuralist Theory (Webster and Goodwin 1996). When there are so many alternative theories, it is a symptom that things are dark and, therefore, there is a lack of satisfactory explanations about those mechanisms. In short, such a plurality of theories reminds us of Hamlet's words, *"something is rotten in the state of Denmark"*. Up to now, the theory that Schurz chooses for supporting his philosophical doctrine (the neo-Darwinian Theory) has not given satisfactory answers on how the gradual formation of genes and proteins that new species need occurs. Then, we should examine the neo-Darwinian Theory in more detail.

The mechanism proposed by most neo-Darwinian biologists can be put in the following way: random changes of genetic stuff that produce slight effects on the organisms. In turn, these slight organic effects are offered to the action of natural selection. Natural selection not only eliminates those characteristics that, in one way or another, do not favour the organisms where they occur, but also, and at the same time, preserves and accumulates those effects that confer some kind of advantage to the organism. Now we arrive to the crucial question: Do we have any strong evidence that this mechanism was responsible for the emergence of living species? Kuhn taught us that scientific theories should be studied from textbooks *ad usum,* as that by H. Curtis (1989). What arguments do we find in them as confirmation of the neo-Darwinian hypothesis on the mechanism of evolution? Usually, textbooks mention two kinds of phenomena in support of the neo-Darwinian view: the industrial melanism and the development of resistance to pesticides by insects and bacteria and other similar process.

However, the problem is that the two kinds of phenomena suppose complex living structures instead of creating them. For this reason, such phenomena are called "micro-evolutionary" processes as opposed to the macro evolutionary ones, which would be responsible for species formation. Neo-Darwinian scientists accept the distinction between micro and macro-evolution for practical reasons, but they state that macro-evolution is not but the action of micro-evolution over long periods of time (Stebbins and Ayala 1981).

Today, the scepticism surrounding neo-Darwinian theory, comes from its lack of explanation for the high complexity of living structures and also of prediction of evolutionary events. This problem had been already envisioned by Bishop William Paley many years before the appearance of Lamarck and Darwin's works. Paley asked how the human eye might have been formed through a gradual process since it is only useful when all of its parts are integrated. According to this, to say that the human eye had been formed by a step by step process is – following the Darwiniam philosophy – like saying that a highly complex machine is able to perform useful functions at each stage of its construction process.

Apparently, we are aiming old objections at the neo-Darwinian theory, objections that many believed to be already answered. But, it is not so. The knowledge of the genetic code, together with the arrival of mapping and sequencing technologies, has provided us with tools to set out traditional objections in a totally new way. Like Bishop Paley, we are also fully conscious that organs, such as the human eye, are of an amazing complexity. However, there is a substantial difference. Whereas the degree of complexity was evaluated in an intuitive way at Paley's time, now we can have a very exact and mathematical idea of biological complexity. For example, the protein lysyl oxidase – like other many proteins that appeared during the Cambrian explosion- required for supporting stout body structures, comprised 400 amino acids in a non repetitive sequence (Ohno 1996). Actual *experimenta and mathematical* knowledge tell us that the probability of arriving to a protein only one hundred amino acids long in a random process is about 1 in 10 to the 65 (10^{65}) (Reidhaar-Olson and Sauer 1990; Behe 1992). It means that it is absurd to think that proteins appeared out of luck!

The formation of highly complex structures is something that, at least in principle, might be simulated in a computer program, an experiment-like conceived by the Nobel prize M. Eigen (1976) at the Max Planck Institute (see also Küppers 1987, 1989, pp. 81–88) and repeated by Dawkins (1989, pp. 43–50). In fact, some thinkers have carried out such experiments trying to demonstrate how living species might have been originated through a Darwinian process of random selection. For that, a statement of the natural language is selected. This statement intends to represent a nucleotide ordered sequence, as those codifying for proteins. The statement is placed as the final target of the computer process and, then, the computer is instructed to produce letters randomly in order to reach the target sequence. However, these computer simulations, although they try to imitate natural evolutionary process, face three problems.

First, when the correct letter appears in the correct place, the computer sets it. But, as we all know well, nature does not act taking into account future states or designs. Second, the target sequences with which the computer is provided are very short, when one compares them to those 400 amino acids long proteins, such as lysyl oxidase and others. Third, certainly and as we showed before, it is possible to make a certain analogy between the random production of a statement in a computer and a genetic sequence in a Darwinian framework (similar analogies have been used by Elliot Sober and Daniel Dennett, among others). But this analogy reflects the full Darwinian process very poorly. For the analogy to be complete, we should add the following requirements: (i) the final statement must be consistent and articulated with a more general text, e.g. the chapter of a book. (ii) each transitory state conducting to the target statement should also be coherent and articulated with the general text. Only with these two additional requirements the analogy would be good enough for representing the evolutionary Darwinian scenery.

It should be clear that we have not expressed that the other three evolutionary theories do not have serious problems for explaining the origin of species neither we are defending the intelligent design doctrine (Meyer 2002, 2004). If we have concentrated on the serious problems of neo-Darwinism it is because it constituted the ontological support for Schurz's doctrine on normic laws. Following Hans Reichenbach and other thinkers, who advocated for a scientific philosophy, we state that philosophies cannot be built separate from science and, in this sense, we should only assume solid and confirmed theories – despite of the fact that some anomalies always exist, as Lakatos said. However, this is not the case when the support cannot explain the origin of high complexity, a conditio sine qua non of any evolutionary theory. However, if we distinguish between micro and macro evolutionary theories, then the former – a very well confirmed theory – would be enough for the justification of many normic laws in the field of life sciences. Naturally, the application of generalized micro evolutionary tenets for understanding higher – social and cultural- entities and their laws should be explored.

Note

1. '. . . who consequently affirm that the other sciences [Mathematics, Physics and Astronomy] are not distinct from Metaphysics, but parts of it; even more, all of them are part of a unique science. However, we usually distinguish and numerate them for practical reasons and in order to learn them. That is so because they are learned and taught as if they were different disciplines'

Acknowledgment The author would like to thank to the anonymous reviewer for his/her helpful and critical comments to the first version of this paper. This research was supported by *Ministerio de Ciencia e Innovacion de España*. Proyecto SEJ 2007-60023.

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Chapter 14
Naturalism and the Unity of Science

Jan Woleński

Science is a complex social phenomenon consisting of people acting in a given way (including research, teaching, participating in conferences and congresses, etc.), the results of such actions, various institutions organizing or supporting scientific activities (including universities, scientific societies, etc.), the material means given to scientists by governments and private sources, etc. This complex whole constitutes science in the institutional understanding (I use this label in a strictly sociological sense, not in that employed by Thomas Kuhn, which has an explicit methodological flavour). It is unified by its role in society and contrasted with other factors of the social structure, like industry, agriculture, art, etc.; this contrast is by no means sharp, because science is related to other regions of social structure in many interrelated ways. Simply speaking, science in its institutional sense constitutes an element of the social division of labour.

On the other hand, science can be also considered from a methodological point of view, that is, as a body of theories, theorems, hypotheses, predictions, explanations, singular descriptions, diagnoses, etc. This body is very complex not only for being composed of elements having a different status (for example, a theory is somehow different from a singular description), but also because we have a considerable variety of disciplines. The diversification of science has a long tradition, going back to the Middle Ages or even earlier. The division into the *trivium* (logic, grammar and rhetoric) and the *quadrivium* (arithmetic, algebra, geometry and music; the last considered as astronomy) anticipated the present discrimination of the stock of science into the humanities and natural sciences (including mathematics).[1] Even this simple bifurcation of science invokes the question of whether both distinguished fields are called science substantially from a methodological point of view or grouped in to one category due to tradition only.[2] Otherwise speaking, is science a generic term or a mass term? The issue appears as more dramatic when we take into account the fact that natural science as well as the humanities also consist of many different fields. Natural science comprises disciplines having inanimate nature (physics,

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J. Symons et al. (eds.), *Otto Neurath and the Unity of Science*, Logic, Epistemology, and the Unity of Science 18, DOI 10.1007/978-94-007-0143-4_14,

chemistry) and life (biology). The situation in the humanities became complicated after the establishment of theoretical social sciences, like sociology, ethnology or cultural anthropology.

If one divides natural science into physics (plus chemistry) and biology, because the latter concerns life, but the former inanimate nature, he or she takes the domain of a given field as a criterion.[3] On the other hand, if one says that mathematics is deductive, but physics is inductive (whatever this means), one proposes a criterion based on a particular method. Similarly, if someone considers sociology theoretical, but maintains that history is descriptive, such a contention is also based on a methodological qualification. This diversity (or even a tension) of approaches between various criteria of looking at science can be illustrated by well-known attempts of classifying science in the nineteenth century. Take two examples. August Comte divided science into six general disciplines: mathematics, astronomy, physics, chemistry, biology and sociology. His ordering was based on two rules: each previous science is more general than its successor, any subsequent science logically assumes all ancestor sciences. Moreover, general sciences are associated with several particular fields, for example, biology with botany. Clearly, Comte wanted to combine generality with the logical relation "s based on" but he also intended to accommodate both to the division according to domains. Wilhelm Windelband and Heinrich Rickert, the leading representatives of the Badenian School of Neo-Kantanism, employing the domain criterion, distinguished *Naturwissenschaften* (sciences about nature) and *Kulturwissenschaften* (sciences about culture), but also added a methodological feature: the latter are idiographic (descriptive), but the former nomological (formulating general laws).[4]

The previous remarks show that attempts to classify the stock of sciences were based on two main ideas. Firstly, the proposals in this respect intended to do justice to the real diversity of scientific fields, but, secondly and contrary to the former task, they stressed that this variety is unified by some underlying principles. This latter aim was additionally argued by pointing out that every particular science fulfils, more or less strictly, some universal methodological standards, which determines that typical scientific activities and their results are fairly specific and differ from other regions of culture, like politics, art, literature, music or religion. Due to these standards, fields not satisfying them, like astrology or alchemy, are cancelled from the stock of science, although they could be temporarily treated as very honorable with respect to their scientific character and very important from the practical point of view.[5]

The view that science satisfies some universal methodological principles was always a deep reason for proposing various conceptions of the unity of science. Historically speaking, the importance of such ideas varies from time to time. In general, all philosophers belonging to positivism consider the unity of science as something of the utmost importance. Of course, the program of logical empiricism is the most famous attempt in the history of this issue. It can be termed as global, because it concerned all sciences to be unified by physicalism. On the other hand, we can point out many local projects, for example, logicism and structuralism in mathematics, the grand unification or the ultimate theory in physics, behaviorism in psychology, functionalism in sociology or historism in the humanities. Typically, globalism in the unity of science leads to radical proposals, but localism results in more moderate solutions. To be just, let me note that contemporary philosophy of science does not consider the unity of science as a fundamental question, contrary to such issues as scientific justification and explanation, the relation between facts and theories, the nature of scientific observation or progress in science (see Gillies 1993 for a report about the main themes in the philosophy of science in the century; note, however, that all such reviews are subjectively colored). On the other hand, our question comes back from time to time, although more frequently as local rather than global.

We can list the following foundations offered for the foundations of science (see Cusey 1977; Morrison 2000; Oppenheim and Putnam 1958):

(a) the method of science;
(b) the language of science;
(c) the structure of science;
(d) the character of the world;
(e) reduction to a chosen field;
(f) the unity of explaining phenomena;
(g) acceptance by experts.

Here are more concrete examples related to (a)–(g):

Ad (a) the empirical method, testing hypotheses, standards of rationality, etc.
Ad (b) physicalism, behaviourism (in psychology);
Ad (c) axiomatic hypothetism (some initial hypotheses are taken as axioms and the remaining of scientific assertions are deduced from it);
Ad (d) materialism, nominalism. Platonism (in mathematics);
Ad (e) reduction to physics, reduction to psychology (the humanities), syntheses of various disciplines;
Ad (f) the priority of nomological-deductive explanation;
Ad (g) various versions of sociologism in the philosophy of science.

Although instantiations presented by Ad(a)–Ad(g) are more specific than (a)–(g), they still employ very general philosophical concepts and positions. Hence, it is quite common to look for examples taken from the practice of particular disciplines.

Unfortunately, messages coming from real research are not univocal. On the one side, general philosophical proposals concerning the unification of science do not arouse a great enthusiasm, particularly in the natural sciences, but, on the second side, local reductive enterprises or syntheses are always welcomed. Some of them became successful, for example, the reduction of thermodynamics to mechanics or the periodical table of chemical elements to quantum mechanics, but other are regarded as only promising and still unfinished, for example, the synthetic theory of biological evolution (combining classical evolution theory and genetics) or quantum field theory (the synthesis of relativity theory and quantum mechanics). Similarly, although many physicists say that the grand unification is a dream, it is difficult to expect that serious efforts in this direction will be entirely abandoned. Perhaps still one point should be mentioned. It concerns progress in various branches of science. Typically, most good and successful examples of scientific unification come from mathematics, physics and chemistry (if reduced to physics). Cases from biology are more questionable. What about the humanities and social sciences? Although there are partial successes (for example, microeconomics as explanatory with respect to macroeconomics), the common feeling is that unifications outside natural science and mathematics are a delicate matter. At this place, I point out only one issue. Since most versions of the unity of science program are modelled on advanced natural sciences, this suggests that other sciences are still underdeveloped, for example, their mathematization has not achieved a sufficient degree of advancement. In fact, the idea of the uneven progress of science is a by-product of the unity of science program. It explains why some representatives of the humanities and social sciences are sceptical about the unity of science, but other too quickly employ projects based rather on philosophical premises than empirical evidence (for example, par force embedding social sciences into the physicalistic jargon).

Doubtless, an exhaustive discussion of the unity of science would have to touch almost of all problems of past and present philosophy of science. I limit my task here only to one aspect of the discussed question, namely related to naturalism. More specifically, I will discuss the humanities in the light of naturalism and the view that the naturalistic position makes it impossible to treat values and norms properly.[6] This point appears as critical, because a firm methodological orthodoxy maintains that research in the humanities cannot proceed without appealing to duties, obligations and evaluations; in order to have a convenient term, I will refer to the normative feature of phenomena investigated in *Kulturwissenschaten*.[7] In particular, the famous operation of *Verstehen*, that is, the specific way of understanding cultural phenomena, must rely on the normative feature as basically primitive and thereby non-reducible to facts. Since naturalists usually propose such a reduction, the humanities open a serious challenge for naturalism. I will argue that naturalism can meet the related difficulties without proposing a reduction of the normative feature to (natural) facts.

What is naturalism? This view was developed by David Hume and can be summarized by the following claims (see Luper 1998):

(1) only natural things and their complexes exist;
(2) only natural epistemic capacities are admitted in science;
(3) we should trust natural epistemic capacities.

Since the points (1)–(3) admit various interpretations, we can distinguish radical and moderate naturalism or global and local variations of this view. For example, physicalistic naturalism offers a very strong reductive kind of naturalism on the level of ontology and epistemology; this kind of naturalism is radical and global. Moderate versions of naturalism adopt a more complex idea of nature, for example, they select a basic level (usually physical bodies) and consider other natural phenomena as supervening on items adopted as primary. Otherwise speaking, the basic furniture of reality is determined by bodies on which other items supervene.

Additional light is shed by answering what naturalism rejects. Possible answers determine various naturalistic outputs. By definition, naturalism is of course at odds with all supranatural elements. However, one should be careful, because we can even find naturalistic theologies (the Stoics, Spinoza, Teillard de Chardin to some extent). If we omit naturalism in theology, we can say that naturalists do not accept universals, autonomous psychical objects, etc; this operates on the ontological level. As far as the matter concerns human epistemic capacities, naturalism opts against intuition (in the sense of Plato or Husserl), contemplation or mysticism. Naturalists do not deny that such acts exist or can be well imagined, but, on the naturalistic outlook they do not produce knowledge. Naturalism abandons teleological explanations, if they are based on final causes in Aristotle's sense, and propose to replace them by functional ones. Contemporary naturalism strongly appeals to biology, processes of adaptation and selection; evolutionary epistemology is a proposal of naturalism in the theory of cognition and knowledge. The last point can be illustrated by the question of how to interpret the certainty of deduction in a naturalistic way. Since cognition consists in acquiring and transforming information, deduction appears as securing possessed information against its dispersion. Its biological explanation points out that it arose in a evolutionary manner as a (a) the certainty of deduction as a device securing possessed information against its dispersion. Explanation is this: logic arose evolutionary as a natural phenomenon and it is a phylogenetic property of the human kind, which manifests itself in concrete cases of deductive arguments. Logic and information supervenes on living organisms.

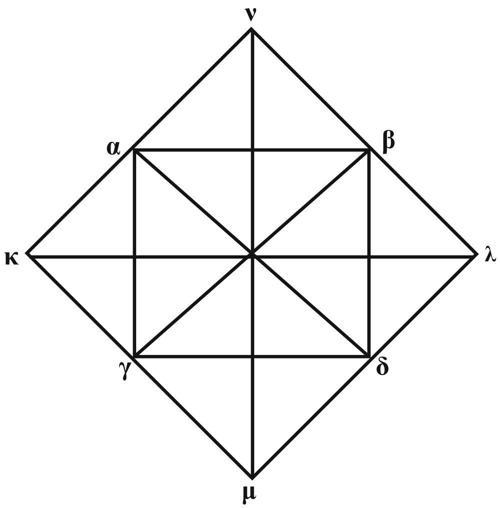
Doubtless, naturalism immediately leads to the issue of the unity of science. Even kinds of naturalism (global, local, radical, moderate) remind us of versions of the unification proposals. The same concerns difficulties in both cases. For example, naturalism has problems with mathematics. They consist in the question of how mathematics as dealing with the abstract world can be interpreted as concerned with the natural world. Even if this difficulty is to be solved by regarding mathematics as a special language applicable to the world, biology and psychology provide a new challenge. Assume that we agree that life and consciousness are natural phenomena. Yet we still have to answer what is the difference between them and proto-natural items (i.e., objects investigated by physics and chemistry). Perhaps a promising solution is suggested by the partial successes of biophysics or biochemistry.[8] As it is very commonly argued, naturalism has no resources for accommodating norms, values and the operation of *Verstehen*. Clearly, it is the same argument which is very frequently advanced against the unity of science projects as comprising the humanities together with the natural sciences. G. E. Moore offered the most popular bullet against naturalism, namely the naturalistic fallacy (see Moore 1903). According to Moore, the fallacy consists in defining value-predicates ("good", etc.) by purely sensory qualities, which refer to sensory properties of things. Moore's solution was that value-predicates denote simple, intuitively apprehended qualities of objects.

My claim is that approaches to the unity of science via a common language for all scientific disciplines are too weak. The same concerns the postulate that all possible sciences employ the same method. Otherwise speaking, linguistic and methodological versions of the unity of science program are inadequate, too limited and do not do justice to the peculiarities of particular fields. I think that progress in the attempts to achieve the unity of science can be expected from the side of naturalism. The main reason is that since this view does not rigidly determine linguistic and methodological issues for the unity of science, it is more flexible than traditional accounts. Generally speaking, the unity of science consists in the natural character of all phenomena that are the objects of scientific research. Of course, this view requires a wider understanding of nature than is assumed in physicalist reductive naturalism. Hence, the unity of science requires a moderate and pluralistic naturalism.[9] However, there is no a priori reason for denying that life, mind, information, norms, values or culture are non-natural, even if not entirely reducible to physical objects. On the other hand, I guess that every naturalistic approach to the unity of science covering the humanities must respect the problem of the naturalistic fallacy. Otherwise speaking, I assume that Moore was perfectly right when he famously argued that reductions of value-predicates to natural ones in his understanding (that is, represented by adjectives referring to sensory qualities) is fallacious and should be abandoned by any sound theory of the humanities and any other considerations in which the normative feature is essentially involved. On the other hand, as I will try to demonstrate, there is nothing in Moore's view and argument about the naturalistic fallacy that makes moderate naturalism impossible.[10]

I will illustrate the issue by the is/ought problem. The logical non-derivability of ought-statements from is-statements (Hume's principle, the Hume guillotine, **HP**) is perhaps considered as a strong case against naturalism, because it proves that norms and, by analogy, values are not reducible to facts. Reversely, if anyone says that ought-statements are formally derivable from is-statements falls into the naturalistic fallacy. I will argue that **HP** is entirely coherent with naturalism. It is sometimes said that **HP** concerns the logical relations between norms and declarative sentences. Although this interpretation is possible, it is historically not faithful to Hume's *Treatise on Human Nature*, where **HP** is introduced (see Woleński 2006; I follow here this paper) as concerning the relation between is-statements and ought-statements, both interpreted as declaratives. Moreover, passing to **HP** as related to norms involves a difficult discussion about the question of whether norms are true or false. I will speak about normative statements of the type "It is obligatory (prohibited, permitted, etc.) that *A*", where *A* is a non-normative declarative statement. Now the problem has the following form:

(∗)is $\mathbf{N}A$ derivable from A, when $\mathbf{N}$ refers to a normative (deontic) modality, that is O (obligatory), P (permitted), F ($O\neg$, prohibited) and I ($P \wedge P\neg$, indifferent).

Logical relations between deontic sentences plus A and $\neg A$ are displayed by the diagram (**D**) (a generalization of the traditional logical square):



Interpretation of particular points runs as follows: α – *OA*, β – *O*¬*A* (= *FA*), γ – *PA*, δ – *P*¬*A*, κ – *A*, λ – ¬*A*, ν – α ∨ β, μ – κ ∧ λ (= *IA*). Applying the well-known rules of the logical square, we obtain, for example, the theorems: *OA* ⇒ *PA*, *FA* ⇒ *P*¬*A*, ¬(*OA* ∧ *FA*), *PA* ∨ *P*¬*A*, ¬(*OA* ⇔ *P*¬*A*) and ¬(*FA* ⇔ *PA*). These formulas are analogous to formulas valid for categorical sentences or alethic modalities; all listed dependencies are validities of standard deontic logic. More importantly, we obtain the generalized Hume principle:

(**GHP**) (a) ¬ ⊢ (*A* ⇒ *OA*) (simple **HP** for obligation);
(b) ¬ ⊢ (*OA* ⇒ *A*) (converse **HP** for obligation);
(c) ¬ ⊢ (¬*A* ⇒ *FA*) (simple **HP** for prohibition);
(d) ¬ ⊢ (*FA* ⇒ ¬*A*) (converse **HP** for prohibition);
(e) ¬ ⊢ (*A* ⇒ *PA*) (simple **HP** for permission; also for *P*¬*A* and ¬*A*);
(f) ¬ ⊢ (*PA* ⇒ *A*) (converse **HP** for permission; also for *P*¬ and ¬*A*);
(g) ¬ ⊢ (*A* ⇒ *IA*) (simple **HP** for indifference);
(h) ¬ ⊢ (*IA* ⇒ *A*) (converse **HP** for indifference).

In more general terms (**GHP**) says that for any normative modality **N** (where is one of *O*, *F*, *P*, *I*), neither ⊢ (*A* ⇒ **N***A*) nor ⊢ (**N***A* ⇒ *A*).[11] Informally speaking, if something obtains, this does not imply that it is normative (the simple version), and it if something is normative, this does not imply that it obtains (the converse version). If we introduce the operator **V** with its intended meaning "it is valued that", we can formulate a counterpart of (**GHP**) for values by: neither ⊢ (*A* ⇒ **V***A*) nor ⊢ (*A* ⇒ **V***A*). Informally, obtaining does not entail that what obtains has a value, and if something has a value, this does not entails that it has a value.

Since justification for (**GHP**) and its counterpart for **V** comes entirely from logic, it is consistent with naturalism as well as with anti-naturalism. We can easily embed

(GTP) into the former by accepting that the normative feature of phenomena as well as their value status are secondary with respect to acts issuing of norms, that is, normative decisions.[12] Now there is no reason to deny that normative decisions and evaluations are natural phenomena. We are also not forced to emotivism or subjectivism in ethics. Everything that is required by naturalism consists in claiming that human beings have natural resources for undertaking normative decisions and evaluations. Of course, a further analysis must be done, for example, by appealing to the theory of moral sentiments (Hume once again) or acts of approving some states of affairs and disapproving others. A proper way seems similar to what is the case in logic: basic generators of normative decisions and evaluations are phylogenetically innate, but they are filtered by experience acquired in ontogenesis. The most important consequence of this approach for the unity of science program points out that statements formulated by the humanities about normative features of phenomena are true or false in the normal sense, provided that we relate them to normative decisions. Consequently, the humanities concern the same world being investigated by the natural sciences. This is the natural world.

Finally, let me observe that **HP** can be generalized in many other directions, not only with respect to evaluations. In fact, diagram (**D**) displays dependencies holding for epistemic modalities, questions, wishes, hopes, wills, etc. Thus, the naturalistic fallacy and its overcoming concerns a wide variety of concepts. The cases contrary to **HP** are actually very exceptional and concern logical and perhaps ontological modalities, if we accept the latter. The formula $A \Rightarrow \mathbf{M}A$ holds logically, if **M** expresses alethic possibility (if something obtains, it is possible), but $\mathbf{M}A \Rightarrow A$ is valid, if **M** signifies necessity (if something is necessary, it obtains). However, the naturalist has no reason to protest against logical modalities, although ontological necessities and possibilities can be suspicious for him or her. Generally speaking, **HP** in all its versions says only that statements of one kind do not follow from statements of a different kind, but not that deontic, epistemic, etc. statements are not about natural phenomena. If we limit factual statements to non-modal statements, then all modal statements (perhaps except alethic ones) become non-factual; on the other hand, nothing prevents us from admitting various kinds of factuality, covering such mental attitudes as thinking, wishing, deciding, claiming, valuing, willing, questioning, hoping, arguing, ordering, etc. Thus, **HP** appears as the very foundation for the naturalistic project of the unity of science.

Notes

1. One terminological matter is important here. I use the term "science" as a counterpart of the German word *Wissenschaft*. This fact determines that I speak about the division into humanities and natural science, not into the humanities and science.
2. In order to simplify the problem, I omit any discussions about so-called practical science, like medicine, jurisprudence, technology, etc. Remember that medieval universities had four faculties: theology, law, medicine and liberal arts and this tradition became stable through history with respect to all, except liberal arts. The last was gradually replaced by the philosophical faculty which included all the special fields that evolved from the trivium and quadrivium, that is, physics, chemistry, biology, philology, linguistics, philosophy, etc. This process was

further strengthened by the structure of scientific societies and academies, which consciously abandoned the structure of medieval universities.

3. I report typical discussions in the nineteenth century.
4. I neglect various further subtleties of this view, in particular, the differences between Windelband and Rickert.
5. There are exceptions to this opinion. Paul Feyerabend is perhaps the most famous advocate of the view that all attempts to distinguish science from non-science by methodological standards of rationality are fundamentally mistaken. This is very well displayed by his slogan "Everything goes [in science]". However, I would like to note that a certain vagueness at the borderline between science and non-science is coherent with the view that both are different. The case can be illustrated by theology. It started in the Christian world as *scientia divina*, which was considered as more scientific than any other field. Now, although nobody denies that some parts of theology, like Biblical theology or comparative theology, are normal humanities, the status of theology as the science about the supranatural world is a controversial matter. Even if theology remains at universities and academic degrees in this discipline are admitted, there is a general agreement that it has a special status. The case of theology is good example of the that science in the institutional sense and science in methodological understanding are different to some extent.
6. This question also applies to the social sciences, at least to some extent.
7. This does not means that I propo a reduction of values to norms.
8. I cannot enter into a deeper analysis of this problem. Let me limit myself to the following declaration: it seems that no general answer can be given and every question must be carefully discussed step by step.
9. The position defended in this paper is consistent with the view that naturalism and the unity of science have only local character. Simply speaking, every success in this respect should be welcomed, even if the complete realization will always be an open issue.
10. In fact, Moore also rejected supranaturalism (transcendentalism in his terminology) consisting in the reduction of values to extranatural properties or facts. This is a hint that Moore understood naturalism as radical.
11. For simplicity, I assume that A is not tautological. In fact, $\vdash PA$, if A is a tautology, and $\vdash OA$, if something is obligatory.
12. This is a simplification. In fact, normative decisions consist in issuing obligations or prohibitions. Permission and indifference express secondary normative features. For examples, A is permitted, if it is not prohibited, A is indifferent, if A is neither obligatory nor permitted. These facts, although relevant for a conceptual analysis of normative systems and normative features, can be neglected here.

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Chapter 15
Searching for the Unity of Science: From Classical Logic to Abductive Logical Systems

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15.1 Introduction

From an informational point of view, an inference or argumentation can be considered as a finite sequence of sentences of a language, not arbitrarily ordered, for which one may distinguish an initial group of sentences called *premises*, followed by another sentence called *conclusion*. The set of premises (or set of reasons) may be empty, but the conclusion has to be present. An argument (of a reasoning) is the pair (Premises, conclusion). According to this perspective (Corcoran 1999) a proposition can be expressed by means of a sentence of a language and, inspite of discussions about the *status* of propositions, the most important is that a proposition has information relative to a certain domain of research, though to simplify we can say that a sentence (which means a proposition) has informative content (Nepomuceno 2007). So a deduction is a valid inference, provided the information in premises contains the information of the conclusion. However, in scientific research deduction is not the only form of inference. Peirce (1931, p. 5) considers three different types of inference in scientific research, namely deduction, induction and abduction.

In inductive inference, the passage from premises to conclusion assumes the information of a new sentence that contains the information of the premises. In fact, in this case, though an induction is taken as correct, its conclusion does not necessarily follows from the premises. In an inductive generalization, for example, from "S_1 is P", "S_2 is P", ..., "S_k is P" to induce "all S are P", uncertainty still remains, except when 1,2,..., k represent all possible cases. Abduction, first called "formulation of hypothesis" by Peirce, has been logically formulated as follows (Peirce 1931, p. 5, 189. Aliseda 2006, p. 36),

The surprising fact, C, is observed,
But if A were true, C would be a matter of course,
Hence, there is a reason to suspect that A is true.

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Lisbon Colloquium for the Philosophy of Science October 25–28th

J. Symons et al. (eds.), *Otto Neurath and the Unity of Science*, Logic, Epistemology, and the Unity of Science 18, DOI 10.1007/978-94-007-0143-4_15,

This formulation may be represented by the following logical argument-schema:

$$\frac{C, A \rightarrow C}{A}$$

in which A is not a logical consequence from C and $A \rightarrow C$ but a provisional hypothesis. The premises are two sentences: the first one is the conclusion of a deductive inference and the second one is also a premise of such inference. This is why sometimes abduction has been seen as a process of reconstructing an "incomplete" deductive reasoning. The information of C is not included in the information of $A \rightarrow C$ but A adds the necessary information, so the information of C is included in the information of $A \rightarrow C$ and the information of A, taken together.

From a logical point of view, according to Hempel's scientific explanation (Hempel 1965), given a scientific theory T and a sentence y that expresses a fact, to give a logical explanation of y is to obtain a new sentence x such that together with T, imply y. That is to say, information of T plus information of x contains the information of y. In fact abduction has been considered the scientific inference par excellence and it can be used in various disciplines, such as linguistics, mathematics, artificial intelligence, social sciences, etc.

Deduction, on the other hand, has been studied by classical logic and its extensions and some important calculi have been defined to give account of it. In fact, even it was the underlying logic considered in the conception of the unity of science by logical positivists. However, the classical logic itself cannot represent processes of discovery, where abduction, like in Artificial Intelligence – Konolige (1996) and Lobo and Uzcategui (1997) – plays an important part, so that a revision of such conception requires new logical perspectives, though logicians have hardly paid attention to abduction, until rather recent times.

To study abduction from a logical point of view, in order to obtain abductive logical systems, when possible, the strength of classical logic may be a good start point, of course. In fact there are several logical models of abduction, among which Aliseda (2006), Hintikaa (1998), Kakas et al. (1998) and Thagard (1998) are mentioned. However, some questions remain without a clear and definitive answer, being one of them to analyze a possibility condition. In this paper we study how exploiting known logical resources to define abductive solution, for which abductive problems are relativized to (deductive) consequence relations whose structural and operational analysis are presented. Summary, to use more formal deductive tools for abduction, that is to say, to determine those conditions needed for defining any abductive calculus. All of that may give new perspective to the idea of unity of science.

15.2 A Classical Point of View

Let L be a first order formal language. $x \in L$ represents that x is a sentence of such language. If A is a set of sentences of L we shall write $A \subset L$. The semantics of L is given in terms of model theory, that is to say, every member of the class of

L-models (or abstract structures of the same type than L or L-structures) is given by a (non-empty) universe of discourse, and an interpretation function, defined from linguistic symbols to the universe or predicates or relations defined in such universe. L has a special sentence, namely $\perp$, whose truth value is "false" in all L-models. An *entailment relation* – or *a logical consequence relation* – is defined as $R \subset \wp(L) \times L$, in accordance with well known requisites that L-models should accomplish. Then the logical form of any scientific argument, as long as it is formally tractable, can be expressed as a pair (set of sentence of L, sentence of L) that either belongs to such relation, when the argument is valid, in the sense of requisites of R, or does not belong to R, if it is not valid. So given $A \subset L$ and $x \in L$, $(A, x) \in R$ or $(A, x) \notin R$. Let $\vDash$ be the classical consequence relation, $\vDash \subset (\wp(L) \times L)$, then for a set $A \subset L$ and $x \in L$, $A \vDash x$ if and only if all models of sentences of A are models of x. In general, a relation $\vDash_*$ that verifies the following structural rules – substructural logics do not, as it can be seen in Schroeder-Heister and Dosen (1999) –, for $A, B \subset L$ and $x, y \in L$, is a closure relation, which will be called *supraclassical*, if it verfies the following:

1. Reflexivity: for every $a \in A$,

$$\overline{A \vDash_* a}$$

2. Monotonicity:

$$\frac{A \vDash_* x}{A, B \vDash_* x},$$

3. Transitivity:

$$\frac{A \vDash_* x, x \vDash_* y}{A \vDash_* y},$$

The classical consequence relation (or classical entailment relation) is one that is supraclassical, compact and closed under uniform substitution. In general, a relation $\vDash_*$ is compact, or verifies compactness, iff for any $A \subset L$ and $x \in L$, if $A \vDash_* x$ then there is a finite subset $A' \subset A$ such that $A' \vDash_* x$. The uniform substitution, which is represented by *subs*, is defined as a function from the class of sentences to itself, with certain restrictions. A relation $\vDash_*$ is closed under uniform substitution iff for all $A \subset L$ and $x \in L$, if $A \vDash_* x$, then $subs\ (A) \vDash subs\ (x)$, where $subs\ (A) = \{subs(y) \mid y \in A\}$.

Since L is an ordinary first order language, the set of logical symbols is included in its vocabulary. Such set may be the following:

$$\{\neg, \wedge, \vee, \rightarrow, \exists, \forall\},$$

that is to say, L has negation, conjuction, disjunction, (material) implication, existential quantifier and universal quantifier. Classical consequence relation $\vDash$ verifies, for $A \subset L$, $x, y \in L$ and a variable κ, the following operative rules:

1. Negation:
   (a) $A \vDash x$ iff $A \vDash \neg\neg x$
   (b) If $A, x \vDash y$ and $A, x \vDash \neg y$, then $A \vDash \neg x$, or ($A, x \vDash y$ and $A, x \vDash \neg y$) iff $A, x \vDash \bot$
2. Disjunction:
   (a) If $A, y \vDash x$ and $A, z \vDash x$, then $A, y \vee z \vDash x$
   (b) $A \vDash x$, then $A \vDash x \vee y$ –or $A \vDash y \vee x$–
3. Conjunction:
   (a) If $A \vDash x \wedge y$, then $A \vDash x$ –or $A \vDash y$–
   (b) If $A \vDash x$ and $A \vDash y$, then $A \vDash x \wedge y$
4. Implication:
   (a) If $A, x \vDash y$, then $A \vDash x \rightarrow y$
   (b) If $A \vDash x \rightarrow y$, then $A, x \vDash y$
5. Existential quantifier
   (a) If $A \vDash \exists\kappa x$, then $A \vDash x(\kappa/t)$ for some t, where $x(\kappa/t)$ is the sentence obtained by substitution of κ for t in x, t is a constant of L
   (b) If $A \vDash x(\kappa/t)$, then $A \vDash \exists\kappa x$
6. Universal quantifier:
   (a) If $A \vDash \forall\kappa x$, then $A \vDash x(\kappa/t)$ for each constant t of L
   (b) If $A \vDash x(\kappa/t)$ for every constant t – another way of restriction, t is not free in previous formulae from which $x(\kappa/t)$ were obtained – then $A \vDash \forall\kappa x$

15.3 New Consequence Relations

Any scientific theory has an underlying logic but it does NOT have to be classical logic. However the classical relation of entailment $\vDash$ may have certain computational advantages. For example, propositional logic and some fragments of first order logic are decidable. So, why not take the formal advantages of classical logic, as a starting point, and define new consequence relations that preserve at least a part of such advantages and attempt to capture other forms of reasoning? A case in point is the work of Makinson (2005), which departs from classical logic to study non monotonic logic by defining "bridges" between them. Our aim is somewhat different, but these methods seem to be suitable for approaching the problem of constructing abductive logics.

In order to define new consequence relations, we should have into account not only the mentioned operative rules, which point out the behavior of logical symbols, but also the structural rules, since together they offer some attractive characteristics from a logical (and computational) point of view. Let us see how new supraclassical consequence relations can be obtained. Given sets of sentences A, $T \subset L$ and a sentence $x \in L$, we define *logical consequence modulo T*: x is logical consequence modulo T from set of sentences A iff for each model of T, if it satisfies A, then it satisfies x. In symbols,

$$A \vDash_T x \text{ iff } A, T \vDash x$$

This should not be reduced merely to classical consequence, since the set of sentences T is a part of the relation itself, that is to say, such set is very essential in the consideration of this kind of relation. According to definition, easily it can be checked that $\vDash_T$ verifies that for every $a \in A$,

$$\frac{}{A \vDash_T a}, \frac{A \vDash_T x}{A, B \vDash_T x} \text{ and } \frac{A \vDash_T x, x \vDash_T y}{A \vDash_T y},$$

that is to say $\vDash_T$ is a supraclassical relation but it is not closed under uniform substitution, since it may be verified that

$$A \vDash_T x \text{ but } subs(A) \nvDash_T subs(x),$$

as it can be seen in the following example (by using standard sentences of a first order language): let T be the unitary set $\{Qa\}$ and the set A=$\{Pa\}$, then $Pa \vDash_{\{Qa\}} Pa \wedge Qa$ but if we replace the constant a by b, then $Pb \nvDash_{\{Qa\}} Pb \wedge Qb$. In this case $\{Qa\}$ is not affected by substitution operation, in fact the relation is $\vDash_{\{Qa\}}$ and, in accordance with its nature, $\vDash$ and $\{Qa\}$ are inseparable.

Given a set of sentences $A \subset L$ and the sentence $x \in L$, we define *logical consequence modulo a natural number* $n \geq 1$: x is logical consequence modulo n from the set A iff for each L-model M, whose universe of discourse has cardinality lesser than (or equals to) n – $|M| \leq n$–, if it satisfies A, then it satisfies x. In symbols:

$$A \vDash_n x \text{ iff, for each } L\text{-model } M, \text{if } M \vDash A \text{ and } |M| \leq n, \text{then } M \vDash x$$

As it can be seen, this new relation verifies reflexivity, monotonicity and transitivity, so it is also supraclassical, but it is not compact: if A is a set that contains formulae representing "a first individual has the property P", a_1, "a second one has the same property", a_2, and so on indefinitely, that is to say $|A| = \omega$ – where ω represents the cardinal of the class $\mathcal{N}$ of natural numbers – for the sentences x, which formalize "all elements of $\mathcal{N} \cup \{\omega\}$ have such property" – all natural numbers plus ω – and a natural number n, $\omega > n \neq 0$, $A \vDash_n x$, by vacuity since none (finite) model satisfies A, but, whatever the finite subset A' maybe, $A' \subset A$, it is verified that $A' \nvDash_n x$. On the other hand, this is a descendent relation, but not ascendent: if $m < n$ and A

$\vDash_n x$, then $A \vDash_m x$, but if $n < r$ and $A \vDash_n x$, it may be that $A \nvDash_r x$, example: $\exists\kappa P\kappa \vDash_1 \forall\kappa P\kappa$, but $\exists\kappa P\kappa \nvDash_r \forall\kappa P\kappa$ for every $r \geq 2$.

We consider a final consequence relation. Given a set of sentences $A \subset L$ and a sentence $x \in L$, we define *logical consequence modulo a class of models* $\mathcal{M}$*: x i*s logical consequence modulo $\mathcal{M}$ of the set of sentences A iff for each model that belong to M, if it satisfies A, then it satisfies x. In symbols:

$$A \vDash_{\mathcal{M}} x \text{ iff for each } M \in \mathcal{M}, \text{ if } M \vDash A, \text{ then } M \vDash x$$

Though we omit a concrete verification to abbreviate, this relation is also supraclassical, but it is not closed under substitution. Of course, if there is a class of models $\mathcal{M}$ such that $A \vDash_{\mathcal{M}} x$, it may be that there is $M \in \mathcal{M}$ such that $M \vDash subs(A)$ but M $\nvDash subs(x)$, because of which $subs(A) \vDash_{\mathcal{M}} subs(x)$. Let us see an example: let Pa, Pb, Pc be formulae such that for all $M \in \mathcal{M}$, $M \vDash Pa$, $M \vDash Pb$ and $M \nvDash Pc$, then $Pa \vee Pb \vDash_{\mathcal{M}} Pa \wedge Pb$, however $Pa \vee Pc \nvDash_{\mathcal{M}} Pa \wedge Pc$.

As in the first case, the term "modulo" is consubstantial to the definition of this relation, so $\vDash_n$ and $\vDash_{\mathcal{M}}$ are also different from the classical relation $\vDash$. Nevertheless, $\vDash_T$, $\vDash_n$ and $\vDash_{\mathcal{M}}$ are supraclassical, because they preserve the known structural rules of reflexivity, monotonicity and cut and logical symbols maintain their semantic values according to studied operative rules.

15.4 Defining Abductive Calculi

Is an abductive calculus definable given a deductive one? As is well known in the literature on abduction, it has been characterized as "deduction in reverse". Viewed this way, and thus taking into consideration a given calculus and the logical formulation of hypotheses, the set of abductive solutions of any sentence may be characterized as the set of all antecedents of such sentence that are true in certain situations, for example, those in which such sentence is true. However, this is too much general and ambiguous: let y be a sentence of L and $\mathcal{M}$ the class of L-models that satisfy y – any supraclassical consequence relation $\vDash_*$ could be considered – then

$$abduction\,(y) = \{z \in L \mid \text{if } M \in \mathcal{M} \text{ then } M \vDash_* z \rightarrow y\},$$

this definition leads to the absurd of obtaining L itself as the defined set, in accordance with the operative rule corresponding to material implication. An alternative could be to take as arguments of "abduction" the sentence and the consequence relation itself, as in:

$$abduction\,(y, \vDash_*) = \{z \in L \mid z \vDash_* y\}.$$

However, by the operative rules for material implication, this is equivalent to the following set

$$\{z \in L \mid \vDash_* z \rightarrow y\},$$

Therefore, the formal treatment of abduction is indeed a much more complex issue. Let us start by presenting the notion of an *abductive problem*. Given a set $A \subset L$, the sentence $y \in L$ and certain (syntactical) irreflexive relation $\mathcal{R}$, which is defined in the set of sentences with the purpose of setting certain relevance conditions, if there is $z \in A$ such that $\langle z, y \rangle \in \mathcal{R}$, then the pair (A, y) is an *abductive problem* with respect to a consequence relation $\vDash_*$ iff it is verified

1. $A \nvDash_* y$, and
2. $A \nvDash_* \neg y$

This notion is not necessarily restricted to classical entailment. In fact the process of constructing a scientific theory may use several types of reasoning. We propose to consider as underlying logic not only the pure classical logic, but logics defined from supraclassical consequence relations. Whatever the case may be, to face an abductive problem implies to search a solution, so the second notion we need is that of the *solution* of an abductive problem. A sentence x is a solution for an abductive problem (A, y) with respect to a consequence relation $\vDash_*$ iff

1. $A, x \vDash_* y$,
2. $A, x \nvDash_* \bot$, and
3. $x \nvDash_* y$

We are adopting the notions of consistent and explicative abduction, as defined in Aliseda (2006). From a formal point of view, abduction is a kind of inference that can be taken as a relation defined from pairs of set of sentences and sentences to sentences, that is to say, as a subset of the cartesian product $(\wp(L) \times L) \times L$. On the other hand, given an abductive problem there may be more than one solution, so to talk about a class of solutions is better. Let (A, y) be an abductive problem with respect to a consequence relation $\vDash_*$, then the set of abductive solutions is the following:

$$Ab_{\vDash *}(A, y) = \{x \in L \mid A, x \vDash_* y;\ A, x \nvDash_* \bot;\ x \vDash_* y\}.$$

Let us define an abductive inference relation, denoted by $\vDash_{AB}$:

$$(A, y) \ \vDash_{AB} x \text{ iff } x \in Ab_{\vDash *}(A, y).$$

Classical calculi are well known, like those based on natural-deduction and axiomatic, which are sound and complete. That is to say, is $\vdash$ represents one of such calculi, for every set of sentences $A \subset L$ and a sentence $x \in L$, it is verified that

$$A \vdash x \text{ iff } A \vDash x,$$

where $\vDash$ represents the classical consequence relation. For simplicity, since soundness and completeness of classical calculi are characteristic properties, we shall introduce the property of *suitability*. A calculus $\vdash$ is suitable for the classical consequence relation iff

1. If $A \vdash x$, then $A \vDash x$
2. $\vdash$ verifies structural rules of reflexivity, monotonicity and cut

Formally, any calculus may also be seen as a relation defined in L, that is to say, a subset of $\wp(L) \times L$, and we can extend the knowledge of their properties to other consequence relations, particularly to all supraclassical ones. In general, we shall say that any calculus $\vdash_*$ is suitable for a supraclassical consequence relation $\vDash_*$ iff for every set of sentences $A \subset L$ and sentence $x \in L$, if $A \vdash_* x$, then $A \vDash_* x$ and $\vdash_*$ verifies the mentioned structural rules.

For our purposes, the most interesting question is whether an abductive calculus is definable, particularly if it is possible to obtain the set of abductive solutions for an abductive problem with respect to a supraclassical consequence relation. Before giving an answer, however, let us present some derived operative rules.

Theorem 1 *For a set of sentences $A \subset L$ and sentences $x,y \in L$, if $\vdash_*$ verifies the negation rules and is suitable for a supraclassical consequence relation $\vDash_*$, then such calculus verifies the contraposition rule. That is to say $A, x \vdash_* y$ iff $A, \neg y \vdash_* \neg x$.*

Proof

Since suitablility is supposed, structural rules are verified. So the following deduction is justified

1. $A, x \vdash_* y$, hypothesis
2. $A, x, \neg y \vdash_* y$, monotonicity
3. $A, x, \neg y \vdash_* \neg y$, reflexivity
4. $A, \neg y \vdash_* \neg x$, second negation rule

To conclude, the reverse is analogous, so it is omitted ■

Another result in order to settle the definability of an abductive calculus, is given in the following:

Theorem 2 *A calculus $\vdash_*$ that is suitable for a supraclassical consequence relation $\vDash_*$ and accomplishes negation rules, verifies the derived $\bot$-rule: $A, x \vdash_* y$ iff $A, \neg y, x \vdash_* \bot$*

Proof

Since $\vdash_*$ is suitable for a supraclassical consequence relation, we can appeal to its structural rules to justify the following deduction

1. $A, x \vdash_* y$, hypothesis
2. $A, \neg y \vdash_* \neg x$, theorem 1

3. $A, \neg y, x \vdash_* \neg x$, monotonicity
4. $A, \neg y, x \vdash_* x$, reflexivity
5. $A, \neg y, x \vdash_* \bot$, second negation rule.

To conclude, the reverse is analogous, so it is omitted ■

From such derived rules the following proposition can be stated,

Theorem 3 *A calculus $\vdash_*$ is suitable for a supraclassical consequence relation $\vDash_*$ and verifies the negation rules iff $\vdash_*$ verifies the operative and structural rules mentioned above.*

First suppose that $\vdash_*$ verifies operative and structural rules mentioned above. Then $\vdash_{* \text{ and}} \vdash_*$ share such operative rules, which imply that it is not possible $A \vdash_* x$, then $A \nvDash_* x$ simultaneously. This is equivalent to say that if $A \vdash_* x$, then $A \vDash_* x$ or, which is the same, $\vdash_*$ is suitable for $\vDash_*$. On the other hand, suppose that $\vdash_*$ is suitable for $\vDash_*$ and verifies negation rules. Let A and x be, $A \subset L$ and $x \in L$, such that $A \vDash_* x$ and suppose that $A \nvDash_* x$, then $A \cup \{x\}$ is consistent, since in other case $A, x \vdash_* \bot$ and, by a corollary of theorem 2, $A \vdash_* \neg x$, from which, by suitability of $\vdash_*$, $A \vDash_* \neg x$, obtaining a contradiction. Then, by following a similar argument to prove classical completeness, an L-model M can be constructed such that M satisfies $A \cup \{\neg x\}$, which is contradictory with the fact that A $\vDash_* x$. So, if $A \vDash_* x$, then $A \vdash_* x$, but every operative rule has a set of sentences (it could be unitary) as antecedent and a sentence as consequent. So in each one of such rules the consequent is logical consequence, in the sense of $\vDash_*$, of the antecedent. ■

Is the notion of suitability applicable to an abductive calculus? Before giving an answer, we need to adapt such notion, which could be based on the concept of deductive calculus. In general, given an abductive inference relation $\vDash_{AB}$, an abductive calculus $\vdash_{AB}$ is a mechanism by means of which some solutions of an abductive problem (A, y) with respect to an inference relation $\vDash_*$ can be obtained. So, $(A, y) \vdash_{AB} B$ represents that B is the set of solutions obtained and, taking into account previous considerations, $B \subseteq Ab_{\vDash*}(A, y)$. Then we shall say that $\vdash_{AB}$ is *abductively suitable* for $\vDash_{AB}$ iff there is $\vdash_*$ such that if $(A, y) \vdash_{AB} B$, then for every $x \in B$ it is verified that $A, x \vdash_* y \vDash_*$ verifies the studied operative and structural rules.

Theorem 4 *An abductive calculus $\vdash_{AB}$ is definable as abductively suitable with respect to an abductive consequence relation $\vDash_{AB}$ iff there exists a (deductive) calculus suitable with respect to the corresponding supraclassical consequence relation.*

Suppose that $\vdash_{AB}$ is definable as abductively suitable with respect to $\vDash_{AB}$. Then, by definition, given any abductive problem (A, y) with respect to an inference relation $\vDash_*$, there is $\vdash_*$ such that if $(A, y) \vdash_{AB} B$, then for every $x \in B$ it is verified that $A, x \vdash_* y$ and $\vDash_*$ verifies the known operative and structural rules (so, it is supraclassical). Then, by theorem 3, $\vdash_*$ is suitable with respect to the supraclassical consequence relation $\vDash_*$.

Reciprocally, suppose that there exits a (deductive) calculus $\vdash_*$ that is suitable with respect to the supraclassical consequence relation $\vDash_*$. Then, for every abductive problem (A, y) with respect to $\vdash_*$, define the following set

$$Ab_{\vdash_*}(A, y) = \{x \in L \,|\, A, x \vdash_* y;\ A, x \nvdash_* \bot;\ x \nvdash_* y\},$$

which $Ab_{\vdash *}$ $(A, y) \subseteq Ab_{\vDash *}$ (A, y), then the abductive calculus $\vdash_{AB}$ such that $(A, y) \vdash_{AB} B$ iff $B = Ab_{\vdash *}$ (A, y), then, by definition, if $x \in B$, then $A,x \vdash_* y$. It should be noted that $(A, y) \vDash_{AB} x$ for every $x \in B$. Since, by hypothesis, $\vdash_*$ is suitable with respect to $\vDash_*$, it verifies the studied operative and structural rules. So, $\vdash_{AB}$ is abductively suitable with respect to $\vDash_{AB}$. ■

Let us see one example of modelling abduction by means of the consequence relations analyzed here. Given a consistent theory T endowed with a specific methodology, $T \subset L$, the study of inferences may be done from a supraclassical relation, namely $\vDash_T$. Then, an abductive problem should be expressed with respect to that consequence relation. Let A and y be such that $A \subset L$ and $y \in L$. Now the syntactic relation $\mathcal{R}$ would be settled taking into account T, A and y —for example, besides other requirements, if $a \in A$ there must be $b \in T$ such that $\langle$a, b$\rangle \in \mathcal{R}$—, so (A, y) is an abductive problem with respect to $\vDash_T$ iff $A \nvDash_T y$ and $A \nvDash_T \neg y$. Then $x \in L$ is a solution iff the three conditions are accomplished, namely

1. A, x $\vDash_T y$,
2. $A, x \nvDash_T \bot$,
3. $x \nvDash_T y$

Similarly, by taking the class $\mathcal{M}$ of models of such theory, $\vDash\mathcal{M}$. In this case, both relations are equivalent: if $\mathcal{M}$ is a class such that when $M \in \mathcal{M}$, $M \vDash T$, for $A \subset L$ and $x \in L$ it is verified that

$$A \vDash_T x \text{ iff } A \vDash_{\mathcal{M}} x.$$

If it is necessary to go on finite fields, we can make use of a specific supraclassical relations, namely $\vDash_n$ for a finite n.

A way to explore this is to consider sets of specific rules, for example when in a theory non-monotonic inferences are admitted, then we could define a *consequence relation modulo R* (Makinson 2005), symbolically, $\vDash_{\mathrm{R}}$. Now, to treat abduction it should be taken into account that some operative rules are difficult to maintain, as “deduction theorem”, which in general fails.

15.5 Concluding Remarks

We should search the unity of science by exploring what logical tools there are in common. In fact this is connected with the point of view according to which such unity should be based on certain unity of scientific methodology, without rejecting the richness of aspects that must be considered. The place of logic in scientific methodology is a matter of controversy and many theorists think that the form of reasoning in scientific practice is not well modelled by logical systems. However, it should be taken into account not only traditional systems of classical logic, but new systems proposed for modelling other forms of reasoning. A computational philosophy of science – defended in Thagard (1988) – represents an integral perspective, so in line with the methodological unity of science, and by appealing to possibilities of representing computationally some historical developments of scientific practices and relations between logic and computer science, logic has its own role in scientific methodology, though intending logic as a notion not restricted to "classical logic".

Abduction, sometimes based on deduction, may be seen as the prototype of scientific reasoning. In spite of some problems, such as *decidability, complexity*, etc., classical logic could be used after all to work out abduction logically. However classical logic is not the only way of modelling current deductions in scientific practices. On the contrary, there can be material inferences that cannot be captured by the classical formal entailment relation. For instance, deductions in the context of a given theory, which may be captured by a consequence relation modulo "such theory". In a similar way, if the universe of research had to be considered finite instead of infinite or when the interest is focused on preferential models or any other special class of models. In these cases, a logical treatment of abduction should be based on the respective consequence relations, since an explanation of a proposition (representing a fact) should take into account the kind of deduction after all. On the other hand, certain attractive characteristics of classical logic are preserved in "supraclassical logic", which can also be used as an advantage to study abduction.

Some suggestions can be made following Makinson (2005) as an inspiration with respect to other logics. This may be a first step to knock down the barrier of classical logic.

It is clear that the search of a plausible hypothesis involves a logical process after all, so that a consequence relation has to be taken into account, then there may be calculi to give account of it, which is the thesis explained above. In fact, some known deductive calculi had been used to work out abduction. This is the case of tableaux, as presented in Aliseda (2006), Mayer and Pirri (1993) and Nepomuceno (2002). About this, given an abductive problem (A, y), we can construct the corresponding tableau from the root $A \cup \{\neg y\}$, then there are open branches – if the tableau were closed, (A, y) is not an abductive problem actually – but every open branch can be closed by adding certain sentences, let B the set of sentences – with certain restrictions, according to definitions – that added to open branches give rise to pair of contradictions, coming to be closed. So if $x \in B$, then $A, x, \neg y \vDash \bot$, because $A, x \vDash y$. In short, tableaux defines $\vdash_{AB}$: $(A, y) \vdash_{AB} B$ iff the tableau of $A \cup \{\neg y, x\}$ is closed, for every $x \in B$. Other abductive calculi are based on *resolution* or *dual resolution*, which give "direct" solutions to abductive problems: $(A, y) \vdash_{AB} B$ iff $x \vDash A \rightarrow y$ for every $x \in B$, as it is shown in Soler-Toscano et al. (2006).

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Chapter 16
The Flat Analysis of Properties and the Unity of Science

Hossein Sheykh Rezaee

16.1 Unity of Science

The idea of the unity of science is one of the old attractive themes for both philosophers and scientists. At least three components can be distinguished in any model of the unity of science. The first element concerns the aspect in which, according to the model, scientific theories are unified. For example, according to Carnap (1938) in his "Logical Foundations of the Unity of Science", scientific theories can be unified in respect of their languages. He claims that all physical terms are reducible to the "thing-language", and if any other science wants to be legitimate, it must only use terms that are reducible to the "thing-language".

The second element concerns the strategy that by following it the unity of science in the alleged respect can be shown. Normally philosophers have used intertheoretic accounts of reduction for this part. For example, if somebody believes that scientific theories are unified in respect of their laws, then she might use Nagelian classic accounts as the second part of her model. However, it is not the case that any model of the unity of science needs an intertheoretic account of reduction. Finally, the third element concerns the generality of the model. This element asserts that the model covers which theories, and is silent about others. For example, Carnap thinks that his model is applicable to *any* scientific theory. Alternatively, some other models claim that they only cover macro-theories, i.e. they provide accounts for the unity of macro-theories with their counterpart micro-theories.

In the model I will defend in this paper, the first element is content of laws: content of some special-science laws (i.e. what they claim about the nomological connections in the world) can be analyzed in terms of content of fundamental laws. Regarding the second element, I do not need a particular account of reduction. Metaphysical considerations about the nature of properties are sufficient to show the unity. Finally, the scope of my account only covers *individual* special-science laws, or more precisely those special-science laws that connect two individual multiply

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J. Symons et al. (eds.), *Otto Neurath and the Unity of Science*, Logic, Epistemology, and the Unity of Science 18, DOI 10.1007/978-94-007-0143-4_16,

realizable predicates. Therefore, this model is silent about the relationship between theories that do not use multiply realizable predicates.

16.2 The Metaphysical Framework

In contrast with the causal (standard) analysis of properties John Heil (2003) has presented a flat analysis, which is composed of two theses. According to the first, which rejects the dominant layered picture of reality, *there is only one flat level of properties*. Let us review Heil's system which leads to this claim. The first principle for Heil (2003a, p.13) is that "Properties are to be distinguished from predicates." This means that not every predicate (even if it applies truly to objects) designates a property. The second principle concerns the nature of properties. When we say two objects share a property, it means that they have something in common. If we assume properties as universals, this principle says that the property is present in both of the objects. Otherwise, if we (like Heil) assume properties as tropes (or modes), the second principle says that the two objects are *exactly similar* in some respect, in respect of that property. Heil (2003a, p.13) puts this principle as follows, "objects share a property only if those objects are precisely similar in some respect." This principle entails that two properties are the same if they are identical (for the universalists), or exactly similar (for the trope theorists).

Now by applying these two principles we can determine whether a predicate designates a single property or not. If predicate "*P*" designates a single property *P*, then any object that satisfies the former possesses the latter. Hence, for the trope theorists any two objects that satisfy the predicate are exactly similar in some respect (in respect of property *P*). Heil (2003a, p.13) expresses this point as follows, "A predicate names or designates a property only if it applies to an object in virtue of that object's possessing a property possessed by every object to which it truly applies or would apply." In other words, if two objects satisfy the same predicate, but they satisfy it in virtue of having two different (not exactly similar) properties, then the predicate does not designate a single property.

Now consider the predicate "is red" as an example. If this predicate designates a single property, then every object that satisfies the predicate must possess this property. However, because of different shades of red we know that objects that we call red are not exactly similar in respect of their colour. They might have different colour tropes, which are similar but not exactly similar. This means that the predicate "is red" does not designate a single property, and there is no property of being red in our ontology. The situation then is that: we have a linguistic entity (a predicate), which does not designate a property; rather it gathers and groups a set of similar (but not exactly similar) tropes under one name.

Heil (2003, Chapter 3) extends this point to multiply realizable *predicates* (say "being in pain"). Here we have linguistic entities that gather a group of similar (but not exactly similar) tropes under one name. There are no corresponding properties to these predicates, because objects satisfy them do not possess the same property. Our ontology includes a flat level of basic properties. According to their similarities, these properties are gathered into different groups with different predicates as labels. However, there is no high-level property in the world.

There are two kinds of predicates. One kind designates single fundamental properties. If scientists discover that the most fundamental properties are, for example, quantum-mechanical properties, predicates that designate these properties belong to the first group. The second group of predicates is those that instead of single fundamental properties designate a group of similar (but not exactly similar) properties. Multiply realizable predicates belong to the second group. There is no single property corresponding to them. Therefore, multiple realization is not a special ontological relation between properties, rather this is a familiar concept that one predicate designates a group of distinct but similar properties. There are not multiply realizable *properties*. There are multiply realizable *predicates* designating sets of similar (not exactly similar) properties.

The second thesis in Heil's system expresses an identity: properties are simultaneously dispositional and qualitative.

> [The Identity Theory] If *P* is an intrinsic property of a concrete object, *P* is simultaneously dispositional and qualitative; *P*'s dispositionality and qualitatively are not aspects or properties of *P*; *P*'s dispositionality, *P*d, is *P*'s qualitativily, *P*q, and each of these is *P*; *P*d = *P*q = *P*. (Heil 2003, p.111)

Heil's second thesis will not be used in this paper and so there is no need to consider it in detail here.

16.3 The Flat Analysis of Properties and the Unity of Science

In this section, I will argue that Heil's first thesis entails a version of the unity of science. As mentioned, there are at least two kinds of predicate: fundamental and multiply realizable predicates. Multiply realizable predicates can be divided into two sub-categories: *individual* and *collective* predicates. Individual predicates are those that individual objects/persons/creatures... can satisfy them. Consider for example the predicate "... is red". This is a multiply realizable predicate, because two objects might be red while their colours are slightly different. In other words, being red in them can be realized by different physical properties, However, an individual object can satisfy this predicate, i.e. this predicates gives information about an individual object and not about a group of them.

However, collective predicates are those that are satisfied by groups of individual objects/persons/creatures... with particular relations among them. Consider for example the predicate "... has a constant birth rate". This is a multiply realizable predicate, which can be satisfied in different ways. In a group of animals there are many different possible combinations to have a constant birth rate. However, one individual creature cannot satisfy this predicate. This is a collective multiply realizable predicate and gives information about *sociology* and relations in a group of animals.

Heil's analysis of multiply realizable predicates fits well with individual predicates. Therefore, in this section the situation of those special-science laws that connect individual predicates will be discussed. Special-science laws with collective predicates will be discussed in the next section. Suppose we have a special science law, connecting two individual multiply realizable predicates. This law says any object possesses a member of the first corresponding set of similar properties, under the proper circumstances, will possess a member of the second corresponding set of similar properties.

Table 16.1 shows a special-science law and its corresponding fundamental realizer laws. "*P*" and "*Q*" are two individual multiply realizable predicates, which designate two sets of similar properties: *p1, p2*... and *q1, q2*... respectively. Now let us see what the content of Law (0) is. *Prima facie*, this law says that any object possessing one of the similar properties {*p1, p2*...}, under the proper circumstances, will possess one of the similar properties {*q1, q2*...}, via one the fundamental laws {*pi*→*qi*}. We can divide this *prima facie* content into three parts: (a) there is a similarity relation between *pi* realizing properties: *p1*≈*p2*≈*p3*..., (b) there is a set of fundamental laws: {*pi*→*qi*}, and (c) there is a similarity relation between *qi* realizing properties: *q1*≈*q2*≈*q3*....

The first point about these three parts is that the last claim is expectable from the first two, i.e. if *pis* are similar, and if any *pi* under the same circumstances brings about a *qi*, then we would expect that *qis* are similar as well. The reason of this claim is this. Properties (at least according to Heil's identity theory) are identical with their dispositionalities. Therefore, similarity between properties means similarity between dispositionalities. When an object has a particular dispositionality (a particular set of causal powers), the object manifests particular behaviours under certain circumstances. Therefore, if two objects have similar dispositionalities, they manifest similar behaviours under the same circumstances. If so, clause (c), which expresses similarity among manifestations of a set of similar properties under the same circumstances, is expectable from the conjunction of clauses (a) and (b), and the content of Law (0) is reducible to contents of (a) and (b).

However, considering actual special-science laws casts a doubt on this *Prima facie* interpretation of Law (0). Consider this law as an example of special-science law: "If a creature suffers from pain (i.e. satisfies the predicate 'being in pain'), and if the creature for some reason does not intend to suffer from pain, then the creature acts to get rid of the source of the pain." According to the previous interpretation, this law says two things about the world: first, it expresses a similarity

Table 16.1 A special-science law and its corresponding fundamental realizer laws

The special-science law	"*P*"→"*Q*" Law (0)
Fundamental realizer law	*p1*→*q1* Law (1) *p2*→*q2* Law (2) ... ...

relation among members of a set of (probably endless) properties that are designated by the predicate “being in pain”, and second, it expresses a set of (probably endless) fundamental laws connecting each of these properties to a corresponding fundamental property.

This interpretation has a problem. When we know the mentioned special-science law we do not know all *actual* similar realizers of “being in pain”. We only know realizers of “being in pain” in some familiar creatures. For example, we know that “being in pain” is realized in human beings by the brain state (property) *p1*, and in octopuses by *p2*, and so on. More importantly, even if we claim that we know all actual realizers of “being in pain”, we cannot claim that we know all *possible* (probably endless) realizers of “being in pain”. By knowing the mentioned law, we cannot claim that we know a possible realizer of “being in pain” in a different possible world and in a radically different creature. In other words, clause (a) is much richer than the special-science law and by knowing the latter we cannot claim that we know the former.

A suggestion to solve this problem might be that we have to limit clause (a) only to those actual realizers of “being in pain” that we know them. Therefore, this law does not enumerate the entire *pis*, and the entire set of fundamental laws $\{pi \rightarrow qi\}$, instead it expresses a similarity relation between few actual samples of *pis* and their corresponding fundamental laws. For example, clause (a) is something like this: there is a similarity relation between properties *p1*. . . *pn*. However, this suggestion ignores the *projectibility* of the special-science law. We want to have a law such that in addition to the actual and familiar examples of pain in familiar creatures, says something about any possible instance of pain.

Therefore, we need an account of the content of special-science laws that saves their projectibility, but does not mention all possible realizers of their predicates. My suggestion is this. Suppose that *p1* is realizer of “being in pain” in a familiar creature (no matter which one, but for the moment suppose human being), and *q1* is a realizer of “avoidance behaviour” in the same creature. The content of Law (0) can be expressed in two parts. (A) A fundamental law expressing that there is a nomological relation between *p1* and *q1*, (Law (1): $p1 \rightarrow q1$), and (B) Under the same circumstances, any property similar to *p1* (say *pi),* brings about a property (say *qi*) similar to what *p1* brings about (*q1*).

The first clause shows that Law (0) is based on pain experience in some familiar and well-known creatures (like human beings), and the second clause guarantees that Law (0) is projectible and so is applicable to other creatures who experience pain. Clause (A) is a fundamental law, a nomological relation between two realizers of the high-level predicates, no matter which one (Law (1) or Law (2) or. . .). Clause (B) is a general principle that is common among all special-science laws, saying that under the same circumstances similar properties bring about similar results. Because this general principle appears in all special-science laws, let me call it “the similarity principle” and consider it in more detail.

The similarity principle is exactly the same principle that we appealed to in order to claim that the third part of the first interpretation of the content of Law (0) is expectable from the first two parts (i.e. (c) is expectable from (a) and (b)). At that stage, I argued that because *pis* are similar and each of them brings about another property (*qis*) by a fundamental law, *qis* should be similar as well. Therefore, it is not surprising that in the second interpretation of the content of Law (0), I added the clause (B) only to save projectibility of the special-science law. In the flat framework of properties, if someone wants to defend special *sciences* with *projectible* predicates, she needs a principle like the similarity principle, saying that similarity among properties that is in fact similarity among their dispositionalities brings about similarity among manifestations of the properties under the same circumstances[1].

Now let us consider the epistemic situation of the similarity principle. It seems to me that this principle is a conceptual truth about the similarity relation. Its justification is not because this principle has been examined many times in many different situations and has enough supportive evidence. In other words, we do not accept this principle as an empirical generalization obtained by *induction*. The reason is clear; induction itself is an application of this principle. We argue, for example, that similar samples of water will behave similarly under the same circumstances; all of them boiling at the same temperature. In other words, in an inductive reasoning, by appealing to the similarity principle, we argue that because similar properties under the same circumstances will bring about similar results, and because we have enough evidence that a particular property under particular circumstances brings about a particular result, therefore any other similar property will bring about the similar result.

The reason for accepting the similarity principle, rather than induction, is that it is a conceptual truth, and its truth stems from the nature of the similarity relation. It is built in the similarity relation such that similarity *per se* entails (or even means) that under the same circumstances two similar things behave similarly. Therefore, although the similarity principle has some empirical content, it is not an empirical claim. It is a conceptual truth about the nature of similarity.

Now let us see where all of these leave us. We can analyze the content of a special-science law, connecting two individual predicates, into two parts: one is a fundamental law belonging to the basic level (say ultimate physics); another one is a conceptual truth about the nature of similarity, which is common among all special-science laws. By keeping in mind that for our present purpose it is not important which one of the fundamental laws (Law (1) or Law (2) or. . .) or which combination of them is placed in the first part of the analysis, we reach this conclusion. There is a unity between individual special-science laws and fundamental laws. This is the unity of content: the content of an individual special-science law (i.e. what it claims about the nomological connections in the world) can be analyzed in terms of content of some fundamental laws plus a conceptual truth about the nature of similarity. This means that although these special-science laws express their contents in unique and different ways, what they say (their contents) are nothing more than what basic sciences (say ultimate physics) say about the world. As far as content of laws is concerned, the individual special-science and basic laws are unified.

16.4 An Open Question

As mentioned, two kinds of multiply realizable predicates can be distinguished. Individual predicates are those that individual objects/persons/creatures... can satisfy them. Collective predicates, on the other hand, are those that can be satisfied by groups of individual objects/persons/creatures... with particular relations among them.

On the basis of this distinction two kinds of special-science laws can be separated. In the first group two individual multiply realizable predicates are connected. As our previous discussion showed this kind of special-science laws can be analyzed in terms of individual basic laws.

The second group of special-science laws, however, are those that connect two collective multiply realizable predicates. As an example consider 'Malthusian Law' from population ecology: "when birth and death rates are constant, a population will grow (or decline) at an exponential rate." This law says something about a group of creatures with a particular sociology. Now the question is "How can we analyze collective special-science laws, which are very popular in special sciences, in terms of basic laws?" One possibility is to take the same strategy that was used to analyze individual special-science laws. In that case we showed that the content of an individual special-science law can be analyzed in terms of contents of individual basic laws plus the similarity principle.

Taking this strategy depends on whether collective multiply realizable predicates designate sets of similar *individual* and basic properties. If this is the case then there is no problem for collective laws, but if this is not then we need a different strategy. It seems unlikely to claim that "... has a constant birth rate" is a linguistic entity designating a set of similar *individual* and basic properties. This predicate says nothing about individual physical properties of animals; rather it says something about a particular relation among them. Therefore, intuitively it seems unlikely to claim that a collective multiply realizable predicate designates a set of similar *individual* and basic properties, and so the mentioned strategy does not work in this case. Another possibility is to say that a collective multiply realizable predicate designates a set of similar *collective* basic properties. This option might save the idea of the unity of science: content of a collective special-science law can be analyzed in terms of contents of collective basic laws and the similarity principle. However, taking this option faces us with questions about the nature of collective basic laws and therefore collective basic properties. Are collective basic properties genuine and real ontological entities that cannot be reduced to individual basic properties? If we take them irreducible, then it means that apart from intrinsic basic properties, we accept collective properties as building blocks in our ontology. But taking them as real properties contradicts the flat analysis of properties, according to which there is only one flat level of *basic* and *intrinsic* properties. In fact it must be said that there are two different categories of basic properties: individual/intrinsic and collective. On the other hand, rejecting collective basic properties as ontological entities faces us with the question that "What do collective multiply realizable predicates designate?"

In other words, it can be said that the flat analysis of properties does not provide a good analysis of collective multiply realizable predicates. It seems unlikely to say that these predicates designate sets of similar individual and intrinsic basic properties. If so, then the question is “What do they designate?”, or “Why are these predicates projectible?” The natural way is to say that they designate sets of similar collective basic properties. But now the question is “What is a collective basic property? Is it reducible to individual basic properties?” A positive answer means that contents of both individual and collective special-science laws are unified with contents of individual basic laws. However, in this case we have to show how a collective basic property can be reduced to individual basic properties. A negative answer means that contents of collective special-science laws are unified with contents of collective basic laws. However, taking this option contradicts the flat analysis of properties, according to which there is only one flat level of individual basic properties. In other words, to save the idea of unity for collective special-science laws costs acceptance of collective basic properties in our ontology. Taking this option by proponents of the flat analysis seems unlikely. However, they still need to explain how we can have projectible and collective multiply realizable predicates.

Notes

1. The clause ‘under the *same* circumstances’ is absolutely vital for the similarity principle. We are not talking about manifestation of one property (or two similar properties) under the *similar* circumstances. Chaotic systems show that one property (or two similar properties) in two similar but slightly different circumstances (different initial conditions) may bring about radically different results. The similarity principle does not guarantee that similarity among circumstances brings about similarity among manifestations.

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Chapter 17
Scientific Reasonableness and the Pragmatic Approach to the Unity of Science

Andrés Rivadulla

We are the heirs of three hundred years of rhetoric about the importance of distinguishing sharply between science and religion, science and politics, science and art, science and philosophy, and so on. This rhetoric has formed the culture of Europe. It made us what we are today (. . .) But to proclaim our loyalty to these distinctions is not to say that there are 'objective' and 'rational' standards for adopting them.

(Richard Rorty 1980, pp. 330–331)

17.1 Introduction

The question of the unity of science is one of the most important issues that has concerned the modern philosophy of science from the beginning. The idea of Unified Science was so important for the Viennese neo-positivists that, from 1933 until its dissolution in 1938, the Vienna Circle edited a collection called *Einheitswissenschaft* with publications of several of the most significant members of the neo-positivist stream. Moreover, already in the USA, the journal *Erkenntnis* changed the name of its number 8, 1939–1940, into *The Journal of Unified Science*, and, finally, an *International Encyclopedia of Unified Science*, that survived until the late 1960s of the past century, replaced both of them. Twenty monographs were published in this *Encyclopedia* in two volumes, from 1938 until 1969; among

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I have written this paper as a member of the research group of *Philosophy of Language, of Nature and of Science*, Reference number 930174, supported by the Universidad Complutense de Madrid, and of research project FFI2009-10249 on *Theoretical Models in Science* financed by the Ministry of Science and Innovation of the Government of Spain.

I am very grateful to an anonymous referee for helpful comments on an earlier version of this paper.

J. Symons et al. (eds.), *Otto Neurath and the Unity of Science*, Logic, Epistemology, and the Unity of Science 18, DOI 10.1007/978-94-007-0143-4_17,

them Hempel's *Fundamentals of concept formation in empirical science*, 1952, and Kuhn's *Structure of Scientific Revolutions*, 1962.

Rudolf Carnap's phenomenalism in *Der logische Aufbau der Welt*, 1928, and Vienna-Circle's physicalism in the 1930s faced the philosophical justification and explanation of the unity of science, that constituted the main aim of the *scientific world-view*, the philosophical programme proposed by Carnap, Hahn and Neurath in the 1929 foundational document of the Vienna Circle: *Wissenschaftliche Weltauffassung-Der Wiener Kreis*. The neo-positivist concern with the unity of science was intended to undermine Wilhelm Dilthey's distinction between natural sciences and humanities, the so-called *Geisteswissenschaften*. Carnap's *Aufbau* particularly was a philosophical monument erected from a logical positivist perspective in favour of the unity of science.

Verstehen vs. *Erklären* (Understanding vs. Explanation) was the contraposition between *Geisteswissenschaften* on the one side, and natural sciences on the other. This contraposition based on the assumed impossibility for the *Geisteswissenschaften* to show an empirical success comparable with the impressive success that the natural sciences allegedly were able to show since the outset of the Scientific Revolution. Thus, in spite of the intended purposes of Diderot and D'Alembert's *Encyclopedie*, 1740, to defend the Unity of Culture, a large tradition supporting this contraposition arose with Johann Gustav Droysen's *Grundriss der Historik*, 1850, Max Weber and Wilhelm Dilthey. The discussion was continued in the second half of the twentieth century by Georg Henrik von Wright, Hans Georg Gadamer and Jürgen Habermas, among others.

The Understanding/Explanation controversy, i.e. the debate *Geisteswissenschaften* vs. *Naturwissenschaften* arose as a reaction of the German historian and social philosophers against the positivist view prompted by Auguste Comte in the philosophy of sciences, by Herbert Spencer in the methodology of sciences and by Adolphe Quetelet in the statistical-sociological sciences. Quetelet, Comte and Spencer broke away from a bi-millenary tradition of Unified Western Culture that not contemplated any substantial difference between humanities and natural sciences, no privileged access to reality was warranted by the natural sciences. In this tradition natural sciences did no enjoy any special status in the whole of culture. Thus the question is whether this break with the tradition of Unified Culture in Europe, and the subsequent reaction introducing a fundamental distinction between humanities and natural sciences, was justified.

In this paper I am going to proceed in three steps. Firstly, I will point to the fact that Carnap's approach to the Unity of Science – a view according to which it was legitimate to give up the contraposition between *Naturwissenschaften, Psychologie* and *Geisteswissenschaften-*, grounded on the philosophical mistake that it was possible to provide a sound explanation for the *foundation* of the whole science on a unique firm basis. Moreover, the subsequent Vienna-Circle's physicalist attempt to save the situation was reasonably rejected by contemporary philosophers of science like Popper and Fleck, and, some years later, by the stream of methodologists that doubted the existence of a neutral empirical basis, and in general by the post-positivist epistemologists.

My second argument will be a more philosophical one: I will treat the question of whether the Unity of Science can be rescued by mimetically applying the method of natural sciences to social sciences and humanities. In order to answer these questions I will scrutinize the alleged overwhelming success of classical science. In particular I will point to the so-called threefold breaking-off of determinism. Moreover I will deal with the question about whether or not the natural sciences do harbour some kind of privileged and exclusive method for the access to reality. I will argue that, as contemporary physics shows, every form of scientific creativity, let us call it *induction, abduction* or *preduction*, only provides with means that allow us to deal *fallibly* with Nature.

Finally, if I am right, and the ideal of secure science reveals itself merely as a myth of rationalism in our scientific culture, I will propose to replace in the realm of science the requirement of rationality by that of *reasonableness*. Reasonableness is a weaker demand than rationality; it is neither tied to the idea of truth as the aim of science, nor to the existence of a secure and unique scientific method. But it is a guarantee that the justification of our conjectures, decisions, and, if possible, even of our fallible beliefs, is based on critical discussion and argumentation. This way makes superfluous any sharp distinction between natural sciences and humanities, but also any superimposed assimilation of the *Geisteswissenschaften* to the natural sciences, either by the way of the foundation of the different sciences on a common ground, or by the reduction to a fictitious physicalist language, or by a mimetic assimilation of the "superior" method of the natural sciences. To sum up, I conceive of the question of the unity of science as a particular case of the unity of Western culture from a pragmatic viewpoint in contemporary philosophy.

Since the defence of the unity of science amounts to overcoming the idea of the methodological dualism *natural sciences/humanities*, I will be first concerned with the task of surmounting the dualism *via* the neo-positivist foundationalist programme of the Unified Science. This is what I am going to do in the third section. In the next step I will face the question of whether Newtonian mechanics was justifiably the model to follow in social sciences and humanities. The existence of a unique proper method of the natural sciences that should be assumed by the social sciences, in order to be considered properly scientific, will be the main topic of the fifth section. Section 6 finally is a pleading on behalf of reasonableness in the sense given above. It is a consequence of the failure of imposing the idea of Unified Science either by postulating the existence of a common ground for natural and social sciences, or by claiming a methodological reduction of the social sciences to the natural sciences.

17.2 One Culture or Two Cultures: This Was the Question

2.1. The question of whether there was a need to differentiate between natural sciences and humanities arose in Western philosophy mainly as a reaction against the positivist philosophy that defended a monist approach to science and humanities consisting in a kind of reduction of the social sciences and humanities to the natural sciences.

A *natural* monistic approach to sciences and humanities dominated nevertheless practically the preceding bi-millenary tradition in Western culture. Indeed a certain *unifying encyclopaedism* was the current way of development of the occidental culture. Beginning with Plato and Aristotle in the fourth century B.C., who practiced an encyclopaedic mastering of all the branches of the Hellenic world: from astronomy to ethics, from politics to logic and mathematics, from rhetoric to zoology, etc.

In the fifth century Martianus Capella fixed in his *De nuptiis Philologiae et Mercurii* the seven liberal arts that in the Middle Age constituted the *trivium* (grammar, rhetoric and dialectic) and the *quadrivium* (arithmetic, music, geometry and astronomy). In the twelfth century, Dominicus Gundissalinus (also known as Gondisalvi and Gundisalvo), an important member of Toledo's translation school, extended in his *De divisione philosophiae* the *quadrivium* to encompass physics, psychology, metaphysics, politics and economics, thus conferring to it a more universal character.

In the seventh century Saint Isidoro of Seville compiled in 20 books in his encyclopaedic work *Etimologiae* the different branches of ancient knowledge.

In the twelfth and thirteenth centuries Robert Grosseteste (1175–1253) contributed to philosophy, theology, physics (optics and cosmogony), geometry, and anticipated clearly Galilei's view on the applicability of mathematics to Nature's knowledge: *It is of the greatest usefulness to take into consideration lines, angles and numbers because without them it is impossible to know anything about natural philosophy*, or *All the causes of natural effects have to be given by means of lines, angles and numbers*. His disciple Roger Bacon (1214–1294) devoted also to theology, philosophy, the methodology of sciences – he introduced the expression *scientia experimentalis*, the theory of light and vision, geography, alchemy and astrology, and like his teacher maintained the viewpoint that *It is impossible to know the things of this world unless you know mathematics*, a view that he completed with a declared experimentalism: *Everything depends on experience.* Bacon's *Opus maius* is considered a kind of encyclopaedia as well. Finally *Doctor Universalis* Albertus Magnus (1193–1280) appears as a paradigm of encyclopaedic wisdom. He undertook the synthesis of Plato's and Aristotle's philosophical thought, but he was also a devoted zoologist, a botanist, and a chemist as well.

In the Modern Age grew a tendency to specialization, in a certain way mitigated by the humanist movement in the Renaissance. Indeed, Galilei and Kepler were mainly physicists and mathematicians. But the starter of the astronomical revolution Nicolaus Copernicus was still a typical medieval scholar. He studied theology, canonical law and medicine, was a practising physician, but he was also a philologer, and an economist, besides being a mathematician and an astronomer.

The traditional view of treating all cultural activities at the same level culminated with André-Marie Ampère. Worldwide known as the founder of electrodynamics, he was also an outstanding philosopher of science, as his 1834 *Essai sur la Philosophie des Sciences* shows. In spite of being a contemporary fellow of Comte and Quetelet, Ampère did not vindicate any special or privileged status for the natural sciences.

He proposed in 1830 a division of all our knowledge in two kingdoms: one comprising all truths related to the material world – *cosmological sciences*, the other dealing with human thought and societies – *noological sciences*. Only the object of investigation distinguished both kinds of sciences, since for Ampère the kingdom of human sciences shows structural features compatible with those of the natural sciences. The classification of the humanities or noological sciences is point to point structurally comparable with the classification of the natural or cosmological sciences.

Both kingdoms are identically structured. The kingdom of cosmological sciences incorporates the classification of all mathematical, physical and natural sciences known at the beginning of the nineteenth century. The classification of the noological sciences proceeds in two sub-kingdoms: the kingdom of the noological sciences properly, and the kingdom of the social sciences. The first ones encompass both the philosophical and the dialegmatic sciences, whereas the second ones encompass both the ethnological and political sciences. Each one of these four sciences is subdivided in two branches, and these in two more, etc., so that there result sciences of first, second and third order, among which we today identify: psychology, metaphysics, ethics, literature, pedagogic, ethnology, archaeology, history, military art, social economics, politics, logic, methodology, elementary ontology, natural theology, theodicy, etc.

Ampère (1834, pp. 14–17) explains not only the legitimacy of the noological sciences, but also the intricate relationships existing between both kinds of kingdoms:

> How can we not see the analogy that exists between the mathematical sciences and the sciences relative to the inorganic properties of bodies? And between these sciences and those dealing with organic beings, and between the latter and the study of the human faculties? And finally, is not the connection of such study with the study of the language of monuments, letters and the fine arts also evident, and of these to the social sciences? (p. 17)

2.2. An opposite stance was prompted by Quetelet, Comte and Spencer, who broke with the traditional view of the Unified Western Culture, thereby promoting a radical monist view of the Unified Science that not only was the root of the neo-positivist doctrine of the Unity of Science, but also motivated the dualist reaction of the contraposition humanities/natural sciences, and the understanding/explanation controversy.

In fact, Comte defended the rejection, that later constituted a fundamental point in the Vienna Circle, of metaphysical thought, and the mimetic application to sociology of the mathematical approach to mechanics as well. Spencer maintained on his side the view of the existence of a method for the discovery of truths characteristic of all sciences. And Quetelet finally applied the probability calculus to the study of social phenomena, thus giving birth to mathematical statistical investigations. *Mundum numeri regunt* was Quetelet's motto.

Auguste Comte's *positive philosophie* was an attempt of encyclopaedic classification of all sciences, from mathematics to *social physics*, pointing to their respective interrelations, and to the hierarchic *unity* of the whole science as well, from which theology and metaphysics are completely discarded. The distinctive character of Comte's positive science is the rejection of the search of essences, first and final causes. But Comte introduced as well the term *sociology* in the sense of *social physics*, to wit (Comte 1825 [1854, p. 150]) as

> a science that has as its proper object the study of social phenomena, to be considered in the same spirit as astronomical, physical, chemical and physiological phenomena, that is to say as subject to natural, invariant laws the discovery of which is the main aim of its research.

In a similar direction Herbert Spencer undertakes a classification of sciences, from Mechanics, Physics and Chemistry to Astronomy, Geology, Biology, Psychology, Sociology. The importance of Spencer's *monist* approach to the Unity of Science lies in the fact that *the same scientific method* is assumed to be shared by all the sciences: *induction*. Thus sociology has the same inductive character as biology and psychology, and it consists (Spencer 1876, pp. vii–viii) of

> the empirical generalizations that are arrived at by comparing different societies and successive phases of the same society.

Adolphe Quetelet was not a philosopher. But he had implicitly assumed a philosophy of science consisting in the application of a quantitative method for the discovery of the laws of social human behaviour. Quetelet called first this science *social mechanics*, whose aim was the discovery of conservation principles, or natural laws, in human affairs, in analogy to physical mechanics. In 1835, *Sur l'homme et le development de ses facultés*, he changes its name and indirectly assumes Comte's expression of *social physics*. Indeed as Lottin (1912, p. 382) claims,

> One of the characteristic features of Quetelet's spirit is his tendency to find for the human species laws *analogous* to the ones that govern the physical world. Imbued with Laplace's *celestial mechanics*, Quetelet sought to found an *analogous science for those phenomena relative to the human species*; he wanted to create a *social mechanics* for the study of the laws governing the *social system*.

Nevertheless, one important point must be taken into account. Contrary to Comte, who rejected the application of probability calculus to sociology, Quetelet makes of this calculus the fundamental tool of his scientific activity, thus creating the science of statistical sociology. The question is that the application of probability to the investigation of social phenomena is as old as the doctrine of chances itself is. And here lies the origin of the so-called *science of theoretical statistics*. From its very beginning (Cf. Rivadulla 1991, 1995), the doctrine of chances was applied to the calculation of probabilities of life (Christian and Lodewijk Huygens, Leibniz, Halley, etc), to the calculation of life insurances (Jan de Witt), and even to justice (Nicolas Bernoulli, *De usu artis conjectandi in iure*, 1709). This was the ground on which it based the extraordinary extension of the application of mathematical probability to every kind of social researches. Déparcieux, Lacroix, Condorcet, Laplace, who in the Introduction to his *Théorie analytique des probabilités*, pp. LXX–LXXII, proposed to apply to the moral and political sciences the method used in celestial mechanics, were some of the most significant theoreticians preceding Quetelet, and the expressions *Political Aritmetick* (Petty 1960), *Social mathematics* (Condorcet,

1793) and *Social mechanics* (Quetelet 1831) form a chain that concluded with Comte-Quetelet's *Social physics.*

2.3. In this situation Johann Gustav Droysen (1808–1884) proposed to separate the aims of natural sciences from those of history. The aim of the first ones was *explanation*, whereas history, and by extension all humanities or *Geisteswissenschaften,* were committed to *understanding*. Understanding the *intention*, i.e. the search for *intelligibility* of human actions, was opposed to provide *causal explanations* of natural phenomena. Here roots the controversy *dualism* vs. *monism* in the methodology of scientific Western thought.

Droysen's view paved the way for a long and fruitful stream of thinkers in social philosophy, among which stand out Wilhelm Dilthey's sharp distinction between *Naturwissenschaften* and *Geisteswissenschaften* (humanities, history, sociology, according to Carnap 1928, §23) in his *Einleitung in die Geisteswissenschaften*, Leipzig 1883 (1922), Hans Georg Gadamer's development of interpretative hermeneutics -as Georg Henrik von Wright (1971, pp. 29–31), asserts, "In explicit opposition to positivism's idea of the unity of science, hermeneutic philosophy defends the *sui generis* character of the interpretative and understanding methods of the *Geisteswissenschaften*", von Wrights viewpoint of *understanding* as "a prerequisite of every explanation, whether causal or teleological", and Jürgen Habermas's modern *theory of communicative action*.

17.3 The Neo-Positivist Approach to the Unity of Science

3.1. The idea of the Unity of Science is, together with the doctrines of the foundation of knowledge and the meaninglessness of metaphysics, one of the three main theses of logical positivism. This idea was proclaimed – "Als Ziel schwebt die *Einheitswissenschaft* vor" – as fundamental in the programmatic document of the Vienna Circle, published in 1929 on occasion of the First Congress on the Epistemology of Exact Sciences, to be hold in Prague. Rudolf Carnp's *Intellectual Autobiography* (1963, p. 52. My italics, A. R.) reminds us of this:

> In our discussions, chiefly under the influence of Neurath, the principle of the unity of science became one of the main tenets of our general philosophical conception. This principle says that the different branches of empirical science are separated only for the practical reason of division of labor, but are fundamentally merely parts of one comprehensive unified science. *This thesis must be understood primarily as a rejection of the prevailing view in German contemporary philosophy that there is a fundamental difference between the natural sciences and the Geisteswissenschaften* (literally 'spiritual sciences', understood as the sciences of mind, culture, and history, thus roughly corresponding to the social sciences and humanities). In contrast to this customary view, Neurath maintained the monistic conception that everything that occurs is a part of nature, i.e., of the physical world. I proposed to make this thesis more precise by transforming it into a thesis concerning language, namely, the thesis that the total language encompassing all knowledge can be constructed on a physicalist basis.

Rudolf Carnap's *Aufbau*, 1928, intended proposal was to present a *rational* and *logical reconstruction* of the real process by which the concepts of science and of every day life are formed. Carnap's main thesis is that any concept – object, in his terminology – is constituted by direct application of Russell's class and relation logic to the domain of the *immediately given*. The result is a *constitutional system* of all concepts (objects), independently of the scientific branch to which they may belong: natural sciences, psychology, cultural sciences. Following, if all concepts of science can be ordered in a unique constitutional system in a way that they are also reducible to the fundamental concepts of the system, and all scientific sentences are also reducible step by step to sentences about these fundamental concepts, then we have here the philosophical explanation of the unified character of science. Reducibility to sentences about fundamental concepts of the constitutional system meant *verifiability.*

The second step took place with the transition from *Aufbau*'s *phenomenalism* to *physicalism* in the very beginning of the thirties. As Rudolf Carnap (1934, pp. 248–249) recognizes, both expressions *physicalism* and *unified science* were proposed by Otto Neurath in the Vienna Circle. According to this new view, the unity of science grounds on the possibility of expressing all scientific sentences in the *physicalist* language. As Carnap (1963, p. 52) claims in "Physicalism and the Unity of Science", in his *Intellectual Autobiography*, "the total language encompassing all knowledge can be constructed on a physicalist basis". In this language are expressible the propositions of biology, but also the sentences of sociology and psychology are translatable into the physicalist language (Cf. Carnap 1931, 1932). Neurath (1931, pp. 408, 422), for whom sociology, *social behaviourism*, is taken in a wide sense, encompassing economics, ethnology, history, ethics, law insofar as they are free of metaphysics, shares also with Carnap the idea of the formulation of all sciences in the physicalist language. Indeed in 1935b, p. 6, he claims

> All sciences must be capable of formulation in the universal language of physics. There is no room, in this respect, for the distinction between natural sciences and sciences of the spirit. Psychology studies the behaviour of human beings that is intersubjectively describable in physical language, i.e. *behaviourism*. Sociology studies the behaviour of human groups, i.e. *social behaviourism.*

Neurath (1935b, p. 7) concludes

> These different scientific disciplines, unified in the same language, associated so as to carry out universally valid predictions, constitute *unified science.*

3.2. The idea of an *encyclopaedia of unified science* had been proposed 1 year earlier by Neurath (1935a) during the *Prager Vorkonferenz der internationalen Kongresse für Einheit der Wissenschaft*, which he (1935b, p. 54) also refers to in the following terms:

> The best model of our scientific ideal cannot be the 'system', but only the *Encyclopaedia* methodically elaborated with the means of the modern logic of science.

Carnap (1934, p. 261) supported Neurath's view of an *encyclopaedia of unified science* that culminated in 1938 with the publication of an *International Encyclopaedia of Unified Science*. In the very first pages of this *Encyclopaedia*, Neurath (1938) claims that this *Encyclopaedia* continues the work of the

French Encyclopaedists, but also improves the work of Comte, Spencer, Mill.[1] Nevertheless, from a theoretical viewpoint no new ideas in relation to the Unified Science appear in Neurath's 1938 paper. Indeed he repeats on p. 20 the view that

> An encyclopedia (in contradistinction to an anticipated system or a system constructed a priori) can be regarded as the model of man's knowledge.
> An encyclopedia and not a system is the genuine model of science as a whole. An encyclopaedic integration of scientific statements,. . ., is the maximum of integration which we can achieve.

Co-operation of scientists makes it possible that the *Encyclopaedia* achieves its aim of integration of the scientific disciplines, i.e. the unification goal.

3.3. Nevertheless in spite of the enthusiasm displayed by Neurath and Carnap the implementation of the unity of science and the realization of the *Encyclopaedia* was not very successful. The *Journal of Unified Science* did not survive its first number, and the *International Encyclopaedia of Unified Science* only published two volumes in 32 years, 20 monographs in total, very valuable each one from a philosophical viewpoint, but with scarcely any relation to the announced aim of the *Encyclopaedia.*

The ideal of Unified Science encountered many problems indeed. From the very beginning this ideal was tied to the problem of the foundation of knowledge. In his Intellectual Autobiography, Carnap (1963, p. 50) claimed:

> I believed that the task of philosophy consists in reducing all knowledge to a basis of certainty. Since the most certain knowledge is that of the immediately given, whereas knowledge of material things is derivative and less certain, it seemed that the philosopher must employ a language which uses sense-data as a basis.

Moreover one of the most characteristic theses of logical positivism, that has mainly contributed to its presence in the history of modern philosophy, the thesis of the elimination of metaphysics, which also partially depended on the thesis of the foundation of knowledge, was simply abandoned, once the unreasonableness of conclusive verification was recognized. [Carnap's later replacement of conclusive verification by gradual confirmation, leading to *inductive logic*, an enterprise in which Rudolf Carnap wasted most of his precious time, was finally abandoned as well as unviable by contemporary philosophers of science.]

The proper foundation of knowledge, which according to Carnap (1931, p. 437) constituted also an urgent task of physicalism, has been one of the most criticized ideas in the post-positivist philosophy of science. Carnap's implementation of the foundationalist programme in *Aufbau* was intelligent but enormously artificial. And when *phenomenalism*, charged with nearly insurmountable philosophical problems, was abandoned on behalf of physicalism, and the reducibility to the fundamental concepts anchored in the elementary experiences that constituted the immediately given was transferred to reducibility or translatability to protocol sentences, the unanimity about the form and function of the protocol sentences disappeared from the bosom of the neo-positivist family. The fictitious character of the protocol sentences was a big handicap for the credibility of the whole enterprise, and the ambiguity, artificiality and vagueness of the *physicalist language* [for instance Carnap 1938, pp.

46–48, does not use any longer this expression, but *physical language*: "That sub-language of the language of science, which contains – besides logic-mathematical terms – all and only physical terms", where with "physical terms" Carnap understands "those terms which we need – in addition to logico-mathematical terms – for the description of processes in inorganic nature". Together with *biology in wider sense*, that constitutes the whole of the rest of the science, and which includes psychology, the social science, the humanities and the history, Carnap considers that the basis of a unity of language is given] did not either contribute to the feasibility of the whole enterprise.

If the troubles and indecisions in the neo-positivist house were not enough, the thesis of the foundation of knowledge, what he later connected to *the bucket theory of mind*, was thoroughly attacked from outside by Popper since his *Logik der Forschung*, 1935 on. Ludwik Fleck (1935, p. 64) declared as well the *complete sterility* of the whole *Erkenntnis* 1 and 2 neo-positivist deal. Norwood Hanson's thesis of the *theory-ladeness of observational language*, and the post-positivist rejection of the existence of a neutral observational language, contributed finally to the definite abandonment of the philosophy of logical positivism.

The difficulties with the realization of unified science must have been so big, that Joergen Joergensen's (1951, pp. 82–83) scepticism on the viability of the whole enterprise in the second volume of the *Encyclopaedia* does not leave any room to doubt:

> Strictly speaking, the thesis of physicalism cannot be considered proved until the reduction to the thing-language of the total number of the concepts of the natural and social sciences is made, which means, of course, never.
> As regards the question of the reduction of scientific theories to a few or even a single deductive system, the prospects are, in the opinion of logical empiricists, much darker than where the question of the reduction of concepts to the physical thing-language is concerned. Not even all physical laws can at present be included in a single deductive theory, and the prospects for a derivation of biological from physical laws -let alone a derivation of psychological or sociological laws from the physical plus the biological laws- are distant, although not hopeless.

3.4. In spite of the difficulties of the neo-positivist approach to the unity of science, a new attempt to rescue the encyclopaedic project of the Vienna Circle is being undertaken nowadays by Shahid Rahman, John Symons and others. To this respect they haven launched a new series: "Logic, Epistemology and the Unity of Science" *in the spirit of* Diderot and Neurath. Rahman and Symons (2004, p. 7) summarize this project in the following terms:

> we see our work, inspired by the Vienna Circle's promotion of the scientific attitude in philosophy, as a part of a very traditional, though radical, Enlightenment project. Like Diderot and D'Alembert before them, Neurath's vision of the Encyclopedia is a cooperative and ambitious enterprise. We would like to see our series continue this tradition. This is why we take Otto Neurath as the inspiration for this series and look to his initial statement of the Encyclopedia of the Unified Sciences as our model.

The inspirers of the renewed unified-science-movement are well acquainted with the difficulties the original project had to face, some of which I have presented in point 3.3. above. But they are also aware of the new means available for the development of the renewed project. I welcome indeed the project, and in the encyclopaedist spirit of co-operation, I claim, from a pragmatic viewpoint, that the unity of science can be defended as a part of the unified western culture.

17.4 Was Newtonian Mechanics Justifiably the Model to Follow for Social Sciences and Humanities?

4.1. A close glance at the situation of modern physics shows that the methodological dualism *Naturwissenshaften* vs. *Gesiteswissenschaften* grounds on a deep mistake about the alleged privileged relationship of the mathematic-experimental sciences to reality.

Although the suspicion of the privileged position of these sciences in the realm of Western culture goes back to Galilei's work, there is no doubt that it reaches the climax with Newton. Already Bernard Fontenelle (1657–1757) in his *Eloge de Sir Isaac Newton*, read before the Académie des Sciences de Paris on November 22nd 1727, claimed that Newton was considered by his contemporaries as their chief and master, and that his *philosophy* was adopted in the whole of England. The French astronomer Alexis-Claude Clairaut (1713–1765) and D'Alembert, among others, qualified in the middle of the eighteenth century Newton's work as revolutionary. Moreover Laplace, Euler, d'Alembert and Lagrange contributed with the development of analytical mechanics in the eighteenth century to the enrichment and consolidation of Newtonian mechanics.

From a philosophical viewpoint the most important contribution to the popularity of Newtonian mechanics was Pierre Simon Laplace's (1749–1827) scientific determinism of his mechanistic world-view. In the Introduction to his *Essai philosophique sur les probalilités*, 1819, Laplace claimed that

> An intelligence that at any given moment knew all the forces that animate nature and the mutual positions of all the entities that compose it, if moreover this intelligence were vast enough to submit its data to analysis, it could embrace in the same formula the movements of the largest bodies of the universe and those of the lightest atom; nothing would be uncertain for it, and the future and the past would be present before its eyes.

According to Popper (1982a, p. 6)

> The fundamental idea underlying 'scientific' determinism is that the structure of the world is such that every future event can in principle be rationally calculated in advance, if only we know the laws of nature, and the present or past state of the world.

Since a deterministic world demands a deterministic theory, Newtonian mechanics would thus be a deterministic theory that truly represents the world.

4.2. The question is that for many reasons Newtonian mechanics is unable to accomplish this task. As Popper (1982a, p. 7) claims, "Newtonian mechanics does not entail 'scientific' determinism". First of all, because of the *problem of three or more bodies*. As Bruns already in 1887 proved, it is impossible to find out an analytical general solution to the problem of the determination of the movement of three particles of non-negligible masses m_1, m_2, m_3 that attract themselves according to Newton's gravitational law. The problem is not integrable and shows a chaotic behaviour. This problem can be considered the origin of the studies of the so-called *deterministic chaos*. As a consequence, since Newtonian mechanics was not even able to provide analytical solutions to the Three-Body Problem in celestial mechanics, it is no right to consider it a deterministic theory. [By the way, deterministic quantum-mechanical Schrödinger's equation also lacks of analytical solutions for multi-electronic systems.]

The second shortcoming "deterministic" classical mechanics was faced with was due to its inapplicability to mass phenomena. Indeed it was meaningless to compute the motion equation for each one of a huge number of interacting constituents. Thus in order to deal with mass phenomena it was inevitable to resort to statistical analysis. At the end of the nineteenth century Ludwig Boltzmann (1844–1906), James Clerk Maxwell (1831–1879) and Josiah Willard Gibbs (1938–1903) developed the classical statistical mechanics as an application of probability to mechanical mass phenomena of discernible particles. When it was necessary to deal with indiscernible particles in the realm of quantum physics, Enrico Fermi (1901–1954), Paul Dirac (1902–1984), Satyendra Nath Bose (1894–1974) and Albert Einstein (1879–1955) developed quantum statistical mechanics already in the twentieth century.

Finally, the development of modern theoretical physics shows further shortcomings of classical mechanics. Indeed, relativity theory extends its applicability domain beyond that of Newtonian mechanics to objects with velocities comparable to that of light in the open space, and to very intensive gravitational fields, like in the proximity of black holes, where for instance time collapses. This means that in the proximity of massive bodies time flows slower than in other circumstances.

Moreover, if we pay attention to the Newtonian celestial model it can be claimed that it fails in domains *in which it should not fail, if it were true.* These failures must be considered more relevant than mere restrictions of its domain of intended applications. Indeed the Newtonian theoretical model was not only incapable to face old challenges like Mercury's perihelion advance. It was also incapable to give satisfactory answer to some intended applications like the light deflection by the Sun and the gravitational redshift. But what is of most philosophical relevance is the fact that the replacement of the Newtonian theoretical model did happen by means of theories for which the *predictive balance* was overwhelmingly favourable and they were *incompatible* with it. From this it follows that the Newtonian model had to be considered as an inadequate model to deal with gravitational phenomena, and that it is meaningless to claim that it does *represent* reality even approximately.

On the other side, quantum mechanics rejected the classical view that physical magnitudes can be measured with unlimited precision, since Planck's constant constitutes an inferior limit for the precision of every measure. Moreover, the probabilistic nature of quantum mechanics incorporated indeterminism into the subatomic domain.

The threefold break of determinism and the inapplicability of classical mechanics to domains where it should offer fruitful results, if it had been a true representation of reality, makes evident that the expectancies woke up by it were not completely justified. Thus it does not seem justified the optimism of those like Quetelet and Comte who saw in the natural sciences, particularly in Newtonian celestial mechanics, a model to follow in social sciences and humanities.

What does it look like the issue of the existence of a *scientific method* in the physical sciences, which are the paradigmatic natural sciences?

17.5 Fallible Strategies as Means of Dealing with Nature in the Methodology of Science

Since the beginning of the methodology of science nearly 2,400 years ago, philosophers have been looking for different ways of scientific discovery. Aristotle's well known inductive inference was intended to provide a method of legitimatization of first principles. Plato preferred a different procedure, which can now be identified with Peircean abduction, as it aimed at the postulation of geometrical hypotheses in order to save the appearances presented by movements of the planets. Old astronomy from Plato until Kepler provides excellent examples of abductive procedures for the postulation of astronomical geometrical models.

From a logical viewpoint both induction and abduction are logically illegitimate. And a middle way like Bayesian probable inference has proven also to be untenable (Cf. Rivadulla 2004b). Nevertheless induction and abduction can legitimately be accepted as methodological strategies providing hypotheses that allow to deal fallibly with Nature. The only condition is to assume that scientific *ars inveniendi* is not submitted to rules, i.e. that there is no algorithmic procedure capable to produce secure knowledge. And this amounts to relinquishing to the quest for truth and certainty at theoretical level in science.

Furthermore there is a third strategy, commonly applied by theoretical physicists, but which philosophers of science seem not to have identified yet. I call it *theoretical preduction*, and it consists in a form of anticipative reasoning that starts from accepted results provided by the available theoretical background which are *methodologically* postulated as premises of the inferential procedure.

The products of preductive inference are theoretical models, factual hypotheses and theoretical laws, and they are fundamentally fallible constructs, since they depend on the assumed theoretical available background, which is not known to be true.[2] Many of them can be very successful indeed. Nevertheless it is not unusual that preduced constructs must be rejected as unviable ways of dealing predictably with Nature: Bohr's atomic model, Rayleigh-Jeans radiation law, Helmoltz-Kelvin gravitational collapse model of the stars' energy, etc are good examples of this. Occasionally empirical data do not oblige to the rejection of preduced theoretical constructs, but do put them seriously in jeopardy, for instance the solar neutrinos problem or the temperature of the solar crown for the case of current solar theoretical models.

Preduction and abduction oppose to each other in the methodology of science. Whereas by means of abduction, which is an ampliative inference, the inferred hypotheses are suggested by available empirical data, by means of preduction the inferred hypotheses are deductively constructed on the basis of the available theoretical framework. The theoretical background is constituted by entire disciplines, like physics, as a whole, insofar their theories and theoretical constructs are assumed to be consistent with each other. Basically *preduction* is an extension of deductive reasoning to the context of scientific discovery.

In conclusion I claim that *induction, abduction and preduction are merely reasoning strategies we use in the methodology of physics, in order to provide predictive hypotheses that allow us to deal fallibly with Nature*. Nothing in the methodology of physics points to the existence of a unique and secure method that could be more or less mimetically assumed as a model to follow by social sciences and humanities. Thus if there is no such method, then it seems that it was neither necessary to plead for a dualist fundamental difference between hard and soft sciences, nor for trying a monistic methodology embodying social sciences and humanities and natural sciences.

17.6 Reasonableness and Rationality in Western Scientific Thought

On August 24, 2006, at the XXVIth General Assembly of the *International Astronomical Union* in Prague, astronomers voted a new definition of the term *planet*. As a consequence of this Pluto was removed from the list of solar planets, and reduced to the new category of *dwarf planets*.

Could not we philosophers follow the example of the astronomers and agree on a definition of (*scientific*) *rationality*? If we did, then we would be in a situation allowing us to determine if some decisions taken by scientists or scientific communities are strictly rational or merely reasonable. But as long as the meaning of *scientific rationality* is not agreed upon, I will have to indicate some features of rationality we are supposed to accept more or less. In order to do this I resort to Rorty (1991, p. 35), according to him

> In our culture, the notions of 'science', 'rationality', 'objectivity', and 'truth' are bound up with one another. Science is thought of as offering 'hard', 'objective' truth: truth as correspondence to reality, the only sort of truth worthy of the name. (. . .) We tend to identify seeking 'objective truth' with 'using reason', and so we think of the natural sciences as paradigms of rationality. We also think of rationality as a matter of. . . being 'methodical'. So we tend to use 'methodical', 'rational', 'scientific', and 'objective' as synonyms.

This is Rorty's *cliché* of rationality in the *strong* sense of the term. It meets the standards of rationality which any scientific realist would agree on. For instance Karl Popper (1983, pp. 6–7):

> By a rationalist I mean a man who wishes to understand the world, and to learn by arguing with others. (. . .) By 'arguing with others' I mean, more specifically, criticizing them; inviting their criticism; and trying to learn from it. The art of argument is a peculiar form of the art of fighting – with words, instead of swords, and inspired by the interest of getting nearer to the truth about the world.

> I believe that *the so-called method of science consists in this kind of criticism*. Scientific theories are distinguished from myths merely in being criticisable, and in being open to modifications in the light of criticism. They can be neither verified nor probabilified.

In a few words, Karl Popper's lemma about rationality is that *rationality is simply openness to criticism* (*op. cit.,* p. 27). With the particularity that for Popper *scientific method* is critical attitude about theories, hypotheses and conjectures, in clear opposition to verification and probabilistic methods. Thus Popper's idea of scientific rationality fits very well to Rorty's *rationality cliché*.

A general tendency to truth approximation and to the assumed existence of a *scientific method* were, according to Kitcher (1993), the general features of what he labelled the *legend of science*. Although the *legend* reached its climax at the times of logical positivism, Kitcher declares in the *Post Scriptum* of his book that the *legend* is not yet dead, for it was in general right about the features of science. It only needs a metamorphosis. But since according to him truth is the most obvious epistemic goal of science, and rationality is a notion that relates together goals and means, it becomes difficult to see how Kitcher's search of a middle way between rationalism and anti-rationalism could be successful.

Paul Thagard's (2004) view on scientific rationality fits also very well to the standards widely accepted by scientific realists: Besides practical goals, science aims at epistemic goals like truth and explanation. For him

> A person or group is rational to the extent that its practices enable it to accomplish its legitimate goals.
> And
> The occasional irrationality of individual scientists or groups is compatible with an overall judgment that science is in general a highly rational enterprise.

Contrary to these realist viewpoints, my reflections on Sections 4 and 5 above show that the quest for truth and certainty is not the aim of scientific research, since history shows that the *theory is not the space of truth*. Moreover, as it can be seen also from foregoing sections, the existence of a unique scientific method is illusory.

Indeed there are situations in science appearing as undoubtedly rational steps. For instance, as I have argued in Rivadulla 2004a, p. 418: "If we are given two theories, and one of them constitutes a limiting case of the other one, then we are in a privileged situation in order to make a rational choice between them. Indeed the existence of limiting cases in mathematical physics allows one to account for theory change as an intrinsically rational process." Nevertheless the claim that one theory constitutes a limiting case of another one, and that the decision on behalf of the other is strictly rational, has nothing to do with truth or certainty. Astonishingly *scientific realist* Karl Popper (1982b, pp. 29–30. My italics, A.R.) seems to share this point of view as his following words betray him:

> The decisive thing about Einstein's theory, from my point of view, is that it has shown that Newton's theory -which has been more successful than any other theory ever proposed- *can* be replaced by an alternative theory which is of wider scope, and which is so related to Newton's theory that every success of Newtonian theory is also a success for that theory, and which in fact makes slight adjustments to some results of Newtonian theory. *So for me, this logical situation is more important than the question which of the two theories is in fact the better approximation to the truth.*

Together with the use of rationality in strong sense, there is also a *weak* sense of rationality, which according to Rorty (1991, pp. 36–37) means the following:

> In this sense, the word means something like 'sane' or 'reasonable' rather than methodical. It names a set of moral virtues: tolerance, respect for the opinions of those around one, willingness to listen, reliance on persuasion rather than force. These are the virtues which members of a civilized society must possess if the society is to endure. In this sense of 'rational', the word means something more like 'civilized' than like 'methodical'. When so construed, the distinction between the rational and the irrational has nothing in particular to do with the difference between the arts and the sciences. On this construction, to be rational is simply to discuss any topic – religious, literary, or scientific – in a way which eschews dogmatism, defensiveness, and righteous indignation.

Besides the episodes of strict rationality I prefer to talk about *reasonability* instead of *rationality* even in Western science, not only in culture in general. Reasonableness and rationality complement each other indeed, since although what is reasonable is not always rational, what is rational is necessarily reasonable. But once we have given up the search for truth and certainty as aim in scientific inquiry, once we recognize that strictly rational moments are the exception and not the rule in science, reasonableness becomes a most common feature of scientific decisions. Since reasonability is guided by such pragmatic principles like critical discussion, fallibility, intersubjective agreement, predictive success, etc., it is a common feature of decisions both in natural sciences and humanities. This offers an argument on behalf of the unity of Western culture.

Notes

1. More on this point in Rahman and Symons (2004, Section 4).
2. I have already presented the idea of *theoretical preduction* in Rivadulla (2008) and (2009).

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Chapter 18
Does Scientific Progress Necessarily Lead to a Unified Science?

C. Ulises Moulines

The ideal of a unified science was a powerful motivation for Otto Neurath's lifelong work in philosophy and science. After all, he decisively contributed to the project of an "Encyclopaedia of Unified Science". In general terms, this ideal was shared by most other members of the Vienna Circle and related groups of philosophers-scientists. However, it is not at all clear whether they all meant the same thing when they spoke of a "unified science". It seems that, for Neurath, it primarily meant a *unified scientific language*. Neurath's long-term aim was to develop a "universal jargon", a sort of scientific Esperanto, by means of which all kinds of scientific ideas and results could be expressed. Neurath sometimes referred to the universal jargon he envisaged as a "*Ding-Sprache*", a "thing-language", and sometimes as a "physicalist language". The latter denomination is somewhat misleading (and has misled many people) because it may be interpreted as the language of advanced physical theories. But this is certainly not what Neurath had in mind. It would be thoroughly incoherent with the rest of his views about the social role science is supposed to play to assume that the universal jargon for expressing truly scientific knowledge should be, say, the language of quantum mechanics or of the general theory of relativity. Rather, the Neurathian "thing-language" is a language consisting of terms referring to ordinary middle-sized objects; such a language should lead to statements that anybody can immediately check in everyday experience. It should be viewed as a language any scientist, or even the "man on the street" for that matter, could understand. Therefore, Neurathian "unified science" refers to the totality of our genuine knowledge as expressed by, and only by, a thoroughly purified thing-language.

Now, it should be clear that this sense of "unified science" is a rather weak one. Even if such a linguistic unification were really possible, it wouldn't necessarily imply a unification of concepts, much less of research methods, and still much less of theories. An example may make this clear. Suppose all people in the world would end up speaking Esperanto (or, what is much likelier, the kind of international

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J. Symons et al. (eds.), *Otto Neurath and the Unity of Science*, Logic, Epistemology, and the Unity of Science 18, DOI 10.1007/978-94-007-0143-4_18,

language presently used and widely known as "Broken English"), then we could still continue to have almost 200 states, more than 600 different nations, several dozens of incompatible religions, and many different, and rivalling, political programs. We could then speak of a "Unified World", but only in a very weak, almost Pickwickian, sense, since in such a world there would still be a huge and contrasting multiplicity of communities in other respects that matter more than language.

The Carnap of *Der logische Aufbau der Welt* had a stronger notion of "unified science" in mind: It meant for him *conceptual* unification of all branches of science. And this, in turn, meant for him that the concepts of all disciplines should be constructible step-by-step out of a common basis (whether this basis is to be interpreted in a "phenomenalistic" or a "physicalistic" sense, was for him a secondary matter). This kind of unification would certainly be stronger than the mere use of a "universal jargon", but it would still fall short of implying a real *unification of scientific knowledge*. As Carnap himself remarked on several occasions, the kind of unified conceptual system he undertook to construct in the *Aufbau* didn't guarantee by itself the possibility to derive all scientific *statements* from a single axiomatic basis, that is, to have one and only *one theory* about the (empirical) world. Obviously, you can devise different, even competing, theories within the same conceptual system.

So, there is a third, much stronger sense in which the slogan of a "unified science" may be understood: *theoretical unification*. We would fulfil this program if we could construct one single fundamental theory to which all other existing, or even conceivable, successful empirical theories could be reduced, and which therefore could explain and predict, either directly or indirectly (via the derived theories), every matter of fact. Now, it is not clear to me whether Neurath, Carnap and the other proponents of the unified-science-program of the 1930s and 1940s did think that, in the long run, such a theory should be, and would be, constructible after linguistic and/or conceptual unification had been accomplished. But whatever *they* really thought, it is to this latter sense of "unification of science" that the reflections contained in this paper are devoted.[1]

Few philosophers of science today think that the idea of a "fundamental theory of everything" is more than wishful-thinking. On the other hand, many present-day physicists, especially particle physicists and cosmologists, and still more especially, superstring theorists, are persuaded that such a theory is just around the corner. For my own part, I rather *tend to* think that the first side is right and the second wrong, as a matter of principle. However, in discussing this kind of issue, which is very important not only scientifically but also philosophically, we should not confine ourselves to making a priori sweeping statements for this or that position; we should first analyze the concepts involved as carefully as we can. In what follows I'll try to make a contribution to this aim.

I said before that the really strong, and most interesting, understanding of the unification-of-science-program is theoretical unification. This is so not only because this sense of unification obviously implies (or presupposes) the two weaker concepts mentioned, but, more importantly, because it has immediate *ontological* consequences. If there were a single fundamental theory from which all others could be derived and which could therefore cover (directly or indirectly) everything that happens, then our "ontological commitments" (to use Quine's phrase) would also appear, in some sense, as unified: They would be the ontological commitments of that theory of everything, and nothing else. From this, it still could not be deduced, without further ado, that the notion of "one world" (or "universe") makes really sense – as I'll try to show below. But, at least, there *might* be some prospects for being able to give a clear meaning to this notion. Theoretical unification does *not* strictly imply ontological unification, but, at least, it makes it plausible. On the other hand, if there are no signs of any theory of everything around the corner, then it is not at all clear what the justification for the notion of "one world" could be. Of course, if we are metaphysical realists, we could then still stamp on the table and shout: "I don't care at all about what theoretical science looks like! Whatever theories we have, there is just one world – THE WORLD!". My only answer to this reaction would be that, taking for granted that scientific knowledge is the most reliable kind of knowledge we have, the metaphysical realist's protest amounts just to a profession of faith and that this paper is not devoted to matters of religion. To my view, the ideas of theoretical unification and ontological unity, though not strictly implying each other, are close enough to each other to go hand in hand. They should be treated simultaneously.

So, let's analyze the prospects for theoretical unification as a matter of principle. Clearly, this notion depends on the logically previous notion of a (scientific) *theory*. Though it is not necessary for the present discussion to spell out all technical details, it is crucial that we have a reasonably precise notion of theory at hand. There are several, more or less worked-out proposals for defining the notion of a theory in present-day philosophy of science. They might differ in several respects; but, whatever the differences, most of them coincide in the idea that an essential element to determine a theory's identity consists of its *models*. This is my own starting point as well.

What is a model? Formal semantics provides an exact explication of this notion: A model is a *system* that satisfies a set of axioms (in the present context of discussion: a set of fundamental laws). And what is a system? The best explication we have of this notion comes from applying the conceptual tools of elementary set theory: a system is a *structure* (in the set-theoretical sense) purporting to represent, in a more or less idealized or approximate way, a particular portion of the reality we experience. Finally, what is a structure? Now we get at the bottom of our chain of definitions: A structure is a *finite sequence* of sets of objects and relations defined on those sets, i.e. it is an entity of the form

$$< D_1, \ldots, D_m,\ R_1, \ldots, R_n >,\ \text{where } \mathrm{R}_i \subseteq D_{j1} \times \ldots \times D_{jk},\ \text{for } i \leq n \text{ and } j_1, \ldots, j_k \leq m.$$

This is the (minimal) formal framework within which I envisage to discuss the issue of theoretical unification. It may not be the only formal framework adequate to discuss it, but it is the best one I know. So, let's explore the consequences of assuming this formal framework for the question of theoretical unification.

For a structure in this sense to be considered an empirical system, the domains D_i (at least some of them) must have a real empirical content (even though it might be highly idealized); that means that they are intended to represent (more or less ideally and approximately) some objects of our experience (in a very broad sense of "experience" which includes objects that may be quite remote from our sense perception but that we assume to exist and to be somehow related to objects we can perceive – in this sense, electrons and black holes are elements of our experience). Furthermore, for the structure to be an empirical system, it must be a model (in the sense of formal semantics just explicated) belonging to a given scientific theory. There is no science without some theory; consequently, an isolated structure, which is no model of any theory, cannot represent anything.

For the rest of this paper, I propose the following terminological convention. We'll say that T is an *empirical theory* only if the identity of T is at least partially determined by a class of models M such that at least some of the elements of M are empirical systems in the sense just explicated. For the issues that concern us here, only empirical theories will be considered.

When a structure $< D_1, \ldots, D_m,\ R_1, \ldots, R_n >$ is a model belonging to a given theory, we say that the sets D_i constitute the *basic domains* or the *universe* of the theory in question. The elements of these sets represent the things the theory is supposed to deal with, its ontology. They are its ontological commitments. In the case of theories of advanced science, say, of modern physical theories, to those domains some other sets, called "auxiliary domains" have to be added. They are needed in order to express magnitudes. Typical examples of such sets are the set of real numbers or the set of three-dimensional vectors. In physical theories, the relations R_j are, at least in part, functions in the set-theoretical sense whose values are real numbers, vectors, or similar mathematical entities. But this is not an essential point here. It is more important to note that the auxiliary domains are not part of what the theory conceives as its "real universe", of what the theory talks about or wants to talk about. In the present context, the auxiliary domains are thought of as useful fictions, allowing for a conspicuous formulation of the physical laws. The kind of thing the theory is about consists of its empirical domains D_i, or to be more precise, of their elements.

Let's consider an example. In a standard formulation of classical particle mechanics, this theory's models will have the form

$$< P,\ S,\ T,\ \text{IR},\ \text{IR}^3,\ s,\ m, f >,$$

where P represents a set of particles, S a portion of space, T a time interval, IR the real numbers, IR^3 the real vectors, while *s, m*, and *f* are the position, mass and force functions, respectively. Assuming this way of settling the theory's models, it becomes clear that the things that are assumed to be real objects are the elements of the domains *P, S*, and *T*, i.e. particles, spatial points and instants. This is the "universe" of classical particle mechanics.

Now, the first thing to notice is that, in other really existing physical theories, the "universe" admitted as real will look, at least at first sight, quite differently because their models will look quite differently as well. For example, in a model-theoretic reconstruction of equilibrium (phenomenological) thermodynamics, the basic domains won't contain particles, spatial points and instants but rather spatial regions and macroscopic states; in classical electrodynamics we'll have fields and instants, whereas stoichiometry's ontology will consist of chemical substances and a discrete (reaction) time.[2] And so on. If we take into account not only physico-chemical theories but also those from other disciplines (biology, psychology, economics, etc.), their "ontological commitments" will certainly appear to be still more different in kind.

Now, it might be objected that this formal explication of the general notion of a theory and of its models has not much to do with the idea of a unified conception of the universe as the totality of really existing things. Isn't it the case that the use of the term "universe" to refer to the basic domains D_i of the models of different theories is a mere metaphor, or still worse, a misleading way of speaking? Isn't it the case that our mind can, at least in principle, put all these domains, all those particles, spatial points, instants, states, fields, substances, etc. into just one big assemblage, precisely called "*the* universe", and that this "big thing" could, at least in principle, be the object of a theoretical examination – of a *unified theory* (even if this theory still doesn't exist in practice yet)?

There is no obvious immediate answer to this question. The possibility of such a unified conception of just ONE BIG THING called "THE WORLD" depends on the nature of existing scientific theories and *of their mutual relationships*. Given the huge amount of existing theories and the extreme diversity of their models, it seems on a first look that the prospects for a unified theoretical view of reality are rather slim. To this, the "theoretical unificationist" may retort that, in spite of the manifest diversity of theories and models, it would be an unbearable exaggeration to assume that all these theories play the same role for the prospects of a unified conception of reality. Some remote "mini-theory" whose models only represent a very small portion of our experience and that is used only by a few number of specialists cannot play the same role as, say, the general theory of relativity for considering the prospects of a theoretical unification of our knowledge. I agree with this objection. I agree that, in order to seriously discuss the prospects for a universal theoretical unification, only a quite particular kind of theory should be taken into account: *fundamental theories*.

Now, what is a "fundamental theory"? The first answer that naturally comes to mind is that a fundamental theory is a (well-confirmed) scientific theory that is *not reducible* to any other (well-confirmed) scientific theory. This is not an altogether bad answer. However, the notion of (theoretical) reduction has come much under fire in recent times, especially when one adopts a model-theoretic view on the nature of theories, as I do here.[3] To explicate the notion of a fundamental theory in terms of models it now seems to me that another meta-theoretical notion is preferable: *embedding*. This notion is intuitively akin to reducibility but it is not quite the same; it is weaker and more "flexible".

Let me try to explicate at least the essential aspects of the notion of embedding I have in mind. For this, let me first remind the reader of the crucial set-theoretical notion underlying it: the notion of a *substructure*. We'll say that a structure $S = < D_1, \ldots, D_m, R_1, \ldots, R_n >$ is a substructure of a structure $S' = < D'_1, \ldots, D'_{m'}, R'_1, \ldots, R'_{n'} >$ iff

(1) $m \leq m'$ and $n \leq n'$;

(2) $\forall i \leq m \exists i' \leq m' (D_i \subseteq D'_{i'})$;

(3) $\forall j \leq n (\exists j_1, \ldots, j_k \leq m (R_j \subseteq D_{j1} \times \ldots \times D_{jk}) \rightarrow \exists R'_{jk'} (R_j = R'_{jk'} / D_{j1} \times \ldots \times D_{jk}))$

Intuitively, *S* is a substructure of *S'* if all the domains of *S* are subsets of some of the domains of *S'* and all the relations in *S* are the restrictions of some of the relations in *S'*.

Now, we'll say that a model (or, more generally: a structure) μ is *embeddable* into a model (or, more generally: a structure) μ' iff there is a homomorphism φ of μ into a substructure of μ'.

Given our definition of the concept of a substructure, it follows that the embedding relation is reflexive and transitive (though not necessarily antisymmetric!); it induces a quasi-ordering in any given class of models.

In a derivative sense, we'll say that an empirical theory *T* is *(globally) embeddable* into a(n empirical) theory *T'* iff all the models of *T that are empirical systems* are embeddable into some models of *T' that are empirical systems*.

Intuitively, an empirical theory *T* is globally embeddable into another empirical theory *T'* if all of the empirical content of *T* is contained in (the homomorphic counterpart of) the empirical content of *T'*, but the latter will normally have "excess" empirical content with respect to the first theory. Analogously to the case of single models, the relation of global embedding between theories is reflexive and transitive; it induces a quasi-ordering on any class of empirical theories.

We could weaken this notion of embedding and make it more adequate to real-life cases of empirical science by introducing a notion of *approximate embedding* by means of some kind of topology on structures. But I don't think it is necessary for the purposes of the present discussion to enter into these technical details.

It seems clear to me (though, of course, this is not the place to provide a formal proof of it) that in real-life science we have many notorious examples of this kind of (exact or approximate) embedding as a relationship between two different theories. For example, it can be argued that Kepler's planetary theory is (approximately) embeddable into Newtonian particle mechanics, that geometrical optics is embeddable into undulatory theory, while the latter is embeddable into classical electrodynamics, that the latter and Newtonian particle mechanics are both embeddable into special relativistic theory, and that this in turn is embeddable into general relativity theory.

Assuming the notion of embedding introduced above and having in mind the examples just mentioned, it is quite immediate now to explicate the notion of a fundamental theory: We'll say that an empirical theory is *fundamental* iff there is no other (different!) empirical theory into which the first is embeddable.

We are now in a position to discuss the prospects for theoretical unification and for a sensible talk of "THE WORLD" in more precise terms. For that, however, we still need an auxiliary concept: the notion of *(independent) compatibility* between empirical theories. Roughly speaking, a theory *T* is (independently) compatible with *T'* if they are logically independent. In more precise terms:

T is (independently) compatible with *T'* iff

(1) Both *T* and *T'* have models that are empirical systems;
(2) Neither *T* is globally embeddable into *T'*, nor vice versa;
(3) The (metatheoretical) statement that a given empirical system *s* is embeddable into a model of *T does not imply* that *s* cannot be embedded into a model of *T'*.

It is straightforward to define *incompatibility*: *T* and *T'* are incompatible iff both are empirical theories, and neither one is embeddable into the other and they are not independently compatible either. The issue of the possibility of a theoretical unification of science can be recast now as an issue about the existence of fundamental theories. A priori, there are four possibilities to consider:

(A) There is no fundamental theory.
(B) There is exactly one fundamental theory.
(C) There are several different fundamental theories but they are all independently compatible.
(D) There are several different fundamental theories and some of them are mutually incompatible.

Since we know there are some really existing scientific theories with empirical models (this is a sort of "almost a priori" matter of fact), alternative (A) would imply that all of them are embeddable into some other theory. Given that the embedding relation is transitive, and that the number of really existing scientific theories is finite (this is another "almost a priori" matter of fact), the picture of the structure of science coming out of (A) would be that of a finite number of "closed embedding circles" (with possibly some "elementary" theories that are embeddable into other theories but don't embed any theory, that is, the circles may have some "legs" directed to the "bottom" consisting of elementary theories).

Alternative (B) is the one that best reflects the intuitions of the theoretical unificationist. There would be just one "big" theory that, while not negating the right to exist to other theories, would embrace, directly or indirectly, everything empirical that these other theories say. This corresponds to the intuitions (and wishes) of the superstring theorists, for example. It seems that it is also what Einstein was after in his later work.

The evaluation of alternative (C) for the program of theoretical unification is a more involved issue. It is certainly not the ideal the theoretical unificationist dreams of. This ideal, we have just noted, is represented by alternative (B). Nevertheless, assuming (C) is the case, the theoretical unificationist could argue (in a rather tongue-in-cheek way) that this situation wouldn't be so bad after all for the idea of unification: He might point out that we should certainly give up, at least for the moment, the claim that there is a single theory covering all that is real, and therefore there would not be a theoretical unification in the strong sense; but in a weaker sense, we could still speak of unification on the ontological level, so to speak. If the several fundamental theories are mutually compatible, then there is a sensible way to speak still of just ONE WORLD: Take all the domains of empirical systems that are models of the different fundamental theories and build their great union; the result is a big set that is perfectly defined, and that we are justified in describing as "THE WORLD". The theoretical unificationist could further argue that all what alternative (C) shows is that, in order to deal with this one world, we have to undertake some division of labour: Some theories deal with some parts of THE WORLD, some other theories with other parts, and they coexist peacefully; and though we cannot assume that these different theories can be put together in such a way as to constitute a single theory with THE WORLD as its model, nevertheless, there is a sense in which we have theoretically unified all our existing knowledge about THE WORLD: We have provided an harmonious set of theories which help each other in carving up THE WORLD. Not the theories but rather the reality they deal with would appear unified. I guess that is the world picture most working physicists have nowadays, and probably almost all other scientists that are not physicists as well.

However, I think this strategy of reinterpreting theoretical unification as a sort of "division of labour" between different theories dealing after all with the same world, is much more problematic than it seems at first sight. The crucial question is this. Granting that we can formally build the set-theoretical union (call it U) of all domains of all empirical models of all fundamental theories, what reasons do we have for claiming that U is THE WORLD – that single reality the unificationist is thinking of? Without additional considerations we have indeed no good reason for claiming that the formal construction of U has any ontologically relevant consequences which would in turn mean that there is in fact a single universe all theories refer to. To make this point clear, let me give a somewhat drastic example. Imagine that, in our times of postmodern tolerance, a neoliberal economist and a fundamentalist theologian meet and start a friendly conversation. The economist talks about economic agents and goods, while the theologian talks about angels and demons. As they notice that they have some difficulty in understanding each other since their respective theories appear to deal with very different kinds of things, they decide, for the sake of peaceful coexistence, that, contrary to appearances, there is indeed one single world they refer to: the world consisting of economic agents, goods, angels, and devils. Would this move really be a way out for the menacing problem of a diversity of universes? Certainly not. The courteous proposal of the economist and the theologian might be helpful as a diplomatic gesture but it doesn't change anything in the tremendous disparity of their respective universes. In order to be able to end up with a genuine common universe, the economist and the theologian should establish some non-trivial links between the objects of the respective domains of their theories; in a word, they should construct a theory that seriously deals with the relationships, say, between economic agents and angels, and between goods and demons. If they are not able to construct such a theory, the talk of a common universe is just spurious.

Let's put the issue in more general and formal terms. Suppose we would have only two fundamental theories T_1 and T_2. Call "U_1" ("the universe of T_1") the union of all the domains of the empirical models of T_1 and call "U_2" ("the universe of T_2") the analogous entity for T_2. We could say that $U_1 \cup U_2$ is the universe common to both T_1 and T_2 ("THE WORLD") only if there is a theory T_3 such that the union of its basic domains constitutes $U_1 \cup U_2$. But then T_1 and T_2 would both be embeddable into T_3, and they would not be fundamental theories anymore. So, alternative (C) would end up being the same as alternative (B). If (C) is supposed to be a genuine alternative, then there is no good reason to admit the existence of a single universe; on the contrary, we would have rather good reasons for speaking of an irreducible plurality of universes. We would have neither theoretical nor ontological unification.

This conclusion would be still more evident in case alternative (D) applies. The existence of several mutually incompatible fundamental theories is the best guarantee we can conceive of for the non-uniqueness of universes and for no unification at all. I don't think it is necessary to argue why.

From the logical analysis of the four possible alternatives about fundamental theories we have laid so far it comes out that the cases (A) and (B) clearly speak for theoretical (and therefore ontological) unification, whereas, in case (C), unification (even in a weak sense) appears to be a quite doubtful assumption; finally, case (D) clearly speaks against the assumption of unification. The foregoing results come out of a purely conceptual analysis of a priori given possibilities. The crucial question now is which one corresponds best to our meta-theoretical knowledge about the actual structure and content of empirical science. This question may, in turn, be interpreted in two different ways: from a synchronic and from a diachronic perspective. In the synchronic perspective, the question boils down to asking which one of the alternatives (A)–(D) best reflects the present state of scientific knowledge. On the other hand, from a diachronic perspective we may ask whether, from what we know about the evolution of truly scientific knowledge during the last two and a half thousand years or so, we can detect a general trend that points to one of the four alternatives identified here. Consider first the question as posed with respect to present-day empirical knowledge.

It seems to me that is not necessary to undertake a painstaking analysis of present-day physics textbooks to come to the conclusion that neither (A) nor (B) are plausible candidates to represent the structure and content of present-day physics (or, more generally, of empirical science). Most people would agree that we have at present at least two fundamental theories in the sense explicated above: general relativity theory and the so-called *standard model* for elementary particles. Both are theories that have empirical models and embed many other physical theories, while not being embeddable into each other. Superstring theorists might argue against the claim that general relativity theory and the "standard model" are fundamental theories by pointing out that superstring theory (in any of its versions) is precisely devised to embed both general relativity and the standard model. But whatever the actual, mathematical relationships between these two theories and superstring theory might be, it should be noted that superstring theory (in any of its versions) is not a fundamental theory in our sense: For one thing, it doesn't have acknowledged empirical models (remember that this is a necessary condition for our notion of a fundamental theory). The prospects for establishing superstring theory as a genuinely empirical theory look at present, in spite of tremendous efforts, rather grim.[4] So, it is safe to assume that, as far as we know, general relativity theory and the standard model are fundamental theories of physics. It is very likely that we should add at least non-equilibrium thermodynamics to this list. I know of no serious attempt to embed non-equilibrium thermodynamics either into general relativity theory or into the standard model (much less, of course, the other way around). Moreover, it is even doubtful that we can assert in good faith that equilibrium thermodynamics is embeddable into (classical) statistical mechanics and that the latter is embeddable into quantum mechanics.[5] As a consequence, we have to conclude that presently we have *several* fundamental theories in physics. Presumably, we have them in other empirical disciplines as well. For example, it could be plausibly argued that the *synthetic theory* in biology is another fundamental theory, since it is not embeddable into any other biological theory (though it embeds evolutionary biology and population genetics, among others). On the other hand, the best ethological theories we have at hand don't seem to be embeddable into the synthetic theory (nor, of course, vice versa). If we go to disciplines outside the natural sciences, the situation appears to be much more confused and it is difficult to tell what the fundamental theories there look like; but whatever they might be, it seems rather implausible to assume they will really be embeddable into biological and/or physico-chemical theories.

In sum, alternatives (A) and (B) don't appear to fit at all with the present overall landscape of empirical science. Of course, I haven't provided a *proof* of this (negative) claim. For this, a lot of work should still be done. First, a neat formal reconstruction of the plausible candidates for being fundamental theories in the different disciplines (general relativity theory, the standard model, non-equilibrium thermodynamics, the synthetic theory, and so on) should be provided – something we are still lacking. Second, the precise nature of their relevant empirical models should be spelt out. Third, their actual relationships to the other theories should be laid out in detail. None of this has been done; but the first impression that comes out from reading the specialized literature is clearly negative for (A) and (B).

Does the present situation in empirical science globally considered correspond rather to the picture suggested by (C) or to the one suggested by (D)? This question is difficult to answer. Among the plausible candidates for being considered as fundamental theories I have listed above, there are quite clear cases of pairs of theories that are not mutually embeddable but that are independently compatible because, intuitively speaking, they "have nothing to do" with each other. For example, there seem to be no incompatibilities between the general theory of relativity and the synthetic theory in biology; similarly between, say, the standard model in particle physics and non-equilibrium thermodynamics. On the other hand, many physicists and philosophers of science are ready to point, as an obvious matter of fact, to general relativity theory and the standard model as a crying case of incompatibility, indeed the most irritating case of this sort in present-day science. That's the reason, indeed, why so much effort is being made to find an alternative fundamental theory in physics – be it superstring theory or something else. Nevertheless, we should be cautious in our evaluation of this case. True, in an intuitive sense one might say that general relativity theory and the standard model are "incompatible" because their fundamental principles stand in no relation to each other and even their ontological commitments (a continuum for the first theory, a discrete ontology for the second one) profoundly diverge. But this would not be enough for incompatibility in the precise sense explicated above. Remember that one of the conditions for real incompatibility we have put forward is that assuming one of the theories leads to the assertion that the other theory *cannot* have empirical models. Only an assertion of this kind is a real menace for a "peaceful coexistence" between two fundamental theories. And I am not sure that this is really the conclusion we have to arrive at when comparing general relativity theory with the standard model. I suspect the issue depends heavily on how we want to interpret the intended range of application of both theories. Are they supposed to have models that represent the same range of experience? It seems that many present-day astrophysicists and cosmologists, especially when they deal with the Big Bang or with black holes, tend to assume a positive answer to this question. But it is not clear to me either that they *obliged* to do so or that all other physicists should follow them in this strong claim. One could perhaps envisage the empirical content of both theories in such a way that each theory minds its own business (the one essentially for the macrocosmos, the other for the microcosmos); that is, one could restrict the range of intended empirical application of both theories in such a way that "peaceful coexistence" becomes possible again. Then, the pair consisting of general relativity theory and the standard model would come out as an instance of alternative (C), and not of (D). Whether or not this is a sensible way-out in the present situation, I cannot tell. For all I know, the question appears to be still open.

At any rate, the result of our discussion is that the global structure of physics, and of empirical science in general, corresponds either to alternative (C) or to alternative (D). Bad news for the theoretical unificationist.

Confronted with this depressing panorama of global science, the unificationist may still counterattack and contend that this panorama reflects the present-day situation of science but that it doesn't correspond to the *overall tendency* of empirical knowledge in the course of its evolution. In other words, the unificationist might adopt a *diachronic* perspective and argue that, whatever the (unpleasant) situation nowadays might be, there are good reasons for assuming that physics, and science in general, if left to its "natural" development, *tends* to unification. That is, the diachronically minded unificationist will presumably argue that there are good reasons to think that the process of scientific development has such an overall structure that it tends by itself to a final stage in which all existing and well-confirmed theories of the social sciences will appear to be embeddable into biological theories, all biological theories embeddable into chemical theories, all chemical theories embeddable into physical theories, and all physical theories embeddable into a truly "Grand Unified Theory" with a single empirical system as its (presumably cosmological) model. This is a form of a well-known epistemological view: *convergentism*.

The diachronic unificationist becomes a convergentist: He/she claims that all historically existing empirical theories tend to an ideal limit consisting of one single theory with a single empirical model. What this model represents could then plausibly be called "THE WORLD", so that we would have ontological unification as well.

What reasons can the convergentist adduce for this vision of scientific development? I assume the reader who has followed me up to this point is someone agreeing with me in the presupposition that the metaphysical arguments of a speculative "philosophy of history (of science)", say, in a more or less Hegelian vein, are out of place here. If we agree on that point, then the grounds that are left for convergentism can solely be of an historical-empirical kind. The convergentist should convince us that the historical data available have an unmistakable convergent structure: Starting with a situation of extreme diversity in the theoretical representation of empirical reality some 2,500 years ago, we have gradually passed to stages of ever decreasing disparity. The diachronic structure of theoretical representation would have, intuitively speaking, the form of a "damped harmonic oscillator".

Is this view of the structure of the history of science adequate? Clearly, anyone purporting to confirm or refute it should be ready to write down a monumental treatise on the history of science. I know of no treatise of this kind, nor is this obviously the place for even sketching it. But a first, admittedly highly cursory, look at the actual data we have about the history of science doesn't seem to warrant anything like the convergentist's vision. For the sake of the argument, let's restrict our attention to the history of physics, or to be more precise, to the history of theories about the physical systems we encounter in our experience. It is true that in the dawn of the scientific spirit as we understand it now (say, with the Pythagorean and the Presocratic thinkers), a great number of different, even incompatible theories about the cosmos were devised. However, a couple of centuries later a first great scientific synthesis was worked out: the Aristotelian, or more precisely, the *Aristotelian-Ptolemaic paradigm*. This was to become indeed a highly articulated "theory of everything" of the kind many present-day physicists would like to have, and most scientists of the time accepted it and worked within it. In the late Middle Ages a new theory emerged that was actually incompatible (in our sense) with it: *impetus theory*. Still later, in the sixteenth and the beginning of the seventeenth centuries, the calendar problem and the invention of the telescope, among other things, led to the abandonment of the Aristotelian-Ptolemaic paradigm. In the course of the seventeenth century, a great number of diverse and partially incompatible theories (associated with the names of Kepler, Galileo, Van Helmont, Descartes, Huyghens, Leibniz and Newton, to mention only the most outstanding protagonists) were devised that were quite successful. As we all know, in this war of all against all which characterised the so-called "Scientific Revolution" of the seventeenth century, it was Newton's particle mechanics that finally won the battle: the most successful theories of the immediately preceding period were eventually embedded into Newton's paradigm, and the rest was just forgotten. For a century or so, Newtonian mechanics established itself almost as a new "theory of (almost) everything", though it never attained the utterly embracing character of the Aristotelian synthesis. (Remember that Newtonians soon had to acknowledge that optics, electricity and chemistry fell out of their paradigm.) In the nineteenth century, the scientific scene was again filled with a "chaos" of disunited and even partially incompatible theories: besides orthodox Newtonian mechanics there were Young's undulatory theory, the caloric theory, Faraday's electromagnetism, phenomenological thermodynamics, statistical mechanics, and what not. In the last decades of the nineteenth century it seemed as if Maxwell's work would lead to a new synthesis, but this hope would soon be disturbed by the emergence of the first and the second theories of relativity, the first and the second quantum theories as well as non-equilibrium thermodynamics, to mention only the most outstanding examples. In sum, this admittedly very brief survey of the history of physical theories (to say nothing of the other disciplines) appears indeed to have the structure of an "oscillator", but not of an "harmonic", much less of a "damped" one... In the course of the history of science as we know it, periods with a strong tendency towards unification have been followed by periods of theoretical disruption.

I conclude. Theoretical (and ontological) unificationism, either in its synchronic or in its diachronic version, doesn't seem to have a solid base to rest on. The question one could then still ask is this: Is this a situation we should be worried about? The dream of an ultimate "theory of everything" and its concomitant ultimate representation of THE WORLD, as the dream of an ultimate World State, may be felt as a beautiful ideal, a noble cause to fight for. And indeed a dozen generations of physicists (not so much other scientists) have seen it that way since the seventeenth century and have deployed tremendous efforts to make it real. However, even if we may acknowledge the historical fact that this dream has promoted many extremely valuable results, we may ask ourselves whether the dream of the Unique Theory of the Unique Universe (like the dream of the World State) may not reveal itself, if we stick to it too stubbornly, as a *nightmare*. Couldn't it be the case that the unfailing efforts to cast every corner of our experience into a single theoretical mould, like the efforts to cast every corner of the planet into a single political mould, would in the long run lead to spiritual death?

I don't know what the answer to this question is. But whatever it may be, I don't think the envisaged situation is just "around the corner". The supermarket of science is still full of quite diverse, and even competing, offers. I don't see why we should be sorry about this. I rather think we should enjoy it.

Notes

1. Here, I want to leave aside the issue of a possible *methodological unification*. The idea that we can establish a single general method of scientific research would convey a *fourth* sense to the expression "unification of science". But I don't intend to explore this possibility here, and this for several reasons. First, there is no room for discussing it in a short paper; second, this idea seems to be, in the present state of affairs, still a much more utopian goal than the other kinds of unification (what is the common general method underlying the methods of purifying chemical substances and video-recording the behaviour of chimps?); third, even by assuming, for the sake of the argument, that the notion of a "unified scientific method" could be explicated

in a reasonable sense, it seems to me that no much would be gained for the really interesting idea of scientific unification as conveyed by the slogan "one world – one science".

2. The reader can find the detailed model-theoretical reconstruction of these examples in Balzer et al. (1987) as well as in Balzer et al. (2000).
3. For a survey of the grave difficulties encountered by attempts to explicate reduction in terms of models, see Niebergall (2002).
4. For a quite recent evaluation of the state of the art with respect to superstring theory see Seife (2005, p. 82).
5. The reader may find a particularly thorough criticism of the usual textbook claims about the cases of thermodynamics and statistical mechanics in Sklar (1992, pp. 137 ff.).

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J. Symons et al. (eds.), *Otto Neurath and the Unity of Science*, Logic, Epistemology, and the Unity of Science 18, DOI 10.1007/978-94-007-0143-4,

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