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Wittgenstein and Philosophy of Science

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The title of this chapter may refer to remarks by Wittgenstein that could be classified as philosophy of science – Wittgenstein wrote on logic, mathematics, psychology, the natural sciences, induction, causality, anthropology, psychoanalysis and Freud – but it may also refer to his relation to philosophy of science as a discipline. I will concentrate on this latter issue and discuss Wittgenstein's influence on how philosophy of science as a discipline developed.

Philosophy of science was formed as a distinct discipline in the early twentieth century around the work of the logical positivists, or logical empiricists, originally in Vienna in the mid-twenties and in other European cities such as Berlin and Prague. It further developed in the United States, where most logical positivists moved to escape persecution by the Nazis or World War II and met the American pragmatist philosophers of science. A major turn in the history of philosophy of science, which has been dubbed "historical," took place in the late 1950s and early 1960s with the work of philosophers such as N.R. Hanson, Stephen Toulmin, T.S. Kuhn, and Paul Feyerabend. In both the formative years and the historical turn in mid-twentieth century, Wittgenstein's philosophy had a major impact, which, in the former case, was initially acknowledged but eventually obliterated and in the latter initially ignored and eventually forgotten.

In this chapter I will concentrate on Wittgenstein's impact on historical philosophy of science. This relation has been very little discussed in the literature, in contrast to Wittgenstein's relation to the logical positivists. It will be useful, however, to first address this latter relation before turning toward the one with historical philosophy of science.

1 Wittgenstein and Logical Positivism

Logical positivism, or logical empiricism, is the school of thought that is most responsible for the shaping of philosophy of science as a contemporary, distinct academic discipline in the early twentieth century. Many philosophers of the past have addressed issues that pertain to science. But it was the logical positivists who identified philosophy with a particular way of doing philosophy of science, who devoted themselves to this

A Companion to Wittgenstein, First Edition. Edited by Hans-Johann Glock and John Hyman.

activity alone – to the neglect of other areas of philosophy – and who defined the problems philosophy of science dealt with and bequeathed to later generations. Logical positivists are the "forebears" (Toulmin, 1969, p.51) of contemporary philosophers of science and their work forms "the core of our philosophical heritage" (Richardson and Hardcastle, 2003, p.x).

Logical positivism originated in a group of philosophers and scientists who had come together in the famous Vienna Circle in the 1920s. This group declared that it was indebted to Wittgenstein and in particular to his *Tractatus-Logico-Philosophicus*, first published in German in 1921. Wittgenstein is listed, along with Einstein and Russell, as a leading representative of the scientific world-conception in the Circle's manifesto of 1929. The members of the circle had read the *Tractatus*, line by line, in 1924–1925 and 1925–1926. Also, from November 1932 to March 1933 we have records of the Circle's protocols that show the development of the theses of the Vienna Circle in relation to the *Tractatus*. Rose Rand kept notes and from her edited transcripts we see what six prominent members of the group (viz., Schlick, Waismann, Carnap, Neurath, Hahn, and Kaufmann) thought about specific theses in relation to the *Tractatus*. They had six options (yes, no, meaningless, missing, indeterminate, no comment), which were marked in the transcripts by a different color or sign. In this way they expressed their views on 27 theses such as, "Language pictures reality," "The meaning [*Sinn*] of a sentence is the method of verification," "A definition is a convention" (see Stadler, 1997, pp.236–7; 323–7).

What the logical positivists found appealing in the *Tractatus,* as they read it, was the rejection of metaphysics as nonsensical, the identification of meaningful discourse with the propositions of natural science, the idea that philosophy is not a body of doctrines but the activity of analyzing language, the understanding of necessary propositions as tautologies, and the emphasis on logic and syntax (cf. Chapter 46, WITTGENSTEIN AND THE VIENNA CIRCLE; Chapter 12, METAPHYSICS: FROM INEFFABILITY TO NORMATIVITY; and Chapter 17, LOGIC AND THE *TRACTATUS*). They had anticipated some of these ideas in their own work, but found in the *Tractatus* a most appropriate expression of what they took to be a shared project. Herbert Feigl (1969, p.4), for instance, a founding member of the Vienna Circle, thought that Schlick attributed to Wittgenstein "profound philosophical insights which [Schlick] had formulated much more lucidly long before he succumbed to Wittgenstein's hypnotic spell." Schlick, on the other hand, considered Wittgenstein's work of inestimable significance (Schlick, [1928] 1979, p.136) and the decisive turning point in philosophy (Schlick, [1930] 1979). Carnap (1963, p.25) had a similar attitude – he thought that besides Russell and Frege, Wittgenstein had the greatest influence on his thinking. As Brian McGuinness (2002, p.185) notes, in general, the *Tractatus* "was greeted like Lohengrin in the opera: Ein Wunder! A miracle!"

The logical positivists, however, were far from a uniform group. The Vienna Circle was characterized by a variety of positions and attitudes. The relation of its members to Wittgenstein was also varied. Schlick and Waismann were very favorably disposed toward Wittgenstein. Waismann was called by Neurath Wittgenstein's "unhappy prophet," while Schlick was thought, again by Neurath, to be "totally Wittgensteinified" (McGuinness, 2002, pp.193, 194). Others were more skeptical or disapproving. Neurath "regarded as metaphysical the early Wittgenstein's ideas about the structure of propositional expressions mirroring the structure of the corresponding facts" and protested every time he thought they were "indulging in metaphysics" (Hempel, 2001, p.256). His protestations were so frequent that he originally proposed to just say "M!"

each time he thought the discussion was turning metaphysical. But later, to avoid constant interruptions, as Hempel recalls (2001, p.256), Neurath suggested to Schlick to let him call out "non-M" whenever they were *not* talking metaphysics. Menger (1982) had also expressed dissatisfaction with certain tenets of the group that were traced back to Wittgenstein.

Wittgenstein had agreed to regularly meet with some of the members of the Circle in the late 1920s. But after a while he preferred to limit his interactions with the group to Schlick and Waismann who undertook to put together an exposition of Wittgenstein's views. Carnap's preoccupation with Esperanto and his scientific (or, one might say, scientistic) orientation were alienating factors and drove Carnap and Wittgenstein apart. Carnap came to acknowledge a striking difference between his (and Schlick's) attitude on the one hand and Wittgenstein's on the other. Carnap (more than Schlick), Neurath, and other members of the Circle had the attitude of the intrepid, rational, unprejudiced, inquiring, enlightened scientist; Wittgenstein, according to Carnap, that of an artist.

Wittgenstein, however, had studied science and engineering and appreciated the rigor and sharpness of the scientific way of thinking. It was Wittgenstein who dismissed Carnap's scientific interest in parapsychological phenomena as not serious. Carnap thought that he would examine an important scientific problem with the impartiality and unprejudiced patience of a dissecting scientist. Wittgenstein "was shocked that any reasonable man could have any interest in such rubbish" (Carnap, 1963, p.26). On the other hand, Wittgenstein was critical of the ideological use of science, of the pretensions of scientists to offer authoritative judgments on all kinds of issues and of reducing any problem to a scientific one.

Wittgenstein's relation to Carnap was also marked by the accusation made by Wittgenstein that Carnap had plagiarized his ideas (cf. Letter to Carnap 20 August, 1932; Stadler, 2003, pp.429–34; Stern, 2007). The distance between them grew to such an extent that, as Carnap remembers in an unpublished portion of his autobiography cited by Stadler, Wittgenstein went to the extreme of not allowing his students at Cambridge to send Carnap the transcripts of his lectures.

> He asked to see the list of names [of friends and interested philosophers], and then approved all but my own. In my entire life, I have never experienced something remotely similar to this hatred directed against me. (Carnap; cited in Stadler, 2003, p.433)

The influence that Wittgenstein and his work exerted on the Vienna Circle is undeniable. It has been professed and acknowledged by the members of the group and has been documented in multiple ways. What has provoked controversy is what precisely this influence was. Wittgenstein himself "watched the development of logical positivism with growing distaste. [...] [B]y the mid-1930s he had disassociated himself entirely from ideas and doctrines which others continued to regard as *his* brain children" (Toulmin, 1969, p.36, emphasis in original). Although Wittgenstein had made several remarks on verification (for instance, in his Lectures of 1930–1933, recorded by G.E. Moore (M), in *Philosophical Remarks*, and in the conversations transcribed by Waismann (WVC)), he distanced himself from the logical positivists' "verification principle." He thought that it made philosophy look too much like mathematics (PPO 334). Anscombe remembers that "when someone mentioned the 'verification principle' at the Moral Sciences Club, Wittgenstein asked who invented it, and having it attributed to

himself, exclaimed 'Who? *Me?*' in a tone of outrage" (Anscombe, 1995, p.405, emphasis in original). In PI §353 Wittgenstein explains:

> Asking whether and how a proposition can be verified is only a special form of the question "How do you mean?" The answer is a contribution to the grammar of the proposition. (PI §353)

What Wittgenstein's "verificationism" was and how it related to that of the Vienna Circle is a controversial issue in the literature (cf. Blank, 2011; Hymers, 2005; and Wrigley, 1989). But in philosophy of science, despite Wittgenstein's disapprobation of the logical positivist doctrine, it was initially thought that the *Tractatus* was a neopositivist book and that the logical positivists were just implementing Wittgenstein's ideas. Where he was thought to have hesitated, the logical positivists were thought to have marched on. Wittgenstein's intimation of ineffability and silence at the end of the *Tractatus*, his distinction of saying and showing, were brushed aside and in their place Carnap erected the logical syntax of science, which opened up space for new problems to be addressed by philosophers of science. Only later did it fully emerge that Wittgenstein's and the logical positivists' projects differed considerably (Hacker, 1996, pp.39–66; also 2001a).

Although Wittgenstein repudiated any association with a "verification principle," it seems clear that for a brief period of time, when he was conversing with the logical positivists in 1929 and later in his lectures until the mid-1930s, he upheld a form of verificationism. "If I can never verify the sense of a proposition completely, then I cannot have meant anything by the proposition either. [...] In order to determine the sense of a proposition, I should have to know a very specific procedure for when to count the proposition as verified" (WVC 47). "The meaning of a proposition is the mode of its verification" (LWL 66). "If you want to know the meaning of a sentence, ask for its verification" (AWL 29). Wittgenstein thought that meaning and verification are internally related in the case of propositions, which he distinguished from hypotheses. Propositions were taken to be judgments about sense-data (e.g., "This is red") and completely verified, while hypotheses were thought to be confirmed or disconfirmed by empirical evidence and not definitely verified (e.g., "This is a chair") (cf. Chapter 12, METAPHYSICS: FROM INEFFABILITY TO NORMATIVITY). He thought that the propositions of physics are hypotheses. Wittgenstein understood the verification of a proposition in the manner that an expectation is related to what is expected, i.e., that we cannot describe expecting *p* without using *p* (PR§§30–1). Verification becomes an a priori affair (LWL 66) that establishes connections between propositions and gives the proposition's grammar (AWL 19).

> How far is giving the verification of a proposition a grammatical statement about it? So far as it is, it can explain the meaning of its terms. Insofar as it is a matter of experience, as when one names a symptom, the meaning is not explained. (AWL 31)

Later Wittgenstein qualified his views about verification stating that it is a mere rule of thumb (M p.59); that it does not constitute the proposition's grammar but makes a contribution to it (PI §353); and that it is one of many ways to get clear about the use of a sentence (see in Hymers 2005, p. 220).

It seems that the logical positivists understood verification quite differently. They had epistemological concerns and treated propositions as experiential hypotheses.

Even Waismann, who was so close to Wittgenstein and made statements that echoed Wittgenstein's thought (for instance, that the proposition contains its method of verification and that you cannot look for a method of verification (WVC 247, 227)), contended that the statements s_1, s_2. ... s_n connected to a proposition by a method of verification are "evidences that *speak for* or *against* the proposition p, that they *strengthen* or *weaken* it" (Waismann, 1978, p.125, emphasis in original). He thought that verification is always incomplete not only because we cannot exhaust the number of tests to verify a proposition but also because something unforeseen may always occur. This is the reason why Carnap preferred confirmation to verification.

> If by verification is meant a definitive and final establishment of truth, then no (synthetic) sentence is ever verifiable [...]. We can only confirm a sentence more and more. Therefore we speak of the problem of *confirmation* rather than of the problem of verification. (Carnap, 1936, p.420, emphasis in original)

Verification for Carnap was a matter of finding out whether "a given sentence is true or false" (1936, p.420). But this test presupposes that the sentence under consideration already has meaning. For Wittgenstein verification explains the sense of a proposition. "How a proposition is verified is what it says" (PR §166). The logical positivists used the principle of verification and the criterion of verifiability to police epistemologically meaningful discourse and develop a whole program of problems for philosophy of science – problems and paradoxes of confirmation, the confirmation of scientific theories, inductive probability, the logic of induction, etc. Wittgenstein, on the other hand, was interested in logical questions of meaning.

Recently, the Wittgenstein–Vienna Circle relation, more precisely the Wittgenstein–Carnap relation, has featured in controversies that surround the understanding of nonsense in the *Tractatus*. James Conant (2001) contends that the standard conception concerning the overcoming of metaphysics as nonsensical, advocated by Peter Hacker in his interpretation of the *Tractatus*, makes Wittgenstein and Carnap too similar since they are both taken to understand metaphysical nonsense as resulting from the violation of rules of logical syntax that govern linguistic usage (cf. Chapter 10, RESOLUTE READINGS OF THE *TRACTATUS* and chapter 12, METAPHYSICS: INEFFABILITY AND NONSENSE). Hacker (2003), on the other hand, criticizes Conant for misinterpreting and misrepresenting both Carnap and Wittgenstein. He insists on the deep differences that separate the two philosophers on multiple fronts, acknowledging at the same time that, in some respects, Carnap's argument regarding the elimination of metaphysics "does indeed converge on, although is not the same as, Wittgenstein's" (Hacker, 2001b, p.335). More recently, Oskari Kuusela (2012) has argued that it is wrong to think of Carnap's method of logical syntax as departing from Wittgenstein's approach. In Kuusela's view, it can rather be seen as an alternative, or a particular development of what Wittgenstein did. While Carnap's syntactical sentences are used to define syntactical concepts and principles, Wittgenstein's allegedly nonsensical elucidatory statements are seen by Kuusela as quasi-syntactical statements that are used to introduce the principles and the concepts of his logical notation. TLP 4.01, for instance, which says that a proposition is a picture of reality, may be taken to be similar to Carnap's example of a quasi-syntactical sentence, "Five is not a thing but a number." The latter, Carnap (1937/2001, p.285) says, is not a proper but a pseudo-object-sentence: it seems to be talking about the number five when in reality it talks about the word "five." Similarly, TLP 4.01, according

to Kuusela, does not make a metaphysical statement about propositions but is meant to introduce a syntactical concept, the propositional variable.

These developments in the literature on early Wittgenstein have not reached philosophy of science, because here there is little interest in Wittgenstein and his relation to the logical positivists. After the exodus of the Vienna Circle members (mostly to the United States), references to Wittgenstein, who had originally such a visible influence on the school, ceased. Characteristically, the philosophy of science readers that were edited by the logical positivists in the United States, which were very instrumental in fixing the identity of the discipline and in forming a community of specialists, did not include any piece by Wittgenstein or any article discussing his work. For instance, the reader edited by Feigl and Brodbeck (1953) contains over 50 articles written by logical positivists, Einstein, Poincaré, Duhem, and Russell but there is no article on or by Wittgenstein. Ronald Giere (1996, pp.337–9) observes that, in general, the European origins of logical empiricism in North America "remained in relative obscurity [...]. It was something noticeably different from what had existed in Europe." Giere's hypothesis is that Carnap and Reichenbach thought their future lay in North America and that they had to choose projects that were appealing to their new cultural and philosophical milieu and audience. The relation to Wittgenstein was obscured and forgotten for other reasons as well. There was the estrangement from Carnap, the fact that Wittgenstein's most vehement advocates were not present in North America (Schlick had been assassinated in 1936 and Waismann, alienated from Wittgenstein, was settled in England), and most importantly, Wittgenstein's philosophy did not fit the orientation that philosophy of science was taking in the New World. Wittgenstein himself thought that philosophy in the United States had taken a radically different turn from his own work (cf. Letter to W.H. Watson, 8 April 1932). As Toulmin put it (1969, p.39), the logical empiricists chose Mach over Wittgenstein.

2 Wittgenstein and the Historical Turn in Philosophy of Science

The historical turn in philosophy of science in the late 1950s and early 1960s, that is, the turn away from the study of scientific theories as sets of statements and toward the study of scientific activity in different historical periods and in the course of time, has been associated mostly with the work of philosophers such as Toulmin, Hanson, Kuhn, and Feyerabend, who were active publishing books and papers at that time in the United States. All four had a relation to Wittgenstein. Two of them knew him personally (Toulmin and Feyerabend), and all of them were influenced by his work in ways that were consequential for the development of philosophy of science.

However, Wittgenstein's role in bringing about a major turn in philosophy of science has been largely overlooked and forgotten for various reasons. Wittgenstein's philosophy was not popular in the United States in the first place (Glock, 2008). Secondly, Wittgenstein was extensively perceived as being associated with the logical positivists, who were the target of the new developments in philosophy of science. Thirdly, Wittgenstein's later philosophy, which, as I will argue, has influenced the historical turn, became widely available to the academic community only after 1953 when *Philosophical Investigations* was published. This meant that it was difficult to appreciate and discuss the Wittgensteinian elements in the work of these groundbreaking philosophers of science. Consequently, this new philosophy of science was inevitably judged

by the standards of the so-called received view, that is, the kind of philosophy of science that was dominant at that time in the United States and was formed and developed on the basis of the logical positivists' work.

Especially after the publication of Kuhn's *The Structure of Scientific Revolutions* in 1962 (second edition 1970), the discussion concentrated on whether Kuhn's, and Feyerabend's, work had breached the standards of rationality and good scientific practice. Indeed, Toulmin, Hanson, and even Feyerabend himself sided with Kuhn's critics, in certain respects disregarding the aspects of their own and Kuhn's work that could not be assimilated to the mainstream debate. Part of what was left out of consideration was precisely the Wittgensteinian elements in the work of the historical philosophers of science.

The neglect of the Wittgenstenian dimension was consequential in two respects. First, historical research was credited as the sole influence on the work of these philosophers (this is mostly the reason the turn was dubbed "historical"). Secondly, the innovative ideas that the historical philosophers of science had drawn from Wittgenstein's work were distorted and found inadequate when seen from the perspective of the received view. Historical philosophy of science was then vehemently criticized and eventually marginalized as a way of practicing philosophy of science. It has proven more influential in other fields such as science studies, where Wittgenstein's influence has also been strong (e.g., Bloor, 1983; Lynch, 1992).

In what follows, I will consider briefly in turn the connection between Wittgenstein and the four philosophers who were most responsible for bringing about the historical turn in philosophy of science in the late 1950s and early 1960s.

3 Toulmin

Stephen Toulmin, having studied physics and mathematics, attended Wittgenstein's lectures at Cambridge in 1941 and again in 1946–1947. In 1953 he published *Philosophy of Science. An Introduction*. In the preface he acknowledges his debt to Wittgenstein and W.H. Watson, whose book *On Understanding Physics* (1959), first published in 1938, he said, he had found to be a continual stimulus (Toulmin, 1953, p.vii). Watson, a physics professor at McGill University, was Wittgenstein's student and friend. He had attended Wittgenstein's lectures in 1929–1931 and had a copy, sent to him by Wittgenstein, of the manuscript of Wittgenstein's lectures of 1933–1934, which became later the "Blue Book." In his book, Watson aims at clarifying physics by doing philosophy in Wittgenstein's sense. To this end, instead of concentrating on knowledge taken to be "fixed and dead" (1959, p.xiv), he highlights, in the spirit of Wittgenstein's philosophy, the importance of scientific activity, of what scientists do, how they use knowledge, what purposes they have, what techniques they employ, and what training they get. He contends that theoretical invention in physics consists "in the erection of new logical structure, that is, in making a *system* of new ideas or devising a new method of representation" (Watson, 1959, p.18, emphasis in original). "Method of representation" is a Wittgensteinian term (see, for instance, PI §50) used also by Toulmin in *Philosophy of Science* (1953, p.34), where Toulmin repeats that "the heart of all major discoveries in the physical sciences is the discovery of novel methods of representation" and goes on to suggest that "the adoption of a new theory involves a *language-shift*" (Toulmin, 1953, p.13, emphasis in original).

Toulmin's *Foresight and Understanding* (1961) is even more clearly Wittgensteinian; although Wittgenstein's name is nowhere mentioned, Toulmin uses Wittgensteinian terminology ("paradigms," "objects of comparison"), and employs Wittgensteinian ideas. He compares, for instance, science to sports in order to claim that both terms cover a wide range of activities with similarities and dissimilarities between them that have multiple purposes. The analogy with Wittgenstein's discussion of games and his idea of family resemblance is quite obvious (PI §66). He also speaks of explanations reaching rock bottom (1961, p.42), which reminds us of Wittgenstein's justifications reaching bedrock (PI §217). In general, Toulmin rejects the attempts to capture what science is by giving what he calls "portmanteau definitions" (1961, p.15), and prefers to present science as a multifaceted activity instead. His account stresses the role of paradigms, that is, of models, ideals, and principles of regularity, to set patterns of expectation, to fix standards of rationality and intelligibility, to identify the anomalous and the accepted. These paradigms are not prejudices; they are, according to Toulmin, preconceived notions "both inevitable and proper" (1961, p.101). "We see the world through them to such an extent that we forget what it would look like without them: our very commitment to them tends to blind us to other possibilities" (1961, p.101). Toulmin's paradigms certainly foreshadow the more famous ones by Kuhn and clearly reflect Wittgenstein's discussion of paradigms and samples. Wittgenstein, who also spoke of preconceived ideas (e.g., Z §331), understood paradigms and samples as symbols that show and establish, by being followed, a particular way of conceiving the world.

4 Hanson

N.R. Hanson never met Wittgenstein but was at Oxford and Cambridge from 1949 to 1957. He was a graduate student at Oxford and held a lectureship at Cambridge from 1953 to 1957. While there he studied Wittgenstein's work and had copies of the manuscripts that were posthumously published as the *Blue and Brown Book* (Lund, 2010, p.26). Hanson's book *Patterns of Discovery* (1958) had a great impact on how philosophy of science developed. The book, especially its first chapter on "Observation," draws heavily on Wittgenstein's discussion of *seeing*. In the first chapter Hanson introduced the concept of theory-ladenness of observation, i.e., the thesis that we do not have pure, unvarnished perceptual data on which we may impose different interpretations, but rather that what we observe is already laden at the most basic level by the scientific theories that we have. According to Hanson, Brahe and Kepler may be aware of the same object when they watch the sun rise at dawn, but they actually *see* different things because of the two different astronomical theories they uphold. In Hanson's view, *seeing* is not a photochemical excitation. "People, not their eyes see. Cameras and eyeballs are blind" (1958, p.6; cf. PI §§281ff). People are the ones who have concepts, beliefs, and theories that permeate perception. This was a provocative thesis in philosophy of science since it undermined crucial tenets of the received view. According to the received view, observation statements, as distinct from theoretical statements, were taken to record pure perceptual experience. They were supposed to function as the terminus of justification, as the source of empirical meaning for the theoretical terms and sentences of scientific theories, as the tie of theories to the world, and as the theory-neutral basis for comparison and rational evaluation of theories. If observation is theory-laden, as

Hanson claimed, then there are no pure and neutral observation statements to perform all these roles. One is entrapped in theory, so to speak, and cannot test the theory against the world.

Hanson explicitly acknowledges his debt to Wittgenstein:

> It was his [Wittgenstein's] *analysis* of complex concepts such as *seeing*, *seeing as*, and *seeing that* which exposed the crude, bipartite philosophy of sense datum versus interpretation as being the technical legislation it really is. By means of philosophy he destroyed the dogma of immaculate perception. (Hanson, 1969, p.74, emphasis in original)

Hanson draws on Wittgenstein's discussion of *seeing* and *seeing as* in what is now labeled "Philosophy of Psychology – A Fragment" (PPF; cf. Chapter 33, WITTGENSTEIN ON SEEING ASPECTS), and uses *Gestalt* figures to show that even though the perceptual stimulus remains the same, what we see differs when we notice the two aspects of the drawings. Following Wittgenstein, he does not attribute the difference to two different interpretations imposed upon the same perceptual data. The concept of *seeing* "does not designate two diaphanous components, one optical, the other interpretative" (Hanson, 1958, p.9). For Hanson, one just sees something different each time, as one just hears that an oboe is out of tune without first interpreting the tones. Hanson, however, in his effort to combat the view that observation is "just opening one's eyes and looking" (1958, p.31), seems to equate *seeing* with *seeing as,* disregarding Wittgenstein's reservations. For Wittgenstein *seeing as* seems to involve an inferential process and, so, cannot be identified with *seeing* in general (see Glock, 1996, "aspect-perception"). Hanson, however, while he expressly denies that he means to identify the two notions, cites approvingly G.N.A. Vesey's statement that "all seeing is seeing as" (1958, p.182, n.5). And he says, *pace* Wittgenstein, that "the logic of 'seeing as' seems to illuminate the general perceptual case" (1958, p.19).

5 Kuhn

Thomas Kuhn was not acquainted with Wittgenstein personally. He came to his philosophy through his encounter with Stanley Cavell. They had regular meetings and conversations when they were both at Berkeley in the late 1950s, when Kuhn was finishing *Structure*. In that book, Wittgenstein makes "a cameo appearance" (Isaac, 2012, p.95) in chapter V, "The priority of paradigms." Wittgenstein is invoked there to account for the cohesion of a normal-scientific research tradition.

Kuhn realized from his historical research that scientists do not learn definitions in the abstract in order to apply them and do not adhere to a set of rules comprising "the scientific method." He couldn't, therefore, explain the practical agreement he found among scientists in scientific communities. He sought help in Wittgenstein's work. "What need we know, Wittgenstein asked, in order that we apply terms like 'chair', or 'leaf,' or 'game' unequivocally and without argument?" (Kuhn, 1970, pp.44–5). Kuhn refers to *Philosophical Investigations* and claims that Wittgenstein, instead of offering a set of characteristics that all games, for instance, share, suggests that the activities we call games bear a close "family resemblance" to each other. Instead of having a definition specifying necessary and sufficient conditions, we only need to have paradigms that resemble, in a crisscrossing way, their subsequent applications. Kuhn, however,

interpreted Wittgenstein as saying that there are natural families, each constituted by a network of certain overlapping and crisscross resemblances. In his view, if there were no natural families, we would have to suppose common characteristics to account for the success in identifying and naming different items.

This is a misunderstanding on Kuhn's part of what Wittgenstein was saying. Kuhn seems to compare the families that Wittgenstein talks about to natural kinds, a thought that implies that the members of each family already have shared qualities that we read off. But Wittgenstein did not want to say that because family members are genetically related, they exhibit certain similarities. His point was exactly that members of a family, despite their genetic relation, do not all share the same phenotypical characteristics (PI §67). For that reason, Danto (1981, pp.58–9), who recognized correctly that Wittgenstein's families are not species, was wrong to think that the concept of the family was "almost appallingly ill chosen" by Wittgenstein since it presupposes genetic affiliation. Danto was wrong because Wittgenstein introduced the idea of family resemblance to combat an essentialist understanding of concepts, i.e., to show that the unity of concepts is not secured by identifying a set of common characteristics, even if we can find them (see Chapter 25, VAGUENESS AND FAMILY RESEMBLANCE). The advantage of Wittgenstein's notion of family resemblance over definitions specifying common characteristics is not that it concerns similarities instead of essences; it is rather that agreement is secured not by appealing to definitions but by bringing together in practice varying applications that resemble each other in crisscrossing and overlapping ways. The similarities are not read off but rather established by the paradigm that is being followed.

Setting aside Kuhn's misinterpretation, following paradigms was the notion that helped Kuhn account for the cohesion exhibited in the practice of normal science. As Joel Isaac (2012, p.105) explains, the chapter on the priority of paradigms, with the reference to Wittgenstein, was not included in the penultimate draft of the book. Here is how Kuhn describes how it came to feature in the final draft:

> I wrote a chapter on revolutions, slowly but not with excessive difficulties [...]. Then I tried to write a chapter on normal science. And I kept finding that I had to – since I was taking a relatively classical, received view approach to what a scientific theory was – I had to attribute all sorts of agreement about this, that, and the other thing, which would have appeared in the axiomatization either as axioms or as definitions. And I was enough of a historian to know that that agreement did not exist among the people who were [concerned]. And that was the crucial point at which the idea of the paradigm as model entered. Once that was in place, and that was quite late in the year, the book sort of wrote itself. (Kuhn, 2000, p.296)

Kuhn denied in his last interview (2000, p.299) that he knew of Wittgenstein's use of the term "paradigm." As a matter of historical fact, it is not clear whether Kuhn took the term from Wittgenstein, or not. Cavell (2010, pp.354–5), for instance, remembers Kuhn telling him that he (Kuhn) knew of Wittgenstein's use of "paradigm." Philosophically, the connection to Wittgenstein was made by Cedarbaum (1983), while Janik and Toulmin (1973, p.284, n.12) thought that Wittgenstein's paradigms are significantly different from Kuhn's. In my view, however, the two concepts, the Kuhnian and the Wittgensteinian, are quite close (Kindi, 2012), despite Kuhn's misinterpretation. They both function as models and prototypes and they both induce and establish consensus by being followed. Learning from a paradigm (e.g., a scientific textbook or

a color sample), involves learning concepts and the use of the corresponding words, learning the objects on which to apply them, learning what is allowed and what is not. Scientists, according to Kuhn, all learn how to solve puzzles (problems that closely resemble the original paradigm), thus developing the consensual practice of normal science.

Wittgenstein's influence on Kuhn, historically and philosophically speaking, has been very little discussed in the literature. There have been some sporadic references and very few more extensive treatments of the issue (Kindi, 1995a and 1995b; also Sharrock and Read, 2002). If, however, the Wittgensteinian elements in Kuhn's work were brought to bear on the debates that followed *Structure*, then issues that have been proven highly controversial (and "incriminating" for Kuhn), such as the issue of conceptual incommensurability, would have been dealt with rather differently. For instance, the problems commonly associated with incommensurability, such as the putative threat of irrationality, would not arise given Kuhn's (and Wittgenstein's) understanding of concepts. Kuhn's critics took concepts to be closed, well-circumscribed entities that subsist, fully or partially, through time. This is a requirement if we are to establish inferential relations between theories in order to explain one by the other, reduce one to the other, or reasonably substitute one for the other. So, when Kuhn spoke of radical differences between concepts of different theories, irrationality ensued since the transition from one paradigm to the next could not be mapped onto a logical inference. But if Kuhn's work is seen from a Wittgensteinian perspective, concepts will not be viewed as entities, but rather as uses of words in different circumstances. And if concepts are seen as uses of words, then the transition from one network to the next is a difference, big or small, in application and the threat of irrationality associated with incommensurability becomes less dramatic. The rationality of transition is not judged by considering reconstructed abstract arguments involving sharply defined, entity-like concepts, but by attending to the particular circumstances of word use in order to assess the actual considerations and options in the range of possibilities available to the scientists in question. Ironically, under this interpretation, the undesired consequences of incommensurability arise not for Kuhn, but for his opponents and their understanding of concepts.

6 Feyerabend

Paul Feyerabend also made use of Wittgenstein's understanding of concepts and of how words have a meaning. In his seminal paper "Explanation, Reduction and Empiricism," which was published in 1962 (the same year as *Structure*), Feyerabend explicitly invoked Wittgenstein (in a footnote) in connection with his (Feyerabend's) contextual theory of meaning (1962, p.68, n.83). In that article he criticizes the formal theories of reduction and explanation in the sciences, advanced most prominently by Ernest Nagel and the logical empiricist Carl Hempel. Feyerabend challenged both the deductive structure of explanation and reduction and the assumption it implies of meaning invariance. Meanings have to remain the same in order for the deductive inference to work. Feyerabend maintained that the meaning of a term is given contextually, i.e., it is "dependent upon the way in which the term has been incorporated into a theory" (1962, p.68) and claimed that elements of many pairs of theories (concepts, principles, laws, etc.) are "incommensurable and therefore

incapable of mutual explanation and reduction" (p.75). The reason is that concepts of an earlier theory cannot be defined on the basis of primitive observational terms of the theory to which a reduction is attempted nor can there be found "correct empirical statements" to correlate corresponding terms and concepts (p.74). Feyerabend's criticism of the formal theories of reduction and explanation challenged a very central project of the logical empiricist philosophy of science that was dominant at the time in the United States.

Feyerabend had studied Wittgenstein's philosophy. He had written a critical review of *Philosophical Investigations* (Feyerabend, 1955), which was translated from German into English by G.E.M. Anscombe. Feyerabend was planning to study philosophy with Wittgenstein at Cambridge but his plans were thwarted by Wittgenstein's death in 1951. He went instead to study with Karl Popper in London. Feyerabend had met Wittgenstein in Vienna, in the context of the "Kraft Circle," which was named after Feyerabend's dissertation supervisor Viktor Kraft, a member of the original "Vienna Circle." Wittgenstein was invited to give a lecture there in 1949 and Feyerabend, who describes the visit as "brief and quite interesting," notes:

> Wittgenstein was very impressive in his way of presenting concrete cases, such as amoebas under a microscope (I cannot now recall the reason this example was used), but when he left we still did not know whether or not there was an external world, or, if there was one, what the arguments were in favor of it. 'You philosophers' said one of the participating engineers in despair, 'are all alike. There you tell us that Wittgenstein turned philosophy upside down. He talks just as much as everyone in this profession and can't give a straightforward answer to a straightforward question.' (Feyerabend, 1966, p.4)

Feyerabend endorsed these complaints later when, in reviewing Hanson's book (1958), he said that his pleasure reading it was

> sometimes a little diluted by the fact that in true Wittgensteinian fashion, many important points were buried beneath examples or aphorisms, or made in a rather indirect and qualified way. (Feyerabend, 1960, p.252)

Notably, the example of amoebas under the microscope mentioned in the quote above is the first example used by Hanson (1958, p.4) to illustrate the theory-ladenness of observation. Two microbiologists observing an amoeba, depending on the theories they have, see either a one-celled or a non-celled animal.

Feyerabend's ambivalent attitude toward Wittgenstein's philosophy is also shown in the comments he sent Kuhn in a letter of 1961. On the one hand he urges Kuhn to study Wittgenstein's remarks on *seeing*, as being original in comparison to Hanson's, and on the other he writes:

> People ask me to talk about Wittgenstein, because he is an influential contemporary philosopher [...]. I refuse to do this. And if they ask 'shall we read the *Philosophical Investigations?*' I reply 'If you want to waste your time, yes'. (Hoyningen-Huene, 1995, p.384)

Early in his career, Feyerabend was certainly influenced by Wittgenstein. His views on meaning bear affinities to Wittgenstein's, since a term's meaning is tied to use and is not dependent upon its connection to pure observation statements. But Feyerabend seems to understand "use" theoretically and not to appreciate the practical dimension

of Wittgenstein's approach. In the same footnote where he mentions Wittgenstein in connection with his (Feyerabend's) contextual theory of meaning, he criticizes Wittgenstein for replacing a Platonism of concepts by a Platonism of games (1962, p.68, n.83). Platonism implies for Feyerabend invariance of meaning; and what he seems to be saying is that Wittgenstein makes meaning invariant within each language-game or theory. This implies that, unlike Wittgenstein, he understands language-games as closed systems consisting of statements that fix completely the meaning of terms (cf. Chapter 26, LANGUAGES, LANGUAGE-GAMES, AND FORMS OF LIFE).

7 Conclusion

Harold Bloom, in his *Anxiety of Influence*, writes that strong poets make poetic history "by misreading one another, so as to clear imaginative space for themselves" (1973/97, p.5). In the course of the twentieth century, Wittgenstein's work has influenced the development of philosophy of science as a discipline at decisive points: first in the beginning, when contemporary philosophy of science was founded, and then around mid-century, when it took a turn away (or even against) its previous practice. In both cases he was creatively read and misread. His philosophy (early and late) offered support for the aims and undertakings of the respective groups of philosophers, but it only functioned as a rung of a ladder. They used it and moved beyond it. In the case of logical positivism, it was relatively clear that Wittgenstein and the logical positivists were heading in different directions and had different priorities. They had a reverential attitude toward science; he was critical of the scientific world-conception. They had formed, originally at least, "a kind of International Liberation Front" (Toulmin, 1969, p.51). Wittgenstein was dismissive of their militant language and perspective. In the case of the historical philosophers of science, Wittgenstein's philosophy helped to liberate them from the cast of the tradition, but it wasn't really studied in depth and hence not really appreciated either by them or their critics. If the Wittgensteinian elements in their work are brought to the fore, the innovative character of this major turn will be better comprehended. Old problems, for instance that of incommensurability, will be cast in a new light and new issues will emerge, for instance issues pertaining to the understanding of science as practice.

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