OBJECTIVITY SANS INTELLIGIBILITY:

HERMANN WEYL'S SYMBOLIC CONSTRUCTIVISM

A Dissertation

Submitted to the Graduate School

of the University of Notre Dame

in Partial Fulfillment of the Requirements

for the Degree of

Doctor of Philosophy

by

Iulian D. Toader

_____

Michael Detlefsen, Co-Director

_____

Don Howard, Co-Director

Graduate Program in Philosophy

Notre Dame, Indiana

December 2011

OBJECTIVITY SANS INTELLIGIBILITY:

HERMANN WEYL'S SYMBOLIC CONSTRUCTIVISM

Abstract

by

Iulian D. Toader

The general topic of this dissertation is the relation between concept formation and the demand that scientific theories should provide an objective and intelligible account of natural phenomena, that is, an account that justifies their mind-independent reality and, at the same time, renders them understandable. More particularly, we consider the view of the mathematician and theoretical physicist Hermann Weyl, that this twofold demand cannot be satisfied, for it pulls science in opposite methodological directions, one driven by Husserl's pure phenomenology, the other by Hilbert's axiomatic formalism.

According to Weyl, scientific understanding requires wholly contentual reasoning and the phenomenological method of concept formation, that is, that concepts be introduced by abstraction from experience. Scientific objectivity requires partly non-contentual or purely symbolic reasoning and the method of formal axiomatics, that is, that concepts be freely created or introduced as mere symbols by stipulating, under certain constraints, fundamental theoretical principles.

This view, which we call *Weylean skepticism*, is important not only because it was propagated by one of the most influential scientists of the twentieth century, but also because it indicates how the tension that Weyl saw between objectivity and intelligibility can be dissolved.

Iulian D. Toader

We criticize, first, the attempt at dissolving this tension by adopting Husserl's pure phenomenological approach to scientific objectivity, which recently re-emerged in the literature. On this approach, contentual reasoning is indispensable for objectivity, which entails, as Weyl emphasized, that scientific concepts without contentual significance must be eliminated. We argue that Weyl realized that the phenomenological approach fails to account for objectivity, since it also entails the elimination of hypothetical elements, and so collapses into phenomenalism, which can support only intersubjectivity.

Secondly, we analyze Weyl's formal axiomatic approach to objectivity, and examine the requirement of categoricity, i.e., that a scientific theory, as a system of symbols, may provide objective knowledge only if its contentual interpretation is univocal up to isomorphism. But we argue, on the one hand, that this requirement fails to be satisfied in quantum physics, and that recent attempts at addressing this failure render theories unable to account for natural phenomena that they were designed to account for. On the other hand, we suggest that objectivity without categoricity commits one to a modal dappling of the world, that is, to the view that the structure of the real world spans many physically possible worlds.

Finally, we argue that the alleged tension between objectivity and intelligibility can be dissolved through a formal axiomatic approach to understanding. Against Weylean skepticism, we submit that the conditions under which purely symbolic reasoning may render natural phenomena understandable are expressed by the notions of simplicity and control. While the former can be conceived of as syntactic elegance, the latter obtains if one shows, by contentual reasoning, that the deviation from actual observations of results based on purely symbolic reasoning is smaller than experimental error.

DEDICATION

For Tee and Sasa

CONTENTS

iv

ACKNOWLEDGMENTS

The first draft of the dissertation was written in Zürich, where I worked through Weyl's manuscripts at the Swiss Federal Institute of Technology, in Summer 2008. My stay there was made possible by a research stipend awarded by Michael Detlefsen through the TransCoop Programme of the Alexander von Humboldt Foundation. I am indebted to Mic for this stipend, as well as for his extensive comments on subsequent drafts, pressing me to clarify and sharpen my arguments. He really helped me appreciate the truth of Aristotle's dictum: "Style to be good must be clear." [1]

The suggestion to work on Weyl came from Don Howard, to whom I am also grateful for making me fully realize the crucial importance of history for understanding contemporary philosophical views about science. Don has been always a genuine source of inspiration and encouragement. It was a privilege and a great joy for me to learn from him and from Mic. It is also a great pleasure to acknowledge the influence that their work has had on my research.

Don's papers and lectures determined me, for example, to investigate Weyl's contribution to the algebraic approach to quantum mechanics and, more specifically, the epistemological problem raised by the existence of unitarily inequivalent representations in quantum field theory. Mic's lectures and his lecture notes, and in particular his extensive analysis of what he calls Dedekind's ideal of "probative

---

[1]Cf. *Rhetoric*, book III, 1404b.

completeness," motivated my attempt to clarify Weyl's criticism of this ideal. But the influence exerted by my two advisors on this dissertation and, more generally, on my philosophical thinking is deeper and more extensive than I could manage to describe here.

I am especially indebted to Patricia Blanchette, for her continuous support and for discussions that helped clarify my own thoughts on several topics covered in this dissertation. I am also grateful to Timothy Bays and Anja Jauernig, for having served on my committees for oral examinations and/or proposal defense, as well as to Katherine Brading and Curtis Franks, for accepting to serve on my dissertation defense committee. For discussion and comments on various parts of my work, as well as for their friendly support, I am very happy to thank my colleagues and friends James Barham, Daniel Cibotaru, Florin Dumitrescu, Cristian Mihut, Chris Porter, and Sean Walsh.

I am very thankful to David McCarty, Colin McLarty, and Bernd Buldt, who kindly offered comments and suggestions; to Michael Weyl and Michael Gasser, for permission to copy and quote archival material and, respectively, for general administrative help with various manuscripts; as well as to Michael Hampe and Norman Sieroka, for their wonderful hospitality. Several studies on Weyl's work in the philosophy of mathematics and physics, recently published by Paolo Mancosu and Thomas Ryckman, should also be acknowledged, as they triggered my need to understand Weyl's relation with Husserl's phenomenology. Similarly, Robert Nozick's and Michael Redhead's studies, focused on invariance and its significance for scientific objectivity, have led me to investigate carefully Weyl's own efforts to find a solution to the problem of objectivity. I have learned a lot about this problem also from Steven French's work on structural realism and quantum physics.

CHAPTER 1

INTRODUCTION: OBJECTIVITY AND UNDERSTANDING

Time and again the passion for understanding has led to the illusion that
man is able to comprehend the objective world rationally, by pure thought,
without any empirical foundations – in short, by metaphysics. I believe
that every true theorist is a kind of tamed metaphysicist, no matter how
pure a "positivist" he may fancy himself. The metaphysicist believes that
the logically simple is also the real. The tamed metaphysicist believes that
not all that is logically simple is embodied in experienced reality, but that
the totality of all sensory experience can be "comprehended" on the basis
of a conceptual system built on premises of great simplicity. The skeptic
will say that this is a "miracle creed." Admittedly so, but it is a miracle
creed which has been borne out to an amazing extent by the development
of science.

Einstein[1]

1.1 The Goal of This Dissertation

It is reasonable to expect that scientific theories aim at providing an objective
and intelligible account of the natural phenomena to which they apply. This ex-
pectation is motivated by a demand of theoretical rationality to render these phe-
nomena understandable and, at the same time, to justify their mind-independent
reality. Nevertheless, the exact conditions required for the satisfaction of this

---

[1]Cf. Einstein 1950a, 342 (47).

twofold demand (and whether they can be met) have been a matter of controversy in the history of philosophy of science.

Berkeley, for example, not only argued that there is no mind-independent reality, but also criticized science for failing to bring about understanding of natural phenomena. He deplored the scientist' use of quantities like fluxions and infinitesimals, of which no clear and distinct ideas can be framed, and claimed that this renders science unintelligible. Since Kant, however, many philosophers have argued that, although there is no *knowable* mind-independent reality, science *can* offer an objective and intelligible account of natural phenomena, provided that one assumes their mind-dependent reality.

No later than the first half of the twentieth century, the German mathematician and theoretical physicist Hermann Weyl held the view that objectivity and intelligibility are fundamentally antagonistic epistemic ideals, which pull science in opposite methodological directions – one driven by Husserl's pure phenomenology, the other by Hilbert's axiomatic formalism. This view, which we may call *Weylean skepticism*, is that modern science, to the extent that it strives to attain objectivity, i.e., to provide knowledge about the mind-independent reality, may do so only at the expense of intelligibility; and, vice versa, to the extent that it aims at intelligibility, may do so only by sacrificing objectivity. To believe otherwise is to endorse what Einstein called a "miracle creed."

The goal of this dissertation is to articulate and evaluate the case that might be made for Weylean skepticism, and the case that Weyl himself made for it. This is important for at least two reasons. First, although the view underlies, as will be shown here, the most characteristic aspect of his philosophical thinking, it seems not to have been recognized by contemporary scholars working on Weyl.

2

We want to bring this aspect here under full light. Secondly, and perhaps more importantly, a careful analysis of his view should help us see what, if anything, would prove capable of dissolving the tension Weyl saw between objectivity and intelligibility, thereby giving support to the claim that scientific theories can help us to understand natural phenomena, as well as to justify their mind-independent reality.

In the remainder of this introductory chapter, we first offer a brief intellectual biography of Weyl, which is meant to emphasize the development of his philosophical ideas that led to the view just described. Then, we give an outline of the main argument that we take to support Weylean skepticism, and indicate two ways of criticizing this argument: one via a pure phenomenological approach to objectivity – an approach that will be rejected, the other via a formal axiomatic approach to understanding – an approach that will be advocated.

1.2 Weyl's Philosophical Portrait

Hermann Weyl (1885-1955) was a German mathematician and physicist, who took his PhD under David Hilbert in Göttingen in 1908, then pursued his career as a Professor in Zürich at the Swiss Federal Institute of Technology (1913-1930), in Göttingen as Hilbert's successor (1930-1933), and in Princeton at the Institute for Advanced Studies (1933-1950). He is now recognized as one of the greatest and "probably the most influential mathematician of the twentieth century," with fundamental contributions to most branches of mathematics.[2] Weyl made important contributions to theoretical physics as well. He further developed Einstein's theory of general relativity and articulated the application of group theory to quantum

---

[2]Cf. Borel 1986 (20), Penrose 1986 (144), Atiyah 1978 (2), Atiyah 2003 (3).

physics.[3] His "gauge-invariance" principle, in particular, has had a great impact on the development of contemporary physics.[4]

Weyl was also active as a philosopher. Early in his career, he defended predicativism and was attracted to phenomenological ideas. During one phase of his foundational thinking, he also defended a version of intuitionism, but soon came to have reservations. He departed from Husserl's phenomenology, and became an ally of Hilbert, only to see formalism, too, challenged by Gödel's incompleteness results. Weyl ended up by suggesting a rather unsettled philosophical position with respect to foundational matters.

This, at any rate, is how his philosophical work is generally portrayed.[5] To be sure, emphasizing certain stages in the evolution of Weyl's philosophical thinking has an undeniable expository value. In an autobiographical presentation, he too noted:

> I grew up a stern Cantorian dogmatist. Of Russell I had hardly heard when I broke away from Cantor's paradise; trained in a classical gymnasium, I could read Greek but not English. During a short vacation spent together, I fell under the spell of Brouwer's personality and ideas and became an apostle of his intuitionism. Then followed Hilbert's heroic attempt, through a consistent formalisation 'die Grundlagenfragen einfürallemal aus der Welt zu schaffen,' and then Gödel's great discoveries. Move and countermove. No final solution is in sight.[6]

One cannot deny that, in a sense, Weyl was an unsettled philosopher. But it is perhaps worth emphasizing that one could learn something important about his philosophical thinking, as well as about the topics that preoccupied him, by

---

[3]Cf. Mackie 1988 (125), Speiser 1988 (175).

[4]Cf. Yang 1986 (224), Mielke and Hehl 1988 (134), Ryckman 2003 (158).

[5]Cf. Mancosu 1998 (128), Bell 2003 (11). See also Pesic 2009 (146), Wilczek 2009 (221).

[6]Manuscript Hs91a:17. Draft of a Princeton talk, delivered in December 1946.

focusing on continuities, rather than on discontinuities, in his work. The analysis of Weyl's foundational writings shows that, despite the foundational moves and countermoves that engaged him, there are philosophical ideas that pervade his work from the earliest to the latest publications.

One could point out, for instance, that Weyl never actually relinquished his predicativism, for he seems to have never changed his mind that this is the only secure reconstruction of real analysis, acceptable in all honesty. Also, he never really forwent his intuitionist advocacy, for he always maintained that constructive mathematical proofs ought to be preferred to "transcendental" or "existential" ones, i.e., to proofs that deploy unbounded existential quantification over infinite domains. Phenomenology does not seem to have been entirely dismissed, either, since Weyl always claimed that to properly believe that, and to understand why, a proposition is true requires evidence, of the sort articulated and defended in Husserl's philosophy.

The main challenge for an analysis emphasizing such continuities in Weyl's thought is his conversion to Hilbert's formalism. The conversion to formalism is suggested by the following argument, presented by Weyl to the mathematical seminar at the University of Hamburg, in 1927:

> If Hilbert's view prevails over intuitionism, which in all appearance is the case, *then I see in this a decisive defeat of the philosophical position of pure phenomenology*, which thus proves to be insufficient for the understanding of creative science even in the area of cognition that is most primal and most readily open to evidence – mathematics.[7]

---

[7] "Setzt sich die HILBERTsche Auffassung, wie das allem Anschein nach der Fall ist, gegenüber dem Intuitionismus durch, *so erblicke ich darin eine entscheidende Niederlage der philosophischen Einstellung reiner Phänomenologie*, die damit schon auf dem primitivsten und der Evidenz noch am ehesten geöffneten Erkenntnisgebiet, in der Mathematik, sich als unzureichend für das Verständnis schöpferischer Wissenschaft erweist." (Weyl 1928a, 149 (200))

A careful analysis of this argument, an analysis that would help us draw Weyl's philosophical portrait more accurately, has to clarify his reasons for thinking that, in the late 1920s, formalism had prevailed over intuitionism, as well as his reasons for maintaining that this victory entails a defeat of pure phenomenology. This clarification requires that the central elements (i.e., the notions of evidence and scientific creativity) of Weyl's assessment of these foundational views be carefully explained. This will allow us to see why he came to the view that objectivity and intelligibility are opposite epistemic ideals of science.

But one may immediately wonder whether Weyl had any substantial reasons for thinking that formalism prevailed over intuitionism or whether he expressed, rather, a mere hypothesis regarding what he thought would become the dominant view in the foundations of mathematics. If the latter, then clearly Weyl's argument would be less than persuasive against a certain dismissive attitude of which Oskar Becker had made him aware:

> It seems to me almost certain that in the public opinion of the mathematicians Hilbert, or presumably a semi-renewal of the old "existential absolutism," will prevail. In general, I would not think very much of this public opinion, which always prefers mediocrity.[8]

At a first glance, what seems to have provided substantial reasons for Weyl's thinking that Hilbert won the dispute with intuitionism is the belief, shared by many in the late 1920s, that a proof of the consistency of arithmetic was, or could be, found. In a 1929 paper, Weyl wrote that this is "a concrete mathematical problem which is not trivial, but at the same time is solvable," and he added that,

---

[8] "Daß in der öffentlichen Meinung der Mathematiker Hilbert, oder vermutlich irgend eine halbe Erneuerung des alten "Existentialabsolutismus" siegen wird, scheint mir beinahe gewiss. Ich würde an sich auf diese öffentliche Meinung nichts geben, die ja immer nur das Mittelmässige bevorzugt." Manuscript Hs91:475, published in Mancosu and Ryckman 2002 (130). Eng. tr. in Mancosu 2002, 141 (129).

in the case of classical analysis, the situation "remains serious, but not entirely hopeless."[9] One might, accordingly, criticize Weyl's argument by pointing out, with hindsight, that the claim that Hilbert's formalism prevailed over intuitionism is simply false, for Gödel's second incompleteness theorem shows precisely that even weak fragments of arithmetic cannot prove their own consistency.

Against this criticism stand, of course, the various attempts to show that (at least a revised form of) Hilbert's foundational program can overcome Gödel's challenge. As is well known, one such attempt proposed an extension of the type of reasoning that may be used in a consistency proof, beyond the limits envisaged by Hilbert.[10] Another attempt argues, on the basis of certain epistemic criteria, for a restriction of the mathematics that needs to be secured via a consistency proof.[11]

But Weyl's reasons for his reconciliation with Hilbert are different than the ones motivating such attempts. Indeed, as he came to suspect that formalized mathematics is a mere game with symbols on paper and, thus, incapable of bringing about understanding, Weyl denied that a consistency proof would be enough to vindicate formalism. Furthermore, in the face of Gödel's challenge, he argued that a consistency proof is not required and that the formalist commitment to finding one is unjustified. As we will see, his reconciliation with Hilbert can be explained by paying attention to Weyl's view about the role of mathematics in physics. It is this view that led him to believe that objectivity and intelligibility are fundamentally antagonistic epistemic ideals of science; or so we want to argue here.

---

[9]Cf. Weyl 1929, 170 (202).

[10]Cf. Gentzen 1936 (73), 1938 (74).

[11]Cf. Detlefsen 1986 (38). Yet another attempt is represented by the so-called "reverse mathematics" program. See Simpson 1999 (169).

1.3 An Outline of the Main Argument

The argument proceeds as follows. We start, in chapter two, with an analysis of Weyl's view about the conditions for belief and understanding in mathematics. In particular, we are interested in clarifying his criticism of an idea defended by Dedekind, according to whom, in science, one should not believe a provable proposition without proof, even if that proposition is intuitively evident. He believed that although evidence may be the proper basis for believing an unprovable proposition, it is never the proper basis for believing a *provable* proposition. Weyl criticized this view by maintaining that evidence is the most proper basis for belief and by arguing that if a provable proposition is evident, then any attempt to prove it is useless.

To clarify Weyl's criticism, we first discuss Bolzano's view, according to which, understanding why a theorem is true requires an objective ordering of truths, one based on proof, rather than on intuitive evidence. This position is then contrasted with Schopenhauer's view, according to which, understanding why a theorem is true requires an ordering of truths based on intuitive evidence, rather than proof. By criticizing the view defended by Dedekind, Weyl seems to have taken a position similar to Schopenhauer's. But to support this position Weyl adopted, as we will see, Husserl's conception that evidence is an experience (*Erlebnis*) of truth. An experience of truth, unlike a feeling (*Gefühl*) of inescapable certainty that a proposition is true, is thought to be an epistemic achievement of the highest type, characterized by the complete satisfaction of the meaning intentions expressed in a proposition through the presentation in intuition of the intended objects. We argue that it is this phenomenological conception of evidence that allowed Weyl to allege that if a provable proposition is evident, then to properly believe that

proposition one is not required to prove it. For, on this conception, if one has an experience of truth, then one has an unexcelled basis for belief.

But did Weyl believe that an experience of truth is also the proper basis for understanding why a theorem is true? If so, wherein resides the ability of such an experience to bring about understanding? This question leads to an analysis of the relation between evidence and understanding. We will see that, despite Weyl's endorsement of the view that mathematical objects are creations of the mind, he rejected the idea that evidence, conceived of as a feeling caused by one's creatorly experiences, is sufficient for understanding why a theorem is true. He seems to have come to believe, on the one hand, that a feeling of evidence cannot show *why* a theorem is true, since, as Husserl pointed out, it cannot even indicate *that* the theorem is true. On the other hand, Weyl seems to have believed that an experience of truth is indispensable for understanding, provided that such an experience is admitted as a form of proof. An experience of truth may, however, be only admitted as a proof that follows the method of what Weyl called "immanent" axiomatics (as opposed to what he called "transcendent" axiomatics).[12] In immanent axiomatics, as we will see, mathematical reasoning is wholly contentual and proceeds from axioms formulated in terms of general concepts obtained by abstraction from what is immediately presented in intuition. We argue that, according to Weyl, understanding requires an immanent axiomatic proof, i.e., a proof that provides a construction of the objects referred to in a theorem, and thereby completely satisfies the meaning intentions expressed in the theorem.

Weyl's view on mathematical understanding raises an important question

---

[12]Weyl's actual term is "transcendental" axiomatics, but in order to better emphasize the opposition to "immanent" and to avoid confusion with the philosophically established meaning of "transcendental," we use "transcendent" axiomatics throughout the dissertation.

about the scientific relevance of large parts of modern mathematics. He maintained, for instance, that proofs that follow the method of transcendent axiomatics – proofs that include transfinite components, such as unbounded existential quantification over infinite domains – fail to bring about understanding: not only can they not show why a theorem is true, they cannot even indicate that it is true. In transcendent axiomatics, concepts are obtained not by abstraction from what is presented in intuition, but freely created by the mind, and introduced as symbols through the positing of the axioms under certain constraints, like consistency. The use of such concepts, Weyl contended, entails that a transcendent axiomatic proof cannot be seen as an experience of truth, since part of its reasoning – the part involving transfinite components – is non-contentual or purely symbolic.

In chapter three, we reveal the source of Weylean skepticism, i.e., of the tension that Weyl saw between the conditions required for understanding and the conditions required for objectivity, by clarifying why he maintained that the latter cannot be met without purely symbolic reasoning. We focus, first, on Weyl's critical reflections on traditional empiricist approaches to modern science, in particular on Hobbes' constructivism. Weyl seems to have thought that any such approach is marred by reliance on empiricist abstraction, for this method does not allow the introduction of hypothetical elements, i.e., real but in principle unobservable entities. This is why, on his view, traditional empiricism collapses into phenomenalism, of the sort defended in Leibniz' monadology, which, according to Weyl, may account at most for intersubjectivity.

Afterwards, we discuss Weyl's reflections on Fichte's constructivism. Fichte had rejected abstraction, in a reply to Kant, as a simply non-sensical method of concept formation, and emphasized the freedom of the mind to create concepts in-

dependently of perceptual experience. As we will see, this is why Weyl was quite enthusiastic about Fichte's view gradually developed in the *Wissenschaftslehre*. But this view, as Weyl also pointed out, fails to provide a correct account of the connection between freely created concepts and our experience, for it claims that what can be known through perceptual experience coincides with what is freely created by the mind and, thus, known independently of perceptual experience. This entails that the *Wissenschaftslehre* actually embraces monadological phenomenalism and, thus, according to Weyl, may offer only an inadequate account of objectivity.

Finally, we present Husserl's criticism of traditional empiricist abstraction, according to which this fails to distinguish between an intending act of the mind and an intended object. He argued that general concepts are introduced by abstraction from the relations between intending acts, rather than from the properties of intended objects. This method of concept formation – the so-called "ideational" abstraction – was used by Weyl, early in his career, for a development of Einstein's general relativity via a purely infinitesimal geometry. This development requires, as we point out, the elimination of concepts abstracted from the relations between intending acts that cannot be completely satisfied through the intuitive presentation of the intended objects (e.g., the concept of Riemannian congruence). This elimination seems to have been justified by the fact that no scientific proposition formulated in terms of such concepts could allow an experience of truth.

Weyl appears to have remained deeply committed to pure phenomenology, as he later argued that a proposition is objective only if one knows that it remains true when relativized to different coordinate systems. For, if one takes a coordinate system as representing the perspective of transcendental subjectivity, as

Weyl did, then a proposition is objective only if one knows that it is true for any transcendental subjectivity. Since, according to Husserl, experiencing the truth of a proposition is enough for knowing that the proposition is true for any transcendental subjectivity, Weyl's view seems to have been that a proposition is objective only if one can experience its truth. However, we argue that he ultimately rejected this view, for he came to believe that the experience of truth cannot be a necessary condition for objectivity, since it entails the elimination of hypothetical elements. According to him, as we will see, the assumption of real but in principle unobservable entities is indispensable for objectivity, as the only means for overcoming phenomenalism. Thus, on what we take to be Weyl's ultimate view, Husserl's phenomenology collapses into phenomenalism and may, therefore, just like Hobbes' and Fichte's constructivisms, support at most intersubjectivity.

Objectivity, Weyl came to believe, requires that scientific concepts (like force, energy, electromagnetic field, etc.) be freely created and introduced as mere symbols through the positing of fundamental theoretical principles. This is, we submit, the main belief that supports Weyl's Hamburg argument quoted above. As we already noted, however, Weyl made a strong claim that purely symbolic reasoning, as in Hilbert's transcendent axiomatics, fails to bring about understanding. Nevertheless, as we discuss in chapter four, Weyl also took the view that purely symbolic reasoning should not, although it might, be considered a mere game with symbols. For he maintained that mathematical concepts (including transfinite components) partake in the theoretical construction of the mind-independent world, and they do so in the same way as the scientific concepts (including hypothetical elements) do. In other words, Weyl's view seems to have been that, although it cannot have any *explanatory* role, the method of transcendent axiomatics has an *objectifying*

role: it is indispensable for scientific objectivity, i.e., for the formation of theories that may provide knowledge about the mind-independent world.

However, a scientific theory, according to Weyl, may provide objective knowledge only if it is categorical, i.e., only if the connection between a theory and its domain of application is univocal up to isomorphism, where the criterion for univocality is expressed by the notion of concordance – the idea that all methods for determining the value of a physical quantity must lead to the same result (within the limits of experimental error). This view on objectivity entails a fundamental limitation of scientific knowledge, in the sense that only the relations between mind-independent objects, and not these objects themselves, may be epistemically accessible through natural science. This limitation indicates again the tension that Weyl saw between scientific objectivity and intelligibility, since he took the latter, as we emphasized already, to require that objects be immediately presented in intuition. But his view on objectivity seems, all by itself, rather implausible, given the mathematical-logical context of the 1920s and the 1930s, when, for various reasons (the Löwenheim-Skolem theorem, Gödel's first incompleteness theorem), one came to believe that most first-order theories cannot be categorical. In fact, Weyl himself realized that current scientific theories fail to satisfy the condition of categoricity, and ultimately adopted a cautious attitude with regard to scientific objectivity.

A significant challenge that seems to have determined Weyl to adopt this position is raised by what can be taken as a failure of categoricity in quantum physics. This failure is indicated, as we will explain in detail, by the existence of unitarily inequivalent representations in quantum physics (e.g., in quantum mechanical statistics and quantum field theory). Recently, however, several responses have

been offered to this challenge. One response advocates the adjustment of these theories through an elimination of unitarily inequivalent representations, which is presumed to regain unitary equivalence. But we argue that this adjustment encounters serious difficulties. In particular, it seems to render theories unable to account for natural phenomena that they were designed to account for. Another response proposes that instead of eliminating unitarily inequivalent representations, one should focus on the structure defined by the relations between the unitarily inequivalent representations that describe physically possible worlds. We argue that this response can support objectivity only if a criterion is found for selecting the representations that describe physically possible worlds from those representations that do not. If such a criterion is given, one is thereby committed to the idea that the real world is modally dappled, in the sense that its structure spans a whole range of physically possible worlds.

In the fifth and final chapter, we first summarize the argument supporting Weylean skepticism, i.e., the view that objectivity and intelligibility are fundamentally antagonistic ideals of science. Then we focus on the connection between free creation of concepts and scientific understanding, a connection that Weyl seems to have denied. Our aim, admittedly of a rather programmatic character, is to show that the tension that he saw between objectivity and intelligibility can be dissolved, by arguing that partly non-contentual or purely symbolic reasoning, i.e., reasoning through scientific idealization, can render natural phenomena understandable.

One condition that we claim is required for understanding is that idealization be controlled via a proof of concordance, i.e., that one prove by contentual reasoning that the outcome of reasoning through idealization does not deviate from

14

actual observations more than by what may be due to experimental error. But this claim faces at least two criticisms. First, one might deny that a concordance proof has any serious epistemic significance. In the face of the provability limitations suggested by Gödel's second incompleteness theorem, Weyl actually contended that the formalist commitment to proving logical consistency is unjustified, and suggested that the mathematical logician should merely attempt to restore consistency if a contradiction comes up, just like the physicist, who attempts to restore concordance only when this is found to be lacking. This contention indicates, however, that Weyl failed to see the epistemic significance of proving consistency and concordance. Without such proofs, the reliability of purely symbolic reasoning, as well as its ability to bring about understanding, would be lost. Secondly, one might claim that even if a concordance proof could be taken to control idealization, this would be insufficient for understanding. But we point out that a further requirement for understanding is that reasoning through idealization be simpler, i.e., more thought-economical, than reasoning without idealization, and argue that if this further requirement is satisfied, then one is justified to deploy idealization in bringing about understanding if one can adequately justify as well the belief that science strives to bring about understanding in the most efficient way.

Our final conclusion is that Weylean skepticism is untenable, that it is reasonable, rather than just a "miracle creed," to expect that scientific theories can provide an objective and intelligible account of natural phenomena, in the sense indicated at the outset. As far as we are able to see, there is no undissolvable tension between the conditions required for scientific objectivity and the conditions required for scientific understanding.

Before we begin, we should note – and this is something to which many of Weyl's readers can no doubt testify – that his writing style does not always serve the clear presentation of philosophical ideas. Hans Hahn was perhaps the first who deplored the occasional obscurity of Weyl's philosophical writing. In his review of *Philosophy of Mathematics and Natural Science*, Hahn noted for instance that "one certainly feels sometimes, in the middle of these often extremely poetical expressions, a mild nostalgia for the neat symbols of logical calculus."[13] This kind of reaction is entirely understandable. For it is far from clear how, e.g., turning our attention to the "life of the mind," as Weyl recommended, would help clarify anything about twentieth century physics and mathematics.[14] Hahn moved, however, from nostalgia to mistrust: "Most of what is discussed here belongs to ... what cannot be said in dry formulas, but only in a beautiful style."[15] Indeed, sometimes one cannot but wish that Weyl had traded (at least some) style for clarity. But we hope that the present dissertation succeeds in illuminating some of the obscure aspects of his philosophical portrait.

---

[13] "Freilich empfindet man vielleicht manchesmal inmitten dieser oft poetisch schwunghaften Sprache ein leises Heimweh nach der schlichten Symbolik des Logikkalküls." (Hahn 1928, 54f (78))

[14] Part of what leads to obscurity is, perhaps, Weyl's drawing on philosophical views that philosophers of science would just too often overlook today. But the English translations of his works (even where approved by Weyl himself, unfortunately) often enhance the obscurity. This is the reason we have sometimes chosen to amend the existing translations or to simply offer new ones.

[15] "Das meiste, wovon hier die Rede ist, gehört ja zu dem, was ... nur in schönem Stile, aber nicht in trockenen Formeln sagbar ist." (Hahn 1928, 55 (78))

CHAPTER 2

EVIDENCE, BELIEF, AND UNDERSTANDING

2.1   Introduction

In this chapter, we analyze Weyl's view on belief and understanding in mathematics. In particular, we are interested in the relation between Weyl's injunction against Dedekind's principle that, in science, one should not believe a provable proposition without proof, and Weyl's rejection of the maker's knowledge view of mathematical understanding, according to which one understands a theorem because one has a feeling of evidence caused by one's having produced the objects that the theorem refers to. We aim to show that Weyl's position, on both issues, is supported by a Husserlian conception of evidence.

Dedekind emphasized, as we will see, that despite the suggestion to the contrary often made in mathematical education in the nineteenth century, intuitive evidence is not a proper basis for believing a provable proposition, and he argued that proof, and only proof, could be such a basis. Like Dedekind, Bolzano too argued that evidence is not a proper basis for believing a provable proposition. According to him, the proper basis for believing a provable proposition is proof, because this is the only means for revealing the objective ordering of truths. By revealing the objective ordering of truths, proof brings about at least one definite epistemic gain: it does not merely indicate *that* a provable proposition is true, but

17

it helps us understand *why* it is true. In opposition to this view, Schopenhauer argued that mathematical proof could indeed help one understand why a theorem is true, rather than merely indicate that it is true, but only if proof reveals an ordering of truths based on intuitive evidence.

Like Schopenhauer, Weyl seems to have believed that understanding obtains only if proof reveals an ordering of truths based on intuitive evidence. Unlike Schopenhauer, however, Weyl endorsed the conception of evidence defended by Husserl in his *Logical Investigations*. This suggests that, like Husserl, Weyl too rejected the view that evidence is a "feeling" (*Gefühl*), a psychological state of compulsion to believe that a proposition is true. Husserl argued that evidence is an epistemic achievement characterized as an "experience" (*Erlebnis*) of truth. It is this type of experience, we submit, that Weyl thought was the proper basis for believing a provable proposition. According to him, then, and against what Bolzano and Dedekind seem to have maintained, it is useless to prove provable propositions the truth of which can be experienced, for provability cannot render an experience of truth an improper basis for belief. But we should note that Weyl's view does not entail that it is also useless to prove provable propositions the truth of which falls short of experience.

This raises a question about the connection between understanding and the experience of truth. In order to answer this question, we first clarify why Weyl considered that the notion of understanding developed within the maker's knowledge tradition, as based on a characteristic feeling of evidence caused by one's creatorly experiences, is not sufficient to show why a mathematical theorem is true. Following Husserl's criticism of the view of evidence as a feeling, Weyl seems to have believed that such a feeling is a merely psychological state, one

that lacks epistemological significance. Afterwards, we argue that, on his view, evidence, conceived of *à la* Husserl as an experience of truth, is the proper basis for understanding why a theorem is true provided that the experience of truth is admitted as a form of proof. But an experience of truth can only be admitted as a proof that follows the method of what Weyl called "immanent" axiomatics, i.e., a proof in which reasoning is wholly contentual and general concepts are introduced by abstraction from intuition. This view on understanding raises a question about the epistemic significance of what Weyl called "transcendent" axiomatics, which we further discuss in chapter four. It also raises a question about his view on the nature of abstraction, as a method for introducing general concepts, which we discuss in chapter three. Finally, as we will explain, it indicates a tension between what he took to be the conditions required for understanding and the conditions required for objectivity – a tension that, as we already suggested in chapter one, is the source of *Weylean skepticism.*

2.2 Weyl on Dedekind on Believing Provable Propositions

In 1918, in his book on the foundations of real analysis, *The Continuum*, Weyl announced a radical project:

> It is not the purpose of this work to cover the "firm rock" on which the house of analysis is founded with a fake wooden structure of formalism – a structure which can fool the reader and, ultimately, the author into believing that it is the true foundation. Rather, I shall show that this house is to a large degree built on sand. I believe that I can replace this shifting foundation with pillars of enduring strength. They will not, however, support everything which today is generally considered to be

securely grounded. I give up the rest, since I see no other possibility.[1]

Weyl attempted to show that a justification of Dedekind's definition of the real numbers as sets of rational numbers is possible only if certain restrictions are imposed on what one admits as a definition. The version of analysis that emerged starts from a basic category of mathematical objects – the natural numbers – as immediately given in intuition, and extends this category by what Weyl called the "mathematical process." The fundamental feature of this process is that it forbids impredicative definitions, i.e., definitions that make reference to sets of objects in which the defined object is an element. This restriction entails, as Weyl acknowledged, that some theorems of classical analysis, like the least upper bound theorem for sets of real numbers, cannot be proved within his system of analysis.[2]

In a small footnote in the book, Weyl also recorded his disagreement with Dedekind's standard for believing what is mathematically provable:

> In the Preface to the first edition of Dedekind's famous *Was sind und was sollen die Zahlen?*, we read that "In science, what is provable ought not to be believed without proof." This remark is certainly characteristic of the way most mathematicians think. Nevertheless, it is a perverse principle. As if such a mediate(d) concatenation of grounds as what we call a "proof," can awaken any "belief" without our assuring ourselves, through immediate insight, of the correctness of each individual step! This (and not the proof) remains throughout the ultimate source of knowledge; it is the "experience of truth." Whoever

---

[1] "In dieser Schrift handelt es sich nicht darum, den "sicheren Fels", auf den das Haus der Analysis gegründet ist, im Sinne des Formalismus mit einem hölzernen Schaugerüst zu umkleiden und nun dem Leser und am Ende sich selber weiszumachen: dies sei das eigentliche Fundament. Hier wird vielmehr die Meinung vertreten, daß jenes Haus zu einem wesentlichen Teil auf Sand gebaut ist. Ich glaube, diesen schwankenden Grund durch Stützen von zuverlässiger Festigkeit ersetzen zu können; doch tragen sie nicht alles, was man heute allgemein für gesichert hält; den Rest gebe ich preis, weil ich keine andere Möglichkeit sehe." (Weyl 1918a, iii; Eng. tr., 1. (191))

[2] Cf. Weyl 1918a, 23; Eng. tr., 31f (191). For a detailed presentation and development of Weyl's predicativism, see Feferman 1988 (51), Feferman 1997 (52).

approaches other disciplines, such as philosophy, in the manner of a mathematician, demanding mathematical definitions and deductions, proceeds no more sagaciously than a zoologist who rejects numbers on the ground that they are not living beings.[3]

Although often quoted in the literature, this remark of Weyl's has not really been given the philosophical attention it deserves. Some authors refer to it in order to emphasize the general importance of immediate insight or intuition in Weyl's overall thinking.[4] Others think that the footnote reveals, more specifically, his early adoption of a phenomenological approach to science and his criticism of formal axiomatic approaches.[5] Still others believe that it reveals Weyl's adoption of a conception of evidence as a feeling of inescapable certainty, inspired by Fichte.[6] Nevertheless, we consider that Weyl's remark and the reasoning behind it have yet to be properly clarified.

What we should immediately note about this remark is that it makes two claims at the same time. First, it criticizes the extension of the mathematical standard for belief (or at least, the standard endorsed, according to Weyl, by

---

[3] ""Was beweisbar ist, soll in der Wissenschaft nicht ohne Beweis geglaubt werden", beginnt die berühmte Dedekindsche Schrift 'Was sind und was sollen die Zahlen?' (Vorwort zur 1. Auflage) Diese Äußerung ist gewiß charakteristisch für die Denkweise der meisten Mathematiker, dennoch ist das ein verkehrtes Prinzip. Als ob ein solcher mittelbarer Begründungszusammenhang, wie wir ihn als "Beweis" bezeichnen, irgend "Glauben" zu wecken imstande ist, ohne daß wir uns der Richtigkeit jedes einzelnes Schrittes in unmittelbarer Einsicht versichern! Diese (und nicht der Beweis) bleibt überall letzte Rechtsquelle der Erkenntnis, sie ist das "Erlebnis der Wahrheit." Wer als Mathematiker an andere Wissenschaften, etwa an die Philosophie, mit der Forderung nach Definitionen und Deduktionen mathematischen Stils herantritt, handelt nicht klüger, als wenn ein Zoologe die Zahlen ablehnte, weil sie keine lebenden Wesen sind." (Weyl 1918a, 11; Eng. tr., 119 (191).)

[4] Cf., e.g., Bell 2000 (10).

[5] Cf. Ryckman 1995, 834f (157). See also Ryckman 2005, ch. 5 (159). da Silva 1997 (34), which is focused on the relationship between Husserl's phenomenology and Weyl's predicativism does not mention the footnote. In Folina 2008, 38 (65), it is stated that Weyl's remark shows that his view on proof is "reminiscent of Descartes for whom the reason a proof is justificatory has to do with the intuitive certainty, or evidence, of each step." As we will see below, however, Weyl's notion of evidence is not Cartesian, but rather Husserlian.

[6] Cf. Sieroka 2010, 175 (168). As we show below, given Husserl's criticism of the view of evidence as a feeling, Weyl seems to have had reasons to reject this view, too.

"most mathematicians") to non-scientific disciplines. Secondly, it also criticizes the application of this standard to scientific disciplines, to mathematics itself. Weyl's argument in support of the latter claim, directed against Dedekind, appears to go as follows: mathematical proof can be a genuine source of knowledge only if it "awakens" belief in the theorem that it proves. To awaken belief is, as Weyl quite cryptically put it, to have an "experience" of truth. Thus, proof can be a genuine source of knowledge only if it provides an experience of truth. This seems to imply that the epistemic character of an experience of truth is such that, if one has an experience of the truth of a provable proposition, then one already has a proper basis for believing that proposition. Therefore, proving a provable proposition the truth of which can be experienced is useless. Contrary to what Dedekind seems to have suggested, it is misleading, even perverse, to claim that one ought not to believe a provable proposition without proof.

What is, or might have been, the support for this argument? In particular, why did Weyl think that an experience of truth, rather than proof, is the proper basis for believing a provable proposition? Why did he believe, as against most mathematicians, that provability could not render this type of experience an improper basis for belief? And did he also think that an experience of truth is also a proper basis for mathematical understanding? Most basically, perhaps, what *is* an experience of truth?

Answering these questions requires an extensive investigation of the various sources that influenced Weyl's view on proof, evidence, and mathematical understanding. As we will see below, Husserl's conception of evidence, as presented in the *Logical Investigations*, seems to have had a major influence on this view. Understanding Husserl's conception helps us indeed to answer some of the ques-

22

tions just raised. In particular, it helps us to clarify Weyl's argument against Dedekind's standard for scientific belief, but also his rejection of a maker's knowledge approach to mathematical understanding. But before we look more closely at Husserl's conception of evidence, however, we want to present in more detail Dedekind's view, and to also recall a view quite similar to it, defended by Bolzano, who believed that if the probation of evident propositions is not found to be impossible, in a certain sense, then it is obligatory.

2.3 Bolzano and Dedekind on Believing Provable Propositions

Dedekind's principle, that "In science, what is provable ought not to be believed without proof" was initially intended as a motto to his 1888 book on arithmetic, *Was sind und was sollen die Zahlen?*.[7] At the very least, this suggests that the principle was thought to have a central significance for his logicist program in the foundations of mathematics. According to this principle, the scientific standard for belief is considered to be different from ordinary standards, in the following way: one has a proper basis for believing that a provable proposition is true only after the proposition has been proved. Dedekind did not deny that one may believe a provable proposition without proof, as one surely does, but only that believing it without proof may be considered adequate as a scientific standard for belief.

Dedekind's principle can be understood as a call to carefully separate what is, from what is not, provable. For, the application of the proposed standard for belief requires that one provide an adequate description of the epistemic conditions that would allow one to realize that a proposition is unprovable, i.e., that it is, as he

---

[7]See Dedekind's letter to H. Weber from November 19, 1878; Dedekind 1932, 486 (37).

put it, "a pure law of thought." It is, of course, difficult to give a compelling delineation of the class of such propositions. As Frege duly noted, Dedekind himself offered no "inventory of the logical or other laws taken by him as basic."[8]

Aristotle, let us also recall, suggested that a certain type of education might help one overcome this difficulty: "It is impossible for anything at the same time to be and not to be. ... This is the most indisputable of all principles. Some indeed demand that even this shall be demonstrated, but this they do through want of education, for not to know of what things one should demand demonstration, and of what one should not, argues want of education."[9] However, whereas Aristotle thought that education is needed to guard against those who want to prove too much, Dedekind considered that the nineteenth century methods of teaching elementary mathematics encouraged one to leave too much without proof:

> So from the time of birth we are continually and in increasing measure led to relate things to things ... this exercise goes on continually, though without definite purpose, in our earliest years; the accompanying formation of judgments and chains of reasoning leads us to a store of real arithmetical truths to which our first teachers later refer as to something simple, self-evident, and given in inner intuition.[10]

Dedekind blamed such teaching methods for encouraging one to consider as simple, self-evident, and immediately presented to the mind, and thus to believe without proof, that which is only the result of judgment and reasoning. On his view,

---

[8]Cf. Frege 1893 (68), viii.

[9]Cf. *Metaphysics*, Book IV, ch 4.

[10]"So sind wir auch schon von unserer Geburt an beständig und in immer steigendem Maße veranlaßt, Dinge auf Dinge zu beziehen ... durch diese schon in unsere ersten Lebensjahre fallende unablässige, wenn auch absichtslose Übung und die damit verbundene Bildung von Urtheilen und Schlußreihen erwerben wir uns auch einen Schatz von eigentlich arithmetischen Wahrheiten, auf welche später unsere ersten Lehrer sich wie auf etwas Einfaches, Selbstverständliches, in der inneren Anschauung Gegebenes berufen." (Dedekind 1888, (36) Eng. tr. in Ewald 1996, 792 (50))

considering arithmetical truths in this way conceals the "chains of reasoning" behind them. Proper belief requires, on the contrary, that these chains be fully revealed. This was, according to him, the task of mathematical proof.[11]

Before Dedekind, Bolzano defended similar views. He argued, for instance, that certainty and conviction are not to be mistaken for the aim of science:

> The purpose of a scientific exposition is *usually* imagined to be the greatest possible *certainty* and *strength of conviction.* It therefore happens that the obligation to prove propositions which, in themselves, are already completely certain, is discounted. This is a procedure which, where we are concerned with the practical purpose of certainty, is quite correct and praiseworthy; but it cannot possibly be tolerated in a scientific exposition, because it contradicts its essential aim.[12]

This passage raises a question about what exactly Bolzano took the purpose of a scientific exposition to be. But it seems clear that, for him, certainty and strength of conviction are not the highest epistemic virtues to be pursued in a scientific exposition. Rather, as we will presently see, those virtues are only provided by proof. What proof aims at, according to Bolzano, is to reveal the objective ordering of truths. This purpose was taken to characterize not only the proper exposition of science, but its very nature:

> The nature of all science lies, in our view, in the presentation of truths as they are objectively connected, and the fact that their proofs are designed to indicate the connection of this truth, considered in and for

---

[11]Dedekind seems to have further believed that "the ability of the mind to relate things to things, to let a thing correspond to a thing, or to represent a thing by a thing, an ability without which no thinking is possible," i.e., the very ability that, when exercised enough, leads to arithmetical truths, is something that needs to be given immediately to the mind, in inner intuition. But he does not seem to have offered, or to have been interested in offering, a more definite view about the epistemic conditions that characterize this ability. For further discussion of Dedekind's view, see Detlefsen 2011 (39).

[12]Cf. Bolzano 1810a, Eng. tr., 103 (16).

itself, with other truths, rather than to provide certainty.[13]

The objective ordering relation among truths was understood as a grounding relation, and was thus meant to reflect an idea that underlies the classical conception of scientific knowledge, as presented by Aristotle: "We suppose ourselves to possess unqualified scientific knowledge of a thing, as opposed to knowing it in the accidental way in which the sophist knows, when we think that we know the cause on which the fact depends, as the cause of that fact and of no other, and, further, that the fact could not be other than it is."[14] In other words, genuine scientific knowledge requires that one understand *why* a fact is what it is, and not only show *that* the fact is so. According to Bolzano, understanding why a fact is what it is requires that one reveal the objective grounding of truths, where the notion of objective grounding is illustrated by the following example:

> The two truths that the three angles of a triangle are always equal to two right angles, and that every quadrangle can be divided into two triangles whose combined angles form the angles of the quadrangle, these two truths form the ground of the truth that the four angles of every quadrangle are equal to four right angles.[15]

It is the objective grounding of truths, as here illustrated, that mathematical proof is designed to reveal. On Bolzano's view, instrumental for proof's success in doing so is the following rule:

---

[13] "Da wir das Wesen aller Wissenschaft darin finden, daß sie die Wahrheiten nach ihrem objektiven Zusammenhange darstellt, daß ihre Beweise – statt des Zwecks Gewißheit zu bewirken, nur den haben, den Zusammnehang anzugeben, in welchem diese Wahrheit an u. für sich betrachtet mit andern stehet." (Bolzano 1810b, 16 (17))

[14] Cf. *Posterior Analytics*, I, 2.

[15] Cf. Bolzano 1837, 245 (19). See Tatzel 2002 (177) and Lapointe 2008 (118), for a detailed analysis of Bolzano's notion of grounding. We should perhaps note that a distinction between grounds, causes, and epistemic reasons, similar to Bolzano's (which is analyzed by Tatzel and Lapointe), was defended also by Schopenhauer in 1813. More on this below.

> I propose for myself the rule that the *evidentness of a proposition* does not free me from the obligation to continue searching for a proof of it, at least until I clearly realize that absolutely no proof could ever be required, and why.[16]

This rule entails that the epistemic conditions that allow one to realize that a proposition does not require probation are not characterized solely by the possession of evidence. On the basis of evidence, it is implied, one would be able to reach merely a subjective ordering of truths. On Bolzano's view, the epistemic conditions which would justify one in judging that a proposition does not require proof are conditions under which it is proper to judge that it *cannot* require proof.

But under what conditions is it proper to judge that a proposition cannot require proof? What kind of evidence, if any, is needed to identify those propositions that cannot require proof? To answer this question, let us note that, on Bolzano's view, mathematics is concerned with transcendental truth:

> Mathematics could be best defined as a *science that deals with the general laws (forms) to which things must conform in their existence.* ... This indicates that our science is concerned not with the proof of the *existence* of these things but only with the *conditions of their possibility.*[17]

These general laws, Bolzano added, are "either so general that they are applicable to *all things completely without exception*, or not. The former laws, put together and ordered scientifically, will accordingly constitute the *first* main part of mathematics. It can be called *general mathesis*; everything else is then *particular mathesis.*"[18] Thus, it seems plausible to believe that Bolzano took the epistemic conditions that would justify one in judging that a mathematical proposition could

---

[16]Cf. Bolzano 1804, 31 (15).

[17]Cf. Bolzano 1810a, Eng. tr., 94 (16).

[18]Cf. Bolzano 1810a, Eng. tr., 95 (16).

not require proof to be the conditions determined by a proposition's transcendental status as a law of general mathesis. But what are, more specifically, these latter conditions?

Besides generality, one other requirement for being a law of general mathesis appears to be simplicity: "the strictly unprovable propositions, or axioms, are only to be sought in the class of those judgements in which both subject and predicate are completely simple concepts."[19] But whatever conditions might be required for determining whether a general concept is simple or not, it is clear that generality and simplicity are not enough to justify one in judging that one has successfully identified the laws of general mathesis. For this also requires that the connection between subject and predicate in the statement of such a putative law be groundless. If the connection is not groundless, then it requires another, more basic law to express its ground. But Bolzano rejected the idea that this ground can reside in the subject itself, or in the predicate, of the statement, or in their connection. He thought that this was just a roundabout way of saying that a general law is groundless, and also derided the idea that a thing can be its own ground as based on sheer ignorance: "if one does not know whether a certain thing M is a ground or a consequence, one assumes it is a consequence, searches for its ground, and finds that that is M."[20] Consequently, Bolzano believed that one should reject the principle of sufficient reason as a law of general mathesis, since such a principle fails to have general applicability: "I maintain that there is no *principle* of sufficient reason, i.e., that it cannot be determined generally (and a priori) what has, and what does not have, a ground. Rather, whether something

---

[19]Cf. Bolzano 1810a, Eng. tr., 205 (16). We come back to the notion of simplicity of axioms, and its epistemic relevance as a necessary condition for understanding, in section 5.5 below.

[20]"Wenn man nicht weiß ob eine gewisse Sache M erster Grund oder Folge sei, so nimmt man an, sie sey Folge, sucht ihren Grund, und findet, daß es M sei." (Bolzano 1810b, 25 (17))

has a ground or not is decided only by considering particular cases."[21]

Bolzano also rejected the Kantian principles of the understanding as laws of general mathesis, since their valid application is restricted to objects that can be given in experience, rather than extended, as the laws of general mathesis should be, to "everything which can in general be an object of our capacity for representation," that is, everything that can be an object of thought, whether individuals or general concepts.[22] These principles, just like the principle of sufficient reason, fail to meet the generality condition.[23] This is important to keep in mind as we turn below to Schopenhauer's argument against the need to prove the intuitively evident. For, as we will see, although he similarly believed that mathematics should be concerned exclusively with transcendental truths, Schopenhauer's conception of transcendentality was more restricted than Bolzano's.

To sum up, the central claims of the view defended by Bolzano and Dedekind with regard to believing what is provable are the following. In science, evidence alone is not a proper basis for believing a provable proposition because it conceals the reasoning behind that proposition, and so it prevents one from understanding why the proposition is true. In order to be properly believed, a provable propo-

---

[21]"Ich halte dafür, daß es gar kein *Princip* des Grundes gebe, d.h. daß es sich gar nicht allgemein (u. a priori) ausmachen lasse, was einen Grund habe, u. was keine habe; sondern es ist erst aus Betrachtung des besonderen Falles entschieden worden, ob etwas einen Grund hat oder nicht." (Bolzano 1810b, 26 (17)) Compare this with Schopenhauer's view: "Alle Wissenschaften nämlich beruhen auf dem Satze vom Grunde, indem sie durchweg Verknüpfungen von Gründen und Folgen sind." (Cf. Schopenhauer 1813, 46 (163).) See also section 4.4 below for Helmholtz' and Weyl's appreciation of the fundamental role of the principle of sufficient reason in science.

[22]Cf. Bolzano 1810a, Eng. tr., 94 (16).

[23]But what are, one might ask, Bolzano's laws of general mathesis? Unlike Dedekind, he did provide an inventory (even if only an avowedly incomplete one), as well as a discussion of these laws. Leaving an analysis of that discussion for another occasion, let us just mention the fundamental law of *Zusammendenkbarkeit*, that any thing can be adjoined in thought to another thing (cf. Bolzano 1810b, 33ff (17)). This law recalls, of course, Dedekind's talk of a basic ability of the mind to relate things to things (see footnote 11 on page 25 above). Nevertheless, it would be mistaken to simply equate these two views.

sition should not be believed merely on the basis of its evidentness. Rather, it should be believed on the basis of proof from unprovable propositions, for only such a proof can reveal the reasoning behind a provable proposition and can help one understand why that proposition is true. Thus, one lacks a proper basis for believing a provable proposition if one has not provided a proof of that proposition based, ultimately, on the pure laws of thought (or, as Bolzano maintained, on the laws of what he called general mathesis).

Before we discuss Weyl's argument against this view, we want to turn to Schopenhauer's earlier injunction against the need to prove what is intuitively evident in mathematics. This offers some more historical background that, as we will see, helps us to better grasp Weyl's own view.

2.4   Schopenhauer on Proving Evident Propositions

To present Schopenhauer's rejection of any demand to prove intuitively evident mathematical propositions, we need to recall Kant's famous idea that purely logical reasoning is unable to lead, all by itself, to mathematical knowledge.[24] In mathematics, one always needs to make appeal to nonconceptual resources, like those of pure intuition.[25] Schopenhauer embraced this idea and claimed that

---

[24]It might be also worth noting that positions similar to Schopenhauer's had been long before him defended by Roger Bacon and Locke. For Bacon, see especially part VI of his *Opus Majus*, 'De Scientia Experimentali' (Bacon 1268, vol. II, 167ff; Eng. tr. 288 (5)). Locke writes in the same vein in part II, 3, and IV, 17, of his *Essay* (122). In the German tradition, a few years before Schopenhauer, Jacobi was the philosopher who first rejected, in 1787, the attempt to prove intuitively evident propositions, on the ground that this leads to nihilism. For discussion, see Franks 2005, 162ff (66).

[25]There has been a long-standing problem, of course, regarding Kant's view on the nature and locus of such appeal, i.e., whether it is only in the presuppositions from which theorems may be derived by means of formal logic, or also in the methods of reasoning employed in mathematical derivations. For a review of the problem, see Shin 1997 (167). As we will show, Schopenhauer's view is that intuition has to be the basis for *both* the axioms *and* the methods of proof in mathematics.

mathematics is never merely a matter of analyzing and judging relationships between concepts. His criticism of the Euclidean method of proof proceeds in the following way:

> [Mathematics] is ... at great pains deliberately to reject the intuitive evidence peculiar to it and everywhere at hand in order to substitute for it a logical one. ... [But] it is only insight into the ground of being which bestows true satisfaction and thorough knowledge, while the mere ground of knowledge always remains on the surface, and can give us cognition *that* it is so, but not *why* it is so. Euclid chose this latter way to the obvious detriment of the science. ... In our view, this method of Euclid in mathematics can appear only as a very brilliant piece of perversity. ... Euclid's logical method for treating mathematics is a useless precaution, a crutch for sound legs. ... We need not and should not leave the peculiar province of mathematics in order to trust merely logical certainty and verify mathematics in a province entirely foreign to it, namely in the province of concepts.[26]

According to Schopenhauer, genuine scientific knowledge can be obtained in geometry by, and only by, explaining *why* an object has a certain property, rather than by merely confirming *that* the object has that property. Thus, geometry can be a genuine science only if its theorems are rendered intuitively evident, since only on the basis of intuitive evidence can one actually show *why* a certain object has a certain property. Intuitive evidence is believed to possess the greatest explanatory power because, as we will see presently, only pure intuition may reveal

---

[26] "[Die Mathematik] ... ist mit grosser Mühe bestrebt, die ihr eigenthümliche, überall nahe, anschauliche Evidenz muthwillig zu verwerfen, um ihr eine logische zu substituiren. ... [Aber] die Einsicht in den [Grunde des Seyns] allein wahre Befriedigung und gründliche Kenntnis gewährt, während der blosse Erkenntnisgrund stets auf der Oberfläche bleibt, und zwar ein Wissen, DASS es so ist, aber keines, WARUM es so ist, geben kann. Eukleides ging diesen letztern Weg, zum offenbaren Nachtheil der Wissenschaft. ... In unseren Augen kann jene Methode des Eukleides in der Mathematik dennoch nur als eine sehr glänzende Verkehrtheit erscheinen. ... Eukleides logische Behandlungsart der Mathematik eine unnütze Vorsicht, eine Krücke für gesunde Beine ist. ... Wir brauchen und dürfen also nicht, um bloss [der logischen Gewissheit] zu trauern, das eigenthmliche Gebiet der Mathematik verlassen, um sie auf einem ihr ganz fremden, dem der Begriffe, zu beglaubigen." (Schopenhauer 1819, 15 (164))

what Schopenhauer called "grounds of being."

By contrast, Euclidean geometry fails to be genuinely scientific, insofar as it justifies mathematical theorems on the basis of conceptual evidence, which, on Schopenhauer's view, has no explanatory power, since it may reveal only what he called "grounds of knowledge," which may only show *that* an object has a certain property. This view can be clarified by paying attention to Schopenhauer's reading of Aristotle.

As noted above, Aristotle famously maintained that scientific knowledge is knowledge of causes. He further added that knowledge is given by means of inferences from given premises, and therefore "the premises must be the causes of the conclusion."[27] But a common criticism, echoed by Schopenhauer, says that Aristotle failed to distinguish clearly between causes and epistemic reasons. By Aristotle's own standards, Schopenhauer added, an argument from what one claims are, but which in fact are not, causes, is a mere sophism. He rejected the Aristotelian view that the definition of a mathematical object, i.e., the description of the nature or essence of the object, is a type of cause, and proposed a threefold distinction between causes, "grounds of knowledge," and "grounds of being." Here is an illustration of this distinction:

> If I ask: Why are the three sides of this triangle equal? The answer is: because the three angles are equal. Now, is the equality of the angles the *cause* of the equality of the sides? No, for here we are not talking of any change, of any effect that must have a cause. – Is it merely a ground of knowledge? No, for the equality of the angles is not merely a proof of the equality of the sides, it is not merely the ground of a judgment: from mere conceptions alone one could not see that because the angles are equal, also the sides must be equal: for the concept of the equality of the sides is not contained in that of the equality of

---

[27]Cf. *Posterior Analytics*, I, 2.

32

the angles. Here, thus, we have no connection between concepts, or judgments, but between sides and angles.[28]

Grounds of being are, thus, to be sharply distinguished from causes, but also from grounds of knowledge. To say that x is a ground of being for y is to say that y cannot be unless x also is. For example, angle equality in a triangle is the ground of being for side equality if and only if the sides of the triangle cannot be equal unless the angles are equal. Clearly, then, grounds of being are distinct from causes, as side equality is not an effect of angle equality. But grounds of being are, on Schopenhauer's view, also distinct from grounds of knowledge. To say that x is a ground of knowledge for y is to say that the concept of y is contained in the concept of x (and, consequently, that judgments about y are logically derivable from judgments about x). For example, the equality of a triangle's angles is a ground of knowledge for the equality of its sides if and only if the concept of side equality is contained in the concept of angle equality, and thus, our judgment about side equality is logically derivable from the judgment about angle equality.

Based on this threefold distinction, Schopenhauer argued that geometry can become a genuine science only if proof is explanatory, i.e., only if it reveals the grounds of being of a theorem, for only such grounds could justify its acceptance as a transcendental truth:

In [Euclidean] Geometry, it is only in the axioms that one actually

---

[28] "Wenn ich frage: Warum sind in diesem Triangel die drei Seiten gleich? So ist die Antwort: weil die drei Winkel gleich sind. Ist nun die Gleichheit der Winkel URSACH der Gleichheit der Seiten? Nein, denn hier ist von keiner Veränderung, also von keiner Wirkung die eine Ursach haben müßte, die Rede. – Ist sie bloß Erkenntnißgrund? Nein, denn die Gleichheit der Winkel ist nicht bloß Beweis der Gleichheit der Seiten, nicht bloß Grund eines Urtheils: aus bloßen Begriffen ist ja nimmermehr einzusehn, daß, weil die Winkel gleich sind, auch die Seiten gleich seyn müssen: denn im Begriff von Gleichheit der Winkel liegt nicht der von Gleichheit der Seiten. Es ist hier also keine Verbindung zwischen Begriffen, oder Urtheilen, sondern zwischen Seiten und Winkeln." (Schopenhauer 1813, 15 (163))

appeals to intuition. All the other theorems are demonstrated, that is to say, a ground of knowledge is indicated, the truth of which everyone is bound to acknowledge: the logical truth of the theorem is thus shown, but not its transcendental truth. The latter lies in the ground of being and not in the ground of knowledge, and never can become evident except by means of intuition.[29]

Hence, geometry can become a genuine science only if appeal to intuition is made not only in the axioms, but also in mathematical reasoning. For otherwise the axioms cannot be revealed as the grounds of being of a theorem, and so this cannot be justifiably accepted as a transcendental truth. Schopenhauer took a mathematical proposition to be justifiably accepted as a transcendental truth only if it is based on Kantian pure intuitions of space and time as necessary conditions for the possibility of all objects of knowledge.[30] Geometry can, thus, become a genuine science only if proof is constructive, i.e., if it provides the intuitive construction of an object that possesses the properties attributed to it in the theorem, from the objects referred to, and the properties attributed to them, in the axioms.

If this is the right way to look at Schopenhauer's view, then his criticism that Euclidean geometry substitutes a ground of knowledge to a ground of being points out that Euclidean proof is not sufficiently constructive:

After such a geometrical demonstration [i.e., one that follows Euclid's method], one has the conviction that the theorem which has been demonstrated is true, but has no insight into *why* that which it asserts

---

[29] "Auf die Anschauung beruft man also in der Geometrie sich eigentlich nur bei den Axiomen. Alle übrigen Lehrsätze werden demonstrirt, d.h. man giebt einen Erkenntnißgrund des Lehrsatzes an, welcher Jeden zwingt denselben als wahr anzunehmen: also man weist die logische, nicht die transzendentale Wahrheit des Lehrsatzes nach. Dieser aber, welche im Grund des Seyns und nicht in dem des Erkennens liegt, leuchtet nie ein, als nur mittelst der Anschauung." (Schopenhauer 1813, 39 (163))

[30] Cf. Schopenhauer 1813, 32 (163).

is as it is. . . . A proof by indicating the ground of knowledge effects mere conviction (*convictio*), not insight (*cognition*): therefore it might perhaps be more correctly called *elenchus* than *demonstratio*."[31]

Schopenhauer believed that geometry was in need of radical reform, one motivated by the need to show that *all* theorems are intuitively evident. Hence, the reform would require an elimination of Euclidean proof:

> The stilted logical proof, which is always foreign to the matter, is generally soon forgotten without detriment to conviction, and could be dispensed with entirely, without diminishing the evidence of geometry, which is entirely independent of proof, which always proves only what we are already through another kind of knowledge fully convinced of. To this extent it is like a cowardly soldier who gives another wound to an enemy killed by someone else, and then boasts that he himself killed him. ... One hopes that there will be no doubt that the evidence of mathematics, which has become the pattern and symbol of all evidence, rests essentially not on proofs, but on immediate intuition. Here, as everywhere, that is the ultimate ground and source of all truth.[32]

One immediate problem with this view regards, of course, the extension of its validity. For one can hardly see how one may seriously believe that Schopenhauer's view holds beyond the simple theorems of elementary plane geometry. But he

---

[31] "Man [hat] nach so einer geometrischen Demonstration zwar die Überzeugung, daß der demonstrirte Satz wahr sei, aber keineswegs einsieht, warum was er behauptet so ist, wie es ist. . . . Der Beweis durch Aufweisung des Erkenntnißgrundes wirkt bloß Überführung (*convictio*), nicht Einsicht (*cognitio*): es wäre deswegen vielleicht richtiger *elenchus*, als *demonstratio* zu nennen." (Schopenhauer 1813, 39 (163))

[32] "Keineswegs ist es der auf Stelzen einherschreitende logische Beweis, welcher, der Sache immer fremd, meistens bald vergessen wird, ohne Nachtheil der Überzeugung, und ganz wegfallen könnte, ohne daß die Evidenz der Geometrie dadurch vermindert würde, da sie ganz unabhängig vom ihm ist und er immer nur das beweist, wovon man schon vorher, durch eine andere Erkenntnissart, völlige Überzeugung hat: insofern gleicht er einem feigen Soldaten, der dem von andern erschlagenen Feinde noch eine Wunde versetzt, und sich dann rühmt, ihn erlegt zu haben. ... Diesem allen zufolge wird es hoffentlich keinem Zweifel weiter unterliegen, dass die Evidenz der Mathematik, welche zum Musterbild und Symbol aller Evidenz geworden ist, ihrem Wesen nach nicht auf Beweisen, sondern auf unmittelbarer Anschauung beruht, welche also hier, wie überall, der letzte Grund und die Quelle aller Wahrheit ist." (Schopenhauer 1819, 15 (164))

maintained, though without further argument, that the whole extent of Euclidean geometry can be thus reformed: "The ground of being is certainly not as evident in all cases as it is in simple theorems . . . Still I am persuaded that it might be brought to evidence in every theorem, however complicated."[33] Furthermore, Schopenhauer also seems to have believed that this is the right view about arithmetical knowledge. Although he did not say much about arithmetic, his view on arithmetical proof did not differ essentially from his view on geometrical proof. Thus, for example, he maintained that the successor relation must be regarded as a ground of being relation, and that only this may form the basis of genuine arithmetical knowledge.[34] In any case, it is hard to see how, on Schopenhauer's view, one can avoid the conclusion that most of modern mathematics is unscientific. And still, as we will see below, Weyl – one of the most influential mathematicians of the twentieth century, may be seen to have held a similar view.

To conclude our discussion up to this point, we should say that, both Schopenhauer and Bolzano seem to have endorsed the idea that, in science, mathematical proof needs to explain why a theorem is true, rather than to merely confirm that this is so. Thus, both Schopenhauer and Bolzano agreed that mathematical proof must, in some sense(s), be explanatory. But they seem to have disagreed about the conditions under which explanation can be achieved. Schopenhauer thought that explanation required elimination of conceptual proof and maximization of intuitive evidence. Bolzano believed that explanation requires, on the contrary, minimization (rather than elimination) of intuitive evidence, and maximization of conceptual proof.

---

[33]Cf. Schopenhauer 1813, 39 (163).

[34] "Jede Zahl setzt die vorhergehenden als Gründe ihre Seyns voraus: zur Zehn kann ich nur gelangen durch alle vorhergehenden, und bloß vermöge dieser Einsicht in den Seynsgrund weiß ich, daß wo Zehn sind, auch Acht, Sechs, Vier sind." (Schopenhauer 1813, 38 (163))

The difference between these two views appears to be ultimately due to distinct conceptions of the notion of transcendentality. Like Kant, Schopenhauer regarded as transcendental any proposition that expresses necessary conditions for the possibility of all objects of knowledge. A mathematical proof is, on his view, explanatory only if it justifies the acceptance of a theorem as a transcendental truth, in this sense. Unlike Kant, Bolzano regarded as transcendental any proposition that expresses necessary conditions for the possibility of all things whatsoever. A mathematical proof is, on his view, explanatory only if it justifies the acceptance of a theorem as a transcendental truth, in this other sense.

The question that we must address now is why did a mathematician like Weyl think that it is useless, even perverse, to prove provable propositions that are intuitively evident. One could argue that he justified this claim in a way that is reminiscent of Schopenhauer. Indeed, there can be no doubt that a cultivated mathematician like Weyl read Schopenhauer, especially as the latter became widely read in the second part of the nineteenth century, and his work still exerted influence at the beginning of the twentieth century.[35] As an indication that Weyl took Schopenhauer's view of mathematics seriously, one may point to his endorsement of Schopenhauer's claim that every number, as an individual, is the ground of being for its immediate successor.[36] Furthermore, Weyl could not have been ignorant of Schopenhauer's resounding attack on the Euclidean method of proof, which was often recalled even after the turn of the century.[37]

---

[35] Cf., e.g., Howard 1997 (97) for Schopenhauer's influence on Einstein.

[36] Cf. Weyl 1927a, 28 (198); Eng. tr., 34 (211).

[37] For instance, in 1902, Meinong wrote the following: "The older epistemology placed 'intuitive' knowledge ahead of the 'demonstrative.' ... One remembers the complaint, so often repeated since Schopenhauer, concerning how Euclidean geometry just forces its propositions upon our conviction, as it were, without leading one to a proper insight into the demonstrated state of affairs." (cf. Meinong 1902, 173; Eng. tr. 127 (133)) Such complaints about Euclidean geometry had been voiced, of course, before Schopenhauer, by Hobbes, Bolzano, and others.

However, even if Schopenhauer's views did have a deep echo in his mind, Weyl could not have overlooked the nineteenth century developments in mathematics (such as non-Euclidean geometries and the arithmetization of analysis), which showed that the Kantian intuitions of space and time are an improper basis for scientific belief. Indeed, rather than embracing the Kantian notion of intuition, Weyl endorsed, as we will see below, the view defended by Husserl in his *Logical Investigations*.

2.5 Husserl on Truth and Evidence as an Experience of Truth

It would not be possible, of course, to present here an exhaustive account of Husserl's conception of evidence. For one thing, his view was in constant development throughout his career, and it is extremely difficult to trace all its changes and apparently endless variations.[38] Nevertheless, insofar as this conception had any influence on Weyl, it is its elaboration in the 1900 *Logical Investigations* – where Husserl argued that evidence (*Evidenz*) is an experience (*Erlebnis*) of truth, rather than a feeling (*Gefühl*) – that we should focus on. For it is precisely this conception of evidence that Weyl was deeply impressed with, as his correspondence with Husserl indicates:

> Despite all the faults you attribute to the Logical Investigations from your present standpoint, I find the conclusive results of this work, which has rendered such an enormous service to the spirit of pure objectivity in epistemology – the *decisive insights about evidence and truth*, the recognition that "intuition" extends far beyond sensuous

---

[38]For a succinct account of this development, see Rosen 1977 (154).

38

intuition – established with great clarity and concision.[39]

In this section, we want to present what Weyl took to be Husserl's "decisive insights about evidence and truth" developed in the *Logical Investigations*, and clarify their role in Weyl's argument against Dedekind's standard for scientific belief. To begin with, let us note that, in the work just mentioned, Husserl emphatically maintained that evidence is fundamental for scientific knowledge:

> The most perfect mark of correctness is evidence; it counts as an immediate inner development of truth itself. ... Ultimately, all genuine, all scientific, knowledge rests on evidence, and as far as evidence extends, the concept of knowledge extends also.[40]

Questions about the nature of evidence as an "inner development" of truth arise here naturally. For Husserl also suggested, as we shall see below, that truth is *adequatio intellectus et rei*: a proposition is true if and only if there is a correspondence or agreement between its meaning and the state of affairs that the proposition refers to. Before we discuss in more detail Husserl's view on truth, and its relation to evidence, we should like to note that it seems natural to read the quotation above as a reiteration of Schopenhauer's exaltation of the crucial role played by intuitive evidence in science. Unlike Schopenhauer, however, Husserl does not argue against those who, like Bolzano, believed that certainty and conviction based on evidence are not the proper aim of science. Rather, Husserl argued

---

[39] "Trotz allem, was Sie etwa von Ihrem heutigen Standpunkt an den Logischen Untersuchungen auszusetzen haben, finde ich darin die Schlussresultate dieses Werk, das dem Geist reiner Sachlichkeit in der Erkenntnistheorie so ungeheure Dienste geleistet hat, *die entscheidenden Einsichten über Evidenz und Wahrheit*, die Erkenntnis, daß 'Anschauung' weit über das Sinnliche hinausreicht, mit größer Klarheit und Prägnanz aufgestellt." (Letter to Husserl, from 26 March 1921. Husserl 1994, 290 (108). Eng. tr. in Ryckman 2005, 113 (159), my emphasis.)

[40] "Das vollkommenste Kennzeichen der Richtigkeit ist die Evidenz, es gilt uns als unmittelbares Innewerden der Wahrheit selbst. ... Im letzten Grunde beruht also jede echte und speziell jede wissenschaftliche Erkenntnis auf Evidenz, und so weit die Evidenz reicht, so weit reicht auch der Begriff des Wissens." (Husserl 1900, 6 (102))

against those who, on the contrary, did believe that certainty and conviction based on evidence are the very aim of science, but nevertheless misunderstood the nature of evidence. His target, thus, is what he called empiricism, and its associated psychologistic account of evidence:

> Empiricism altogether misunderstands the relation between the ideal and the real: it likewise misunderstands the relation between truth and evidence. Evidence is no accessory feeling, either arbitrarily attached, or attached by natural necessity, to certain judgments.[41]

To understand the criticism that Husserl raised here against empiricism, that is, the view defended, according to him, by philosophers like Mill, Meinong, and Alois Höfler, we need to see how these philosophers, themselves, conceived of the relation between truth and evidence. We also need to examine how they defined the notion of feeling (*Gefühl*).

As one would expect, at the end of the nineteenth century and the beginning of the twentieth century, no universally accepted definition of feeling was available in the philosophical or psychological literature. However, a definition of feeling, which did perhaps pass muster on all accounts, was given by Rudolf Eisler in his famous *Dictionary of Philosophical Concepts*: "*Feeling* is a subjective state in which the ego takes position with regard to the modifications that he experiences, with regard to his experiences."[42] Thus, a feeling seems to have been understood as the result of one's psychological reaction to one's own experiences.

---

[41] "Wie der Empirismus überhaupt das Verhältnis zwischen Idealem und Realem im Denken verkennt, so auch das Verhältnis zwischen Wahrheit und Evidenz. Evidenz ist kein akzesorisches Gefühl, das sich zufällig oder naturgesetzlich an gewisse Urteile anschließt." (Husserl 1900, Prolegomena 51 (102))

[42] "Gefühl ist der subjektive Zustand, in welchem das Ich Stellung nimmt zu den Modifikationen, die es erfährt, zu seinen Erlebnissen." (Eisler 1904 (49))

The nature of the relation between an experience and the result of one's re-action to it was more carefully described by Höfler, in his 1903 *Introduction to Logic and Psychology.* He formulated a psychological law, according to which, every feeling has a psychological presupposition.[43] A psychological presupposition is a representation (in perception, memory, or imagination) or an act of judging, which appears to causally determine a certain feeling. According to Höfler's account of mental causation, the feelings caused by our acts of judging (*Akte des Urteilens*) may be called "feelings of knowledge" (*Wissensgefühle*). Among these, one can distinguish "logical feelings" (*logische Gefühle*) as those feelings that are caused by acts of judging like 2+1=1+2 or that $a^2 + b^2 = c^2$, where c represents the length of the hypotenuse, a and b the lengths of the other sides of a right triangle.[44] Just like one's subjective reaction to hearing a beautiful song is a sensible feeling of pleasure, and one's subjective reaction to burning one's hand is a sensible feeling of pain, one's subjective reaction to judging that 2+1=1+2 or that $a^2 + b^2 = c^2$ is a logical feeling of knowledge. Evidence, on Höfler's view, is a psychological state (*einen psychischen Zustand*) that can be experienced as a characteristic (*Merkmal*) of such acts of judging.

Now, what is the basis of the criticism that Husserl raised against a view like Höfler's? It is fair to say that, to a certain extent, Husserl misdescribed this view, when he said that, for the empiricist, evidence is a feeling. For, as we have just seen, although evidence is considered to be a psychological state, Höfler did not define it as a feeling, i.e., as a subjective reaction to one's experiences, but rather as a particular characteristic of certain acts of judging. But of course, one could insist that any psychological state is, in a certain sense, a feeling. Mill,

---

[43] "Jedes Gefühl hat eine psychologische Voraussetzung." (Höfler 1903, 291 (93))

[44] Cf. Höfler 1903, 313 (93). See also Höfler and Meinong 1890, 122 (94).

for instance, considered that "a feeling and a state of consciousness are, in the language of philosophy, equivalent expressions: everything is a feeling of which the mind is conscious. ... Feeling, in the proper sense of the term, is a genus, of which sensation, emotion, and thought, are subordinate species."[45] Evidence, too, would then have to be considered as a feeling.

At any rate, Husserl argued that, if one takes evidence to be a feeling, then one turns logic into a science concerned with the description of merely psychological phenomena. One does so because one fails to distinguish between the real and the ideal, that is, between the *real* acts of judging, which are dependent on the psychological profile of epistemic subjects, and the *ideal* content or meaning of judgments, which is independent of such subjects. The former are, indeed, the object of descriptive psychology, but only the latter should be the object of logic correctly conceived of as a normative science. On the empiricist view, however, the investigation of logical laws becomes an investigation of the psychological laws according to which acts of judging with evidence can lead to other acts of judging with evidence.

Furthermore, the failure to distinguish between real acts of judging and ideal meaning of judgments shows, according to Husserl, that the empiricist conception of the relation between evidence and truth is mistaken. More specifically, Husserl argued that empiricism cannot establish evidence as a criterion for truth: "How can we know that this feeling [of evidence] indicates the truth?"[46] The causal relation between an act of judging with evidence and the feeling that what is judged is true does not guarantee that what is judged is true. For, Husserl seems to have thought, this causal relation remains within the real domain of psychological

---

[45]Cf Mill 1843, 35f (135).

[46]"Woher wissen wir, dass dieses Gefühl die Wahrheit anzeigt?" (Husserl 1984, 154f (107))

states, whereas the proper conditions for truth do not belong to this domain.

In order to present Husserl's own account of evidence and of its relation to truth, and to understand why he believed that his account should be preferred to the empiricist view, let us briefly note that, before him, Brentano similarly argued against Sigwart, who had maintained that an evident judgment involves a feeling pertaining to the ideas judged upon:

> The peculiar nature of insight – the clarity and evidence of certain judgements which is inseparable from their truth – has little or nothing to do with a feeling of compulsion. ... no awareness of a compulsion to judge in a certain way could, as such, guarantee the truth of the judgement.[47]

Hence, Brentano seems to have believed that evidence can justify belief only if it is inseparable from truth. However, if evidence is understood as a feeling, as a psychological state, then it is, presumably, separable from truth and, therefore, cannot justify belief. But what does it mean to say that evidence, properly understood, is inseparable from truth? In a letter to Husserl, Brentano, himself, attempted to spell out the sense of this inseparability:

> Whoever really makes an evident judgement really *knows* and is certain of the truth; whoever really knows something with *immediate* evidence is *immediately* certain of the truth. This is unaffected by the fact that the one who judges came into being, is subject to causation, and is dependent upon the particular cerebral organization which we have. *To the one who judges with evidence, the truth is secured in itself, and not by reflection on such preconditions. ... Having the insight is sufficient to assure one that no one else can have a contrary insight. Not even God Almighty could provide one; for this very assumption*

---

[47] "Die Eigentümlichkeit der Einsicht, die Klarheit, Evidenz gewisser Urteile, von der ihre Wahrheit untrennbar ist, hat wenig oder nichts mit einem Gefühle der Nötigung zu tun. ... kein Bewußtsein einer Notwendigkeit, so zu urteilen, könnte als solches die Wahrheit sichern." (Cf. Brentano 1930, 63; Eng. tr., 54 (22))

would be absurd and inconsistent with the concept of evidence.[48]

Thus, Brentano seems to have believed that inseparability from truth is built into the very definition of the concept of evidence. In other words, he seems to have thought that evidence was logically sufficient for truth, that it is logically impossible to have evidence for a false proposition. By contrast, Husserl's account of the relation between truth and evidence should be seen as an attempt to show that this relation is neither a matter of psychology (as it was for Höfler) nor a matter of mere definition (as it was for Brentano). On Husserl's view, as we explain below, evidence is an instantiation of the universal idea of truth.

In the introduction to his *Logical Investigations*, the relationship between truth and evidence is articulated in the following way:

> Evidence is rather nothing but the "experience" of truth. Truth is of course only experienced in the sense in which an ideal can be an experience in a real act. In other words: *Truth is an idea, whose particular case is an actual experience in the evident judgment. ... The experience of the agreement* between meaning and what is meant and is itself present, between the actual *sense of an assertion* and the given *state of affairs*, is evidence, and the *idea* of this agreement is truth.[49]

---

[48] "Wer wahrhaft evident urteilt, wahrhaft *erkennt*, der ist der Wahrheit sicher; wer wahrhaft *unmittelbar* evident erkennt, *unmittelbar* der Wahrheit sicher. Und dies wird nicht dadurch beeinträchtigt, daß er als Urteilender angefangen hat, verursacht worden ist und im Besondern von unserer Gehirnorganization abhängig ist. *Dem evident Urteilenden ist die Wahrheit nicht in Reflexion auf solche Vorbedingungen, sondern in sich selbst gesichert. ... Genug, er hat die Einsicht, um auch zu wissen, daß kein anderer die entgegengesetzte haben kann. Selbst Gottes Allmacht könnte sie keinem geben; denn die Annahme wäre dem Begriff der Evidenz widerstreitend und absurd.*" (Cf. Brentano 1930, 156f; Eng. tr. 136f (22))

[49] "Evidenz ist vielmehr nichts anderes als das "Erlebnis" der Wahrheit. Erlebt ist die Wahrheit natürlich in keinem andern Sinne, als in welchem überhaupt ein Ideales im realen Akt Erlebnis sein kann. Mit anderen Worten: *Wahrheit ist eine Idee, deren Einzelfall im evidenten Urteil aktuelles Erlebnis ist. ... Das Erlebnis der Zusammenstimmung* zwischen der Meinung und dem selbst Gegenwärtigen, das sie meint, zwischen dem aktuellen *Sinn der Aussage* und dem selbst gegebenen *Sachverhalt* ist die Evidenz, und die *Idee* dieser Zusammenstimmung die Wahrheit." (Husserl 1900, Prolegomena 51 (102))

Why might Husserl have thought that the conception of evidence presented in this passage could show that the relation between truth and evidence was neither a matter of psychology, nor a matter of mere definition? This is what we need to clarify. More specifically, we need to explain why evidence, understood as an experience of truth, could serve as a criterion of truth.

One plausible suggestion is to take Husserl's idea of truth as a universal, more specifically, as a Platonic idea.[50] Hence, the idea of truth is related to a true proposition in the same way in which the idea of equality is related to two equal sticks. In other words, the agreement between the meaning of a proposition and the state of affairs meant by that proposition is an instantiation of the idea of truth, just like the agreement in length between the two sticks is an instantiation of the idea of equality. One might say, thus, that one has an "experience" of truth in the same way in which one has an "experience" of equality.

However, although truth represents the agreement between meaning and what is meant, *adequatio intellectus et rei*, on Husserl's view, it is not this agreement that is an instantiation of the idea of truth. Rather, he claimed that it is the *experience* of this agreement, rather than the agreement itself, that is an instantiation of the idea of truth. Analogously, although equality is a length agreement, it is not the agreement in length between two sticks, but the visual perception of this agreement, that would be an instantiation of the idea of equality. If this is the right way to understand Husserl's view, then, by contrast with both Brentano and Höfler, the relation between evidence and truth is one of instantiation.

It might seem obvious that, on Husserl's view, evidence can serve as a criterion of truth. That is, an experience of an agreement between the meaning of a propo-

---

[50]This suggestion was made by Günther Patzig: "One is speaking here of an idea in Plato's rather than Kant's sense, and so also of its particular cases." (Patzig 1973, 184 (143))

sition and what the proposition means seems to guarantee that the proposition is true. But we should ask what exactly is the relation between evidence and an evident proposition. From the fact that evidence is an instantiation of the idea of truth, it does not immediately follow that an evident proposition is true. For this to obtain, evidence, i.e., the experience of agreement, has to be a constitutive part of that proposition. It is not clear, however, how the experience of an agreement between meaning and object, can be a constitutive part of a proposition. One might say that such an experience is a sort of phenomenal quality of a proposition. But when may a proposition be said to possess this phenomenal quality?

One can find an answer to this question in Husserl's sixth *Logical Investigation*, where he developed the view according to which an experience of truth is to be understood as a complete satisfaction of a meaning intention, that is, as we explain below, as a certain type of epistemic achievement. Briefly put, what Husserl seems to have maintained is that a proposition may be said to possess evidence when the meaning intention expressed by that proposition is completely satisfied, i.e., when we have "set directly before us" the intended objects and states of affairs.

The first thing to note is that Husserl spoke of evidence in different senses, corresponding to different possible levels of satisfaction of a meaning intention.[51] Thus, evidence may be anything from an experience of partial agreement to an experience of complete agreement between the meaning of a proposition and the state of affairs that is meant by that proposition. Full agreement obtains when the intention expressed by a proposition is completely satisfied, i.e., when the intended state of affairs is entirely presented to the mind in intuition or perception.

---

[51]Cf. Husserl 1900, VI, 37 (102).

In this case, the notion of evidence is understood in a strict sense, as an experience of the identity between the object meant and the object presented in intuition. Partial agreement obtains when the intention expressed by a proposition is partially unsatisfied, i.e., when only part of the object or state of affairs meant is presented in intuition. When the intention expressed by a proposition remains entirely unsatisfied, i.e., when the intention is empty, that is, when the object meant is not given in intuition, then one says that a proposition lacks evidence.

One should further observe that this view implies that evidence, in the strict sense, may be achieved in stages, and presupposes a gradual ascent from empty to completely satisfied meaning intentions:

> The discussion of possible relationships of satisfaction points to *a goal in which increase of satisfaction terminates, in which the complete and entire intention has reached its satisfaction*, and not an intermediate and partial, but an *final and ultimate satisfaction.* ... Where a presentative intention has achieved its ultimate satisfaction, the genuine *adaequatio rei et intellectus* has been brought about.[52]

Hence, the difference between propositions that express merely empty or partially satisfied intentions and propositions that express completely satisfied intentions is epistemically significant. For, as this passage seems to suggest, only the latter may be said to be true. Thus, evidence as an experience of truth is, on Husserl's view, a criterion for truth, but *only if understood in a strict sense, i.e., as the achievement of complete satisfaction of meaning intentions.* If a proposition is evident in the sense that the intended objects and states of affairs are given in intuition exactly

---

[52] "So weist die Erwägung der möglichen Erfüllungsverhältnisse auf ein *abschließendes Ziel der Erfüllungssteigerung hin, in dem die volle und gesamte Intention ihre Erfüllung* und zwar nicht eine intermediäre und partielle, sondern *eine endgültige und letzte Erfüllung* erreicht hat. ... Und wo sich eine Vorstellungsintention ... letzte Erfüllung verschafft hat, da hat sich die echte *adaequatio rei et intellectus* hergestellt." (Cf. Husserl 1900, VI, 37 (102))

as they are meant, then one is justified to believe that the proposition is true.

Unlike Brentano, Husserl reasonably allowed for the possibility of error, that is, for judgments that are taken to be evident, but are in fact false. This may be explained by the failure, on the part of the epistemic subject, to realize that the goal of complete satisfaction of her meaning intentions has not been attained and, consequently, that the evidence for a certain proposition does not instantiate the idea of truth. Husserl's view also seems to allow for uninstantiated truths, that is, truths that we do not have an experience of. These are propositions that express empty or at least partially unsatisfied intentions.

How did Weyl apply this phenomenological conception of evidence, which he was avowedly much impressed with, to mathematics? Why might he have thought that this could refute Dedekind's view about what constitutes, in science, the proper basis for believing provable propositions?

2.6 The Experience of Truth and the Question about Understanding

According to Dedekind's principle, as we have seen above in section 2.3, in science, one ought not to believe a provable proposition without proof. For, as Bolzano had emphasized, proof and only proof can reveal the objective ordering of truths. To be believed in a proper way, a provable proposition must be ultimately derived from unprovable propositions in a way that reflects this objective ordering. By contrast, as we have seen in section 2.4, Schopenhauer argued that genuine scientific knowledge requires that the ordering revealed by proof be based on intuitive evidence. More exactly, it requires the construction in intuition of the objects referred to in a theorem from the objects referred to in the axioms.

Weyl's claim, presented in section 2.2, that if a provable proposition is evident,

then proving it is entirely useless, suggests that he adopted a largely Schopenhauerian position. For Weyl, too, believed that a proof could provide genuine scientific knowledge only if the ordering of truths revealed by that proof is based on intuitive evidence. But his notion of evidence could not have been framed by the Kantian intuitions of space and time. Given the historical development of modern mathematics, especially through the nineteenth century, he was aware that intuitive evidence, of the Kantian sort defended by Schopenhauer, is insufficient to justify mathematical belief. In 1913, in the preface to the first edition of his first book, *The Idea of Riemann Surfaces*, Weyl explained why proof should be valued in mathematics:

> It used to be common and, as far as I can tell, it remained common until today in all presentations of the theory of Riemann surfaces, to accept the representation of a *curve* as it appears to be given in our sensorial intuition, without conceptual fixation, and to make a naive use of those properties which impose themselves on us by this representation with a sort of intuitive evidence (e.g., of the proposition that a curve has two sides). But, there can be no doubt about this today, the "intuitive evidence" does not absolve us from the necessity to provide even for these truths *proofs*, which rest in the end on the axioms of arithmetic. Such proofs become necessary at least when those flowing intuitions extend (as it happens in the practice of mathematics as an exact science) to general abstract concepts and get, as it were, paralysed.[53]

[53] "Es war früher üblich und ist, soviel ich sehe, bis jetzt in allen Darstellungen der Theorie der Riemannschen Flächen üblich geblieben, die Vorstellung der *Kurve*, wie sie in unserer sinnlichen Anschauung gegeben vorzuliegen scheint, ohne begriffliche Fixierung herüberzunehmen und von denjenigen Eigenschaften, welche sich uns an dieser Vorstellung mit einer Art anschaulicher Evidenz aufdrängen (z.B. von dem Satz, dass eine Kurve zwei Ufer hat) einen naiven Gebrauch zu machen. Die "anschauliche Evidenz" enthebt uns aber, daran kann heute kein Zweifel mehr sein, keineswegs der Notwendigkeit, für eben diese Wahrheiten *Beweise* zu erbringen, die letzten Endes auf die Axiome der Arithmetik gestützt sind; zum mindesten werden solche Beweise nötig, sobald jene fließenden Anschauungen sich (wie es das Verfahren der Mathematik als exakter Wissenschaft mit sich bringt) zu allgemeinen abstrakten Begriffen ausgeweitet haben und in ihnen gleichsam erstarrt sind." (Weyl 1913, III (190))

The "paralysis" of geometrical intuition was induced, of course, by the introduction in analysis of mathematical concepts like, e.g., continuous but nowhere differentiable functions. The young Weyl realized that the introduction of such concepts indicates that intuitive evidence does not absolve the mathematician of the responsibility of trying to prove evident propositions. His own project in the book mentioned was to attempt an arithmetization of the theory of Riemann surfaces, in the spirit of the earlier arithmetization of analysis. Undoubtedly, he believed that such attempts are extremely valuable, in that they help to properly justify propositions that are believed on the basis of intuitive evidence, like the intermediate value theorem or the Jordan curve theorem, thus contributing to the development of mathematical knowledge.[54]

Soon thereafter, however, Weyl came to understand the notion of evidence in a phenomenological sense, as an experience of truth. This is indicated, as we emphasized above, in his correspondence with Husserl, as well as by the phenomenological idiom used in his own publications. Thus, Weyl seems to have come to believe that mathematical proof can be a genuine source of knowledge only if it allows an experience of truth, which seems to imply that if one already has an experience of the truth of a provable proposition, then believing that proposition does not require proof. This led him to make the remark that Dedekind's principle that, in science, one ought not to believe a provable proposition without proof, is a mere epistemological perversity.

We are now in a position to explain, more exactly than before, what Weyl believed could justify this remark. The adoption of a Husserlian conception of evidence as an experience of truth suggests that Weyl's rejection of Dedekind's

---

[54]For a recent discussion of rigorous proofs of the Jordan curve theorem, see Hales 2007 (79).

principle was supported in the following way: if one experiences the truth of a provable proposition, then this completely satisfies the meaning intentions expressed by that proposition, by setting directly before one's eyes the objects and states of affairs that the proposition refers to. This epistemic achievement was taken by Weyl to be one of the highest sort, one that provides the most proper basis for believing a provable proposition, or at least a basis as proper as the typical basis for believing unprovable propositions. Therefore, on Weyl's view, proving a provable proposition that is evident, in Husserl's sense, is useless, since no mathematical proof can evidentially surpass an experience of truth, as a basis for belief. Quite the contrary, Weyl appears to have believed that a proof is of the highest epistemic quality only when it reaches the epistemic achievement characteristic of an experience of truth.

Furthermore, in *The Continuum*, Weyl noted that mathematical proof, as usually understood, employs propositions that lack evidence, and takes inferential steps that are not supported by evidence:

> The presentation of the fact that a judgment U is a consequence of the axioms can and must be done ... through a generally ramified organism of 'elementary' inferences, which in order to be communicated must be artificially transformed in a chain of interlocking links. It is in this way that mathematical *proof* comes about; in it, all insight that is to be satisfied is concentrated on the logical inferences and is no longer directed at the objects and states of affairs judged upon.[55]

Given Weyl's advocacy of the phenomenological conception of evidence, this obser-

---

[55] "Die Aufzeigung der Tatsache, daß ein Urteil U Folge der Axiome ist, kann und muß ... durch einen im allgemeinen vielverzweigten Organismus "elementarer" Schlüße geschehen, der dann noch zum Zwecke der Mitteilung künstlicherweise in eine Glied an Glied schließende Kette umgewandelt werden muß. So kommt der mathematische *Beweis* zustande; alle zu vollziehende Einsicht konzentriert sich in ihm auf die logischen Schlüße und ist nicht mehr auf die beurteilten Sachen und Sachverhalte gerichtet." (Weyl 1918a, 11; Eng. tr., 17f (191))

vation should be taken to say that in a mathematical proof, as usually understood, one's meaning intentions remain empty or unsatisfied. This is due to one's failure to set directly before oneself the objects and states of affairs the proof refers to. But, according to Weyl, this entails that mathematical proof, as usually understood, cannot be a genuine source of knowledge. This is why Weyl recommended that mathematicians should search for mathematical "organisms," i.e., for proofs that do not obscure, but reveal, intuitive evidence. Again, given his advocacy of the phenomenological conception of evidence, this recommendation should be taken to say that mathematicians should search for proofs that are capable of providing an experience of truth by completely satisfying the prover's meaning intentions. To completely satisfy the prover's meaning intentions, Weyl seems to have believed that mathematicians should provide constructive proofs, i.e., proofs that set directly before us the objects and states of affairs referred to by the axioms and construct, on this basis, the objects and states of affairs referred to by a theorem.[56]

Husserl was, unsurprisingly, extremely satisfied with Weyl's phenomenological standpoint.[57] But many others considered the phenomenological conception of evidence as untenable. Schlick, for example, criticized quite harshly this conception and rejected the claim that an experience of evidence (*Evidenzerlebnis*) can be "the sufficient criterion and unmistakable indicator of truth":[58]

---

[56]As an aside, it may be interesting to note that Weyl's view was shared by physicist Erwin Schrödinger, his colleague at the ETH in Zürich, who wrote: "Man muss die Physiologie und Biologie und womöglich die Phylogenie eines mathematischen Apparates verstehen, dann hat man etwas davon." (Letter to Weyl, Nov 6, 1929) This remark is important in the context of Schrödinger's development of his wave version of quantum mechanics, and his charge against the unintelligibility of Heisenberg's matrix version.

[57]See Husserl's letter to Weyl from April 10, 1918 (van Dalen 1984, 3 (183)).

[58]"das ausreichende Kriterium und untrügliche Kennzeichen der Wahrheit" (cf. Schlick 1918, 129 (161))

[Evidence] represents nothing more than a word for the requirement to stop doubting at this point. By means of this word, misgivings are defeated, not reconciled. ... We have repeatedly rejected the invocation of evidence as the highest authority and last resort as perverse and inappropriate.[59]

More pointedly, perhaps, Schlick maintained that even if one conceived of evidence as an experience of truth, rather than as a feeling, one would still make the fundamental mistake of considering "truth and the mark of truth ... as something that pertains to the individual judgment itself, without considering other judgments and the real states of affairs."[60]

In the remainder of this chapter, we want to bring under attention an epistemological question that Weyl's phenomenological conception of evidence appears to have left unanswered. The question is motivated by the fact that Weyl appears to have believed that an experience of truth is not only an unexcelled basis for believing that a theorem is true, but should also be taken as the proper basis for explaining why a theorem is true. So the question is what characteristic(s) of the experience of truth might have led him to believe this? More generally, in what sense could an experience of truth render a mathematical proof explanatorily deeper than a proof which does not seem to permit such an experience?

Weyl was definitely aware of the importance of providing explanatory proofs, i.e., proofs that help one understand why a theorem is true. This is shown, for example, in a 1932 paper:

---

[59] "[Evidenz] stellt ... nichts dar als ein Wort für die Forderung, an diesem Punkte mit dem Zweifel Halt zu machen. Durch dies Wort werden Bedenken niedergeschlagen, nicht versöhnt. ... Wir haben die Anrufung der Evidenz als höchste Instanz und letzte Zuflucht wiederholt als verkehrt und untunlich abgelehnt." (cf. Schlick 1918, 129 (161))

[60] "die Wahrheit und das Kennzeichen der Wahrheit gedacht werden als etwas am einzelnen Urteil selber Haftendes, ohne Rücksicht auf andere Urteile und auf Wirklichkeiten." (Cf. Schlick 1918, 129 (161)). For a detailed analysis of Schlick's theory of truth, see Howard 1992, 200-205 (96). For Weyl's reaction to Schlick's theory, see Weyl 1918b, 59f (192).

> We are not content to get convicted, as it were, rather than convinced of a mathematical theorem by a long chain of formal inferences and calculations leading us as blindfolded from link to link. We would like to be shown besides the goal also the way we are to follow in general outline, to *understand* the underlying ideas of the proof and their connections. Instead, a modern mathematical proof just as any modern machine or experimental arrangement smothers, by the complexity of technical details, the simple principles on which it is based.[61]

Hence, Weyl acknowledged that mathematical proof may produce conviction, i.e., show *that* a theorem is true. However, understanding *why* a theorem is true requires stronger epistemic conditions than mere conviction, it requires that one reveal the "inner ground" of the theorem. According to him, the most important reason for which a modern mathematical proof fails to bring about understanding is its opacity. This implies that, on Weyl's view, understanding requires that proof be transparent. But what makes proof transparent?

Proof, one might argue, is transparent just in case the mathematical objects that the proof refers to are, themselves, transparent. This claim is typically endorsed by those who believe, like Weyl did, that mathematical objects, numbers in particular, are creations of the mind. However, as we will see presently, he be-

---

[61]Manuscript Hs91a:72. In another manuscript, Hs91a:27, published as 'Axiomatic versus Constructive Procedures in Mathematics' in 1985 by T. Tonietti, the same passage underwent some minor modifications: "We are not content to get convicted, as it were, rather than convinced of a mathematical truth by a long chain of formal inferences and calculations leading us as blindfolded from link to link. We would like to be shown not only the goal but also the way and general outline we are to travel to that goal, to *understand* the underlying ideas of the proof and their connections. Indeed a modern mathematical proof just as any modern machine or experimental arrangement effaces, so to speak, by the complexity of technical details the simple principles on which it is based." (Weyl 1985, 14 (218)) The published version missed Weyl's original emphasis of the word "understand." The German version is: "Wir geben uns nicht gerne damit zufrieden, einer mathematischen Wahrheit überführt zu werden durch eine komplizierte Verkettung formeller Schlüße und Rechnungen, an der wir uns sozusagen blind von Glied zu Glied entlang tasten müssen. Wir möchten vorher Ziel und Weg überblicken können, wir möchten den inneren Grund der Gedankenführung, die Idee des Beweises, den tieferen Zusammenhang *verstehen*. Es ist ja mit einem modernen mathematischen Beweis kaum anders als mit einer modernen Maschine oder einer modernen physikalischen Versuchsanordnung: die einfachen Grundprinzipien sind eingebettet und dem Blicke fast entzog durch eine Fülle technischer Details." (Weyl 1932a, 348 (205))

lieved that the transparency of mathematical objects, which is due to one's having created them, is insufficient for understanding if it is conceived of as a feeling of evidence, i.e., as a subjective reaction to one's creatorly experiences. We suggest that Weyl was led to this view by Husserl's criticism of empiricism. We further submit that, on Weyl's view, the experience of truth is not only a proper basis for believing a provable proposition, but also one that is indispensable for understanding why the proposition is true, for only an experience of truth can make its proof transparent.

2.7   Weyl on the "Understanding from Within"

In this section, we argue that Weyl's commitment to a maker's knowledge view about the nature of mathematical objects, according to which these objects are created by the mind and thus fully transparent to the mind, does not entail commitment to the view that this transparency of objects is enough to explain why a mathematical theorem is true.

Let us start by noting that, in the German cultural space, the maker's knowledge tradition received its earliest expression in the works of Nicholas of Cusa. He argued, against Plato and his followers, that mathematics is based on acts of creation, rather than acts of contemplation:

> Man is a second god. For just as God is the Creator of real beings and of natural forms, so man is the creator of conceptual beings and of artificial forms that are only likenesses of his intellect, just as God's creatures are likenesses of the Divine Intellect. And so, man has an intellect that is a likeness of the Divine Intellect, with respect to creating. [...] Plato said ... that the circle's quiddity (which is simple and incorruptible and free of all contraries) is seen by the intellect alone. ... [But] if Plato had considered [that man's intellect is like God's intellect with respect to creating], assuredly he would have found that our

mind, which constructs mathematical entities, has these mathematical entities, which are in its power, more truly present within itself than as they exist outside the mind.[62]

What Cusa seems to have meant here is that the fact that mathematical objects are produced by the mind guarantees their being "more truly present" within the mind than the objects that are not produced by the mind. The true presence within the mind of the mathematical objects makes us able allegedly to obtain precise, or divine-like, knowledge of mathematical objects: "mathematical entities are notional entities, we know them precisely, by our reason's precision, since they proceed from our reason."[63]

Before we analyze this view, let us further note that, like Cusa, Weyl also believed that as far as the certainty (*Gewißheit*) of mathematical insight is concerned, "the human intellect does not fall short of the divine intellect."[64] This claim seems to be supported by his endorsement of the maker's knowledge view of the nature of mathematical objects: "The numbers are to a far greater measure than the objects and relations of space a free product of the mind and therefore

---

[62]Cf. Cusa 1458, 7ff (31).

[63]Cf. Cusa 1460, 43 (32) (see Breidert 1977 (21) and Hösle 1990 (95), for a discussion of Cusa's anti-Platonism). Similar epistemological views were, before Cusa, advanced by Maimonides, and after him, by Hobbes (see section 3.2 below) and Vico (cf. Vico 1725 (185)). Kant expressed himself along similar lines, most clearly perhaps in the third *Critique*: "One sees completely only into what one can make oneself and bring about in accordance with concepts." ("Denn nur soviel sieht man vollständig ein, als man nach Begriffen selbst machen und zustande bringen kann.") Schopenhauer, as we have seen in section 2.4 above, defended similar views. Salomon Maimon also noted: "All concepts of mathematics are thought by us, and at the same time presented as real objects through construction *a priori*. Thus we are in this respect similar to God. ... God thinks all real objects, not merely according to the principle of contradiction so highly prized in philosophy, but rather as we (albeit in a more complete manner) think the objects of mathematics, i.e., He produces them immediately through thought." (quoted in Franks 2005, 141. For discussion, see Lachterman 1992 (116), Buzaglo 2002 (23).) For two valuable book-length discussions of the maker's knowledge tradition, see Pèrez-Ramos 1988 (145) and Miner 2004 (136).

[64]"der ... Gewißheit nach, aber stehe in jeder gewonnenen mathematischen Einsicht der menschliche Intellekt dem göttlichen nicht nach." (Weyl 1927a, 15 (198); Eng. tr., 16 (211))

transparent to the mind."[65] Hence, in Weyl's opinion, arithmetic is characterized by its objects' being transparent to the mind, where this transparency appears to be due to their being presented to the mind from within, rather than from without. This characteristic seems to entail a special kind of knowledge of mathematical objects, more certain than whatever knowledge we might have of objects that are not produced by the mind. But in spite of his commitment to this maker's knowledge view of the nature of mathematical objects, Weyl came to doubt that the view offers an adequate epistemology of mathematics. Evidence for this claim appears in the following passage, where he dismissed the notion of "understanding from within" as inadequate for mathematics:

> Ideas and intuitive understanding versus calculation and strict logical deductions: what is the real point in these contrasts, what is the secret of understanding a mathematical fact? Certain epistemological schools, I mention the name of Wilhelm Dilthey, have claimed understanding from within, hermeneutics, as the proper basis for historical research and the humanities, whereas the natural sciences seek to explain rather than to understand [from within] the phenomena. The words "intuition, understanding" appear here with a certain [mystical] nimbus indicating a depth and immediacy of their own. In mathematics we prefer to look at things more soberly.[66]

Why did Weyl believe that the depth and immediacy of the "understanding from within" or "intuitive understanding," as the notion was conceived of in Dilthey's

---

[65] "Die Zahlen in ganz anderem Maße freies Erzeugnis des Geistes und darum auch für den Geist durchsichtig sind als die Objekte und Beziehungen des Raumes." (Weyl 1927a, 19 (198); Eng. tr., 22 (211))

[66] "Was aber ist das Geheimnis eines solchen Verstehens mathematischer Sachverhalte, worin besteht es? In der Philosophie der Wissenschaften hat man neuerdings versucht, das *Verstehen*, die Hermeneutik als die Grundlage der Geisteswissenschaften dem naturwissenschaftlichen *Erklären* gegenüberzustellen, und die Worte Intuition, Verstehen erscheinen da mit einem gewissen mystischen Nimbus, eine eigene Tiefe und Unmittelbarkeit anzeigend. In der Mathematik werden wir vorziehen, die Dinge etwas nüchterner anzusehen." (Weyl 1932a, 348 (205)) The English translation is Weyl's own (Weyl 1985, 15 (218)).

hermeneutics, while presumably appropriate for the human sciences, is inadequate for mathematics? To answer this question, let us observe that Dilthey's doctrine of *Verstehen* has been usually interpreted as having developed along the lines of the maker's knowledge view.[67] Indeed, according to him, the essential feature that distinguishes the methodology of the human sciences from that of the natural sciences is that the object of the former, but not that of the latter, is a product of the mind of the scientist:

> These sciences [i.e., the human sciences] have an entirely different foundation and structure than the sciences of nature. Their object is composed of given, elementary units, which we understand from within. ... There is a specific type of experience that takes place here [in the human sciences]: the object builds up itself only gradually before the eyes of the progressing science. Individuals and actions are the elements of this experience, whose nature is the absorption in the object of all the powers of the mind.[68]

Dilthey seems to have believed that in the human sciences objects are products of the mind, and that we experience these objects in a particular way, different than that in which we may experience the objects of the natural sciences. For example, he added, we reach intuitive understanding in the theory of the states because states are created by us; and we understand history because we make the objects that history is concerned with. The rejection of this view, by Weyl, suggests that

---

[67]Isaiah Berlin, for instance, wrote that the maker's knowledge view, as formulated by Vico, "uncovered a species of knowing not previously clearly discriminated, the embryo that later grew into the ambitious and luxuriant plant of German historicist *Verstehen* – empathetic insight, intuitive sympathy, historical *Einfühlung*, and the like." (Berlin 1969, 375 (13)) See also Tuttle 1976 (181).

[68]"Diese Wissenchaften haben eine ganz andere Grundlage und Struktur als die der Natur. Ihr Objekt setzt sich aus gegebenen, nicht erschloßenen Einheiten, welche uns von ihnen verständlich sind, zusammen. ... Und es ist eine eigene Art von Erfahrung, die hier stattfindet: das Objekt baut sich selber erst vor den Augen der fortschreitenden Wissenschaft nach und nach auf; Individuen und Taten sind die Elemente dieser Erfahrung, Versenkung aller Gemütskräfte in den Gegenstand ist ihre Natur." (Dilthey 1883, 109 (41))

the type of experience that one has of one's own creations is, according to him, not sufficient for understanding a mathematical theorem. As we have seen in the previous section, understanding a mathematical theorem requires, on Weyl's view, that one give a transparent proof for the theorem, i.e., a proof that helps one understand why the theorem is true. Thus, his point seems to have been that, in mathematics, creatorly experience is not sufficient to make a proof transparent. But what could justify this point?

Let us look more carefully at the maker's knowledge view. According to this view, as we have seen already, our creation of mathematical objects entails that our knowledge in mathematics is deeper and more certain than in other sciences, the objects of which are not made by us. For creation offers a special kind of experience, one that a maker is thought to have of her own creations just in virtue of being their maker. But one should ask what renders this type of experience capable of furnishing any knowledge at all?

One might argue that the making itself provides a privileged observation point, from where the object made – the artefact – can be most properly observed. This means that the artefact is transparent to the mind in virtue of an experience associated with observation from a vantage point. But why would the ability to observe an object from that vantage point entail that the maker's knowledge of it is deeper or more secure than the knowledge other observers, located at different observation points, might have of that same object? Indeed, why would that ability entail that the maker has any knowledge at all of that object? As has been noted, "simple balances were constructed in antiquity hundreds of years before Archimedes discovered the principle upon which they operate, and for us post-Archimedeans a simple glance at such artefacts tells us much more than their

makers could have known."[69] The special character of the maker's knowledge might also be said to be based on the maker's privileged access to her own creative intentions. Then, those who constructed simple balances before Archimedes could really be said to have possessed a special kind of knowledge. But if what is needed for the maker's knowledge of an artefact is only knowledge of her creative intentions, then this type of knowledge is not obtained in virtue of being a maker, but rather in virtue of being an intender. Thus, one could have had maker's knowledge of simple balances, before Archimedes, even if nobody had actually produced any such thing![70]

Yet, one may think that the special epistemic character of the maker's knowledge derives from a certain feeling of evidence that the creator has with respect to her own creations. A mathematician, thus, obtains a deeper and more certain knowledge of mathematical objects because her creatorly experiences in making these objects give rise to such a feeling of evidence. But if this is so, and if it is indeed correct to see Dilthey's notion of understanding from within as developed along the lines of the maker's knowledge view, then Weyl's justification for rejecting the understanding from within as inadequate for mathematics is that the feeling of evidence caused by one's having created the mathematical objects a theorem refers to is insufficient for rendering its proof transparent, i.e., is incapable of revealing the axioms from which the theorem is derived. In other words, the feeling of evidence caused by a mathematician's creatorly experiences cannot explain why the theorem is true. This claim, we suggest, was taken by Weyl to be based

---

[69]Cf. Gaukroger 1986, 40 (72).

[70]One might want to point out, nevertheless, that the maker's knowledge is not *only* knowledge of intentions, but also knowledge that the artefact has been actually produced. But this seems to be based on mere observation and, thus, not of a special epistemic character (cf. Mackie 1974, 104f (126)).

on Husserl's criticism of the empiricist view of evidence as a feeling, discussed above in section 2.5. As we have seen there, Husserl argued that philosophers like Mill, Höfler, and Meinong misconceived the nature of evidence and, thus, failed to establish it as a criterion of truth. Weyl appears to have taken this to imply that the understanding from within, as supported by a feeling of evidence, cannot explain *why* a proposition is true, since it cannot even indicate *that* the proposition is true. Hence, he concluded, one should be looking at mathematical things more soberly, that is, one should investigate more carefully the conditions required for understanding in mathematics.

What did Weyl take these conditions to be? In particular, what did he think about the relation between understanding and Husserl's own notion of evidence as an experience of truth? Does the experience of truth fare any better than the understanding from within with respect to explaining why a mathematical theorem is true? What might be the conditions under which a Husserlian experience of truth can be considered explanatory? We want to suggest, in the following section, that Weyl took an experience of truth to be indispensable for mathematical understanding, but only if admitted as a form of proof.

2.8 Weyl on Immanent Axiomatics and Understanding

According to Weyl, by contrast to what he called transcendent axiomatics, illustrated by Hilbert's axiomatization of geometry and real analysis, immanent axiomatics is a method of proof aimed at bringing about understanding: "The purpose of this sort of axiomatics [i.e., of immanent axiomatics] is to *understand*."[71] As we have seen above, understanding a mathematical theorem amounts, on his

---

[71]Cf. Weyl 1985, 14 (218).

view, to determining the inner ground of that theorem, i.e., to explaining why the theorem is true. This suggests that Weyl believed that immanent axiomatics should be thought of as a means for explaining why a theorem is true. But what are the features of immanent axiomatics that confer explanatory power to immanent axiomatic proofs? There are two main features that need to be emphasized in this respect. First, on Weyl's view, immanent axiomatic proofs are always *new* proofs, which attempt to supersede, in an epistemic sense, proofs already available. Secondly, as we will presently see, the immanent axioms are not considered unprovable propositions. Rather, they need to be, in a certain sense, proved.

Let us start with Weyl's remark that the mathematician's attempts to solve problems and prove theorems are indispensable for the formation of general mathematical concepts. In his Fields Committee President speech, speaking of Kodaira's work in algebraic geometry, Weyl noted: "Only if someone has the courage of attacking the primary concrete problems in all their complexity, will the general concepts gradually emerge which resolve the difficulties and ease the further progress."[72] What does it mean to say that general concepts emerge by attacking and eventually solving concrete mathematical problems?

> The case is this: all these nice general notions do not fall into our laps
> by themselves. But definite concrete problems were first conquered in
> their undivided complexity, single-handed by brute force, so to speak.
> Only afterwards the axiomaticians came along and stated: Instead of
> breaking in the door with all your might and bruising your hands, you
> should have constructed such and such a key of skill, and by it you
> would have been able to open the door quite smoothly. But they can

---

[72]Cf. Weyl 1954a, 619 (215). The concept that Weyl referred to is that of a Kodaira dimension, a numerical invariant on the basis of which Kodaira attempted to classify the algebraic varieties, such that each family of algebraic varieties has a given Kodaira dimension (although more than one family can have the same Kodaira dimension). The emergence of this concept allowed, as Weyl remarked, a "profound generalization of the well-known fact that every compact Riemann surface belongs to an algebraic function field."

construct the key only because they are able, after the breaking in was successful, to study the lock from within and without. Before you can generalize, formalize and axiomatize, there must be a mathematical substance.[73]

Weyl seems to have denied that the axiomatist could actually solve problems and prove theorems that have not already been given a solution or proof. He suggested that non-axiomatic proofs and solutions – what he metaphorically called the "mathematical substance" – must be already available for the axiomatist to be able to form general concepts and formulate her axioms. Weyl thought that the goal of axiomatics was not to prove theorems, but to find *new* proofs for those already proved. He also suggested that there are particular epistemic gains that are brought about by axiomatic proofs. But it is not clear why he believed that mathematical understanding might be an epistemic gain of *immanent* axiomatics, in particular. That is, it is not clear why he believed that an immanent axiomatic proof would be needed for understanding why a theorem is true. To clarify this point, let us look at Weyl's general characterization of immanent axiomatics:

> Immanent axiomatics as applied in algebra or topology or some other concrete branch of mathematics is neither based on external evidence nor on hypotheses, but the axioms are proved to hold for the mathematical objects in the individual situations to which the axioms are applied. ... Axiomatics as used in concrete mathematical research is *au fond* a much simpler affair [than transcendent axiomatics]. One just

---

[73] "Es ist nähmlich so, daß alle die schönen allgemeinen Begriffe nicht von selber den Menschen in die Hände fallen. Sondern zuerst sind bestimmte konkrete Probleme, in ihrer unzerlegten Komplexität, sozusagen durch brutale Gewalt von Einzelnen bezwungen worden. Erst nachher kommen die Axiomatiker und stellen fest: Statt die Tür mit aller Anspannung der Kräfte einzudrücken und sich die Hände blutig zu reißen, hätte man sich einen so und so beschaffenen kunstvollen Schlüssel konstruieren sollen, und mit ihm wäre die Tür ganz leise wie von selber zu öffnen gewesen. Aber den Schlüssel können sie doch erst konstruieren, weil sie nach gelungenem Durchbruch das Schloss von hinten und vorn, von aussen und innen studieren können. Bevor man generalisieren, formalisieren und axiomatisieren kann, muss eine mathematische Substanz da sein." (Weyl 1932a, 357 (205); Weyl 1935, 438 (207))

formulates some of the fundamental facts which, as one can prove, hold for the object of investigation, which is usually a set of freely constructed elements. The questions of consistency, independence and completeness are here [i.e., in immanent axiomatics] but of minor importance.[74]

Hence, on Weyl's view, immanent axioms should not be regarded as hypotheses or postulates, nor are they empirical propositions. In a certain sense, such axioms are to be *proved.* This claim is, of course, quite strange, since axioms are typically considered to be unprovable propositions, i.e., propositions that are in no need of proof or may not even allow of proof. It goes against the old Aristotelian view, recalled in section 2.3 above, that only the uneducated man seeks to prove things that one should not require proofs for. But it also seems to go against Weyl's own idea that, if one has an experience of their truth, mathematical propositions – even those that are provable – are in no need of proof to provide a proper basis for believing them. As we have seen already, he defended this idea against Dedekind, and claimed that it is not proof, but rather the experience of truth, that offers the most proper basis for believing a provable proposition.

However, these tensions disappear if one assumes the experience of truth as a type of proof. On this assumption, to prove a proposition one needs to experience its truth, that is, as we have seen in section 2.5 above, one must present before one's eyes, as Husserl put it, the objects that completely satisfy the meaning intentions expressed in that proposition. If this is what Weyl had in mind, as it seems plausible to think, then immanent axioms may be said to be provable in the sense that their truth is experienceable.

This clarifies, to a certain extent, his view on the relation between the experience of truth and mathematical understanding: one cannot understand why a

---

[74]Cf. Weyl 1985, 14 (218).

mathematical theorem is true without an experience of the truth of immanent axioms. But it seems clear that Weyl could not have taken the experience of the truth of immanent axioms to be sufficient for mathematical understanding. The reason for this is that, as he suggested, though it may start from evident axioms, mathematical proof often proceeds in ways less than transparent. For proof transparency to obtain, one must experience not only the truth of the axioms but also the truth of the theorem derived from them. As emphasized in section 2.6 above, Weyl believed that this requires that one construct in intuition the objects referred to in the theorem from those referred to in the axioms. For only the construction of these objects can completely satisfy the prover's meaning intentions expressed in the theorem. Briefly stated, then, Weyl's view seems to have been that mathematical proof can bring about understanding only if it is constructive, which entails that he regarded immanent axiomatic proof as constructive, i.e., as proceeding through wholly contentual reasoning.

This view on understanding raises several questions. First, it is not clear what Weyl thought about how mathematical concepts emerge in immanent axiomatics and how they acquire contentual significance. Quite generally, the emergence of concepts might be understood as a process of creation that is either free or constrained by intuition. As we will see in section 4.2 below, free creation characterizes, according to Weyl, Hilbert's transcendent axiomatics: transcendent axioms are postulates formulated in terms of concepts that are radically separated from intuition (although they may be suggested by intuition), rather than propositions whose truth can be experienced. By contrast, the method of concept formation that appears to characterize immanent axiomatics may be conceived of as a process of creation by abstraction from what is intuitively presented in experience.

This seems to be suggested by Weyl's opinion that the concepts of group, field, or Kodaira dimension are obtained from the "mathematical substance," i.e., from the properties and relations of the mathematical objects considered intuitively in non-axiomatic proofs and solutions. We defer a closer discussion of Weyl's view on concept formation to the next chapter, where we analyze his critical engagement with Fichte's and Husserl's philosophical doctrines.

Secondly, in the passage quoted above, Weyl claimed that the consistency, independence and completeness of immanent axiomatic systems are of "minor importance." Since, according to him, this type of axiomatics aims at understanding, to say that that consistency, independence, and completeness are of minor importance is to say that they are of minor importance with respect to understanding. This claim might be taken to mean that consistency, independence, and completeness are unnecessary for understanding, which entails that one may understand why a theorem is true despite the inconsistency, non-independence, and/or incompleteness of the system in which the theorem is proved. However, it is hard to see how a proof can bring about understanding, if it is based on an inconsistent and/or non-independent system of axioms.[75]

But Weyl's claim that the consistency, independence, and completeness of immanent axiomatic systems are of minor importance might also be taken to mean that *their proof* is unnecessary. This entails that one may understand why a theorem is true despite having no proof of consistency, independence, and completeness. In fact, Weyl seems to have believed that proving completeness, in the sense of syntactic completeness, would be adverse to understanding:

Completeness in this sense would be ensured only by the specification

---

[75]Weyl's view on the relation between understanding and completeness, in the sense of categoricity, is discussed in section 4.3 below.

of a method to strictly regulate proof procedures, one that provably leads to a decision for every relevant problem. Mathematics would thereby be trivialized. But such a "philosopher's stone" has not been found and never will be found. Mathematics does not consist in omnilaterally developing logical consequences from given assumptions; but intuition, the life of the scientific mind, poses the problems, and these cannot be solved like calculations, by applying a strict scheme.[76]

Weyl's argument seems to be that syntactic completeness, if proved, would entail a trivialization or mechanicization of mathematical activity. But trivialization is adverse to understanding because it transforms genuine problem-solving into mere calculation, without any experience of truth. Why would proving syntactic completeness entail trivialization? According to him, if one knew that a system is syntactically complete, that all problems are decidable, one would start to "omnilaterally develop logical consequences" from the axioms and then one would just "discard the 'uninteresting' consequences." But it is not clear what justifies this claim. As is well known, there are mathematical systems, like Presburger arithmetic (basically, first order Peano arithmetic without multiplication), that are syntactically complete, but not trivial in any sense.[77]

Another question raised by Weyl's view on understanding is how well does this account for actual mathematical practice, especially considering the development of modern mathematics? As already noted above, it is hard to see how, on Schopenhauer's view, one could avoid the conclusion that most of modern mathematics is unscientific. But can Weyl's view, which does not seem to essentially differ from Schopenhauer's, avoid this conclusion? As we will see in chapter four, the answer is both yes and no. For, on the one hand, Weyl considered transcendent axiomatic proofs as unscientific, since he believed that they lack transparency and,

---

[76]Cf. Weyl 1927, 20f (198), Weyl 1949, 24 (211).

[77]Cf. Presburger 1930 (148).

thus, hide the inner ground of a mathematical theorem, i.e., they cannot explain why the theorem is true, as they dispense with the construction of mathematical objects. On the other hand, Weyl considered that transcendent axiomatics is, in a certain sense, scientific, but only if one takes into account its relation to physics. For, as he noted, this type of axiomatics has an indispensable role in the theoretical construction of the real world, that is, in the formation of objective scientific theories.

2.9 Conclusion

We started this chapter with an analysis of Weyl's argument against the view defended by Dedekind, according to which the proper basis for believing a provable proposition is proof, rather than intuitive evidence. The proper basis for belief is proof, and only proof, because, as Bolzano emphasized, proof, and only proof, can reveal the objective ordering of truths. We have seen that Schopenhauer rejected this view on the basis of the idea that genuine scientific knowledge requires that the ordering revealed by proof be based on intuitive evidence. Weyl, we argued, adopted a largely Schopenhauerian position, as he maintained that evidence, conceived of *à la* Husserl as an experience of truth, i.e., as an epistemic achievement of a complete satisfaction of a prover's meaning intentions, is an unexcelled basis for belief, and that the provability of a mathematical proposition cannot render this basis inadequate for proper belief.

Afterwards, we discussed the relation between an experience of truth and mathematical understanding. We argued that, led by Husserl's criticism of the view of evidence as a feeling, Weyl rejected the maker's knowledge view of mathematical understanding as based on a feeling of evidence, and came to think that an ex-

perience of truth is also the proper basis for understanding why a mathematical theorem is true. This assumes, however, that such an experience can be admitted as a form of proof. But the only form of proof that the experience of truth may take is, as we have seen, that of an immanent axiomatic proof, i.e., a proof that provides a construction of the objects that a theorem refers to. On Weyl's view, then, understanding why a theorem is true requires wholly contentual reasoning.

In the next chapter, we will see that Weyl thought that the method of concept formation that he believed could support both understanding and objectivity is Husserl's ideational abstraction, rather than traditional empiricist abstraction. However, we argue that Weyl eventually came to realize that scientific objectivity requires the introduction of hypothetical elements, i.e., real but in principle unobservable entities, and thus it requires free creation as a method of concept formation and partly non-contentual or purely symbolic reasoning. This fact reveals, as will be emphasized, a fundamental tension between what he took to be the necessary conditions for understanding and the necessary conditions for objectivity.

CHAPTER 3

ABSTRACTION, FREE CREATION, AND OBJECTIVITY

3.1   Introduction

In the present chapter, we consider Weyl's views about the formation of scientific concepts and its connection with objectivity. As we have seen in the previous chapter, he believed that understanding requires wholly contentual reasoning and, as will be clarified below, he endorsed the view that concepts emerge by abstraction from immediate experience. But we will also see that Weyl eventually came to realize that scientific objectivity requires that concepts be introduced by free creation, rather than abstraction, and that non-contentual or purely symbolic reasoning is indispensable for our knowledge of the mind-independent world. This indicates that, on Weyl's view, there is a fundamental tension between the necessary conditions for intelligibility and the necessary conditions for objectivity.

We start with Weyl's remarks on the traditional empiricist view that physical concepts are obtained through abstraction from perceptual experience. He believed that this view, as represented, for example, by Hobbes' constructivist view of natural science, fails to account for what physicists actually do when they do physics, since it fails to account for hypothetical elements, i.e., real but in principle unobservable entities. By empiricist abstraction one cannot introduce, for example, the concepts of force, energy, or electromagnetic field, since neither force

70

nor energy nor field can be given in perceptual experience. Therefore, Weyl concluded, traditional empiricism cannot account for scientific objectivity, that is, for the physicists' attempt to provide knowledge of the mind-independent world, although it might account, just like Leibniz' monadological phenomenalism, for mere intersubjectivity. Then, we discuss Weyl's interpretation of Fichte's constructivist view in the *Wissenschaftslehre*. We present the latter's argument against abstraction and his emphasis on the freedom of the mind to create concepts, and then explain the reason for Weyl's rejection of Fichte's view. Weyl noted that Fichte misconceived the connection between freely created concepts and our perceptual experience, as he claimed that what can be known through perceptual experience coincides with what is freely created by the mind and, thus, known independently of perceptual experience. This seems to have led Weyl to believe that the *Wissenschaftslehre* embraces Leibniz' monadological phenomenalism and, thus, fails to account for objectivity.

Early in his career, Weyl endorsed Husserl's phenomenological view that objectivity demands what the latter called "ideational" abstraction, rather than traditional empiricist abstraction. Ideational abstraction introduces general concepts on the basis of a certain relation between intending acts of the mind, rather than by disregarding certain properties of the intended objects. We show how Weyl modified the general theory of relativity in light of this view, and explain that the epistemological significance of this modification resides in the elimination from the physical theory of the concepts obtained from intending acts that cannot be satisfied by the intuitive presentation of the intended objects. In his 1927 *Philosophy of Mathematics and Natural Science*, Weyl developed the view that a scientific proposition can be considered objective only if one knows that it

71

is true when relativized to any coordinate systems. This view may also be understood along phenomenological lines, as it actually comes down to saying that a proposition is objective only if one knows that it is true for any transcendental subjectivity, i.e., as we will explain, only if one has an experience of its truth.

We argue, however, that Weyl ultimately turned against the phenomenological approach to scientific objectivity. To support this claim, we show that he came to reject the idea, defended by Husserl, that the experience of truth is necessary for objectivity. Weyl's rejection of this idea was based on the observation that the truth of a proposition about hypothetical elements, i.e., about real but in principle unobservable entities, cannot be experienced, for no such entity can be presented in intuition. He came to disagree with Husserl's rejection of the positing of such entities, and to realize that pure phenomenology, just like Hobbes' and Fichte's constructivisms, collapses into monadological phenomenalism. The positing of hypothetical elements in physics led Weyl to believe that scientific objectivity requires the introduction of concepts that are not obtained by abstraction, but freely created through the stipulation of fundamental theoretical presuppositions (e.g., Newton's laws, Maxwell's equations, and the like). It is this belief that suggested to him, as we will see in chapter four, that the scientific significance of Hilbert's transcendent axiomatics should be sought in connection with the epistemic ideal of objectivity, rather than that of intelligibility.

3.2   Weyl on Hobbes' Constructivism and Empiricist Abstraction

In this section, we want to explain why Weyl considered that traditional empiricism, Hobbes' view in particular, fails to account adequately for the objectivity of scientific knowledge. Then, in the next section, we will discuss his reasons for

thinking that Husserl's phenomenology could avoid the problems raised against traditional empiricism, while preserving an essentially similar view of scientific concept formation.

Hobbes' view on physics can be best understood by comparison with his view on geometry. He thought that geometry was unique with respect to the epistemic character of its results, that it is a demonstrable art, a *scientia*, and could share its status only with "civil philosophy:"

> Of arts, some are demonstrable, others indemonstrable; and demonstrable are those the construction of the subject whereof is in the power of the artist himself, who, in his demonstration, does no more but deduce the consequences of his own operation. The reason whereof is this, that the science of every subject is derived from a precognition of the causes, generation, and construction of the same; and consequently where the causes are known, there is place for demonstration, but not where the causes are to seek for. Geometry therefore is demonstrable, for the lines and figures from which we reason are drawn and described by ourselves; and civil philosophy is demonstrable because, we make the commonwealth ourselves. But because of natural bodies we know not the construction, but seek it from the effects, there lies no demonstration of what the causes be we seek for, but only of what they may be.[1]

The fact that, on Hobbes' view, geometry and civil philosophy are demonstrable seems to be due to the geometer's and the civil philosopher's power to successfully construct their objects of inquiry, i.e., geometrical figures and the commonwealth. Their success appears to be guaranteed by the fact that the geometer and the civil philosopher possess the causes from which, and the operations whereby, such objects are constructed. The knowledge that they obtain of these constructed

---

[1] Cf. Hobbes 1656, 183f (92). For useful analyses of Hobbes' view on geometry and demonstration, see Sacksteder 1980 (160), Jesseph 2007 (109).

objects is, as one might put it, maker's knowledge.[2]

Note, however, that Hobbes did not claim that the physicist cannot be a constructor or a maker. Hobbes seems to have suggested that, in physics, knowledge of the true causes and operations for the generation of physical objects is not a necessary condition for theoretical construction. For he says that in physics "there lies no demonstration of what the causes be we seek for, *but only of what they may be.*" He, thus, seems to suggest that there is room for construction in physics, but not the type of construction that one employs in mathematics. The physicist is a maker, but not a maker of the real world. What, then, is the physicist a maker of?

Hobbes' view about construction in physics can be explained by considering a thought experiment that he offered in his *De Corpore*.[3] He maintained there that the way physics proceeds can be best understood if one imagines an annihilation of the world, following which nothing remains but man, who preserves his ability to think, imagine, and remember. Hobbes noted that, before the annihilation of the world, bodies are taken to exist independently of the mind. As mind-independent, bodies have magnitude (or extension), and insofar as they have magnitude, bodies constitute real space. According to him, all natural phenomena reduce to bodily changes caused by real motions, i.e., motions in real space. Therefore, if one seeks knowledge of natural phenomena, one needs to determine the real motions that cause bodily changes. But real motions are in principle completely inaccessible to us – we do not possess them, as it were. How can the physicist, then, obtain knowledge of natural phenomena?

---

[2]Cf., e.g., Pèrez-Ramos 1988 (145), Miner 2004 (136), Jesseph 2007 (109). For a more detailed discussion of the maker's knowledge principle, see section 2.7 above.

[3]Cf. Hobbes 1655, II, 7 (91).

The world has been annihilated, Hobbes added, only for the sake of a new genesis on the basis of our own "phantasms," which had been obtained either by abstraction from definite bodies, i.e., bodies as distinguished from one another, or from bodies taken indefinitely, i.e., by abstraction from the qualitative characteristics that distinguish bodies from one another. The former are phantasms of "light and colour, and heat and sound, and other qualities which are commonly called sensible."[4] The latter are phantasms of space, time, and motion. On Hobbes' view, the physicist aims at obtaining knowledge of natural phenomena by drawing consequences from her operations with such phantasms:

> If we do but observe diligently what it is we do when we consider and reason, we shall find, that though all things be still remaining in the world, yet we compute nothing but our own phantasms. For when we calculate the magnitude and motions of heaven and earth, we do not ascend into heaven that we may divide it into parts, or measure the motions thereof, but we do it sitting still in our closets or in the dark.[5]

The relevant question is, of course, whether by drawing consequences from operations with her phantasms the physicist can indeed obtain knowledge of natural phenomena. What Hobbes seems to suggest is that although the physicist's knowledge cannot be maker's knowledge of natural phenomena (since the physicist lacks the elements and the operations required for their *construction*), it is nevertheless knowledge enough insofar as it is based on the elements and the operations available to the physicist for a *reconstruction* of natural phenomena.

Now, how did Weyl interpret this view? Here is a comment he made in his *Philosophy of Mathematics and Natural Science*:

---

[4]Cf. Hobbes 1655, 391f (91).

[5]Cf. Hobbes 1655, 92 (91).

> Hobbes developed the view (*English Works* VII, 183ff) that we know
> with certainty only in those sciences which *construct* their objects
> from the conditions residing within the knowing subject. To him, it
> is not the mental images that are the reality, but their content that
> makes its construction possible. In contrast with the mere *cognitio*,
> this synthetic process of generating the phenomenon from its grounds
> is the *scientia* in the strict sense. This takes place within the natural
> science as far as mathematical deduction is possible.[6]

As we have seen above, according to Hobbes, natural phenomena cannot be constructed from their grounds, i.e., from real motions, for such motions are not in the physicist's possession – they do not reside within the subject. This is why Hobbes believed that natural science is mere *cognitio*, and not *scientia*. By contrast, on his view, mathematics is *scientia*, and not mere *cognitio*, since mathematical objects can be constructed from their grounds, i.e., from imaginary motions, for such motions are in the mathematician's possession – these do reside within the subject.

Weyl's comment, however, points out that there is no unbridgeable division between Hobbes' *cognitio* and *scientia*, since mathematical reasoning is actually used in physics quite extensively. Thus, according to Weyl, there is *scientia* within *cognitio* insofar as not only the mathematician, but also the physicist has the power to construct objects from elements within the mind. These elements, as he emphasizes, are not mental images – the sense data – but ideas or phantasms abstracted from them. Weyl seems to have thought that Hobbes believed that the objects thus constructed by the physicist are, in a certain sense, real. Thus,

---

[6] "Hobbes entwickelt den Satz (*English Works* VII, S.183ff), daß wir mit Gewißheit nur in den Wissenschaften erkennen, welche ihren Gegenstand *konstruieren* aus den im erkennenden Subjekt gelegenen Konstruktionsbedingungen. Ihm sind nicht die Bewußtseinsbilder die Wirklichkeit, sondern das in ihnen Enthaltene, das ihre Konstruktion möglich macht. Im Unterschiede von der bloßen *cognitio* sei dieser synthetische Fortgang der Erzeugung der Erscheinung aus ihrem Grunde die *scientia* in strengem Verstande. Eine solche finde innerhalb des Naturerkennens soweit statt, als mathematische Ableitung möglich ist." (Cf. Weyl 1927a, 111f (198), Eng. tr., 151 (211))

according to Weyl, Hobbes' view was that to the extent that the physicist proceeds in accordance with the method of *scientia*, physical knowledge can be both certain and objective.

Nevertheless, Weyl argued that this view is wrong. He maintained that the problem with it is that the phantasms within the mind – sensible qualities and imaginary motions – are merely empirical concepts, a "Rückstand der Erfahrung," a mere residue of our perceptual experience.[7] It is this limitation to empirical concepts that makes Hobbes' view and, more generally, traditional empiricism, unable to provide an accurate account of scientific objectivity. For, Weyl wrote, modern science goes beyond perceptual experience, in the following sense:

> We are not satisfied with intuitively isolable elements but interpret a series of properties which always appear together as an indication of a concealed something. This leads to *hypothetical elements*, such as atoms, forces, electro-magnetic field, etc. Moreover, we learn to interpret not only the observable properties but also the reactions that occur if one system is brought together with another as manifestations of such hypothetical elements and of their intensive and quantitative values.[8]

Hence, modern science goes beyond perceptual experience in the sense that it postulates hypothetical entities, like forces and fields, which are assumed to be real but in principle unobservable. For example, in classical field theory, one posits the existence of a real but in principle unobservable field around a conductor, a field that exerts an observable influence on other bodies. The concept of a field,

---

[7]Cf. Weyl 1927a, 79 (198); Eng. tr., 112 (211).

[8]"Man bleibt nicht bei anschaulich abhebbaren Elementen stehen, sondern faßt eine Reihe stets zusammen auftretender Beschaffenheiten als Anzeichen eines verborgenen Etwas auf: dies führt zu *hypothetischen Elementen*, wie z. B. den Atomen, den Kräften, dem elektromagnetischen Feld. Aber nicht nur die vorfindbaren Beschaffenheiten, sondern auch die Verhaltungsweisen eines Systems beim Zusammenbringen mit anderen lernt man deuten als Bekundungen derartiger Elemente und ihres intensiven oder quantitativen Wertes." (Weyl 1927a, 107 (198); Eng. tr., 146 (211))

however, cannot be an empirical concept, i.e., it cannot be obtained by abstraction from the properties of a particular object given in perceptual experience, for no electromagnetic field can be an object of perceptual experience. Thus, Weyl's main reason for rejecting the traditional empiricist approach to scientific objectivity, Hobbes' view in particular, appears to be due to its inability to account for the introduction of so-called theoretical concepts.

This criticism emphasizes that basic propositions of modern science could not even be formulated, if traditional empiricist abstraction was assumed as a method of concept formation. Physics would be crippled, were one to limit its conceptual apparatus to mere empirical concepts. In particular, if this apparatus were thus limited, one would fall short of attaining objective knowledge, i.e., knowledge about the real, mind-independent world, even though one might attain intersubjectivity:

> As long as I do not go beyond what is simply given, or more exactly, what is merely given at the moment, there is obviously no need to support through an objective world that which is given. Even if I include memory and in principle acknowledge it as valid testimony, if I furthermore accept as data the contents of the consciousness of others on equal terms with my own, thus opening myself to the mystery of intersubjective communication, I would still not have to proceed as we actually do, but might ask instead for the 'transformations' which mediate between the images of different consciousnesses. Such a presentation would fit in with Leibniz's monadology.[9]

---

[9] "Solange ich bei dem schlechthin Gegebenen, genauer: dem schlechthin Gegebenen des Augenblicks stehen bleibe, ist offenbar kein Bedürfnis vorhanden, es durch eine objektive Welt zu unterbauen. Aber auch wenn ich Erinnerung hinzunehme und ihre Triftigkeit grundstützlich anerkenne, wenn ich die Inhalte fremden Bewußtseins als den meinen gleichberechtigte Data akzeptiere, mich dem Geheimnis der intersubjektiven Verständigung öffnend und ihr Glauben beimessend, brauchte ich nicht so zu verfahren, wie wir wirklich tun, sondern könnte statt dessen nach den 'Transformationen' suchen, welche zwischen den Bildwelten verschiedener Bewußtsein vermitteln. Eine solche Darstellung wäre der Leibnizschen Monadologie angemessen." (Weyl 1927a, 83 (198); Eng. tr., 117 (211))

Hence, Weyl seems to have thought that, by limiting the conceptual apparatus of physics to empirical concepts, traditional empiricism would fare no better than Leibniz's monadological phenomenalism, with respect to scientific objectivity. Weyl thought that monadological phenomenalism cannot account for scientific objectivity because it does not account for what physicists actually do when they do physics. The reason for thinking so was that, on Leibniz's view, as Weyl understood it, the physicist would not need to posit hypothetical entities at all, since her account of natural phenomena may be exclusively based upon the harmonious connection between what is immediately given to different consciousnesses. This account, however, is at variance with what physicists actually do when they do physics. In particular, Weyl believed that it is at variance with classical field theory. On the Leibnizean view, he argued, "preestablished harmony takes the place of the reciprocal effects that are transmitted through the field."[10] In other words, even if one takes into account the "images of different consciousnesses," i.e., even if scientific theories remain invariant across different, but equivalent, observational perspectives, one still fails to obtain knowledge of the mind-independent world. For, according to the monadological phenomenalist, no mind-independent world *needs* to be stipulated, in the first place, to support that which is given to consciousness.

Weyl's rejection of the traditional empiricist approach to science is also indicated in a letter to the German mathematician Otto Hölder, in which Weyl noted

---

[10] "An Stelle der Wechselwirkung, die für uns durch das Feld vermittelt wird, tritt die prästabilierte Harmonie." (Weyl 1927a, 133 (198); Eng. tr., 174 (211)) As is well known, the clearest textual evidence for Leibniz' phenomenalism comes from his famous 1704 letter to de Volder: "Matter and motion, however, are not so much substances or things, as they are the phenomena of percipient beings, whose reality is located in the harmony of the percipient with himself (at different times) and with other percipient beings." (Leibniz 1969, 537 (120)) For a detailed discussion of this and other related passages, see Jolley 1986 (110), Rutherford 1990 (156), and Garber 2009, 267-301 (71).

that the set-theoretical paradoxes were actually due to an empiricist account of concept formation:

> The failure to recognize that the *meaning* of a concept is logically prior to its *extension* is widespread today; even the foundations of our set theory suffer from it. It seems to spring from the peculiar theories of abstraction of the empiricist theory of knowledge; against these compare the brief but striking remarks in Fichte's *Transcendental Logic* and the more careful exposition in Husserl's *Logical Investigations*.[11]

Thus, Weyl seems to have believed that Fichte's and Husserl's criticisms of empiricist abstraction can justify the rejection of unrestricted comprehension in set theory.[12] They do so by claiming that the meaning of a general concept is, in a certain logical sense, prior to the objects that fall under that concept. To properly understand this claim, we want to look more closely at these two criticisms.

In the next section, we present Fichte's argument against abstraction and his emphasis on the free creative activity of the mind. We explain why, despite the fact that Weyl found this emphasis correct, he nevertheless rejected Fichte's constructivism as unable to support objectivity. Afterwards, we discuss Husserl's critical remarks on empiricist abstraction, and his proposal of a new, phenomenological method of concept formation – the so-called "ideational" abstraction. We show how Husserl's method influenced Weyl's early work on general relativity, and then argue, as against a recent interpretation, that Weyl eventually rejected the

---

[11] "Die Verkennung der Tatsache, daß der *Sinn* eines Begriffs das logische prius gegenüber dem *Umfang* ist, ist heute gang und gäbe; an ihr leiden auch die Grundlagen unserer Mengenlehre. Sie scheint den sonderbaren Abstraktionstheorien der sensualistischen Erkenntnistheorie zu entstammen; vgl. dawider die kurzen schlagenden Bemerkungen Fichtes in seiner "Transzendentalen Logik", die sorgfältigeren Darlegungen in Husserls "Logischen Untersuchungen"." (Weyl 1919, 44 (194); Eng. tr., 110 (194))

[12] In Weyl's own predicative version of real analysis, developed in *The Continuum* as a way around paradox, comprehension is restricted to formulas whose bound variables range over natural numbers only.

phenomenological approach to objectivity.

3.3 Weyl on Fichte's Constructivism and Free Creation

Weyl's interaction with Fichte's philosophy has been, in general, overlooked by Weyl scholars. Quite naturally, one might wonder what would a mathematician find interesting in the Fichtean *Wissenschaftslehre* to study it so intensely as Weyl did.[13] But, as we have just noted, in a 1919 letter to Otto Hölder, Weyl referenced Fichte's criticism of abstraction, which indicates that, in all appearances, Weyl agreed with Fichte's emphasis on the ability of the mind to create concepts independently of perceptual experience, rather than by abstraction from such experience. However, as we will point out, he rejected Fichte's view as insufficient to support objectivity. To understand Weyl's criticism, it is necessary to present, even if only briefly, the main aspects of the *Wissenschaftslehre*.

The purpose of Fichte's project, as stated perhaps most emphatically in his *Sonnenklarer Bericht*, is to "construct simply a priori, from basic principles, the whole common consciousness of all rational beings."[14] What are the basic principles on which such a construction is to be based, and how is construction supposed

---

[13]In his published work, Weyl referred almost exclusively to Fichte's popular work *Die Bestimmung des Menschen*, but according to manuscript Hs91:75, Weyl also read at least the 1794 *Grundlage des gesamten Wissenschaftslehre*, the 1797 *Erste Einleitung*, the 1810 *Wissenschaftslehre*, the *Grundriss des Eigentümlichen der Wissenschaftslehre*, and the 1806 *Die Einweisung zum seligen Leben*. It is also known that Weyl was an active participant in the philosophical circle of Fritz Medicus, the proponent of a neo-Fichtean philosophy and also curator of a new edition of Fichte's *Selected Works* (cf. Sieroka 2010 (168)). Thus, it is perfectly reasonable to suspect that Weyl's engagement with Fichtean philosophy was not a mere intellectual extravagance. As an aside, it might be worth mentioning that Weyl was not the only major scientist interested in Fichte. In ways that are still far from being clearly understood, Fichte exerted some influence also on Helmholtz' view on science. Cf. Köhnke 1986 (114), Heidelberger 1993 (82).

[14]"[Die Wissenschaftslehre] construirt das gesammte gemeinsame Bewußtseyn aller vernünftigen Wesen schlechthin a priori, seinen Grundzügen nach." (Fichte 1801, 603 (60))

to proceed in order to produce everything that is contained in the mind of a rational being?

The first thing that anyone engaged philosophically needs to do is, according to Fichte, the following:

> Attend to yourself; turn your gaze from everything surrounding you and look into yourself: this is the first demand philosophy makes upon anyone who studies it. Here you will not be concerned with anything that lies outside of you, but only with yourself.[15]

This introspective path, Fichte contended, leads to the absolute ego. In his review of Gottlob Ernst Schulze's *Aenesidemus*, Fichte explained more exactly why he believed that introspection can lead to the absolute ego: "The absolute subject, the *ego*, is not given in an empirical intuition but is posited through an intellectual one. ... one will never become conscious [of the absolute ego] as something empirically given."[16] On his view, then, the absolute ego is not merely there to be discovered through introspection, where introspection is taken to be passive observation, of the kind illustrated (so Fichte believed) by sensible intuition. Rather, the absolute ego is posited through intellectual intuition. As he put it: "*The I originally simply posits its own existence.*"[17] But what does this mean? How did Fichte conceive of intellectual intuition?

---

[15] "Merke auf dich selbst: kehre deinen Blick von allem, was dich umgiebt, ab, und in dein Inneres – ist die erste Forderung, welche die Philosophie an ihren Lehrling thut. Es ist von nichts, was außer dir ist, die Rede, sondern lediglich, von dir selbst." (Fichte 1797a, 422; Eng.tr., 7 (56))

[16] "Das absolute Subject, das Ich, wird nicht durch empirische Anschauung gegeben, sondern durch intellectuelle gesetzt; ... des absoluten Subjects ... wird man sich nie, als eines empirisch gegebenen, bewusst." (Fichte 1792, 10; Eng. tr., 142 (53)) The notion of positing (*Setzung*) through intellectual intuition was introduced in Fichte's response to Schulze's criticism of Reinhold's reconstruction of Kantian philosophy. For a detailed account of this response, see Neuhouser 1990, 70-76 (140).

[17] "*Das Ich setzt ursprünglich schlechthin sein eigenes Seyn.*" (Fichte 1794, 98 (54))

One doesn't have the slightest idea what transcendental philosophy – and Kant especially – is speaking of if one thinks that, when an act of intuition occurs, there exists outside the intuiter and the intuition some further thing, perhaps some matter, at which the intuition is directed (somewhat like the way common sense tends to conceive of bodily vision.) What is intuited comes to be through the intuiting itself, and only through it. ... In intuition, reason (the I) is by no means passive, but absolutely active; in it, it is *productive imagination.* Through intuition something is projected, somewhat like – if one wants an analogy – the way in which the painter projects the completed shape out of his eye onto the surface, that is, he *looks toward*, so to speak, before the slower hand can copy the outline of the shape.[18]

On Fichte's view, intellectual intuition, as opposed to sensible intuition, is not mere *Schauen* or *Sehen* – the seeing of something that is already there to be seen. Rather, it is *Hinschauen* or *Hinsehen* – a seeing that produces that which is seen, a free creative activity, i.e., an activity unconstrained by perceptual experience. Thus, according to Fichte, the positing of the ego through intellectual intuition amounts to a free creation of the ego. As he suggestively put it: "I am thoroughly my own creation."[19] Furthermore, the construction of the "whole common consciousness of a rational being," i.e., the entire representational content of a rational mind, also obtains, on his view, as a result of the creative activity of the mind. In particular, as we will see presently, he maintained that *all* concepts, including the concepts typically considered empirical, are freely created, rather than abstracted from perceptual experience.

---

[18] "Man hat nicht die leiseste Ahnung, wovon bei der transscendentalen Philosophie, und ganz eigentlich bei Kant die Rede sey, wenn man glaubt, dass beim Anschauen es ausser dem Anschauenden und der Anschauung noch ein Ding, etwa einen Stoff, gebe, auf welchen die Anschauung gehe, wie etwa der gemeine Menschenverstand das leibliche Sehen zu denken pflegt. Durch das Anschauen selbst, und lediglich dadurch entsteht das Angeschaute. ... Die Vernunft (das Ich) ist in der Anschauung keinesweges leidend, sondern absolut thätig; sie ist in ihr *productive Einbildungskraft.* Es wird durch das Schauen etwas hingeworfen, etwa, wenn man ein Gleichniss will, wie der Maler aus seinem Auge die vollendete Gestalt auf die Fläche hinwirft, gleichsam *hinsieht*, ehe die langsamere Hand ihre Umrisse nachmachen kann." (Fichte 1796, 57sq; Eng. tr. 54sq)

[19] "Ich bin durchaus mein eigenes Geschöpf." (Fichte 1800, 256; Eng. tr., 73 (59))

Fichte's explicit rejection of abstraction was presented in a series of lectures offered at Jena, in which he attempted to distinguish his transcendental idealism from pure logic. The motivation for this attempt was given by Kant's following remark:

> I clarify herewith that I consider *Fichte's Wissenschaftslehre* as a completely untenable system. For pure theory of science is nothing more or less than mere *logic*, which cannot reach with its principles as far as the material of knowledge, but abstracts, as pure logic, from its content .[20]

Since pure logic abstracts from content and considers relations between judgments according to their mere form, Kant implied, it is unable by itself to reveal the necessary conditions for the possibility of knowledge. Thus, his criticism here points out that the *Wissenschaftslehre* cannot be considered as transcendental idealism, not to mention as a proper or correct development of his own critical philosophy, as Fichte had maintained.[21]

In order to distinguish his doctrine from "mere logic," Fichte believed that it was sufficient to show that abstraction, which was taken by Kant as the standard method for the formation of general concepts, is incompatible with the *Wissenschaftslehre*.[22] The simplest way of proving this incompatibility was by showing that abstraction is an inadequate method. "Our task," Fichte wrote, is "to examine factual knowledge with regard to what is, in it, a matter of intuition and what is a matter of thinking." Once this task is complete, Fichte hoped, one should

---

[20] "Erkläre ich hiermit: daß ich *Fichtes Wissenschaftslehre* für ein gänzlich unhaltbares System halte. Denn reine Wissenschaftslehre ist nichts mehr oder weniger als bloße *Logik*, welche mit ihren Principien sich nicht zum Materialen des Erkenntnisses versteigt, sondern vom Inhalte derselben als reine Logik abstrahirt." (Kant 1799, 370 (112))

[21] For a detailed discussion of Kant's criticism, see Martin 2003 (131).

[22] For Kant's view on the formation of general concepts, see his *Jäsche Logik* (Kant 1800, (113))

see that "The thinking in knowledge, the concept, call it what one may, does not become, but it is. The whole doctrine of logic about the formation of concepts through abstraction is thereby rejected as utterly false."[23] But why did Fichte believe that abstraction was an incorrect account of the formation of concepts? What does it mean to say that concepts simply are, and do not become? Let us look, first, at how he thought abstraction was supposed to proceed:

> The logicians produce concepts ... by abstraction. A horse, for example, is given in the factual intuition as something undetermined: I analyze its characteristics and find that it is similar to other things, which I have many times already observed, then I name the intuited thing a horse.[24]

Hence, on Fichte's view, abstraction seems to require an analysis of a given particular thing, to determine the individual characteristics that one wishes to abstract, as well as the individual characteristics that one wishes to abstract from. However, Fichte went on, the logicians usually forget to ask about the nature of these very characteristics. Some of these are, e.g., the place that a particular thing occupies, its magnitude, the law according to which it came to exist, and so on. But these characteristics, he noted, need other general concepts in order to be determined: the concepts of space, quantity, extension, law, etc. Therefore, without such general concepts, a particular thing cannot be conceived and, thus, an abstraction cannot be performed. But how are general concepts obtained then? Here is what

---

[23] "Unsere Aufgabe war, das faktische Wissen zu prüfen in Rücksicht dessen, was darin Sache der reinen Anschauung ist, und was des Denkens. ... Das Denken im Wissen, der Begriff, oder wie man es nennen will, wird nicht, sondern er ist. Die ganze Lehre der Logik von Entstehung der Begriffe durch Abstraktion fällt damit als durchaus falsch hinweg." (Fichte 1812, 316 (61))

[24] "Die Logiker lassen die Begriffe ... entstehen durch Abstraktion. Es ist in der faktischen Anschauung gegeben z.B. ein Pferd, als unbestimmtes Etwas: ich zerlege nun seine Merkmale, finde, da es ähnlich ist dem und dem anderen, welches ich schon mehrmals bemerkt habe, und nenne nun das also Angeschaute ein Pferd." (Fichte 1812, 331 (61))

Fichte thought that the logicians failed to see, and why they did so:

> Had they not analyzed, had they known that the concept, e.g., the concept of horse, exists solely in conceiving of some thing as a horse, i.e., in judging that the thing is a horse, they would have not arrived at this tasteless derivation. ... Had they found this originary existence of the concept solely in the originary conceiving itself, they would have also discovered that this conceiving and understanding as such must be based on a law, and that the whole conceiving of the horse must proceed thusly: such and such a determined thing is a horse; those determinations are given in factual intuition; therefore, the thing is a horse. Once they had grasped this, then wondering where the major premise (such and such a determined thing is a horse) came from would have undoubtedly driven them to realize that thinking in general is an intuition of the absolute laws, to which the fact conforms, but which are not themselves fact. Thus, the supersensorial nature of thinking and, with it, the transcendental philosophy would have come under clear light for them.[25]

Thus, to say that general concepts *are*, that they do not *become*, seems to mean that they are not abstracted from particular objects which are judged upon, but that concepts exist only through our acts of conceiving or judging. Thus, for example, the concept horse does not come into existence by abstracting from the particular characteristics of individual horses. The concept horse is rather produced through thinking an "absolute law," such as that expressed by the major

---

[25] "Hätten sie nicht zerrissen, hätten sie gewußt, da der Begriff, hier des Pferdes, nur ist im Begreifen eines Etwas als Pferdes, d.h. im Urtheilen, da es ein Pferd sei; so wären sie auf diese abgeschmackte Ableitung gar nicht gekommen. ... Hätten sie aber dies ursprüngliche Sein des Begriffs lediglich im ursprünglichen Begreifen selbst aufgefunden; so wärden sie auch wohl entdeckt haben, da diesem Begreifen und Verstehen, als solchem, ein Gesetz zu Grunde liegen müße; da das ganze Begreifen des Pferdes so einhergehen müße; ein Solches, so Bestimmtes ist ein Pferd; nun ist das in faktischer Anschauung Gegebene also bestimmt, mithin ist es ein Pferd. Hätten sie aber einmal dies begriffen, so würde die Verwunderung, woher denn der *maior*: ein so und so Bestimmtes ist Pferd, komme, sie ohne Zweifel zu der Erkenntnis getrieben haben, da das Denken überhaupt sei eine Anschauung absoluter Gesetze, nach denen das Faktum sich richtet, die aber selbst nicht Faktum sind; und so hätte ihnen das klare Licht aufgehen können über das übersinnliche Wesen des Denkens und damit die Transscendentalphilosophie." (Fichte 1812, 331 (61))

premise in the above passage: "such and such a determined thing is a horse." What Fichte seems to have suggested here is that the concept horse is obtained through the pure activity of positing through intellectual intuition. For a law like "such and such a determined thing is a horse" is, on his view, a positing (*Setzung*), a decree, whereby the concept horse is first introduced. Then, presumably, just like the concept horse, all other general concepts can be said to be posited by the mind, i.e., freely created independently of, and prior to, their application to empirical facts.

This conceptual creativity of the ego is what ultimately explains, in Fichte's opinion, the difference between pure logic and his *Wissenschaftslehre*. While logicians deploy abstraction as a procedure of concept formation, Fichte regarded abstraction as non-sensical. This justified, according to him, the claim that the *Wissenschaftslehre* is not "mere logic," but genuine transcendental philosophy:

> We stand on the same position as Kant in his Critique of Pure Reason. Kant had there the same opponent, which we fight against here [i.e., the dogmatic or transcendental realist]. But Kant abandoned him, and confronted another opponent: the investigation whether thinking is creative and producing itself the object is also critique of pure reason, where pure reason denotes to him [i.e., to Kant's new opponent, i.e., to Fichte himself] the self-creative thinking.[26]

However, Kant's criticism of Fichte does not, and could not, reject the creative activity of the ego. For, after all, Kant's own categories and the concepts of space and time, as well as other concepts like motion, force, and matter, are not obtained by means of abstraction from experience. Rather, they are produced by

---

[26] "Wir stehen auf demselben Standpunkt wie Kant in seiner Kritik der reinen Vernunft. Dort hatte Kant denselben Gegner, den wir hier bestreiten. Er ließ ihn stehen, und stellte ihm einen andern Gegner gegenüber: die Untersuchung, ob das Denken schöpferisch sei und selbst hervorbringend das Objekt, heißt eben Kritik der reinen Vernunft, indem reine Vernunft das selbstschöpferische Denken ihm bezeichnet." (Fichte 1812, 108 (61))

the mind prior to and independently from experience, as necessary conditions for the possibility of experience. What seems to have triggered Kant's criticism is Fichte's attributing to *intellectual* intuition a fundamental epistemic role. As is well known, Kant had rejected intellectual intuition as a necessary condition for the possibility of knowledge that can be obtained by beings like us.[27] Kant could not accept the claim that *all* concepts are freely created, and that this creative activity, through intellectual intuition, may be taken to be sufficient for knowledge. Kant was worried, in the first place, about the idea that the activity of the ego, as conceived of by Fichte, renders completely dispensable the sensibility of epistemic subjects endowed with cognitive faculties like ours.

In opposition to Kant, Fichte seems to have thought that all knowledge is *a priori*, obtained through the mind's free creative activity alone, i.e., through its intellectual intuition. He considered that the free creative activity of the mind is sufficient for the construction of "an image [...] of the real consciousness, which is present without any assistance from philosophy, and which we all have: in this consciousness, the same things should be found as are produced in the system, and they should stand in the same relations to one another as those produced in the system."[28] In fact, he put it quite bluntly: "The Wissenschaftslehre deduces a priori, without any consideration of perception, that which should occur only as a result of perception, and so a posteriori."[29] This view seems to entail that nothing

---

[27] Cf. Kant 1781/1787, B72 (111). See also Kant's own notes inserted in his copy of the first edition of the first *Critique* at A248, on page 346 of Guyer's translation.

[28] "ein Bild ... des wahren wirklichen, ohne alles Zuthun der Philosophie, vorhandenen Bewusstseyns, das wir alle haben: in diesem Bewusstseyn soll dasselbe Mannigfaltige liegen, und in eben dem Verhältnisse zu einander stehen, in welchem dasselbe in dem Producte eures Systemes steht." (Fichte 1801, 600 (60))

[29] "Die Wissenschaftslehre leitet sonach, ohne alle Rücksicht auf die Wahrnehmung, *a priori* ab, was ihr zufolge eben in der Wahrnehmung, also *a posteriori*, vorkommen soll." (Fichte 1801, 579 (60))

could be known through perception that cannot actually be known independently of perception. More exactly, it seems to entail not only that any concepts that one might think are obtained by abstraction from perceptual experience are actually freely created, but also that any perceptual content from which one might think abstraction derives these concepts is actually free created.

Given this peculiar aspect of Fichtean epistemology, the question arises what influence, if any, might it have had on Weyl, and especially on his own view about the introduction of theoretical concepts in physics. As we have noted above, Weyl came to believe that the introduction of such concepts like force, energy, and electromagnetic field, through the stipulation of fundamental presuppositions like Newton's laws and Maxwell's equations, rather than by abstraction from perceptual experience, is an indispensable condition for the formation of objective scientific theories. This seems to suggest that what Weyl found rightly emphasized in Fichte's *Wissenschaftslehre* is the emphasis on free creation as a method of concept formation.

Nevertheless, while he seems to have agreed with Fichte's emphasis on the free conceptual creativity of the mind, Weyl unequivocally rejected the idea that all knowledge is in fact *a priori*. He took Fichte's *Wissenschaftslehre* to be an "outrageous execution" of the project of constructivism, because in it "the *a priori* coincides ... in the end with the *a posteriori*."[30] No doubt, Weyl found Fichte's doctrine to be wrong insofar as it considers the free creation of concepts as sufficient for knowledge by completely ignoring the crucial role of perceptual experience in the process of knowledge acquisition. But Weyl seems to have realized that Fichte's view raises an important question about the nature of the

---

[30] "Das *a priori* fällt ... schließlich mit dem *a posteriori* zusammen." (Weyl 1954b, 643 (216))

connection between what is given in perception and what is freely created by the mind. As we will see in the next chapter, this correspondence needs to be properly articulated if the necessary conditions for our knowledge of the mind-independent world are to be identified.

Furthermore, one might argue that, in a certain sense, Fichte's constructivism falls as short as Hobbes' constructivism of providing an adequate account of objectivity. For if the purpose of the *Wissenschaftslehre* is indeed to show that the *a priori* coincides with the *a posteriori*, i.e., that nothing could be known through perception that cannot be known independently of perception, then, just like Hobbes, Fichte could not accommodate the way in which modern science goes beyond perceptual experience. On his view, modern physics would be crippled because its conceptual apparatus, although freely created, is confined to what one typically (although, according to Fichte, wrongly) believes to be empirical.

It seems, thus, plausible to think that Weyl came to believe that whereas Hobbes' constructivism collapses, as we have seen above, into Leibniz' monadological phenomenalism, Fichte's constructivism actually embraces this phenomenalism. As a matter of fact, this seems to have been a common interpretation of Fichte's *Wissenschaftslehre*, advocated, for example, by Schelling: "Since Leibniz, ... we see that the real, the finite, is generally placed in the region of the ideal. The whole real world has no existence in itself, but only in the representations of the soul. ... Fichte takes up this idealism, which is denial of the independent being of the real, and, in this regard, he does not go beyond Leibniz."[31] On this interpretation, then, Fichte's doctrine might support, at best, an intersubjective

---

[31]Quoted in Latta 1898, 182 (119). A reading of Fichte in the light of Leibniz' monadology has been defended also more recently: "Leibniz's conception of the monad is in many ways the true philosophical antecedent both of Fichte's notion of the I and of its essential ambiguities." (cf. Grondin 1994, 183ff (77))

agreement between different images of real consciousness, but cannot account for objectivity.

In a series of lectures on Fichte given in 1917, Husserl, too, seems to have endorsed this interpretation. He noted that "for him [i.e., for Fichte] the central theoretical question is that of the existence, or the type of existence, of the spatial-temporal reality, of the world in the natural [scientific] sense of the word,"[32] and then pointed out that Fichte's answer is based on the claim that "what the subject always has before itself, as substrate of action, as object of its activity, that must be immanent to the subject, it must be already stipulated," i.e., produced through the subject's creative activity.[33] But Husserl's own answer to the same question is quite different. To be sure, he agreed that what the subject has before itself, i.e., what is immediately available to it in experience, must be considered immanent, but he disagreed about what is it that the subject could be said to have before itself and, thus, about what could be considered immanent. Husserl's answer, which we discuss in the next section, exercised an important influence on Weyl, as will also be shown further below.

3.4   Husserl on Ideational Abstraction and Objectivity

In a lecture course given in the Spring semester of 1909, in which Husserl presented the view to be later developed in the first book of his famous *Ideas*, he stated the problem of the objectivity of our knowledge as follows:

---

[32] "Die theoretische Frage, die ... für ihn im Zentrum steht, ist die der Existenz oder Existenzart der raümlich-zeitlichen Wirklichkeit, der Welt im natürlichen Wortsinn." (cf. Husserl 1917, 271 (105))

[33] "Was immer das Subjekt vor sich hat, als Substrat des Handelns, als Objekt seiner Betätigung, das muß, als ein ihm Immanentes, selbst schon Erhandeltes sein." (cf. Husserl 1917, 275 (105))

> The main problem [is] the problem of how the knowing conscious-
> ness can, in its own flow of interrelated and variedly formed acts of
> knowledge, transcend itself and validly posit and determine an ob-
> ject, the constituents of which are not to be found immanently within
> consciousness, which never comes in consciousness to absolutely indu-
> bitable self-givenness, but nevertheless should, according to the mean-
> ing of natural science, exist in itself, whether it happens to be known
> or not.[34]

Hence, Husserl considered that it was the fundamental task of phenomenology
to give an account of the possibility of non-immanent objects, based on reflec-
tion on immanent acts. Taking his cue from Descartes, but reminding also of
Hobbes, Husserl maintained that addressing this task requires "phenomenological
reduction," i.e., a methodological annihilation of the world, followed by a recon-
struction on the basis of what is immediately given to our consciousness. More
precisely, the phenomenological reduction was meant to dislodge the natural atti-
tude of uncritical acceptance of the existence of mind-independent objects and of
our knowledge about them, an attitude that characterizes, according to Husserl,
the physical sciences.

Unlike Descartes, however, Husserl believed that the substantial ego of the
Cogito also needs to be methodologically annihilated, and thus, that what is imme-
diately given are just the ego's *cogitationes*: acts of thinking, willing, perceiving,
etc. Since the objects that are intended by such acts "never come in conscious-
ness to absolutely indubitable self-givenness," that is, since they transcend our
consciousness, objectivity cannot be attained on the assumption that such ob-
jects exist. Instead, Husserl seems to suggest that objectivity requires reflection

---

[34] "Das Leitproblem [ist] das Problem, wie das erkennende Bewusstsein in seinem Fluss man-
nigfach gestalteter und ineinander gewobener Erkenntnisakte sich selbst transzendieren und in
gültiger Weise eine Gegenständlichkeit setzen und bestimmen kann, die in ihm nach keinem Be-
standstück reell zu finden ist, in ihm nie und nirgends zu absolut zweifelloser Selbstgegebenheit
kommt, während sie doch dem Sinn der Naturerkenntnis gemäß an sich existieren soll, ob sie
zufällig erkannt wird oder nicht." (Husserl 1909, 73 (103))

on the very acts of the mind, since their being immediately present to the mind is absolutely indubitable.

One may, no doubt, immediately start worrying about the idea that mere reflection on mental acts may be sufficient for objectivity. What could be achieved by means of a method based on reflection upon such acts, and how could whatever is so achieved ensure the objectivity of knowledge, i.e., that our beliefs express relations between mind-independent objects? Of course, Husserl, himself, was aware of this difficulty, which he called "the puzzle of transcendence" (*Rätsel der Transzendenz*): in the sphere of immediately given acts "we see no possibility for establishing objectively valid statements and, thus, no possibility of science."[35]

Husserl's approach to this puzzle developed on the basis of the idea that, given the restriction imposed by the phenomenological reduction, objectivity should not be dependent on the actual existence of the objects intended by our acts of thinking, i.e., it should be attained *ohne Rekurs auf Setzung irgendeiner Transzendenz*. Furthermore, if it is to be objectively valid, a proposition should not refer to anything that changes when mental acts are changing, but should preserve an invariant meaning within the continuous flow of such acts. On his view, briefly put, objectivity requires that one obtain invariant, general ideas through reflection on our mental acts. In order to explain how this approach was thought to provide a solution to the problem of objectivity, and to properly introduce Husserl's method for obtaining general ideas, we should start from his criticism of traditional empiricist abstraction, a criticism referenced by Weyl in the aforementioned letter to Hölder.

---

[35] "In dieser Sphäre sehen wir keine Möglichkeit für die Etablierung objektiv gültiger Aussagen und somit für eine Wissenschaft." (Husserl 1909, 83 (103))

Husserl critically addressed British empiricism, in his Second Investigation.[36] He dismissed, for instance, Locke's account of abstraction as "nonsensical," on the ground that Locke considered the abstract idea of a triangle to be "neither oblique, nor rectangle, neither equilateral, equicrural nor scalene, but all and none of these at once." Husserl thought that this view was absurd, because it entails that an object could have contradictory properties and also that the abstract idea of a triangle is itself triangular. "Locke," Husserl maintained, "should, above all, have reminded himself that a triangle is something which has triangularity, but that triangularity is not itself something that has triangularity."[37]

Long before Husserl, of course, Berkeley had also rejected Locke's abstract ideas. In particular, Berkeley maintained that it is "useless and impracticable" to think that mathematical propositions hold of such ideas:

> Thus when I demonstrate any proposition concerning triangles, it is to be supposed that I have in view the universal idea of a triangle; which ought not to be understood as if I could frame an idea of a triangle which was neither equilateral nor scalenon nor equicrural. But only that the particular triangle I consider, whether of this or that sort it matters not, doth equally stand for and represent all rectilinear triangles whatsoever, and is in that sense universal.[38]

Berkeley safeguarded the generality of mathematical propositions by requiring that no particular features of a diagram be used in proofs: "It is true, the diagram I have in view includes all these [particular features], but then there is not the least mention made of them in the proof of the proposition."[39] Thus, Berkeley

---

[36]Cf. Husserl 1900 (102). According to Husserl, theories similar to those proposed by Locke and Berkeley came to be defended also by German-speaking philosophers like Erdmann, Twardowski, and Cornelius. For a discussion of the relevant historical-philosophical context, see Rollinger 1993 (153), Chrudzimski and Huemer 2004 (28).

[37]Cf. Husserl 1900, 359 (102).

[38]Cf. Berkeley 1710, 15 (12).

[39]Cf. Berkeley 1710, 16 (12).

defended a view according to which abstract ideas can and must be dispensed with, as unnecessary for an account of general mathematical knowledge.

On Husserl's reading, however, Berkeley's view amounts to no more than saying that a certain orientation of our attention, away from the particular aspects of a diagram – the concrete object presented to us in perception – is sufficient to guarantee the generality of mathematical knowledge. But Husserl rejected this sufficiency claim by denying Berkeley's contention that a particular triangle can be universal, in the sense that it can "stand for and represent" all other triangles.[40] According to Husserl, this contention is based on a confusion, i.e., on a failure to distinguish between the basis of abstraction and the result of abstraction, between a concrete object and a mathematical object. For one cannot disclose a mathematical object by an orientation of attention away from the particular aspects of the concrete object. One cannot do so, Husserl seems to have thought, because a concrete object remains always concrete, even when one does not pay attention to its particular aspects. Consequently, one cannot claim that mathematical knowledge is rendered general, as Berkeley did, even if one does not "mention" the particular aspects of a certain diagram, that is, more precisely, even if one denies them a role in the justification of mathematical theorems.

For our purposes here, more important than an evaluation of Husserl's criticism of Locke and Berkeley is a clarification of Husserl's claim that his own view is different from traditional empiricism. To be sure, he did not deny that abstract ideas are necessary for an explanation of the general character of our knowledge. But he thought that the empiricists failed to distinguish between a presentative or intending act of consciousness and an intentional object, i.e., the object intended

---

[40]Cf. Husserl 1900, 376f (102).

by that act, and believed that an abstract idea can be obtained from the object by disregarding some of its particular aspects. By contrast, on Husserl's view, a general idea is obtained by directing our attention to our own acts of consciousness, rather than to their intended objects. To clarify what this means, let us look at one attempt to explain, in one of his lectures, how general ideas can be obtained by reflecting on such acts:

> [A general essence is] that which remains identical in the mere 'repetition' of the act of consciousness, which in any case we can never register as actual repetition (as an exact identity relation), but can only suppose as an idealization in thinking. We obtain thus the ideal of absolute identity and, corresponding to this, an idea ... which constitutes the full essence of the singular act of consciousness."[41]

What Husserl had in mind here is clearly a method of abstraction. For he claims that a general idea is obtained on the basis of a certain relation between mental acts, which is, more precisely, a relation of "absolute identity," rather than one of "exact identity." Let us try to explain what this might mean.

Two mental acts are said to be exactly identical if and only if one of them actually repeats the other. By contrast, two mental acts are said to be absolutely identical if and only if one of them ideally repeats the other, rather than actually. A mental act is said to ideally repeat another mental act when it can, presumably, be imagined, rather than perceived, as repeating the latter. Thus, for example, one obtains the general idea of redness on the basis of the relation of absolute identity between our mental acts intending a red object, i.e., between an act intending a

---

[41] "[Eine allgemeine Idee ist] dasjenige ... was identisch bleibt in der bloßen 'Wiederholung' der *cogitatio*, die wir als wirkliche Wiederholung (als exakte Gleichheitskette) allerdings nie konstatieren, aber idealisierend-denkend supponieren können. Wir gewinnen dann das Ideal absoluter Gleichheit und ihr entsprechend eine Idee ..., die das volle Wesen der singulären *cogitatio* ausmacht." (Husserl 1909, 87f (103))

red object and an act that ideally repeats it. Similarly, the general idea of an equilateral triangle is obtained on the basis of the relation of absolute identity between an act of thinking about an equilateral triangle and an act that ideally repeats it.

Husserl's method seems to be, essentially, a process of abstraction. On his view, then, a general concept is introduced neither by isolating and/or disregarding certain features of an object given in sensuous perception, as the empiricists believed, nor by free creation through intellectual intuition, as Fichte maintained. Rather, a concept is obtained on the basis of the relation of absolute identity between mental acts. Husserl dubbed this method "ideational" abstraction:

> Naturally I do not mean abstraction merely in the sense of a setting-in-relief of some non-independent moment in a sensible object, but ideational abstraction, in which instead of the non-independent moment, its 'idea', its universal, is brought to consciousness, to actual being-given."[42]

How does ideational abstraction help one solve Husserl's puzzle of transcendence, i.e., the problem of scientific objectivity? By contrast with the general ideas of traditional empiricism, which are, as he put it, non-independent moments, that is, aspects of an object that are dependent on our experience of that object in sensuous perception, Husserl's general ideas are independent moments, that is, they are said to exist independently of such experience. Thus, one seems to be justified in believing that a scientific proposition may be considered objectively valid, i.e., capable of expressing relations about mind-independent objects, if it is

---

[42] "Natürlich meine ich hier nicht die Abstraktion in dem bloßen Sinne der Hervorhebung irgendeines unselbständigen Moments an einem sinnlichen Objekte, sondern die ideierende Abstraktion, in welcher statt des unselbständigen Moments seine 'Idee', sein Allgemeines zum Bewußtsein, zum *aktuellen Gegebensein* kommt." Cf. Husserl 1900, 52 (102)) Similar claims may be found in Husserl 1913, 74 (104).

formulated in terms of ideationally abstracted general ideas. But Husserl, himself, dismissed this belief:

> Scientific knowledge should be objectively valid and grounded in its objective validity. It cannot be empty intention, but should everywhere be based on grounds bestowing justification or validity. ... From all this we conclude that scientific knowledge is knowledge through judgment, that science rises on truth, that truth is laid in objectively valid judgments and grounded in the intuitive grounds of judgments.[43]

What Husserl seems to have meant here is that science may provide objective knowledge, i.e., knowledge about the mind-independent world, only if scientific propositions are justifiably believed to be true. On his view, as we have shown in section 2.5 above, one is justified to believe that a proposition is true only when one has an experience of its truth. Let us recall that, according to Husserl, an experience of truth is an epistemic achievement characterized by the complete satisfaction of one's meaning intentions expressed in a proposition, i.e., by presenting in intuition the objects and states of affairs meant by that proposition. This entails that one is justified to believe that a proposition is true only if the general ideas in terms of which the proposition is formulated are instantiated through the intuitive presentation of the objects and states of affairs meant by that proposition. For example, one is justified to believe the proposition "This is a red object" only if the idea of redness is instantiated by presenting in intuition the red object referred to by that proposition.

---

[43] "Wissenschaftliches Erkennen soll objektiv gültiges und in seiner objektiven Gültigkeit begründetes Erkennen sein. Es will nicht leeres Meinen sein, sondern überall auf die Recht oder Gültigkeit verleihenden Gründe zurückgehen. ... Gehen wir etwa davon aus, dass wissenschaftliches Erkennen urteilendes Erkennen ist, dass Wissenschaft auf Wahrheit geht, dass Wahrheit im objektiv gültigen Urteil gesetzt und in einsichtiger Urteilsbegründung begründet wird. (Husserl 1909, 93 (103)) Recall also the passage from the *Logical Investigations*, quoted in section 2.5 above: "Ultimately, all genuine, all scientific, knowledge rests on evidence, and as far as evidence extends, the concept of knowledge extends also." (Husserl 1900, 6 (102))

Thus, on Husserl's view, objectivity requires not only that scientific propositions be formulated in terms of general ideas, but also that the objects intended be presented in intuition as an instantiation of those general ideas. This is why he wrote that scientific knowledge "cannot be empty intention," i.e., that scientific propositions cannot be objectively valid if the meaning intentions that they express are not completely satisfied by objects actually given in intuition, that is, if the corresponding general ideas are not instantiated. Propositions formulated in terms of uninstantiated ideas cannot be objectively valid because they cannot be justifiably believed to be true.

Now, Husserl's approach to scientific objectivity recalls, we should perhaps note, Kant's famous adage in the first *Critique*: "Thoughts without content are empty, intuitions without concepts are blind. ... Only through their union can [objective] knowledge arise." (B75) Weyl, like other philosophers in the first decades of the twentieth century, criticized Kant by emphasizing that modern physics rendered the Kantian inventory of pure forms of understanding and sensibility obsolete. In his correspondence, Weyl noted that "the 12 categories are simply grotesque. Everywhere he [i.e., Kant] comes to speak about problems of natural science (space, matter), he remains far behind Leibniz."[44] Weyl took issue, in par-

---

[44] "Die 12 Kategorien sind einfach eine Groteske. Wo immer er auf Probleme zu sprechen kommt (Raum, Materie), die die Naturwissenschaft berühren, bleibt er weit hinter Leibniz zurück." (Manuscript Hs91:2. This is a letter to Magdalena Aebi, a student of Cassirer and friend of Gonseth and Bernays, and the author of a book titled *Kants Begründung der deutschen Philosophie.*)

ticular, with Kant's construction of the concept of matter.[45] But Weyl also took issue with the construction of the concept of Riemannian congruence in general relativity. He adopted Husserl's phenomenological approach and believed that this justified the elimination of that concept, which led to an important modification of the theory of general relativity, to which we turn now.

3.5 Weyl's Phenomenological Approach to Objectivity

Husserl's view, just presented above, suggested to Weyl, in 1918, an important modification of Einstein's theory of general relativity. To explain the nature of this modification, let us note that in the introduction to his famous *Space, Time, Matter*, Weyl wrote that the objects that general relativity refers to must be considered as merely intentional objects:

> The real world, each of its constituents and all their determinations are and can only be given as intentional objects of consciousness acts. The consciousness-experiences, which I have, are simply given, just as I have them. They do not consist, of course, in the mere stuff of sensations, as the positivists often state, but in a perception, e.g., there stands indeed corporeally an object for me to which that experience is related in a completely characteristic way known to everyone, but not more closely describable, which, following Brentano, should be

---

[45]Briefly put, in the *Metaphysical Foundations of Natural Science*, Kant rejected the view that matter is something solid that fills space and therefore has the property of impenetrability, for, he claimed, solidity does not entail impenetrability. He maintained that, in order to account for the latter, one needs to postulate (besides attractive forces) also some repulsive forces, which would help that which fills space resist penetration attempts. But Weyl pointed out that Kant's dynamical construction "hangs in the air" since it gives no indication how the force acting at the center of a body can be decomposed into attractive and repulsive forces, and thus, it allows for arbitrary decompositions (cf. Weyl 1949a, 169 (211)).

designated by the expression '*intentional object*'.[46]

It seems fair to say that he adopted a phenomenological approach to general relativity. Furthermore, at this stage in his career, Weyl was not only aware of Fichte's and Husserl's criticisms of empiricist abstraction, but also appears to have endorsed ideational abstraction as a method for the introduction of general concepts:

> In acts of reflection we are capable of bringing the *essence*, the *being-thus* of phenomena into prominence, to be noticed for itself, without de facto being able to detach it from the individual being of the intuitively given *in which* it appears. Here [is] the origin of *concepts*![47]

What Weyl seems to have described here is the phenomenological method of ideational abstraction, presented in the previous section, a method whereby general essences are obtained by reflection on our mental acts, rather than "detached" from the objects at which these acts are directed, as traditional empiricism maintained. According to Weyl, then, ideational abstraction is the proper method for the introduction of scientific concepts.

For example, the concept of inertial mass was introduced by Galileo on the basis of the relation of kinematic equivalence: two bodies have the same inertial mass when neither overruns the other when they collide with equal velocities. It seems

---

[46] "Die wirkliche Welt, jedes ihrer Bestandstücke und alle Bestimmungen an ihnen, sind und können nur gegeben sein als intentionale Objekte von Bewußtseinsakten. Das schlechthin Gegebene sind die Bewußtseinserlebnisse, die ich habe – so wie ich sie habe. Sie bestehen nun freilich keineswegs, wie die Positivisten vielfach behaupten, aus einem bloßen Stoff von Empfindungen, sondern in einer Wahrnehmung z. B. steht in der Tat leibhaft für mich da ein Gegenstand, auf welchen jenes Erlebnis in einer jedermann bekannten, aber nicht näher beschreibbaren, völlig eigentümlichen Weise bezogen ist, die mit Brentano durch den Ausdruck '*intentionales Objekt*' bezeichnet sein soll." (Weyl 1918b, 3 (192))

[47] "In Akten der Reflexion sind wir imstande, das *Wesen*, das *So-Sein* der Phänomene zur Abhebung zu bringen, für sich zu bemerken, ohne es doch von dem einzelnen Sein des jeweils anschaulich Gegebenen, *in dem* es erschient, de facto lösen zu können. Hier der Ursprung der *Begriffe*!" (Weyl 1920, 114 (195))

correct to suppose that, on Weyl's view, this concept emerges through reflection on acts of thinking about material bodies taken as intentional objects, rather than by detachment from these bodies themselves. Similarly, it may be suggested that, according to him, mathematical concepts are introduced (in immanent axiomatics, see section 2.8 above) through ideational abstraction. The concepts of group, field, etc., emerge through reflection on the acts of thinking about mathematical objects, rather than by abstraction from these objects themselves. For example, commutativity of addition $a + b = b + a$ between any elements a, b of a group is obtained through reflection on the acts of thinking 1+2=2+1, 2+3=3+2, and so on, rather than by abstraction from numbers.

The phenomenological approach to general relativity, as we will presently see, was taken by Weyl to justify the introduction of a non-Riemannian concept of congruence. On the basis of this concept, Weyl developed a "purely" infinitesimal geometry as a new mathematical framework for Einstein's theory, one that would allegedly allow us to attain objective knowledge in general relativity. Here is, briefly, Weyl's argument.[48]

In Riemannian geometry – the geometry of Einstein's theory of general relativity – the concept of congruence is defined on the basis of the assumption that one can always compare the length of two vectors, no matter what positions they occupy in the space-time manifold. Thus, for instance, one can compare the length of one vector with the length of another obtained from it by means of any congruent displacement through the (infinite) space-time manifold. This assumption, however, is unjustified; or so Weyl claimed. Finite displacements fail to support length comparison and, thus, fail to support Riemannian metrics.

---

[48]Cf. Weyl 1918b, ch. 2 (192), Weyl 1918c (193). For a technical presentation of Weyl's proposal of a purely infinitesimal geometry, see Fogel 2008, ch. 2 (64).

To defend his claim, Weyl associated an infinitesimal Euclidean space with every point in the space-time manifold – the so-called tangent space, considered to represent the space of intuition of an idealized observer, i.e., the space of those objects that can completely satisfy the meaning intentions of this observer's mind. In light of our discussion in the previous section, a scientific proposition in the general theory of relativity can be considered objectively valid, on Weyl's view, only if its truth can be experienced. Given the restriction of intuition to the tangent space, this view entails that a proposition can be considered objectively valid only if it refers to objects in the tangent space, for only those objects can actually be presented in intuition to completely satisfy the meaning intentions expressed in the proposition. But unlike the infinitesimal displacement of a vector, which always remains within the tangent space associated to its point of origin in the space-time manifold, a finite displacement takes a vector outside the tangent space.[49] Outside the tangent space, however, objects cannot be given in intuition and thus the meaning intentions of our idealized observer remain empty. In other words, the Riemannian concept of congruence remains uninstantiated by objects given in intuition. Therefore, no scientific proposition formulated in terms of this concept can be justifiably believed to be true and, thus, no such proposition can be objectively valid.

This is why Weyl thought that the Riemannian concept of congruence had to be eliminated. He proposed a modification of general relativity along the lines of a purely infinitesimal geometry. In other words, he introduced a new concept of congruence, according to which two vectors can be compared only by means of infinitesimal displacement, for only such a displacement remains within the

---

[49]The same point can be made, of course, with regard to infinite displacements.

tangent space of intuition. Thus, on Weyl's view, only those propositions in general relativity that contain the Weylean concept of congruence can be objectively valid. It seems right to say, then, that the epistemological significance of Weyl's phenomenological approach to Einstein's theory resides in the elimination of the Riemannian concept of congruence, and more generally, the elimination of all scientific propositions that lack intuitive evidence (in Husserl's sense).

Weyl's argument can be, of course, resisted in various ways. One could, for example, challenge Weyl's stupendous claim that the satisfaction of one's meaning intentions is possible only within the tangent space associated to every point in the space-time manifold. Indeed, it is not clear at all why one should accept Weyl's idealization of the space of intuition as an infinitesimal Euclidean space.[50] It is not clear, for that matter, why the phenomenologist should accept this idealization, either.[51] Be that as it may, before we turn to pointing out the problematic consequences of this phenomenological view, let us indicate other reasons for thinking that Weyl was indeed committed to, and never gave up, his phenomenological approach to scientific objectivity. To bring such reasons to light, one has to advert to his later writings. So, in the remainder of this section, we want to present the conception of objectivity offered in chapter thirteen of Weyl's *Philosophy of Mathematics and Natural Science* – one of the few chapters that were almost entirely rewritten, rather than merely translated, for the 1949 English edition of the book – and to present the support that might be given for a phenomenological interpretation of this conception. In the next section, we will criticize this interpretation and argue, more generally, that Weyl ultimately rejected the phenomenological approach to scientific objectivity.

---

[50]Cf. Howard 2005 (98)

[51]Cf. van Atten 2008 (182).

Weyl attempted to identify, in chapter thirteen of his book, a necessary condition for objectivity. A proposition is objectively valid, he maintained, only if, when complete, its truth value does not change when the semantic value of its constituent parts changes. Here is how he put it:

> Our knowledge stands under the norm of *objectivity.* ... Epicurus certainly thought that the vertical is objectively distinguishable from all other directions. He gives as his reason that all bodies when left to themselves move in one and the same direction. Hence the statement that a line is vertical is elliptic or incomplete, the complete statement behind it being something like this: the line has the direction of gravity at the point P. Thus the gravitational field, which we know to depend on the material content of the world, enters into the complete proposition as a contingent factor, and also an individually exhibited point P on which we lay our finger by a demonstrative act such as is expressed in words like 'I,' 'here,' 'now,' 'this.' Only if we are sure that the truth of the complete statement is not affected by free variation of the contingent factors and of those that are individually exhibited (here the gravitational field and the point P) have we a right to omit these factors from the statement and still to claim objective significance for it. Epicurus's belief is shattered as soon as it is realized that the direction of gravity is different in Princeton and in Calcutta, and that it can also be changed by a redistribution of matter.[52]

In other words, the proposition 'This line is a vertical line' can be considered to describe an objective state of affairs only if we know that the truth of its complete version, 'This line is a vertical line, relative to the direction of gravity at this point,' is independent of any changes of the semantic value of its indexicals. Furthermore, we also have to know that its truth is independent of any contingent factors, like the distribution of matter in the world. For, if the distribution of matter changes, our proposition may change its truth value. Thus, objectivity, on Weyl's view, requires not only independence from the semantic value of indexicals, but also

---

[52]Cf. Weyl 1949a, 73 (211).

independence from contingent factors.

To meet this double independence requirement, different frames of reference or coordinate systems, have to be taken into account. A scientific proposition is objective, then, only if we know that its complete version remains true across different frames of reference; in other words, only if the complete proposition remains true when relativized to different coordinate systems. According to this criterion, objectivity may be attained in classical mechanics by Galilean transformations, in special relativity by Lorentz transformations, and in general relativity by Einstein's coordinate-free formulation of the fundamental equations.

But here one should note that, from early on, in his *Das Continuum*, Weyl had defended the following view about the nature of a coordinate system:

> The coordinate system is the unavoidable residue of the annihilation of the ego in that geometrico-physical world which reason sifts from the given under the standard of 'objectivity – a final scanty token in this objective sphere that existence is only given and *can* only be given as the intentional content of the conscious experience of a pure, sense-giving ego.[53]

This idea shows up again in later works, including his 1927 monograph, as well as in chapter thirteen of its 1949 English translation, where Weyl similarly noted that "the objectification, by elimination of the ego and its immediate life of intuition, does not succeed without remainder, and the coordinate system remains as the necessary residue of the annihilation of the ego."[54]

---

[53]Cf. Weyl 1918a; Eng. tr., 94 (191).

[54]"Die Objektivierung durch Ausschaltung des Ich und seines unmittelbaren Lebens der Anschauung gelingt nicht restlos, das nur durch eine individuelle Handlung (und nur approximativ) aufzuweisende Koordinatensystem bleibt als das notwendige Residuum der Ich-Vernichtung." Weyl 1927a, 57 (198); Eng. tr., 75 (211).

The apparently pervasive theme of the annihilation of the ego, and the idea that the coordinate system is the unavoidable residue of such an annihilation, have been recently considered as an indication of Weyl's *definitive* commitment to a phenomenological conception of objectivity.[55] What is taken to justify this commitment is an interpretation of the coordinate system as representing the perspective of transcendental subjectivity, i.e., the perspective that survives the phenomenological reduction. Weyl's theme is, thus, taken to give an "explicit recognition to the thesis of transcendental subjectivity,' the purified consciousness that is the residue of the phenomenological reduction."[56] Transcendental subjectivity, it is concluded, conceived of "in Husserl's sense of the 'absolute being' of 'pure consciousness' surviving the phenomenological reduction, plays the fundamental role in Weyl's understanding of the constitution of objectivity in physical theory."[57]

This interpretation, one must admit, fits very nicely with Weyl's own early declarations, in the preface to *Space, Time, Matter*, endorsing Husserl's phenomenological approach (and quoted at the beginning of the present section). On this interpretation, then, saying that a scientific proposition can be considered objective only if we know that its complete version remains true when relativized to different coordinate systems is tantamount to saying that that proposition is objective only if we know that its complete version remains true for any transcendental subjectivity. But we should note that, on Husserl's view, to know that a proposition is true for any transcendental subjectivity is tantamount to having intuitive evidence for it:

---

[55]Cf. e.g. Ryckman 2005, especially 5.3.4 (159).

[56]Cf. Ryckman 2005, 119 (159).

[57]Cf. Ryckman 2005, 110 (159).

> If someone experiences the evidence of A, it is *evident* that no second person can experience the absurdity of this same A, for that A is evident means that A is not merely meant, but also genuinely given, and given as precisely that which is meant. In the strict sense, it is itself present.[58]

Hence, on the phenomenological interpretation of Weyl's conception of scientific objectivity, a proposition is objective only if it is intuitively evident, that is, in light of our discussion in section 2.5 above, only if one has an experience of its truth, in Husserl's sense. On this interpretation, Galilean and Lorentz transformations, as well as Einstein's general covariance, should be considered as means for satisfying the requirement that scientific propositions be intuitively evident. This suggests, in particular, that general covariance should be understood as a means for overcoming epistemic relativism by guaranteeing the intersubjectivity of the scientific propositions of the theory of general relativity.

However, on Einstein's own view, general covariance was not to be understood in this way. Rather, it was meant to guarantee that the theory has physical content, i.e., that "point coincidences" such as that of a pointer with a scale are preserved under coordinate transformations. In other words, general covariance was meant to guarantee that the propositions of general relativity are objectively valid.[59] Weyl, himself, eventually realized the fact that phenomenology can support at most intersubjectivity, and this led him to believe that Husserl's conception offers an inadequate account of natural science. Or at least this is what we want

---

[58] "Erlebt jemand die Evidenz A, so ist es *evident*, daß kein zweiter die Absurdität desselben A erleben kann; denn, daß A evident ist, heißt: A ist nicht bloß gemeint, sondern genau als das, als was es gemeint ist, auch wahrhaft gegeben; es ist im strengsten Sinne selbst gegenwärtig." (Cf. Husserl 1900, VI, 39 (102))

[59] The view that general covariance should be understood as a means for overcoming epistemic relativism had been also proposed by Joseph Petzoldt, but criticized by Einstein himself. For discussion, see Howard 1992, 172ff (96). For a recent discussion of Einstein's general covariance, see Norton 2003 (141).

to argue in the next section. This argument is important, we believe, the more so as it seems to have been unduly ignored in the relevant literature.

3.6 Weyl's Turn Against Husserl on Objectivity

Weyl's departure from a phenomenological approach to scientific objectivity might be thought to be due to his interests in physics in the mid 1920s switching from general relativity to quantum mechanics. For, one may suppose, he realized how difficult it is to assign a fundamental role to transcendental subjectivity in a quantum mechanical context.[60] However, his rejection of the phenomenological approach to objectivity need not (although it might) have been motivated by his reflections on quantum mechanics. As we argue below, this rejection may be shown to have been based on the observation that already in classical physics one deploys a method of concept formation that indicates a radical separation of conceptual content from what can be intuitively presented to the transcendental subjectivity.

Let us first note that what might have also led Weyl to reject the phenomenological approach to objectivity was an interpretation of Husserl's phenomenology in the light of Leibniz' monadology. Husserl, himself, seems to have regarded monadology as a forerunner of his own phenomenology. In his lecture courses from 1923/1924 he remarked, for example, that

> Leibniz has grasped and used metaphysically the fundamental proper-
> ties of intentionality, by discussing the fundamental properties of the

---

[60]This supposition does not mean, of course, that assigning a fundamental role to transcendental subjectivity in quantum mechanics is impossible. Indeed, to give just one example, Fritz London and Edmond Bauer proposed the controversial view that it did have such a role in explicating the measurement problem (cf. London and Bauer 1939 (123). See also French 2002 (69)).

monad under the titles of perception, the struggle to effect the transition from perception to perception, and, especially, the representation of what is not immanently present but still perceptually given to consciousness.[61]

Furthermore, Husserl's own student, Dietrich Mahnke, was among the first to point out that phenomenology can be seen as a *Neuleibnizianismus*.[62] More recently, it has been similarly emphasized that Husserl defended "a Leibnizian sort of monadology as the condition of the possibility of objectivity."[63] But even independently of this interpretation of Husserl's phenomenology, we believe Weyl had strong reasons to depart the phenomenological conception of scientific objectivity.

As we have noted above, Weyl realized that, already in classical physics, concepts are introduced that are not abstracted from the properties of objects immediately presented in intuition, but are freely created, i.e., implicitly defined through the stipulation of the basic principles of a theory, like Newton's laws of motion or Maxwell's equations. The character of the introduction of theoretical concepts in physics justified, as we have seen, Weyl's rejection of the traditional empiricist approach to scientific objectivity, as represented by Hobbes' constructivist view, as well as Weyl's critical remarks on Leibniz' monadological phenomenalism. Weyl criticized Hobbes on account of his inadequate account of the formation of scientific concepts. Abstraction, he argued, renders scientific concepts a "residue of experience" and, thus, cripples physics by rendering it unable to validly determine objective states of affairs and relations between mind-independent objects. Weyl

---

[61] "Leibniz hat bei der Erörtung der Grundeigenschaften der Monade unter den Titeln Perzeption, strebender übergang von Perzeption zu Perzeption und insbesondere Repräsentation von reell nicht Gegenwärtigem und doch perzeptiv Bewußtem die Grundeigenschaften der Intentionalität erfaßt und metaphysisch verarbeitet." (Husserl 1923/1924, 196f (106))

[62] Cf. Mahnke 1920, 249 (127))

[63] Mohanty 1995, 46 (137). For a discussion of several lines of connection between Husserl and Leibniz, see the articles in Cristin and Sakai 2000 (30).

also criticized Leibniz' monadology for reducing natural science to an investigation based upon the harmonious connection between what is immediately given to different consciousnesses, thereby advocating phenomenalism and upholding a conception of objectivity as intersubjectivity.

The introduction of concepts by free creation led Weyl to believe that what is achieved in theoretical physics "is not an intuitive insight into particular or general states of affairs and a *description* that faithfully copies the given, but a theoretical, in the last analysis purely symbolic *construction* of the world."[64] Thus, for Weyl, physics is a constructive activity that has two main characteristics: the construction that physics achieves is a "purely symbolic" one, and it is a construction "of the world." One might think that Weyl made here a confusion that is common to, and commonly charged of, certain constructivist views – that he is guilty of blurring the distinction between scientific theories and the real world.[65] However, Weyl's notion of a symbolic construction of the world is epistemological, rather than metaphysical. In other words, as we will see more clearly in the next chapter, this notion should be taken to emphasize that physics aims at constructing an objective *image* of the real world, in mere symbols, rather than the world itself.

But this merely indicates that Weyl came to reject the phenomenological approach to scientific objectivity, which he had endorsed in the *Space, Time, Matter* and, as we have seen, as far as the 1949 English translation of his 1927 mono-

---

[64]"Was hier [in der theoretischen Physik] geleistet wird, ist nicht anschauende Einsicht in singuläre oder allgemeine Sachverhalte und eine das Gegebene treu nachzeichnende *Deskription*, sondern theoretische, letzten Endes rein symbolische *Konstruktion* der Welt." (Weyl 1928a, 149 (200))

[65]For this criticism of constructivism, see Devitt 1997, 241f (40). This is a charge often directed at social constructivism about science, but also at certain remarks by philosophers of science like Thomas Kuhn and Nelson Goodman. For a more recent view according to which in quantum physics one "produces" the real world, see Pickering 1984 (147).

graph. More precisely, it indicates his rejection of the view that objectivity is to be based on a relativization to coordinate systems as the perspectives of transcendental subjectivities surviving the annihilation of the ego by phenomenological reduction. But what justifies this rejection?

In order to reveal the main reason that appears to have justified this rejection, let us note that Husserl, himself, acknowledged that natural science cannot dispense with hypothetical elements, as these play a crucial role in scientific explanation. However, he made a distinction between the positing of unobserved entities and the positing of (what he took to be) *in principle* unobservable entities:

> An explanation of perceptually given processes by means of hypothetically assumed causal realities, by means of unknown objects (as, e.g., the explanation of certain planetary disturbances by means of the assumption of a new still unknown planet, Neptune) is something different in principle from an explanation in the sense of a physical determination of experienced things through physical explanatory means as atoms, ions, and the like.[66]

Husserl denied that the world of physics is different than that of the objects given in perceptual experience, and maintained that physics cannot justify the assumption of "an *unknown* world of physical realities ... explaining the appearances *causally.*" Hypothetical elements, he added, are not the "*symbolic representative* of something hidden," something that is bound to remain unknown because it is in principle unobservable. For him, it is nonsensical to posit any in principle

---

[66] "Eine Erklärung der wahrnehmungsmäßig gegebenen Vorgänge durch hypothetisch angenommene Ursachrealitäten, durch unbekannte Dinglichkeiten (wie z.B. die Erklärung gewisser planetarischer Störungen durch die Annahme eines noch unbekannten neuen Planeten Neptun) etwas prinzipiell anderes sei, als eine Erklärung im Sinne physikalischer Bestimmung der erfahrenen Dinge und durch physikalische Erklärungsmittel nach Art der Atome, Ionen u. dgl." (Husserl 1913, 111 (104))

unobservable and, thus, unknowable real entities, as causing what is presented in perception. For, he seems to have thought, it is a mistake to extend the scope of causality beyond the realm of intentional objects. "Atoms, ions, and the like," are, according to Husserl, categories of thought whereby the physicist may attempt to determine physical reality, but is actually unable to give them any intuitive significance:

> Not even a Divine physics can make simply intuited determinations out of categorial thought-determinations of realities, any more than a Divine omnipotence can bring it to pass that someone paints elliptic functions or plays them on the violin.[67]

But the view about hypothetical elements, suggested in this passage, appears to be in conflict with Husserl's own view about the conditions required for scientific objectivity, which we discussed in section 3.4 above. For how can any scientific propositions about hypothetical elements be objective, if objectivity requires that these propositions should be intuitively evident?

As we have seen already, on Husserl's view, a scientific proposition can be objective only if it is justifiably believed to be true, i.e., only if its truth is experienced. In other words, a proposition is objective only if objects can be presented in intuition that completely satisfy the meaning intentions expressed by that proposition. But Husserl seems to have thought, at the same time, that the meaning intentions expressed by propositions that refer to certain hypothetical elements cannot be satisfied, that they are merely empty intentions. For, whereas unknown planets can be at least in principle observed, atoms and ions (or, more accurately, things like forces and fields) are in principle unobservable entities. Clearly, on

---

[67] "Auch eine göttliche Physik kann aus kategorialen Denkbestimmungen von Realitäten keine schlicht anschaullichen machen, sowenig göttliche Omnipotenz es machen kann, daß man elliptische Funktionen malt oder auf der Geige spielt." (Husserl 1913, 52 (104))

Husserl's own view, this entails that no scientific proposition about the latter can be objective, since no such proposition can be intuitively evident.

Husserl's own life-long attempt to solve this conflict undoubtedly deserves further discussion, within a larger context of a general analysis of the phenomenological approach to the epistemic role of hypothetical and transfinite elements in science. Also, it is no less important to address the question whether the phenomenological method of concept formation – the ideational abstraction – *can* allow the introduction of hypothetical and transfinite elements, in the first place.[68]

While we appreciate the importance of such questions for the phenomenology of mathematics, we want to focus here on Weyl's own attempt to come to terms with the conflict raised by Husserl's view on objectivity. We want to discuss, in the next chapter, what we take to be Weyl's ultimate answer to the question about scientific objectivity, that is, his attempt to identify exactly the necessary conditions under which a scientific proposition can, *despite its lack of intuitive evidence*, express relations between real, mind-independent objects.

3.7   Conclusion

We discussed, in this chapter, the relation between concept formation and scientific objectivity. In particular, we focused on Weyl's remarks on abstraction, and presented his criticism of the traditional empiricist view about physics, as illustrated by Hobbes's constructivism. According to Weyl, traditional empiricist abstraction fails to account for what Weyl took to be a necessary condition for

---

[68]It is perhaps worth noting that Oskar Becker argued that Husserl's method is insufficient for the introduction of transfinite elements. Becker believed that these could be introduced by means of an extension of ideational abstraction, by what he called the "transfinite structure-complication of pure consciousness," i.e., by phenomenological reflection on the potentially infinite iteration of our own mental acts (cf. Becker 1927 (9)).

scientific objectivity: the introduction of hypothetical elements. Then, we noted that Weyl was more attracted to Fichte's constructivism due to its emphasis on the freedom of the mind to create concepts independently of our perceptual experience, rather than by abstraction from experience, but we also explained Weyl's misgivings with Fichte's view.

Afterwards, we discussed the early influence exerted by Husserl's phenomenological philosophy on Weyl's own conception of scientific objectivity. According to this philosophy, the formation of scientific concepts requires a reorientation of attention away from objects and toward the immanent and, therefore, indubitably given, acts of consciousness. Weyl's implementation of the phenomenological method of ideational abstraction led to a purely infinitesimal geometry, which was, according to him, needed for establishing the objectivity of the general theory of relativity. Later on, however, Weyl came to believe that the phenomenological method is at variance with what physicists actually do when they do physics, i.e., with the positing of real but in principle unobservable entities, and came to maintain, against Husserl, that intuitive evidence cannot be a necessary condition for objectivity. Weyl seems to have realized that, just like Hobbes' and Fichte's constructivisms, Husserlian phenomenology cannot go beyond Leibniz' monadological phenomenalism.

In the next chapter, we discuss what we take to be Weyl's ultimate view on objectivity. As we will see, he came to believe that scientific theories should be understood as systems of symbols (by analogy, as he pointed out, with Hilbert's transcendent axiomatization of mathematics), but that such systems can provide objective knowledge only if they are categorical. We present some reasons for thinking that categoricity fails to obtain, and then suggest how objectivity may

be obtained without categoricity.

CHAPTER 4

OBJECTIVITY, CATEGORICITY, AND UNITARY INEQUIVALENCE

4.1 Introduction

In this chapter, we want to clarify Weyl's ultimate view on objectivity, that is, his view on the conditions under which a scientific theory may provide knowledge of the mind-independent world. This clarification will strengthen our claim that, according to him, there is a fundamental tension between scientific objectivity and intelligibility.

We start by presenting Weyl's reasons for thinking that Hilbert's transcendent axiomatics fails to meet the conditions for scientific understanding and may, therefore, be taken for a mere game with symbols on paper. Weyl came to maintain, as we will see, that the mathematical concepts of transcendent axiomatics (including its transfinite components), even if unable to help us understand why a theorem is true, partake in the theoretical construction of the mind-independent world, and they do so "in the same way" as the scientific concepts (including the hypothetical elements) do. This seems to suggest that his view was that, just like the hypothetical elements, transfinite components are indispensable for the formation of a scientific theory that aims at providing knowledge about the mind-independent world. In other words, Weyl came to think that the free creation of concepts, i.e., their introduction as symbols through the positing of the basic principles of

the theory, is a necessary condition for objectivity. The natural question arises, however, what further condition(s), besides their being freely created, should such concepts satisfy, if objectivity is to be attained?

Weyl believed that objectivity further requires categoricity, i.e., a univocal coordination between theory and its domains of interpretation, up to isomorphism. A physical theory is objective only if it can be univocally coordinated to our perceptual experience, and only if this experience can be isomorphically mapped to the mind-independent world. We show that Weyl took the criterion for the univocal coordination between theory and experience to be expressed by a concordance requirement, i.e., that all methods for determining the value of an observable quantity must lead to the same result (within the limits of experimental error), and that his emphasis on the isomorphism between experience and the mind-independent world implies that only the relations between mind-independent objects, rather than these objects themselves, may be epistemically accessible to us. This view entails, as we will explain, that categorical theories cannot bring about understanding of natural phenomena. But it also entails, as we will see, that categoricity is actually not enough for objectivity, unless one presents convincing reasons that the mind-independent world may be considered an interpretation of a categorical theory.

To be sure, Weyl's view about categoricity as a requirement for objectivity seems less than tenable, since results like the Löwenheim-Skolem theorem and Gödel's first incompleteness theorem showed that natural first-order formalizations of most common mathematical theories are not categorical. In fact, Weyl himself came to doubt that physical theories are categorical. One reason for doubt seems to have been the fact that his view runs against an established theoreti-

cal result in quantum physics – the failure, under certain circumstances, of the Stone-von Neumann theorem. This failure entails that the algebraic structure of a quantum field theory has an infinite number of unitarily inequivalent representations, with physical quantities taking different values in each unitary equivalence class of representations. This seems to imply that, in an appropriate sense, such a theory fails to be categorical and, thus, fails to provide objective knowledge. Ultimately, as we will see, Weyl remained unsettled with regard to scientific objectivity.

Recently, however, several responses have been given to the challenge raised by unitary inequivalence. First, one defended the idea that quantum theories should be adjusted so that unitary equivalence is restored. But we argue that any adjustment that purports to eliminate all but one unitary equivalence class of representations is problematic, since it renders theories unable to account for natural phenomena that they were designed to account for (such as, e.g., phase transitions). Secondly, one proposed the view that, while the unitarily inequivalent representations of the algebraic structure of a theory should not be eliminated, the theory can provide objective knowledge in spite of its lack of categoricity. This would require that the structure of the theory be expanded such that the real world is described by the structure of the relations between unitarily inequivalent representations, rather than by the algebraic structure of the theory or by any of its unitarily inequivalent representations. The expanded structure is taken to span a whole range of physically possible worlds, each of which is described by an unitarily inequivalent representation. But we point out that it is not clear that this expansion is sufficient for objectivity, since nothing seems to guarantee that some unitarily inequivalent representations that describe physically possible

worlds are not within that range. However, if a criterion is found for identifying all representations that may be assumed to describe physically possible worlds, this would justify the belief that science provides knowledge of the mind-independent world, but a world that is, in a sense to be explained, modally dappled.

4.2 Weyl on Transcendent Axiomatics and Objectivity

In this section, we discuss Weyl's reasons for believing that Hilbert's formal or, as Weyl put it, transcendent axiomatics, despite its alleged failure to bring about mathematical understanding, is nevertheless scientifically significant. More specifically, we want to clarify why Weyl believed that, although it cannot have an *explanatory* role, transcendent axiomatics has an essential *objectifying* role.

"Transcendent axiomatics," Weyl wrote toward the end of his career, "has something paradoxical and shocking, because one must try to learn to abstract radically from the familiar intuitive significance of the terms occurring in the axioms as undefined concepts."[1] What seems to have always appeared to him paradoxical and shocking is the fact that in a formal system, like the one Hilbert gave in 1900 for real analysis, for example, the axioms are taken to be (partially) uninterpreted statements, rather than intuitively evident, and their stipulation is constrained only by such metatheoretical requirements as consistency, independence, and fruitfulness. Weyl was opposed, in his very first writings, to Hilbert's claim that classical analysis is just one interpretation of the formal system and that in order to justify the existence of real numbers as a complete totality one

---

[1]Cf. Weyl 1985, 14 (218).

needs to prove the axioms consistent.[2] Later, after Hilbert started to think about proving consistency syntactically, by proof-theoretical methods, Weyl considered that mathematics would thereby be transformed into a mere game with meaningless symbols:

> That from [the constructivist] point of view only a part, perhaps only a wretched part, of classical mathematics is tenable is a bitter but inevitable fact. Hilbert could not bear this mutilation. ... he succeeded in saving classical mathematics *by a radical reinterpretation of its sense* without reducing its inventory, namely, by formalizing it, thus transforming it in principle from a system of intuitive statements into a game with formulas that proceeds according to fixed rules.[3]

It is worth noting that, for Weyl, at the time, it was not the potential lack of success that threatened Hilbert's attempt to save classical mathematics. The major problem was, according to Weyl, that the attempt turns mathematics into a game: "Mathematics becomes, in Hilbert's theory, a game with signs and formulas; the formulas, which consist of signs, have no meaning which they wish to convey, but they are the material of the game of demonstration: according to the rules of the game new formulas are constructed from those already at hand."[4] However, this remark seems to overlook the fact that not all of Hilbert's signs and formulas were meaningless, but only those of ideal (or transfinite) mathematics. It also

---

[2]Weyl maintained that the concept of real number, although clearly defined by the formal axioms, is not extensionally definite, i.e., that, unlike the set of natural numbers, the set of real numbers is not, as Hilbert had claimed, a complete totality, i.e., a maximal class of objects. See Weyl 1919 (194).

[3]"Daß von [dem intuitionistischen] Standpunkt aus nur ein Teil, vielleicht nur ein kümmerlicher Teil der klassischen Mathematik zu halten ist, ist eine bittere, aber unumgängliche Tatsache. Hilbert ertrug diese Verstümmelung nicht. ... es ihm gelang, die klassische Mathematik *durch eine radikale Umdeutung ihres Sinnes* ohne Minderung ihres Bestandes zu retten, nämlich durch ihre Formalisierung, durch welche sie, prinzipiell gesprochen, sich aus einem System einsichtiger Erkenntnisse verwandelt in ein nach festen Regeln sich vollziehendes Spiel mit Formeln." (Weyl 1928a, 148; Eng. tr., 483 (200))

[4]Cf. Weyl 1929, 155 (202).

assumes that operations with meaningless signs and formulas cannot be seen as something other than moves in a cognitively irrelevant game. This assumption is, in fact, what motivated Weyl to think that formalization raises a difficult epistemological problem, for he believed that comparing mathematics to a game like chess sheds a distorting light upon the activity of the mathematician:

> Ever since Newton in his modesty spoke of playing with pebbles on the shore of a wide ocean, the attitude that we mathematicians play a nice game that ought not to be taken too seriously has enjoyed considerable popularity. In my opinion, it is fundamentally unsound. Whatever analogies there are between the mental activities of a mathematician and a chess player, the problems of the former are serious in the sense that they are bound up with basic truth, truth about the world that is and truth about our existence in the world.[5]

Weyl seems to have implied that, if Hilbert's mathematics is to be considered at all mathematics, it was imperative that one explain how its problems are serious, in the sense indicated in this passage, it was imperative that one clarify its cognitive significance: "Without doubt, if mathematics is to remain a serious intellectual concern, then some *sense* must be attached to Hilbert's game of formulae."[6] If one is unable to reveal its sense, so was Weyl's worry, formalized mathematics would have to be regarded as just a game, isolated from our epistemic efforts and maybe even unworthy of serious intellectual pursuit. Hence, Weyl raised the following question: "Hilbert's mathematics may be a pretty game with formulas, more amusing even than chess; but what does it have to do with knowledge, since its formulas should admittedly have no contentual significance by

---

[5]Cf. Manuscript Hs91:17, 16.

[6]"Ohne Zweifel: soll Mathematik eine ernsthafte Kulturangelegenheit bleiben, so muß sich mit dem Hilbertschen Formelspiel irgendein *Sinn* verknüpfen." (Weyl 1925, 540 (197); Eng. tr., 140 (197))

virtue of which they would express intuitive truths?"[7] This very question seems to indicate, again, that Weyl merely overlooked the fact that not all of Hilbert's formulas lack contentual significance. Furthermore, one can answer the question simply by pointing out that transcendent axiomatic proofs, i.e., proofs that deploy unbounded existential quantification over infinite domains, and so, formulas without contentual significance, must have a contentual output. Thus, even such formulas do have cognitive relevance, as instruments used in the derivation of formulas with contentual significance. But what Weyl seems to have been worried about is that a mathematical proof can have contentual output only if it is wholly contentual, or contentual in all its parts. Hence, he seems to have believed that only if mathematical reasoning is wholly contentual can mathematics remain a serious intellectual concern. This further indicates that Weyl failed to grasp the epistemological import of Hilbert's commitment to proving the reliability of non-contentual or purely symbolic mathematical reasoning.[8]

At any rate, a more serious concern raised by Weyl is that non-contentual or purely symbolic mathematical reasoning lacks any explanatory power. As we have seen in section 2.8 above, on his view, only immanent axiomatic proof can bring about mathematical understanding, since only this type of proof may possess the transparency demanded for understanding. The transparency of an immanent axiomatic proof is conceived of in Husserlian terms, as an experience of truth, that is, as an epistemic achievement of a complete satisfaction of the prover's meaning intentions. In other words, Weyl believed that one can understand a

---

[7] "Die Hilbertsche Mathematik mag ein hübsches Formelspiel sein, amüsanter selbst als das Schachspiel; aber was hat sie mit Erkenntnis zu tun, da doch eingestandenermaßen ihre Formeln keine inhaltliche Bedeutung haben sollen, derzufolge sie einsichtige Wahrheiten auszudrückten?" (Weyl 1927a, 49 (198); Eng. tr., 61 (211))

[8] We come back to this issue in section 5.4 below.

mathematical theorem only if its proof provides a construction of the objects and relations referred to in the theorem from the objects and relations referred to in the axioms. But, as it was clear to him, Hilbert's transcendent axiomatic proofs do not provide such constructions, but deploy non-contentual or purely symbolic reasoning, and so they cannot be regarded as an experience of truth. Transcendent axiomatic proofs are partly opaque, they fail to completely satisfy the prover's meaning intentions. Therefore, Weyl concluded, such proofs cannot bring about mathematical understanding. From this, he seems to have concluded that they have no cognitive relevance and may be considered a mere formula game.

Hilbert, quite naturally, resisted the formula game view of mathematics and repeatedly pointed out that the scientific relevance of formal axiomatics is given by its use as an instrument of great scientific and epistemic value. For instance, in a paper presented in 1927 to the mathematical seminar at the University of Hamburg, he attacked the game view in the following way:

> The formula game ... has, besides its mathematical value, an important general philosophical significance. For this formula game is carried out according to certain definite rules, in which the *technique of our thinking* is expressed. These rules form a closed system that can be discovered and definitely stated. The fundamental idea of my proof theory is none other than to describe the activity of our understanding, to make a protocol of the rules according to which our thinking actually proceeds.[9]

Hence, in a certain sense, Hilbert admitted that formalized mathematics is a game. But he believed that this is not a *mere* game, since it is the very game that we call thinking. This idea is based on the fact that, according to him, the rules of the formula game are, and should be, the rules according to which our thinking

---

[9]Cf. Hilbert 1927, 475 (87).

124

proceeds. In other words, the idea is based on Hilbert's belief that the formal expression of the technique of our thinking provides an accurate description of, but also the right prescription for, the activity of the mind.

To be sure, Weyl acknowledged that the formula game, the manipulation of meaningless symbols according to stipulated rules, describes at least part of the mathematician's way of thinking:

> We forget about what the symbols stand for. The mathematician is concerned with the catalogue alone; he is like the man in the catalogue room who does not care what books or pieces of an intuitively given manifold the symbols of his catalogue denote. He need not be idle; there are many operations which he may carry out with these symbols, without ever having to look at the things they stand for.[10]

Weyl came to believe that the mathematician engaged in symbolic reasoning "refrains from constructing the mathematical objects."[11] However, Weyl never abandoned the view that the construction of mathematical objects ought to be preferred to purely symbolic reasoning, *if understanding is what the mathematician is looking for.* Consequently, while he acknowledged that the rules of the formula game are the rules according to which mathematical thinking *does* sometimes proceed, Weyl denied that they can be the rules according to which mathematical thinking *should* proceed. On his view, purely symbolic reasoning has no epistemic value for mathematics itself.

However, Weyl also came to believe that the rules of the formula game *should* be the rules according to which thinking sometimes proceed, if *objectivity, rather than understanding*, is the epistemic ideal that one seeks to attain. In other words, he came to believe that although purely symbolic reasoning has no epistemic value

---

[10]Cf. Weyl 1940, 714 (208).

[11]Cf. Weyl 1940, 717 (208).

for mathematics itself, it does have great epistemic value for the natural sciences. This view suggests that Weyl ignored Hilbert's own emphasis on the fact that transcendent axiomatics is more than a mere game not only because it is of great epistemic value to physics, but rather as a formal instrument of great epistemic value to mathematics itself, as well as to everyday thinking.[12] Indeed, as early as in 1924, Weyl wrote: "I see only *one* possibility of attributing it [i.e., formalized mathematics], including its transfinite components, an independent intellectual significance."[13] This possibility is later on described as follows:

> If Hilbert is not just playing a game of formulae, then he wants a theoretical mathematics in contrast to Brouwer's intuitive one. But where is that transcendent world carried by belief, at which its symbols are directed? I do not find it, unless I completely fuse mathematics with physics and assume that the mathematical concepts of number, function, etc. (or Hilbert's symbols), generally partake in the theoretical construction of the real world *in the same way* as the concepts of energy, gravitation, electron, etc.[14]

It is far from being immediately clear what Weyl's interpretation of Hilbert's view is in this passage. On this interpretation, if Hilbert's mathematics is to be more than a mere formula game, it must be regarded as being fused with physics, that is, as partaking "in the same way" as physics in a theoretical construction of the real world. What might this mean? What might it mean to say that *physical*

---

[12]Cf., e.g., Hilbert 1918 (85).

[13]"Ich sehe nur *eine* Möglichkeit, ihnen einschließlich ihrer transfiniten Bestandteile eine selbständige geistige Bedeutung beizulegen." (Weyl 1924, 451 (196))

[14]"Wenn Hilbert nicht ein bloßes Formelspiel treibt, so will er eine theoretische im Gegensatz zu Brouwers intuitiver Mathematik. Aber wo ist jenes vom Glauben getragene Jenseits, auf das ihre Symbole richten? Ich finde es nicht, wenn ich nicht die Mathematik sich völlig mit der Physik verschmelzen lasse und annehme, daß die mathematischen Begriffe von Zahl, Funktion usw. (oder die Hilbertschen Symbole) prinzipiell *in der gleichen Art* an der theoretischen Konstruktion der wirklichen Welt teilnehmen wie die Begriffe Energie, Gravitation, Elektron u. dergl." (Weyl 1925, 540 (197); Eng. tr., 140 (197), my emphasis)

concepts partake in a theoretical construction of the world? And what might it mean to say that *mathematical* concepts do so, too?

At a later stage in his career, Weyl made the following remark about the character of physical concepts and what this implied with regard to the nature of theories:

> In the development of physics, the physical concepts have revealed themselves more and more as free constructions, mere *symbols* handled according to certain rules; theoretical physics becomes a system as thoroughly formalized as Hilbert's mathematics.[15]

Thus, Weyl seems to have considered physical concepts as mere symbols defined implicitly through the relations expressed by the fundamental principles of a theory. Such principles, although they may be suggested by intuition, are freely stipulated and taken to be merely symbolic expressions, rather than contentual propositions. This view on concept formation was motivated, as we have seen in the previous chapter, by the need to take into account the positing of hypothetical entities (energy, force, field, and the like) as an attempt to overcome phenomenalism. The idea that physical concepts partake in a theoretical construction of the world emphasizes, then, according to Weyl, the objectifying role of such concepts. In other words, it emphasizes the fact that physical concepts, considered as mere symbols without contentual significance, are indispensable for the stipulation of the fundamental principles of a theory capable of providing knowledge about the mind-independent world. A theory cannot provide such knowledge, unless it takes the form of a transcendent or formal axiomatic system.

If this is the right way to understand Weyl, then to say that Hilbert's mathematics partakes in the theoretical construction of the real world is to say that

---

[15]Cf. Weyl 1946, 604 (210).

mathematical concepts (including the transfinite ones) have, just like the physical concepts, an essential objectifying role. In other words, mathematical concepts are indispensable, in the sense just explained, for the formation of scientific theories capable of providing objective knowledge. However, since the free creation of concepts may hardly be enough for objectivity, what might justify Weyl's belief that scientific theories, as transcendent axiomatic systems, *can* provide knowledge about mind-independent reality? What further conditions must be satisfied by a scientific theory for objectivity to obtain? We address this question in the next section.

4.3   Weyl on Objectivity and Categoricity

In this section, we attempt to show that, on Weyl's view, a necessary condition for objectivity is the categoricity of a physical theory, and that its satisfaction demands that the relations between mind-independent objects be isomorphic with the relations between our perceptions. Then we take up the question about the justification that Weyl provided in support of the idea that this isomorphism may obtain.

As noted above, Weyl maintained that the development of physics led one to conceive of scientific theories as transcendent axiomatic systems. He also believed that this development was determined by the physicist's search for objective theories, and he expressed this belief quite unequivocally: "To fulfill the demand of objectivity, we construct an image of the world in symbols."[16] But how should one think about the relation between a symbolic image of the world – a scientific theory, and the world itself as a domain of its interpretation?

---

[16]Cf. Weyl 1949a, 77 (211).

"Objectivity," Weyl maintained in an often quoted remark, "means invariance with respect to the group of automorphisms."[17] As he explained, this implies that the relations expressed by the statements derived in a theory are objective only if they remain invariant under the group of one-to-one transformations within the object domain of the theory that leave invariant the relations expressed by the fundamental presuppositions of the theory.[18] For example, the theorems of Euclidean geometry (or, rather, Hilbert's axiomatization of it) can be taken to express objective relations only if these relations remain invariant across the one-to-one transformations of the domain of geometrical objects onto itself that leave invariant the relations expressed by the axioms.

Weyl's idea that invariance under a certain class of transformations of the structure of a theory is required for a physical theory to express objective relations has recently resurfaced in the literature.[19] But we should ask whether invariance under *automorphic* transformations can really be taken as the criterion for objectivity. Making invariance under automorphic transformations the criterion for objectivity seems to presuppose that we either do not have more than one domain of objects as a possible interpretation, or that a unique domain among the possible ones can be identified as the domain with respect to which invariance is to be measured. In the first case, we would be just wrong, since a physical theory may typically be interpreted over different domains of objects. In the second case, the unique domain either is not the domain of objects in the mind-independent world, and so invariance would be insufficient for objectivity, or it is, and so invariance is redundant.

---

[17]Cf. Weyl 1952, 132 (213).

[18]Cf. Weyl 1949a, 72ff (211).

[19]Cf., e.g., Nozick 2001 (142), Kosso 2003 (115), Debs and Redhead 2007 (35).

By contrast, it seems more appropriate to say that the statements of a physical theory express objective relations only if these relations remain invariant under *isomorphic* transformations of the interpretations that leave invariant the relations expressed by the fundamental laws of the theory. If this is correct, then the view that should be attributed to Weyl is that objectivity requires categoricity. But what would justify this view? Furthermore, should we also think that, according to him, categoricity is enough for objectivity? Before we address these questions, let us try to clarify how Weyl conceived the notion of categoricity.

In the winter semester of 1931, after having accepted a position as Hilbert's successor in Göttingen, Weyl taught a course on the recent developments of the axiomatic method. Speaking about the completeness of an axiomatic system, he told his students: "We only require that [any] two concrete interpretations of a complete system of axioms are isomorphic to each other. ... One designates this conception of completeness also as *categoricity of the system*."[20] To be sure, the idea of completeness as categoricity was already present in his 1927 monograph, *Philosophy of Mathematics and Natural Science*, although the term "categoricity" would be used only in the 1949 English translation of the book:

> One might have thought of calling a system of axioms complete if the meaning of the basic concepts present in them were univocally fixed through the requirement that the axioms be valid. But this ideal cannot be realized, for the isomorphic mapping of a contentual interpretation is surely just another contentual interpretation. The final formulation is therefore this: an system of axioms is complete, or *categorical*, if any two contentual interpretations of it are necessarily

---

[20] "Wir verlangen nur von einem vollständigen Axiomensystem ..., daß zwei inhaltliche Interpretationen ... zueinander isomorph sind. ... Man bezeichnet diese Fassung der Vollständigkeit auch als *Kategorizität des Axiomensystems*." (Manuscript Hs91a)

isomorphic.[21]

Hence, Weyl believed that the notion of categoricity denotes a certain property of completeness.[22] According to him, a univocal determination of the meaning of the concepts in the axioms, i.e., a one-to-one mapping of these concepts onto the domain of one particular interpretation of the system, is not sufficient to make an axiomatic system complete. What this requires is a one-to-one mapping of the concepts onto the domain of the "essential" interpretation of the theory. In other words, an axiomatic system is complete only if it is categorical, i.e., only if the meaning of concepts is univocally determined *up to an isomorphism* of all interpretations of the system.

Weyl's view, then, seems to have been that objectivity can obtain only if a scientific theory is complete in this sense, that is, only if the correlation between its basic concepts and the objects it can apply to is univocal up to isomorphism. But how might this view be understood? It seems obvious that one cannot claim that there is a univocal correlation between concepts and any mind-independent objects, for this assumes that one already has epistemic access to mind-independent objects, which begs the question of scientific objectivity. Furthermore, to claim that the correlation between concepts and mind-independent objects is univocal *up to isomorphism* would assume that the relations between mind-independent ob-

---

[21] "Man könnte nun darauf verfallen, ein Axiomensystem vollständig zu nennen, wenn durch die Forderung der Gültigkeit der Axiome der Sinn der in sie eingehenden Grundbegriffe eindeutig fixiert wäre. Dieses Ideal ist aber nicht zu erfüllen; denn gewiß ist jede inhaltliche Interpretation, die aus einer vorliegenden durch isomorphe Abbildung hervorgeht, wiederum eine solche. Die endgültige Formulierung ist darum diese: *Ein Axiomensystem ist vollständig, wenn irgend zwei inhaltliche Interpretationen desselben notwendig isomorph sind.*" (Weyl 1927a, 21f (198); Eng tr., 25 (211))

[22] Edward Huntington had actually used the term "sufficiency" to express part of a more comprehensive notion of completeness, the very part that came to be called categoricity after Oswald Veblen introduced the term "categoricity," giving credit for it to John Dewey (cf. Huntington 1902, 264 (99), Veblen 1904, 346 (184)). For historical background on the notion of categoricity, see Awodey and Reck 2002 (4).

jects can be isomorphically mapped to relations between objects in other domains of interpretation. Quite similarly, this begs the question of scientific objectivity, for it assumes that one already has epistemic access to the relations between mind-independent objects.

Before we take a closer look at Weyl's view on the relation between categoricity and objectivity, let us consider his remarks on the relation between categoricity and understanding, as we find them formulated in the following passage:

> A science can determine its domain only up to an isomorphic mapping. In particular it remains entirely indifferent as to the 'essence' of its objects. That which distinguishes the real points in space from number triples or other interpretation of geometry one can only *know* by immediate, living intuition. But intuition is not in itself some blessed tranquility, which it would never be able to leave behind. Rather, intuition presses on toward the chasm and adventure of *cognition*. However, it would be a chimera to expect cognition to reveal to intuition an essence deeper than that openly available to intuition. The idea of isomorphism designates the natural insurmountable boundary of scientific cognition.[23]

It is important to note here Weyl's distinction between what might be called *phenomenal* knowledge (*Kenntnis*) and *scientific* knowledge or cognition (*Erkenntnis* or *Wissen*). This distinction seems to be based on the fact that while phenomenal knowledge requires that objects be immediately presented in intuition, scientific cognition has no such requirement. Indeed, as we have seen already, on Weyl's

---

[23] "Eine Wissenschaft kann ihr Sachgebiet immer nur bis auf eine isomorphe Abbildung festlegen. Insbesondere verhält sie sich gegenüber dem 'Wesen' ihrer Objekte ganz indifferent. Das, was die wirklichen Raumpunkte von Zahlentripeln oder andern Interpretationen der Geometrie unterscheidet, kann man nur *kennen* in unmittelbarer lebendiger Anschauung. Aber das Schauen ist nicht selige Ruhe in sich, aus der es niemals herauszutreten vermöchte, sondern drängt fort zum Zwiespalt und Wagnis der *Erkenntnis*; Schwärmerei aber ist es, von der Erkenntnis zu erwarten, dass sie ein tieferes Wesen als das der Anschauung offen daliegende – der Anschauung enthülle. Der Isomorphiegedanke bezeichnet die selbstverständliche unübersteigbare Schranke des Wissens." (Weyl 1927a, 22 (198), Eng. tr., 26 (211))

view, science goes beyond what is immediately presented in intuition by positing hypothetical elements, i.e., real but in principle unobservable entities. By doing so, however, science forgoes the ability to bring about understanding, since, as we recall from our discussion in chapter two above, the immediate presentation of objects in intuition was, according to him, an indispensable condition for the experience of truth, without which no understanding could be brought about. Thus, the thought that isomorphism is "the natural insurmountable boundary of scientific cognition" points out that one may obtain scientific cognition of the relations between objects in a domain of interpretation (more exactly, of those relations that can be mapped isomorphically to the relations between objects in any other domain of interpretation), but one could never thereby bring about understanding. In other words, Weyl seems to have believed that categoricity is, as a matter of fact, an obstacle to scientific understanding.

This belief finds further support in his remark that immanent axiomatic systems – the very purpose of which, as we discussed in section 2.8 above, is to bring about understanding– are non-categorical systems, for they typically have non-isomorphic models: "While in the [transcendent] axiomatics one was mostly concerned with axioms which determine the structure of the system completely as, e.g., the axioms of Euclidean geometry do for Euclidean space, we have here, in algebra, to do with [immanent] axioms satisfied by many different individual number fields that are not mutually isomorphic."[24] For example, the field axioms can be satisfied by the rational numbers, the real numbers, etc., and these number

---

[24]Cf. Weyl 1985, 13 (218).

fields are, of course, not isomorphic.[25]

Although, on Weyl's view, categoricity seems to be an obstacle to understanding, there is good reason, as we emphasized above, to attribute to him the view that categoricity is necessary for scientific objectivity. A justification for this view is also suggested in the following passage:

> This thought [i.e., the thought of isomorphism as a boundary] has clarificatory value for the metaphysical speculations about a world of things in themselves behind the phenomena. For it is clear that under such a hypothesis the phenomenal world must be isomorphic to the absolute one (where, of course, the coordination needs to be univocal only in the direction thing in itself → phenomenon); for "we are justified, when different perceptions offer themselves to us, to infer that the real conditions are different" (Helmholtz, *Wissenschatliche Abhandlungen*, II, 656) Thus, even if we do not *know* the things in themselves, still we have just as much *cognition* about them as we do about the phenomena.[26]

Hence, Weyl's belief that objectivity requires categoricity would be justified only if the relations between our perceptions could be isomorphically mapped to the relations between the objects in the mind-independent world. This condition, however, raises several problems.

First, it is not clear that categoricity is also sufficient for objectivity. It seems

---

[25]It is interesting to note, however, that although these number fields are not isomorphic, the algebraic closure of any of them *is* unique up to isomorphism. Thus, for example, if K_1, K_2 are two algebraic closures of a field F, there is an isomorphism $\phi : K_1 \to K_2$ which is the identity map on F (see, e.g., Artin 1991, 528 (1)). This suggests that if invariance is measured with respect to algebraic closures only, the immanent axioms of field theory *are* categorical.

[26]"Auch für die metaphysischen Spekulationen über eine Welt der Dinge an sich hinter der Erscheinungen hat dieser Gedanke aufklärenden Wert. Denn es ist bei einer solchen Hypothese klar, dass die Erscheinungswelt der absoluten isomorph sein muß (wobei freilich die Zuordnung nur in der einen Richtung Ding an sich → Erscheinung eindeutig zu sein braucht); denn "wir sind, wenn verschiedene Wahrnehmungen sich uns aufdrängen, berechtigt, daraus auf Verschiedenheit der reellen Bedingungen zu schließen" (Helmholtz, *Wissenschatliche Abhandlungen*, II, S. 656). Wenn wir also auch die Dinge an sich nicht *kennen*, so *wissen* wir doch genau so viel über sie wie über die Erscheinungen."(Weyl 1927a, 22 (198), Eng. tr., 26 (211))

clear that the content of a physical theory interpreted over the domain of perceptual experience is more evident than the content of the same theory interpreted over the domain of objects in the mind-independent world. If categoricity is restricted to interpretations on which the content of the theory is most evident, or at least as evident as the content of the same theory when interpreted over the domain of perceptual experience, then categoricity would be insufficient for objectivity. This suggests that objectivity requires not only categoricity, but also that the mind-independent world can really be taken as an interpretation of the theory. But why would this be the case?

Secondly, even if categoricity is not restricted in this way, and the mind-independent world may be taken as an interpretation of the theory, how might one justify, without begging the question of objectivity, the view that the required isomorphism between our perceptual experience and the mind-independent world actually obtains? On the face of it, it seems obvious that there should be more mind-independent objects than perceptions, so that such an isomorphism cannot obtain.[27] To see how Weyl attempted to address these questions, we need to look more closely at Helmholtz' principle, invoked in the above quotation, to explain what motivates Weyl's acceptance of it, and what he had to say against it.

4.4   Weyl on Univocality and Concordance

In support of an isomorphic mapping of the relations between the objects given in perceptual experience and the relations between the objects in the mind-independent world, Weyl invoked Helmholtz' principle that "we are justified, when

---

[27]One might want to point out here that objectivity need not actually require isomorphism, but rather merely a *partial* isomorphism between our perceptual experience and the mind-independent world. For the notion of partial isomorphism in this context, see da Costa and French 2003 (33).

different perceptions offer themselves to us, to infer that the real conditions are different." To explain this principle, and to see how it might have been taken to support this mapping, let us refer to Helmholtz' famous claim, in the *Treatise on Physiological Optics*, that "the law of sufficient reason is really nothing more than the *urge* of our intellect to bring all our perceptions under its own control."[28] How can this law, thus understood, help us bring perceptions under control?

In his theory of perception, Helmholtz maintained that a perception is just a sign produced by an object.[29] This belief was motivated, at least in part, by the desire to reject the naive realist view that there exists a resemblance relation between perceptions and objects. Obviously, Helmholtz thought, this view begs the question of objectivity, since it assumes that one may compare perceptions and objects, which seems to require epistemic access to objects independently of perception. But it is not clear why he believed that the relation he thought existed between perceptions and objects may not beg the same question. He seems to have suggested that it is in the nature of perception to be produced by an object, or at least that it is so under typical physiological and psychological conditions. Although questionable, this suggestion seems to entail that the claim that there exists a determinate relation between perceptions and objects does not require epistemic access to objects independently of perception, and so it does not beg the question of objectivity.

At any rate, Helmholtz maintained that "it belongs to the nature of a sign that for the same object always the same sign be given."[30] This entails that the same object, under the same circumstances, should always produce in us the same

---

[28]Cf. Helmholtz 1867, 34 (83).

[29]Cf. Helmholtz 1903 (84).

[30]"Zum Wesens eines Zeichens gehört nur, daß für das gleiche Objekt immer dasselbe Zeichen gegeben werde." (Helmholtz 1903, 357 (84))

perception. Otherwise, it could happen, for example, that the same leaf, under the same circumstances, appear to us green, at some times, and not green, at other times. But then our perceptions cannot be said to be under control. Helmholtz argued that in order to prevent this to be the case, we need to stipulate the principle that a difference in perception is always determined by a difference in the domain of objects. He seems to have taken this stipulation to be motivated by the law of sufficient reason.

Weyl noted that even if Helmholtz' principle is satisfied, it may still happen that more than one object produces the same perception. For instance, given our perceptual limitations, different shades of green may appear to us as the same color. Then, although for a different reason than above, our perceptions cannot be said to be under control. Weyl maintained that in order to prevent this to be the case, we need to stipulate another principle: "The objective image of the world may not admit of any diversities which cannot manifest themselves in some diversity of perceptions."[31] This principle suggests that it is scientific objectivity that demands bringing perceptions under control. Control can be achieved, according to Weyl, only if both principles are obeyed. But if both principles are obeyed, then this is enough for an isomorphism between perceptions and objects to obtain.

As we will see in the next section, Weyl actually came to doubt that this isomorphism can actually obtain. Nevertheless, it seems clear now that even if it did obtain, the isomorphism between perceptions and objects would not be sufficient for objectivity. For, as we have seen in the previous section, objectivity also requires that a scientific theory be interpreted over the domain of percep-

---

[31]Cf. Weyl 1949a, 117f (211).

tual experiences in a way that allows for a univocal correlation between scientific concepts and our perceptions. But how can such an univocal correlation obtain?

In the previous chapter, we have seen that, on Weyl's view, the epistemic ideal of objectivity requires that physical concepts like force, energy, electromagnetic field, and the like, be freely created as mere symbols with no contentual significance. These concepts cannot be introduced through abstraction from experience, since no force, energy, or field can be directly perceived. This entails that the interpretation of a physical theory deploying such concepts over the domain of perceptual experience cannot allow for an univocal correlation between concepts and perceptions, if an univocal correlation is conceived of as a one-to-one coordination. According to Weyl, interpreting the theory over the domain of perceptual experience may only allow for a correlation between the *entire* theory and our experience:

> In the systematic theory, one should present . . . a formal scaffold of mere symbols, without explaining what the symbols for mass, charge, field strength, etc., mean and only afterwards explain how the entire symbolic structure is connected with our immediate experience.[32]

This raises, of course, the question in what sense can a holistic correlation between theory and experience be univocal. An answer to this question was proposed by Hans Reichenbach, who maintained that a holistic correlation is univocal in the sense that the methods whereby the physical quantities of interest in a theory are measured converge: "Univocality means ... that a physical quantity, determined

---

[32] "In der systematische Theorie sollte man . . . ein Formelgerüst aus bloßen Symbolen hinstellen, ohne zu erklären, was die Symbole für Masse, Ladung, Feldstärke, usw. bedeuten, und nur am Ende beschreiben, wie die ganze symbolische Struktur mit unserer unmittelbarer Erfahrung verknüpft ist." (Weyl 1949b, 311 (212))

from different observational data, is expressed by the same measure number."[33]
In other words, for Reichenbach, the criterion for univocal correlation seems to
be the sameness of numerical expressions associated with a physical quantity by
different methods of measurement. One example that he offered suggests that the
sameness relation may allow for a divergence within the margins of error due to
the experimental setup:

> If, based on Einstein's theory, one calculates a light deflection of 1.7"
> by the sun, but finds instead 10", then this is a contradiction, and such
> contradictions decide always upon the validity of a physical theory.
> The number 1,7" is obtained on the basis of equations and experiences
> on other material. The number 10" is, in principle, not obtained in
> a different manner, for it is by no means directly read off, but con-
> structed from reading data with the help of quite complicated theories
> of the measuring instruments. One can thus say that *one* chain of
> calculations and experiences assigns to the real event the number 1,7,
> the *other* the number 10, and this is the contradiction.[34]

The property that Reichenbach illustrated in this passage is precisely what Hilbert
and Bernays later called "external consistency" and what Weyl called "concor-
dance." Concordance was, more precisely, defined as follows:

> Concordance. The definite value, which is assigned, in a certain indi-
> vidual case, to a quantity occurring in the theory, is determined on the

---

[33] "Eindeutigkeit heißt ... daß eine physikalische Zustandsgröße bei ihrer Bestimmung aus ver-
schiedenen Erfahrungsdaten durch dieselbe Messungszahl wiedergegeben wird." (Reichenbach
1920, 43 (151))

[34] "Berechnet man etwa aus der Einsteinschen Theorie eine Lichtablenkung von 1,7" an der
Sonne, und würde man an Stelle dessen 10" finden, so ist das ein Widerspruch, und solche Wider-
sprüche sind es allemal, die über die Geltung einer physikalischen Theorie entscheiden. Nun ist
die Zahl 1,7" auf Grund von Gleichungen und Erfahrungen an anderem Material gewonnen; die
Zahl 10" aber im Prinzip nicht anders, denn sie wird keineswegs direkt abgelesen, sondern aus
Ablesungsdaten mit Hilfe ziemlich komplizierter Theorien über die Meßinstrumente konstruiert.
Man kann also sagen, daß die *eine* Überlegungs- und Erfahrungskette dem Wirklichkeitsereignis
die Zahl 1,7 zuordnet, die *andere* die Zahl 10, und dies ist der Widerspruch." (Reichenbach
1920, 41 (151))

basis of the theoretically posited connections and the contact with the perceptually given. *Every such determination must lead to the same result.*[35]

Just like Reichenbach, then, Weyl seems to have taken the criterion for an univocal correlation between a theory and experience to be expressed by the notion of concordance, which requires that the methods that proceed via highly theoretical considerations must lead to results that agree (within the limits of experimental error) with results obtained through observational methods. As another elementary illustration of concordance, consider the prediction of the location of a comet. On the basis of classical mechanics, the location of a comet at a certain moment t is calculated from observations of the location of the comet at previous moments. The result is then compared with the direct observations of the location of the comet at t. Concordance requires that theoretical calculations are in agreement with direct observations.

If this is a correct interpretation of Weyl's view, then according to him, objectivity demands not only that (1) free creation be deployed as a method of concept formation, i.e., that a scientific theory be conceived of as a transcendent axiomatic system, but also that the theory be categorical. This means, as we have just seen, that (2) between the concepts of the theory and our perceptions there has to be an univocal correlation, i.e., that there must be a concordance (within the limits of experimental error) between the various methods used to determine the numerical values of the physical quantities in a theory, and that (3) both Helmholtz' principle and its Weylean converse must be obeyed, i.e., that an isomorphism must

---

[35] "Einstimmigkeit. Der bestimmte Wert, welcher einer in der Theorie vorkommenden Größe in einem individuell bestimmten Fall zuzuschreiben ist, wird auf Grund der theoretisch gesetzten Verknüpfungen und der Berührung mit der wahrnehmungsmßig Gegebenen ermittelt. *Jede derartige Ermittlung muß zu dem gleichen Ergebnis führen.*" (Weyl 1927a, 87 (198); Eng. tr., 121 (211). See also Weyl 1931, 17 (204)) Cf. Hilbert and Bernays 1939, 291 (89). See also section 5.3 below.

obtain between our perceptions and the objects in the mind-independent world.

Weyl eventually came to believe, nevertheless, that physical theories fail to satisfy the condition of categoricity, for even if (2) were satisfied, there is reason to think that (3) cannot be. He ultimately adopted, as we will see, an unsettled position with respect to scientific objectivity. In the next two sections, we want to clarify his reasons for adopting such a position.

4.5   The Non-Categoricity of First-Order Theories

In the 1950s, toward the end of his career, Weyl explicitly noted the failure of categoricity in current physics, and came to conclude that objectivity may remain an unattainable epistemic ideal:

> The truth as we see it today is this: The laws of nature do not determine univocally the one world that actually exists, not even if one concedes that two worlds arising from each other by an automorphic transformation, i.e., by a transformation which preserves the universal laws of nature, are to be considered the same world.[36]
>
> Who could seriously pretend that the symbolic construct *is* the true real world? Objective Being, reality, becomes elusive; and science no longer claims to erect a sublime, truly objective world above the Slough of Despond in which our daily life moves. ... the objective Being that we hoped to construct as one big piece of cloth each time tears off; what is left in our hands are – rags.[37]

In this section, we want to present some of the reasons for Weyl's ultimate position on scientific objectivity. But first we would like to note that, as a matter of fact, he expressed doubts that physical theories could actually satisfy the requirement of categoricity as early as 1927. Such doubts transpire, for example, in the following

---

[36]Cf. Weyl 1952, 27 (213).

[37]Cf. Weyl 1954b, 627 (216).

passage from his *Philosophy of Mathematics and Natural Science*:

> The positing of the real external world does not guarantee that this
> constitutes itself from appearances through the cognitive work of rea-
> son establishing concordance. For this much more is needed – that the
> world be ruled by simple elementary laws. The mere positing of the
> external world does not actually explain what it nevertheless ought to
> explain, and the question about its reality mingles inseparably with
> the question about the reason for the lawful-mathematical harmony of
> the world. The ultimate answer lies, thus, beyond all knowledge, in
> God alone.[38]

This passage seems to suggest that, on Weyl's view, concordant scientific theo-
ries are not able to provide knowledge about the mind-independent world. But
what motivated this view? According to him, as we have seen in the previous
section, objectivity requires, beside concordance, that our perceptions be isomor-
phic with the objects of the mind-independent world. If such an isomorphism
obtained, it would entail that the simple laws of physics actually apply to the
mind-independent world. According to Weyl, however, this assumes without jus-
tification that the world itself is simple, rather than complex. But whether this is
so can be known, he seems to have believed, only by its creator.

Perhaps a more convincing reason for the claim that scientific theories fail
to be categorical is the fact that first-order theories with an infinite model have
non-isomorphic models – a fact of which Weyl was of course aware, since it is a
consequence of the Löwenheim-Skolem theorem. Indeed, as early as 1922, Skolem
showed that Zermelo's set-theory is not categorical, since one can consistently

---

[38] "Die Setzung der realen Außenwelt garantiert nicht dafür, daß diese in der Vernunft sich aus
den Erscheinungen durch die Einstimmigkeit schaffende Erkenntnisarbeit konstituiere; dazu ist
vielmehr noch nötig, daß sie von einfachen Elementargesetzen durchwaltet sei. Die bloße Setzung
der Außenwelt erklärt also eigentlich nicht, was sie doch erklären sollte, sondern die Frage
nach ihrer Realität fließt untrennbar zusammen mit der nach dem Grunde für die gesetzlich-
mathematische Harmonie der Welt. So liegt die letzte Antwort denn doch, jenseits des Wissens,
allein in Gott." (Weyl 1927a, 89 (198), Eng. tr., 125 (211))

build non-isomorphic models for it.[39] This line of thought was also taken by von Neumann, who also attempted to formulate new axioms, in the hope that a categorical system would thus be obtained, but eventually concluded that "no categorical axiomatization of set theory seems to exist at all" and more generally that "there probably cannot be any categorically axiomatized infinite systems at all."[40]

In this context, it appears rather curious that Weyl would consider the notion of categoricity of central importance to scientific objectivity. One might think, however, that Weyl was not concerned with first-order theories, and that he had nothing against quantification over property variables. But this is not true. From his early predicativist account of analysis on, Weyl constantly rejected second-order logic. In *The Continuum*, for instance, when criticizing Russell's theory of types, he expressed this rejection in the following terms:

> A "hierarchical" version of analysis is artificial and useless. It loses sight of its proper object, i.e., number. Clearly, we must take the other path – that is, we must restrict the existence concept to the basic categories (here, the natural and rational numbers) and must not apply it in connection with the system of properties and relations (or the sets, real numbers, and so on, corresponding to them).[41]

According to Weyl, quantification over property variables turns logic into a metaphysical doctrine, as he put it in a later review of Russell's mathematical logic:

> In the resulting system [i.e., the system of *Principia Mathematica*] mathematics is no longer founded on logic, but on a sort of logician's paradise, a universe endowed with an "ultimate furniture" of rather

---

[39]Cf. Skolem 1922, 298 (174).

[40]Cf. von Neumann 1925, 412 (186).

[41]Cf. Weyl 1918a; Eng. tr., 32 (191).

complex structure and governed by quite a number of sweeping axioms of closure. The motives are clear, but belief in this transcendental world taxes the strength of our faith hardly less than the doctrines of the early Fathers of the Church or of the scholastic philosophers of the Middle Ages.[42]

But one might want to suggest that Weyl believed it possible to define the notion of isomorphism in a predicative way, and that, consequently, he did not see any tension between the requirement of categoricity and the rejection of second-order logic. To the best of our knowledge, even if this was indeed his view, Weyl never expressed it in writing.[43]

At any rate, the apparent tension between the rejection of second-order logic and the requirement of categoricity for scientific objectivity was, no doubt, increased by Gödel's "surprising" discoveries, which according to Weyl show "to what extent the intuitively certain goes beyond what (in an arbitrary but fixed formalism) is capable of mathematical proof."[44] More exactly, the first incompleteness theorem shows that any first-order theory at least as strong as Peano arithmetic contains an undecidable proposition p, i.e., it shows that neither p nor not-p can be derived in that theory. That is, the theorem shows that any first-order theory at least as strong as Peano arithmetic is syntactically incomplete. But this entails, assuming the completeness of the underlying logic, that such a

---

[42]Cf. Weyl 1946, 272 (210).

[43]As a matter of fact, the conditions for the categoricity of a first-order theory have been identified only ten years after Weyl's death, when it was proved that if a first-order theory (in a countable language) is k-categorical, or categorical in an uncountable cardinality k, i.e., if it has one model of cardinality k up to isomorphism, then it is categorical in all uncountable cardinalities (cf. Morley 1965 (138), and for a recent review Hedman 2006, 205-211 (81)). After ten more years, the same result has been proved for first-order theories in uncountable languages (cf. Shelah 1974 (166)).

[44]Cf. Weyl 1944, 126 (209).

theory is not categorical, since it has at least two non-isomorphic models.[45]

Nevertheless, one could argue that all this is not enough to justify Weyl's ultimate position with respect to scientific objectivity. For one might suggest that, even if Weyl rejected second-order logic in the context of his predicative reconstruction of real analysis, he might have accepted it in the context of a theoretical construction of the world in physics. As we have discussed above, Weyl's conversion to Hilbert's formalism seems to have been motivated by the idea that the method of transcendent axiomatics is necessary for the theoretical construction of the world, that is, for the formation of physical theories capable of providing objective knowledge. And it is plausible to think that, after this conversion, Weyl dropped his predicativist principles, at least in the context of physics. But even if this was the case, he provided no reason that could justify the acceptance of second-order logic as a necessary condition for scientific objectivity.

Be that as it may, perhaps the most serious challenge to Weyl's view on scientific objectivity and, thus, a stronger reason for skepticism, was provided by the phenomenon of unitary inequivalence in quantum physics, to which we turn now.

4.6 The Non-Categoricity of Quantum Physical Theories

In order to present the problem raised by the phenomenon of unitary inequivalence for Weyl's view on scientific objectivity, let us begin by noting that he played

---

[45]We should perhaps recall that it was Huntington who had noted that categoricity entails syntactic completeness: "A set of postulates having this property [i.e., a unique model up to isomorphism] has been called a "categorical," as distinguished from a "disjunctive," set; see Veblen 1904, 346. Every proposition concerning a class K, a relation $<$, and an operation $+$, is either deducible from the postulates of this set, or in contradiction with them." (Huntington 1905a, 17 (100)) Later on, Huntington noted, however, that, if the underlying logic is not complete, certain sets of postulates might be categorical, but not syntactically complete (cf. Huntington 1905b, 210 (101)). For a concrete example, see Corcoran 1980 (29) where a second-order formalization of arithmetic is given, one that is categorical but cannot prove that zero is not a successor.

a major role in the introduction of group theory to quantum physics.[46] Having worked for some time on the abstract theory of group representations, Weyl came to see the importance of algebra for quantum mechanics.[47] He found abstract algebra to be the mathematical instrument needed for overcoming the lack of unity and mathematical rigor that characterized quantum mechanics, which was at the time "a conglomeration of essentially different, independent, heterogeneous and partially contradictory fragments," as von Neumann described it.[48] In particular, Weyl noted that the wave and the matrix mechanics are equivalent, insofar as they can be seen as interpretations of a common algebraic structure:

> This newer mathematics, including the modern theory of groups and "abstract algebra," is clearly motivated by a spirit different from that of "classical mathematics," which found its highest expression in the theory of functions of a complex variable. The continuum of real numbers has retained its ancient prerogative in physics for the expression of physical measurements, but it can justly be maintained that the essence of the new Heisenberg-Schrödinger-Dirac quantum mechanics is to be found in the fact that there is associated with each physical system a set of quantities, constituting a non-commutative algebra in the technical mathematical sense, the elements of which are the physical quantities themselves.[49]

However, given that the algebra associated with the observables in a physical system could have different interpretations, the question arises under what conditions could these observables be univocally determined up to isomorphism? To answer

---

[46] Of course, in the early 1920s, Wigner also played a major role in this direction, as his work in chemistry and crystallography made him realize that group theory immensely facilitated his calculations, and eventually led him to also realize its significance for quantum mechanics. Wigner was actually the first to express various symmetries of a quantum system in terms of groups of rotations and permutations (cf. Wigner 1927 (219), Wigner 1931 (220)). For discussion, see Chayut 2001 (26))

[47] Cf. Weyl 1927b (199). See also Mackie 1988 (125).

[48] Cf. von Neumann 1932, 4 (188).

[49] Cf. Weyl 1928b, viii (201).

this question, let us start with the basics and consider a physical system consisting of a free particle with one degree of freedom.[50] The observables that characterize the system (i.e., momentum and position) are standardly associated self-adjoint unbounded operators on a Hilbert space, P and Q, which obey the Heisenberg commutation relation:

$$PQ - QP = \frac{\hbar}{2\pi i}\mathbf{1}$$

Momentum and position, let us note, are implicitly defined by the Heisenberg commutation relation. The question is whether they can be univocally determined, and if so under what conditions. Since P and Q are unbounded, $PQ - QP$ is not everywhere defined on the Hilbert space, and thus the Heisenberg commutation relation does not always have a solution. To overcome this problem, Weyl reformulated the Heisenberg commutation relation in terms of unitary operators, $U(\alpha) = e^{\frac{2\pi i}{\hbar}\alpha P}$, $V(\beta) = e^{\frac{2\pi i}{\hbar}\beta Q}$, for any real numbers α and β. These unitary operators are bounded and, thus, $PQ - QP$ is defined everywhere in the Hilbert space. This eliminates the cases in which the Heisenberg commutation relation has no solution.[51] The commutation relation is reformulated accordingly:

$$U(\alpha)V(\beta) = e^{\frac{2\pi i}{\hbar}\alpha\beta}V(\beta)U(\alpha)$$

The advantage of this new equation – the Weyl commutation relation – is that its solutions make possible the univocal determination of momentum and position. This idea was formulated by Marshall Stone, without rigorous proof, as a theorem, which was subsequently proved by von Neumann: a system of unitary operators

---

[50]We follow here von Neumann 1931 (187) and speak of one degree of freedom for the sake of simplicity of notation. But the same considerations apply for a system with any finite number of degrees of freedom.

[51]Cf. Weyl 1928b, IV, 14 (201). See also von Neumann 1931, 571 (187).

$U(\alpha), V(\beta)$ that obey the Weyl commutation relation is univocally determined up to unitary equivalence, or is reducible to such systems.[52]

As Stone noted, the significance of the theorem had been already pointed out by Weyl.[53] The theorem justifies Weyl's claim that the various formulations (by Schrödinger, Heisenberg, and Dirac) of the quantization of a physical system are equivalent, in the rigorous sense that they are unitarily equivalent representations of the same operator algebra on the Hilbert space, that is, the algebra determined by the Weyl commutation relation. But it is not clear what Weyl thought about the conditions that have to be assumed in order for the theorem to hold.

There are two main conditions that have to be met for the Stone-von Neumann theorem to hold. First, as we have seen, one has to substitute unitary operators $U(\alpha), V(\beta)$ for unbounded operators P and Q. In other words, one has to reformulate the Heisenberg commutation relation as the Weyl commutation relation. Without this reformulation, the Hilbert space representations of the Heisenberg commutation relation are not unitarily equivalent, for there are representations in which it has no solutions, i.e., representations in which momentum and position cannot be determined. However, we know today that this reformulation, although necessary, is not sufficient for unitary equivalence. There exist unitarily inequivalent representations of systems with a finite number of degrees of freedom, whose observables obey the Heisenberg commutation relation, but not the Weyl

---

[52]Cf. Stone 1930, 174 (176) and von Neumann 1931, 577 (187). More rigorously, for a Hilbert space $\mathcal{H}$ and a structure preserving mapping π from an abstract algebra $\mathfrak{A}$ to $\mathcal{H}$, any irreducible representation $(\mathcal{H}, \pi)$ of $\mathfrak{A}$ is univocally determined up to a unitary transformation (*bis auf eine unitäre Transformation eindeutig festgelegt*). Any two representations $(\mathcal{H}_1, \pi_1)$ and $(\mathcal{H}_2, \pi_2)$ are unitarily equivalent if and only if there is a unitary operator $\hat{U} : \mathcal{H}_1 \to \mathcal{H}_2$ such that $\pi_1(A) = \hat{U}\pi_2(A)\hat{U}^*$ for all elements $A \in \mathfrak{A}$.

[53]Weyl was indeed aware of the Stone-von Neumann theorem. He acknowledged Stone's precise formulation of the theorem, as well as von Neumann's emerging proof. See Weyl 1930, 407 (203).

commutation relation.[54]

Secondly, and more importantly, the Stone-von Neumann theorem applies only to physical systems with a finite number of degrees of freedom. If one considers physical systems with an infinite number of degrees of freedom (i.e., an infinite number of particles and an infinite volume), as in quantum statistical mechanics and quantum field theory, unitary equivalence fails. In the case of physical systems with an infinite number of degrees of freedom, this theorem is false, since the algebra determined by the Weyl commutation relations possesses an infinity of unitarily inequivalent representations. This is significant for Weyl's view on scientific objectivity, for unitary inequivalence entails that a theory describing a system with an infinite number of degrees of freedom fails, in an appropriate sense, to be categorical. This shows that, on his view, our predictively most successful theories do not provide objective knowledge.

This is a serious challenge, which philosophers attempted to respond to in recent literature. The first response points out that the failure of the Stone-von Neumann theorem just indicates the need to adjust our physical theories in a way that allows us to regain unitary equivalence. The second response emphasizes that one needs to alter, in a certain sense, the conditions under which objectivity may obtain, despite the failure of this theorem. But it may be argued that neither of these responses addresses the challenge in a fully satisfactory manner, although the latter is, as we will see, more promising.

One recent proposal for the adjustment of quantum field theory is to eliminate the unitarily inequivalent representations, and simply consider fields *as if* they had only a finite number of degrees of freedom.[55] The proposal is based on the

---

[54]Cf. Schmüdgen 1983 (162).

[55]Cf. Wallace 2006 (189).

assumption that at least some degrees of freedom (i.e., those at short distance and high energy) may be considered a "pure mathematical artefact," which is taken to justify the belief that their corresponding Hilbert space representations can be "cut off" as having no physical significance. The cut-off procedure is meant to establish that the resulting theory, the so-called *effective* quantum field theory, has only one unitary equivalence class of representations.

However, one problem with this proposal is that, even if unitarily inequivalent representations are physically insignificant, by cutting them off one loses an important heuristic tool for the development of a physical theory. To see this, consider an analogy with mathematics, where non-categoricity is sometimes thought to provide precisely such a tool. John Wesley Young had emphasized, already in 1911, the heuristic advantages of non-categorical systems of axioms:

> Is it always desirable that the set of assumptions from which we build up a mathematical science be categorical? The answer depends upon the object we have in view. It is an advantage to keep the set non-categorical as long as possible, for the reason that, if we build up a science on a set of assumptions which is non-categorical, there will be more than one system of things which satisfies the assumptions, that is, there will be at least two essentially distinct concrete representations of it. There will thus be a gain in generality. ... In general, *by starting with a non-categorical set of assumptions, we can develop a part of several abstract sciences at the same time.* We obtain, in that way, a theory which may be part of a great many different theories.[56]

Young seems to have thought that the existence of non-isomorphic interpretations of an axiomatic system is a significant advantage, in that its efficiency with respect to the development of a theory is greater than that of a categorical system. The elimination of non-isomorphic interpretations entails, on his view, the loss of an

---

[56]Cf. Young 1911, 49f (225).

important tool, and should, therefore, be postponed until after a certain stage in the development of the theory. Similarly, one could argue, the elimination of the unitarily inequivalent representations of a physical theory may not be a heuristically optimal procedure, even if they are physically insignificant.

But a more serious problem with this elimination stems from the fact that these representations are actually *not* physically insignificant. For an illustration of this problem, let us briefly consider how unitary inequivalence helps one account, in quantum mechanical statistics, for phase transitions. Examples of such transitions are evaporation, magnetization, superconductivity, and the like. The classical thermodynamic description of a physical system says that, at certain temperatures and pressures, the system can be in multiple phases at equilibrium. For example, at a temperature of -38.83C and a pressure of 0.2mPa, mercury exists at equilibrium in three phases, solid, liquid, and gas. Also, water, ice and steam coexist at 0.01C and 611Pa. In quantum statistical mechanics, in order to account for phase transitions, that is, to derive the equations that govern the behavior of a physical system undergoing such transitions, one typically considers the system in the thermodynamic limit. In other words, one stipulates that the physical system has an infinite number of degrees of freedom, i.e., an infinite number of particles and an infinite volume, but a finite density. Unless the system is considered in the thermodynamic limit, the partition function that describes its behavior does not display any singularities, i.e., any non-analyticities in the free energy.

It is easy to see the consequence of adjusting quantum statistical mechanics by eliminating unitarily inequivalent representations. Such an elimination makes it impossible to account, in quantum statistical mechanics, for phase transitions, and also entails that there is no physical difference between the multiple phases

151

a system may be in at equilibrium.[57] Thus, in quantum mechanical statistics, unitarily inequivalent representations are, in a certain sense, indispensable for an account of natural phenomena like phase transitions and, therefore, ought not to be eliminated.

However, the problem raised by the elimination of unitarily inequivalent representations in quantum mechanical statistics may strike one as irrelevant to quantum field theory. Indeed, the argument that it is wrong to consider in quantum field theory a physical system as if it had only a finite number of degrees of freedom, because in quantum mechanical statistics one needs to consider a physical system as if it had an infinite number of degrees of freedom, might seem unconvincing. For one might easily question the justification of the latter claim by denying that going to the thermodynamic limit is the right approach to phase transitions.[58] It would presumably be more convincing to argue that it is wrong to consider in quantum field theory a physical system as if it had only a finite number of degrees of freedom, because considering that physical system as having an infinite number of degrees of freedom is necessary. But *is it* necessary, in quantum field theory, to consider a physical system as having an infinite number of degrees of freedom, rather than to consider it as if it had only a finite number of degrees of freedom? The answer to this question is yes: unitarily inequivalent representations *are* indispensable for providing an account of the natural phenomena to which quantum field theory applies. For the equations that govern such phenomena are typically derived as the result of spontaneous symmetry breaking, and spontaneous symmetry breaking is understood as the singling out of one unitary equivalence class

---

[57]Cf. Ruetsche 2003, 1335-1338 (155).

[58]See, e.g., Callender 2001 (24).

among the infinitely many unitarily inequivalent representations of the theory.[59]

The underlying idea that one cannot dispense with unitarily inequivalent representations in quantum field theory suggests another response to the challenge raised by unitary inequivalence against Weyl's view on objectivity. This response admits that one should not eliminate these representations, and proposes instead that one adopt a "profoundly modal" conception of structure, that one should "modally expand the relevant notion of structure."[60] In other words, objectivity would require a modal expansion of the abstract algebra of quantum field theory, that is, the algebra determined by the Weyl commutation relations. The result "would have to be viewed as a structure that effectively spans physically possible worlds, rather than being confined to the actual one." But how can this modal structure be defined? And how is it supposed to support objectivity?

The suggestion here is meant to accommodate the fact that no unitary equivalence class of representations can, by itself, provide knowledge about the structure of the real world, for reasons already discussed above. So the modal structure that one should search for cannot be that of a particular unitary equivalence class of representations. Instead, it may be taken to be a structure defined by the relations between unitarily inequivalent representations themselves. But even if such a structure may be defined, what does it mean to say that it "spans" physically possible worlds? This seems to assume that unitarily inequivalent representations describe physically possible worlds. But can all of them do so? If not, which of them do? What is it that renders a representation of the abstract algebra physically possible? How can one distinguish the unitarily inequivalent represen-

---

[59]Cf. Earman 2004 (42).

[60]Cf. French 2010 (70). The view that quantum physics describes the "modal structure of the world" is also defended in Ladyman and Ross 2007 (117).

tations that describe physically possible worlds from those that describe physically impossible worlds?

Let us suppose that a criterion can be found that identifies all unitarily inequivalent representations that describe physically possible worlds.[61] The view about objectivity that seems to be supported by this response to the challenge under consideration is, we have to note, essentially different from Weyl's own view, since it allows – indeed it demands – that the requirement of categoricity be dropped. On this view, the structure of the real world is defined not by the abstract algebraic structure of a theory, but by the "modal" structure determined by the relations between the unitarily inequivalent representations of that abstract algebraic structure. But if the real world may be represented in this way, then, as we briefly explain below, the real world must be considered modally dappled.

The notion of a dappled world has been motivated by the observation that the methodology of natural science, i.e., its predictive and explanatory strategies, are diverse rather than uniform. This determined some to believe that the world is dappled, in the sense that it is ontologically diverse, rather than uniform, and that scientific methods and laws apply locally, rather than globally.[62] For instance, in phenomenological theories, like hydrodynamics, laws apply only to a certain domain of objects, and they do so only under certain conditions. Similarly, in the theories of fundamental physics, like quantum field theory, laws apply to a

---

[61]One criterion that has been proposed for identifying unitarily inequivalent representations that describe physically possible worlds is given by the so-called Doplicher-Haag-Roberts theory, according to which, the representations that should be taken to describe physically possible worlds are those that are locally different from a vacuum representation, i.e., one that corresponds to the vacuum state of the physical system under consideration (where local differences between representations are determined by the excitations of the vacuum state). Hans Halvorson takes this criterion to support a position that he calls "representation realism" (Halvorson 2006 (80)).

[62]Cf. Cartwright 1999 (25).

different domain of objects, and they do so, again, only under certain conditions.

This is, of course, a controversial issue. One has recently argued, for instance, that methodological and nomological diversity does not entail ontological diversity. Thus, theories might be dappled, but not the world itself.[63] Whereas the ontology of the so-called phenomenological theories is one of idealized objects, like continuous fluids, the ontology of fundamental physics is not: quarks and gluons, or some such other things to be discovered, are what the real world is made of. This argument has been, in turn, criticized from an instrumentalist perspective, by pointing out that the ontology of fundamental physics is as idealized as that of a phenomenological theory.[64] Thus, the world of physics *is* dappled, but it is not the real world: even if methodological and nomological diversity entails ontological diversity, all scientific ontology is one of idealized objects.

However, the argument against the dappling of the real world can be criticized also from a different perspective. For, as already suggested, if the structure of the real world may be defined via the relations between the unitarily inequivalent representations of an algebraic quantum field theory, then the real world must be, in a certain sense, dappled. The failure of unitary equivalence in quantum field theory indicates, as we have seen, that one should consider the relations between its unitarily inequivalent representations, rather than any unitary equivalence class, as defining the structure of the real world. But if unitarily inequivalent representations may be taken to describe physically possible worlds, it follows that the structure of the real world should be defined via the relations between physically possible worlds. This indicates that the real world is modally dappled, i.e., that

---

[63]Cf. Sklar 2003 (173).

[64]Paul Teller emphasized that quarks and gluons are idealized objects because their existence is based on a quantization scheme that assumes a flat or regularly curved space-time (cf. Teller 2004 (179)).

the real world is ontologically diverse, rather than uniform, in a modal sense.[65]

The view that the real world is modally dappled raises, to be sure, several questions, which remain to be further investigated. For example, one might point out, even if one could accommodate the notion of a real world as distinct from the actual physical world, it is not clear whether *all* unitarily inequivalent representations that describe physically possible worlds can be identified.[66] But if one fails to identify all unitarily inequivalent representations that describe physically possible worlds, then one does not seem to be far better off than the advocate of the outright elimination of unitarily inequivalent representations, for one would seem to likewise cut off representations that may not be physically insignificant, thereby incurring the problematic consequences recorded above.

4.7   Conclusion

In this chapter, we discussed Weyl's ultimate view on scientific objectivity. We started from his observation, to which we were led in the previous chapter, that objectivity cannot be reached through wholly contentual reasoning and abstraction (of a traditional empiricist or phenomenological variety) as a method of scientific concept formation, but demands non-contentual or partly symbolic reasoning and free creation as a method of concept formation, whereby concepts are defined implicitly or contextually by their position as symbols in the fundamental laws of

---

[65]We should note, *en passant*, that the important metaphysical significance of what we called above modal dappling is that it refuted actualism, i.e., the view according to which everything that exists is actual, rather than fundamentalism, i.e., the view that all physical reality is governed by fundamental laws, which was Cartwright's main target. Drawing more general lessons from this interaction between physics and metaphysics is left here for further closer consideration.

[66]What justifies the belief, shared by the proponents of the Doplicher-Haag-Roberts criterion, that some representations that are not locally different from the vacuum representation may not describe physically possible worlds?

a scientific theory. We argued that this approach to scientific concept formation determined Weyl to believe that Hilbert's mathematics, even if it lacks explanatory power, has a crucial objectifying role: it is indispensable for the formation of objective scientific theories. We then explained the relation between objectivity and the categoricity of a theory, and presented Weyl's notion of concordance as a criterion for the univocal coordination between freely created concepts and perceptual experience, which, if it is taken to hold up to isomorphism, renders a theory complete or categorical.

After pointing out that already certain results in mathematical logic raise doubts about Weyl's view on objectivity, we discussed the non-categoricity of quantum physics, suggested by the failure of the Stone-von Neumann theorem in quantum field theory. We argued that the current proposals for coming to terms with the existence of unitarily inequivalent representations are problematic, since they lead to the elimination of representations that have physical significance and, thus, may render theories unable to account for the natural phenomena that they were designed to account for. This problem may be overcome, however, if a criterion for identifying the representations that describe physically possible worlds is found. In this case, the world that quantum field theory attempts to provide scientific knowledge of turns out to be a modally dappled world, i.e., a world whose structure spans physically possible worlds.

In the final chapter of the dissertation, we summarize the argument for Weylean skepticism, and then argue, against this type of skepticism, that the tension that Weyl saw between objectivity and intelligibility can be dissolved by clarifying the relation between transcendent axiomatics and scientific understanding.

CHAPTER 5

CONCLUSION: FREE CREATION AND UNDERSTANDING

5.1   Summary of the Main Argument

In the preceding chapters, we argued that, according to Weyl, the conditions required for scientific objectivity could only be satisfied at the expense of intelligibility, and that, conversely, the conditions required for scientific understanding could only be satisfied by sacrificing objectivity. We take this idea to emphasize a skeptical aspect of his philosophical thinking, perhaps its most characteristic aspect, albeit one that contemporary scholars working on Weyl have unduly neglected. In this chapter, we first summarize the argument that we presented in support of this idea, and then criticize one of its main premises – the claim that scientific understanding requires wholly contentual reasoning. We want to defend the view that free creation as a method of concept formation has a significant contribution to bringing about scientific understanding, which may thus be obtained through partly non-contentual or purely symbolic reasoning, provided that one can establish that such reasoning is simple and reliable, in a sense to be explained.

Weyl rejected the notion of understanding from within, as we have seen in section 2.7 above, as inadequate to mathematics, i.e., as unable to account for the kind of proof transparency required to explain why a mathematical theorem is true. As developed along the maker's knowledge tradition, the understanding from

within may be taken as based on a feeling of evidence that is allegedly produced by one's creatorly experiences. Under the influence of Husserl's phenomenology, Weyl came to see, so we argued, that if one takes evidence as a feeling, one misconceives the nature of evidence and the nature of its relation to truth. Nevertheless, he did not fail to take note of the various attempts to consider the understanding from within as a scientific approach to knowledge. In "Physics and Biology," one of the texts appended to the 1949 English edition of his *Philosophy of Mathematics and Natural Science*, Weyl cited the work of Richard Woltereck, who had developed an account of biology from the perspective of intuitive understanding:

> Scientists would be wrong to ignore the fact that theoretical construction is not the only approach to the phenomena of life; another way, that of understanding from within (interpretation), is open to us. Woltereck, in a broadly executed *Philosophie der lebendigen Wirklichkeit*, has recently ventured to describe in some detail the "within" of organic life.[1]

It might sound strange to approach biological phenomena from this perspective, i.e., to attempt to provide objective knowledge about living organisms on the basis of a feeling of evidence produced by one's creatorly experiences. For, although we may be said to have such experiences in the case of simple balances, mathematical objects, or commonwealths, the same obviously does not hold in the case of living organisms.[2]

Weyl, at any rate, noted that the conditions required for understanding from within are entirely distinct from those required for scientific objectivity: "Both roads run, as it were, in opposite directions. . . For objective theory the un-

---

[1]Cf. Weyl 1949a, 283 (211).

[2]It might seem, nevertheless, less strange to consider living organisms *as if* they were artefacts and, thus, that one may have a feeling of evidence produced by one's experiences considered *as if* they were creatorly experiences. See, for example, Cheney and Seyfarth 1990 (27).

derstanding from within can serve as a guide to important problems although it cannot provide their objective solutions."[3] But this raises the question if, according to Weyl, there is a different notion of understanding that, whether it serves as a guide or not, may not conflict with objectivity. In particular, it raises the question whether understanding, as based on a Husserlian conception of evidence as an experience of truth, rather than on a conception of evidence as a feeling, can provide objective solutions. Can the road of understanding, conceived of phenomenologically, and that of scientific objectivity, run in the same direction?

Husserl's conception of evidence as an experience of truth, which we discussed in section 2.5 above, was developed around the idea that a proposition is evident if and only if the meaning intentions that it is taken to express are completely satisfied by actually presenting in intuition the intended objects. Weyl argued, as we have seen, that this conception supports the idea that provability cannot render evidence an improper basis for belief, which led him to conclude that Dedekind's principle that, in science, one ought not to believe a provable proposition without proof is a mere epistemological perversity. Weyl also seems to have thought that experiencing the truth of a proposition is a proper basis for understanding why it is true, provided that the experience of truth is admitted as a proof. As we have stressed above, an experience of truth may only be admitted as an immanent axiomatic proof, that is, one based on wholly contentual reasoning and ideational abstraction as a method of concept formation. Thus, the question was raised, can ideational abstraction and contentual reasoning be sufficient conditions for objectivity?

We have argued, in chapter three, that Weyl realized that they can not, for

---

[3]Cf. Weyl 1949a, 284 (211).

neither traditional empiricism nor pure phenomenology can account for the possibility of objective scientific knowledge. He came to believe that unless one allows the introduction of hypothetical elements in physics, i.e., real but in principle unobservable entities (like the electromagnetic field), one cannot overcome phenomenalism, of the sort proposed, according to Weyl, in Leibniz' monadology. But concepts like that of an electromagnetic field should be regarded as freely created concepts, rather than ones obtained by abstraction from intuition, since an electromagnetic field cannot be presented in intuition. This indicates that Weyl took non-contentual or purely symbolic reasoning as necessary for scientific objectivity.

Two problems appear here. First, this view clearly leads to a tension between the conditions required for understanding and those required for objectivity. This tension underlies the position that we called Weylean skepticism. Secondly, the idea that the free creation of concepts is necessary for objectivity may strike one as less than plausible: how can knowledge acquired by means of concepts freely created by the mind be regarded as knowledge about the mind-independent reality? In response to the latter problem, we discussed Weyl's requirement of categoricity, i.e., the idea that a theory can be objective provided that it is categorical, only to see that there are mathematical logical grounds for doubting that this requirement can be satisfied (section 4.5 above), and furthermore that categoricity actually fails in quantum physics (section 4.6). Thus, we argued, if objective knowledge is to be obtained, one must drop or modify this requirement.

In the remainder of this chapter, we attempt to show that Weylean skepticism is untenable. In order to do so, we want to identify the conditions that may be required for scientific understanding without an experience of truth of the sort envisaged by Husserl and Weyl.

5.2   Einstein on Free Creation and Understanding

Let us start by emphasizing the role that hypothetical elements are normally thought to have in bringing about scientific understanding. Boltzman, for example, noted the following:

> In order to understand the phenomena which actually occur, we may draw conclusions from hypothetical assumptions, that is, from processes which, though possible on analogy with similar phenomena in other circumstances, cannot be observed and may not even be observable in the future, owing to their speed or small size or something similar.[4]

This remark raises, of course, a question about the exact relation between hypothetical assumptions and scientific understanding. The typical answer to this question emphasizes that there is a causal connection between the unobservable processes and the observable natural phenomena that one purports to understand. It is the existence of this causal connection that seems to provide support for scientific understanding. For example, this seems to be the idea that underlies Einstein's view in the following passage:

> We can distinguish various kinds of theories in physics. Most of them are constructive. They attempt to build up a picture of the more complex phenomena out of the materials of a relatively simple formal scheme from which they start out. Thus the kinetic theory of gases seeks to reduce mechanical, thermal, and diffusional processes to movements of molecules – i.e., to build them up out of the hypothesis of molecular motion. When we say that we have succeeded in understanding a group of natural processes, we invariably mean that

---

[4] "Zum Zwecke des Verständnisses der wirklich gesehenen Erscheinungen können wir Folgerungen aus hypothetischer Voraussetzung ziehen, das heißt aus Vorgängen, die zwar nach sonstigen analogen Erscheinungen möglich wären, die aber – ob ihrer Schnelligkeit oder Kleinheit oder dergleichen – direkt nicht zu sehen sind und oft auch in Zukunft nicht zu sehen sein werden." (quoted in Flamm 1983, 261 (63), German version on page 277)

a constructive theory has been found which covers the processes in question.[5]

According to Einstein, one may be said to bring about understanding of natural phenomena only if a constructive theory is formulated, that is, a theory that can reconstruct these phenomena on the basis of certain hypothetical assumptions. In particular, one may be said to understand the observable behavior of gases just in case a description of this behavior can be derived from the fundamental equations of molecular motion. But why would one think that such a derivation may provide understanding? One way of looking at this is to consider that one understands the observable behavior of gases because the derivation of its description from the equations is backed up by a causal connection between molecular motion and the phenomena, i.e., the mechanical, thermal, and diffusional processes of gases.

The idea that understanding is somehow dependent on a causal connection is quite plausible, indeed, especially if one considers more mundane examples. For it is true that one understands, say, why one's house burned if one knows not only that it burned because of its faulty wiring, but also how faulty wiring may lead to fire.[6] That is, one understands why one's house burned only if one knows that there is a causal connection between its faulty wiring and its having burned. This example suggests a general belief: "understanding is not some sort of super-knowledge, but simply more knowledge: knowledge of causes."[7]

However, things appear to be more complicated than this. For Einstein's view also suggests that the introduction of concepts like that of molecular motion, in terms of which hypothetical assumptions – the fundamental equations of molecular

---

[5]Cf. Einstein 1919, 228 (43).

[6]Cf. Pritchard 2009 (149).

[7]Cf. Lipton 2004, 30 (121).

motion – are formulated, is necessary for understanding. But he famously opposed the view that scientific concepts are introduced by abstraction from perceptual experience and, thus, that they have a direct connection with experience. Therefore, according to Einstein, scientific understanding requires that the concepts that are typically considered to have a direct connection with experience, as well as the relations between such concepts, be logically derived from some fundamental concepts and relations, which are not directly connected with experience. These latter may, themselves, be derived from other, even more fundamental concepts and relations, which are freely posited:

> An adherent to the theory of abstraction or induction might call our layers "degrees of abstraction"; but I do not consider it justifiable to veil the logical independence of the concept from the sense experiences. The relation is not analogous to that of soup to beef but rather of check number to overcoat. ... As a matter of fact, we are dealing with freely formed concepts.[8]

Hence, it seems correct to say that Einstein believed that understanding requires that concepts be freely created, rather than created by abstraction from perceptual experience. This indicates that, according to him, a formal axiomatic approach to science, of the type advocated by Hilbert, is indispensable for scientific understanding. But if this is a correct account of Einstein's view, then this raises doubts about the idea that scientific understanding always requires a constructive theory, that is, the idea, which Boltzman also seems to have had in mind, that only a causal connection between physical processes can support scientific understanding. For, on a formal axiomatic approach to natural phenomena, the derivation of the equations governing these phenomena from the fundamental presuppositions

---

[8]Cf. Einstein 1936, 293f (45). See also Einstein 1944 (46).

of a theory might not be backed up by any causal connection. Indeed, to give just one example, this is the case of thermodynamic phase transitions, the derivation of which typically requires, as we have seen in section 4.6 above, the idealizing assumption that physical systems are infinite.

Einstein's view raises a fundamental question about the relation between scientific idealization and understanding. We want to sketch, in the following sections, what we take to be the most plausible answer to this question, that understanding requires that reasoning through idealization be controllable and simpler, in a certain sense, than reasoning without idealization.

5.3 Understanding and Idealization Control

Scientific idealization is often said to be "the cornerstone of understanding essential features of certain kinds of physical systems."[9] But how, more precisely, should one conceive of the relation between idealization and understanding?

One suggestion is that idealization procedures are indispensable for scientific understanding because they "provide the kind of detailed knowledge required to answer causal questions."[10] Taking a physical system to the thermodynamic limit, as in the example discussed in section 4.6 above, would thus allegedly allow one to identify the causes of phenomena like phase transitions. But this does not seem to be correct. For in what sense might one say that there is a causal connection between the physical processes in an infinite physical system and observable phenomena like the phase transitions? Indeed, to see what is wrong with this suggestion, one may consider again the aforementioned mundane example, in which one seeks to understand why one's house burned. By analogy, the suggestion seems

---

[9]Cf. Morrison 2009, 127 (139).

[10]Cf. Morrison 2009, 127 (139).

to amount to the claim that in order to understand why one's house burned, one must consider that house as an infinite physical object, for this would reveal the causal connection between its faulty wiring and the fire.

The relation between scientific idealization and understanding should be approached, we submit, by appeal to the notion of *idealization control.* One can find this notion, for example, in the recent philosophical literature on statistical mechanics:

> The theory of the thermodynamic limit has been highly fruitful. Results may be rigorously obtained in interesting idealized cases. Furthermore, there is in this theory a nice sense of "control" over the limiting process in that one can not only prove things about the limit but get estimates on deviation from the limit in finite cases.[11]

But what does it mean to say that one has a sense of control over a scientific idealization? How could an idealization be controlled? What may be required for this to be the case? Unfortunately, no clear conception of idealization control seems to be available: "Admittedly this notion of controllability is one that is vague and open-ended, and one that would require a great deal more in the way of explication before it could be considered truly understood and a legitimate concept to employ in methodology."[12]

However, a clear conception of idealization control can be obtained by considering more closely the notion of concordance, first discussed in section 4.4 above. According to Weyl, let us recall, concordance characterizes a scientific theory just

---

[11]Cf. Sklar 1993a, 78f (170).

[12]Cf. Sklar 2000, 45 (172). See also Batterman 2005, 235 (8): "The idea that some idealizations are controllable and others uncontrollable is prevalent in physics. However it is a difficult task indeed to try to make the distinction precise. Very roughly, let us say that an idealization is controllable means that it is possible, via appeal to theory, to compensate in some way for the idealization."

in case its highly theoretical methods, which deploy scientific idealization, lead to results that agree (within the limits of experimental error) with the results obtained through observational methods. It seems natural to believe that a proof of concordance can provide a "nice sense of control" over scientific idealization.[13] In order to prove concordance, one can point out that the use of idealization in scientific reasoning renders this reasoning partly non-contentual or purely symbolic. Thus, the suggestion here is that to gain control over idealization requires that one prove, through contentual reasoning, that the outcome of purely symbolic reasoning agrees with the outcome of contentual reasoning, within the limits of experimental error.

This, of course, is basically the view defended by Hilbert. He noted, for example in his 1919/1920 lectures, that despite progress in the foundations of mathematics, the most important things – the external consistency (or concordance) of classical analysis and the (logical) consistency of arithmetic – are still unproved.[14] Hilbert thought that these proofs would definitely establish the reliability of the

---

[13]It is fair to note that Sklar, himself, gestured towards a conception of idealization control via a "comparison between our idealized solutions and the solutions obtained when a less idealized description of the system is invoked." (Sklar 2000, 62 (172)) But he also noted that we need the "theoretical resources to tell us in what ways, and to what degree, the conclusions we reach about the idealized model can be expected to diverge from the features we will find to hold experimentally of the real system in the world." (Sklar 1993b, 258 (171)) This suggests, interestingly, that he believes that idealization is controllable even if a theory lacks concordance, provided that the type and extent of disagreement between observational and theoretical methods can be accurately described in advance.

[14]"Zum Beispiel ist noch nicht einmal wirklich bewiesen worden, dass die Regeln für das gewöhnliche mathematische Rechnen stets zu übereinstimmenden Ergebnissen führen müssen. Noch viel weiter sind wir davon entfernt, einen Beweis für die Widerspruchslosigkeit der Arithmetik zu besitzen; vielmehr wird diese überall da, wo man sonst die Widerspruchslosigkeit einer mathematischen Theorie beweist, immer schon vorausgesetzt." (Hilbert 1992, 34f (88)) The same idea, that not only logical, but also external consistency, needs to be proved was later on acknowledged also by Gödel, who stated that in order to secure the classical rules of the mathematical calculus, the formalist needs to show "that the rules of the equational calculus applied to equations demonstrable in [a formal system] S between primitive recursive terms yield only correct numerical equations (provided that S possesses the property which is asserted to be unprovable)." (Gödel 1972, 305 (76))

non-contentual parts of mathematical reasoning, in the sense that consistency and concordance proofs that proceed entirely through contentual reasoning would justify the belief that results obtained by means of non-contentual reasoning are true. The idea that such proofs should proceed entirely through contentual reasoning motivated his *finitary* attitude:

> The objects of number theory are for me . . . the signs themselves, whose shape can be generally and certainly recognized by us – independently of space and time, of the special conditions of the production of the sign, and of insignificant differences in the finished product. The solid philosophical attitude that I think is required for the grounding of pure mathematics – as well as for all scientific thinking, understanding, and communication – is this: *In the beginning was the sign.*[15]

It seems to be clear from this passage that, according to Hilbert, a finitary attitude, motivated by the idea that consistency and concordance proofs should proceed entirely through contentual reasoning, is also necessary for scientific understanding. This may be taken to mean, arguably, that in order to achieve an idealization control that is sufficiently strong for bringing about scientific understanding, finitary reasoning is necessary for consistency and concordance proofs.[16] Here is how Bernays explained the way such proofs are supposed to proceed.

"It was Hilbert's idea," Bernays wrote, "to use for this purpose [i.e., for the purpose of giving consistency and concordance proofs] the symbolic formalizing of mathem[atical] theories, so that instead of conceptual structures we get figurative structures.'[17] For example, in order to prove the concordance of classical analysis, one must consider its conceptual structure as a figurative structure and show that

---

[15]Cf. Hilbert 1922, 1121f (86).

[16]Of course, precisely what Hilbert considered to be finitary reasoning has been a matter of debate, which need not concern us here. For discussion, see Zach 2001 (226), chapter 4.

[17]Manuscript Hs973:18, in the Bernays collection at the ETH Zürich.

no symbolic expression of contradiction could be derived by following its formal rules of reasoning. Similarly, in order to prove, say, quantum statistical mechanics concordant, one must take its conceptual structure as a figurative structure and show that it is impossible to derive a symbolic expression of contradiction by following its formal rules of reasoning. More precisely, one has to prove that it is impossible to derive a symbolic expression of a deviation – larger than experimental error – of the results based on theoretical methods from the results obtained by observational methods.

Adopting a finitary attitude, i.e., regarding the conceptual structure of a theory as a figurative structure, is thus necessary for obtaining idealization control. One can argue that the kind of idealization control provided by a finitary proof of concordance can guarantee that the epistemic quality of scientific results is not vitiated by idealization, since the (meta-theoretical) evaluation of the corresponding figurative structures does not involve idealization, i.e., it proceeds entirely by means of contentual reasoning.[18] Thus, epistemic quality is not vitiated because a finitary proof of concordance can show that reasoning through idealization is as justificatorily powerful as reasoning without idealization.

Here we encounter two problems. First, one might deny that this is really the epistemic significance of a finitary proof of concordance. Weyl himself came to believe that, in fact, neither concordance nor consistency needs to be proved, and he argued, as we will see in the next section, that the formalist commitment to proving them is unjustified. Was Weyl right in thinking so, or did he merely fail to realize the epistemic significance of such proofs? Secondly, one might insist that even if concordance could be established, and so even if idealization could be

---

[18]For a defense of this interpretation of Hilbert's view, see Detlefsen 1986 (38).

169

controlled, this is not sufficient for understanding. We should, therefore, ask what further conditions, if any, might be required for reasoning through idealization to be (at least) as *explanatorily* powerful as reasoning without idealization. We shall offer a tentative answer to this question in section 5.5 below.

5.4   Weyl on Consistency and Concordance Proofs

As emphasized in section 4.2 above, Weyl seems to have believed that proving consistency and concordance is insufficient for vindicating Hilbert's formalism with respect to mathematical understanding. In this section, we want to show that, after Gödel's second incompleteness theorem, Weyl thought that proving consistency and concordance was not necessary, either. An argument for the latter claim can be found in the following passage:

> [The formalist's] interest in the fact that no contradiction occurs can hardly be justified or can at most be justified by the following remark. If two games lead to the formulas b and ∼b, then, if a is an arbitrary given formula, it is possible to obtain the formula a by two additional moves, as final result. It is consequently *a priori* certain that one can prove any arbitrary formula a and one has a simple fixed rule according to which to do it. In this case the game would be tedious; still it would only be tedious if I knew the contradiction.[19]

The argument that Weyl attributed to the formalist as a means to justify the commitment to proving consistency seems to be the following. If a contradiction exists in a system, then one knows that (and how) every formula can be derived in the system. But if one knows that (and how) every formula can be derived, then the game of proving is a tedious one. According to Weyl, the formalist's motivation to prove consistency stems from the desire to show that this game is

---
[19]Cf. Weyl 1929, 158 (202).

not tedious.

Weyl considered this a bad argument. On his view, the existence of a contradiction is not itself enough to make the proving game tedious. Rather, the contradiction has to be known, for only if one knows the contradiction can one know that (and how) every formula can be derived in the system. Thus, according to Weyl, since tediousness is a consequence of known inconsistency, but not a consequence of unknown inconsistency, the formalist's motivation to prove consistency, and thereby show that the game is not tedious, is misguiding, if no contradiction has been yet revealed in the system.

The formalist could point out, nevertheless, that while a system might be tedious only if one knew a contradiction, an inconsistent system would be worthless as a cognitive tool even if one didn't. For, its unknown inconsistency implies its unreliability, which entails that the formulas that are actually proved in the system are not properly justified. That one is not aware of their lack of proper justification does not, of course, turn their actual proofs into proper justifications. The formalist's motivation to prove consistency does not stem, then, from the desire to show merely that the proving game is not tedious, but rather from the desire to show that it is not unreliable.

Some denied, however, that only a consistency proof can establish reliability. Wittgenstein, as is well known, famously set out "to alter the attitude to contradiction and to consistency proofs."[20] His motivation for doing so was given by the attempt to understand what a purported consistency proof might achieve. Wittgenstein asked: "How can a proof have put the calculus right in principle? How can it have failed to be a proper calculus until this proof was found?"[21]

---

[20]Cf. Wittgenstein 1956, 213 (222).

[21]Cf. Wittgenstein 1956, 219 (222).

and suggested that the idea that only a consistency proof can show that a formal system is reliable is simply based on a misunderstanding. For suppose, said Wittgenstein, that

> I wanted to play this game in such a way as to follow rules "mechanically" and I "formalized" the game. But in doing this I reached positions where the game lost all point; I therefore wanted to avoid these positions "mechanically". – The formalization of logic did not work out satisfactorily. But what was the attempt made for at all? (What was it useful for?) Did not this need, and the idea that it must be capable of satisfaction, arise from a lack of clarity in another place?[22]

The formalist's commitment to proving consistency stems, Wittgenstein added, from a lack of clarity with respect to the nature of a mathematical proposition. Mathematical propositions are, on his view, rules of syntax, instructions that tell us how to proceed.[23] Can two rules, he asked, contradict one another?

> Suppose two of the rules were to contradict one another. I have such a bad memory that I never notice this, but always forget one of the two rules or alternately follow one and then the other. Even in this case I would say everything's in order. The rules are instructions how to play, and as long as I can play they must be right. They only cease to be all right the moment I *notice* that they are inconsistent, and the only sign for that is that I can't apply them any more. For the logical product of the two rules is a contradiction, and the contradiction no longer tells me what to do. And so the conflict only arises when I notice it. While I could play there was no problem.[24]

Hence, just like Weyl, Wittgenstein believed that if no contradiction is known, then the formalist's commitment to proving consistency has no serious motivation, even

---

[22]Cf. Wittgenstein 1956, 217 (222).

[23]Cf. Wittgenstein 1975, 145 (223).

[24]Cf. Wittgenstein 1975, 321f (223).

if the system is actually inconsistent. For, on his view, only known inconsistency implies unreliability.[25] The search for a consistency proof, Wittgenstein further suggested, is motivated only by Hilbert's anxiety to "set our minds at rest." But, according to Wittgenstein, as long as we are not aware of a contradiction, our minds should not need to be set at rest at all. Mathematics, in other words, does not need a consistency proof in order to be considered reliable.

Wittgenstein's position regarding the significance of a consistency proof is as untenable as Weyl's own view, since one can similarly point out that an inconsistent system would be unreliable even if one were not aware of its inconsistency. A finitary consistency proof was meant to establish that no inconsistency exists and, thus, that the system is reliable. But the desire to establish reliability does not entail that Hilbert, himself, was skeptical about the consistency of mathematics, and that he needed a consistency proof in order to allay his own anxiety, his own restlessness of mind.[26] Rather, according to the formalist, a consistency proof would show that a proper justification of theorems does not necessarily require the restrictions that the intuitionist and predicativist impose on mathematical proof, but that, as one sometimes put it, such restrictions are based on mere prejudice.[27]

At any rate, Weyl continued to believe that the formalist's commitment to proving consistency is unjustified. Fully aware of the limitations entailed by Gödel's second incompleteness theorem with regard to the ability of a formal axiomatic system to prove its own consistency, Weyl recommended that the mathematical logician ought to adopt the same attitude with regard to consistency that the physicist adopts with regard to concordance:

---

[25]Cf. Wittgenstein 1956, 219 (222). For more detailed discussions of Wittgenstein's view on consistency, see Shanker 1987, 220-258 (165), Rodych 1997 (152).

[26]For a development of this point, see Franks 2009, ch. 2 (67).

[27]This is, at least, how Bernays put it. Cf. Bernays 1922, 15f; Eng. tr., 219 (14).

> Do we really have to be guaranteed the consistency for eternity, or could we just defer the revision of our formalism until a contradiction arises from it? The mathematician can hardly accept such resignation. The physicist, however, works with physical laws which are tested against all known phenomena. To him, it is a matter of course that these laws might at some point be thrown over board due to some newly discovered facts.[28]

Hence, Weyl advocated a view according to which the problem of consistency in mathematics should be treated in the same way as the problem of concordance in physics. In physics, he believed that rather than being proved, concordance is and must be merely restored, if it is found to be lacking, that is, in case of disagreement (larger than may be due to experimental error) between observation ("newly discovered facts") and any result obtained by reasoning through idealization. Similarly, instead of trying to prove consistency, mathematicians should merely attempt to restore it, when they become aware of a contradiction.

This view is unsatisfactory in at least a couple of ways. It indicates once again that Weyl failed to see the epistemic significance of a consistency proof. Without a consistency proof, as we already pointed out, no proper justification can be given in a system that includes transfinite components. But the view also indicates that he failed to see the epistemic significance of a concordance proof. As we suggested in the previous section, a concordance proof should be taken to provide idealization control, without which no scientific understanding may be obtained.

However, even if one agrees that this is the epistemic significance of a concordance proof, the question still remains whether idealization control is enough

---

[28] "Muss uns wirklich die Widerspruchslosigkeit für alle Ewigkeit garantiert werden, oder können wir nicht eine etwa nötige Revision des Formalismus vertagen bis zu jenem Augenblick, da tatsächlich aus ihm ein Widerspruch hervorgegangen ist? Dem Mathematiker fällt dieser Verzicht schwer. Der Physiker arbeitet mit physikalischen Gesetzen, deren Bewährung darin liegt, daß sie mit allen bekannten Erscheinungen in Einklang stehen; ihm ist es selbstverständlich, daß er stets darauf gefasst sein muss, sie einmal durch neu entdeckte Tatsachen über den Haufen geworfen zu sehen!" (Weyl 1953, 533 (214))

for scientific understanding. Can we say that one really understands natural phenomena, and that one really understands why a mathematical theorem is true, if these are derived by provably concordant reasoning through idealization?

5.5   Understanding and Simplicity

What further conditions might be required for provably concordant reasoning through idealization to have (at least) as much explanatory power as reasoning without idealization? In this section, we focus on the relationship between simplicity and understanding. We would like to argue that, at least in some cases, one may understand natural phenomena derived by means of provably concordant reasoning through idealization, provided that such reasoning is simpler, i.e., more thought-economical, than reasoning without idealization.

Einstein, as is well known, believed that the free creation of concepts can serve scientific understanding provided that the theoretical principles formulated in terms of concepts freely introduced by the theorist are, in a certain sense, simple and economical: "It is the very essence of our striving for understanding that, on the one hand, it attempts to encompass the great and complex variety of man's experience, and that on the other, it looks for simplicity and economy in the basic assumptions."[29] He also suggested that the simplicity of basic assumptions depends on their economy, i.e., on the number of concepts used to formulate them: "[as] the distance in thought between the fundamental concepts and laws on one side and, on the other, the conclusions which have to be brought into relation with our experience grows larger and larger, the simpler the logical structure becomes –

[29]Cf. Einstein 1950b, 357 (48). Here is also a more recent declaration: "To have theories which we can actually apply in describing and understanding the world we have no choice but to work with nature to do what it does not sufficiently do by itself: We must simplify further." (Teller 2001, 394 (178))

that is to say, the smaller the number of logically independent conceptual elements which are found necessary to support the structure."[30]   Thus, simplicity would require not only that the freely created concepts used to formulate the laws be simple, as one might be inclined to believe, but also that their number be minimal.

One could ask, of course, why would fewer concepts entail simpler laws? Here one might point out, for example, that the fewer the concepts used in formulating the basic laws, the fewer the symbols used to express them. One might also argue that the fewer the concepts used in formulating the basic assumptions, the fewer the number of entities, or kinds of entities, postulated by the theory.[31]   But one should emphasize perhaps that it is not only simplicity of basic laws, but simplicity of reasoning as well, that would be needed for scientific understanding. For, in the practice of science, as Weyl so often pointed out, the simplicity of laws may be often obscured by the complexity of reasoning.

The question about the criteria for simplicity of reasoning was considered by Hilbert as one of the most fundamental epistemological questions about mathematics.[32]   The most common approach to this question regards simplicity as a measure of descriptive length or, as one might put it, as a type of syntactic elegance: simplicity requires the minimization of the number of symbols (and lines) in a proof.[33] But how can syntactic elegance contribute to bringing about understanding? The answer that we suggest below is that provably concordant reasoning through idealization may be capable of bringing about understanding

---

[30] "Der gedankliche Abstand zwischen den grundlegenden Begriffen und Grundgesetzen einerseits und den mit unseren Erfahrungen in Beziehung zu setzenden Konsequenzen andererseits immer mehr zunimmt, je mehr sich der logische Bau vereinheitlicht, d.h. auf je weniger logisch voneinander unabhängige begriffliche Elemente man den ganzen Bau zu stützen vermag." (Einstein 1934, 273 (44))

[31] For recent discussions of these old ideas, see, e.g., Barnes 2000 (7), Baker 2003 (6).

[32] See Hilbert 1918, 1113 (85) and, for a historical account, Thiele 2003 (180).

[33] For an overview, see Pudlák 1998 (150).

only if its syntactic elegance renders it more thought-economical than reasoning without idealization.

At the end of the nineteenth century, Ernst Mach maintained that science, as developed by biological organisms like us, can be conceived of as an adaptation, that is, as an optimization strategy for obtaining genuine scientific knowledge of natural phenomena – a strategy that maximizes epistemic benefits and minimizes epistemic costs:

> Within the short span of a human life and with man's limited powers of memory, any knowledge worthy of the name is unattainable except by the *greatest* economy of thought. Science itself, therefore, may be regarded as a minimum problem, consisting of the completest possible presentation of facts with the *least possible expenditure of thought*.[34]

If Mach is right that science can be regarded as an optimization strategy, then science strives for maximizing epistemic benefits and, at the same time, minimizing epistemic costs. On his view, epistemic benefits are maximized when science provides the most complete presentation of facts, and epistemic costs are minimized when the presentation is most thought-economical. According to Mach, scientific idealization (for instance, the concept of a space of more than three dimensions) is essential in this respect: reasoning through idealization is most thought-economical because it focuses attention on signs and operations with signs, rather than on the objects signified and the operations with such objects. That purely syntactic operations are most thought-economical is obvious, Mach believed, in mathematics:

---

[34]"In der kurzen Zeit eines Menschenlebens und bei dem begrenzten Gedächtnis des Menschen ein nennenswerthes Wissen nur durch die *grösste* Oekonomie der Gedanken erreichbar [ist]. Die Wissenschaft kann daher selbst als eine Minimumaufgabe angesehen werden, welche darin besteht, möglichst vollständig die Thatsachen mit dem *geringsten Gedankenaufwand* darzustellen." (Mach 1883, 461 (124); Eng. tr., 490)

Even a *total* disburdening of the mind can be effected in mathematical operations, for operations of counting hitherto performed are *symbolised* by mechanical operation with signs, and our brain energy, instead of being wasted on the repetition of old operations, is spared for more important tasks. The merchant pursues a like economy, when, instead of directly handling his bales of goods, he operates with bills of lading or assignments of them.[35]

Hence, on Mach's view, if science can be regarded as an optimization strategy, and if reasoning through idealization is more thought-economical than reasoning without idealization, then one is justified to deploy reasoning through idealization in attempting to provide genuine scientific knowledge, i.e., the most complete presentation of facts. One might similarly argue that if science strives to bring about understanding of natural phenomena in the most efficient way, and if reasoning through idealization is, as a matter of fact, more thought-economical than reasoning without idealization, then one is justified to deploy reasoning through idealization in attempting to bring about understanding.

But this argument is open to criticism. Mach, himself, acknowledged that reasoning through idealization is typically believed to cause unintelligibility. He also claimed, however, that this criticism is due to a failure to see science as an optimization strategy:

The mathematician who pursues his studies without clear views on this matter [i.e., on the nature of science] must often have the uncomfortable feeling that his paper and pencil surpass him in intelligence. Mathematics, thus pursued as an object of instruction, is scarcely of more educational value than busying oneself with the Cabala. On the

---

[35] "Bei mathematischen Operationen kann sogar eine *gänzliche* Entlastung des Kopfes eintreten, indem man einmal ausgeführte Zähloperationen durch mechanische Operationen mit Zeichen *symbolisiert* und statt die Hirnfunction auf Wiederholung schon ausgeführter Operationen zu verschwenden, sie für wichtigere Fälle spart. Aehnlich sparsam verfährt der Kaufmann, indem er, statt seine Kisten selbst herumzuschieben, mit Anweisungen auf dieselben operirt." (Mach 1883, 459 (124); Eng. tr., 488)

contrary, it induces a tendency toward mystery, which is pretty sure
to bear its fruits.[36]

Thus, Mach considered that although one is right to believe that symbolic reasoning may be a cause of unintelligibility in educational contexts, one is wrong to think that this is so in scientific practice. An optimization strategy for teaching science is, as perhaps ought to be, different than an optimization strategy for scientific research.

The above argument is also problematic, perhaps more seriously so, in that it does not make clear one's justification for believing that science strives to bring about understanding *in the most efficient way.* Mach's own conviction that science is an optimization strategy may, indeed, be defended on the basis of biological principles of adaptation, but as Husserl emphasized, this basis is epistemologically inadequate.[37] To be adequately defended, the belief that science strives to bring about understanding in the most efficient way should be justified on a rational, rather than biological, basis.

However, the conviction that science is an optimization strategy *can* be rationally justified. For Husserl, himself, took the search for the smallest possible set of axioms for a given domain of investigation to be a consequence of what he called the principle of maximum rationality:

If all matters of fact obey laws, there must be some minimum set of laws, of the highest generality and maximum deductive independence,

---

[36] "Wer Mathematik treibt, ohne sich in der angedeuteten Richtung Aufklärung zu verschaffen, muss oft den unbehaglichen Eindruck erhalten, als ob Papier und Bleistift ihn selbst an Intelligenz überträfen. Mathematik in dieser Weise als Unterrichtsgegenstand betrieben ist kaum bildender, als die Beschäftigung mit Kabbala oder dem magischen Quadrat. Nothwendig entsteht dadurch eine mystische Neigung, welche gelegentlich ihre Früchte trägt." (Mach 1883, 460 (124); Eng. tr., 489.)

[37] Cf. Husserl 1900, Prolegomena, chapter 9 (102). See also McGinn 1972 (132).

from which all other laws can, by mere deduction, be derived. ... This goal or principle of maximum rationality we recognize with insight to be the supreme goal of the rational sciences. It is evident that it would be better for us to know laws more general than those which, at a given time, we already possess, for such laws would lead us back to grounds deeper and more embracing.[38]

According to Husserl, identifying the smallest possible set of most general and logically independent axioms minimizes epistemic costs while maximizing epistemic benefits by increasing the explanatory power of a scientific theory. This entails that his principle of maximum rationality justifies rationally and, thus, on his view, adequately, Mach's conviction that science is an optimization strategy. Furthermore, it suggests that the belief that science strives to bring about understanding in the most efficient way can also be justified on a rational basis.

Nevertheless, one could point out that whereas Husserl's principle of maximum rationality may be taken to justify thought-economy via a minimization of axiomatic basis, it does not justify thought-economy via a minimization of the expenditure of thought in reasoning from the axioms. One might, thus, point to an ambiguity in the above argument: the type of optimization strategy based on partly non-contentual or purely symbolic reasoning is not the type of optimization strategy that scientists are justified to adopt in attempting to bring about understanding. For, as Weyl emphasized, such reasoning lacks the complete transparency given by the experience of truth that characterizes wholly contentual reasoning. As a consequence, the use of idealization in attempting to bring about

---

[38] "Ordnet sich alles Tatsächliche nach Gesetzen, so muß es einen kleinsten Inbegriff möglichst allgemeiner und deduktiv voneinander unabhängiger Gesetze geben, aus welchen sich alle übrigen Gesetze in reiner Deduktion ableiten lassen. ... Dieses Ziel, bzw. Prinzip größtmöglicher Rationalität erkennen wir also einsichtig als das höchste der rationalen Wissenschaften. Es ist evident, daß die Erkenntnis allgemeinerer Gesetze als jener, die wir jeweils schon besitzen, wirklich das Bessere wäre, sofern sie eben auf tiefere und weiter umfassende Gründe zurückleiteten." (Husserl 1900, Prolegomena 56 (102))

understanding is not rationally justified.

Note, however, that one's insistence on regarding the experience of truth as an indispensable condition for scientific understanding would also entail that neither is the type of optimization strategy based on the minimization of axiomatic basis the type of optimization strategy that scientists are justified to adopt in attempting to bring about understanding. For if one believes that understanding why a theorem is true requires that one experience its truth, in the phenomenological sense clarified in chapter two of this dissertation, then one believes, as Weyl seems to have done, that understanding why a theorem is true demands that this be as evident as the axioms. This entails that one's insistence on regarding the experience of truth as an indispensable condition for scientific understanding reveals a strategy based on the maximization, rather than minimization, of axiomatic basis. But this goes against Husserl's own principle of maximum rationality.

Summing up, we can say that one is justified in believing that understanding can be brought about by means of provably concordant reasoning through idealization, provided that the requirement of simplicity, in the sense of thought-economy, is also satisfied. Undoubtedly, the development of this view requires further work. In particular, one would need to show that the type of reasoning whereby natural phenomena like, say, phase transitions are typically accounted for is simple and controllable, in the senses specified above. But the little we have said here about this view on scientific understanding seems enough to motivate resistance to Weylean skepticism. We conclude that this type of skepticism is untenable because, as far as we are able to see, there is no irremediable conflict between the conditions required for scientific objectivity and those needed for scientific understanding. In opposition to what Weyl seems to have believed, it is reasonable

to expect that scientific theories can provide an objective and intelligible account of natural phenomena, that is, an account that justifies their mind-independent reality and renders them understandable. However, a more detailed account of the conditions under which one can attain objectivity without categoricity, and understanding without an experience of truth, needs to be articulated and evaluated.

BIBLIOGRAPHY

[1] Artin, M. (1991) *Algebra*, Prentice Hall, New Jersey.

[2] Atiyah, M. (1978) 'The Unity of Mathematics' in *Bull. Lond. Math. Soc.* 10, 69-76, reprinted in *Collected Works*, vol 1, Clarendon Press, Oxford, 279-286.

[3] Atiyah, M. (2003) 'Hermann Weyl' in *Biographical Memoirs* 82, 320-335.

[4] Awodey, S. and E. Reck (2002) 'Categoricity and completeness. Part I: Nineteenth-century Axiomatics to Twentieth-century Metalogic. Part II: Twentieth-Century Metalogic to Twenty-first-Century Semantics' in *History and Philosophy of Logic*, 23, 1-30, 77-94.

[5] Bacon, R. (1268) *The 'Opus majus' of Roger Bacon*, ed. by J.H. Bridges, Frankfurt, Minerva, 1964. Partial Eng. tr. as 'On experimental science' in *Medieval Philosophy*, ed. by J. F. Wippel and A. B. Wolter, The Free Press, 1969, 288-291.

[6] Baker, A. (2003) 'Quantitative Parsimony and Explanatory Power' in *The British Journal for the Philosophy of Science* 54, 245-259.

[7] Barnes, E. C. (2000) 'Ockham's Razor and the Anti-Superfluity Principle' in *Erkenntnis* 53, 353-374.

[8] Batterman, R. W. (2005) 'Critical phenomena and breaking drops: Infinite idealizations in physics', in *Studies in History and Philosophy of Modern Physics* 36, 225244.

[9] Becker, O. (1927) *Mathematische Existenz*, Max Niemayer, 1973.

[10] Bell, J. (2000) 'Hermann Weyl on Intuition and the Continuum' in *Philosophia Mathematica*, III, 8, 259-273.

[11] Bell, J. (2003) 'Hermann Weyl's Later Philosophical Views: His Divergence from Husserl' in R. Feist (ed.) *Husserl and the Sciences*, University of Ottawa Press.

[12] Berkeley, G. (1710) *A Treatise concerning The Principles of Human Knowledge*, in *The Works of George Berkeley*, ed. by A. A. Luce and T. E. Jessop, London, Nelson, 1964, vol. 2.

[13] Berlin, I. (1969) 'A Note on Vico's Concept of Knowledge' in *Giambattista Vico. An International Symposium*, ed. by G. Tagliacozzo, Baltimore, The Johns Hopkins Press, 371-377.

[14] Bernays, P. (1922) 'Über Hilberts Gedanken zur Grundlegung der Arithmetik' in *Jahresbericht der Deutschen Mathematiker-Vereinigung* 31, 10-19. Eng. tr. in P. Mancosu (ed.) *From Brouwer to Hilbert. The Debate on the Foundations of Mathematics in the 1920s*, Oxford Univ. Press, 215-222.

[15] Bolzano, B. (1804) 'Betrachtungen über einige Gegenstände der Elementargeometrie,' Eng. tr. in *Bolzano's Mathematical Works*, Oxford Univ. Press, 2004, 25-81.

[16] Bolzano, B. (1810a) 'Beyträge zu einer begründeteren Darstellung der Mathematik,' Eng. tr. in *Bolzano's Mathematical Works*, Oxford Univ. Press, 2004, 83-137.

[17] Bolzano, B. (1810b) 'Allgemeine Mathesis' in *Gesamtausgabe*, Nachgelassene Schriften, vol. 5: *Mathematische und Philosophische Schriften 1810-1816*, Frommann Verlag, Stuttgart, 13-64.

[18] Bolzano, B. (1817) 'Rein analytischer Beweis des Lehrsatzes, daß zwischen je zwey Werthen, die ein entgegengesetztes Resultat gewähren, wenigstens eine reelle Wurzel der Gleichung liege,' Eng. tr. in *Bolzano's Mathematical Works*, Oxford Univ. Press, 2004, 251-278.

[19] Bolzano, B. (1837) *Wissenchaftslehre*, in *Gesamtausgabe*, vol. 12, Frommann Verlag, Stuttgart, 1969. Selections edited and translated by Rolf George as *Theory of science*, University of California Press, 1972.

[20] Borel, A. (1986) 'Hermann Weyl and Lie groups' in *Hermann Weyl, 1885-1985*, ETH, Zrich, 53-82.

[21] Breidert, W. (1977) 'Mathematik und symbolische Erkenntnis bei Nikolaus von Kues' in *Mitteilungen und Forschungsbeitrge der Cusanus-Gesellschaft* 12, 116-126.

[22] Brentano, F. (1930) *Wahrheit und Evidenz*, ed. by Oskar Kraus, Leipzig, F. Meiner. Eng. tr. as *The True and the Evident*, ed. by R. Chisholm, New York, The Humanities Press, 1966.

[23] Buzaglo, M. (2002) *Solomon Maimon: Monism, Skepticism, and Mathematics*, University of Pittsburgh Press.

[24] Callender, C. (2001) 'Taking Thermodynamics Too Seriously' in *Studies in History and Philosophy of Modern Physics* 32, 539-553.

[25] Cartwright, N. (1999) *The Dappled World. A Study of the Boundaries of Science*, Cambridge University Press.

[26] Chayut, M. (2001) 'From the Periphery: the Genesis of Eugene P. Wigner's Application of Group Theory to Quantum Mechanics' in *Foundations of Chemistry* 3, 55-78.

[27] Cheney, D. L. and R. M. Seyfarth (1990) *How monkeys see the world: inside the mind of another species*, University of Chicago Press.

[28] Chrudzimski, A. and W. Huemer (eds.) (2004) *Phenomenology and analysis : essays on Central European philosophy*, Frankfurt am Main, Lancaster.

[29] Corcoran, J. (1980) 'Categoricity' in *History and Philosophy of Logic* 1, 187-207.

[30] Cristin, R. and K. Sakai (eds.) (2000) *Phänomenologie und Leibniz*, Freiburg, Karl Alberg.

[31] Cusa, N. (1458) *De Beryllo*, Eng. tr. by J. Hopkins in *Nicholaus of Cusa: Metaphysical Speculations*, Minneapolis, A.J. Banning Press, 1998.

[32] Cusa, N. (1460) *De Possest*, Eng. tr. by J. Hopkins in *A Concise Introduction to the Philosophy of Nicholas of Cusa*, Minneapolis, A.J. Banning Press, 1986.

[33] da Costa, N. and S. French (2003) *Science and partial truth: a unitary approach to models and scientific reasoning*, Oxford University Press.

[34] da Silva, J. J. (1997) 'Husserl's phenomenology and Weyl's predictivism' in *Synthese* 110, 277-296.

[35] Debs, T. A. and M. Redhead (2007) *Objectivity, Invariance, and Convention*, Harvard University Press.

[36] Dedekind, R. (1888) *Was sind und was sollen die Zahlen?*, Braunschweig, Vieweg.

[37] Dedekind, R. (1932) *Gesammelte mathematische Werke* III, Braunschweig, Vieweg.

[38] Detlefsen, M. (1986) *Hilbert's Program: An Essay on Mathematical Instrumentalism*, Dordrecht, Kluwer.

[39] Detlefsen, M. (2011) 'Dedekind Against Intuition: Rigor, Scope and the Motives of His Logicism' forthcoming in *Proceedings of the Logic and Knowledge Conference*, University of Rome-Sapienza, June 16-19, 2010.

[40] Devitt, M. (1997) *Realism and Truth*, second edition, Princeton University Press.

[41] Dilthey, W. (1883) *Einleitung in die Geisteswissenschaften* in *Gesammelte Schriften* I, Leipzig, 1914.

[42] Earman, J. (2004) 'Curie's Principle and spontaneous symmetry breaking' in *International Studies in the Philosophy of Science* 18, 173-198.

[43] Einstein, A. (1919) 'What is the Theory of Relativity?' in *The London Times*, November 28, reprinted in *Ideas and Opinions*, Crown Publishers, 1954, 227-232.

[44] Einstein, A. (1934) 'On the Method of Theoretical Physics' in *Mein Weltbild*, Amsterdam, reprinted in *Ideas and Opinions*, Crown Publishers, 1954, 270-276.

[45] Einstein, A. (1936) 'Physics and Reality' in *The Journal of the Franklin Institute* 221, 3, reprinted in *Ideas and Opinions*, Crown Publishers, 1954, 290-323.

[46] Einstein, A. (1944) 'Remarks on Bertrand Russell's Theory of Knowledge' in *The Philosophy of Bertrand Russell*, ed. by P. A. Schilpp, reprinted in *Ideas and Opinions*, Crown Publishers, 1954, 18-24.

[47] Einstein, A. (1950a) 'On the Generalized Theory of Gravitation' in *Scientific American* 182/4, April 1950, reprinted in *Ideas and Opinions*, Crown Publishers, 1954, 341-356.

[48] Einstein, A. (1950b) 'Message to the Italian Society for the Advancement of Science' in *Impact*, Autumn 1950, reprinted in *Ideas and Opinions*, Crown Publishers, 1954, 356-359.

[49] Eisler, R. 1904 *Wörterbuch der philosophischen Begriffe*, available online at http://www.textlog.de/eislerwoerterbuch.html

[50] Ewald, W. (1996) *From Kant to Hilbert: A Source Book in the Foundations of Mathematics*, Clarendon Press, Oxford.

[51] Feferman, S. (1988) 'Weyl vindicated: *Das Kontinuum* 70 years later' in *In the Light of Logic*, New York, 1998.

[52] Feferman, S. (1997) 'The significance of Hermann Weyl's *Das Kontinuum*, available online at http://math.stanford.edu/ feferman/papers/DasKontinuum.pdf

[53] Fichte, J. G. (1792) 'Aenesidemus, or Concerning the Foundations of the Philosophy of the Elements issued by Prof. Reinhold in Jena' in *Allgemeine Literatur-Zeitung*, 1794, reprinted in *Sämtliche Werke und Nachlass*, vol. 1. Eng. tr. in G. di Giovanni and H. S. Harris (trs. and eds.) *Between Kant and Hegel*, SUNY Press, 1985.

[54] Fichte, J. G. (1794) *Grundlage der gesamten Wissenschaftslehre* in *Sämtliche Werke und Nachlass*, vol. 10, Eng. tr. in *Science of Knowledge*, New York, Appleton-Century-Crofts, 1970, 89-286.

[55] Fichte, J. G. (1796) *Grundlage des Naturrechts nach Principien der Wissenschaftslehre*, in *Sämtliche Werke und Nachlass*, vol. 3, Eng. tr. in *Foundations of Natural Right: According to the Principles of the Wissenschaftslehre*, Cambridge Univ. Press, 2000.

[56] Fichte, J. G. (1797a) 'Erste Einleitung in die Wissenschaftslehre' in *Sämtliche Werke und Nachlass*, vol. 1, Eng. tr. in *Introductions to the Wissenschaftslehre and Other Writings*, Indianapolis and Cambridge, Hackett, 1994.

[57] Fichte, J. G. (1797b) 'Zweite Einleitung in die Wissenschaftslehre' in *Sämtliche Werke und Nachlass*, vol. 1, Eng. tr. in *Introductions to the Wissenschaftslehre and Other Writings*, Indianapolis and Cambridge, Hackett, 1994.

[58] Fichte, J. G. (1798) *Wissenschaftslehre Nova Methodo*, Eng. tr. as *Foundations of Transcendental Philosophy*, Cornell Univ. Press, 1992.

[59] Fichte, J. G. (1800) *Die Bestimmung des Menschen* in *Sämtliche Werke und Nachlass*, vol. 2, Eng. tr. as *The Vocation of Man*, Hackett, 1987.

[60] Fichte, J. G. (1801) *Sonnenklarer Bericht an das grössere Publikum, über das eigentliche Wesen der neuesten Philosophie. Ein Versuch die Leser zum Verstehen zu zwingen* in *Werke* III, ed. by Fritz Medicus, Felix Meiner, Leipzig, 1908.

[61] Fichte, J. G. (1812) 'Über das Verhältniss der Logik zur Philosophie oder transscendentale Logik' in *Sämtliche Werke und Nachlass*, vol. 9, *Nachgelassenes zur theoretischen Philosophie* I, Berlin, Walter de Gruyter, 1971.

[62] Fichte, J. G. (1813) *Einleitungsvorlesungen in die Wissenschaftslehre*, in *Sämtliche Werke und Nachlass*, vol. 9: *Nachgelassenes zur theoretischen Philosophie* I, Berlin, Walter de Gruyter, 1971.

[63] Flamm, D. (1983) 'Ludwig Boltzman and his Influence on Science' in *Studies in History and Philosophy of Science* 14, 255-278.

[64] Fogel, B. (2008) *Epistemology of a Theory of Everything: Weyl, Einstein, and The Unification of Physics*, PhD Dissertation at the University of Notre Dame.

[65] Folina, J. (2008) 'Intuition Between the Analytic-Continental Divide: Hermann Weyl's Philosophy of the Continuum' in *Philosophia Mathematica*, III, 16, 25-55.

[66] Franks, P. W. (2005) *All or Nothing: Systematicity, Transcendental Arguments, and Skepticism in German Idealism*, Harvard University Press.

[67] Franks, C. (2009) *The Autonomy of Mathematical Knowledge*, Cambridge University Press.

[68] Frege, G. (1893) *Die Grundgesetze der Arithmetik*, Jena.

[69] French, S. (2002) 'A phenomenological solution to the measurement problem? Husserl and the foundations of quantum mechanics' in *Studies in History and Philosophy of Modern Physics* 33, 467-491.

[70] French, S. (2010) 'Unitary Inequivalence as a Problem for Structural Realism' (unpublished manuscript).

[71] Garber, D. (2009) *Leibniz: Body, Substance, Monad*, Oxford University Press.

[72] Gaukroger, S. (1986) 'Vico and the Maker's Knowledge Principle' in *History of Philosophy Quarterly* 3/1, 29-44.

[73] Gentzen, G. (1936) 'Die Widerspruchsfreiheit der reinen Zahlentheorie' in *Mathematische Annalen* 112, 493-565. Eng. tr. in *The Collected Papers of Gerhard Gentzen*, Amsterdam, North-Holland, 1969, 132-213.

[74] Gentzen, G. (1938) 'Neue Fassung der Widerspruchsfreiheitsbeweis für die reine Zahlentheorie' in *Forschungen zur Logik und zur Grundlegung der exakten Wissenschaften* 4, 19-44. Eng. tr. in *The Collected Papers of Gerhard Gentzen*, Amsterdam, North-Holland, 1969, 252-286.

[75] Gödel, K. (1961) 'The modern development of the foundations of mathematics in the light of philosophy' in S. Feferman *et al.* (eds.) *Collected Works* III, Oxford University Press.

[76] Gödel, K. (1972) 'Some remarks on the undecidability results' in S. Feferman *et al.* (eds.) *Collected Works* II, Oxford Univ Press, 1986, 281-306.

[77] Grondin, J. (1994) 'Leibniz and Fichte' in *Fichte: historical contexts/contemporary controversies*, ed. by D. Breazeale and T. Rockmore, Humanities Press, 181-190.

[78] Hahn, H. (1928) 'Philosophie der Mathematik und Naturwissenschaft' in *Monatshefte der Mathematik und Physik* 35, 51-55.

[79] Hales, Th. C. (2007) 'The Jordan Curve Theorem, Formally and Informally' in *The American Mathematical Monthly* 114, 882-894.

[80] Halvorson, H. (2006) 'Algebraic quantum field theory' in *Handbook of the philosophy of physics*, ed. by J. Butterfield and J. Earman, Kluwer, 731-922.

[81] Hedman, S. (2006) *A First Course in Logic. An introduction to model theory, proof theory, computability, and complexity*, Oxford University Press.

[82] Heidelberger, M. (1993) 'Force, Law, and Experiment: The evolution of Helmholtz's Philosophy of Science' in D. Cahan (ed.) *Hermann von Helmholtz and the Foundations of Nineteenth-Century Science*, Univ of California Press, 461-497.

[83] Helmholtz, H. v. (1867) *Handbuch der physiologischen Optik*, Leipzig: Leopold Voss. Eng. tr., New York, Dover Publications, 1962, vol. 3.

[84] Helmholtz, H. v. (1903) *Vorträge und Reden*, Braunschweig, Vieweg und sohn.

[85] Hilbert, D. (1918) 'Axiomatisches Denken' in *Mathematische Annalen* 36, 405-415, Eng. tr. in W. Ewald, *From Kant to Hilbert*, II, Clarendon Press, 1107-1115.

[86] Hilbert, D. (1922) 'Neu Begründung der Mathematik. Erste Mitteilung' in *Abhandlungen aus dem mathematischen Seminar der Hamburgischen Universitt* 1, 157-177, Eng. tr. in W. Ewald, *From Kant to Hilbert*, II, Clarendon Press, 1117-1134.

[87] Hilbert, D. (1927) 'Die Grundlagen der Mathematik' in *Abhandlungen aus dem mathematischen Seminar der Hamburgischen Universitt* 6, 65-85, Eng. tr. in von Heijenoort (ed.) *From Frege to Gödel*.

[88] Hilbert, D. (1992) *Natur und mathematisches Erkennen*, Birkhuser, Basel.

[89] Hilbert, D., and P. Bernays (1939) *Grundlagen der Mathematik*, vol. 2, Berlin, Springer, 2nd ed., 1970.

[90] Hintikka, J. (1974) 'Practical vs Theoretical Legacy – An Ambiguous Legacy' in *Practical Reason*, ed. by S. Krner, Yale Univ. Press, 83-102.

[91] Hobbes, Th. (1655) *De Corpore* in *The English Works of Thomas Hobbes of Malmesbury*, vol. 1, London, J. Bohn, 1839.

[92] Hobbes, Th. (1656) *Six Lessons* in *The English Works of Thomas Hobbes of Malmesbury*, vol. 7, London, J. Bohn, 1843.

[93] Höfler, A. (1903) *Grundlehren der Logik und Psychologie*, Wien.

[94] Höfler, A. and A. Meinong (1890) *Philosophische Propädeutik. Logik*, Wien/Prag and Leipzig.

[95] Hösle, V. (1990) 'Platonism and Anti-Platonism in Nicholas of Cusa's Philosophy of Mathematics' in *Graduate Faculty Philosophy Journal* 13, 79-112.

[96] Howard, D. (1992) 'Einstein and *Eindeutigkeit*: A Neglected Theme in the Philosophical Background to General Relativity' in J. Eisenstaedt and A. J. Kox (eds.) *Studies in the History of General Relativity*, Birkhuser, Boston, 154-243.

[97] Howard, D. (1997) 'A Peek behind the Veil of Maya: Einstein, Schopenhauer, and the Historical Background of the Conception of Space as a Ground for the Individuation of Physical Systems' in J. Earman and J. D. Norton (eds.) *The Cosmos of Science: Essays of Exploration*, University of Pittsburgh Press, Konstanz: Universittsverlag, 87-150.

[98] Howard, D. (2005) ' "No Crude Surfeit": A Critical Appreciation of The Reign of Relativity' available at http://www.nd.edu/ dhoward1/Reign.pdf

[99] Huntington, E. V. (1902) 'A complete set of postulates for the theory of absolute continuous magnitude' in *Transactions of the American Mathematical Society* 3, 264-279.

[100] Huntington, E. V. (1905a) 'A Set of Postulates for Real Algebra, Comprising Postulates for a One-Dimensional Continuum and for the Theory of Groups' in *Transactions of the American Mathematical Society* 6, 17-41.

[101] Huntington, E. V. (1905b) 'A Set of Postulates for Ordinary Complex Algebra' in *Transactions of the American Mathematical Society* 6, 209-229.

[102] Husserl, E. (1900) *Logische Untersuchungen*, Max Niemeyer, 7th ed., 1993. Eng. tr. by J. N. Findlay, Humanities Press, 1970.

[103] Husserl, E. (1909) *Einführung in die Phänomenologie der Erkenntnis. Vorlesung 1909*, Husserliana VII, Springer, 2005.

[104] Husserl, E. (1913) *Ideen zu einer reinen Phänomenologie und phänomenologischen Philosophie. Erstes Buch: Allgemeine Einführung in die reine Phänomenologie*, M. Nijhoff, 1976.

[105] Husserl, E. (1917) 'Fichtes Menschheitsideal' in *Aufsätze und Vorträge: 1911-1921*, Husserliana XXIV, M. Nijhoff, 1987.

[106] Husserl, E. (1923/1924) *Erste Philosophie. Erster Teil*, Husserliana VII-VIII, M. Nijhoff, 1956.

[107] Husserl, E. (1984) *Einleitung in die Logik und Erkenntnistheorie. Vorlesungen 1906/07*, Husserliana XXIV, M. Nijhoff.

[108] Husserl, E. (1994) *Briefwechsel* VII, Kluwer Academic.

[109] Jesseph, D. (2007) *Scientia* in Hobbes, *unpublished manuscript*.

[110] Jolley, N. (1986) 'Leibniz and Phenomenalism' in *Gottfried Wilhelm Leibniz: Critical Assessments*, ed. by R.S. Woolhouse, Taylor and Francis 1994, 150-167; first published in *Studia Leibniziana*, XVIII, 1, 38-51.

[111] Kant, I. (1781/1787) *Kritik der reinen Vernunft*, Eng. tr. by N. Kemp Smith, New York, St. Martin's Press, 1965.

[112] Kant, I. (1799) 'Erklärung in Beziehung auf Fichtes Wissenschaftslehre' in *Immanuel Kant: Gesammelte Schriften*, Akademie Ausgabe, vol. XII, 370-372.

[113] Kant, I. (1800) *Jäsche Logik* in J. M. Young (ed.) *Lectures on Logic*, 521-640.

[114] Köhnke, K. Ch. (1986) *Entstehung und Aufstieg des Neukantianismus. Die Deutsche Universitätsphilosophie zwischen Idealismus und Positivismus*, Suhrkamp, Frankfurt am Main.

[115] Kosso, P. (2003) 'Symmetry, Objectivity, and Design' in in *Symmetries in Physics: Philosophical Reflections*, ed. by K. Brading and E. Castellani, Cambridge University Press, 413-424.

[116] Lachterman, D. (1992) 'Mathematical Construction, Symbolic Cognition and the Infinite Intellect: Reflections on Maimon and Maimonides' in *Journal of the History and Philosophy* 30, 497-522.

[117] Ladyman, J. and D. Ross (2007) *Every Thing Must Go: Metaphysics Naturalized*, Oxford University Press.

[118] Lapointe, S. (2008) 'Bolzano, *a priori* knowledge, and the Classical Model of Science' in *Synthese* online.

[119] Latta, R. (1898) 'Introduction' in *Leibniz. The Monadology and Other Philosophical Writings*, Oxford, Clarendon Press.

[120] Leibniz, G. (1969) *Philosophical Papers and Letters*, ed. by L. E. Loemker, Dordrecht, Reidel.

[121] Lipton, P. (2004) *Inference to the Best Explanation*, Routledge, second edition.

[122] Locke, J. (1690) *An Essay Concerning Human Understanding*, London.

[123] London, F. and E. Bauer (1939) *La théorie de l'observation en mécanique quantique*, Hermann, Paris.

[124] Mach, E. (1883) *Die Mechanik in ihrer Entwickelung: historisch-kritisch dargestellt*, Leipzig. Eng. tr. as *The Science of Mechanics: a critical and historical exposition of its principles*, Chicago, 1893.

[125] Mackie, G. (1988) 'Hermann Weyl and the Application of Group Theory to Quantum Mechanics' in *Exact Sciences and their Philosophical Foundations*, ed. by W. Deppert *et. al.*, Verlag Peter Lang, Frankfurt am Main, 107-129.

[126] Mackie, J.L. (1974) 'Comment on Hintikka's 'Practical vs Theoretical Legacy – An Ambiguous Legacy" in *Practical Reason*, ed. by S. Körner, Yale Univ. Press, 103-112.

[127] Mahnke, D. (1920) 'Die Neubelebung der Leibnizschen Weltanschauung' in *Logos* 9, 363-379, reprinted in Cristin and Sakai 2000, 247-263.

[128] Mancosu, P. (1998) 'Predicativity and an Intuitionistic Excursion' in *From Brouwer to Hilbert. The Debate on the Foundations of Mathematics in the 1920s*, ed. by P. Mancosu, Oxford University Press, 65-85.

[129] Mancosu, P. (2002) 'Phenomenology and Mathematics: Weyl at a crossroads' in A. Gethmann-Siefert and J. Mittelstraß (eds.) *Die Philosophie und die Wissenschaften. Zum Werk Oskar Beckers*, Wilhelm Fink, 129-148.

[130] Mancosu, P. and Th. Ryckman (2002) 'Mathematics and Phenomenology: The Correspondence Between O. Becker and H. Weyl' in *Philosophia Mathematica* 10, 130-202.

[131] Martin, W. M. (2003) 'Nothing More or Less than Logic: General Logic, Transcendental Philosophy, and Kant's Repudiation of Fichte's *Wissenschaftslehre*' in *Topoi* 22, 29-39.

[132] McGinn, C. (1972) 'Mach and Husserl' in *Journal of the British Society for Phenomenology* 3, 146-157.

[133] Meinong, A. (1902) *Über Annahmen*, Eng. tr. at University of California Press, 1983.

[134] Mielke, E. W. and F. W. Hehl (1988) 'Die Entwicklung der Eichtheorien: Marginalien zu deren Wissenschaftsgeschichte' in in *Exact Sciences and their Philosophical Foundations*, ed. by W. Deppert *et. al.*, Verlag Peter Lang, Frankfurt am Main, 191-232.

[135] Mill, J. S. (1843) *A System of Logic*, in *John Stuart Mill's Philosophy of Scientific Method*, ed by Ernest Nagel, Hafner Publishing Company, 1950.

[136] Miner, R. (2004) *Truth in the Making*, New York and London, Routledge.

[137] Mohanty, J. N. (1995) 'The development of Husserl's thought' in *The Cambridge Companion to Husserl*, ed. by D. W. Smith, Cambridge University Press, 45-77.

[138] Morley, M. (1965) 'Categoricity in Power' in *Transactions of the American Mathematical Society* 114, 514-538.

[139] Morrison, M. (2009) 'Understanding in Physics and Biology. From the Abstract to the Concrete' in *Scientific Understanding. Philosophical Perspectives*, ed. by H. de Regt *et al.*, University of Pittsburgh Press, 123-145.

[140] Neuhouser, F. (1990) *Fichte's Theory of Subjectivity*, Cambridge Univ. Press.

[141] Norton, J. (2003) 'General covariance, gauge theories, and the Kretschmann objection' in *Symmetries in Physics: Philosophical Reflections*, ed. by K. Brading and E. Castellani, Cambridge University Press, 110-123.

[142] Nozick, R. (2001) *Invariances. The Structure of the Objective World*, Harvard University Press.

[143] Patzig, G (1973) 'Husserl on truth and evidence' in *Readings on Edmund Husserl's Logical Investigations*, ed. by J.N. Mohanty, Martinus Nijhoff, The Hague, 1977, 179-196.

[144] Penrose, R. (1986) 'Hermann Weyl, space-time and conformal geometry' in *Hermann Weyl, 1885-1985*, ETH, Zrich, 25-52.

[145] Pèrez-Ramos, A. (1988) *Francis Bacon's Idea of Science and the Maker's Knowledge Tradition*, Oxford.

[146] Pesic, P. (2009) 'Introduction' to H. Weyl, *Mind and Nature. Selected Writings on Philosophy, Mathematics, and Physics*, ed. by P. Pesic, Princeton University Press, 2009, 1-19.

[147] Pickering, A. (1984) *Constructing Quarks. A Sociological History of Particle Physics*, The University of Chicago Press.

[148] Presburger, M. (1930) 'Über die Vollständigkeit eines gewissen System der Arithmetik ganzer Zahlen, in welchem die Addition als einzige Operation hervortritt' in *Sprawozdanie z I Kongresu Matematyków Krajów Słowiańskich*, Warsaw, 92-101. Eng. tr. as 'On the Completeness of a Certain System of Arithmetic of Whole Numbers in Which Addition Occurs as the Only Operation' in *History and Philosophy of Logic* 12, 225-233, 1991.

[149] Pritchard, D. (2009) 'Knowledge, Understanding and Epistemic Value' in *Royal Institute of Philosophy Supplement* 64, 19-43.

[150] Pudlák, P. (1998) 'The Lengths of Proofs' in *Handbook of Proof Theory*, ed. by S. R. Buss, Elsevier, 547-637.

[151] Reichenbach, H. (1920) *Relativitätstheorie und Erkenntnis apriori*, Berlin, Springer.

[152] Rodych, V. (1997) 'Wittgenstein on Mathematical Meaningfulness, Decidability, and Application' in *Notre Dame Journal for Formal Logic* 38, 195-224.

[153] Rollinger, R. D. (1993) *Meinong and Husserl on abstraction and universals*, Amsterdam, Rodopi.

[154] Rosen, K. (1977) *Evidenz in Husserls deskriptiver Transzendentalphilosophie*, Verlag Anton Hain, Meisenheim am Glan.

[155] Ruetsche, L. (2003) 'A Matter of Degree: Putting Unitary Inequivalence to Work' in *Philosophy of Science* 70, 1329-1342.

[156] Rutherford, D. (1990) 'Phenomenalism and the Reality of Body in Leibniz's Later Philosophy' in *Studia Leibnitiana* 22, 11-28.

[157] Ryckman, Th. (1995) 'Weyl, Reichenbach and the Epistemology of Geometry' in *Studies in the History of the Philosophy of Science*, 25, 6, 831-870.

[158] Ryckman, Th. (2003) 'The philosophical roots of the gauge principle: Weyl and transcendental phenomenological idealism' in *Symmetries in Physics: Philosophical Reflections*, ed. by K. Brading and E. Castellani, Cambridge University Press, 61-88.

[159] Ryckman, Th. (2005) *The Reign of Relativity: Philosophy in Physics 1915-1925*, Oxford Studies in Philosophy of Science, Oxford University Press.

[160] Sacksteder, W. (1980) 'Hobbes: The Art of the Geometricians' in *Journal of the History of Philosophy* 18, 131-146.

[161] Schlick, M. (1918) *Allgemeine Erkenntnislehre*, Berlin, Springer, 1st ed.

[162] Schmdgen, K. (1983) 'On the Heisenberg commutation relation' in *Journal of Functional Analysis* 50, 8-49.

[163] Schopenhauer, A. (1813) *Über die vierfache Wurzel des Satzes vom zureichenden Grunde: eine philosophische Abhandlung*, Eng. tr. published by Open Court, 1974.

[164] Schopenhauer, A. (1819) *Die Welt als Wille und Vorstellung*, Haffmans Verlag, Zrich, 1988. Eng. tr. published by Dover, New York, 1969.

[165] Shanker, S. G. (1987) *Wittgenstein and the Turning-Point in the Philosophy of Mathematics*, SUNY Press.

[166] Shelah, S. (1974) 'Categoricity of uncountable theories' in *Proceedings of the Tarski Symposium*, American Mathematical Society, 187203.

[167] Shin, S.-J. (1997) 'Kant's Syntheticity Revisited by Peirce' in *Synthese* 113, 1-41.

[168] Sieroka, N. (2010) *Umgebungen: symbolischer Konstruktivismus im Anschluss an Hermann Weyl und Fritz Medicus*, Chronos Verlag.

[169] Simpson, Stephen G. (1999) *Subsystems of Second Order Arithmetic*, Perspectives in Mathematical Logic, Springer, Berlin.

[170] Sklar, L. (1993a) *Physics and Chance*, Cambridge University Press.

[171] Sklar, L. (1993b) 'Idealization and Explanation: A Case Study from Statistical Mechanics' in *Midwest Studies in Philosophy* 18, 258-270.

[172] Sklar, L. (2000) *Theory and Truth*, Oxford University Press.

[173] Sklar, L. (2003) 'Dappled Theories in a Uniform World' in *Philosophy of Science* 70, 424-441.

[174] Skolem, T. (1922) 'Some remarks on axiomatized set theory' in *From Frege to Gödel: A Source Book in Mathematical Logic, 18791931*, ed. by J. van Heijenoort, Harvard University Press, 1967, 290-301.

[175] Speiser, D. (1988) 'Gruppentheorie und Quantenmechanik: the Book and its Position in Weyl's Work' in *Exact Sciences and their Philosophical Foundations*, ed. by W. Deppert *et. al.*, Verlag Peter Lang, Frankfurt am Main, 161-189.

[176] Stone, M. H. (1930) 'Linear Transformations in Hilbert Space. III. Operational Methods and Group Theory' in *Proceedings of the National Academy of Sciences of the USA* 16, 172-175.

[177] Tatzel, A. (2002) 'Bolzano's Theory of Ground and Consequence' in *Notre Dame Journal of Formal Logic* 43, 1-25.

[178] Teller, P. (2001) 'Twilight of the Perfect Model Model' in *Erkenntnis* 55, 393-415.

[179] Teller, P. (2004) 'How We Dapple the World' in *Philosophy of Science* 71, 425-447.

[180] Thiele, R. (2003) 'Hilbert's Twenty-Fourth Problem' in *The American Mathematical Monthly* 110, 1-24.

[181] Tuttle, H.N. (1976) 'The Epistemological Status of the Cultural World in Vico and Dilthey' in *Giambattista Vico's Science of Humanity*, ed. by G. Tagliacozzo and D.P. Verene, Baltimore, The Johns Hopkins University Press, 241-250.

[182] van Atten (2008) 'Thomas Ryckman: The Reign of Relativity. Philosophy in Physics 1915-1925' in *Husserl Studies* 24, 73-78.

[183] van Dalen (1984) 'Four Letters from Edmund Husserl to Hermann Weyl' in *Husserl Studies* I, 1-12.

[184] Veblen, O. (1904) 'A System of Axioms for Geometry' in *Transactions of the American Mathematical Society* 5, 343-384.

[185] Vico, G. (1725) *The New Science of Giambattista Vico*, Eng. tr. of the 1744 edition, Cornell Univ. Press., 1984.

[186] von Neumann, J. (1925) 'An axiomatization of set theory' in *From Frege to Gödel: A Source Book in Mathematical Logic, 18791931*, ed. by J. van Heijenoort, Harvard University Press, 1967, 393-413.

[187] von Neumann, J. (1931) 'Die Eindeutigkeit der Schrödingerschen Operatoren' in *Mathematische Annalen* 104, 570-578.

[188] von Neumann, J. (1932) *Mathematische Grundlagen der Quantenmechanik*, Springer Verlag.

[189] Wallace, D. (2006) 'In Defence of naiveté: the Conceptual Status of Lagrangian Quantum Field Theory' in *Synthese* 151, 33-80.

[190] Weyl, H. (1913) *Die Idee der Riemannschen Fläche*, second edition, Chelsea Publishing Company, New York, 1947.

[191] Weyl, H. (1918a) *Das Kontinuum*. In *Das Kontinuum und andere Monographien*, Chelsea Publishing Company, 1960. Eng. tr. as *The Continuum*, Thomas Jefferson Univ. Press, 1987.

[192] Weyl, H. (1918b) *Raum-Zeit-Materie*, Springer, Berlin, the 6th ed., 1923.

[193] Weyl, H. (1918c) 'M. Schlicks *Allgemeine Erkenntnislehre*' in *Jahrbuch ber die Fortschritte der Mathematik und Physik* 46/1924, 59-62.

[194] Weyl, H. (1919) 'Der circulus vitiosus in der heutigen Begründung der Analysis' in *Jahresbericht der Deutschen Mathematikervereinigung* 28, 85-92, reprinted in *Gesammelte Abhandlungen* II, 43-50. Eng. tr. in *The Continuum*, Thomas Jefferson Univ. Press, 1987, 109-117.

[195] Weyl, H. (1920) 'Das Verhältnis der kausalen zur statistischen Betrachtungsweise in der Physik' in *Schweizerische Medizinische Wochenschrift*, reprinted in *Gesammelte Abhandlungen* II, 113-122.

[196] Weyl, H. (1924) 'Randbemerkungen zu Hauptproblemen der Mathematik' in *Mathematische Zeitschrift* 20, 131-150, reprinted in *Gesammelte Abhandlungen* II, 433-452.

[197] Weyl, H. (1925) 'Die heutige Erkenntnislage in der Mathematik' in *Symposium* 1, 1-32, reprinted in *Gesammelte Abhandlungen* II, 511-542. Eng. tr. in P. Mancosu (ed.) *From Brouwer to Hilbert. The Debate on the Foundations of Mathematics in the 1920s*, Oxford Univ. Press, 123-142.

[198] Weyl, H. (1927) *Philosophie der Mathematik und Naturwissenschaften*, Oldenbourg, Berlin, Eng. tr. as Weyl 1949a.

[199] Weyl, H. (1927b) 'Quantenmechanik und Gruppentheorie' in *Zeitschrift fr Physik* 46, 1-46, reprinted in *Gesammelte Abhandlungen* III, 90-135.

[200] Weyl, H. (1928a) 'Diskussionsbemerkungen zu dem zweiten Hilbertschen Vortrag über die Grundlagen der Mathematik' in *Abhandlungen aus dem mathematischen Seminar der Hamburgischen Universität* 6, 86-88, reprinted in *Gesammelte Abhandlungen* III, 147-149. Eng. tr. in J. van Heijenoort (ed.) *From Frege to Gödel*, 482-484.

[201] Weyl, H. (1928b) *Gruppentheorie und Quantenmechanik*, Leipzig, Hirzel.

[202] Weyl, H. (1929) 'Consistency in Mathematics' in *The Rice Institute Pamphlet* 16, 245-265, reprinted in *Gesammelte Abhandlungen* III, 150-170.

[203] Weyl, H. (1930) *Gruppentheorie und Quantenmechanik*, second edition, Leipzig, Hirzel. Eng. tr. as *The Theory of Groups and Quantum Mechanics*, Dover, 1931.

[204] Weyl, H. (1931) *Die Stufen des Unendlichen*, Gustav Fischer, Jena.

[205] Weyl, H. (1932a) 'Topologie und abstrakte Algebra als zwei Wege mathematischen Verständnisses' in *Unterrichtsblätter für Mathematik und Naturwissenschaften* 38, 177-188, reprinted in *Gesammelte Abhandlungen* III, 348-358.

[206] Weyl, H. (1932b) 'Zu David Hilberts siebzigstem Geburtstag' in *Die Naturwissenschaften* 20, 57-58, reprinted in *Gesammelte Abhandlungen* III, 346-347.

[207] Weyl, H. (1935) 'Emmy Noether' in *Scripta Mathematica* 3, 201-220, reprinted in *Gesammelte Abhandlungen* III, 425-444.

[208] Weyl, H. (1940) 'The Mathematical Way of Thinking' in *Science* 92, 437-446, reprinted in *Gesammelte Abhandlungen* III, 710-718.

[209] Weyl, H. (1944) 'Obituary: David Hilbert 1862-1943' in *Obituary Notices of Fellows the Royal Society* 4, 547-553, and *American Philosophical Society Year Book* 387-395. Reprinted in *Gesammelte Abhandlungen* III, 121-129.

[210] Weyl, H. (1946) 'Review: The Philosophy of Bertrand Russell' in *The American Mathematical Monthly* 53, 208-214, reprinted in *Gesammelte Abhandlungen* III, 599-605.

[211] Weyl, H. (1949) *Philosophy of Mathematics and Natural Science*, Princeton University Press, Eng. tr. of Weyl 1927a.

[212] Weyl, H. (1949b) 'Wissenschaft als symbolische Konstruktion des Menschen' in *Eranos Jahrbuch* 16, 375-431, reprinted in *Gesammelte Abhandlungen* IV, 289-345.

[213] Weyl, H. (1952) *Symmetry*, Princeton University Press.

[214] Weyl, H. (1953) 'Über den Symbolismus der Mathematik und mathematischen Physik' in *Studium generale* 6, 219-228, reprinted in *Gesammelte Abhandlungen* IV, 527-536.

[215] Weyl, H. (1954a) 'Address of the President of the Fields Medal Committee' in *Proceedings of the International Congress of Mathematicians*, reprinted in *Gesammelte Abhandlungen* IV, 609-622.

[216] Weyl, H. (1954) "Erkenntnis und Besinnung (Ein Lebensrückblick)" in *Studia Philosophica*, reprinted in *Gesammelte Abhandlungen* IV, 631649.

[217] Weyl, H. (1968) *Gesammelte Abhandlungen*, K. Chandrasekharan (ed.), Berlin, Springer.

[218] Weyl, H. (1985) 'Axiomatic versus Constructive Procedures in Mathematics', ed. by T. Tonietti, *The Mathematical Intelligencer* 7/4, 10-17.

[219] Wigner, E. (1927) 'Über nicht kombinierende Terme in der neueren Quantentheorie' in *Zeitschrift fr Physik* 40, 492-500.

[220] Wigner, E. (1931) *Gruppentheorie und ihre Anwendung auf die Quantenmechanik der Atomspektren*, Braunschweig, F. Vieweg und Sohn.

[221] Wilczek, F. (2009) 'Introduction' to H. Weyl, *Philosophy of Mathematics and Natural Science*, Princeton University Press, second edition, 2009, 1-8.

[222] Wittgenstein, L. (1956) *Remarks on the foundations of mathematics: Bemerkungen ber die Grndlagen der Mathematik*, ed. by G. H. von Wright, Blackwell.

[223] Wittgenstein, L. (1975) *Philosophical Remarks*, ed. by R. Rhees, Barnes and Noble.

[224] Yang, C. N. (1986) 'Hermann Weyl's contribution to physics' in *Hermann Weyl, 1885-1985*, ETH, Zrich, 7-21.

[225] Young, J. W. (1911) *Lectures on Fundamental Concepts of Algebra and Geometry*, New York, Macmillan Company.

[226] Zach, R. (2001) *Hilbert's Finitism: Historical, Philosophical, and Metamathematical Perspectives*, PhD Dissertation, University of California, Berkeley.