

# **Meaning Change in the Context of Thomas S. Kuhn's Philosophy**

**Jouni-Matti Kuukkanen**

## **Declaration**

I hereby declare that

- (a) this thesis has been composed by me;
- (b) the work in the thesis is my own and;
- (c) the work in the thesis has not been submitted for any other degree or professional qualification.

Jouni-Matti Kuukkanen

# Contents

<b>Contents .....</b>	<b>iii</b>
<b>Acknowledgements.....</b>	<b>iv</b>
<b>Abstract.....</b>	<b>v</b>
<b>List of abbreviations .....</b>	<b>vii</b>
<b>1. Multiple aspects of meaning change.....</b>	<b>1</b>
<b>2. Meaning change and philosophy of science .....</b>	<b>13</b>
<b>3. Genesis and development of meaning theorising .....</b>	<b>43</b>
<b>4. Knowledge representation and similarity relations.....</b>	<b>78</b>
<b>5. Kuhn's conceptual anti-realism.....</b>	<b>116</b>
<b>6. Meaning change historically .....</b>	<b>158</b>
<b>7. Historical perspective.....</b>	<b>195</b>
<b>8. Afterwords .....</b>	<b>232</b>
<b>Bibliography .....</b>	<b>240</b>

## Acknowledgements

I would like to express my gratitude to my principal supervisors for their invaluable support: Alexander Bird helped to initiate this project and John Henry oversaw the completion of it. I also thank Peter Milne for his comments. Over the years, several scholarships and grants in various forms have economically enabled me to carry out this doctoral thesis. I should thank all the following institutions for their financial assistance: College of Humanities and Social Science at the University of Edinburgh, Arts and Humanities Research Board, Jenny and Antti Wihuri Foundation, and The Academy of Finland.

## Abstract

Thomas S. Kuhn claimed that the meanings of scientific terms change in theory changes or in scientific revolutions. In philosophy, meaning change has been taken as the source of a group of problems, such as untranslatability, incommensurability, and referential variance. For this reason, the majority of analytic philosophers have sought to deny that there can be meaning change by focusing on developing a theory of reference that would guarantee referential stability. A number of philosophers have also claimed that Kuhn's view can be explained by the fact that he accepted and further developed many central tenets of logical empiricism. I maintain that the genesis of Kuhn's meaning theorising lies in his historical approach and that his view of meaning change is justified. Later in his career he attempted to advance a theory of meaning and can be said to have had limited success in it. What is more, recent cognitive science has unexpectedly managed to shed light on Kuhn's insights on the organisation of information in the mind, concept learning, and concept definition. Furthermore, although Kuhn's critique of Putnam's causal theory of reference has often been dismissed as irrelevant, he has a serious point to address. Kuhn thought that the causal theory that works so well with proper names cannot work with scientific terms. He held that conceptual categories are formed by similarity and dissimilarity relations; therefore, several features and not only one single property are needed for determination of extension. In addition, the causal theory requires universal substances as points of reference of scientific terms. Kuhn was a conceptualist, who held that universals do not exist as mind-independent entities and that mind-dependent family resemblance concepts serve the role of universals. Further, at the beginning of his career, Kuhn was interested in the question of what concepts or ideas are and how they change in their historical context. Although he did not develop his theorising on this issue, I demonstrate that this is a genuine problem in the philosophy of history. Finally, Kuhn argued that scientists cannot have access to truth in history because we cannot transcend our historical niche, and as a consequence, the truth of a belief cannot be a reason for theory choice. Instead

of truth, we can rely on justification. I also discuss Kuhn's idea that problem-solving is the main aim of science and show that this view can be incorporated into coherentist epistemology.

## List of abbreviations

*ET*    *Essential Tension*

*RSS*    *The Road Since Structure*

*SSR*    *The Structure of Scientific Revolutions*

## 1. Multiple aspects of meaning change

This thesis is a result of two interests and objects of study. It can be placed between an analytical account of meaning change and an interpretative study of Thomas S. Kuhn's philosophy. The impetus for this was inspired several years ago by two rather different pieces of philosophical writing. The first is Donald Davidson's essay "On the Very Idea of a Conceptual Scheme", and the other is Kuhn's treatise *The Structure of Scientific Revolutions*. After reading these I had a pre-analytic intuition that meaning change is something that naturally occurs in history rather than a philosophical problem, and that, by changing our basic ontology, it alters the way we understand reality.

From a historical point of view, Kuhn's and Feyerabend's claim, for example, that the meaning of 'mass', 'time', and 'space' changed in the transition from Newtonian mechanics to Einsteinian sounds uncontroversial. If it is true that Newton understood mass, time and space as absolute, how could there not have been a change in meaning? My feeling was, to take another example, that if 'earth' in Ptolemy's system meant something unique and stable in the centre of the Universe, Copernicus must have changed its meaning, just as Kuhn asserts. However, I could see what Davidson was saying in the article, and that it was, in part, directed against Kuhn's and Feyerabend's ideas of meaning change, untranslatability, and incommensurability. His message is that the idea of untranslatable conceptual schemes or the idea that there is something that organises our experience making reality, or better, what counts as real, relative to a scheme implies a contradictory position philosophically: different (incommensurable) points of view make sense only if there is a common co-ordinate system on which to plot them, and this is an assumption that undermines the claim of dramatic incomparability. Nevertheless, I took his article as an illustration of how exciting a phenomenon meaning change can be, not only because the claim of meaning change appeared to accord so well with historical or cultural studies, but also because I presumed that we do not need to

Meaning Change in the Context of Thomas S. Kuhn's Philosophy

accept all the premises and commitments implicit in Davidson's argument. The thesis of meaning change seemed to show that our basic ontology changes from time to time altering the view of what and what kind of entities our world is inhabited by. It was this that primarily interested me. Once there was mass, time and place but later something else - mass\*, time\* and place\*. Or, once there were such things as phlogiston, cadaveric particles, and inertia, but in another time oxygen, bacteria, and momentum. This conclusion prompted an adjacent question that seemed to lead to an interesting investigation of the origins of our world view: What is the causal factor that brings about such changes? I took it for granted that there is such a 'meaning', upon which this kind of investigation can rely.

The doctoral thesis, therefore, was meant to be a study on meaning change carried out in the analytic framework. The primary questions of that putative study were intended to be as follows: What is that 'meaning' that changes? How do we recognise such changes? And what philosophical consequences would such a position entail? Once I realised that there is a large amount of writing on meaning change and incommensurability in philosophy, the seemingly natural conclusion was that studying the debate on meaning change would clarify the outlined position. That is what has been achieved, but with an unexpected outcome. After taking a closer look at the notions of meaning and meaning change, any illusion that such a view could be vindicated philosophically by the existing body of literature soon disappeared. Meaning change and its implications have come to signify a specific set of problems that ought to be overcome by an appropriate philosophical analysis, rather than a historically illuminating account of the emergence and transformation of our basic scientific ontologies.

The debate on meaning change in philosophy has been vivid and diverse over a period of several decades, but it has recently centred on a certain view that has achieved a high degree of acceptance among contemporary analytic philosophers. That discussion is studied in Chapter 2. The first element of this view is a historical interpretation of meaning change. It is suggested that the historical philosophers' thesis of meaning change, as advanced by Kuhn and Feyerabend, is a further development from some of the basic tenets of logical empiricism. This alleged link

Meaning Change in the Context of Thomas S. Kuhn's Philosophy becomes understandable if we remember that the dream of logical positivism, that meaning could be fixed permanently by observation, evaporated gradually. Logical empiricism, a successor of logical positivism, had firstly come to accept through Carnap that not all terms can be given an explicit definition by reducing them to the observational language. Carnap suggested that the job could be done by so called reduction sentences. However, soon after he came to a more radical conclusion, i.e. that theoretical language needs to be taken in addition to observational language as a fully legitimate scientific language. This admission brought the idea that the meaning of at least some expressions is determined by their place in theory. In other words, meaning is in some cases theory-laden. According to this interpretation, Kuhn, Feyerabend and others continued from this point sweeping away the foundation of the logical empiricists' double language model by questioning whether observation can ever be neutral, and also by arguing that it is, in fact, theory-laden. This makes the meaning of all expressions, whether tied directly to observation or not, dependent on theory.

The proposition that the meanings of terms change in the history of science has become a premise for many philosophically problematic conclusions. The best-known consequence is incommensurability, customarily interpreted as incomparability. The point is that if theories and their terms are about different things altogether, or if we cannot translate between hypotheses of rival theories, then theories cannot be compared in a commensurable way. The question that has been asked is: If there is not such a precise and a rational comparison, how can we maintain the view that science has progressed? What is more, it seems evident that if theories in the past did not deal with the same subject matter as contemporary ones, then progress as convergence to truth is merely a pipe dream. This type of worry has given an incentive for many to argue that meaning does not alter. Consequently, meaning change has been (re)interpreted by the tools of philosophy of language in order to allow that interpretation. First of all, let us separate sense from reference. A typical assumption in philosophy after Frege is that sense determines reference. If we, then, take sense as theory-laden, and therefore change-sensitive, we can see the route to change in reference as well. It seems that Kuhn and Feyerabend implicitly accepted a wide descriptive theory of reference, according to which a description

Meaning Change in the Context of Thomas S. Kuhn's Philosophy comprising a large number of propositions determines reference. It is only natural that a theory, or some part of it, which functions as a descriptive determinant of reference, changes, making a shift in reference also possible. Yet, it seems that there is a problem only if reference changes, because it alone can offer us a point of comparison. If we can find a theory that makes reference invariant, then that is all that is important philosophically because it allows a comparison of the truth-values of propositions. The 1970s child, the causal theory of reference, first developed by Putnam and Kripke, appeared to postulate just that. It is a theory that does not employ descriptions and that can, therefore, guarantee invariance of reference in theory changes.

The above view undeniably makes a plausible story, and it offered me a potential way to proceed further. After understanding that the causal theory is not free of problems either, but is in fact in need of modification, the doctoral thesis could have focused on searching for a suitable theory of reference. However, I soon also realised that the story is inadequate for several reasons. Let me start with the historical explanation offered.

The presumption implied in the above interpretation, namely that the meaning-change thesis is an account with a uniform historical genesis, forces us to look into the background of the philosophers that formulated the thesis. This is one of the major tasks of Chapter 3. The idea that Kuhn had adopted the philosophical framework of logical empiricism so that he was able to extend the tenets of it, struck me as being especially surprising once Kuhn's education, which involved no formal studies of philosophy, is taken into account. By comparing two of those who advanced the thesis of meaning change, namely Kuhn and Feyerabend, we can see that the meaning-change account has more than one origin. Feyerabend is actually closer to being a late empiricist than Kuhn. The former worked under and with philosophers who were very closely affiliated with logical positivism or empiricism. At the beginning of his career, he associated himself with that tradition while also attacking it. Kuhn's interest in such philosophical notions as meaning and meaning change emerges from his studies of the history of science. He knew logical empiricism only in its outline and criticised the implied view of history on the basis

Meaning Change in the Context of Thomas S. Kuhn's Philosophy of his historical research. Studies on the history of science seemed to suggest to Kuhn that meaning is some kind of holistic notion that changes in theory transitions. This origin could not be further from Carnap's detailed philosophical investigations of meaning. In other words, Kuhn's meaning-change thesis is a positive construction out of historical studies, while Carnap's view of meaning, which allows changes of meaning in some cases, is a concession in the face of insurmountable difficulties in regard to the Received View of logical positivism.

However, it is not only that the intellectual backgrounds of Kuhn and Feyerabend are different, but also that their attitudes to meaning and meaning change are divergent. After the initial formulation of the meaning-change idea, Kuhn saw these notions as central and also as worthy of further later development, while Feyerabend wished to discuss meaning only, as he said, "in the gossip columns" (Feyerabend 1981b, 113) and moved on to other issues. This makes it reasonable to exclude Feyerabend from consideration and to focus instead on what Kuhn had to say on meaning and meaning change. I show in Chapter 3 that Kuhn did attempt to specify his ideas on meaning and meaning change, and he can, moreover, be said to have had some success in this, although not in having managed to develop a sophisticated account. Kuhn's early intuition that meaning is holistic and psychological, or sociopsychological, is better expressed later in his applications of Wittgenstein's family resemblance notion. He rejects the classical theory of concepts that defines concepts by sufficient and necessary conditions. Kuhn tries to develop a theory of concept learning according to which concepts are learned by observing the similarities and dissimilarities of objects. This ostensive learning results in the family resemblance concepts that are defined by the net of overlapping and crisscrossing similarity relations. He further speculates on how information is stored in the mind, suggesting that there is a kind of holistic structure that not only stores information in the mind but also conceptualises the world and experience for us. This holistic structure is variously called at different times 'conceptual scheme', 'mental module', and 'lexicon'.

Kuhn's interest in the theory of concepts, the learning of concepts, and in the organisation of information in the mind is something that has long been largely

Meaning Change in the Context of Thomas S. Kuhn's Philosophy ignored in philosophical discussions. It is now the case that virtually all these issues have received a new momentum in applications from cognitive science. There are a number of scholars who have approached Kuhn's philosophy from a specific cognitive-historical orientation. Chapter 4 thoroughly explores the cognitive link and also takes into account the research of those few that have earlier discussed some of the above insights of Kuhn.

A question of special interest is whether cognitive science can explicate Kuhn's meaning change by framing it as a certain type of conceptual change. I show that Kuhn's insight that information is organised and stored in some kind of holistic structure is now seen to be receiving support from recent research in cognitive science. There is a large amount of empirical evidence that can be interpreted as corroboration of that idea; furthermore, there are a number of suggestions as to the specific shape of that structure. The earliest proposal was that of something called a semantic network; later, the notion of a frame that stores information in clusters of feature associations has become more popular. Despite the fact that Kuhn's theory of concept learning does not appear to receive similar empirical support, it is of interest on its own, even with its limited applicability to concepts that deal with observable objects. Finally, it is true that even though Kuhn advocated the family resemblance view of concept and sought to apply it in the philosophy and history of science, he never advanced an explicit theory of concept. Nevertheless, scholars with a historical-cognitive orientation believe that research in cognitive psychology has given an empirical vindication to Wittgenstein's family resemblance concept. However, I think this is far too strong a claim. The past failure of the classic account of concept does not make it impossible to find a definition by sufficient and necessary conditions in the future. I maintain that what is fundamentally at stake is how extension is determined, i.e. whether instances that fall under a concept are determined by their mutual similarity to each other and dissimilarity to instances that fall under another concept, or whether they are determined by a set of necessarily shared features. For this reason, I treat Kuhn's suggestion of the family resemblance account, which is also closely linked to his idea of concept learning, as a proposal on how extension is determined. On the whole, I argue in this chapter that Kuhn's insights can be given a more precise, although extended and applied, interpretation

Meaning Change in the Context of Thomas S. Kuhn's Philosophy by recent studies in cognitive science. Perhaps surprisingly, this makes Kuhn look as if he was ahead of his time.

Kuhn's theorising on meaning and meaning change may thus be said to be more nuanced and multi-faceted than has been perceived over the last few decades in philosophy. It is not necessarily only a source of problems, but it is also something that is potentially constructive. Nonetheless, it is true that the causal theory of reference has brought an interesting new theory to philosophy. One of the suggestions in regard to the causal account is that it solves the problems that are associated with meaning change. What is crucial is that it is seen to enable commensurability in science. This prompts us to ask the question whether such a suggested solution to Kuhn's meaning change is viable. The question is posed and studied in Chapter 5.

The setting for this is generally fruitful because Kuhn explicitly drafted his response to Putnam's causal theory. Except in respect of proper names, Kuhn flatly rejects the causal theory in the context of the history of science. This rejection is grounded on two arguments. Firstly, Kuhn argues that normally it is impossible to fix the reference of a term by one single property alone. Rather, we need several features to make a difference to the contrasting categories of a term. Furthermore, it is not possible to make an undisputable distinction between essential and accidental properties. Kuhn suggests that reference is fixed by crisscrossing and overlapping similarities and dissimilarities between objects without a distinction between essential and superficial properties. Secondly, I show that the Putnam-Kuhn argument hides a genuine metaphysical difference. The causal theory of reference requires a single abstract entity or a universal, usually understood as an essence, where a term refers to, and which stays invariant in theory transitions. While a single reference point may be established with proper names, it is a lot more problematic with common nouns that are, however, most important in science. I argue that Kuhn is an anti-conceptual realist who does not have an account of universals. More precisely, he is the kind of conceptualist according to which individuals are real entities in the external world, but universals are real only as concepts in mind. Their function is to categorise objects. Moreover, my view is that when dealing with theory

Meaning Change in the Context of Thomas S. Kuhn's Philosophy changes Kuhn is talking about extension, i.e. a set of individual things or particulars, and not of reference, i.e. abstract entities or universal substances.

The causal theory cannot work if there are no universals that would give us a single common point of comparison. For this reason we are left in Kuhn's philosophy with changing sets of individuals that are determined by similarity and dissimilarity relations. That theory has been accused of blurring the line between what falls and what does not fall into a conceptual category, extending conceptual categories without limits. The problem is the so-called problem of wide-open texture. However, this criticism can be countered by understanding that the function of dissimilarity is that it excludes one instance of a category from qualifying as an instance of another.

All the chapters above deal with meaning change in the context of philosophy or cognitive science philosophically interpreted. It is possible to allege that historically, meaning change is yet something else, which might explain the initial persuasiveness of the meaning-change view in *The Structure of Scientific Revolutions*. From a historical perspective, the primary question is not where and how our terms refer, but the main interest is in the thinking and the products of thinking of past scientists. The crucial question is naturally how this intensional meaning, concept, or "thought product" should be understood in the historical context.

I show firstly in Chapter 6 that Kuhn, in his earliest historical writings, was indeed preoccupied by questions that are typical in the history of ideas and intellectual history: What are ideas or concepts? How do they come about? And how do they change or transform? Further, I examine two theoretical traditions that have given an answer to the above questions. Here mere terminological differences should not confuse us. It does not matter whether we are talking of concepts or ideas as long as the concern for the basic intellectual units in the history of thought is the same. Moreover, a concept or an idea can be taken as a meaning that a term denotes and as the notion that Kuhn is talking about in his historical texts.

The first tradition studied is Arthur Lovejoy's project on the history of ideas, with the unit idea as a central notion. This has generally been seen as a failure among

Meaning Change in the Context of Thomas S. Kuhn's Philosophy historians, but I argue that the traditional method of writing the histories of concepts and ideas actually presupposes something like a unit idea. That is, if one writes a history of a concept, such as the 'atom', all instantiations need to have something in common. And yet, it is true that reliance on a totally unchangeable unit idea overemphasizes continuity and conceals interesting points of discontinuities in the thinking of past scientists. The second tradition studied here, the cognitive history and philosophy of science, makes the family resemblance concept the basic theoretical tool. By that notion it is able to take discontinuities into account. The suggestion of the cognitive history and philosophy of science is more precisely that concepts are stereotypes or prototypes: Concept is taken as a summary of features of instances that fall under a concept and features are understood as having weights or emphases reflecting how likely it is that a particular object has this or that property. Unfortunately, due to the commitment to family resemblance, this proposal allows a situation where two instantiations of the same concept do not share any common feature. Consequently, any history written that relies on the family resemblance notion produces a narrative that postulates continuity merely via terms or via the minds of individual scientists. For this reason, it is questionable whether it results in the history of concepts or intensional meanings. Furthermore, the notion of stereotype is interesting, but the problem with this specific account is that it assumes that concept is formed by observing real existing objects. And yet it is clear that I can possess a concept without having constructed it in such a way. Neither is it a requirement of having concepts that a concept should correspond to any real entity or property in the world.

My solution is to accept the insight of Lovejoy that the logic of history writing requires that all exemplifications of the same historical concept share something. Yet I also accept Nersessian's idea that discontinuities and their representation matter. Furthermore, with regard to the cognitive history and philosophy of science, I take historical concepts as psychological or sociopsychological entities that are subject to natural explanation, this being unlike Lovejoy's history of ideas. For these reasons, I suggest that a concept or a meaning historically can be understood as something that has a minimal, "necessary", shared component, but it does not have an unambiguous or full definition beyond that. In

other words, a history of a concept implies that sub-concepts of the covering historical concept have a common core, which guarantees their conceptual membership, but it also allows that all other, out-of-core, elements can change. The full explicit definition is unreachable, because no illuminating definition of the concept can offer an exhaustive account of features that vary in the course of its (or its exemplifications') history. This conclusion stems from the belief that, historically, it makes the best sense. In light of this, I give an example of meaning change as an 'element' in 18<sup>th</sup> century chemistry. Further, I (re)interpret the notion of incommensurability. Incommensurability may be taken as the practical difficulty of achieving translation and reaching comprehension in a situation where the same expressions imply radically different assumptions.

If, then, the causal theory of reference cannot be applied in the history of science, it naturally raises the question whether we are forced to accept incomparability as an inconvenient fact of history. The question is an important one, as it has been a key concern in philosophy for four decades. In the penultimate chapter, I argue that incomparability is not a necessary consequence of Kuhn's position. First of all, it is an exaggeration to say that Kuhn would have definitely propagated such a view. On several occasions, he writes that there are some values that are used in intertheoretic comparisons. What is really at stake is the comparison of truth-values. Kuhn contends that that cannot be done if being true means correspondence to absolute mind-independent reality, and therefore, the correspondence theory of truth does not have a use in historical explanations focused on theory choice. Scientists simply do not have access to truth, which means that they cannot tell truth from falsity in the situation of theory choice. Kuhn appeals to the pessimistic meta-induction to prove the point: all that has been claimed to be true previously has turned out to be false; for this reason, there is good reason to suppose that all that we believe now to be true is not true. Kuhn puts forward his historical or developmental perspective according to which we are tied to our historical situation without an Archimedean neutral platform. All that we can do is to accept the existing body of beliefs and to revise it from that point. While there is no access to truth, as God would have, we can ask for the justification of our beliefs. However, Kuhn

Meaning Change in the Context of Thomas S. Kuhn's Philosophy argues that we ought not to require justification of single beliefs *per se* but we can ask for justification of changes of beliefs in the whole web of beliefs.

Kuhn is epistemologically conservative, which is a position that can be supported not only by historical studies but also by evidence from cognitive science. This evidence suggests that drastic changes in our web of beliefs are impossible without there being an alternative available and that they are difficult to achieve in any case. I further argue that Kuhn's philosophy of science and his epistemology can be incorporated into the coherentist epistemology. Firstly, conservatism is a natural consequence of the coherentist theory of justification; hence, epistemological conservatism receives an explanation if we assume that Kuhn is a coherentist. More importantly, Kuhn characterised science as problem-solving, and problem-solving is something that can be linked unproblematically to the concept of coherence. Admittedly, however, not all Kuhn's intertheoretical values of comparison contribute directly to coherence, but also they can be linked to it via their role in problem-solving. In this view, science is an attempt to increase the coherence of theories. In addition, coherence offers us a common standard to be used in a theory comparison: one should accept a more coherent theory if there is one available for the same task. While it may be argued that convergence to truth underlies and explains increasing coherence, I point out that the concept of verisimilitude is still in need of conceptual clarification, and in addition to that, convergence should be corroborated historically. Kuhn insists that there is no ontological convergence, although the case remains empirically undecided. At the very end, I draft briefly a pragmatic-historical view on the basis of Kuhn's philosophy. This sees us as natural beings in touch with nature, but also situates us historically into a certain niche, with the consequence that theory construction inherently involves a perspective.

This thesis has turned out to be a study of multiple aspects of meaning change in or in relation to Kuhn's philosophy of science. Meaning change has multiple aspects because it has been and/or can be seen in a philosophical, cognitive or purely historical framework. My overall view is that the customary philosophical interpretation is exaggerated and one-sided. The claimed consequences are not quite those that have been assumed. Further, Kuhn's philosophy offers an answer to many

Meaning Change in the Context of Thomas S. Kuhn's Philosophy

worries from his specific epistemological anti-realist perspective. Kuhn's anti-realism is, of course, rejected by scientific realists, but that does not make his philosophy unreasonable in any obvious way. The cognitive perspective, in turn, is a refreshing point of view in that it takes into account Kuhn's later views and is able to occasionally show real points of contact with recent cognitive science. Finally, historical meaning change is only implied in Kuhn's work. Although it is something that is historically intuitive and may be implied in many studies on history, it is also something that was left undeveloped in Kuhn's philosophy and has remained such until this day.

## 2. Meaning change and philosophy of science

The idea that the meanings of scientific terms change in theory changes, or at least in scientific revolutions, was introduced to the philosophy of science essentially by two historically-oriented philosophers of science: Thomas S. Kuhn and Paul K. Feyerabend. They were united in their belief, as were the other historical philosophers of their time, that the philosophy of science should base its argumentation on the realistic understanding of past scientific practice. Accordingly, Kuhn and Feyerabend mostly argued for meaning change by simply introducing case studies or examples that were meant to demonstrate the phenomenon. The initial plausibility of Kuhn's and Feyerabend's claim derives also from the historical grounding of their argumentation. However, the assertion that the meanings of scientific terms change was not received favourably in the philosophy of science. In this chapter I will examine how discussion on meaning change has evolved since the 1960s when the meaning-change thesis was first introduced.

Firstly, I will look at some of the claims on meaning change made by Kuhn and Feyerabend. In this chapter these claims function as an introduction to the debate that they initiated in philosophy. The next chapter is devoted to a study of their message and underlying assumptions. Secondly, I outline how the problem of meaning change was typically interpreted. As this already implies, Kuhn's and Feyerabend's idea of meaning change was predominantly seen as a problem. Thirdly, I show into what kind of theoretical background the problem of meaning change was placed. In the last decade several philosophers have quite unexpectedly found an immediate intellectual predecessor of the historical philosophers in logical positivism and empiricism. Finally, I focus on the view that finds a solution on the basis of the analysis of the alleged theoretical background of meaning change. According to this view, a solution to the problem of meaning change can be found from the contemporary applications of the philosophy of language. More precisely, theories of

Meaning Change in the Context of Thomas S. Kuhn's Philosophy  
reference are thought to pave the way for overcoming the set of problems brought by  
Kuhn's and Feyerabend's meaning change.

### ***Kuhn's and Feyerabend's meaning change***

A famous example of Kuhn's meaning change is the meaning change of central terms in the transition from Newtonian to Einsteinian mechanics. Kuhn considers whether we could derive Newtonian dynamics from relativistic dynamics. Some have suggested that this is possible if we restrict the range of the parameters and the variables of the Einsteinian statements. By limiting the conditions of the application of the derived laws we would be able to deduce a set of N-laws from Einstein's mechanics that are identical to the Newtonian laws. However, what is carried out in such a derivation appears to be a trick. The derivation does not give Newton's laws, unless we can admit an interpretation impossible before Einstein. Kuhn writes:

The variables and parameters that in the Einsteinian  $E_i$ 's represented spatial position, time, mass, etc. still occur in the  $N_i$ 's and they still represent Einsteinian space, time and, mass. ... Unless we change the definition of the variables of the  $N_i$ 's, the statements we have derived are not Newtonian. If we do change them, we cannot properly be said to have *derived* Newton's laws. (*SSR*, 101-2)

Kuhn adds also that "(T)his need to change the meaning of established and familiar concepts is central to the revolutionary impact of Einstein's theory." (*SSR*, 102) We find passages that imply a similar view throughout *The Structure of Scientific Revolutions*. Kuhn tells us in a straightforward way what 'space' meant in Newtonian mechanics, and how it changed from that:

The laymen who scoffed at Einstein's general theory of relativity because space could not be "curved" – it was not that sort of thing – were not simply wrong or mistaken. Nor were the mathematicians, physicists, and philosophers who tried to develop a Euclidean version

## Meaning Change in the Context of Thomas S. Kuhn's Philosophy

of Einstein's theory. What had previously been meant by space was necessarily flat, homogenous, isotropic, and unaffected by the presence of matter. (*SSR*, 149)

Furthermore, Kuhn's thesis is not limited to the Einsteinian revolution. His favourite historical example deals with another famous revolution - the Copernican revolution in astronomy.

Consider, for another example, the men who called Copernicus mad because he proclaimed that the earth moved. They were not either just wrong or quite wrong. Part of what they meant by 'earth' was fixed position. Their earth, at least, could not be moved. (*SSR*, 149)

And the meaning change in the Copernican revolution does not only concern 'planet', but also 'sun':

The Copernicans who denied its traditional title 'planet' to the sun were not only learning what 'planet' meant or what the sun was. Instead, they were changing the meaning of 'planet' so that it could continue to make useful distinctions in a world where all celestial bodies, not just the sun, were seen differently from the way they had been seen before. The same point could be made about any of our earlier examples. (*SSR*, 129-30)

It is important to note the last sentence in the above quotation. Kuhn clearly extends meaning change to cover a large number of cases. The point that meaning change is not confined to a couple of the most famous revolutions in the history of science is also made clear in the following passage:

Though subtler than the changes from geocentrism to heliocentrism, from phlogiston to oxygen, or from corpuscles to waves, the resulting conceptual transformation [in the Einsteinian revolution] is no less decisively destructive of a previously established paradigm. (*SSR*, 102)

Feyerabend writes of meaning change in synchrony with Kuhn. The difference is, though, that he explicitly calls into question "the principle of meaning invariance" in his 1960s articles. The principle says that the meaning of scientific terms stays invariant in theory changes. He says that this idea played a decisive role in Platonism and in the Cartesian physics and mechanics. (Feyerabend 1981a, 46) In these traditions, key terms of physics, such as 'matter', 'space' and 'motion', and in metaphysics, those of 'god' and 'mind', are supposed to stay invariant in any explanations involving them. According to Feyerabend, this is inconsistent with empiricism and historically inaccurate.

Feyerabend claims that in the classical, pre-relativistic physics, the concept of mass (of length and of duration) was absolute. The mass of a system was not influenced by its motion in the chosen co-ordinate system. By contrast, Feyerabend writes that in the relativistic physics mass is a relational property, and one has to take into account the co-ordinate system in which spatiotemporal descriptions are conducted. In brief, Feyerabend tells us that in the pre-relativistic physics we are measuring an intrinsic property of the system under consideration, whereas in the relativistic physics we are measuring a relation between the system and certain characteristics of a domain. Like Kuhn, he argues that for this reason no reduction or derivation relation between these two systems of physics is possible. (Feyerabend 1981a, 81-2)

Another example by Feyerabend deals with the relation between the medieval impetus theory and Newton's physics. The question is whether the momentum of a moving object in the Newtonian theory is an analogue of the impetus in the medieval theory of motion. If this were so, it would be possible to reduce the impetus theory to Newtonian mechanics. He implies that logical empiricists have assumed something like this, most notably Ernst Nagel in his *Structure of Science* (1961). However, Feyerabend flatly rejects this possibility. 'Impetus' and 'momentum' mean something different. 'Impetus' is a kind of force that pushes a body along, like a cart drawn by a horse, while 'momentum' is the result rather than the cause of the motion. Secondly, the inertial motion of classical mechanics occurs by itself without any causes, unlike its Aristotelian and medieval counterparts. (Feyerabend 1981a,

65) His conclusion is that the meaning of 'force' has not stayed invariant in the Newtonian scientific transformation.

Feyerabend takes preliminary steps towards a more formal explication of his conception of meaning change. He writes:

What happens here when a transition is made from a theory T' to a wider theory T (which, we shall assume, is capable of covering all the phenomena that have been covered by T') is something much more radical than incorporation of the *unchanged* theory T' (unchanged, that is, with respect to the meanings of its main descriptive terms as well as to the meanings of the terms of its observation language) into the context of T. What does happen is, rather, a *replacement* of the ontology (and perhaps even of the formalism) of T' by the ontology (and the formalism) of T, and a corresponding change of the meaning of the descriptive elements of the formalism of T' (provided these elements and this formalism are still used). This replacement affects not only the theoretical terms of T' but also at least some of the observational terms which occurred in its test statements. That is, not only will description of things and processes in the domain in which T' had been applied be infiltrated, either with the formalism and the terms of T, or if the terms of T' are still in use, with the meanings of the terms of T, but the sentences expressing what is accessible to direct observation inside this domain will now mean something different. In short, introducing a new theory involves changes of outlook both with respect to the observable and with respect to the unobservable feature of the world, and corresponding changes in the meanings of even the most 'fundamental' terms of the language employed. (Feyerabend 1981a, 44-5)

Feyerabend's view appears to be that in theory transitions basically all terms are affected by changes of their meanings. There is no difference in this respect between theoretical and observational terms. Moreover, meaning change counts as a replacement of ontology.

I have outlined above what Kuhn and Feyerabend initially claimed on changes of meaning. It is clear that they rely crucially on the historical argumentation and on some specific examples from the history of science. The assumptions that underlie their view or the implicit theory of meaning are not explicit at this point. Next, I will discuss how the idea of meaning change was received in the philosophy

Meaning Change in the Context of Thomas S. Kuhn's Philosophy of science. Since the first argument for meaning change in the early 1960s there have appeared a large number of papers written directly or indirectly on it. Although meaning change can be seen as a problem as such, commonly the idea of the meaning change of scientific terms is thought to be part of the argument for incommensurability between scientific theories. I do not intend to cover all the articles on meaning change or incommensurability as this would require separate research<sup>1</sup>, but instead I will give a representative description of the discussion on meaning change and incommensurability in the philosophy of science.

### **Early responses**

Kuhn's and Feyerabend's views came under intense criticism soon after their first formulation<sup>2</sup>. Frederick Suppe's introductory chapter "The Search for Philosophic Understanding of Scientific Theories" in *The Structure of Scientific Theories* serves well as a description of the early responses to meaning change<sup>3</sup>. The meaning-change thesis was really born only a few years before by Kuhn, Feyerabend, and a few other historical philosophers of science or scientists, such as David Bohm, Stephen

---

<sup>1</sup>The Centre for Philosophy and Ethics of Science at the University of Hanover has a comprehensive bibliography on incommensurability that is also continuously updated. The bibliography can be found on-line at <http://www.unics.uni-hannover.de/zeww/inc.conf.litlist.eng.html>. For references of the discussion on incommensurability in relation to Kuhn, see Hoyningen-Huene (1993, 207).

<sup>2</sup> Probably the first profound evaluation of Kuhn's views is in the 1965 symposium, and later in a corresponding publication, *Criticism and the Growth of Knowledge* (Lakatos and Musgrave, 1970). Shapere's early papers (1964, 1966) are also worth mentioning because, in addition to more typical criticism, Shapere brings forward more untypical polemic. That is, Kuhn's and Feyerabend's views do not, according to Shapere, arise from the actual history of science after all, and the implied conception of meaning is neither clear nor (any reference to the notion of meaning) helpful for understanding the history of science.

<sup>3</sup> The chapter is included in the volume that was born out of a symposium on the structure of scientific theories held in Urbana 1969. The symposium was supposed to gather together philosophers, historians of science and scientists to evaluate new alternative analyses to logical empiricism in regard to the structure of scientific theories. It drew audiences as large as 1200, and its participants' views were, according to Suppe, "fairly representative of the current spectrum of philosophical thinking about the nature of scientific theories" and shows, therefore, "a revealing picture of what philosophers currently think about the nature of theories and their roles in the scientific enterprise". Because the symposium collected audiences from different backgrounds, it was decided to include an introductory chapter to the philosophical literature on scientific theories of the century. Suppe says that the result, his chapter, is "as comprehensive a critical survey of the philosophical literature on theories as has been published" (Suppe 1977, vii).

Meaning Change in the Context of Thomas S. Kuhn's Philosophy  
Toulmin and Norwood Russell Hanson. Suppe's analysis lays the framework for the future discussion on meaning change since in his criticism we find many of the seeds of later critique on meaning change.

Suppe formulates first weak and strong theses of meaning change and attributes one or the other to such philosophers as Feyerabend, Kuhn, Bohm, Toulmin, and Hanson. After that, he introduces five objections to these theses. The objections apply to both versions, and therefore, for our purpose, it is enough to take into account only one formulation of the meaning-change thesis. The strong meaning-change thesis says:

All of the principles of the theory contribute to the meanings of the terms occurring in them; hence any change in theory alters the meanings of all the terms in the theory. (Suppe 1977, 199)

Suppe argues that the first problem with the thesis is that the advocates of meaning change do not specify what it is to be a change in theory and what it is to be a change in meaning. Suppe asks whether a redetermination of the value of a physical constant counts as a change, both in theory and in the meanings of the terms in that theory. He points out that it is not clear whether there is any difference in change in belief, in theory, and in meaning. According to Suppe, Feyerabend thinks that any sufficiently general point of view concerning matter of fact is a theory. Suppe wonders if any change of belief is, therefore, a change in theory or in meaning, or both. (Suppe 1977, 200)

The second problem is that two theories could never contradict each other if the meaning-change thesis is correct. This follows from the idea, implied in the thesis, that the meaning of a term is determined by the theory it occurs in. An example of this kind of theory-dependence of meaning, given by Suppe, is how in Bohr's theory of the atom, angular momentum and radiant energy cannot have continuous values, which they, in contrast, do have under classical thermodynamics. Any supposed contradiction is deceiving because the meanings of the terms are different. Even an expression of disagreement with the terms of the theories is

Meaning Change in the Context of Thomas S. Kuhn's Philosophy impossible because the terms have their meaning in virtue of the theories, and the theories assign different meanings to the same terms or words. The third problem is merely an addition to what has just been said. There can be neither agreement between theories for the same reason that disagreement is impossible. Again, this is a consequence of the premise that the meaning of all terms is theory-laden. There is no neutral observation language in which to express either agreement or disagreement, and as a result, theories talk beside each other. Suppe asks whether there is any sense in which theories can be seen as alternatives. (Suppe 1977, 201)

The fourth problem that follows from the meaning-change thesis is that it threatens to make science a non-empirical discipline. This is the case if every principle of the theory contributes to the meaning of the terms. The rejection of any principle results in a change in meaning. These principles seem to be, therefore, analytic because, according to Putnam's idea of analyticity in the same volume, which Suppe cites, the mark of a statement being analytic is that its denial requires alteration in the meaning of terms. The fifth and final problem is that the testing of theories becomes circular, and no observation report can disconfirm or falsify a theory. Suppe asks us to consider a case where a prediction P of a theory disagrees with the result of an observation, and the observation report therefore entails not-P. But if the prediction P is part of the original theory, its denial alters the theory and changes the meaning of the descriptive terms in P. The descriptive terms in the statements P and not-P cannot have, therefore, the same meaning and the seeming contradiction of not-P with P is just that, only an apparent contradiction, and thereby, does not count as a disconfirmation of the theory. Only those observation reports are relevant to testing that are consistent with the theory. This is what makes testing circular. (Suppe 1977, 201-2)

To recapitulate, the above objections can be expressed as a set of four problems: 1) A specification problem (first), 2) no agreement/disagreement problem (second and third), 3) an analyticity of theories problem (fourth), and 4) a circular testing problem (fifth). Since meaning change has often been taken merely as a problem that needs to be overcome, there have not been too many attempts to find a specification. This thesis is in part an attempt to correct that situation and try to find

Meaning Change in the Context of Thomas S. Kuhn's Philosophy

some formulations of meaning change. Furthermore, to say that scientific theories are immune to empirical refutation or corroboration strikes me as an absurd statement, because empirical evidence certainly matters in science in practice. However, more interestingly, three latter problems can be subsumed under another more general problem, thereby allowing discussion on a more general level. If theories cannot agree or disagree, are analytic, and subject to only circular testing, then they do not appear to be comparable in order to allow a rational theory choice between them. What is more, it is also questionable whether they are commensurable. The problem is, in other words, the same infamous problem of incommensurability that has caught the attention of numerous philosophers over a period of several decades. This notion is next discussed under the heading of the incommensurability thesis.

### ***The incommensurability thesis***

After intense early discussion on their proposals, it appears that there was a collapse of interest among philosophers in the mid 1970s in taking Kuhn's and Feyerabend's philosophy seriously. One can even sense the feeling of irritation and frustration towards their responses to the early criticism.<sup>4</sup> Yet it would be wrong to say that there has been a lack of interest at any point in what Kuhn and Feyerabend claimed. One can justifiably declare, to use John Preston's expression, that there "has been spilt a tremendous amount of ink" over incommensurability since 1962 when it was first introduced (Preston 1997, 102). And yet, despite this, it is difficult to spell out what incommensurability means because the specification of it varies among philosophers. Normally all discussants have separated at least two distinct types of

---

<sup>4</sup> Suppe's "Afterword" in *The Structure of Scientific Theories* conveys vividly the Zeitgeist in philosophy. According to Suppe, Feyerabend "generally has ignored and refused to take seriously criticism, preferring instead to develop increasingly extreme versions of his general view". Kuhn has, Suppe writes, taken critique seriously and tried to modify his view, but this has meant a loss in originality. Furthermore, he says, "their reactions to the critical attacks levelled against their earlier work have played a significant part in the declining influence of their work during the 1970s". As a result, an increasing numbers of philosophers of science have rejected Kuhn's approach "as irredeemably flawed, although not as hopeless as Feyerabend's". (Suppe 1977, 636-49)

Meaning Change in the Context of Thomas S. Kuhn's Philosophy  
incommensurability.<sup>5</sup> Any definition of incommensurability that I know recognises  
meaning or semantic incommensurability. Incommensurability in this sense is  
important to the current study because meaning change has become closely  
associated with semantic incommensurability.

Semantic incommensurability stems from the idea that is currently under  
investigation in this thesis, i.e. from meaning change. The common feature of all  
formulations of semantic incommensurability is the conception that meaning varies  
or is different between theories. The consequence, as Suppe has already noted, is that  
the terms in the statements deduced from theories that are assumed to be in  
competition do, in fact, mean something different. Because theories discuss different  
issues, they are not rivals. What is more, it is hard to see how they could be  
compared in that situation.

One of the clearest formulations of semantic incommensurability is described  
by Howard Sankey. Sankey calls semantic incommensurability the  
incommensurability thesis, which he characterises with three semantic relations  
between alternative theories: meaning variance, translation failure, and content  
incomparability. According to Sankey, two alternative scientific theories are  
incommensurable if, and only if:

- (i) the meaning of the vocabulary by theories varies between theories
- (ii) translation is impossible from the vocabulary of one theory into the  
vocabulary of the other

---

<sup>5</sup> Here are some examples of how incommensurability has been categorised. Ian Hacking in  
*Representing and Intervening* (1983) finds three kinds of incommensurability: topic-  
incommensurability, dissociation and meaning-incommensurability. W.H. Newton-Smith in *The  
Rationality of Science* (1981) similarly separates three different kinds of incommensurability that are  
due to value variance, radical standard variance and radical meaning variance. Alexander Bird says  
that there are two distinct but related sources of incommensurability. One stems from the nature of  
paradigms as benchmarks for the evaluation of theories, and the other from the role played by  
paradigms in establishing meaning (Bird 2000, 150-1). Preston notes that Kuhn's and Feyerabend's  
notions of incommensurability are different, which Feyerabend also noticed (Preston 1997, 103). Even  
if this is so, one type of Kuhn's incommensurability, semantic, appears to be Feyerabend's  
incommensurability.

- (iii) as a result of (i) and (ii), the content of such theories may not be compared. (1997<sup>6</sup>, 4-5; 2000, 127)

One difference to what Suppe had said earlier is that Sankey makes the premise (ii) explicit, and there are further novelties to be found in Sankey's analysis of incommensurability. The most important contribution is analysing also the notion of meaning itself more explicitly. It has become increasingly popular to interpret meaning change via the Fregean distinction between sense and reference. With the exception of Israel Scheffler (1967) and Michael Martin (1971, 1972), the early responses did not attempt to solve the meaning-change problems with the help of Frege's distinction. Now, this new focus does not only seem to give a more nuanced analysis of meaning change, but it also appears to indicate a fresh historical interpretation of meaning change, and best of all, a solution to the problems that it brought.

The question is, first of all, what is that meaning that changes in meaning change: sense or reference, or both? Sense is often called intension, or just "meaning", in contrast to reference. In Frege's theory the sense of a term is "the mode of presentation", or how the reference is presented. Sense determines reference and can change without reference changing, but not the other way round. Frege's famous example is of how the two expressions 'Morning star' and 'Evening star' differ in their sense but not in their reference, i.e. they both refer to the planet Venus. In other words, the question now is: How extensive is Kuhn's and Feyerabend's meaning change?

To begin with, meaning change can be interpreted as a change in sense. It sounds natural to say that, if there is meaning change in the first place, it is primarily a change in sense. For instance, 'Newtonian mass' and 'Einsteinian mass' differ in

---

<sup>6</sup> Page numbers refer to Sankey's article "Incommensurability: the Current State of Play", that can be found on his home page at [http://www.hps.unimelb.edu.au/bio\\_hsankey.html](http://www.hps.unimelb.edu.au/bio_hsankey.html). The paper is also published in *Theoria* (1997), which is listed in the bibliography of this thesis. But note that there are several journals called *Theoria* that appear to address more or less related themes. This is not the Swedish Journal of Philosophy, but apparently the one published by the Spanish Society of Logic,

Meaning Change in the Context of Thomas S. Kuhn's Philosophy sense as they connote different things. Perhaps one is absolute and the other relative, and so on. But is there also a referential change? It seems that the answer is in the affirmative, if we are to believe Kuhn and Feyerabend. Kuhn writes:

But the physical referents of these Einsteinian concepts are by no means identical with those of the Newtonian concepts that bear the same name. (Newtonian mass is conserved: Einsteinian is convertible with energy. Only at low relative velocities may the two be measured in the same way, and even then they must not be conceived to be the same). (*SSR*, 102)

Compare this to Feyerabend's characterisation of change in meaning in his article "On the 'Meaning' of Scientific Terms", which is an answer to criticism of his meaning theory:

We shall diagnose a change of meaning either if a new theory entails that all concepts of the preceding theory have zero extension or if it introduces rules which cannot be interpreted as attributing specific properties to objects within already existing classes, but which change the system of classes itself. (Feyerabend 1981b, 98)

Kuhn writes about the difference in "physical referents" between Einsteinian and Newtonian concepts, and Feyerabend about "zero extension" of the old theory in comparison to the new. It seems evident, therefore, that Kuhn and Feyerabend also allow referential change.

Sankey and others point out that Kuhn's and Feyerabend's view on meaning change as both change in sense and reference, becomes understandable if we suppose that it implies a descriptive theory of reference.<sup>7</sup> The first step in this explanation is that there is a holistic theory of meaning, or the principle of the theory-ladenness of

---

Methodology and Philosophy of Science (SLMFCE) and the Spanish Society of Analytical Philosophy (SEFA).

<sup>7</sup> Interpretation of the Kuhn-Feyerabend meaning change in this fashion can be found in the following publications: Sankey (1994, 1997, 2000), Hacking (1983, ch. 6), Newton-Smith (1981, ch. 7), Bird (2000 ch. 5; 2002, 2004a), and Nersessian (1984, ch. 2).

Meaning Change in the Context of Thomas S. Kuhn's Philosophy meaning, presupposed in Kuhn's and Feyerabend's meaning change. That is, the meaning of a term is determined by its place in some kind of system or structure, such as a theory, a conceptual scheme, or just a "context". Feyerabend does explicitly recognise this, and calls his theory of meaning a "contextual theory of meaning", which he attributes to Wittgenstein (1981a, 74; see also 1965, 180). Kuhn does not bring forward his own theory although he writes often about meaning. Yet, it is apparent that his meaning implies holism in some form. The suggested explanation as to why there is a change in sense is that meaning is theory-dependent, and as a consequence of this, an alteration in theory changes sense automatically. In the most radical form of holism, any change, no matter how small, is enough to alter meaning. Remember here also Suppe's idea that even the denial of a statement counts both as theory change and meaning change. All in all, theory change is virtually equivalent to meaning change if the meaning-change view implies holism.

The second step in the explanation is to combine this very "unFregean idea" (see Hacking 1983, 76), i.e. meaning or sense is theory-laden, with two properly Fregean principles: sense determines reference and different sense may pick out different reference. If sense undergoes a change in theory transition, then reference may shift as well. Whether there is a shift in reference depends on whether sense<sub>1</sub> and sense<sub>2</sub> can refer to the same object. If there is a contradiction between the theories or their principles that define senses, then co-referentiality is not a possibility. For example, let us assume that the 'mass' term is defined by the theory in which it is incorporated. When a scientific community abandons Newton's theory and accepts Einstein's theory, the sense of 'mass' changes. If mass cannot be taken as being both absolute and relativistic, then the 'Newtonian mass' and the 'Einsteinian mass' cannot refer to the same entity. This would constitute a rational explanation as to why there is a referential shift in the theory transition.

We can now say that the first premise in Sankey's incommensurability thesis implies both alteration in sense and reference. More generally, an appeal to the principle of the theory-ladenness of meaning and the descriptive theory of reference offers us a theoretical explanation of the position adopted by Kuhn and Feyerabend. But what, then, is the historical context? Why did Kuhn and Feyerabend end up with

Meaning Change in the Context of Thomas S. Kuhn's Philosophy

such a view? In comparison to the early responses, recent interpretations yield something of a surprise. First assessments of the historical philosophers of science, and especially Kuhn and Feyerabend, saw their philosophy as the embodiment of discontinuity in relation to the previous dominant philosophical tradition, that of logical positivism/empiricism. This early view does indeed sound plausible if one thinks about some central elements in Kuhn's and Feyerabend's philosophy. They both championed the view that observation is theory-laden; the meaning of scientific terms vary; science does not develop steadily by reductions or incorporations, but rather by the abandonment of earlier theories; the history of science is not an accumulation of facts; and theories are incommensurable, making the idea of progress problematic. These theses undoubtedly contradict the philosophy of logical positivism. Moreover, the historical philosophers used extensively detailed studies of history, in contrast to the more logic-methodological approach employed by logical positivism and empiricism. The surprise here is that the recent assessments find inherent connections between logical empiricism - a successor of logical positivism - and the historical philosophy of science. In some analyses, Kuhn and Feyerabend actually continue the preceding tradition and bring the re-evaluation of some of the original tenets of logical positivism by logical empiricists to their natural end.<sup>8</sup> I will present below the main line of this type of argument.

Let us remind ourselves of the main doctrines of logical positivism. Theories were formulated in first order logic. In addition to logical machinery, there were thought to be two classes of non-logical terms: observational and theoretical. Observational terms refer directly to observable physical objects or observable attributes of physical objects.<sup>9</sup> Theoretical terms are given an explicit definition by observational terms and by correspondence rules, which connect theoretical

---

<sup>8</sup> The following publications suggest either parallel views or a continuity of views between logical positivism/empiricism and the historical philosophy of science, especially Kuhn's philosophy: Sankey (1997, 2000), Bird (2000, 2002, 2004c), Earman (1993), English (1978), Irzik and Grünberg (1995), Newton-Smith (1981, ch 7), Papineau (1979, ch 1), Reisch (1991), and to a lesser extent also Nersessian (1984).

<sup>9</sup> In the days of the Vienna circle, some took the view that terms in the basic or protocol sentences referred to phenomenal experience. The most elaborate expression of this position is arguably Carnap's *Der Logische Aufbau der Welt* (1961). This idea was soon given up, presumably because it threatens the intersubjectivity of science.

Meaning Change in the Context of Thomas S. Kuhn's Philosophy expressions directly to observational expressions. The fundamental premise of logical positivism is that observation is neutral, which guarantees a determined and stable meaning of observational expressions. Theoretical expressions alone are void of meaning and should be reducible to the observational ones in order to be meaningful.<sup>10</sup>

Advocates of the above view, notably Rudolph Carnap (1953)<sup>11</sup>, soon noticed that there are serious problems with the reduction of theoretical terms. Definition of dispositional terms, such as 'soluble' or 'fragile', by observational terms posed a particular problem. After earlier attempts with observational language had failed, Carnap suggested that they could be defined by reduction sentences, i.e. by reducing them to experimental conditions. This step is significant because it rates as an admission that observational language is not enough to fix the meaning of all terms in scientific language. This is, in other words, the first step in the process of the liberalisation of the criteria for scientific language. The second crucial step comes when Carnap (1956)<sup>12</sup> realises that not even his earlier effort to define with reduction sentences is enough. He noticed that this attempt ultimately fails because it makes a term meaningful only if certain test conditions are fulfilled. Unfortunately we are not able to determine all test cases, and so, the meaning remains indeterminate. For this reason, Carnap postulates a separate theoretical language ( $L_T$ ) that cannot be determined by observational language. Let Carnap himself express this:

In ["Testability and Meaning"] I recognised this "open" character of scientific terms, that is, the incompleteness of their interpretation. At that time I tried to do justice to this openness by admitting the addition of further dispositional rules (in the form of reductions sentences ...). I think now that the openness is more adequately represented in  $L_T$ . (Carnap 1956, 67)

---

<sup>10</sup> Suppe (1977, 17-118) offers a good summary of the "Received View" of logical positivism and of amendments made to it by logical empiricists.

<sup>11</sup> "Testability and Meaning", first published in two parts in 1936 and 1937.

<sup>12</sup> "The Methodological Character of Theoretical Concepts", first published in 1956.

## Meaning Change in the Context of Thomas S. Kuhn's Philosophy

The acceptance of theoretical language is a long way from the first idea of logical positivism. The upshot is that theoretical terms are defined by meaning postulates, i.e. the relations to both observational and theoretical terms determines meaning. This could be expressed in brief by saying that the meaning of theoretical terms is established by their role in theory.

The historical philosophers of science, Kuhn, Feyerabend and Hanson, in particular, famously questioned whether observation is neutral. They argued that one's theory or background shapes one's observation. If this is so, then the foundation of the double language model of logical empiricism is hollow. Observation was supposed to guarantee, by giving direct cognitive significance to observational expressions, that at least observational expressions receive meaning unambiguously. Now, the theory-ladenness of observation breaks this bedrock of scientific language. If observation is theory-dependent, then observational language is theory-dependent as well. For this reason, we might say that logical empiricists conceded that the meaning of theoretical terms is theory-dependent, but that the historical philosophers extended this principle to cover all the terms in science, both theoretical and observational alike. In other words, they can be seen as widening the application of "the contextual theory of meaning", which is already found in late logical empiricism. In this view, the historical philosophers took the third and final step in the process of liberalisation of the criteria for scientific language.

From these considerations we arrive smoothly at Sankey's first premise of the incommensurability thesis. The conclusion so far is that all meaning is theory-laden. Therefore, a change in the theoretical context in which terms are incorporated alters the meaning of terms. The result is the full-scale meaning change. We may say that for the logical positivists only those terms referred whose reference were determined by observation. Their sense might be thought to be the sensation that picks out the object one refers to. For Kuhn and Feyerabend, in turn, there is not a principled distinction between observational and theoretical vocabulary. Sense is determined by a theory that a term is part of, and reference is whatever fulfils the theoretical claims

Meaning Change in the Context of Thomas S. Kuhn's Philosophy of that theory.<sup>13</sup> To repeat, it follows that a change in theory results in a change in sense, and hence, possibly also in reference.

Sankey reminds us that that incomparability cannot be inferred just from meaning variance. We need a further assumption. That is the second premise of the incommensurability thesis: there is no translation between vocabularies from different theories. Translation failure is a consequence of the contextual theory of meaning. It is easy to see this if we consider a holistic theory of meaning in its radical form. That is, let us suppose that the meaning of all terms in theory is dependent on the whole theory or on the meaning of all other terms. As a consequence, any change in theory is a change of meaning of all terms. There is no way to express meanings in any other theoretical context, and translation is thereby an impossibility. It is, in principle, possible to also advocate more limited forms of holism, which has the consequence of partial untranslatability. Yet, the argument for untranslatability appears, crucially, to be the same for the part to which it applies.

Why then does untranslatability lead to incomparability? Even though there is no translation between the vocabularies of different theories, one might suppose that a comparison is possible by some other means. There is indeed a reasonable discussion on the relation between incommensurability and incomparability. The majority of philosophers interested in the incommensurability problematic have taken a short route from incommensurability to incomparability by thinking that the former simply means the latter, or at least immediately implies it.<sup>14</sup> Paul Hoyningen-Huene and Bird correctly remark that there is no such necessary link (Hoyningen-Huene 1993, 218-21; Bird 2000, 149-50). Incommensurability is a metaphor borrowed from mathematics and means that there is no common measure, i.e. theories are not co-measurable. An example of a common measure could be a ruler

---

<sup>13</sup> Alexander Bird has an elaborate analysis along these lines. According to Bird, Kuhn implies that key scientific terms have an intension that depends on certain theoretical claims. The dependence is thick, i.e. the intension depends on a wide range of theoretical claims. The dependence is also strict, i.e. all descriptions of theoretical claims have to be true of an entity in order for a term to refer. (Bird 2000, 167-8)

<sup>14</sup> That this is so can be seen from Hoyningen-Huene's extensive list of works that adopt this interpretation (1993, 218; note 118).

Meaning Change in the Context of Thomas S. Kuhn's Philosophy that is divided into centimetres and is used to measure different lengths.<sup>15</sup> Yet, incommensurability allows that, even if there is no such "point-by-point" comparison, comparison in some form is possible.

The point is that philosophers want an objective criterion to enable the systematic comparison of the truth and falsity of theories' consequences. Kuhn denies the possibility of any such (theory) neutral standard against which theories could be compared. The rejection of an autonomous observation language, the neutral language for the logical positivists, means the denial of what was thought to provide such a common unit and measure. Whatever consequences one can deduce from two theories, they are expressed in different languages, and their meanings are not translatable due to meaning holism. No observation report, or any other report for that matter, constructed with the vocabulary of one theory is relevant to another. (Bird 2000, 149-56) Theories "speak only to themselves, not each other". Whatever other criteria of comparison there might be, they do not seem very interesting because they cannot be intertheoretic truthfunctional measures. This is why the argument of the incommensurability thesis ends with the exaggerated conclusion that the content of theories cannot be compared.<sup>16</sup>

### ***Enemies of science?***

As already stated, the majority of philosophers who have studied incommensurability have taken the view that incommensurability eventually leads in one way or another to incomparability. I think that, on the whole, Sankey's incommensurability thesis

---

<sup>15</sup> According to Sharrock and Read, incommensurability was first noticed by Pythagoreans, and it can be seen by a thorough understanding of Pythagoras' law. Consider a right isosceles triangle. Pythagoras' law says that if each of the shorter sides is one unit in length, then the hypotenuse is the square root of two units in length. The problem is that the square root of two is not a rational number. As a consequence, there is no absolute commensurable measure between hypotenuse and sides because  $\sqrt{2}$  cannot be expressed accurately as a fraction. (Sharrock and Read 2002, 141-2) It is naturally possible to compare them approximately, but it is not the same as the exact "point-by-point" commensuration.

<sup>16</sup> Moreover, it is also an exaggeration with regard to Kuhn because he allowed there to be values that can be used in comparison. On this, see note 18 below and Chapter 7.

Meaning Change in the Context of Thomas S. Kuhn's Philosophy captures the main line of argumentation brought forward in philosophy over the recent decades very well. Meaning change and untranslatability form an unholy alliance in the minds of many philosophers with the result that scientific theories are incomparable. Moreover, this argument is thought to challenge the view of science as a rational and progressive endeavour. Consequently, responses have been angry, dismissive, or have sought to overcome the problems that the incommensurability thesis brings. Let us see next why the Kuhn-Feyerabend case for meaning change seems to have such far-fetching outcomes.

The first thing to mention is a consequence that derives from the holistic theory of meaning. It may be argued that it is absurd to claim that any difference of beliefs, or of theory, makes a difference in meaning. That is because it would suggest that no two persons could ever understand each other. Presumably every person's set of beliefs is at least a little bit different, which has a consequence according to meaning holism, that the terms and sentences of one person never mean the same as those of another. People cannot then agree or disagree with each other because they talk beside each other. Yet, the situation can look even more startling. Not even one individual person can mean exactly the same with his or her expressions at two different times if there is only one tiny change in his or her set of beliefs. A holistic theory of meaning and meaning change, thus, tend to make communication and understanding impossible. This type of criticism is common in the debates on holism. If communication and understanding require existence of meaning and possession of synonymous meanings between participants, then this criticism hits its target. The question of holism will be dealt with on further occasions in the thesis below.

The second worrying consequence stems from the idea of incommensurability as incomparability. Namely, if there is no way to compare theories, then theory choice cannot be rational. It is a rather common view that Kuhn's and Feyerabend's philosophy implies the irrationality of theory choice. That is, theories are chosen only by irrational means, such as propaganda, taste, or by some other subjective criteria.<sup>17</sup> However, it is not clear whether this is a fair charge.

---

<sup>17</sup> For an extensive selection of references that advance this point, see again Hoyningen-Huene (1993, 221; note 132).

If incommensurability does not necessarily imply incomparability, as was argued in the last section, there can be means to compare and choose between theories. In relation to this, we saw an argument for a kind of incomparability due to translation failure between theories. This does not yet mean that one could not find some other forms of common measure. Perhaps Kuhn's suggestion of problem-solving in *The Structure* could work as such.<sup>18</sup> Nevertheless, the charge of irrationality is commonly directed at Kuhn and Feyerabend.

One should bear in mind that we have been dealing above with semantic incommensurability and arguments from semantic incommensurability to incomparability. A third alarming consequence of Kuhn's and Feyerabend's philosophy comes directly from this argument. If there is no way to compare truths and falsities of derived statements from different theories, then it is not possible to say that theory A is more truth-like, or has greater verisimilitude, than theory B. In other words, it is not possible to assess the progress of science in terms of verisimilitude. Whatever other ways there might be for comparison of theories, it is not possible to compare the truth content of theories. As to which theory represents the world better, there is no answer. Theories are not rivals but merely alternatives. This does indeed challenge the view of science as progressing, if progress is taken as progress towards truth.

A further troubling fall-out has to do with meaning change as referential shift. How should we understand the claim that before and after theory change the same terms do refer but not to the same entities? Remember Kuhn's claim of this kind of change in the Einsteinian revolution. A descriptive theory of reference makes this view understandable, but what does it mean ontologically? Is it that before and after

---

<sup>18</sup> Already in *SSR* Kuhn indicates that persuasion in theory choice can be rational. It is possible to compare problem-solving success, quantitative precision, novelty prediction capability and aesthetic value between new and old theories (152-155). In the "Postscript" of *SSR* Kuhn writes that the reasons by which one can be persuaded are accuracy, simplicity, and fruitfulness (199). Furthermore, Kuhn says later that there are five standard criteria for evaluating the adequacy of a theory: accuracy, consistency, scope, simplicity and fruitfulness (1977b, 322). Although these values appear to be indeterminate, and therefore incompletely determine the concrete evaluations, this is a clear indication that comparison is possible. (See also Hoyningen-Huene 1993, 147-154) Hoyningen-Huene argues that Kuhn allows comparison of incommensurable theories (1993, 218-223). Feyerabend also sees that, even if theories are semantically incommensurable, and thus, semantically incomparable, there are several other ways to compare theories (See Preston 1997, 115-123).

Meaning Change in the Context of Thomas S. Kuhn's Philosophy  
such a change scientists are referring to different things altogether? Are there two different masses, for example? A scientist in the Newtonian paradigm would refer to the Newtonian mass, and a scientist in the Einsteinian paradigm would refer to the Einsteinian mass. If scientists are, hence, literally talking about different things before and after a theory change or a revolution, there is a further strong reason to regard the progress of science to be impossible. Science does not attempt in this view to learn more about the same things but instead studies entirely different entities at different times.

The above description of referential shift sounds much like idealism – almost as if the world somehow changes when scientists' theories change. Kuhn especially seems to give grounds for this interpretation. Consider the following passages:

I have so far argued only that paradigms are constitutive of science. Now I wish to display a sense in which they are constitutive of nature as well. (*SSR*, 110)

The historian of science may be tempted to exclaim that when paradigms change, the world itself changes with them. ... In so far as their only recourse to that world is through what they see and do, we may want to say that after a revolution scientists are responding to a different world. (*SSR*, 111)

In a sense that I am unable to explicate further, the proponents of competing paradigms practice their trades in different worlds. (*SSR*, 150)<sup>19</sup>

Remember also that Feyerabend claimed that theory change is an ontological change. It is not clear what exactly he means by this, but it sounds rather similar to Kuhn's

---

<sup>19</sup> But note that Chapter X, which contains the second quotation above, is named "Revolutions as Changes of *World View*" (my italics). Notice also that he writes confusingly "though the world does not change with a change of paradigm, the scientist afterward works in a different world" (*SSR*, 121); and "Whatever he may then see, the scientist after a revolution is still looking at the same world" (*SSR*, 129). I will discuss Hacking's interpretation and my attempt to reconcile these passages in Chapter 5.

Meaning Change in the Context of Thomas S. Kuhn's Philosophy world change. It is obviously possible to offer other kinds of interpretations than the idealistic one.<sup>20</sup> Yet, the idealistic interpretation has definitely some plausibility. Kuhn seems to be saying that the world itself changes in paradigm change, and Feyerabend talks about a change in ontology in theory changes. If the world with its ontology thus changes, then so naturally do references. In any case, the use of 'reference' needs some kind of explanation. I will discuss this whole problematic extensively in Chapters 5 and 7.

Meaning change seems to be, thus, a root cause for a group of problems in science and beyond: the impossibility of communication and understanding, the irrationality of science, the non-truth-convergence of science and referential variance. These problems, and especially the three latter ones that endanger the rational and truth-progressive image of science, have preoccupied the minds of philosophers during recent decades. I think it is fair to say that apart from few exceptions, most philosophers have taken a negative view on meaning change. Let Sankey serve again as a spokesperson and express this concern of so many philosophers:

The claim by Kuhn and Feyerabend that reference varies in the course of scientific theory change is of particular concern to philosophers of a *scientific realist* persuasion. Scientific realists defend the view that the aim of science is to discover the truth about an objective reality, and that scientific progress consists in an increasing convergence on the truth about such a reality. But, if the history of science consists in repeated transitions between theories which refer to none of the same things, then it is impossible for progress to occur in the sense required by the scientific realist. For if later theories refer to none of the same things to which earlier theories referred, then it is impossible for the transition between such theories to involve an increase of truths known about common items of a shared, objective reality. (Sankey 2000, 129-30)

---

<sup>20</sup> Bird interprets Kuhn's use of 'reference' as meaning 'internal reference', i.e. reference is not reference to an entity independently of theory but the hypothetical entity posited by the theory. According to Bird, the meaning of 'reference' in Kuhn is not far from the customary meaning of 'sense' or 'intension'. (2002, 13) Hoyningen-Huene, in contrast, sees Kuhn as a Kantian with the distinction between the world-in-itself and the phenomenal world (1993, 31-42). Reference would not be to things-in-itself, but to "things" in the phenomenal world. Hacking understands Kuhn as a nominalist: the world that does not change is the world of individuals, while "the world of kinds of things" in which scientists work, changes (Hacking 1993). Again, I return to this debate in Chapter 5.

Naturally, if one is not a realist, then the phenomenon of meaning change may not feel so challenging. However, anti-realistic philosophers are in a clear minority at the moment in the present era of post-logical empiricism (at least in the Anglo-American world), which is perhaps also the era of the post-historical philosophy of science; therefore, philosophers have typically treated meaning change as a cause for problems, and most discussants have sought to suggest something that would overcome these problems. Increasingly, these suggestions have concentrated on the notion of reference. In the last section of this chapter I will introduce a popular way in which the above problems are thought to be solved or avoided.

### ***Reference invariance as a solution***

The first person to suggest a referential solution to the Kuhn-Feyerabend meaning change was Israel Scheffler. His fundamental idea is that “for the purposes of mathematics and science, it is sameness of reference that is of interest rather than synonymy” (Scheffler 1967, 57). Scheffler explains this idea by citing Frege's famous example. The expression ‘Evening Star’ refers to the same object as the expression ‘Morning star’, but they are not synonymous. According to Scheffler, the identity of these expressions is scientifically interesting because interchanging them preserves the truth of the sentences. The difference in meaning, i.e. sense, does not prevent this exchange and is thus primarily a concern for the linguist. (Scheffler 1967, 56-7)

Scheffler says that references may stay the same even if the meanings of terms change. The common reference may survive cases of meaning change as the sense changes. What is more, opposing theorists who have different beliefs and whose terms' sense differ may yet refer to the same objects. (Scheffler 1967, 60) Applied to the meaning-change debate in general, Scheffler's suggestion takes the following form: a change in sense does not imply a change in reference, and science is interested in sameness of reference because sameness of reference is enough for

Meaning Change in the Context of Thomas S. Kuhn's Philosophy

the comparison of truth-values. In order to see how comparison is possible in the case where terms' senses differ, but where references are the same, let us take the above example of Frege, as used by Sankey. The components of the two expressions 'The Evening Star is a star' and 'The Morning Star is a planet' differ in their sense. But because 'The Evening Star' and 'The Morning Star' are co-referential, i.e. both refer to the planet Venus, and stars cannot be planets, the two expressions contradict each other. Both cannot be true<sup>21</sup>. (Sankey 2000, 128).

Scheffler can thus be credited with being the first to express a referential solution to meaning change and incommensurability. The crux is that the sameness of reference is a solution to all the problems mentioned above. It allows a comparison of theories via their truth-values, and also gives us then a rational criterion for theory choice. Moreover, if we know that science is dealing with the same subject matter at different times, and we can determine the truth and falsity of claims, we might be able to retain the idea that the verisimilitude of theories has increased and increases in the history of science. And, if reference is what scientists are interested in, then perhaps the problems caused by differences in sense, such as untranslatability, do not matter that much in the philosophy of science anyway. However, Sankey is right in saying that Scheffler was working in the tradition of a classic description theory of reference that determines a term's reference by the satisfaction of its associated description (Sankey 1994, 36). Scheffler does indeed imply that even though a different description or a different sense may refer to the same object, it is in any case the beliefs or senses that determine the reference. If we want to guarantee the sameness of reference in all circumstances then this is a disadvantage because it allows referential discontinuity. The descriptive contents of the two terms that determine the reference may be so different that they pick out different things; a case in point is the 'atom' of modern physics and the 'atom' of the ancient Greeks. Or perhaps, as Kuhn and Feyerabend are sometimes seen to be

---

<sup>21</sup> Alternatively, we may think of a pair of sentences that are referentially related so that they contradict each other even though the senses of component expressions differ. Let us assume that one sentence is a negation, and the other is a positive statement, and furthermore that the reference of the component expression of one is identical with that of the other. In this case, one has to be false and the other true. We may express this formally as follows:  $P_a \ \& \ \sim Q_b$  is contradictory, if  $P=Q$  and  $a=b$ . (See Sankey 1994, 39; note 3)

Meaning Change in the Context of Thomas S. Kuhn's Philosophy arguing, the two descriptions are contradictory and cannot be true of the same reference, and therefore, they fail to co-refer. In an earlier example of two apparently different references of 'mass' terms in the Newtonian and Einsteinian theories, the former is an intrinsic absolute property and the latter relative.

Confronted with the problems of the descriptive theory of reference, numerous philosophers have concluded that the Frege-Russell type of descriptivism cannot be an answer to incommensurability. That is because, even though reference is enough for a comparison of theories, the descriptive theory allows referential discontinuity, and hence, full-scale meaning change and incommensurability. Furthermore, it is reasonable to demand that the satisfaction of our occasionally totally incorrect beliefs should not decide our references. The improved answer appears to stem from theories that were originally not designed to neutralise the incommensurability thesis, but which seem to provide a solution anyway. I will now briefly examine Hilary Putnam's and Saul Kripke's ideas in order to show how the popular causal theory of reference was born.<sup>22</sup>

Putnam's and Kripke's suggestions are fundamentally anti-Fregean. It is worth studying Putnam's Twin Earth example (Putnam 1975b). The imaginary Twin Earth is like our Earth, except for a difference in the composition of the stuff people call 'water'. While 'water' on the Earth is composed of H<sub>2</sub>O, on the Twin Earth it is made of an unknown substance XYZ. H<sub>2</sub>O and XYZ are similar in their observable properties, and therefore, people on the Earth and the Twin Earth have the same beliefs about them: it is liquid, it is transparent, it quenches thirst and can be found in lakes etc. One should also note that people are identical on these earths, including with regard to their use of language. As a result, in the time before chemistry had

---

<sup>22</sup> Two early papers that extended Scheffler's point about references are Michael Martin's (1971) "Referential Variance and Scientific Objectivity" (see also Martin 1972) and Hartry Field's (1973) "Theory Change and Indeterminacy of Reference". Martin applies set theory to the problem of comparison. His main point is that an overlap in the sense of either extensional containment or intersection makes comparison possible. Field's idea is that terms may "partially denote" entities and subsequently undergo "denotational refinement". For example, Newton's mass, according to Field, partially denoted relativistic mass and proper mass, and in the Einsteinian revolution went under refinement to denote only the latter. Both of these suggestions are, however, overshadowed by the causal theory of reference. One further point to notice is that the success of the descriptive theory depends on how much theory, not necessarily all, determines reference, as Papineau (1996) has remarked. See below.

Meaning Change in the Context of Thomas S. Kuhn's Philosophy developed enough to reveal the difference in composition, let's say 1750, Oscar and twin-Oscar believe exactly the same beliefs about what they both call 'water'. Now, let us suppose that the difference is revealed and reported to the people and the twin-people. The question is: What do they say on the Earth in this kind of situation? Is meaning of 'water' on the Twin Earth different from ours, or is the meaning the same, but the reference is different? According to Putnam, we would say that the Twin Earthian's "water" is not really water, which is to say that its meaning is different from ours. Putnam wants to argue here on two separate points. The first point is that it is not enough to be in a certain psychological state in order to know meaning. Knowing meaning requires also knowing about the term's reference. Oscar and Twin-Oscar were in the same psychological state, but the meaning of their 'water' was still different. Therefore, knowing meaning cannot be defined as being in a certain psychological state. Putnam's second point has to do with the determination of reference. Oscar and twin-Oscar had the same intension of water until the discovery, but they still referred to different substances by their terms 'water'. Therefore, sense or intension cannot determine reference, as Frege thought.

The moral of Putnam's story is that intension or sense is not sufficient to determine meaning and pick out reference. He goes further than this by arguing that sense is neither necessary to determine reference and that reference may be fixed pragmatically. Putnam (1975a) formulates a causal theory of reference, which says that a user of language is connected by a certain kind of causal chain to a situation in which a name is given to an entity (200). According to this theory, 'water' refers directly to H<sub>2</sub>O, no matter what descriptions are given of it. The determination of reference takes place in a situation in which a term is first introduced. In such a circumstance a sample of stuff is named by a term, and so the reference of 'water' is fixed, perhaps ostensively, to a sample of H<sub>2</sub>O. Kripke (1980), who was the first to advance a causal theory of reference, suggests that the initial determination of the term's reference can be likened to the baptising of a child. In a baptism, a child is named and retains that name throughout the life. Similarly, a name can be given to a natural kind, such as water, in a direct causal relationship. Later users inherit the use of the name and its relation to reference. The main point is that the initial baptising via direct relation to a natural kind is responsible for reference determination. It does

Meaning Change in the Context of Thomas S. Kuhn's Philosophy not matter if the original baptisers or later users have many false beliefs about the natural kind because the description of a natural kind does not play any role in fixing reference. We need to add a further specification to point out a difference between the child's baptism and the natural kind baptism in science. Human beings are separate particular objects, and therefore, a naming ceremony does not pose any problems. Natural kinds, in turn, are not such middle-sized particulars. It is not possible, for example, to baptise all the water in the world at once. The trick suggested by the causal theorists is, as mentioned earlier, to name a sample of the baptised stuff and extend the usage to cover all possible samples of that natural kind.

### ***After simple causalism***

The causal theory of reference is, unsurprisingly, not the last word in this saga. Since the introduction of causalism, philosophers have pointed out several problems with it. Because the purpose of this thesis is not to try to find a viable theory of reference, I will not engage in an extensive detailed discussion of the problems of causal theories of reference. Instead, I will simply list some problems using some of the bibliographical references associated with them. Before concluding this chapter, I will also comment on the direction the debate on reference is heading at the moment.

The Putnam-Kripke type of simple causalism is subject to at least the following problems. Simple causalism:

- (1) is unable to grant changes in reference because reference is fixed once and for all in the baptism. Sometimes reference, however, appears to be changed. According to Fine, that happened with the shift of 'electron' from referring to the unit charge of electricity to a specific kind of particle. (Fine 1975, 25; also Sankey 1994, 55-6; Sankey 1997, 7)
- (2) does not give an adequate explanation of how reference of theoretical or hypothetical terms, such as 'positron', 'neutrino', and 'quark' can be

Meaning Change in the Context of Thomas S. Kuhn's Philosophy determined. In other words, it is not clear how baptising can be done in the absence of any observable entities.

- (3) postulates reference to intuitively non-referring terms, such as 'phlogiston' and 'deoxygenated air'. Putnam and Kripke suggest (Putnam 1975a, 200; Kripke 1980, 132) that baptising of theoretical terms is fixed by "causal description", i.e. by the following formula (which would be an answer to the second problem): Whatever is responsible for the phenomena W is named X. Unfortunately, that would make 'phlogiston' referring as it was initially thought to be responsible for combustion. (Enç 1976, Nola 1980)
- (4) unable to explicate how the kind may be individuated by ostension. Pointing and perceiving cannot specify to what exactly is being referred to. (Sterelny 1983, 121)
- (5) unable to explain the meaning of non-referring terms. In its strictest form the causal theory of reference equates meaning with reference. This seems unsatisfactory because many terms have been, and are used in science that do not, or have not been found to refer. (Suppe 1977, 203; note 520)

There seems to be a consensus that simple causalism as such is not a satisfactory theory of reference either. Reactions to the above criticism, though, vary. Some philosophers have attempted to modify the simple causal theory to better answer its critique. Perhaps the most faithful modification is Michael Devitt's *Designation* (1981). His suggestion that a term may be multiply grounded makes referential change possible due to a change in the pattern of groundings over time. Judging by the latest discussion on theories of reference, it seems that most philosophers agree that the causal theory needs a descriptive element in order to function. Even Devitt has come to this conclusion (see Devitt and Sterelny 1999, 11). A theory born out of this realisation is called causal descriptivism. According to causal descriptivism, reference is based on the causal relation between a term and an object, but is supplemented by a description (Sankey 1994, Ch. 3; 1997, 9-13) Philip Kitcher has argued that it is a mistake to think that there is only one mechanism of reference fixing. He has tried to develop a theory in which the reference

Meaning Change in the Context of Thomas S. Kuhn's Philosophy

determination of a term is associated with multiple ways of fixing, such as ostension and description (Kitcher 1978, Kitcher and Stanford 2000). Devitt and Sterelny consider several combinations between two extremes: a descriptive theory of reference fixing with no reference borrowing, and a pure causal theory of fixing with borrowing. According to Devitt and Sterelny, there are several possible "hybrid theories" (Devitt and Sterelny 1999, 96-101) There are also some philosophers who return more explicitly to descriptivism, making it more limited than, for example, the alleged implied theory of Kuhn and Feyerabend. Such a modified descriptive theory is the Ramsey-Carnap-Lewis account of reference determination, which is suggested by Papineau (1996)<sup>23</sup> and advocated by Bird (2000, 185-88). This theory identifies a core meaning that picks out reference. The core meaning stays invariant between two theories despite the changes in some out-of-core beliefs. Still at least one unmentioned possibility is Ilkka Niiniluoto's idea that employs the similarity account of approximate truth and truthlikeness in reference determination (Niiniluoto 1999, 132). The debate on theories of reference certainly continues, and it can be said that it is far from certain which theory shall emerge as the most successful.

## ***Conclusion***

In this chapter I have examined how Kuhn's and Feyerabend's meaning change has been interpreted and received in the philosophy of science. In contrast to the early responses, the meaning-change thesis is seen in the recent analyses to have taken a further conclusive step in the development of some basic tenets of logical positivism. Kuhn and Feyerabend are understood to have generalised the contextual theory of meaning, already found in logical empiricism, so as to cover all scientific terms, and as a consequence, making all terms, not only theoretical, sensitive to meaning change. The crucial move on the way to full-scale meaning holism and meaning variance was through questioning the neutrality of observation. Meaning change is conceived primarily as a change in sense, which also often leads to a shift in

---

<sup>23</sup> See also the original proposal by Lewis (1970).

Meaning Change in the Context of Thomas S. Kuhn's Philosophy

reference. One of the most troublesome consequences is incommensurability, which questions whether communication and understanding, rational comparison, and scientific progress as truth approximation are possible. It may be argued that it also leads to a kind of idealism. Meaning change is perceived, therefore, as the source of lots of problems and as a threat to scientific realism in philosophy. As a result of this, discussion on meaning change and incommensurability has focused on constructing arguments against the variance of meaning and the incommensurability thesis. A large number of philosophers have argued that the invariability of reference is the solution, and there are by now many differing suggestions for a good theory of reference. It is likewise widely accepted that if a term's reference stays invariant in theory change, then comparison between theories is possible, and the realists' image of science as approximating truth can be retained. The most successful theory of reference during recent decades has arguably been the causal theory of reference. Yet it has been pointed out that there are also many problems with the causal account, and the debate on the nature of reference determination is as yet open-ended.

### **3. Genesis and development of meaning theorising**

In the previous chapter we saw briefly how a number of philosophers thought that Kuhn and Feyerabend owe more to logical empiricism than has been recognised. In this chapter I will examine what the appropriate context is for these two philosophers. This examination is however concentrated more on Kuhn for the reason that is explicated below. My overall conclusion is that, although there are undeniable parallels between late empiricism and Kuhn, one should be wary of reading too much into these parallels. That is because connecting Kuhn too closely to logical empiricism conceals fundamental differences in the two views. In particular, we should not confuse a parallel with a historical continuum. The genesis of Kuhn's views differs drastically, for instance, from those of Carnap. Firstly, I again take a brief look (but more detailed than in the last chapter) at some of the philosophers who emphasise the similarities between Kuhn and logical empiricism. After that, I argue that the proper origin of Kuhn's argumentation is in his studies of history. Thirdly, I show how Kuhn's notion of meaning developed in the course of his career from the early idea that stemmed from his historical interest towards a more philosophically explicit account. A particularly significant source of influence was Wittgenstein's later philosophy. My contention is that Kuhn did succeed in explicating some of his insights on meaning although he may not be said to have achieved a high level of sophistication in his meaning theorising.

#### ***Logical empiricism and Kuhn***

At the beginning of the 1960s Feyerabend and Kuhn seemed to be on the same front in an attack against logical empiricism. However, their careers took a different turn by the end of that decade. Kuhn's interest in the traditional problems in philosophy increased; consequently, he tried to answer to criticism that was directed at his

Meaning Change in the Context of Thomas S. Kuhn's Philosophy philosophy. In contrast, Feyerabend appeared to lose interest in such problems. That is well illustrated in the debate on meaning and meaning change. Kuhn continued to emphasise the significance of meaning until the end of his career. Feyerabend's last writings that constructively address meaning and meaning change all appeared in the mid 1960s.<sup>24</sup> Feyerabend had already said in 1965, "As far as I am concerned, even the most detailed conversations about meanings belong in the gossip columns and have no place in the theory of knowledge" (Feyerabend 1981b, 113). He agrees to deal with meaning only because it occurs so often in philosophical debates. Feyerabend's decision to develop his radical 'epistemological anarchism' and such "methodological rules" as 'anything goes' also diminished his influence in philosophy, although it may have increased outside philosophy for the same reason.

The rationale for my decision not to pursue Feyerabend further is precisely that he did not develop his sketchy idea of meaning. Even more importantly, Kuhn's and Feyerabend's advancements of the meaning-change thesis stem from different sources, and they cannot, therefore, be lumped into one. Ironically, it is Feyerabend, and not Kuhn, who is best understood against the tradition of logical empiricism. The supervisor of Feyerabend's thesis was a former member of the Vienna Circle, Viktor Kraft. Furthermore, Feyerabend studied under Popper in London, and according to his own words, "fell for" falsificationism (Feyerabend 1995, 89). He was also deeply familiar in many other ways with logical positivism and in touch with the philosophers from that tradition. He regarded himself as an empiricist until the end of the 1960s trying to improve and reconcile empiricism with realism.<sup>25</sup> Moreover, Feyerabend directed his critique in some of his early texts directly against the meaning theory of logical positivism and also against such logical empiricist theories as Nagel's theory of reduction and Hempel's theory of explanation<sup>26</sup>. In light of this,

---

<sup>24</sup> The four most important articles in which Feyerabend discussed meaning and meaning change are "Explanation, Reduction and Empiricism", "Problem of Empiricism", "On the 'Meaning' of Scientific Terms" and "Reply to Criticism: Comments on Smart, Sellars, and Putnam". The first was published in 1962 and the rest in 1965. All except "Problems of Empiricism" (see Feyerabend 1965) are reprinted in Feyerabend 1981b.

<sup>25</sup> In an article "Science without Experience" (Feyerabend 1981b) Feyerabend argues that science without experience, or sensory elements, is possible, thereby giving up empiricism.

<sup>26</sup> See, for example, "Explanation, reduction and empiricism" (in Feyerabend 1981b) and "Problems of empiricism" (Feyerabend 1965).

Meaning Change in the Context of Thomas S. Kuhn's Philosophy

Feyerabend may be said to have been still working in the 1960s in the tradition of logical empiricism. His criticism of the empiricist theory of meaning was essentially based on the challenge of the idea of observation as neutral. This stance is also partially responsible for his contextual theory of meaning<sup>27</sup>. He naturally attempted to make philosophy on the whole more compatible with the history of science, just as Kuhn did, but the influence of logical empiricism was much stronger in Feyerabend than in Kuhn. This different genesis of their arguments, and the fact that Kuhn continued to philosophise on meaning, make their views disparate in important respects, which become even more clearly visible later in Kuhn's career. The apparent similarities may conceal the differences.

Let us, then, take a look at the differing ways in which some recent philosophers have connected Kuhn to logical empiricism. Jane English (1978) was probably the first who gave a detailed analysis of the parallels between Kuhn and Carnap<sup>28</sup>. English labels Kuhn's and Feyerabend's views of meaning change as contextualist in the sense that every substantial change in theory changes at least meanings of terms that occur in revised sentences. According to English, this makes intertranslation, contradiction and progress as increasing verisimilitude, impossible. English's main point is that, in contrast to what has been thought, Carnap's view faces these same objections as the meaning-change view. George A. Reisch (1991) explains Carnap's appreciation of Kuhn's *Structure of Scientific Revolutions* in a letter on the possible publication of it in 1960 by the similarity of their views and claims "that logical empiricism, insofar as that program rested on Carnap's shoulders, was not substantially upstaged by Kuhn's book" (264). That is, both Carnap and Kuhn adopted the distinction between radically different theories, languages or conceptual frameworks that change during the revolutionary period. They are incommensurable and can therefore be evaluated only pragmatically. John Earman (1993) writes that many of the post-positivist themes are extensions from

---

<sup>27</sup> Also the later Wittgenstein played a role in Feyerabend's theory of meaning, and in Kuhn's as well. It appears, however, that they adopted Wittgenstein's idea differently. See footnote 37 of this chapter.

<sup>28</sup> It is, however, interesting to note that, as early as 1969 at the symposium on the structure of scientific theories in Urbana, Kuhn was accused of being too close to, or of adopting some principles from logical positivism by such philosophers as Shapere (507), Putnam (513), and Achinstein (516). A similar accusation is made also by Suppe in 1977 (647) (All page numbers refer to Suppe 1977).

Meaning Change in the Context of Thomas S. Kuhn's Philosophy

Carnap and other logical positivists and empiricists. Such themes are at least the non-existence of neutral facts, incommensurability in the form of failure of intertranslatability and paradigm as analogous to Carnap's linguistic framework. Also, according to Earman, a holistic view of meaning emerges from logical positivism. Gürol Irzik and Teo Grünberg (1995) conduct a comparative analysis between Carnap and Kuhn, and they as well want to challenge the conventional wisdom that the post-positivist philosophy of science constituted a radical rupture without too many affinities with logical positivism. They attempt to show that Carnap's linguistic frameworks function in the same way as Kuhn's lexical structures; Carnap's scientific theories resemble, at least in some respects, Kuhn's paradigms; and Carnap's and Kuhn's characterisations of scientific revolutions are nearly identical. Furthermore, they seem to agree with Earman and disagree with Earman that the Kuhnian type of semantic holism is inherent in later Carnap and that semantic incommensurability is a direct consequence of it. Michael Friedman (2003), in turn, agrees with Reisch that both Kuhn and Carnap accepted the philosophy of linguistic frameworks and he deepens this analysis by studying the influence of neo-Kantianism. These affinities reflect, Friedman writes, an early-twentieth-century intellectual situation encompassing both the history and philosophy of science. Finally, Alexander Bird presents Kuhn in his various very recent publications on Kuhn<sup>29</sup> as the last empiricist rather than the first post-empiricist. Bird argues that Kuhn failed to break entirely with the preceding tradition and consequently kept many theses from the logical empiricist tradition. Some of these are, for instance, intensionalism, meaning holism, and scepticism towards theoretical entities. In terms of meaning and meaning change, Bird (2002) introduces a catchy phrase that "Kuhn's incommensurability thesis can be seen as positivism plus the theory-dependence of observation" (14). This is essentially the view that emerged in the last chapter in my analysis of Sankey's incommensurability thesis.

---

<sup>29</sup> Especially Bird 2000, 2002 and 2004b. Also Bird 2003a, 2003b, 2004a, 2004c and 2005 support the view.

All, or nearly all, these views imply or explicitly state that both Carnap and Kuhn held a view of meaning holism<sup>30</sup>. Their opinion is that meaning holism explains why Kuhn advanced the meaning-change thesis. However, it is not always clear whether these various writers are ready to claim that, regarding meaning and meaning change, there is also a historical continuum between Kuhn and logical empiricism. Some, such as Reisch and Irzik and Grünberg attempt to show merely analogies, while Earman and Bird argue that they at least partially shared the same tradition, and therefore, certain of Kuhn's views can be traced to logical empiricism.<sup>31</sup> I will not go into a deeper analysis of what others have said of the relationship between Carnap and Kuhn, but will formulate my own view of it instead. I contend that, with respect to Kuhn's view on meaning and meaning change, there is no historical continuum between Kuhn and logical empiricism. Furthermore, even though the comparison of the views is naturally entirely legitimate, it should not hide the fact that Kuhn's thinking stems from a different origin than those with apparently similar views in logical empiricism; and that his notion of meaning, in particular, is not just a further development of that notion from logical empiricism.

### ***Kuhn's historical rationale***

In a very broad sense Kuhn and logical empiricism may be said to have shared the same tradition. However, they did not share it so specifically that this claim would have historical explanatory value in the context of this thesis. For instance, it is true that Kuhn adopted intensionalism, i.e. that intension or sense picks out extension or reference. But the group that has accepted this principle is rather large and not reducible to empiricism. Moreover, there are not that many pure "externalists", i.e.

---

<sup>30</sup> Earman thinks that the route to semantic holism can be traced only to Carnap's *Aufbau*. The rest of the analysts disagree. To me it seems that they are right; Carnap's later philosophy clearly had features of holism.

<sup>31</sup> John Preston (2004) and Bird (2004c) engage in a debate over the question of whether "the theoretical context view of meaning" that is attributed to Kuhn can be said to be positivistic, and if it can, then in what sense? Bird claims that it is positivistic "aetiologically", i.e. the view was developed

those who think that reference or extension can be determined without any intensional elements and that intension is not needed to explain any part of meaning. The improvement in moving from intensionalism to externalism in semantics, i.e. to simple causalism, is actually in doubt at the moment, as argued at the end of the last chapter. Furthermore, continuity between logical empiricism and Kuhn on the one hand, and logical empiricism and the realistic post-empiricist philosophy on the other, cuts both ways. Admittedly, both Kuhn and logical empiricism held the idea of linguistic frameworks and a holistic view of meaning in some form. However, Kuhn did not share with logical empiricism the belief that, for instance, the history of science is cumulative, or the conviction that the meaning defining part can be clearly separated from the non-meaning defining part. The realistic philosophy does indeed assume that scientific knowledge is cumulative. And despite Quine's writings, the sharp distinction between meaning constituting and non-constituting parts has still many advocates in the present philosophy.

It is clear that Kuhn's understanding of the theories of logical empiricism was superficial at the beginning of his career. First of all, we should remember that he was, as Kuhn himself expressed, "a physicist turned historian for philosophical purposes" (RSS, 320-1). As a consequence of this, although his interest was philosophical, he did not know philosophy very well at the beginning of his career. After Earman had attempted to show the parallels between Kuhn and logical positivism, Kuhn felt compelled to answer:

Whatever role the problems encountered by positivism may have played in the background for *The Structure of Scientific Revolutions*, my knowledge of the literature that attempted to deal with those problems was decidedly sketchy when the book was written. In particular, I was almost totally innocent of the post-*Aufbau* Carnap, and discovering him has distressed me acutely. (RSS, 227)

---

by the logical positivists, and Kuhn adopted it from others who have followed that path further (see Bird 2004c, 338-40).

## Meaning Change in the Context of Thomas S. Kuhn's Philosophy

How limited Kuhn's knowledge of logical empiricism was becomes apparent from an interview in which Kuhn tells that in his early career "I read some von Mises; I certainly read Bridgman's *Logic of Modern Physics*; I read some Philipp Frank; I read a little bit of Carnap, but not the Carnap that people later point to as the stuff that has real parallels to me". (RSS, 305-6) Kuhn's ignorance can also be seen in references in *SSR* or in other early works. In *SSR* there are two unimportant references to philosophers that could properly be said to be logical empiricists; one is to Ernest Nagel and the other to Philipp Frank.

It is important to underline his ignorance of logical empiricism in order to see the proper context of Kuhn's thoughts on meaning. He had some idea of logical positivism, but it was such a general idea that Kuhn himself later called it an "everyday image of logical positivism" (RSS, 306). The image entailed that science proceeds from facts given by observation. Facts are objective and interpersonal, i.e. accessible to all normally equipped human observers. They are also prior to laws and theories. After discovery, facts become data for science, and they subsequently form an objective and secure basis for science (RSS, 106-7). Kuhn says that he opposed this kind of view (RSS, 306) and that he was primarily motivated by difficulties in logical positivism and empiricism (RSS, 106). At this point, we have to ask why Kuhn objected to logical empiricism so much and what role such an opposition played in his thinking. In order to answer these questions, we need to ask a yet larger question: What were Kuhn's main objectives in his career?

Wes Sharrock and Rupert Read in their book, *Kuhn. Philosopher of Scientific Revolution*, think that Kuhn had two major objectives: one was historical and the other philosophical. According to Sharrock and Read, the historical aim was to get people to understand the past on its own conditions and not in terms of the present. Too often, past science was used either to explain or justify the present scientific achievement to the public and to science students. (Sharrock and Read 2002, 7) Sharrock and Read write that the demand to consider the different periods of science in their own right "rather than as mere stepping stones to the present" may seem a modest and plausible suggestion, but that it could have far-reaching implications and transform the way "the relationship between changing ideas" in science is understood

Meaning Change in the Context of Thomas S. Kuhn's Philosophy (Sharrock and Read 2002, 8). The view that Kuhn opposed, they say, is so called Whig-history, according to which the past is a preparation for or even a prefiguring of the present. The abandonment of this conception of history would mean that the present science is understood merely as the latest date in history, not its fulfilment. Kuhn's second, philosophical, objective was to change the public image of science. Insofar as the image of science is based on the history of science, it is distorted because the picture of past science is distorted. Sharrock and Read argue that Kuhn attempted to correct the view of science as continuously cumulative, and replace it with an image that would be more adequate in the light of history research.

I think that Sharrock and Read are right about Kuhn's purposes.<sup>32</sup> They also correctly emphasise that the acceptance of Kuhn's historical point of view would have far-reaching consequences in the philosophy of science, although I feel that Sharrock and Read have not really fully developed this idea. In Chapter 7 I will show how Kuhn's historical inclination leads to epistemological anti-realism or epistemological scepticism. The relation of the cumulative image of science to an "everyday image of logical positivism", or potentially even to a more sophisticated view of that philosophical school, is that the former is based on the latter. If science really discovers facts in the true sense of the word, i.e. objective and neutral states of affairs, they naturally cumulate expanding scientific knowledge in the course of time. Once a fact is found, it would be unreasonable to stop believing in it because it just is "how things are" in the world. A historian's job is, then, to verify and chronicle this accumulation and explain deviations from the secure path of knowledge. Kuhn thought that the cumulative view is not a tenable picture of the history of science. It is, rather, at odds with the image of science that arises from the actual studies of history.

The starting point and an object of criticism in arguably Kuhn's most important work, *The Structure of Scientific Revolutions*, is the conception of history in science text-books. Kuhn takes it to be as accurate as an image of culture drawn

---

<sup>32</sup> I do not think, though, that their reading of Kuhn as a Wittgensteinian therapeutic philosopher is convincing because Kuhn clearly had philosophical ambitions based on his historical point of view. More on this, see this chapter below and Chapters 4 and 7.

Meaning Change in the Context of Thomas S. Kuhn's Philosophy from tourist guides. He implies that history is viewed in them as a "repository for anecdotes and chronology" (*RSS*, 1). Text-books present history as cumulative. The job that is left for a historian is to determine by whom and when each law, fact and theory was discovered. Beyond that, s/he needs to explain why the even more rapid and straightforward development of science to the present point has been hampered by errors, myths and superstition. Indeed, the main objective in *SSR* is to correct this image found in text-books. Kuhn writes:

This essay attempts to show that we have been misled by them [text-books] in fundamental ways. Its aim is a sketch of the quite different concept of science that can emerge from the historical record of the research activity itself. ... Rather than seeking the permanent contributions of an older science to our present vantage, they [historians of science] attempt to display the historical integrity of that science in its own time. (*RSS*, 1-3)

The historians of science who adopt the outlined alternative method of studying history examine, for instance, the relationship of Galileo's views, not to modern scientists, but to his contemporaries. Furthermore, they try to give maximal coherence to those views and opinions. Finally, Kuhn tells us that "these historical studies suggest the possibility of a new image of science" and his "essay aims to delineate that image by making explicit some of the new historiography's implications". (*SSR*, 1-3)

Delving deeper into Kuhn's mind, we also need to ask what made Kuhn, who was a scientist after all, conclude that the image of history taught in science education was wrong. The incident that leads to this realisation is undoubtedly the most profound and significant single intellectual experience in Kuhn's career. It is worth quoting Kuhn here at length in order to catch his insight:

My own enlightenment began in 1947, when I was asked to interrupt my current physics project for a time in order to prepare a set of lectures on the origins of seventeenth-century mechanics. ... Like most earlier historians of science, I approached these texts knowing

what Newtonian physics and mechanics were. Like them, too, I asked of my texts the questions: How much about mechanics was known within the Aristotelian tradition, and how much was left for the seventeenth-century scientists to discover? Being posed in a Newtonian vocabulary, those questions demanded answers in the same terms, and the answers were clear. Even at the apparently descriptive level, the Aristotelians had known little of mechanics; much of what they had had to say about it was simply wrong. ...

Generalisations of that sort were widely current and apparently inescapable. But they were also puzzling. When dealing with subjects other than physics, Aristotle had been an acute and naturalistic observer. In such fields as biology or political behaviour, his interpretations of phenomena had often been both penetrating and deep. How could his characteristic talents have failed him so when applied to motion? How could he have said about it so many apparently absurd things? And above all, why had his views been taken so seriously for so long a time by so many of his successors? The more I read, the more puzzled I became. Aristotle could, of course, have been wrong – I had no doubt that he was – but was it conceivable that his errors had been so blatant?

One memorable (and very hot) summer day those perplexities suddenly vanished. I all at once perceived the connected rudiments of an alternate way of reading the texts with which I had been struggling. For the first time I gave due weight to the fact that Aristotle's subject was change-of-quality in general, including both the fall of a stone and the growth of a child to adulthood. ... More consequential was my recognition that the permanent ingredients of Aristotle's universe, its ontologically primary and indestructible elements, were not material bodies but rather the qualities which, when imposed on some portion of omnipresent neutral matter, constituted an individual body of substance.

...those aspects of my new understanding of Aristotle's enterprise should indicate what I mean by the discovery of a new way to read a set of texts. After I achieved this one, strained metaphors often became naturalistic reports, and much apparent absurdity vanished. I did not become an Aristotelian physicist as a result, but I had to some extent learned to think like one.

... The discovery of hermeneutics<sup>33</sup> did more than make history seem consequential. Its most immediate and decisive effect was instead on my view of science.

---

<sup>33</sup> Note that, although Kuhn was apparently somewhat acquainted with the continental hermeneutical tradition, his use of 'hermeneutics' does not indicate that he had adopted some specific hermeneutical theory that he applied systematically in his studies. (See Hoyningen-Huene 1993, 21) 'Hermeneutics' is here probably best understood as a practical attitude to historical texts that tries to understand them and maximise the coherence of the texts in the context of their own time. Or alternatively, it may be

...While discovering history, I had discovered my first scientific revolution, and my subsequent search for the best readings has often been a search for other episodes of the same sort. They are the ones that can be recognised and understood by recapturing out-of-date ways of reading out-of-date texts". (*ET*, xi-xiii)

I think that the significance of the Aristotle experience on Kuhn's thinking becomes evident from the above quotation. He felt that he could finally understand Aristotle so that "strained metaphors became naturalistic reports, and much apparent absurdity vanished", and that he had learned to think like an Aristotelian physicist. Kuhn calls this moment his "first scientific revolution". This new, "out-of-date", way of reading made history "consequential" and had a "decisive effect" on Kuhn's view of science. He returns to this incident on several occasions. First of all, Kuhn's daughter, Sarah Kuhn, says that the importance of this experience cannot be overstated as his children heard about it from time to time (Andresen 1999, 56). Kuhn also mentions it in various papers. The citation was taken from the "Preface" of the volume *Essential Tension*, first published in 1977. An article, "A Function for Thought Experiments", from 1964 describes in detail one aspect of the Aristotle reading; how the meaning of 'speed' was different in Aristotle compared to the physics that came after him. In an essay "What are Scientific Revolutions", published in 1987, Kuhn gives a very much similar, although longer, description of the above reading of Aristotle. In an article that appeared in 1989, "Possible Worlds in History of Science", Kuhn extends the attempt to understand apparently nonsensical "out-of-date" texts in relation to his other encounters with historical figures, such as Newton, Volta, Bohr and Planck<sup>34</sup>. In an interview less than a year

---

taken as an attitude that tries to understand the thinking of people in the past in as authentic a way as possible. Andresen calls this orientation "empathetic historiography" (Andresen 1999, 55). Likewise, Kuhn's children, Sarah and Nat Kuhn, said rather aptly in an interview that Kuhn's "scientific methodology was based on an intellectual empathy that enabled him to see things from another person's point of view" (Andresen 1999, 60).

<sup>34</sup> "A historian reading an out-of-date scientific text characteristically encounters passages that make no sense. That is an experience I have had repeatedly whether my subject was an Aristotle, a Newton, a Volta, a Bohr or a Planck. ... Apparent anomaly is thus ordinarily evidence of the need for local adjustment of the lexicon, and it often provides clues to the nature of that adjustment as well. An important clue to problems in reading Aristotle's physics is provided by the discovery that the term translated 'motion' in his text refers not simply to change of position but to all changes characterised by two end points." (*RSS*, 59-60) For a similar account that extends the reading also to Boyle, Carnot

Meaning Change in the Context of Thomas S. Kuhn's Philosophy

before his death in 1995, Kuhn recalls the Aristotle experience. He says: "I had wanted to write *The Structure of Scientific Revolutions* ever since the Aristotle experience. That's why I had gotten into history of science" (*RSS*, 292). It is essential to understand that this event is not interesting only for biographical or psychological reasons, but also because it colours virtually the whole of Kuhn's later career and his thinking on the the notion of meaning in particular.<sup>35</sup>

Kuhn expressed particular anger, probably in part because he felt himself misunderstood, towards the attitude that ridicules past thinkers by pointing out how absurd were the things they believed or how incoherent their thinking was. Kuhn's objective was to maximise the coherence of their thinking and assume historical "integrity" to his objects of research. The key to Kuhn's reading of Aristotle was that he did not try to make sense of it from a modern perspective, but rather on its own conditions. That implied, as Kuhn described, getting "inside the heads of the people" (*RSS*, 276) or to "try to think as they did" (*ET*, 110; similarly *ET*, 8), a special skill that Kuhn believed he possessed. Such an attempt cannot be done piece-by-piece or by looking for one-to-one matches between the meanings of the terms of past and modern science. It is necessary to understand the whole world view of past scientists. For example, one needs to recognise that neither Aristotle nor his contemporaries meant what modern scientists do by 'motion' and also that his problems were in some way different. This, together with a few other realisations of Aristotle's philosophy, explain the odd usage of 'speed' in Aristotle's texts from the modern perspective.

In respect of the understanding of past science, Kuhn contrasted philosophers and scientists to historians. The former are primarily concerned with what is right and wrong, and for this reason, tend to study historical texts in relation to what is

---

and Maxwell, see Kuhn's "Remarks on Receiving the Laurea of the University of Padua", as quoted in Caneva 2000 (102).

<sup>35</sup> For an interesting analysis of how various incidents in Kuhn's personal life might have affected his intellectual life, see Andresen 1999. Andresen appears to be in agreement with the author of this thesis on the significance of the Aristotle experience: "Kuhn's sudden understanding of Aristotle's thought, which he considered to be an especially significant event, confirmed the efficacy of a methodology grounded in empathetic historiography" (55). For yet further confirmation of the significance of the Aristotle experience, see Kuhn's remarks at the university of Padua, as quoted in Caneva 2000 (100).

Meaning Change in the Context of Thomas S. Kuhn's Philosophy known now, picking out the true and the false. According to Kuhn, the latter try simply to understand why a particular person or persons thought as they did. (RSS, 315) The objective to figure out truths and falsities may be an appropriate attitude for a scientist, but it leads to a distortion when projected as such to the history of science. The problem for scientific communities regarding their view of history is that in scientific revolutions their conception of what is true and false may change drastically. Kuhn believed that scientific communities engage, therefore, in rewriting history after each scientific revolution. The reason for having the Whig-conception of history is that it makes both the student and the professional feel like participants in a long-standing historical tradition, who are working on the same set of fixed problems with the same set of canons, and that gives a tremendous psychological motivation for the participants in a scientific community. (SSR, 137-8) Kuhn even compares a member of a mature scientific community to a typical character of Orwell's *1984* because s/he is "a victim of a history rewritten by the powers that be". (SSR, 167).

This brings us back full circle to where we started from. The fight against Whig-history that views history as heading towards the present was fundamental to Kuhn. Kuhn thought that philosophically the image derives from the doctrines of logical positivism and is typically found in science text-books. Whig-history may be pedagogically effective and increase the cohesion of the group, but it is historically unacceptable. What one has to learn is to study past scientists and communities on their own conditions. An intrinsic part of that attitude is to try to figure out what they meant by their terms. This leads inevitably to the realisation that, even if they might have used the same terms as we do, the meanings attached to those terms may be very different from ours. Understanding the proper meanings of the expressions used in history reveals the world view of past scientific communities, and is, therefore, essential for understanding the past science itself.

This is, in short, the root of Kuhn's meaning talk. It stems from an attempt to make sense of old texts that, at first sight, look incoherent and absurd. Kuhn's notion

---

Kuhn says, for example, "My attempt to come to terms with Aristotle's texts determined my future life".

Meaning Change in the Context of Thomas S. Kuhn's Philosophy of meaning implies two basic components that he carried forward throughout his career. The first is that Kuhn is talking about meaning either in the psychological or sociopsychological sense. He is interested in what people thought or believed. The presumption is that people expressed their views in their language, and that is why studying "meanings" is so important. The second component is that Kuhn strongly believed that meaning has to be holistic in some sense. It is not possible to understand the meaning of just one term. If a person understands one, s/he has to understand many, because meanings are somehow dependent on each other. Later, Kuhn tried to give a more precise definition of his notion of meaning and also took part in the discussion on the nature of meaning. The increasing influence of certain philosophical ideas, notably that of Wittgenstein, is clear in his theorising, but these two elements preserve their place in Kuhn's philosophy. Later in the thesis we see that determination of extension begins to increasingly preoccupy Kuhn's mind soon after the early historical studies.

Nowdays it is often pointed out that Carnap and Kuhn both held the holistic view of meaning and were committed to the meaning-change thesis. That may be right. However, now we can see also that the genesis of their views is different in crucial ways. Carnap arrived at his position after all the difficulties with the Received View of logical positivism, which were briefly described in the last chapter. Kuhn's "meaning theory" and its holistic flavour are, in turn, positive constructions that arose from Kuhn's studies of history. Moreover, Kuhn thought that respecting the holistic feature of meaning is a necessary precondition for understanding historical texts. It would be inappropriate to suppose or imply that Kuhn continued from the point that logical empiricism had reached. It is true that Kuhn and other historical philosophers challenged the idea of neutral observation and observation language, which was a central tenet in logical positivism. This questioning can be seen as adding to the difficulties of logical empiricism, making the tradition, on the whole, look quite bankrupt. But to suggest a clear continuity in the meaning theory would suggest that Kuhn and Carnap shared the same theoretical body and were led the same way towards a certain conclusion, only for Kuhn to end up with a more radical view. The proper context is that Carnap was led to his view by the careful examination of the Received View, while Kuhn aimed at better comprehension of old

Meaning Change in the Context of Thomas S. Kuhn's Philosophy writings and arrived at a somewhat similar view by reading and interpreting historical scientific texts.

I disagree, therefore, with Bird's claim that Kuhn's notion of meaning is aetiologically positivistic (see note 8 above). Rather, it derives from Kuhn's historical-interpretative objective. Bird (2004c) criticises Read and Sharrock's (2002a) view of Kuhn as a Wittgensteinian therapeutic philosopher, a view which Preston (2004, 332) also appears to share, on the ground that it is poorly supported by the textual evidence. I am in agreement here with Bird, and this point is also apparently conceded by Read and Sharrock (e.g. 223, note 3). But the same can be said of Bird's and others' attempts to connect Kuhn to logical empiricism via his notion of meaning. This is also poorly supported by textual evidence, which Bird, in turn, admits. He says: "the truth of my claim [that Kuhn's meaning is aetiologically positivistic] cannot be established directly, because Kuhn does not even articulate a theoretical context view of meaning in *The Structure of Scientific Revolutions*, let alone justify it or say what his source for it is" (Bird 2004c, 340). This is in contrast, for example, to Feyerabend who directly linked his "contextual theory of meaning" to the later Wittgenstein. But now we see that there is textual evidence for linking Kuhn's theory of meaning to the historical interpretation of scientific texts. Furthermore, it is not convincing to try to support the thesis of Kuhn's connection to logical empiricism, as Bird does, by the fact that Kuhn rejected "anti-positivistic and naturalistic" development in semantics, i.e. the causal theory of reference, because of his hidden positivism. As I argue in Chapters 4 and 5, Kuhn's rejection can be seen as not only containing a serious philosophical criticism of the causal theory, but it can be also explained by a genuine metaphysical difference between Kuhn and the causal theorists. What is more, the rejection can be interpreted as receiving empirical support, and hence, can be seen as being compatible with the naturalistic philosophy.

The divergent origin of his philosophy and logical empirism is also something that Kuhn himself remarked on in an interview. After confessing to being embarrassed at not knowing Carnap better at the time of writing *SSR*, Kuhn insists:

the view that emerges in *Structure* is not the same as the Carnap view, but it's interesting that coming from what were partially different ... Carnap staying within tradition had been driven to this – I had rebelled already and come to it from another direction, and in any case we were still different. (*RSS*, 306).

It is indicative that Kuhn claims that not only the genesis of their views is different, but that the views themselves are not identical. The remainder of the chapter is devoted to closely studying what Kuhn said about meaning, and how his view changed over the course of time. In short, I will show how Kuhn's views "were still different". This exploration makes clear the distinctiveness of Kuhn's thinking in general, and meaning theorising in particular.

### ***The development of Kuhn's notion of meaning***

'Meaning', whatever that precisely turns out to be, was together with the concept of incommensurability, arguably the most important notion in Kuhn's philosophy. Kuhn tells us how he thought that he had been talking about meaning change in *SSR*, only realising, to his surprise, how few references to meaning there were. Kuhn himself explained this by his ignorance of the philosophical debate on meaning, which had led him to rely on gestalt switches. (*RSS*, 298). He was also disappointed that the other historical philosophers abandoned the problem of meaning, which he held as central in philosophy (*RSS*, 309-10). In any case, the whole of Kuhn's career is a testimony of how important that notion was to him. He tried to improve his "theory" of meaning until the end of his life.

I think it is now clear that Kuhn's knowledge of philosophy of his own time was rather limited, and he could certainly not be compared to the philosophical sophistication of Carnap. At the time of drafting *SSR*, he had in mind probably some kind of "contextual theory of meaning", i.e. that the meaning of a term depends on the context in which it is incorporated. Yet, because his notion of meaning was still very sketchy, one should avoid reading too much into it without also paying attention

Meaning Change in the Context of Thomas S. Kuhn's Philosophy to what Kuhn postulated later. I believe that Kuhn is later able to specify certain aspects in his notion of meaning that capture the original historical insight better; therefore, it is reasonable to say that, over a period of time, we can find improvement in his meaning theorising.

### **Early period - crude account**

In Kuhn's early major historical work, *The Copernican Revolution*, there is little direct talk of changes in meaning. Instead, Kuhn writes a lot about how the conceptual scheme changed in the Copernican revolution. However, the historical material presented in the book reappears in *SSR* as one of the many examples of meaning change. The passages in the last chapter, all taken from *SSR*, seem mostly to state only that the meaning of scientific terms change. It is difficult to find in them much specification of in what sense meaning changes, or what this meaning is that changes. Nonetheless, from a purely historical perspective, they can be made fully intelligible. If one knows the Ptolemaic and Copernican astronomy even superficially, s/he probably understands the point of saying that 'Earth' has a different meaning in these two systems. In the former, the Earth is a unique stable entity in the centre of the universe around which other bodies revolve, and in the latter, the Earth is one of many planets and is, therefore, circling around the sun<sup>36</sup>. Similarly, in the pre-Einsteinian period, space was thought to be flat, homogenous, isotropic, and unaffected by the presence of matter, but nothing like this after Einstein's theory. Why not then say that the meaning of 'space' had changed. Or, to take one more example, if the pre-Lavoisier's 'element' was a fundamental metaphysical substance found in all bodies and responsible primarily for their observable qualities, while Lavoisier's 'element' was a so-far-indecomposable material substance responsible only for the chemical properties of bodies, then it seems reasonable to say that there was a change in the meaning of 'element' in

---

<sup>36</sup> Notice that Kuhn does not treat the Earth as a proper name in *SSR*. It is written as a common noun, 'earth' (*SSR*, 200)

Meaning Change in the Context of Thomas S. Kuhn's Philosophy of chemistry in the 18<sup>th</sup> century. All in all, I do think that these passages can be read as expressions of Kuhn's intuition as a historian.

Nevertheless, Kuhn already takes steps in *SSR* towards a philosophically more principled position. The first thing to notice, which is also in accordance with Kuhn's historical viewpoint, is that he talks about "networks" (*SSR*, 45) or "relationship between concepts" (*SSR*, 149). This could just be the basic intuition of a historian who had realised that understanding Aristotle's concepts requires the mastering of quite a few of them. It is, however, something more; namely, the later Wittgenstein's influence is already clear in *SSR*. What Kuhn picks out from Wittgenstein is the idea of concept as family resemblance. Kuhn refers to Wittgenstein's question of how much we need to know in order to be able to apply terms like 'chair', 'leaf' or 'game'. He answers that it does not require knowing consciously, or even intuitively, what a chair, leaf or game is, i.e. grasping some set of attributes that these, and only these, have in common. Although some of the attributes are shared by a number of games, chairs or leaves, there is no single set of attributes that is shared by all and only these members of the class. According to Kuhn's Wittgensteinian idea, the recognition and naming of objects is possible by noticing family resemblances between objects. Different types of chairs are chairs because they resemble each other. They are, thus, similar to, and also different from, each other. Kuhn concludes that "for Wittgenstein, in short, games, and chairs, and leaves are natural families, each constituted by a network of overlapping and crisscross resemblances. The existence of such a network sufficiently accounts for our success in identifying the corresponding object or activity."(*SSR*, 45)

I think Kuhn took Wittgenstein on board because Wittgenstein's ideas caught Kuhn's historically motivated holistic intuition and promised a specification of that view.<sup>37</sup> For example, Kuhn tells us that the Wittgensteinian idea of family resemblance emerges from historical research. He says that in studying the evolution

---

<sup>37</sup> It is interesting that Feyerabend, who also advocated "contextual theory meaning", was influenced by Wittgenstein. He had written a simplified version of Wittgenstein's *Philosophical Investigations*, and he attributes his contextual theory of meaning to Wittgenstein. (Feyerabend 1981b, 74; note 68) Yet, Feyerabend did not fully utilise the family resemblance notion. He emphasises merely that the meaning of a term depends on the way in which it is incorporated into a theory.

Meaning Change in the Context of Thomas S. Kuhn's Philosophy of concepts such as 'element', 'mass', 'force', 'space', 'caloric', or 'energy' with the intention of finding out what they mean, a historian needs both to examine what is said about them, and how they are used. According to Kuhn, this leads to the discovery of a number of different criteria that govern the use of concepts and not to one unambiguous set of criteria. The "coexistence" of these standards can be understood only by studying the other scientific or extrascientific beliefs of people using these concepts. (*ET*, 259)

Another issue that further proves Kuhn's strong historical inclination at the beginning of his career is that in the 1950s, Kuhn's earliest papers were all on historical topics, while none were on philosophical themes. Later this turned upside down, and Kuhn wrote almost exclusively on philosophical or meta-historical themes. Kuhn frequently analyses the meanings of specific terms or concepts in the early articles. It is interesting to think that Kuhn was at that point concerned with the identification of historical concepts and ideas in resemblance to studies on the history of ideas and intellectual history, but Wittgenstein's influence, and also the 1970s discussion on reference, directed his interest away from historical preoccupation to these philosophical topics. Kuhn's relation to the history of ideas and intellectual history is discussed more extensively in Chapter 6.

The employment of family resemblance in the characterisation of concepts defies the classic schema of defining concepts by a set of necessary and sufficient conditions<sup>38</sup>. Another factor that comes with the family resemblance conception is the idea that concepts (laws and theories) are not learned in abstract but via their application to "some concrete range of natural phenomena" (*SSR*, 46). The idea seems to be roughly that the meaning of 'chair', for example, is learned by applying the word to different individual chairs. That way we form a concept of a stereotypical chair, i.e. an idea of the properties that a typical chair has.

---

<sup>38</sup> Interestingly, Kuhn latterly also gave credit for realising this to Quine: "his analytic-synthetic paper was coming out. ...[it] had a considerable impact on me because I was wrestling already with the problem of meaning, and at least to discover that I didn't have to be looking for necessary and sufficient conditions was extremely important". (*RSS*, 279) Yet, references, both direct and indirect, to Wittgenstein are multiple. Quine's paper probably further convinced Kuhn that the classic account was wrong.

## Meaning Change in the Context of Thomas S. Kuhn's Philosophy

Furthermore, according to Kuhn, it is not possible to learn to see the world piecemeal, or item by item, unless the conceptual categories are already in place. Such an example of piecemeal learning would be discovering a new chemical element that is in conformance with the accepted conceptual category, or catching sight of a new house. Kuhn wants to say that people organise and categorise their experience in chunks, and significant changes are reflected in the net of resemblance relations. Kuhn's example is a hypothetical case where a child transfers 'mama' from all humans to all females, and then to his mother. S/he does not only learn what 'mama' means, or who the mother is, but also what differentiates males from females and something about their behaviour towards him or her. This case is compared to Copernicans who denied that the sun was a planet. By doing this they changed the meaning of 'planet' so that it could be used to make useful distinctions between different celestial bodies. During scientific revolutions, concepts "fall into new relationships", or even "the whole conceptual web ... had to be shifted and laid down again on nature whole" (*SSR*, 149).

There is still one further potentially significant idea implied in the family resemblance notion. Kuhn claims in relation to thought experiments that "people learn their concepts and world together" (*ET*, 253). As a consequence, learning what a term means is at the same time learning something about the world. Learning what 'mama' means is to learn all sorts of things about the world; for instance, how 'mamas' behave towards a baby. And importantly, that is part of the meaning of 'mama'. Thus Kuhn assumes that there is not a principled distinction between the meaning constituting and the (non-meaning constituting) synthetic parts in our web of beliefs. In a familiar Quinean vocabulary, this is expressed metaphorically by saying that one cannot draw a line between a dictionary and an encyclopedia, or can say positively that linguistic meaning and empirical knowledge are intertwined.

Before moving on to deal with further developments in Kuhn's thinking on meaning, there remains one issue that needs to be mentioned from Kuhn's early period. In the last chapter I quoted Kuhn's words where he claims that "physical referents" of space, time or mass are different in the Newtonian and the Einsteinian paradigms. That passage has attracted a lot of attention over the years. Remember

Meaning Change in the Context of Thomas S. Kuhn's Philosophy also Bird's and Sankey's (among others) idea that Kuhn had implicitly adopted a wide descriptive theory of meaning, or a "strict" and "thick" intensionalism in Bird's terminology. According to strict and thick intensionalism, all theoretical claims belong to an intension, and a term refers only if all of them are true. Yet a mere descriptive theory of reference does not seem to explain Kuhn's claim that both of the 'masses' refer to different references, unless we suppose that Kuhn thought that there are literally two different references. Bird thinks, therefore, that Kuhn uses the word 'referent' rather sloppily. Its meaning, he suggests, is akin to intension or sense (Bird 2002, 459). Bird proposes that "thin" intensionalism might suit Kuhn better, because it requires that only some of the theoretical assumptions have to be true of an entity in order for a term to refer to it. If we find a common core of theoretical assumptions that characterise the objects of the terms as used both in the Newtonian and Einsteinian frameworks, then they would refer to the same entity in the world. (Bird 2000 174-6) There are, then, two alternative explanations. One is that Kuhn really thought that there are two references or even two worlds and a paradigm shift changes a world and/or the reference of terms. This makes Kuhn an extremist of an idealistic kind. This is an implausible interpretation in light of Kuhn's claims to the contrary, for example, that there is only world, as we saw in the last chapter. The other is that Kuhn had (implicitly) adopted some kind of descriptive theory of reference, only it was too strict and, therefore, an untenable one. This is clearly a better interpretation. Yet, it appears to presuppose that Kuhn's mindset was very similar to the advocates of the causal or causal descriptive theory, i.e. that there are singular references and the only problem is to determine how terms refer. I do not think this is how Kuhn thought.

Kuhn's use of language was indeed philosophically-speaking rather loose on occasions<sup>39</sup>. I leave the specification of my view to Chapters 5 and 6 (especially pages 187-91). It is enough to say here that the key is the understanding of the relationship between the notions of reference and extension, as well as the fact that in this passage, Kuhn was making a historical point about the transformation of beliefs.

## Meaning Change in the Context of Thomas S. Kuhn's Philosophy

I argue later that Kuhn did not approve the notion of reference as it is applied in the modern *philosophy of science*, but he did, nevertheless, have a notion of extension. And yet it is questionable whether either of these notions is applicable without qualifications to the paragraph in question. Although I think that Bird is on the right track, it may be better to express the talk of 'reference' as denotation to mental concepts of some kind, or to sets of belief without the incorporation of the notion of reference at all, and specifically, without reference to mind independent entities. Admittedly, the fact that Kuhn used words 'physical referents' poses a challenge for this interpretation and may leave the impression that it is too far removed from Kuhn's text. However, as already mentioned, it is important to understand that Kuhn's use of reference does not appear to be in accordance with the standard usage either in philosophy or ordinary language, i.e. it is hard to conceive it as a connection between a linguistic expression and an external object (see Bird 2002, 458). Furthermore, the passage in question poses a challenge to any reading and any interpretation has to strike a balance between textual faithfulness, philosophical plausibility and coherence with the rest of Kuhn's philosophy. My view is that Kuhn was here merely giving a *description* of a revolutionary episode in scientific thinking. Therefore, using the language of contemporary philosophy, the philosophical content of the passage can be expressed by saying that Kuhn's 'referent' is about hypothesised entities not existing independently of mind but as constructed by (the minds of) scientists. Finally, it is worth pointing out that this is not idealism but intellectual history.

### **Middle period – some specifications**

---

<sup>39</sup> The prime example is Margaret Masterman's study that pointed out twenty-one different ways of employing the notion of 'paradigm'. Kuhn himself noticed this in "Postscript" of *Structure*. (SSR, 174)

## Meaning Change in the Context of Thomas S. Kuhn's Philosophy

In the middle period, which covers the time since the publication of the 2<sup>nd</sup> edition of *SSR* with "Postscript" in 1970 to the end of that decade, Kuhn outlines, relying on the family resemblance idea, an alternative theory of concept to the classic one<sup>40</sup>. On several occasions, he makes an attempt to spell out how concepts are learned and also how the family resemblance account can, in the first place, successfully divide objects into conceptual categories<sup>41</sup>. During the 1970s after the first wave of harsh criticism, Kuhn clearly adopts a more philosophical stand, despite the fact that he also published a fully historical research piece, *Black-Body Theory and the Quantum Discontinuity* (1978) at the end of that decade. I contend that Kuhn does succeed in specifying his theory, although it apparently did not achieve a high level of philosophical sophistication, and did not find, therefore, many advocates later in philosophy. Despite this, it does however pave the way for further developments in meaning theorising.

In the "Postscript" in *SSR*, Kuhn returns to the family resemblance notion. The general idea is the same: objects are grouped together in similarity sets. He emphasises that a change in similarity and dissimilarity relations is a sign of scientific revolution. In such cases

Objects that were grouped in the same set before are grouped in different ones afterward and vice versa. Think of the sun, moon, Mars, and earth before and after Copernicus; of free fall, pendular, and planetary motion before and after Galileo; or of salts, alloys, and a sulphur-iron filing mix before and after Dalton. Since most objects within even the altered sets continue to be grouped together, the names of the sets are usually preserved. Nevertheless, the transfer of a subset is ordinarily part of critical change in the network of interrelations among them. (*RSS*, 173)

---

<sup>40</sup> Note that Kuhn's discussion on the Putnam-Kripke causal theory of reference is left for Chapter 5.

<sup>41</sup> Kuhn deals with the notion of family resemblance in the following papers: "Postscript" (first published 1970, *SSR*, 2<sup>nd</sup> ed.), "Logic of Discovery or Psychology of Research" (first published 1970; in *ET*), "Reflections on my Critics" (first published 1970; in *RSS*), "Second Thoughts on Paradigms" (first published 1974; in *RSS*), and "Discussion on Second Thoughts on Paradigms" (first published 1974; in *ET*). With regard to family resemblance, "Second Thoughts" is arguably the most elaborate and important text.

## Meaning Change in the Context of Thomas S. Kuhn's Philosophy

A change in similarity and dissimilarity relations is bound to cause communication difficulties, although it does not block communication completely. Conversely, during the period of normal science a group in a paradigm shares the same resemblance relations. Furthermore, Kuhn makes sharing similarity and dissimilarity relations a sign of a language community.

Kuhn is particularly preoccupied by the question of how similarity sets are learned. They are common to communities, but it is not clear how new members may acquire them. An answer could naturally be that in scientific revolutions people undergo a gestalt switch type of sudden conversion, and as a consequence of it, immediately learn to see the world differently. However, Kuhn is rather more interested in how a child who has not had any previous similarity sets, or a student who becomes a new member of a specialist scientific community, can come to possess an initially alien similarity set of the group. Kuhn compares children who first learn about dogs and cats, tables and chairs, or mothers and fathers, to learning from new instances in a scientific community. This process of learning involves a person who has already mastered the prevailing similarity sets and who points to objects telling into what category each of them falls. A person may make mistakes and is corrected by the teacher until s/he learns to use concepts correctly. The ostensive learning does not require any previous understanding of the criteria of categorization:

They are prior, that is, to a list of criteria which, joined in a symbolic generalization, would enable us to define our terms. Rather, they are parts of a language-conditioned or language-correlated way of seeing the world. Until we have acquired them, we do not see a world at all. (*RSS*, 171)

Kuhn apparently assumes that people are able to acquire the relevant category defining criteria or mechanism directly, without conscious mediation by mind from the world. This indicates that the process is unconscious and takes place pre-linguistically. Kuhn does indeed refer to “neural processing”, which can be programmed differently and which establishes the pre-condition for learning to

Meaning Change in the Context of Thomas S. Kuhn's Philosophy cluster objects. (*SSR* 197, note 14) Interestingly, Kuhn appears to be anticipating some recent developments in cognitive science. This theme is discussed in the next chapter.

Kuhn also extends his theory of learning beyond immediately ostensible objects. The usage of 'duck' can be learned in a learning process where ostension plays a major role. Kuhn thinks that after mastering the application of 'duck', it is also possible to learn to apply that of 'swimming duck' and then the expression 'there is a duck swimming' by the same means, given that a person has learnt syntax elsewhere. (Kuhn 1977, 504) It is not quite clear how this would work, but probably he regards 'swimming duck', for example, as a subkind of 'duck', which can be separated from it by its difference (not-swimming) in the same way as similarity sets are obtained. Furthermore, Kuhn thinks that learning the meaning of such terms as 'mass', 'force', and 'acceleration' is possible, in principle, the same way as learning the meaning of observable terms. One learns them by applying a symbolic law-sketch, such as ' $f=ma$ ', which can take various forms in problem situations and in this way we learn to notice the similarity between these and other problem situations. (Kuhn 1977, 504; *SSR*, 190-1)

The end result - a natural family bound together by similarity and dissimilarity relations - is thus not a definition in the mode of necessary and sufficient criteria. According to Kuhn, a person who has acquired a certain similarity set knows the meaning of a term in the sense that s/he is able to "apply these labels unequivocally and without effort". This "meaning" is clearly empirical, since by learning an application a person has learnt something about nature. (*ET*, 312) The similarity set counts, thus, as knowledge acquired of the world, which is subsequently stored in the mind. Even though a similarity set is not a definition, it creates expectations, and therefore, it is a basis for rational action. Kuhn writes that, when a person who is already acquainted with swans sees a bird that is like a swan, s/he "may reasonably presume" that it requires the same food as the other birds and breeds with them, or one can draw a "behavioral conclusion" that the sight of a swan indicates that there is water nearby. (*ET*, 285-6; 312).

However, the existence of neural processing that divides objects into groups is not yet enough for categorisation if the world itself is arbitrary. According to Kuhn, an ability to find natural families, such as ducks, swans, and geese depends on the “empty perceptual space between the families to be discriminated. If, for example, there were a perceived continuum of waterfowl ranging from geese to swans, we should be compelled to introduce a specific criterion for distinguishing them.”(SSR, 197) A natural family “is a class whose members resemble each other more closely than they resemble the members of other natural families.” (ET, 285) Kuhn believes that objects always fall into one or another family: “the entire population of the world can always be divided (though not once and for all) into perceptually discontinuous categories. In the perceptual spaces between these categories there are believed to be no objects at all.” (ET, 285) A continuum between kinds is thus not acceptable. We need to be able to tell swans apart from ducks and geese, and vice versa. It is, though, another question as to how a differentiating criterion can be established in the absence of unambiguous definition by a set of sufficient and necessary criteria. The demand that success in dividing objects into natural families presupposes the existence of an “empty space” between them may be read as implying realism, i.e. that the world itself has “joints” which scientists mirror. However, this would be surprising if we take into account Kuhn's inclination to anti-realism elsewhere in his philosophy. Consequently, I do not think that conceptual realism is behind this idea, although entity realism might be<sup>42</sup>. This question will be considered extensively in the two following chapters.

Kuhn is at pains to point out that learning to cluster objects into similarity sets does not require an answer to the question: ‘similar with respect to what?’. He writes:

it is a truism that anything is similar to, and also different from, anything else. It depends, we usually say, on the criteria. To the man who speaks of similarity or of analogy, we therefore at once pose the question: similar with respect to what. In this case, however, that is just the question that must not be asked. (ET, 307)

---

<sup>42</sup> On this, see Andersen 2000 (320-2).

By rejecting this question, Kuhn is actually raising a problem that a Wittgensteinian similarity account is faced with. It is always possible to find some similarity between an instance of one concept and that of another. Therefore, it seems that family resemblance does not limit the extension of concepts, and we are driven to a situation where there is, for example, a continuum from ducks to geese and to swans. Kuhn did not offer a clear explanation of how this problem may be avoided, but Hanne Andersen (2000) has discussed this on Kuhnian grounds and has developed a possible solution. She argues that Kuhn's employment of dissimilarity relations in addition to family resemblance succeeds in limiting the extension of concepts. This would explain the idea of empty perceptual space between natural families instead of conceptual realism, because it would give us a differentiating criterion. This needs much more explication and I will also return, therefore, to this problematic in the two next chapters.

#### **Last period – further explication**

In the "Foreword" to Hoyningen-Huene's book on Kuhn, *Reconstructing Scientific Revolutions* (1993), Kuhn once again reminds us of what his main concern has been. After noticing that *SSR* has several references to changes in word meanings, and even more, to changes of the visual gestalt, Kuhn says that meaning change is more fundamental because the central concepts of incommensurability and partial communication are based on it (Kuhn 1993, xii). Yet, in "Afterwords", written as a response to papers inspired by Kuhn's philosophy and presented in a conference held in honour of Kuhn in 1990 in MIT, Kuhn candidly recognises that his "early assessment of incommensurability was 'crude'" (*RSS*, 228). He says he had been preoccupied for much of his career with "the underpinnings of incommensurability", i.e. the question of what it is for words to have meanings and in what ways they are fitted to the world they describe. That is the reason, Kuhn declares, why he emphasised similarity/dissimilarity relations as a way to acquire concepts in professional communities. The newest innovation is that such relations form a

Meaning Change in the Context of Thomas S. Kuhn's Philosophy taxonomy, or "professional ontology", which is shared by practitioners in the field. Into a taxonomy is stored what the community knows about the world, and changes in it are central episodes in scientific revolutions. (Kuhn 1993, xii-xiii)

In the latter part of his career, from the early 1980s until the mid 1990s, Kuhn really attempted to seriously pin down his conception of meaning beyond what he had done before. Remarkably, there are no publications in this period on the history of science at all. Kuhn did attempt to systematise his earlier views, and to my mind, he succeeds in the task in the sense that some of his earlier themes are characterised more clearly.<sup>43</sup>

The family resemblance conception is carried forward from Kuhn's earlier works. It is described in a familiar way, and incorporated with his more novel ideas. Kuhn writes that if we are interested in the concept of science, for example, we do not need to, or in fact we cannot define science by a set of necessary and sufficient conditions that unambiguously tell us what field is science and what is not. This is because not all fields of science share the same features; for example, not all sciences are experimental or predictive, although many are. Some group activities are recognised as science because of their resemblance to other fields in the same cluster and also because of differences from activities in the other clusters. As both similarity and difference relations are needed in order to master the concept, learning to use the term 'science' involves one in learning the use of other terms at the same time, such as 'art', 'engineering', 'medicine', and 'philosophy'. Knowing the meaning of 'science' requires us, therefore, to know the semantic field that also contains other disciplines. More specifically, one needs to know what position 'science' has in that field, and that, moreover, is equivalent to knowing what science is. (*RSS*, 213-14)

In relation to the clusters characterised by family resemblance, Kuhn emphasises that a historian has to be sensitive to differences in how objects are categorised. For instance, one should not talk as if Greece before the time of

---

<sup>43</sup> The most important papers of Kuhn's later career regarding meaning are "Commensurability, Comparability, Communicability (first published 1983), "Rationality and Theory Choice" (first published 1983), "The Road since *Structure*" (first published 1991), and "Afterwords" (first published 1993). All are reprinted in *RSS*.

Aristotle had a division between philosophy and science. In fact, we may have to conclude that they did not have an enterprise quite classifiable as science or philosophy in the modern sense. Kuhn writes:

Finding and disseminating a vocabulary that permits description and understanding of older times, or of other cultures, is central to what historians and anthropologists do. Anthropologists who refuse the challenge are called "ethnocentric"; historians who refuse it are called "Whig". (RSS, 213)

Kuhn's objective, and one that was very apparent during his early career, of arguing against Whig-history is again exemplified here, and is connected to a new theoretical notion, 'taxonomy', in his philosophy. To take the above example of scientific disciplines, the names of disciplines label taxonomic categories. The clusters, or groupings of disciplines together, form taxonomies that are embodied in the vocabulary and applied in virtue of the associated field of characteristics. (RSS, 213-14) In a taxonomy, terms or kind terms are ordered hierarchically. To Kuhn, being a kind term is part of what the term means. Interestingly, Kuhn's conception of taxonomic terms is not limited to so-called natural kinds, since natural kinds, artificial kinds, or social kinds are all taxonomic terms. (RSS, 92; similarly RSS, 229) Something is a taxonomic term, then, if it is part of hierarchically organised taxonomies. This can be taken to mean that taxonomic term categories compose a set of classes structured in subset relations. However, an important caveat needs to be mentioned here. The subset relation between classes does not mean in Kuhn's theory that individuals in a subset necessarily inherit common features from supersets as that would be in contradiction with the family resemblance account.

Kuhn elevates discussion to a yet more theoretical level. The central theoretical notion in his later career is 'lexicon'. He asks us to imagine that each individual referring term, i.e. taxonomic term, is

a node in lexical network from which radiate labels for the criteria that he or she uses in identifying the referents of the nodal term.

Those criteria will tie some terms together and distance them from others, thus building a multidimensional structure within the lexicon. (RSS, 52)

'Criteria' should be understood here as the above described family resemblance criteria that are associated with kind terms that together form a taxonomy in which kind terms define each other mutually by their similarities and differences. Kuhn claims that meaning consists of its structural relations to other terms in the network, and taken by itself, none of them has an independently specifiable meaning. (RSS, 55)

A further terminological change is that Kuhn reinvokes his early use of 'conceptual scheme'<sup>44</sup>. "I might more appropriately speak of concepts than of words. What I have been calling a lexical taxonomy might, that is, better be called a conceptual scheme". A conceptual scheme, Kuhn understands, not as a set of beliefs but as "a particular operating mode of a mental module". A mental module is a prerequisite to having beliefs and supplies and bounds the set of beliefs that are possible. Furthermore, a mental module is taxonomic. (RSS, 94) Elsewhere, Kuhn describes his insight in the following way:

What emerges is a mental module that permits us to learn to recognize not only kinds of physical object (e.g. elements, fields, and forces), but also kinds of furniture, of government, of personality, and so on. In what follows I shall refer to it frequently as the lexicon, the module in which member of a speech community store the community's kind terms. (RSS, 229)

The mental module is thus some kind of cognitive structure in people's minds that organises and stores the community's kind terms taxonomically.

---

<sup>44</sup> The expression 'conceptual scheme' is already frequently used in *The Copernican Revolution* (1985), originally published in 1957. Anticipating his later usage, Kuhn says that they are frameworks for the organisation of knowledge, for guiding future research and helping to recall data. (Kuhn 1985, 37-41)

There may appear to be a conflict here in Kuhn's characterisation of taxonomy and lexicon. First, Kuhn was talking of taxonomy as some kind of linguistic structure, which is formed of terms, but then he starts talking about concepts and the mental modules that are mental entities. Therefore, we may ask at this point whether lexicon is a linguistic or psychological notion? In the light of some later writings it seems that the answer is the latter. But then the concept of lexicon is applicable to individuals only, and Kuhn's philosophy of science becomes solipsistic. This would be an unexpected conclusion because Kuhn had a strong interest in scientific communities and science as a group activity.

The above dilemma may be regarded as yet another inconsistency in Kuhn's philosophy. However, I think that this conclusion would be incorrect. I believe that Kuhn's primary interest as well at the beginning as at the end of his career was in scientific communities. The apparent inconsistency may be explained by noticing that 'lexicon' is indeed a psychological notion, but that there is a communal equivalent to it. Kuhn asks us to conceive lexicon "as a module within the head of an individual group member" (RSS, 104). Yet, in addition to a mental module, Kuhn also introduced the concept of 'lexical structure'. He says: "What members of a language community share is homology of lexical structure" (RSS, 52).

Kuhn argues that the mere understanding of a concept is enough to allow the deduction of predictions of properties and the behaviour of objects. These expectations are stored "knowledge" that serve as criteria for the term application. Kind terms in (an individual's) lexicon thus induce expectations of the object referred. A peculiarity of Kuhn's theory of lexical structure is that expectations do not need to be the same in a community. If expectations of an object vary, it is still enough for communication that the lexical structure is the same. (RSS, 239; 242) Alternatively, the criteria that picks out objects in the world does not need to be the same, only the taxonomic structure needs to be the same, i.e. the sets of objects. If the taxonomic structures are different, then "the world" is different, language becomes private, and there will be communication difficulties until one of the discussants acquires the language of the other. (RSS, 52) The community has

“primacy” over its members in the sense that the shared conceptual or taxonomic structure holds the community together and separates it from others. Kuhn writes:

what characterizes members of the group is possession not of identical lexicons, but of mutually congruent ones, of lexicons with the same structure. The lexical structure which characterizes a group is more abstract than, different in kind from, the individual lexicons or mental modules which embody it. And it is only that structure, not its various individual embodiments, that members of the community must share. The mechanics of taxonomizing are in this respect like its function: neither can be fully understood except as grounded within the community it serves. (*RSS*, 104; similarly Kuhn 1993, xii-i; *RSS*, 242)

Thus Kuhn elevates the social aspect of meaning, the shared lexical structure of a community, to a central position. Variation between lexicons or mental modules of individuals is allowed, as long as the lexical structure is held in common. Lexical structure functions, then, as a distinguishing feature of a scientific community, as well as a scientific revolution.

### ***Conclusion***

The proper genesis of Kuhn's philosophy is the study of history. Kuhn's experience of reading Aristotle awakened him to a recognition of the difference between the world views of ancient and modern scientists. It proved that if we want to understand the history of science, past science and its textual products have to be studied on their own conditions, not in relation to modern science. Part of this realisation is that one cannot assume that past scientists meant what we or modern scientists mean by the same terms. In order to find out the meaning of the terms used by them, one needs to pay attention to the context. This led to the statement that meanings have changed and that the meaning of a term depends on its relation to other terms. Kuhn's 'meaning', thus, stems from what he, as a practising historian, observed.

## Meaning Change in the Context of Thomas S. Kuhn's Philosophy

Kuhn was “a physicist turned historian for philosophical purposes” (*RSS*, 320-21). His shift from science to history is explained by a eureka-like-realisation with Aristotle that made him conclude that history is badly misunderstood. Throughout his career, Kuhn was preoccupied by the idea that a historian has to do justice to the past, even though he himself ceased doing historical studies in the latter part of his career. This is still evident later, for example, in his concern with the correct description of lexical structures of communities. However, his ultimate interest was to say or to correct something in the image of science as a whole. Thus his aim was philosophical. In many places, Kuhn expressed that the notion of meaning, together with the notion of incommensurability, were the most important themes in his philosophy. He thought that by understanding the role of these we can grasp something essential of the nature of science. Kuhn did, therefore, spend an increasing amount of time on philosophical considerations.

In comparison to the early theorising on meaning, Kuhn succeeded over the years in developing and specifying his characterisation of meaning. That is naturally not to say that he ever achieved an elaborate level with his conception of meaning or that he had something like a meaning theory to offer, although he may have supplied the ingredients for one. The question of whether Kuhn's notion of meaning was meant to be a theory of meaning or something else is discussed further in the following three chapters. Nevertheless, Kuhn's basic insight that meaning is holistic and sociopsychological (or occasionally psychological) is much better argued in the last part of his career. At the beginning, as Kuhn himself noted, his understanding of meaning as a philosophical notion was very limited. The first philosophical insight that he actively used was Wittgenstein's notion of family resemblance. Kuhn asserted that the meaning of words is not defined by a set of sufficient and necessary conditions, but is defined more loosely, by family resemblance. An important part of this realisation is the idea that concepts are categorised and recognised not only by similarity, but also by difference relations.

In the middle period, Kuhn took his first serious steps towards an alternative theory of meaning by trying to explicate how family resemblance could work in practice. As a consequence, he develops an account of how concepts are learned.

## Meaning Change in the Context of Thomas S. Kuhn's Philosophy

Learning takes place in a situation where a teacher, by the method of ostension, teaches the names of objects to a learner. Knowledge acquired of objects is grouped in similarity sets, which enables their future application. This all comes nicely together in the last period when Kuhn starts talking about lexicon on the individual level, and lexical structure on the communal level, that form taxonomic structures storing kind terms and their related information. Concepts or their labels, kind terms, can be metaphorically understood as nodes with radiating connections to criteria that are used to apply the concept or the term. People in one community do not need to share the application criteria or expectations of an object, but they have to share the common lexical structure. The homogeneity of lexical structure is a prerequisite for there being a community; it also separates one community from another.

Kuhn's similarities with logical empiricism should not blind one from seeing the important differences between them. In particular, one ought to understand that the genesis of their views is different. The supposition that there is a continuum between logical empiricism and Kuhn is, in my view, simply unjustified in relation to most parts of Kuhn's philosophy. Continuity between logical empiricism and Kuhn may be established best by a common interest to the themes that were current in the general philosophical climate of the time, such as the question of whether there is pure observational language and neutral observation. We need to remember that Kuhn was not well acquainted with any philosophical tradition at the beginning of his career. Regarding meaning, Kuhn took almost all his conscious influences from somewhere other than from logical empiricism. Even the most important philosophical influence on Kuhn's thinking, the later Wittgenstein, is very unlike logical positivism and empiricism.

Dialogue on the genesis of Kuhn's views and the proper context of his philosophy is certain to continue. My conclusion is that Kuhn drafted his notion of meaning on the basis of his historical experience and used the later Wittgenstein in attempts to specify it. His ideas on taxonomy, lexical structure, and mental module continued this line of reasoning. Taking everything into consideration, it is fruitful to see Kuhn as outlining something new that has only recently been understood more precisely, rather than to see him as developing something old. In the next chapter, I

Meaning Change in the Context of Thomas S. Kuhn's Philosophy  
will consider what kind of theories can be built on Kuhn's insights on meaning and  
concept.

#### 4. Knowledge representation and similarity relations

Throughout his career Kuhn argued that conceptual and empirical knowledge are intertwined and stored in the mind in some kind of organised structure. At the end, he introduced the notion of lexicon and its communal equivalent lexical structure, which consist of kind terms in a taxonomic structure. Kind term categories are classes of objects that fall under a concept.<sup>45</sup> They are, thus, Kuhn's earlier 'natural families'. Objects are categorised as kinds, not because some specific description of them all is true, but because of family resemblance criteria, according to which, instances of a kind resemble each other more than they resemble instances of other categories. Each instance fulfils some of the category features, but not necessarily all. In short, this is the "meaning theory" that emerges from Kuhn's later writings.

However, it is not clear whether Kuhn actually wanted to develop a meaning theory or not. There are indications that, despite being poorly acquainted with philosophy, Kuhn initially had a view of what meaning is. This is the impression that Kuhn's words at the end of the 1960s convey, when he claims to have become much less confident about what meaning is.<sup>46</sup> Yet, in response to Mary Hesse's comment on Kuhn's "Commensurability, Comparability, Communicability" in 1982, Kuhn says, "traditional meaning theory is bankrupt and that some sort of replacement, not purely extensional, is needed". According to Kuhn, Hesse misses the point that his "brief remarks about homologous taxonomies" are concerned "directly and literally with meaning" (*RSS*, 55). In the 1990s the uncertainty returns. Kuhn writes: "to the

---

<sup>45</sup> There is no uniform usage of such notions as category, class, and kind. Philosophers have tended to use 'category' more narrowly than cognitive scientists and psychologists, meaning some kind of ultimate class whose number is small. A case in point is Aristotle or Kant. The latter use it to mean basically any class for which there is a common name, i.e. lexical category. In the thesis I will adopt the more liberal understanding, which is also consistent with how Kuhn used it. In the adopted usage 'category', 'class', and 'kind' are used interchangeably unless indicated specifically otherwise. See Hacking (2001, 476-80) for differences and similarities in understanding these concepts in philosophy and cognitive science and psychology.

Meaning Change in the Context of Thomas S. Kuhn's Philosophy  
extent that I'm concerned with language and meanings at all ... it is with the meaning of a restricted class of terms. Roughly speaking, they are taxonomic terms or kind terms." (RSS, 92) This also seems to be Kuhn's position later. Responding to Hacking's paper in which Hacking tries to eliminate all talk of meaning from Kuhn's philosophy, Kuhn says that "with respect to kind terms, aspects of a theory of meaning remain at the heart of my position" (RSS, 229). Kuhn travels thus from confidence about meaning to uncertainty, then becomes more assured again, until he finally settles on a more limited concern with meaning.

The safest conclusion at this point is that there is no meaning theory *per se* to be found in Kuhn, but that Kuhn tried, nevertheless, to bring forward some essential aspects of meaning that he thought indispensable for an understanding of the history of science. Interestingly, Kuhn's meaning theorising has received a new momentum by some recent innovations in cognitive psychology and science. During the last and the present decade various philosophers have attempted to specify Kuhn's insights by the tools developed in these disciplines<sup>47</sup>. Furthermore, a number of philosophers have also applied cognitive science directly to the history of science.<sup>48</sup> I will examine first how cognitive science may be used to explicate Kuhn's insights. This examination is concentrated on the relationship between Kuhn's suggestion of how information is stored in the mind and on some recent proposals in a sub-discipline of cognitive science - knowledge representation. Secondly, I will study how a number of philosophers have sought to extend the domain of application of some contemporary innovations in cognitive science for the advancement of a theory of

---

<sup>46</sup> "At least I would agree if I *still* felt at all confident that I knew what a 'meaning' or a 'partial definition' was". (Kuhn 1977, 506) (Italics mine)

<sup>47</sup> Andersen, Barker and Chen 1996; Andersen and Nersessian 1998; Barker, Chen and Andersen 2003; Nersessian 2003. In separation from these, Bird (2000, 71-5; 2004c, 2005) has also raised the possibility of cognitive interpretation of some of Kuhn's ideas. His focus is, however, different. While the former see a potential application primarily to Kuhn's theory of concept and conceptual change, Bird interprets the notions of paradigm and paradigm change with the tools of cognitive science. Their theoretical orientation is also different. The former see that the frame notion, and especially Lawrence Barsalou's theory of it, is the most fruitful theoretical orientation. Bird, in turn, is interested in applying connectionism to Kuhn's philosophy. I will concentrate here on the application from the former direction but return later commenting on the relation between these two alternatives. Yet another cognitive scientific application is an explanation of reasoning practices behind problem-solving and the emergence of new conceptual structures by the following mechanisms: model- or case-based and mental modeling. (On this see, Nersessian 2003, 195-204; Nickles 2003).

<sup>48</sup> Barker 2001; Nersessian 1985, 1992, 1998; Thagard 1992, 2000.

Meaning Change in the Context of Thomas S. Kuhn's Philosophy concept on the basis of Kuhn's idea of similarity relations. Moreover, there are also earlier studies on similarity relations independent of cognitive science. Barry Barnes and Mary Hesse have tried to develop a meaning theory relying on that notion. I will, therefore, also take a look at their thoughts on the subject. At the end, I draw all aspects together in order to see what the philosophical contribution of the debate is. In this chapter I will evaluate the applicability of one such contribution in particular, i.e. Kuhn's theory of learning concepts.

### ***Kuhn and knowledge representation***

As we saw in the last chapter, Kuhn was concerned with the questions of how we acquire, structure and store "knowledge"<sup>49</sup> of the world in our minds. His theory of learning similarity relations addresses the question of how information on the world is acquired. He attempts to answer the questions of how information is structured and stored by assuming, first, that there is a conceptual scheme and, later, that there is something called a 'lexicon' or 'mental module' that stores kind terms in taxonomic trees. Kuhn's interest in figuring out how our beliefs, or sets of beliefs, are structured is something that has not been studied much in philosophy. Admittedly, there are some philosophers who have taken an interest in this issue, such as Quine with his web of beliefs or conceptual schemes, and also others who have argued strongly against any idea of organised schemes, such as Davidson with his argument against the existence of conceptual schemes. However, arguments in philosophy have generally had little to say about the specifics of that putative structure. By contrast, cognitive science has had a keen interest in this for decades. It is noteworthy that Kuhn was originally explicitly interested in cognitive psychology, of which an indication is the introduction of the notion of gestalt switch in *SSR*. However, he

---

<sup>49</sup> There is a terminological difference between philosophy on the one hand, and Kuhn and cognitive science on the other. Whereas 'knowledge' is usually taken as something like true justified belief in philosophy, both Kuhn and cognitive science use it more freely. 'Knowledge' is close to a set of beliefs in the philosopher's language. Philosophically, it could also be understood as information, for the reason that there is no commitment to truth.

Meaning Change in the Context of Thomas S. Kuhn's Philosophy failed to follow the development in cognitive psychology (or science). This is unfortunate because, had he retained his interest, it might have helped him to specify some of his intuitions.

In philosophy, a kind of tacit assumption seems to have been that beliefs are not stored in any specific structure, other than as a list of unordered beliefs or propositions in the mind. I say 'tacit' because that the case is so is not necessarily argued for, but simply assumed. For example, the assumption underlying the debate on theory choice is that people simply either confirm or abandon their beliefs from a list of propositions, depending on the evidence available. Likewise, the presumption in cognitive science until the 1960s was that propositions in the mind just form an unordered list. They were normally represented by the first order predicate calculus, the main tool of representation in philosophy until today. With just a few rules of derivation a person is, in principle, able to deduce conclusions from a set of beliefs. Belief revision would in this view proceed roughly along the following lines: We add a new belief, and with the help of derivation rules deduce certain consequences that lead to the deletion of some old beliefs, for example, in the case of a contradiction between a new and old belief, leaving the set changed. There have been naturally many, Kuhn and Imre Lakatos included, who have pointed out that scientists tend to be more stubborn and often just to stick to their old beliefs, even when confronted with contrary evidence. This is particularly the case if some of the fundamental beliefs are contradicted by evidence and there is no clear alternative belief system available for replacement. This can be taken as an indication that there is actually something beyond singular beliefs that at least partially constrain changes of beliefs. Yet, despite the debate on theory choice and rationality, not much has been said about the reasons for such odd, but seemingly historically warranted conservatism in epistemology.<sup>50</sup>

---

<sup>50</sup> Just to take one example: Scientists calculating paths of newly discovered planets did not rush to modify the Newtonian physics that had been used as the basis for calculations when they did not get the expected result. Instead, they suggested that a further planet disturbs the path of the planet, which would explain the deviation. Sometimes this strategy seems to have worked, as with Uranus whose path was disturbed by an unknown planet, Neptune. But sometimes it does not, as was the case with Mercury. The planet Vulcanus was never found in the vicinity of it. It is remarkable that not even in the latter case the validity of Newtonian physics was in doubt. (See Lakatos 1999, 68-9)

## Meaning Change in the Context of Thomas S. Kuhn's Philosophy

In the 1960s, it was realised in cognitive science that the idea of an unordered list of beliefs as a knowledge base is untenable. We can find both negative and positive evidence for this conclusion. Firstly, it was pointed out that in respect of the human mind, the view is psychologically unrealistic because it does not reflect how people process their systems of beliefs. Searching for information from a large list of unstructured sets of propositions would be laborious and time-consuming and does not accord with the ease with which information is actually retrieved. (Stevenson 1992, 29) Such a system would also run the risk of combinatorial explosion, which can be shown in computer simulations, because it does not constrain what information is used and what gets deduced in the system (see Thagard 1984, 235; 1992, 23).

Allen Collins' and Ross Quillian's research (1969) suggested that information stored in the mind has a specific structure. They set out to test the verification times of certain simple sentences and noticed that the verification of simple sentences with similar grammatical structure varies considerably. The sentence "a canary is a canary" was verified faster than the sentence "a canary is a bird", which was verified faster than "a canary is an animal". The same is true in the same order of the following sentences: "a canary is yellow", "a canary can fly", and "a canary has skin". They proposed that beliefs of animals, and beliefs generally, are stored in a semantic network composed of nodes representing concepts such as CANARY, BIRD, and ANIMAL<sup>51</sup>, and of attribute descriptions associated with each concept. Furthermore, they argued that concepts form taxonomic trees. For example, a canary and an ostrich are birds, and birds are animals, or a shark and a salmon are fishes and fishes are animals. Subkind concepts (of ANIMAL), such as BIRD, and its subkind CANARY, inherit attributes in the semantic network from higher order concepts, and have, in addition, their own species specific features. The attributes describing canaries in Collins' and Quillian's semantic network, for example, are that it can sing and it is yellow; attributes describing birds are that it can fly, it has wings, and it is feathered; and attributes describing animals are that it has skin, it can move around, it eats, and it breathes. If we now suppose that it takes more time to move further in

---

<sup>51</sup> Capitalisation of a word signifies here mental concepts.

this hierarchical structure, we can explain why confirmation of the sentence “a canary is yellow” can be carried out faster than that of “a canary has skin”. That is because, before verification of the latter sentence can be made, a person has to move in a semantic network upwards, first to the BIRD node, and then to the ANIMAL node, because having skin is a general feature of animals, which BIRD as a subconcept inherits. Yellowness is, in contrast, a specific feature of canaries; therefore, finding this information does not require much searching in the network, and for that reason an assertion that a canary is yellow can be verified quickly. In the light of such empirical information, Collins' and Quillian's semantic network seemed a psychologically plausible suggestion for information ordering.<sup>52</sup>

However, the semantic model has since faced difficulties which have prompted scientists to improve the model. Nevertheless, the fundamental assumption that information organisation is best represented in terms of multiple interconnected associations, relationships and pathways is now widely shared among cognitive scientists (e.g. Gross & McIlveen 1997, 105-6). What is important is that there is a lot of empirical evidence for this general idea, of which some is presented above. This gives, in turn, more force to Kuhn's suggestion that there is some kind of mental lexicon that organises and stores information, and which can be accessed when needed.

One problem with the semantic network model is that it does not portray accurately enough how quickly information is in some cases retrieved. It makes no distinction either between instances of a concept. Yet, some instances of a concept are, in fact, taken to be more typical than others, and are therefore verified faster. In the model there should be no difference. For example, a robin is judged to be a more typical bird than a penguin; as a consequence of this, the sentence “a robin is a bird” is verified faster than the sentence “a penguin is a bird”. In the semantic model their status should be equal since logically they are both kinds of bird. Psychologist Eleanor Rosch generalised these conclusions in her studies on instances of concepts. Rosch's research showed clearly that not all instances of a concept are as

---

<sup>52</sup> On Collins' and Quillian's semantic networks, see for instance Stevenson (1992, 30-4) or Solso (1995, 235-8).

Meaning Change in the Context of Thomas S. Kuhn's Philosophy representative. For example, the Dani tribe in New Guinea took a particular colour as the best example of the colour concept. Afterwards there have been numerous studies with other type of concepts such as tools, clothing, furniture, animals, trees, fish and birds that come to the same conclusion: some members of a category are better and more typical examples than others. Human concepts thus show graded structures varying on the goodness of the example. (Andersen et al. 1996, 352)<sup>53</sup>

When considering individual objects that fall under concepts, the semantic model is confronted with further problems. It was noticed above that some representatives of a conceptual category are judged to be more stereotypical. Paying further attention to individual objects, we notice that attributes associated with a concept of which an object is an instance may be true generally of that type of individual, but not of a particular individual of the kind. Canaries can normally sing, but there may be a canary that cannot. However, it should still presumably be counted as a canary. The problem is that this kind of deviation is not allowed in the semantic network model, because being a member of a category is an all-or-nothing affair: an instance either fulfils membership criteria, or it does not. If not, then an individual is not a member of the kind concept in question. (e.g. Stevenson 1993, 29-34)

The above criticism motivates an examination of two related questions. We need to re-consider how information is stored in the mind. We cannot simply assume that it is as logically organised as the original semantic model implied. What is needed is a more complex and nuanced way to present conceptual information that could allow some instances to be more typical than others and to also allow for a variation in features that decide the membership of kind categories. Secondly, we have to ask how concepts can be defined. There seem to be strong reasons to think that defining membership by features that apply equally and necessarily to all members is an unsatisfactory theory of concept.

Interestingly, Rosch explicitly called into question the classic account of concept definition by a set of necessary and sufficient conditions. All members of a

---

<sup>53</sup> See also Lakoff (1987, 41-2) on various experiments that were used to test and substantiate

Meaning Change in the Context of Thomas S. Kuhn's Philosophy category just do not share any set of exactly the same features. Features determining membership can be better described as a large set over which individual instances overlap, but do not coincide. Naturally, this idea was brought forward in philosophy much earlier by Wittgenstein with his notion of family resemblance<sup>54</sup>. And, Rosch does in fact refer to Wittgenstein's account of concepts. We may, therefore, think that Rosch and various other cognitive psychologists since have managed to give independent empirical support to Wittgenstein's theory of concepts<sup>55</sup>. The lesson of Rosch is that if we want a psychologically realistic notion of concepts, i.e. if we are interested in the human concepts, we should not stick to the classical account. That is because it is almost impossible to find – or at least all attempts have proved to be unsuccessful (perhaps with the exception of mathematics) - any human concept that can be characterised by a set of necessary and sufficient conditions. Kuhn's role, as we saw in the last chapter, was to apply Wittgenstein's account to the philosophy of science. It is therefore remarkable that Kuhn's insight can be supported by research in cognitive science.

Rosch's and others' studies that indicate the inadequacy of the semantic network model were also observed in artificial intelligence in cognitive science.<sup>56</sup> One of the most successful ideas for improvement was Marvin Minsky's (1980) suggestion that information is stored in frames that have an internal structure that contains a cluster of "knowledge" associated with a concept.<sup>57</sup> More precisely, a

---

prototype effects.

<sup>54</sup> I believe it is widely accepted that Wittgenstein was the first serious critic of the classical view. Yet, Hacking thinks William Whewell made the point much earlier. In addition, it is claimed that Nietzsche, John Stuart Mill, and William James also criticised the classical view before Wittgenstein. (See Hacking 2001, 480)

<sup>55</sup> Whether they also manage to give vindication, as Andersen, Barker and Chen claim (Barker et al. 2003, 218-9) for a family resemblance theory of concepts, see below.

<sup>56</sup> However, note that not every philosopher approves of the above type of philosophical interpretation of Rosch's results. According to Lakoff, Rosch denied in her later research that studies of prototypes can be used to characterise the structure of the category as it is represented in mind and that prototypes constitute mental representations, or that prototype studies even provide any alternative theory of mental representation. In particular, Lakoff considers that it is a mistake to think that Rosch's research yields a conclusion that conceptual categories are graded. (Lakoff 1987, 42-5) This may appear to contradict what Andersen, Barker and Chen say about Rosch. Yet, they all endorse Barsalou and the idea that concepts have internal structures. That makes all instances 100 per cent members of a conceptual category but, nevertheless, manages to accommodate Rosch's typicality results. (See below).

<sup>57</sup> It should, however, be mentioned that the frame concept resembles the notion of schema that was introduced into cognitive psychology in the 1930s and described how people store information of

Meaning Change in the Context of Thomas S. Kuhn's Philosophy

frame can be characterised as a collection of slots and slot fillers that describe a stereotypical item. Alternatively, we may say that a frame is composed of attributes and their values. The stipulation that a frame describes a stereotypical item means that its description does not function as a definition as in the classical account of concepts. In other words, description is not true of all instances that are in the extension of a concept. All that can be said is that, typically, such objects have features postulated in the frame, and a frame thus induces expectations of what kind of things objects are. Because the question is of stereotypes, a slot can have different values than is generally expected to be true of an instance. Expressed in computer science jargon, a slot can have different values than a default value in the frame. Let us introduce a simple example of a possible frame of DOG<sup>58</sup>:

### DOG

<b>Slot/Attribute</b>		<b>Slot filler/Value</b>
superset:		animal
number of legs:	default	four
subset:	optional values	collie, spaniel
situations:	optional values	park, house, shop...
owner's actions	optional values	walk, feed, buy ... etc.

Above we have a typical "knowledge" of dogs. First of all, dogs have normally four legs, and that is why FOUR is here a default value. It is however possible that, for instance, some injured dog has only three legs. The frame also contains information of where one can see dogs: in parks, houses, shops, and naturally in numerous other places not mentioned here. Here, all values are optional implying that there is no single stereotypical situation where one can come across a dog. Furthermore, there is also information of what an owner might do with dogs in

---

objects or events together. 'Frame' is also similar to the script concept. The difference is that the former is used to characterise information of concepts and the latter of typical events.

Meaning Change in the Context of Thomas S. Kuhn's Philosophy

various places, and similarly, there is no default value. In addition, the frame here includes SUPERSET, indicating that dogs are classified as animals and also SUBSET, saying that collies and spaniels are kinds of dogs. SUPERSET and SUBSET as attributes show how a frame can form a taxonomic structure. That is, the value ANIMAL opens a new hierarchically higher order frame that contains stereotypical information on animals. Similarly, COLLIE and SPANIEL as values of SUBSET open new frames downwards. Frames forming taxonomies mean also that a frame lower in the taxonomy inherits stereotypical features of higher order frames. If dogs have typically four legs, so also do collies, typically. Yet, note that membership determination is different from typicality. In other words, an atypical dog that does not correspond to a stereotypic dog is still a dog, i.e. a member of the dog category. On the basis of the above frame we could form a specific description, for instance, of some particular dog that we have seen, or a characterisation of a particular breed. In that case we would realise some of the optional values. If that dog is typically met in parks, then it becomes a default value. Or if its colour (attribute) is black (value), then this attribute-value combination is realised. This does not mean that that particular dog could not be met elsewhere than in the park, or that a white dog of that breed would not be possible. Naturally, we are not talking of physical possibility or impossibility, but of the functioning of the descriptive mechanism in the frame theory.

The frame model answers the criticism of the semantic network model. Because a frame is composed of features describing a typical object, it explains why, for example, a robin may be taken as a more representative sample of BIRD than a penguin, even though both are kinds of birds. A penguin with its longish body and standing posture is not what a person (at least living in Britain or in continental Europe) normally expects of birds. Because the frame model allows a variation in values, or even empty slots, it is able to take into account that individual members of a category do not necessarily fulfil the description of all features commonly true of members of a category. For the same reason, the frame model offers a presentation of the family resemblance account of concepts. For example, chairs form a natural

---

<sup>58</sup> Taken from Stevenson 1993, 36.

Meaning Change in the Context of Thomas S. Kuhn's Philosophy family bound together by family resemblance. A frame of CHAIR, a description of a typical chair, may include the following information of chairs: four legs, support for the back, and sitting as a function. Even if these are default values for CHAIR, the frame allows also a three-legged object, and an object without a support that otherwise resemble instances under the CHAIR frame, to be classified as chairs. What is important is that they are more similar to other members that belong to the CHAIR category than to members in the contrasting categories, such as TABLE, and that is enough for classification and naming.

Lawrence Barsalou (1992) offers us some further terminological explications. It is worth taking a look at his theories because, as we can soon see below, Andersen, Barker and Chen have employed Barsalou's ideas for understanding Kuhn's theory of concepts. According to Barsalou, concept means the descriptive information that people represent cognitively for a category. Barsalou argues that concept resembles intension or sense. His model of frame also includes, in addition to the attribute-value sets, structural invariants representing conceptually "relatively" invariant structure between attributes, as well as the inclusion of constraints that express dependence between the values of frame attributes.<sup>59</sup> However, I can safely ignore these sophistications as for the purpose of this thesis the main idea of frame is enough.

Barsalou's model can be located in the scale of the non-classical alternative theories of concepts. The classical view may be described by three basic assumptions: (1) the representation of a concept is a summary description of an entire class of instances that fall under it; (2) the features that represent a concept are singly necessary and jointly sufficient to define that concept; and (3) features are nested in subset relations, i.e. if a concept C is a subset of concept Y, the defining features of Y are nested in those of C. For this reason, features are sometimes referred to as

---

<sup>59</sup> One of Barsalou's examples on invariants is how the CAR frame, which has attributes DRIVER and ENGINE, are related by a structural invariant OPERATES, which expresses the conceptual understanding that the driver controls the engine's speed. An example of an attribute constraint is the dependence between SPEED and DURATION in the TRANSPORTATION frame. That is, when a form of transportation becomes faster, its duration becomes shorter over the same distances. (See Barsalou 1992, 30-40)

Meaning Change in the Context of Thomas S. Kuhn's Philosophy defining or essential. (Smith and Medin<sup>60</sup> 1981, 22-5) It may be said that there are two main rival accounts in cognitive science: the probabilistic or prototype account of concepts and the exemplar view of concepts. The former accepts the first assumption but rejects the two latter ones. The essence of such a view is that the representation of a concept is a measure of the central tendency of the properties or patterns of instances. The features describing instances are not necessary ones. Properties can take weights that reflect the probability that an instance has them. (Smith and Medin 1981, 61-2) The latter suggestion rejects all three assumptions and says that concepts are represented by their individual exemplars rather than by an abstract summary of properties. Instances are thus directly compared to an exemplar representation of a concept (Smith and Medin 1981 143-4). Barsalou seems to be in agreement with the probabilistic account in accepting the first assumption and rejecting the latter two. Yet, in contrast to the probabilistic view, his theory may not be described as a feature list model. The difference is that the feature list models take features as independent, while Barsalou's model emphasises the fact that multivalued features are integrated by structural connections.

The moral of our exploration of recent research in cognitive science is that it appears to give support to Kuhn's insights. Kuhn's early idea of conceptual schemes as "frameworks for the organisation of knowledge" (1985, 41) and his later preoccupation with lexicon, lexical structures, and mental modules that store and organise kind concepts, come strikingly close to the studies and theoretical innovations in cognitive science. Therefore, it is exciting to see how those innovations may be used for explicating Kuhn's philosophy, and also more generally, for enhancing our understanding of scientific change. As already mentioned, it seems at present that there is little doubt, either in the field of cognitive psychology or artificial intelligence, that beliefs or the informational content of belief systems are stored in some kind of interrelated structure. In the light of this, it is worth remembering what Kuhn said in 1982 of lexical structures:

---

<sup>60</sup> Smith and Medin's *Categories and Concepts* (1981) is probably the most comprehensive study on the discussion of theories of concepts in cognitive psychology.

Imagine, for a moment, that for each individual a referring term is a node in a lexical network from which radiate labels for the criteria that he or she uses in identifying the referents of the nodal term. Those criteria will tie some terms together and distance them from others, thus building a multidimensional structure within the lexicon. (*RSS*, 52)

The description sounds very much like those of semantic networks or of the frame model, if we remember their representation of information by concept-attribute description or attribute-value structure and their taxonomy building feature. Kuhn's similar interest and fruitfulness in respect of cognitive science is something that has only been recognised in the 1990s. Perhaps surprisingly, this fact makes Kuhn appear ahead of his time.

An intermediate conclusion is that the frame model gives an interesting and useful suggestion of how information is stored in the mind. In addition, it gives a medium for explicating the family resemblance account of concept. Many philosophers have, furthermore, found the notion of frame to be particularly useful for describing conceptual changes in the history of science. Paul Thagard has written generally of frames (e.g. 1984a, 1984b) and has placed the notion in the context of the history of science, even though he has not applied it in Kuhn's philosophy. In his model of conceptual revolutions (1992; see also 1990, 2000), Thagard employs the frame model, having been influenced by George Mille's WordNet, which is an electronic lexical reference system based on psycholinguistic theories of the organisation of the human lexical memory. Thagard's idea of concepts as complex structures akin to frames gives a special priority to kind and part-whole relations that establish hierarchies and express factual information in rules that may be more complex than simple slots. (Thagard 1992, 28-33) The notion of frame has been utilised even more directly by Hanne Andersen, Peter Barker and Xiang Chen in their collective articles (1996, 1998, 2003), and also by Nersessian with Andersen (2000). They have attempted to illuminate Kuhn's insight on the basis of Lawrence Barsalou's frame theory. I will now take a look at their project.

***The Andersen-Barker-Chen application of the frame model***

Andersen, Barker, and Chen<sup>61</sup> have formulated a theory that incorporates what Kuhn says about learning and defining concepts with the frame model as developed by Barsalou. Kuhn's idea was that new concepts are learned by recognising similarities and dissimilarities between objects. In practice, the process of learning starts from observing exemplars or individuals. Kuhn's example in "Second Thoughts on Paradigms" (*ET*) is Johnny-boy who learns to recognise swans, geese and ducks under the supervision of his father. This process involves firstly, ostension and naming a bird by the father; secondly, naming is tried by Johnny; and finally, correction when needed is made by the father. In this way Johnny learns to identify similarities and dissimilarities between objects. As a consequence, he forms similarity classes. Andersen et al. emphasise that objects in one class need only to resemble each other so that each object is successfully named and separated from others. Therefore, concepts corresponding to these similarity classes are family resemblance concepts.

Andersen et al. invoke Rosch's studies that proved how concepts show graded structures to support the family resemblance notion of concepts. Importantly, they take note of Rosch's conclusion that in order to take into account the fact of graded structures, concepts themselves must have a complex internal structure. This conclusion is now widely accepted among cognitive psychologists. (See Andersen et al. 1996, 353; Lakoff 1987, 45-6) This is where Barsalou's frame model comes into the picture. Namely, we may represent the complex structure of concepts by attributes and values.<sup>62</sup> Their fundamental conclusion is that the frame model, in fact,

---

<sup>61</sup> The writers have written several articles collectively. If I talk generally on their account, I will refer to them just as Andersen et al. The presentation of their view is, in general, based on their three collective articles: Andersen et al. 1996, Barker et al. 2003, and Chen et al. 1998.

<sup>62</sup> Bird (2005) thinks that connectionism offers a natural interpretation of pattern recognition, no matter whether the question is regarding the recognition of objects or problem-situations. Connectionism is indeed a potent alternative to the frame model in explaining pattern recognition. Both start with Kuhn's assumption that recognition is a result of repeated observations and applications to similar objects, which results in mental models of the recognition target. The difference is that the frame theory takes this model to be composed of the attribute-value structure,

Meaning Change in the Context of Thomas S. Kuhn's Philosophy supports the existence of graded structures. Because of flexibility, the frame model can accommodate variable representations of instances in one and the same frame. Of the instances that can be represented in the frame, the one that is represented most often in the structure becomes a prototype concept. That is, the commonly used attribute/value combination forms the prototype. Kuhn's similarity and dissimilarity relations are taken into account by the possibility of the different values that a given attribute can take. A prototype bird may be a robin with its associate values in the frame; yet, an ostrich or a penguin may be represented as atypical birds in the same frame with different value realisations. In the Andersen et al. application, generic concepts, which behave as universal concepts, are thus interpreted as prototypes. An instance of a generic concept, which is in other words an individual, is a better or worse exemplification of the prototype.<sup>63</sup> This is, again, an illustration of how graded structures can be embedded in the frame model.

Kuhn's theory of concepts, interpreted through Barsalou's frame model, also allows interpersonal variation in features that are used to pick out objects. Andersen et al. bring forward a case that shows how dissimilarity and similarity relations between duck, goose and swan can be constructed by five attributes (size, colour, beak, neck, gait), which each have two possible values. For example, person A recognises certain birds as swans by their large size and long neck. Person B, in turn, pays attention to their rounded beak and long neck, while the third person C picks them out by their white colour and rounded beak. In the same way we can form similarity sets for geese and ducks without any one shared feature by the members of that community. The crucial thing is that the extension of a specific concept is the same for all persons. (Andersen etc. 1996, 353-56) This reminds us of Kuhn's

---

making a clear separation between data structure and processing, while connectionism sees the resulting model as neural pathways of which some become stronger due to frequent use. In the current state of cognitive psychology and science there seems to be no consensus as to which theory offers a more realistic representation of information storing and other related cognitive functions. All seem to agree that we should let research in these fields decide the issue. Yet, it may be pointed out that in regard to the theory of concept, there are problems that connectionism has not answered satisfactorily so far (see Thagard 1992, 27).

<sup>63</sup> Another way to interpret the frame model is to understand a generic concept as a genuine set, in which case the relation of nodes representing individual to generic concepts is one of set membership. (See Stevenson 1992, 33) I will discuss the possible interpretations in the following chapter.

Meaning Change in the Context of Thomas S. Kuhn's Philosophy  
insistence that the structure of lexicon has to be the same in a community, but the features that are used to determine the structure need not be the same.

Another feature of Kuhn that the frame model identifies well is the idea that conceptual knowledge induces expectations. We expect birds to be able to fly because that is our prototype of birds. The knowledge stored in the frame has thus behavioural consequences. If the frame for SWAN includes the information that swans eat bread crumbs, I may well take bread crumbs with me when going to a pond. Yet, because the frame merely presents a stereotype, not a definition, there is no problem if an entirely rational expectation turns out to be false. In this case and especially if the scenario is repeated, one may be prompted to make changes in the default composition of the frame.

A further Kuhnian element that can be incorporated into Barsalou's frame model is taxonomy building. According to Andersen et al., a taxonomy is a specific structure in the conceptual field defined by a frame. Being in the same taxonomy means sharing the same frame for an upperhand concept. Differences in taxonomy reflect differences in frame and in the concept generating mechanism. Let us now look briefly at how it would work in practice. An example of Anderson et al. is the frame for the 'celestial body' of around 1700. Attributes and their possible values in parentheses are: orbit centre (star, planet, none), orbit shape (elliptic, parabolic, other including hyperbolic), distance (far, medium, near), luminance (self-luminous, reflecting), and size (large, medium, small). For example, STAR is a subkind of CELESTIAL BODY with the following specific attribute/value combinations: orbit centre/none, orbit shape/other, distance/far, size/large, and luminance/self-luminous. Similarly, MOON is a subkind of CELESTIAL BODY with the following attribute/value realisations: orbit centre/planet, orbit shape/elliptic, distance/near, size/small, and luminance/self-luminous. (Andersen et al. 1996, 357-8) This can be further subdivided into kind categories, if, for example, there are different types of moons. Although this case is not directly used by Kuhn, I believe it is in the spirit of his philosophy. In general, then, Andersen et al. project attempts to give support for the family resemblance concept and for Kuhn's employment of the notion with the help of recent cognitive science. Their belief is that the family resemblance account

Meaning Change in the Context of Thomas S. Kuhn's Philosophy is the only realistic notion psychologically, and also that it can be used to adequately represent historical concepts of science.

However, we should still spell out clearly what their concept of concept is. Barsalou's frame model appears to be merely a tool for them, albeit a very useful and important one. Let us remember that for Barsalou 'concept' means the descriptive information that people represent cognitively for a category, which comes close to what intension or sense is. His concept is clearly concept as stereotype in Putnam's sense. Now, Andersen et al. make clear that knowing concept is not equal to knowing the frame, because one may not know all the information in the frame, but yet use the concept correctly (Andersen et al. 1996, 353-4). But 'concept' does seem to be neither a collection of particular attribute/value combinations, as Barsalou's representation of information for a category might be said to be, because a concept may be "represented by many different value combinations" (Andersen etc. 1996, 355). And people using similar frames, or knowledge bases, may construct different prototypes for the same concept (Chen et al. 1998, 10-11). Furthermore, they say that in the case which showed how a person with an American cultural perspective has a prototype of 'bird' similar to a robin, and a person with a Chinese perspective has a prototype of 'bird' similar to a swan, "subjects were asked to generate the prototype of a specific concept". It is noteworthy that they do not talk of different prototypes of the same objects, but of the same concept. In other words, this "specific concept" is BIRD, but not in its intensional or stereotypic sense.

It appears that the concept of concept according to Andersen et al. is extensional in a specific sense<sup>64</sup>. A criterion determining the sameness of concepts is not a specific attribute/value combination or a stereotype, but the co-extensiveness of objects. The Andersen et al. account would not pass the famous Fregean test for a

---

<sup>64</sup> Hacking makes an interesting remark regarding the uses of 'concept' in cognitive science, analytic philosophy, and in ordinary English. Typically, philosophers talk of what falls under a concept, which is not the understanding in standard English. A philosopher's understanding is different from what Frege called an "associated idea", which is roughly what cognitive scientists try to capture. Yet, cognitive scientist Medin's concept is half-way between concept as an extensional concept and associated idea. (Hacking 2001, 478) In light of the investigation of Andersen et al., this characterisation strikes me as apt.

Meaning Change in the Context of Thomas S. Kuhn's Philosophy  
rational thinker, because their notion of concept is not, after all, intensional<sup>65</sup>. Let us suppose there is a person who knows both an American and a Chinese prototype of birds, but does not realise that they refer actually to the same objects. Perhaps s/he thinks that there are two separate winged and flying orders of creatures, which is, I think, conceivable. S/he could then rationally believe that a certain animal is a bird (in the American sense), but also that it is not a bird\* (in the Chinese sense), if the animal in question is very close to the former prototype. A Fregean would say that this is because s/he has two distinct concepts of birds<sup>66</sup>. Andersen et al. talk about the same concept with different prototypes, and for them the concept is thus the same if the same objects fall under both prototypes.

The same conclusion also seems to follow if we pay attention to what they oppose. That is, they argue against concept as defined by a set of necessary and sufficient conditions. A natural question is: What is behind that idea? Definitions by a set of sufficient and necessary conditions are a norm in philosophy. Yet, this type of definition can be used for at least two different purposes: real definition or linguistic definition. The former attempts to state what are essential properties for something to be something. The latter tries to define meaning in the sense of analyticity or synonymy. In the philosophy of science real definitions are arguably more of interest. For example, we might try to make a list of the properties of 'atom'. Something is, then, an atom if it fulfils those conditions. Talking about references, this would be a familiar descriptive theory of reference. What is essential for us now is the fact that a set of necessary and sufficient conditions is used to define properties that an entity has to have in order to belong to the extension of a concept. They "pick out" objects belonging to the class.

Andersen et al.'s notion of concept is naturally not a real definition that tries to pin down essential properties. We might say that it is a "real" definition as used by a person or persons at a certain time, i.e. what people who use concepts think objects falling under concepts are. In other words, as has been evident all the time, they talk

---

<sup>65</sup> Note that their meaning theory may be still described as intensional, because extension is determined by intensional elements. See more below.

<sup>66</sup> For a clear and illuminating modern account of Frege's theory of propositions and concepts that also gives the background to the view adopted here, see Schiffer (2000).

Meaning Change in the Context of Thomas S. Kuhn's Philosophy about human concepts. Their denial is that among human concepts any unambiguous set that defines concept can be found. What is really at stake here is criteria that determine objects falling into a class. They argue that such criteria have to be understood flexibly: Not all instances of a concept fulfil all criteria, nor do all persons use the same criteria to pick out the same instances. What unites the family resemblance account of concept with the classical account is that they both try to figure out how extension is determined. The difference is that the latter tries to find a strict definition that applies equally to all members. The former account takes this to be impossible, and is therefore focused on finding the framework in which several characterisations of instances are possible. The family resemblance account of concept allows variation in the set of properties that pick out instances. What differentiates the family resemblance account from the classical one is that, in the latter, a change in extension determining criteria is likely to cause a change in extension itself, while in the former that is not so. The essential point is that the sameness of concept in the family resemblance account is defined by the sameness of extension, not via the sameness in the extension determining mechanism or in the criteria that vary between instances of the same concept.

This brings us to familiar philosophical ground. Philosophically, it might be clearer if we say that a description associated with a term is its intension or sense, and in the Andersen et al. theory that is a specific attribute/value combination. An addition, though, is that we cannot find one such specific combination per term but that extension determining intension is a collection of such combinations that are used variably in picking out instances. Interestingly, Putnam wanted to reject the view that an intension that is an expression of a stereotype of objects determines reference. He writes that a three-legged tiger is still a tiger, even though it does not correspond to a stereotype of tigers. Putnam argued for a new kind of essentialism where reference is determined directly by linking a term to a kind that comprises objects that share the same underlying structure, whatever that turns out to be. However, it may be maintained the Andersen et al.'s account of the Kuhnian family resemblance does avoid Putnam's critique. They have shown exactly that there can be variable criteria that determine an extension of a term. A stereotype might be violated, but because of the family resemblance mechanism, extension determination

Meaning Change in the Context of Thomas S. Kuhn's Philosophy is successful. Thus, even though something like an intension picks out objects in extension, a three-legged tiger is counted as a tiger.

The Andersen et al. view also has affinities with the so-called cluster theory of reference, which is normally attributed to Wittgenstein and John Searle. According to the cluster theory, a proper name is associated with a cluster of descriptions that express its sense and determine its reference. A name refers to the object that most, but not necessarily all, of the descriptions denote. (See Searle 1997; also Devitt and Sterelny 1999, 50) The difference is naturally that Andersen et al. are not interested in proper names, but in general terms. However, the idea that descriptions loosely determine the object referred to is the same. Moreover, closer to our topic, Alexander Bird has argued that "loose intensionalism" suits Kuhn better. This means that terms have associate sets of descriptions that play a role in fixing reference and extension. According to loose intensionalism, fixing is not a matter of an entity satisfying the descriptions exactly. For example, an entity is in the extension of a term if it satisfies X per cent of the descriptions in the intension. (Bird 2000, 176-79) Andersen et al. do not talk about percentages but the underlying idea is the same. Only part of the associate description of a concept needs to be fulfilled, and there is no implication that those descriptions need to be the same for all entities.

### ***Barry Barnes's finitism and Mary Hesse's network model***

There have not been many studies on similarity relations. However, before Andersen, Barker and Chen, two other scholars worked on the notion. One is Barry Barnes, whose theory of "finitism" appears to be influential in science studies. The other is Mary Hesse, who has, in turn, influenced Barnes's thinking. I start with Barnes' finitism.

In his *T.S. Kuhn and Social Science* (1982), Barnes also invokes Kuhn's example of Johnny learning to distinguish between ducks, geese, and swans. He writes familiarly that instances of a cluster both resemble and differ from each other. No cluster is formed of identical or indistinguishable particulars. Therefore, a person

Meaning Change in the Context of Thomas S. Kuhn's Philosophy is always confronted by "an array of similarities and differences" (Barnes 1982, 28), when s/he is faced with a new putative instance of a term. Judgement that an instance belongs to one class or to another is contingent depending on the perceived similarity. The future application is guided only by the communal routine. The proper use of a term is, thus, when the usage is taken communally to be proper. Yet, past experience and the use of a concept does not determine the future usage. The usage is conventional and not predetermined by a set of rules or instructions. Sometimes it occurs that the routine is not able to help. Barnes argues that in such situations standard usage must be actively developed and extended via further social interaction. According to Barnes, it is negotiable, for instance, whether deuterium oxide is water, whether nine-carat (eight-carat, seven-carat etc.) gold can be classified as gold, and whether totemism is a religious practice. The crucial point is that concept application is revisable and open-ended. (Barnes 1982, 22-35)

Barnes calls finitism the conception of knowledge according to which proper usage is developed step-by-step involving a succession of "on-the-spot judgements". His radical claim is that nothing external determines what concepts refer to or determine the truth and falsity of verbal statements. Moreover, his overall conclusion is that knowledge is conventional, because what a term is taken to refer to, and what sentences are taken to be true, depends on contingent future judgements.<sup>67</sup> Barnes opposes what he calls "extensional semantics". According to him, in that view, the

---

<sup>67</sup> Barnes implies that Mary Hesse advocated finitism. Reference is made to chapters 8 and 12 in Hesse's *The Structure of Scientific Inference* (Barnes 1982, 30). However, I would hesitate to attribute Barnes' finitism to Hesse. First of all, finitism does not appear to be a theory for Hesse. In Chapter 8 Hesse does talk about finitism, but in reference to assigned probabilities as confirmation of prediction from hypothesis. The point is that if the basic language of the confirmation theory is infinite, then infinite hypotheses are possible. This makes it impossible to have anything but zero probability without violating the axiom that the probability of their total disjunction is 1. Hesse considers, therefore, theories for the purpose of confirmation as quantified only over finite domains. As far as I can see, Hesse's finitism is just a requirement that the domain for theories is finite. Hesse's theory is also some kind of hybrid between instrumentalism and realism. For example, Hesse is committed to the truth-values of theoretical statements, but she does not believe that their truths cumulate. Yet she believes forms of laws approximate, and entities and systems of entities are significantly similar between one theory and another after that in the history of science. Admittedly, she clearly holds the view that several systems of classification are possible and classification is open-ended. But I do not find anywhere a statement that reference and truth are entirely conventional. In fact, she explicitly says that recognition of similarities and dissimilarities is correct on at least some occasions. Although we cannot judge from evidence when it is correct, it does "commit us to some form of ontology with regard to the reference of universal terms". (Hesse 1974, 48) I fail to see how Hesse's stand could be compatible with the radical conventionality of knowledge of Barnes' finitism.

Meaning Change in the Context of Thomas S. Kuhn's Philosophy universe is divided into two parts, and an empirical concept is true of one and false of the other. The set of things of which it is true is the "extension" of the term. Barnes draws from this a conclusion that "to talk of the extensions of terms is to imply that future proper usage is determined in advance, that everything already lies within or without the extension of a term". Furthermore, unlike "extensionalism", finitism does not presume that "any such entity as an extension" is associated with the concept. The same goes for "intension" or "sense", which Barnes does not think exist. (Barnes 1982, 31-4)

The first point to evaluate in Barnes' finitism is its relation to Kuhn's philosophy. In contrast to finitism, Kuhn appears to assume that all instances belong to one or another class. Consider the following quotation:

The experience of generations has to date confirmed that all observed objects fall into one or another natural family. It has, that is, shown that the entire population of the world can always be divided (though not once and for all) into perceptually discontinuous categories. In the perceptual spaces between these categories are believed to be no objects at all (*ET*, 285).

Barnes might, though, say that the real point is whether naming is pre-determined or not. I still think that at any moment, for Kuhn, it is, although the criteria for determination may change. It is, in other words, determined insofar as the objects have the same and not unexpected features or similarity/dissimilarity relations as objects before. A person has developed some criteria in practice that decide whether an instance goes to this or that similarity set. It is, we might say, a default assumption that any object falls always into a category.

Nevertheless, Barnes is right in emphasising the open-endedness of naming. It is always possible that a new instance does not quite fit the classification scheme that we have created. But this is just a natural consequence if we let nature influence what type of kind concepts we form. As Kuhn said, "Nature cannot be forced into an arbitrary set of conceptual boxes" (*RSS*, 159). To use Barker, Chen, and Andersen's example, it is likely that the seventeenth century ornithology that divided birds into

Meaning Change in the Context of Thomas S. Kuhn's Philosophy either "water bird" or "land bird" assumed that all birds belong to one or other of the categories. It is not the case that each time a new bird is seen, we have no clue as to how to name it. Both our chosen criteria and the bird itself affect our classification. Barnes is right in saying that nothing strongly determines how a bird is named - of course not. But certainly there is something that guides and constrains our future categorising. Ornithologists in the seventeenth century classified on the basis of beak shape and foot structure. When they discovered a South American bird called "Screamer", which had one feature of each class - webbed feet like waterbirds, but pointed beak like land birds - they had to revise the system of categorising. Sundevall introduced a more complicated scheme, which accommodated the anomaly. (Barker etc. 2003, 227-8)

The problem here is how Barnes understands the concept of extension. A conventional understanding of an extension of a predicate is that the extension is all those things to which the predicate applies. An extension of 'horse' is all horses; an extension of 'red' is all red things etc. There is no implication that extension could not be changed or revised, should we change for one reason or another the extension determining criteria. Barnes also points out that this occurs constantly in the history of science. To talk about extension is in this sense simply to talk about individual things in the world that are counted as some kinds. There is nothing that prohibits us from retaining the notion of extension and still think that it is not permanently fixed.<sup>68</sup>

In fact, I do think that Barnes himself is interested in how extension is determined. He supposes that judgement on whether to include an instance into a class is contingent. This is not difficult to accept, if we think what has been said about the family resemblance features that determine classes. Another question is how to determine that an object X is a. Barnes thinks that all that a person can do is to find a clue from the past practice. In this way, Barnes links object categorising to Wittgenstein's rule following: naming is not determined by the properties of objects,

---

<sup>68</sup> However, that view is possible, as can be seen for example in Bird (2000, 162). Yet, mere employment of the notion of extension is not enough to commit to such a view, as I argue in detail in the next chapter.

Meaning Change in the Context of Thomas S. Kuhn's Philosophy but by the rule-bound behaviour of the community. The problem of rule following, inspired by Wittgenstein, is a problem of its own, and there is no need to discuss that here. Nevertheless, I am inclined to agree with Bird that it is questionable whether Kuhn actually commits himself to such a view (Bird 2000, 223). He clearly does invoke the family resemblance notion, but not the view that categorising is determined merely by community rules. Let us think about the above example of water birds and land birds in order to see how an object might be named. Why did people change their system of classification when the Screamer was found? The answer is simple: because the Screamer had neither a rounded beak nor webbed foot to make it a water bird, or a pointed beak and clawed foot to make it a land bird. In other words, they changed the system because of a bird! They clearly had specifiable criteria to compare to nature and on that basis were able to reach a rational decision. These criteria might be called intension because they determine extension. But, as we have seen, it is not a mysterious entity associated with words, but is instead an entirely natural stipulation with which people operate when including and excluding object into kind categories.

Barnes correctly focuses on how an individual is named and emphasises that it is always undetermined whether it will fit with the existing system. He is, however, wrong to think that everything depends on the conventions of the community. There are features that are associated with each natural family and used as a basis for classification. It is not conventional that a particular screamer does not have, for instance, a rounded beak. I am, nevertheless, not saying that there is nothing conventional in categorising, and that Kuhn was, for example, a realist in thinking that categorising is just "carving the nature at its joints". There is, indeed, a conventional aspect in categorising, and Kuhn clearly was not a realist in this sense. Yet, there is better explanation for conventionality. This is something that Hesse brings up and which I will study more extensively in the next chapter.

Hesse develops a "network model of theories" that relies on Wittgenstein's notion of family resemblance. It is much in the spirit of Kuhn and Andersen et al. in the sense that it essentially takes categorising to be a recognition of the similarities and dissimilarities between predicates. She talks about "a resemblance theory of universal terms", and she brings forward a useful theoretical distinction in regard to

this. The distinction of interest is universal versus individual. According to Hesse, there are two classic types of theories of universals: the absolute theory and the resemblance theory. The former asserts that P is correctly predicated of an object *a* in virtue of its objective quality of P-ness. The epistemological question: 'What is it about *a* that leads us in the absence of other information to correctly predicate P of *a*?' would be answered by saying that the quality of P-ness is directly recognised in the experience which leads to the predication of *a*. Similarly, the ontological question: 'What is it about *a* that makes it P?' would be answered by saying that it is the absolute quality of P, whether it is directly recognisable or not. In contrast, according to the resemblance theory, P of objects *a* and *b* is predicated because of a sufficient resemblance between *a* and *b*. Hesse illustrates the difference by asking why it is that we classify some *a*, for example, as 'red'. It is not because we recognise redness as such, but because we see *a*, *b* etc. (post boxes, St George's cross, roses etc.), and learn to associate the term 'red' in virtue of resemblance with each other and the difference to such objects as sky, grass and common salt. (Hesse 1974, 45-6)

In thinking about Kuhn's example of learning to recognise birds, the corresponding answers would be that we can name individual swans as swans, either because they all partake in the somehow recognisable universal property of being a swan, or because individuals resemble each other and differ from some others in some respects, and for this reason, we end up by putting them in one class. I think it is clear what Kuhn thinks the right answer should be. My view is that a plausible interpretation of Kuhn's position can be found following this line of reasoning. That is, I think that Kuhn is an anti-conceptual realist, i.e. he does not believe that universals exist as mind-independent. As a consequence, universal properties cannot be used to explain our systems of classification. Whether Kuhn is better described as a nominalist or a conceptualist is discussed in the following chapter. At this point, the important issue that Hesse made us recognise is that the family resemblance account goes against the absolute account of universals or conceptual realism. My interpretation of Barnes' stance is that a similar position would also fit him. Classification is conventional and open-ended because it is not given by the world, but created by us. Yet it does not make all facts conventional. Although it is

Meaning Change in the Context of Thomas S. Kuhn's Philosophy  
contingent that we divide birds as we do, it is not contingent that, say, certain birds resemble each other as they do. Again, it is conventional that we pay attention to those similarities and dissimilarities, and not to some others. Barnes' conventionalism of knowledge goes too far in claiming that every fact is dependent on us.

Hesse writes that her network model does not solely deal with extension because classes are not defined merely by the objects contained in them. They also involve "intensional reference", i.e. classes depend on recognitions of similarities and differences in producing the initial classification. Furthermore, intensional reference is the relation that exists between a descriptive predicate in a given language and a property of an object when the statement ascribing the predicate to the object is true. The relation between intensional reference and extension is defined by the following dependency: the same extension does not entail the same intensional reference, but the same intensional reference does entail the same extension. As Hesse herself recognises, intensional reference characterised in this way, comes close to what is more commonly known in philosophy as sense or intension. However, there is a crucial difference, writes Hesse. According to her, sense is often used to express a definition of a term, or to express a synonymous relation between two expressions. Hesse argues that sense, understood this way, is dependent on context and is trivial: whenever there is a change in the context that defines a term, sense changes. Hesse commits herself, in other words, to the wide theory-dependency of meaning discussed in Chapter 2. Taking everything into account, the upshot for Hesse is that intensional reference is not the same as sense. The former is dependent on the physical conditions under which the recognition of a property is correct. Consequently, the "meaning of P as intensional reference changes when some or all of the empirical situations to which we have conventionally learned by recognition of similarities and differences to assign the predicate P, are deliberately ascribed the predicate not-P<sup>69</sup>" (Hesse 1974, 63).

On the one hand, intensional reference is a less sensitive notion than the context-dependent sense. In other words, the latter may change while the former may

Meaning Change in the Context of Thomas S. Kuhn's Philosophy stay invariant. But Hesse's understanding of sense is holistic, and therefore as we have seen, any change in the context alters sense. In contrast, even if some of my beliefs about birds change, I may continue recognising birds as before, i.e. they have the same similarity/dissimilarity relations or intensional reference as before. Hesse's understanding of sense here does not appear to be entirely useful. It is the same holistic sense that was attacked because of the incommensurability consequences and that was thought to be avoided by the causal theory of reference. Moreover, understanding sense in that wide sense is less common than its understanding in a more limited form: a non-holistic intentional notion that determines extension or/and reference. Therefore, Hesse's intensional reference is much closer to a philosopher's sense or intension than Hesse recognises because it is that notion in Hesse's theory that defines extension.<sup>70</sup>

On the other hand, intensional reference appears to be a more sensitive notion than the Andersen et al. concept. To Hesse, intensional reference may change while extension stays invariant. For example, if there is a change in the "threshold of degrees of similarity delimiting the class [of Dutch painter]'Vermeer', ... such as the desirability of making distinctions between paintings by a master and copies by his immediate pupils"(Hesse 1974, 63), then intensional reference is changed and possibly, but not necessarily, extension is changed as well. But, crucially, to Hesse, change in intensional reference counts as meaning change. This would make persons A, B, and C above that had different but co-extensive similarity sets for swans to have different meanings, because the empirical applications of the term 'swan' are different. To Andersen et al., in turn, change in the similarity relations without a change in the extension does not count as a change in concept, i.e. as meaning change.

---

<sup>69</sup> Note that here I use a different notation from that of Hesse's.

<sup>70</sup> In another sense, the distinction between intensional reference and sense would be illuminating. All writers discussed in this chapter are primarily interested in intensional meaning as a function to extension. None of them is interested in intension as a propositional content of mind that explains a person's behaviour. The question of interest would be regarding what it means to possess a certain concept, not what is in the extension or how the extension is determined. This issue is further discussed in Chapter 6.

What may be troubling for some in Hesse's thinking is her strong emphasis on the physical conditions in determining intensional reference. She seems to limit the application of intensional reference to observable entities of which similarities and dissimilarity relations are easy to determine.<sup>71</sup> This is a question that can also be asked of Kuhn's family resemblance account. The question more specifically is, how closely is intensional reference, or Kuhn's family resemblance criteria, tied to empirical conditions or observable objects? In other words, is the family resemblance theory applicable only to empirical concepts or also to more abstract concepts?

### ***The theory of concept learning evaluated***

At the beginning of this chapter I examined Kuhn's changing position in respect of developing a theory of meaning. The appropriate conclusion is that there is no full-blooded meaning theory to be found in Kuhn's work. In any case, the question of what meaning is, or even what a meaning theory should include, is one of the most difficult and fundamental disputes in philosophy<sup>72</sup>. Therefore, I think that first and foremost Kuhn's philosophising of similarity relations should be understood as a theory of learning concepts. His theory is that concepts are learned ostensibly by recognising similarities and dissimilarities between objects. So far, I have confined my analysis to concepts that have a clear empirical application. In Chapter 3, I mentioned that Kuhn thought that the same theory is applicable in more theoretical cases in science via recognition of the similarity of problem situations. I will now briefly evaluate how widely applicable Kuhn's theory of learning concepts is.

---

<sup>71</sup> It may also be asked whether the account faces the same difficulties as that of Carnap's when he tried to define theoretical terms by their test conditions. Carnap's problem was that there will be always a new situation where a term can and will be applied, and thus there will be innumerable meanings, which is very uneconomical. However, the expression is underdetermined because it is impossible to take all possible physical conditions into account. (On this, see Carnap 1956 and Papineau 1979, Ch. 1) Most importantly, a consequence implied was the incommensurability thesis.  
<sup>72</sup> To obtain a taste of different possible orientations, see, for example, Gilbert Harman's short article "Three Levels of Meaning" (in Harman 1999). Or, alternatively, just think what, for instance, Frege, Quine and Dummett think a meaning theory should be and can be.

## Meaning Change in the Context of Thomas S. Kuhn's Philosophy

The fundamental tenet of Kuhn's theory of learning concepts is that concepts are learned in an ostensive situation by interaction between an instructor and a pupil, without the conscious specification of the application criteria for concepts. Thus they are learned in practical situations. We need to ask, firstly, what enables this kind of learning? How is a pupil able to recognise previously unknown objects and learn a concept that s/he has not possessed before? Hoyningen-Huene argues that there are three preconditions for this process. Firstly, the pupil must be able to understand the instructor's acts of reference as such, and not, for example, see them as mere physical motions. Secondly, such acts must be understood not just as reference, but as reference to something particular. Thirdly, the pupil has also to understand ascription and the exclusion of instances into similarity sets. (Hoyningen-Huene 1993, 80)

On the two former points, the best we can do is to suppose, together with such scholars as Quine (1969, 114-138), that there are fundamental innate abilities that enable the recognition of relevant objects. Therefore, people have an ability, developed over the course of evolution to understand the act of pointing and be inclined to conceptualise the world with some most basic categories, such as things, attributes, relations, as Hoyningen-Huene suggests (1993, 80). One does not need to know the concept bird, but one needs to be able to divide objects into separate physico-material entities and regard them as having properties before a person is able to learn the concepts of birds. In fact, the innateness hypothesis also appears to be empirically well-grounded. Children seem to have, for example, a born ability to perceive objects as whole units (e.g. Stevenson 1993, 198-9). Quine also extends innate ability to the third point. He says that a standard of similarity is innate (Quine 1969, 123). Kuhn's referral to "a mental module that permits us to learn to recognise not only kinds of physical object (e.g. elements, fields and forces) but also kinds of furniture, of government, of personality and so on (*RSS*, 229)", and his conviction that it is possessed by animals and that it evolved is an indication that he would have accepted the innateness hypothesis. Another possibility, suggested by Andersen, is to treat the third question as a so-called problem of wide-open texture (Andersen 2000a, 325-6). The problem for the family resemblance theory is how to limit the extension of concepts if we only compare similarities of instances. The difficulty is that there

Meaning Change in the Context of Thomas S. Kuhn's Philosophy

are innumerable possible similarities between objects. Andersen's solution is to refer to features or dissimilarity relations that exclude an instance from falling into the wrong categories, which thereby keeps the boundaries of concepts sharp. In general, I am sympathetic to Andersen's idea and will evaluate it more specifically in the following chapter. It is not, in any case, incompatible with the thesis that we have an innate ability to recognise similarities and dissimilarities between objects, but it can be seen rather as an attempt to specify the mechanism of recognition.

An important indirect endorsement to Kuhn's theory of learning concepts comes from the previously mentioned empirical studies. Rosch's and others' studies on the graded structures of concepts make the idea of learning concepts by recognising similarities quite plausible. If concepts are learned by observing the similarities and dissimilarities between objects, it is only natural that some instances are judged to be more typical than others, because objects falling under the concept are not identical. In general, I do think that there are no serious flaws in Kuhn's main theory of concept acquisition. His ideas were rather tentative, but we have seen that they accord well with some detailed studies in cognitive science. Later applications by Andersen et al. appear admittedly to be still tentative, although they do not strike one as being incorrect. Nevertheless, I think their application of the frame model succeeds better in giving a representation of concept, than of explaining what the actual mechanism of learning is. A description of an ostensive learning situation and the resulting family resemblance concept represented by frames do not appear to explain the internal reasoning practices that lead to this result. For this reason, I believe that more theoretical work is needed before the micro mechanism of learning can be said to be understood.

Another question to consider is how widely the theory of concept learning could be applied. The problem is, of course, that Kuhn presented his theory in "Second Thoughts on Paradigms" (in *ET*) with an example in which objects are middle-size material objects that can easily be ostended, i.e. birds. There are such concepts in science, usually taken from ordinary language, such as 'planet', 'star', 'satellite', 'gold' etc., but perhaps the most important concepts that appear in scientific theories are rarely such. Let's just think about terms in one of Kuhn's

Meaning Change in the Context of Thomas S. Kuhn's Philosophy  
favourite examples, in the theory of dynamics: 'mass', 'force', 'time' certainly cannot be ostended, and no contrasting concept is available as with 'swan', 'duck' and 'goose'. Kuhn also noticed the difficulty in extending his theory of concepts to certain scientific concepts. Yet he thought that

The same technique, if in a less pure form, is essential to the more abstract sciences as well. I have already argued that assimilating solutions to such problems as the inclined plane and the conical pendulum is part of learning what Newtonian physics is. Only after a number of such problems have been assimilated, can a student or a professional proceed to identify other Newtonian problems himself. The assimilation of examples is, furthermore, part of what enables him to isolate the forces, masses and constraints within a new problem and to write down a formalism suitable for its solution. (*ET*, 313).

Kuhn's suggestion was that more abstract scientific terms are learned together in problem situations. For example, the law, or law-schema,  $F=ma$  can be applied in different circumstances and it can, accordingly, take different expressions<sup>73</sup>. This gives a clue for the future so that a student can look for similar problem situations and apply concepts in those cases in a similar way.

Andersen and Nersessian have tried to put Kuhn's theory into use. They rely on Kuhn's later distinction between 'nomic' and 'normic' generalisations that he briefly introduced. The former can admit exceptions, such as the generalisation 'liquids expand when heated', while the latter do not, such as Boyle's law for gases. This difference corresponds, according to Kuhn, to the way concepts are learned. Nomic concepts are learned like his main theory of concept acquisition states, i.e. via recognising similarity relations. Normic concepts are learned, in turn, in applications of scientific laws. (*RSS*, 230-1) Andersen's and Nersessian's solution is an extension of the frame schema to represent the similarity class of problem situations for nomic concepts. For example, Nersessian has argued that 'electromagnetic field' cannot be separated from the problem situation in which it participates; furthermore, the various instances of the field concept in the theories of Faraday, Maxwell, Lorentz

Meaning Change in the Context of Thomas S. Kuhn's Philosophy and Einstein exhibit a family resemblance structure. (Andersen and Nersessian 1998, see also Nersessian 1984)

However, I think that Kuhn's extension of his theory of learning concepts to abstract terms is on much less firm ground than his initial theory that covers ordinary concrete middle-sized objects. Therefore, I think that much more research is needed to see whether this is a realistic theory of learning concepts in science. Andersen's and Nersessian's application is interesting, but they do not sufficiently prove that the learning of abstract scientific terms actually occurs by the recognition of similar problem situations. Perhaps such innovations as case- or model-based reasoning may provide supplementary explanations (e.g. Nickles 2003; Nersessian 1992, 2003). More specifically, I think Nersessian's application of the frame schema (1984) in the history of science is better described as a suggestion of how to represent historical concepts, i.e. more as an issue in knowledge representation than as a theory of learning concepts. For these reasons I remain sceptical of the idea that Kuhn's theory of learning concepts can be generalised to cover all concepts in science.

### ***Conclusion: Kuhn's three contributions***

Perhaps the most interesting outcome of this chapter is the realisation that some of Kuhn's insights can be given independent empirical support. Therefore, my verdict on the question of whether cognitive science manages to illustrate some of Kuhn's fundamental ideas is positive. We have seen that throughout his career Kuhn took an interest in what form "knowledge" is structured in our minds. Sometimes he wrote just about conceptual schemes, while at others he wrote about lexicons or mental modules that all essentially serve this same function. His idea that there is some such structure that can be explicated is corroborated by research in cognitive science. It is not necessarily clear what specific form is the best candidate, and the debate goes on in this theme, but we can say that Kuhn was clearly onto something. I think that Kuhn's stubborn preoccupation with the questions of how the information of objects

---

<sup>73</sup> On what kind of different forms there are, see *SSR*, 188-9.

Meaning Change in the Context of Thomas S. Kuhn's Philosophy is stored and organised in the mind, even if the philosophical climate was not always very amicable to these type of studies, has finally paid off. I believe, therefore, that the first main contribution of Kuhn's theorising in the context of this thesis is that he took seriously the idea that information is structured in the mind, tried to specify the shape of the structure in the way that still has relevance to cognitive science, and most importantly, applied this idea to explanations of scientific change. And, interestingly, this all has occurred in the spirit of naturalistic philosophy. There is nothing non-empirical or non-scientific in this theorising. As all practitioners admit in cognitive psychology, their views are open to correction if evidence asks us to reformulate our suggestion of the structure of representation. In considering theory change, the idea that there is some structure that stores information begins to gain more prominence later in the thesis. I mentioned earlier Kuhn and Lakatos as philosophers who have reminded us of the epistemological conservatism of scientists. The idea that knowledge has a structure that needs restructuring may be an explanation of conservatism. I will continue this debate on theory change in Chapter 7.

Kuhn had also something potentially interesting to say on at least one aspect of meaning. As already stated, Kuhn's stipulation that instances under concepts are related by their mutual similarities and dissimilarities can be understood with the traditional philosophical terminology. It is an intensional theory in the sense that it relies on the idea that people use certain psychological criteria to determine extension. In other words, Kuhn is primarily interested directly in extension and indirectly in the determination of extension. Kuhn argued that people can have different criteria for recognising classes of objects, but yet still have the same concept because there is no difference in their respective classes of objects. As Kuhn joked, no matter whether you differentiate between men and women by the features of their being dressed or naked, it is important that people make the same class distinctions (*RSS*, 50). Alternatively expressed, Kuhn was interested in the homology of lexical structure, i.e. that people pick out the same objects, even if their criteria for picking them out and expectations in regard of objects vary. This aspect of Kuhn's philosophy has gone virtually unnoticed. Talk of "wide intensionalism" that makes extension or reference change as soon as sense changes does not do

Meaning Change in the Context of Thomas S. Kuhn's Philosophy  
justice to Kuhn if his later writings are also taken into account. 'Extension' in Kuhn's philosophy is more stable than that.

However, it is not easy to find an interpretation of Kuhn's terminology that makes it consistent overall. Kuhn's ability to define and be philosophically consistent improved significantly later in his career, but I think he suffered from the problem of looseness of language throughout his career - something that Masterman pointed out forcefully on the usage of 'paradigm' in *SSR*. As a consequence, in thinking about 'concept' and 'meaning', and their relation, it is not easy to find a fixed interpretation. For example, in *SSR* Kuhn at times writes almost interchangeably of meanings and concepts. Then he says that the meaning of celestial bodies, such as 'planet' or 'earth' changed when they were placed in different similarity sets. This is similar to his later understanding of 'concept' as extensional. Nevertheless, the usage of 'concept' eventually settles at something like this. His understanding of 'meaning' also becomes rather stable after all. In one of the last texts he writes:

The expectations acquired in learning a kind term, though they may differ from individual to individual, supply the individuals who have acquired them with the meaning of the term. ... Changes in expectations about a kind term's references are therefore changes in its meaning, so that only a limited variety of expectations may be accommodated within a single speech community. So long as two community members have compatible expectations about the referents of a term they share, there will be no difficulty. (*RSS*, 231)

A bit earlier he had said that meaning change is "change in the way words and phrases attach to nature, change in the way their referents are determined." (*RSS*, 29)  
Thus, 'meaning' appears to be those expectations or what has been learned of individuals for the purpose of classifying objects into similarity classes. All in all, 'concept' is very near to extension, and 'meaning' is very near to intension or sense, remembering its family resemblance nature. For Kuhn to end up with this view is slightly surprising because it is so much the mainstream usage in philosophy. That is

Meaning Change in the Context of Thomas S. Kuhn's Philosophy where I, anyway, believe his theorising comes to at the end. Naturally, there are many other points of divergence, as we have seen and will still see in the thesis.

The second contribution of Kuhn's theorising of similarity relations is, then, his theory of extension determination, which says that extension is determined by observing similarities and dissimilarities of objects. In relation to it, there are two things that should be noticed. One is that the theory is naturally closely related to Kuhn's theory of concept learning. Secondly, it is something that Andersen et al. have called a family resemblance theory of concept, which is supposed to question the classical account of concept. Although Kuhn never put things like this, this extension of his philosophy is possible without any discernible distortion of Kuhn's thinking. The question that we need to ask is whether the case against the classical account is proven. That question has to be asked from Andersen, Barker and Chen, who make the claim.

Andersen, Barker and Chen have created an interesting implementation of Kuhn's idea of family resemblance concept by applying Barsalou's frame model. Their use of Barsalou's idea strikes me as both empirically robust and instrumental. The strongest domain of application is in concept learning and use. The fact that there is a large amount of empirical research in the background makes the model more forceful. Yet, arguably, it is not the only way we can substantiate the idea of family resemblance. Nevertheless, it does manage to catch some of the important insights of Kuhn, and I think also that their creation is fruitful, which can be seen in successful illustrations of cases in the history of science which were briefly mentioned in the thesis. But are they right in claiming that cognitive psychology has given "empirical vindication of the family resemblance account" and that empirical findings have shown that "necessary-and-sufficient condition definition of concepts will be impossible" (Barker et al. 2003, 219)? I do not think so.

As the writers themselves say, Rosch's and others' research of the graded structures of concepts describe the behaviour of people ordering exemplars in categories according to their typicality (Chen et al 1998, 11). This does not yet prove that definition in the form of necessary and sufficient conditions is impossible. Different persons may use different features to classify objects, and they may apply

Meaning Change in the Context of Thomas S. Kuhn's Philosophy concepts so that instances of their concept application do not share any set of common features, and for this reason they may judge some instances as being more typical exemplifications of the concept. But this does not mean that it is *impossible* to find a set of common features of all instances. Interestingly, Andersen and Nersessian appear to explicitly admit this. They say that the fact that concepts are not defined does not mean that they could not be (Andersen and Nersessian 2000, 227). Moreover, Smith and Medin write that so-called empirical support for the family resemblance account of concepts which goes against the classical view can be taken as a theory of how objects are actually identified. That often occurs through some perceivable properties. Yet one can, in principle, maintain that concept is defined by its core in the classical way and thus avoid direct criticism of how the actual classification of objects occurs. (Smith and Medin 1981, 20; 27-9; 57-60) Smith and Medin even construct a possible addition to the classical account that makes it compatible with the findings of Rosch (but which, however, gets into trouble for other reasons) (Smith and Medin 1981, 36-8).

We may think that Wittgenstein's famous family resemblance "theory" of concepts reflects the failure to find even one single definition of a natural concept by necessary and sufficient conditions. Yet, the past failure does nothing to make future success impossible, as we have learned from numerous studies on the problem of induction. The argument of impossibility based on a lack of progress by the classical view makes the sentence "no one has yet determined the defining features of concept X" equivalent to "there are no defining features of concept X" (see Smith and Medin 1981, 30-1). I think it is possible that somebody is able to discover satisfactory definitions in the classical fashion. Perhaps we need a little more work in order to find those features that all instances share. The fact that people actually do not have such a set in their mind and use a shared set of features in concept application does not yet make the task of defining impossible. Maybe at some point it will be possible to agree on the set of features that characterise swans, for example. Naturally, I am not saying that it is possible, as empirically, evidence does point to the contrary. All that I argue is that Andersen et al. in their reference to cognitive psychology have not proved the contrary either. Strictly speaking, they are talking about the learning and the use of concepts. Still, nothing said here makes the family resemblance account

less interesting as a theory of concept. In the case of Kuhn, who did not try to explicitly develop a theory of concept, I will treat his family resemblance account as a rival theory of extension determination. In the next chapter we will see whether it can successfully determine extension.

The third contribution I take to be Kuhn's theory of learning concepts. Although we have seen that its application is likely to be limited, Kuhn has nevertheless brought forward an interesting theory. Its domain of application is indeed with empirical concepts. And interestingly, it has, as has the idea of information structuring, received empirical support from the cognitive sciences. Yet in order to give Kuhn's theory a chance to work, a necessary pre-condition is the assumption that people have an innate ability to conceptualise the world in a certain way. Furthermore, as a theory of learning concepts, it also offers a theory of how objects are categorised. Kuhn's suggestion, which Andersen et al. endorse, is that objects are put into classes according to their respective similarities and dissimilarities. We need to recognise that this theory can apply only to some concepts. Firstly, objects that can be categorised directly by their similarity are perceivable objects, and there is a large number of concepts that have no instances with observable qualities. Thus, they cannot be constructed by their observable similarities and dissimilarities. Andersen et al. tried to extend Kuhn's theory of learning concepts to abstract concepts via their use in problem situations. Even if it made sense and reflected the actual practice in science, for which more argumentation is needed, it does not give an immediately evident theory of how classification functions with abstract concepts. Supposedly, they are not classified by the problem situations that they occur in.

Finally, what is interesting in Barnes' application is his emphasis on the openness of future categorising. This is something that is implied in Andersen et al.'s historical cases. However, I think his view of the conventionalism of knowledge is too far-fetched. The naming and categorisation of objects is not as undetermined as he suggests. Both the world and our old systems of classification constrain and direct the future usage. I do think also that we are able consciously to know what kind of criteria are used in classification; if not at the time of learning concepts, then after we

Meaning Change in the Context of Thomas S. Kuhn's Philosophy

have learned them. Criteria may be ambiguous and vary interpersonally, but nevertheless can still be within our reach. To my mind, Andersen et al.'s applications are able to show this. The problem is that Barnes feels too much aversion towards extension. We can quite neutrally take extension to be the set of objects falling under a concept and still think that extension may change, and has changed, in the course of history. Finally, Hesse's most interesting contribution was the idea that the family resemblance account goes against conceptual realism or against the idea that there are mind independent universals that determine our classes of objects. I personally think that this is roughly the position that Kuhn holds.

Our exploration of Kuhn's meaning theory is far from over. I have been able to say that Kuhn's meaning is about criteria that determine extension. I have not yet said anything of Kuhn's attitude to the causal theory of reference. Neither have I dealt with the debate with Putnam on the issue. Kuhn objected to the causal theory as a general theory of reference, but for what reason? Nor have I said much of what Kuhn thinks of the relation between similarity sets and the world, other than that he might have opposed conceptual realism. His position has to be specified in regard to this. We need to ask whether there is any limit on what kind of similarity sets we can form. Furthermore, it is important to inquire whether family resemblance can maintain the distinct boundaries between natural families. Cannot we always find similarities in some sense and conclude that practically all objects are similar in some respects? In other words, does Kuhn's family resemblance account of extension function satisfactorily? All these issues will be discussed in the following chapter.

## 5. Kuhn's conceptual anti-realism

We have seen so far that the function of similarity and dissimilarity relations is to define the extension of terms. The family resemblance account of concepts, grounded on similarity and dissimilarity relations between instances, is meant to be an alternative to the classical account of concepts that relies on the idea that all instances of a concept share the same features. Kuhn argued that by recognising similarities and dissimilarities between objects, people are able to group them in natural families or kind categories. Now we come to the crucial question in regard to that account. The question is whether there is any limit to what kind of natural families we can form. Is it, as Barnes indicates, entirely up to us, or does the world force the form of our categories? Barnes' stand would be a form of anti-realism, or more precisely, of constructivism. I mentioned earlier that Kuhn implied that our categories are not arbitrary because nature constrains our conceptual categorising. But if this is so, we have to ask what role the human mind can have in such a picture. Are we just trying, then, to mirror reality, as a scientific realist argues?

The key to revealing Kuhn's stance on the issue of realism and anti-realism is his dispute with Putnam's causal theory of reference. Kuhn was a critic of Putnam's view that has become popular in many modified forms in philosophy, as Chapter 2 showed. By studying this debate closely we will find out where Kuhn stands, and furthermore, be able to assess his point of view in relation to some other central parts of his philosophy, notably in relation to his theorising on concepts and epistemology. The first issue to be tackled is essentialism in the causal theory. This is an assumption that certain natural objects share an underlying essence that makes them members of a natural kind. Kuhn opposed such a view on the grounds that, firstly, in normal circumstances it is impossible to categorise by one feature only, and secondly, it is not possible to make a separation between essential and accidental features in the first place.

We can also discover properly metaphysical reasons for Kuhn's opposition. I argue that Kuhn was an anti-realist in respect of universals. When talking of comparison, for example, Kuhn always dealt with individuals and their sets, and never of universal properties. This conceptual anti-realism violates one of the requirements needed for a causal theory to function, i.e. that there is a constant (universal) in theory changes as a common point of reference. As an extension of this, I will show how Kuhn's theory of categorisation by similarity and dissimilarity relations departs from conceptual realism and also from nominalism. The family resemblance account has even been taken as a solution in the dispute between realism and nominalism (see Bambrough 1970). I have chosen to call Kuhn's theory conceptualism because of Kuhn's emphasis on mental concepts, which are formed by learning similarity and dissimilarity relations between instances and which order individuals and create the basic conceptual categories.

There is yet another related issue that has to be addressed in this chapter. We need to ask, given that nature does not dictate our conceptual categories, how we can have borders between kind categories, if all that we have is similarities and dissimilarities between instances? Cannot we move in all directions finding always some similarity between even very different objects? This question touches on the issue of how tenable Kuhn's theory of extension determination by family resemblance is. The answer is that the dissimilarity relations function as an excluding factor of instances from categories, and for this reason manage to keep a separation between kinds of objects.

### ***Causal theory and general terms***

Kuhn was against the causal theory of reference as a general solution to meaning change. He focused his criticism on Putnam's Twin-Earth case introduced in the second chapter of this thesis. In the current upsurge of interest in Kuhn, we find three different takes on the Kuhn-Putnam debate in three different recent monographs on Kuhn's philosophy. Bird sides with Putnam and says that Kuhn misses Putnam's

Meaning Change in the Context of Thomas S. Kuhn's Philosophy

point that the extension of 'water' on the Earth does not include anything composed of XYZ, and that is the reason why people discovering the Twin Earth would report that 'water<sub>E</sub>' and 'water<sub>TE</sub>' do not have the same meaning (Bird 2000, 183). Andersen, and Sharrock and Read, in turn, think that Kuhn introduces a worthwhile point. The latter two write that Kuhn's claim was that Putnam's scenario is impossible because it does not take into account that kinds are organised systematically in taxonomies of which components are dependent on each other. Taxonomy infects a "whole style of thinking", and Putnam's description, therefore, is flawed. Any discovery of XYZ that has the same surface properties as our water would prompt a revision in the chemical theory in a holistic fashion. (Sharrock and Read 2002, 184-8; see also Read and Sharrock 2002). Andersen's point of view is that Kuhn's disagreement reflects his rejection of realism. She describes Kuhn's position as "neither purely realist, nor sheerly constructivist, but something in-between". That "something in-between" position comes down, according to Andersen, to a Kantian distinction between the thing-in-itself and its appearance. Using terminology borrowed from Hoyningen-Huene, Andersen says that Kuhn's conceptual categories were of "the phenomenal world", not of "the world-in-itself" (Andersen 2001a, 60). Andersen tries to develop this position in her article "Reference and Resemblance" and comes to the conclusion that neither realism, nor anti-realism is correct. Instead, she suggests a position that explains referring as "carving the world at its joints", but this world is "a phenomenal world with variable joints", and that "it exists only in a historical process in which they [variable joints] are transmitted gradually from one generation to the next". Furthermore, Andersen holds that similarity relations are constitutive of the phenomenal objects. (Andersen 2001a, 50; 55).

I think, first of all, that Bird does not quite see what Kuhn was trying to say. That may be because Kuhn as an anti-scientific realist takes an opposing view to that of Bird. In fact, Bird sincerely admits in one of his articles that his defence of Putnam is from a realist point of view and excludes the Kuhnian type of criticisms that object to the realist way of framing the argument (Bird 2004a, 90). Secondly, Sharrock and Read raise an interesting issue, namely the observation that Kuhn's theory of the classification of objects is holistic. Yet, merely saying that does not

Meaning Change in the Context of Thomas S. Kuhn's Philosophy specify what Kuhn's objection to the causal theory is. We need to say something more of the relation between holism in this specific sense and the Putnam-Kripke idea of essences. Why does Kuhn's referential holism prevent us from discovering essences? Furthermore, given holism, why would it not be possible to find a match between the whole theory and the corresponding large part of the world without gradual one-to-one matching? Bird returns to the Kuhn-Putnam debate in his recent article "Kuhn on Reference and Essence" (Bird 2004a) by attempting to show that Read's and Sharrock's argument does not pose a threat to the causal theory or essentialism. I think Bird gets the better of Read and Sharrock in this dispute, not necessarily because the latter have no good point to make, but rather because they do not make it well enough. Thirdly, I agree with Andersen's main claim that at the root of the debate is Kuhn's rejection of Putnamian realism. Her reference to Kuhn's Kantianism is also well-placed. However, although I think that Andersen's view that Kuhn's categories were dynamic and historical, which new generations inherit, is interesting and worth studying, her overall position strikes me as unfortunate. The ontological status of the 'phenomenal world' is not clear at all. If they are made of similarity relations created by us, as she says, then what else is her position but that of the idealist or constructivist? As such, it would be a form of anti-realism that is not very attractive. And if we create "the world", what is there to discover in its "joints"? I think Andersen ends up with a position that is not internally coherent, and therefore, indefensible as an alternative to realism and anti-realism. Moreover, I believe the rhetoric of "carving the nature at its joints" is not helpful and makes the matter rather more confused.

In order to understand why Kuhn opposed the Putnam-Kripke causal theory, we need to reconstruct his argument against it.<sup>74</sup> Kuhn already displays philosophical sophistication in his evaluation of the causal theory. This can be seen from his comment on how traditional empiricism had tried to determine reference of proper names by an associated description of the reference. What is also apparent is that his

---

<sup>74</sup> His view is presented in "Possible Worlds in the History of Science" (in *RSS*). Note that "Dubbing and Redubbing: The Vulnerability of Rigid Designation" (Kuhn 1990) is a reduced version of the former paper. Also, "Metaphor in Science" (in *RSS*) is relevant.

Meaning Change in the Context of Thomas S. Kuhn's Philosophy  
attitude towards the causal theory is not overall negative. Kuhn thinks that by denying the descriptive determination of the reference of proper names, the causal theory has brought "a great advantage" (RSS, 198, similarly RSS, 312-3). The problems arise if the same theory is also extended to natural kind terms:

When one makes the transition from proper names to the names of natural kinds, one loses access to the career line or lifeline which, in the case of proper names, enables one to check the correctness of different applications of the same term. The individuals which constitute natural families do have lifelines, but the natural family itself does not. (RSS, 199)

Kuhn gives us an example of the case in which the causal theory works, and of the other in which it does not. A single act of ostension is, according to Kuhn, enough to fix reference of 'Richard Boyd'. If a person's memory is good, s/he will be able to recognise Richard Boyd after many years. The situation is entirely different if one is presented with the deflected needle of a galvanometer and simultaneously told that the cause of the deflection was an 'electric charge'. In this case a person needs something more than just a good memory to apply the term correctly in a thunderstorm or to the cause of the heating of an electric blanket. Kuhn concludes this case somewhat ambiguously by saying that with natural kinds "a number of acts of ostension are required". Nonetheless, his argument is that in regard to natural kind terms such as 'swan', 'goose', 'electric charge' etc., an establishment of reference requires exposure not only to "varied members of that kind but also to members of others – to individuals, that is, to which the term might otherwise have been mistakenly applied." (RSS, 200).

A few pages later in the same essay ("Metaphor in science" in RSS) Kuhn writes that "the techniques of *dubbing*" and "of *tracing lifelines*" allow us to track astronomical individuals such as the earth, moon, Mars and Venus, through episodes of theory changes. There is no change in the "lifelines" of individuals in transition from the heliocentric to the geocentric theory. 'Mars', for example, still refers to the same individual, i.e. Mars. But there was a change in natural families, for example, because 'the moon' and 'the sun' did belong to the family of 'the planets', but 'the

Meaning Change in the Context of Thomas S. Kuhn's Philosophy  
earth' did not, and so on. A consequence of the transitions is that the list of features that determine the "referents" of that term changed. (RSS, 205). Kuhn makes his case exceptionally forcefully in the last interview. Giving credit first to Kripke and Putnam for the development of a theory of reference for proper names he says, "I simply could not reconcile myself to saying, 'if heat is molecular motion, then it always was molecular motion.' " But it was the Copernican revolution that brought the fundamental realisation:

Look, you can trace the individual planets, Mars, heavenly bodies through the Copernican revolution – what you can't trace through it is 'planets'. Planets are just a different collection before and afterward. There was a sort of localized break that fitted very closely. And now it turns out that some people, to an extent that surprises me and others, simply say, "In the Ptolemaic systems planets go around the Earth and in the Copernican system they go around the sun." But that's an incoherent statement! ... I do not think it [the causal theory] works for common nouns." (RSS, 312-13)

Why is the statement incoherent? Because it includes a contradiction. 'Planet' is a set of individuals whose members are different in the Ptolemaic and Copernican astronomy and thus induce contradictory statements. No description of this kind can be applied to both sets without absurdity. We cannot say that, for example, the sun i.e. a planet in the Ptolemaic system, goes around the earth and also that it i.e. the centre of the universe in the Copernican system, goes around itself. Nor can we say that the earth goes around the sun and that it also goes around itself. We see that the same individual - being a member of the set 'planet<sub>p</sub>' and not being a member of the set 'planet<sub>c</sub>' - assumes contradictory descriptions in these contexts. In this case, we need to differentiate between the reading of 'planet' in the Ptolemaic and Copernican frameworks. Kuhn emphasises that although terms may survive in transitions, the changes in membership of the sets of items that terms are used to refer are "massive" (RSS, 85).

We may draw intermediate conclusions on the discussion of the causal theory so far. Firstly, Kuhn thought that the causal theory of reference may work with proper names, but not with common nouns or natural kind terms. Secondly, Kuhn

Meaning Change in the Context of Thomas S. Kuhn's Philosophy writes of individuals that form natural families. He does not talk of fixing reference straight to a natural kind or any other abstract or universal being. The underlying assumption is that reference fixing of kind terms has to be done via individuals falling under the kind category in question. Thirdly, he points out that the composition of kind categories changes in the course of history. Thus, kind term categories may be different collections of individuals at different times. A case in point is the Copernican revolution.

Kuhn is not alone in being sceptical about extending the application of the causal theory from proper names to kind terms. Keith Donellan (1983) remarks that the theory of natural kind terms is intimately connected with the causal theory of reference. Further, he writes that the causal theory was applied to singular terms, which leaves one wondering whether the transition to general terms works. It is neither obvious that the general terms taken from natural language, such as 'tiger', 'water', 'gold', and 'heat' are applicable as such in science, nor that terms taken from science function in ordinary language. Donellan's argument is that Putnam's and Kripke's reliance on rigid designation and identity, which seems to work so well for proper names, is not enough to give us a theory of natural kind terms, which goes strikingly well in accord with Kuhn's main line of argument. Donellan writes that there is a crucial difference between proper names and general terms. In the case of proper names there is one individual to be referred to, but in the case of general terms there is not. 'Tiger' does not designate any individual (at least normally), but a certain species. This is problematic for Putnam and Kripke, because they need to assume that general terms refer to abstract entities or universal substances. Further problems also arise here. According to Donellan, rigid designation is not able to make a difference between intuitively natural and non-natural kind terms if the reference is taken to be to such abstract entities. 'Bachelorhood' and 'water' or 'tiger' refer as rigidly<sup>75</sup>. Furthermore, rigid designation cannot guarantee the identity

---

<sup>75</sup> Donellan refers here to David Kaplan. Kaplan's point is that not all rigid designators are proper names. For example, designators like 'red' in 'Your eye is red' and 'penguin' in 'Peter is a penguin' are not normally thought to be proper names, yet they may be rigid, if regarded as designating the appropriate entities. Kaplan says that if 'red' designates the property of being red, it is probably rigid. If it designates, in turn, the class of red things, it is not rigid. Further, he regards almost all single words excluding particles as rigid designators. The upshot seems to be that if a reference is taken to a

Meaning Change in the Context of Thomas S. Kuhn's Philosophy of kinds for general terms. The extension of terms such as 'hearted thing' and 'livered thing' is the same, but we may say that they name different kinds. Thus co-referentiality, or even rigid co-referentiality, does not show the identity of kinds.

In order to obtain the same results for general terms as for proper names, Putnam and Kripke need to refer to the "important physical properties" of an instance and its relationship to instances of the same kind. But it is not at all obvious what will be chosen as important physical properties. Donellan drafts an alternative imaginary Twin-Earth story. In that story, the difference between the earths is the fact that on the Twin-Earth one of the isotopes of a particular chemical substance – gold, for example - forms almost all of that substance as it appears in nature. Donellan admits to not knowing whether this is actually the case on our Earth. If this is so, it does not matter for the sake of the story since this makes the Earth even more similar to the twin-Earth. If this is not so, the only difference is with the distribution of things, and not with the physical possibility, and no question of the reformulation of scientific theories arise. In this respect, Donellan's story is less radical than Putnam's. Donellan assumes that the Twin-Earthians identify things by their isotope numbers, not by their atomic numbers. For example, gold is not an element with the atomic number 79 but with a certain isotope number. The rare isotopes having the atomic number 79 would not "really" be gold. Donellan's conclusion from his story is two-fold. Firstly, there is "slackness" in how ordinary language terms for kinds are mapped onto the scientific classifications. Secondly, the extension of the term 'gold' on the Earth and the Twin Earth diverge, and as a consequence, the truth-values of certain sentences also vary. In general, it cannot be the case that natural kind terms in ordinary language have the same extension before and after scientific discoveries, and that there is the same mapping of those terms on to the discoveries.

John Dupré has also provided material worthy of serious consideration by all advocates of the natural kind concept. (Dupré 1993, chs. 1-3; 2002) Dupré remarks, in the same way as Donellan, that the examples of natural kinds that Putnam and Kripke offered were drawn from ordinary language whose essence was supposed to

---

universal property (as Kaplan does), it designates rigidly, no matter whether a term is putatively a natural kind or not. (Kaplan 1970, 518; note 31)

Meaning Change in the Context of Thomas S. Kuhn's Philosophy

be determined by science. Typical examples are 'water', 'gold' and 'lemon'. What we are looking for is an essential property that a thing that is a member of a natural kind must have. Putnam and Kripke's emphasis on ordinary language is unfortunate, because almost without exception ordinary language terms fail to correspond to a scientific equivalent that would form a natural kind. Dupré proves the point by giving numerous examples of biological terms, both in the vegetable and animal kingdoms. One of his examples deals with the case of American pears. People familiar with American pears can easily make a distinction between a prickly pear and cholla on the basis of leaf shape. Yet in biology they belong to the same genus, and so the property of being a prickly pear is not recognised in biology. Or to take another example: whereas gastronomically it is fairly important to separate between onions and garlic, in biological classification there is no difference. (see Dupré 1993, 26-34) Dupré's general point is not that these kind terms and distinctions do not make sense, but that they are biologically meaningless. In other words, ordinary language makes distinctions that do not have a biological significance. Moreover, in biology the basic unit of classification is the species. According to Dupré, no biologist would claim that higher order concepts, such as genera, families etc. would form a natural kind. Yet, ordinary language terms typically refer to such higher levels. Many names of trees refer to genera, while some names of birds refer to genera and some to families etc. Thus, an attempt to pick out ordinary language kinds and specify their essence by science is bound to fail, or its success will be limited at least.

Note that Kuhn writes specifically of the problem with biological species and of difficulties with determining gene structure (see *RSS*, 84; note 30). Much more importantly, we have seen that Kuhn was not alone in doubting whether the causal theory can also be extended to general terms and specifically to scientific kind terms. Kuhn's main reason for this, as well as Donellan's and Dupré's, is that, in contrast to proper names, it is difficult to find a singular point of reference with general terms. I think that those defending the causal theory relying on the notion of natural kind depreciate the practical problems that extension to general terms in science causes. Bird (2004a, 63), for example, correctly reminds Read and Sharrock that essentialism and referentialism are different theses: the former is a metaphysical thesis about

Meaning Change in the Context of Thomas S. Kuhn's Philosophy things, while the latter is a thesis about words. But this realisation should be enough to take the problems associated with attempts to make the causal theory work with scientific *terms* in general more seriously. Let us now proceed further and see what the fundamental reasons are for Kuhn's rejection of reference fixing in the mode of causal theory.

### ***Contra essentialism***

I now want to pay attention to Kuhn's attack on Putnam's Twin-Earth case<sup>76</sup> that the above Kuhn-scholars discussed. Kuhn's first point of criticism is focused on the imagined reaction to the situation where scientists find a substance like our water that has complicated chemical composition XYZ. Instead of saying that Twin-Earthians' 'water' means XYZ, people would, according to Kuhn, respond 'Back to the drawing Board! Something is badly wrong with the chemical theory'. That is because both H<sub>2</sub>O and XYZ, which is thus an abbreviation of an elaborate chemical formula, are drawn from the chemical theory which is incompatible with the existence of the latter kind of entity. Kuhn says that it would be in the light of the modern chemistry, for example, too heavy to evaporate at normal terrestrial temperatures. The discovery of XYZ would necessitate changes in the chemical lexicon, and after that, 'H<sub>2</sub>O' might not refer to what we call 'water'.

I think Kuhn's claim is reasonable. It would be totally surprising if scientists find a substance which is an unknown combination of familiar chemical elements and molecules, but which produces the same surface properties as the substance made of H<sub>2</sub>O. Yet, I do not think this alone threatens Putnam's argument. What it primarily shows is that Putnam's story is implausible as an example (see Bird 2000, 182-3). Scientists might well report back home in an even more Kuhnian fashion that 'What they call 'water' is a substance with the chemical composition of XYZ. It has the same superficial properties as water. But how on earth is that possible? Back to

---

<sup>76</sup> See "Possible Worlds in the History of Science" (RSS, 58-90).

Meaning Change in the Context of Thomas S. Kuhn's Philosophy  
the drawing board! Something is wrong with the chemical theory.' It may well happen that the investigation would produce the result, as Kuhn says, that 'water' cannot be said to be 'H<sub>2</sub>O'. Maybe scientists have not, after all, discovered the essence of water. Perhaps the substances that are known as 'H<sub>2</sub>O' and 'XYZ' are only two of several isotopes of a more fundamental chemical combination, which would then determine what 'water' is. But this conclusion is compatible with Putnam's claim, which is that the essence of a kind determines the meaning of a kind term.

A problem is, of course, that one may continue this fictive story appealing to intuitions, practically as long as one wants without necessarily persuading the opponent, because the intuitions themselves do not coincide. This becomes very apparent from Donellan's (above) and Dudley Shapere's papers (below) in which they manage to turn the Putnam-Kripke example against itself. But if Putnam's point is that the essence of a natural kind defines meaning, I do not think that Kuhn's argument above does anything to prove that point wrong. Kuhn's insistence on the need to restructure a lexicon may be read merely as a reminder that the established usage of terminology forbids some descriptions without changes in our system of beliefs. Before calling a spiritual healer who succeeds in curing cancer a medical doctor, we would need both to make changes in our medical theory and our definition of a doctor.

However, there is admittedly a deeper issue stemming from the lexical dependency of scientific terms, as Sharrock and Read pointed out. This has to do with Kuhn's theory of how terms are needed to form contrasting relationships with each other in order to successfully pick out objects. This theory is already familiar to us from the previous chapters. But we have not yet seen why Kuhn opposed essentialism, other than through his implication that another type of theory is needed to explain why certain objects are given a name of the kind. The question that needs to be asked is: Why could we not use one essential property to determine what water is, for example? Let us now move on to Kuhn's second point on Putnam's Twin-Earth, which turns out to be more important.

Firstly, Kuhn draws attention to the fact that water is neither identical with  $H_2O$ , nor is  $H_2O$  the reference of 'water'. He says that  $H_2O$  can be in three states of aggregation: solid, liquid and gaseous. Consequently, ' $H_2O$ ' picks out not only water, but also ice and steam. Kuhn writes that around 1750, which is Putnam's choice of year with regard to the development of chemistry, 'water' referred to only a liquid substance. This fact is historically important, because states of aggregation established a major difference between chemical substances. Water was thought to be an elementary body that was essentially liquid. For some it even referred to a generic liquid. Chemistry needed to go through a scientific revolution at the turn of the 18<sup>th</sup> century in order to allow the possibility that a certain chemical substance could be in three different states of aggregation.<sup>77</sup> This revolution also brought the discovery that water is actually composed of two gaseous substances, hydrogen and oxygen.

However, Kuhn's fundamental point is not that it is impossible for historical subjects to imagine that a substance can be in three different states of aggregation. The crucial claim is that the reference of 'water' cannot be  $H_2O$  because the latter covers a much wider domain than we want, i.e. liquid, ice and steam. According to Kuhn, a more proper description would be that 'water' refers to liquid  $H_2O$  or close-packed  $H_2O$  particles in relative rapid motion. By this description we can obtain a rough equivalence between the reference of 'water' in 1750 and now<sup>78</sup>. Yet this may

---

<sup>77</sup> Note that Bird (2004a, 85-6) disputes this historical description, claiming that phase transitions were widely regarded as changes in one and the same species long before Lavoisier. However, Bird's counter that refers only to Aristotle and Locke does not quite convince me. I think that the relevant context here is chemistry before Lavoisier, and especially the French chemical community that formed the background for Lavoisier's innovation. The French Stahlans were in the dominant position at the time, and they at least took water to be a generic elementary substance, i.e. a substance that causes certain "liquid" observable qualities in bodies. For them, a "solid water" would be an entirely different thing than a liquid one. A potential cause of confusion is the fact that 'water' can be taken either as our ordinary observable water or as a metaphysical elementary water, and their relation in the 18<sup>th</sup> century chemical thinking is not clear.

<sup>78</sup> Bird argues that this concedes to realism because it is an admission that modern science is capable of picking out "the same stuff" as people did in 1750. If 'stuff' is understood as all that exists in lakes and rivers; rains from the sky and comes from showers; is sold in bottles etc., as Kuhn seems to indicate, there is undoubtedly no problem with the equivalency, but that does not yet concede to realism of at least the essentialist kind that is often associated with the causal theory. In this case, the question is of the equivalency of samples, i.e. extension, which is determined by two different but co-extensive descriptions. The real problem is to fix the reference to an essence, and that is what Kuhn denies can be done. "Stuff" needs to be understood as a universal or abstract substance and the determination of reference to that single property has to be possible. Kuhn had reasons to object to

## Meaning Change in the Context of Thomas S. Kuhn's Philosophy

sound like stating a triviality: of course H<sub>2</sub>O covers also ice and steam in addition to water, but not vice versa. However, this trivial observation has a far-reaching consequence. Before fleshing it out, it is worth noting that Kuhn is not interested in tinkering with the itself reasonable point that, except under special laboratory conditions, we can ever actually have liquid that is totally pure H<sub>2</sub>O. The point underlying Kuhn's critical claim is: "But this modern description leads to a new network of difficulties, difficulties that may ultimately threaten the concept of natural kinds and that meanwhile must bar the automatic application of causal theory to them" (*RSS*, 82). Problems arise if we need more than one property to describe a natural kind. Kuhn writes that if only one single essential property is required by each natural kind, then the causal theory has promise because we may be able to establish the identity relation between a name and its reference via that property. This reminds us of the case with proper names. But when two properties are needed, such as liquidity and H<sub>2</sub>O denoted by their names, then each name of a property denotes a larger class than they do conjoined. Naming properties that limit the class becomes crucial:

For if two properties are required, why not three or four? Are we not back to the standard set of problems that causal theory was intended to resolve: which properties are essential, which accidental; which properties belong to a kind by definition, which are only contingent? Has the transition to a developed scientific vocabulary really helped at all? I think it has not. (*RSS*, 82-3)

---

these postulations, as I argue below. It is still another question that the description is constructed from a modern perspective with modern terminology, e.g. 'H<sub>2</sub>O' and 'close-packed particles'. It is always possible to describe the past so that it shows a natural development to the present, both in and out of the history of science. (For instance, the time that Finland was a grand duchy of Russia, 1808-1917, can be seen, as it indeed has been sometimes seen, merely as preparation for independence). But that type of description is naturally not a genuine historical explanation because it does not give, for example, an account of the use of the term 'water' in 1750, which is surely odd from our perspective. It does not describe either how actual people determined their extension or reference. In regard to the theory of reference or extension, Kuhn is interested in a historically explanatory theory, and so a mere referral to the existence of essence is not such without permanent reduction to Whig-history that glorifies our present state of being and knowing. For more on this, see Chapter 7.

What Kuhn really wants to say is that except perhaps in rare circumstances, we need to identify more than one property that an instance in a class must have in order to be a member of it. Kuhn thinks, as we have seen, that categories are formed by recognizing similarities and dissimilarities between instances. Samples of 'water' may be thought to be both H<sub>2</sub>O and liquid. Liquidity is needed to separate it, for example, from steam and ice. The most crucial further point, I believe, is that what properties we choose is not given or self-evident. We need to make some selection, i.e. pay attention to some features and ignore others. In other words, it is not given which properties should be regarded as essential and which as accidental. There is no "theory-independent way of distinguishing *fundamental* or important similarities from those that are *superficial* or *unimportant*". (RSS, 205) Kuhn writes further:

The so-called superficial properties are no less necessary than their apparently essential successors. To say that water is liquid H<sub>2</sub>O is to locate it within an elaborate lexical and theoretical system. ... If water is liquid H<sub>2</sub>O, then these properties are necessary to it. If they were not realized in practice that would be a reason to doubt that water really was H<sub>2</sub>O. (RSS, 83)

Kuhn is talking of necessity for categorising and usage in a certain theoretical system, not of physical necessity. In other words, so-called superficial properties have a necessary role to play in separating instances of kinds from each other and assigning expectations to substances.

Furthermore, Kuhn says that new theories raise difficult questions of discrimination between objects, and superficial qualities are called upon. For example, one may ask whether deuterium is really hydrogen, or if viruses are alive. We are asking here how to draw "boundary lines that delimit referents of 'water, 'living things' etc." And this questions the notion of natural kind, because the notion relies on the premise that an essence of an individual of a kind can be determined. To Kuhn, defining a kind normally requires taking several chosen properties into account. Kuhn reminds us of a case of a biological species that reveals a special difficulty. That is, even individuals who unproblematically belong to a species do not have the same set of genes. Do we not need also "superficial criteria", such as the

Meaning Change in the Context of Thomas S. Kuhn's Philosophy  
ability to breed, to make clear criteria that set boundaries between specimens? (*RSS*,  
84)

However, Bird makes a reasonable remark that we may need not claim as much as Kuhn implies. That is, we do not need to draw the conclusion from Putnam's story that water and H<sub>2</sub>O are identical. The necessary moral of Putnam's story is only that "in all possible worlds water consists (largely) of H<sub>2</sub>O; nothing compels us to think the reverse, that every occurrence of H<sub>2</sub>O is an instance of water" (Bird 2000, 183). Bird is right. We need only think "one way", i.e. from 'water' to the world. And from our modern perspective, we would say that all samples of water (ignoring impurities) are made of H<sub>2</sub>O. This is true, but is it enough to fix the extension of 'water'? Clearly that information is not helpful when thinking about the original naming ceremony. It might be helpful if we have already succeeded in forming the category 'water'. But the problem is how to make that category? It seems that in doing this we need to specify some further qualities, such as liquidity. Moreover, crucially, we cannot later give up those properties as classification criteria, or at least not without replacing it by a description with the equivalent function in a lexical network. This can be seen if you consider the following situation. We may say that 'water consists of H<sub>2</sub>O', but also that 'steam consists of H<sub>2</sub>O', and further that 'ice consists of H<sub>2</sub>O'. If this is all the information we have, we are left to wonder what on earth the difference between these is. Why do we have three terms instead of one? And so we are back to the situation of trying to separate and specify them further. This point becomes all the more potent when we remember, as already stated, that the causal theory is a thesis of words, not a metaphysical thesis of essences. 'Water', 'steam' and 'ice' are all ordinary words, and the difference of their use requires an explanation, even if they have a shared essence. Of course we can stretch the boundaries of language, but to me it seems rather difficult to deny that the use of these terms are inherently related to a distinction between liquid, gaseous and solid substances. We need to make a difference by naming some properties that the samples in each category have. We may use such properties as liquid, gaseous and solid in this task; perhaps we may also use some others, such as boiling and freezing points, optical wavelengths and so on. The fundamental point is that it is always we people, in and outside of science,

Meaning Change in the Context of Thomas S. Kuhn's Philosophy  
who decide criteria that are used to put objects in categories, and we almost always need to use more than one property to establish an appropriate difference to other categories: "categories are interdefined" (*RSS*, 30).

The question is not that something is water only if it has the surface properties of water, or that water can be defined in terms of surface properties alone – these being the claims that Bird attributes to Read and Sharrock (Bird 2004a, 77-8). Bird says, moreover, that that is exactly the idea that Putnam's and Kripke's causal theory tried to undermine. Firstly, Kuhn did not talk of definitions in the classical mode, i.e. definitions by necessary and sufficient conditions ("if and only if"), but his definitions, if they can be called such, were the family resemblance type. The crucial function of the family resemblance definition is the separation of instances of different kinds, not the postulation of the set with exactly certain features. More importantly, Kuhn's assertion is that there is no self-evident distinction between essential and accidental properties; that is why they are all needed. Bird sees also that Kuhn was leaning towards the causal theory at one point, but then made "the wrong turning" towards an emphasis on incommensurability (Bird 2004a). However, I do not detect a change of mind in this respect. We have seen that Kuhn appreciated the application of the causal theory to proper names but not to general terms. This is a position that stayed constant. In the context that prompted the above interpretation by Bird, Kuhn says, "In the process I attack the often implicit assumption that anyone who knows how to use a basic term correctly has access, conscious or unconscious, to a set of criteria which define that term or provide necessary and sufficient conditions governing its application" (Kuhn 1977b, 302; note 11). Kuhn also talks of "direct attachment" to nature. That may sound as if he were giving support to the causal theory, but I do not think this is the case. It is true that he thought that the mind is directly, without mediating description, connected to nature. That is why he wrote of the mental module that automatically does the organising of received information. Yet, his target of criticism is the concept as defined by necessary and sufficient conditions that picks out instances by fulfillment of those conditions. According to Kuhn, the application of concepts relies rather on the ability to recognise similarities and dissimilarities between objects. Although the idea of direct connection is similar to what the causal theory implies, Kuhn's thinking hardly lends

Meaning Change in the Context of Thomas S. Kuhn's Philosophy support to the causal theory as a general theory of reference determination simply because Kuhn denied that there are natural kinds in the sense that the causal theory requires. The connection between a term and objects is established by similarity and dissimilarity observations.<sup>79</sup>

Kuhn's claim that there is no obvious way to decide between essential and accidental properties can be also supported by Shapere's argumentation. First of all, Shapere makes an insightful remark that the notion of essential is unpleasantly obscure. There are properties that a substance may possess, which are, nonetheless, not counted as essential. More importantly, an essential property does not appear to be necessarily a fundamental one. For example, being an element of atomic number 79 is not a fundamental property of gold, at least in terms of quantum mechanics. (Shapere 1982, 4-5) Shapere turns Putnam's and Kripke's science fiction stories against their conclusions. According to Shapere, it would be unscientific if a scientist takes a substance to be essentially something because that prevents any changes, even if a change were reasonable on empirical grounds. He says that if there is a substance that is in all other respects like gold, except for the fact that it is not an element of atomic number 79, scientists would rather ask "Why is gold in this region not an element of atomic number 79?", than exclude that item out of hand as being gold. The same applies to Putnam's water example. It would be an unreasonable apriorism to exclude anything that does not have a chosen essence. Shapere thinks like Kuhn:

it is not just one property or set of properties – the "essential" ones – that determines or affects how scientists will apply terms in new situations; all the (true) properties may ... play a role, and furthermore, the properties and behaviour of other entities (substances etc.) may also play a role. (Shapere 1982, 7)

---

<sup>79</sup> The fact that extension is determined also by contrasts, i.e. by differences from other objects, in addition to similarities of objects in the same category, places Kuhn interestingly somewhere between the English analytic and the continental tradition. Although extension in Kuhn's theory is determined by objects, unlike Saussure's structuralism where the important relationship is the one between symbols, Kuhn in analogy to Saussure and other structuralists emphasises the importance of differential relations to other objects in the system.

What is more, Shapere holds that a reasonable attitude in science is to try to find relationships and differences between substances, use them as bases for classification, shape the vocabulary in the light of these relationships and differences, and refuse to make any kind of *a priori* commitment to a philosophical view (Shapere 1982, 10).

One more issue still needs to be mentioned here. In the second chapter I briefly introduced the so-called *qua problem*. This is the problem that ostension is not enough to fix reference because we need also to specify what exactly is referred to. The connection of the *qua*-problem to Kuhn's idea of kind determination is clear. Kuhn's point was that, in order to specify a kind, we need to pick out several properties that determine that kind<sup>80</sup>. Sterelny points out that an object can be a member of many kinds, and in ostension we should somehow specify what kind is ostended. Sterelny asks us to suppose that we go to Mars and come across a catlike animal. We introduce a term 'schmat', and so schmats are animals that bear a certain relationship to this individual schmat. But we still do not know what that relationship is. There can be many possible relationships, because schmat may be a member of many kinds: physical object, animate object, animate object of a certain biochemical kind, animate object with certain structural properties, schmats, schmats of a certain sex, schmats of certain maturational state etc. Ostension does not specify the kind in question. Sterelny mentions, and simultaneously rejects, the possibility that we might appeal to descriptive vocabulary or to the "schmat theory". That is because the descriptive vocabulary is made of other kinds, and we have the same problem of specifying extension with them. Sterelny adds that calling this holism (rather than circularity) is unhelpful. (Sterelny 1983, 120-1; see also Sankey 1997, 8; 2000, 133-4).

Nevertheless, Kuhn chose the descriptive way. He would have given a description of 'schmat' by some properties that form a similarity class that is in contrast to other kinds, perhaps 'sdogs' and 'stigers', for example. Sterelny goes the other way and defends the causal theory, and together with so many other

---

<sup>80</sup> Note, as should be clear from the previous chapter, that the bundle of properties is not identical to a kind. A set of things may be identified by several different "bundles".

Meaning Change in the Context of Thomas S. Kuhn's Philosophy philosophers, "solves" the problem by positing the existence of the shared structure, in this case the shared genetic structure that determines the extension of 'schmat'. Or more precisely: " 'Schmat' applies to all animals that have a structure that is the same as that in the paradigm schmat responsible for the causal powers associated with the exemplar." (Sterelny 1983, 121) And, according to Sterelny, the same form of argument in terms of the atomic structure applies to kind terms like 'gold' and 'platinum'.

Thus, Sterelny addresses basically the same problem of how to define a kind category indicating the inadequacy of the original causal theory, although he ends with the opposite conclusion to Kuhn. Criticism from a Kuhnian point of view at this stage is that Sterelny's idea is not enough to define a kind category because of the problems in picking out a singular essence. Sterelny still needs his "schmat theory", or more generally, a descriptive vocabulary in this task. Kuhn's solution is such a holistic one that Sterelny rejects because he uses descriptive vocabulary in defining extension. Extensions of kinds are mutually limited, but this does not make it necessarily dysfunctional. The worry is that extension(s) cannot be limited because they rely on each other. As we will see below, Andersen has tried to overcome the so-called problem of wide-open texture by referring to the dissimilarity relation between instances of contrasting concepts. The idea is that an extension may be limited via a feature that excludes an instance of the closely related concept. In the same way we are able to answer Sterelny's critique. Sets can thus actually restrict each other without shared essences. I return to this theory at the end of the chapter.

We can see here that Kuhn's criticism of the causal theory is more serious than is commonly assumed. The point is, on the most general level, that without some description it is impossible to recognise objects as exemplars of kinds. This criticism is now widely accepted, as was shown at the end of Chapter 2. Furthermore, reference determination cannot be carried out by people themselves straight to a natural kind or an essence. Scientists in the past did not know essences and it is not certain that we know them now. In Kuhn's theory, extension is determined by features that can be accessed by historical subjects. If we start describing kinds, we need to pick out several properties that together establish a kind category and that stand in contrast to other kind categories. Moreover, it is not obvious what properties

Meaning Change in the Context of Thomas S. Kuhn's Philosophy should count as essential and what as accidental. A referentialist would, as a semantical externalist, probably insist that it does not matter whether people have direct access because nature, or more specifically, natural kinds themselves do the determination of reference of our terms for us. Of course that may be so, but we do not know<sup>81</sup>. It is important to notice that this strategy makes the argument reliant on the belief that there are natural kinds, categories of objects with the shared essence, which transforms the dispute metaphysically. The question is whether there are such entities. Finally, Kuhn's emphasis on the several features needed for extension determination does bring in through the back door his theory of categorising by a network of similarities and dissimilarities.

### ***Universals and natural kinds***

We now come to the stage that is possibly one of the most important in Kuhn's entire philosophy. Besides the above argument that several properties without a clear distinction between essential and accidental properties not only specify but also determine a kind at large, which contradicts the requirement of the causal theory, there is a further disagreement that is metaphysical by its nature. The notion of natural kind implies that all individuals of a certain kind are related by the fact that they share the same essence. As a consequence, the essence is not something that, by already being a property, belongs to any individual, but is instead a universal in which all instances of the same kind participate or which they share in. The essential

---

<sup>81</sup> Preston points out that the Putnam-Kripke view involves semantic scepticism (Preston 2004, 330). I believe that what he means is that it is odd to think that we have a theory of meaning, according to which meaning is reference, while also maintaining that we may not be able to tell the reference. Bird objects to this, saying, he prefers to call the view semantic fallibilism, that we do not need to be personally acquainted with, for example, Ronald Reagan in order to know the reference of 'Ronald Reagan'. Bird continues that, similarly, one may know the extension of 'water' without knowing the microstructure of water (Bird 2004c, 340-1). I agree, but this counts as a shift in the argument. The Putnam-Kripke claim is that the reference of 'water' is H<sub>2</sub>O, not that the extension of water is everything similar to this or that droplet. The latter is a Kuhnian statement. I do not understand how we could know that the reference of water is H<sub>2</sub>O without being aware that there exists such a structure. I think it is clear that in 1750 knowing the microstructure was not possible for the reason that chemistry had not developed enough. Do we have to conclude that they did not know the meaning/reference of their terms? Are we sure we know them?

Meaning Change in the Context of Thomas S. Kuhn's Philosophy property underlying a natural kind is thus a universal. The causal theory postulates continuity and comparability via this universal property. Even though beliefs change, terms continue to refer to the same substance that defines a natural kind. And when we know that terms in different theories refer to the same substance, we can compare what is claimed of it. Now I wish to argue that Kuhn was an anti-realist in regard of universals as mind independent entities. He never made an explicit argument for it, but it is clear from the context that universals do not have any use in Kuhn's philosophy. After the elaboration of Kuhn's conceptual anti-realism, I discuss how feasible Kuhn's theory of the functioning of the scientific classification terms is.

At the beginning of the chapter I suggested that Kuhn's focus in respect of theory change was on individuals and sets of individuals. He wrote of individual astronomical bodies that form natural families differently before and after Copernicus, of samples of water classified by several features, of swans, geese and ducks making natural families, and so on. We saw how he was concerned that the establishment of reference to a natural kind required exposure to "varied members of that kind but also to members of others – to individuals". There is an associated theoretical distinction to be drawn that helps us to see what is at stake. Although quite often 'extension' and 'reference' are used synonymously, they should not be taken as synonymous: reference is about a universal property, but extension is about things and sets of things<sup>82</sup>. For example, the term 'red' may be said to refer to the property redness, but to have as its extension the set of all red things. Alternatively, in regard of general terms, reference is taken to universal substances, while extension stays on the level of individual things and sets composed of individuals.<sup>83</sup> In the case of proper names, there is naturally no difference between extension and reference. The reference of 'George W. Bush' is George W. Bush, and the extension of 'George W. Bush' includes one individual, George W. Bush. As Kuhn argued, there is a big difference in this respect between proper names and natural kind terms. We may

---

<sup>82</sup> Cf., for example, *The Cambridge Dictionary of Philosophy*, "Intension", page 439.

<sup>83</sup> Of course, it is possible to hypothesise that, for instance, horses in the extension of 'horse' share the property of being a horse, and hence, the reference of horse can be fixed to such an existing abstract property. In this way, extension and reference would become nearly identical. Yet, the notion of extension does not imply such an assumption and does not by itself yield such a metaphysical stipulation.

Meaning Change in the Context of Thomas S. Kuhn's Philosophy trace the lifelines of the names of individual planets, but our job is more complicated if we try to do the same with the general name 'planet' or 'water'. Reference requires a singular substance, an abstract entity or a universal property. So the crucial question for the causal theory is whether there are such universals that are instantiated by particulars and that form a natural kind category.

When Kuhn wrote of theoretical changes, he was, thus, interested in how sets of individuals changed. Furthermore, he inquired what criteria were used to determine the set. He did not make an assumption like Putnam, Kripke and others that there is a natural kind with an essence, i.e. a universal property to which reference is determined by ostension of a sample of that kind. Sometimes Kuhn talked about 'reference', but by paying attention to the context it is clear that he usually meant what philosophers mean by 'extension', or then he had been talking about a reference to an individual. The only philosopher in the Kuhn commentary that recognises this important difference is Bird. He writes that when Kuhn talks about the referential change of 'planet', he actually means that the set of things in the extension of 'planet' has changed. According to Bird, we may think some terms such as 'gold' have both extension and reference. They are respectively all things made of gold and "the substance gold, perhaps best thought of as the single entity, the natural kind gold" (Bird 2000, 160). 'Planet' would not have a reference in this sense unless we think there is some property of being a planet.

Bird's stipulations make sense philosophically but the curious conclusion that Bird draws is that it is not even obvious that there has been a shift in extension. What has changed, Bird says, is merely what people believed to be the extension<sup>84</sup>. From this perspective, people once called the Sun a planet but do not do so any more because they had the wrong belief that the Sun behaved like Mars, Venus, etc. in having a large orbit about the centre of the local system. And so, "if the Sun is not in the extension of 'planet' now, it never was. People can be mistaken about the extensions just as they can be about references." (Bird 2000, 161) I think this a bold view both historically and metaphysically, i.e. of a description of the history of science and in terms of our capability of knowing the truth as moderns. I will discuss

Meaning Change in the Context of Thomas S. Kuhn's Philosophy  
this issue and find out Bird's and others' grounding for this type of view in Chapter 7. It is enough to say now that I think this distorts what I believe is the Kuhnian view, both metaphysically and historically. From the Kuhnian perspective, when describing history and assessing the relation of the present state of scientific knowledge to that of the past, it is important to take into account the epistemological question of whether we are capable of attaining knowledge in the proper philosophical sense. I will draft a full response from this point of view in the penultimate chapter of the thesis.

The distinction between extension and reference is, nevertheless, crucial for understanding where Kuhn stands. My argument is that the dispute between those that advocate a straight referential solution, such as Putnam and Kripke and their followers, and Kuhn, comes down to the question of the existence of universals. And this dispute is, in other words, an argument between realism and anti-realism in respect of universals. Omitting any difficulties in determining what an essential property of an object is in each case, Kuhn's scepticism regarding the existence of universals explains why he could not advocate causal theories on kind terms or common nouns. If there are no universal properties on which to fix reference, then there is no referential continuity, and nor is there referential comparability. What we can still compare in this situation are sets of individuals, although that was not Kuhn's cure for incomparability either.

Ian Hacking has noticed Kuhn's conceptual anti-realism and suggested a nominalistic solution personally to Kuhn. I think he rightly raised the question of where Kuhn stands on the dividing line between realism and nominalism. Let us, therefore, have a look at their exchange. Hacking's starting point is the tension in Kuhn's claim that "though the world does not change with a change of paradigm, the scientists afterward work in a different world ... I am convinced that we must learn to make sense of statements that at least resemble these" (*SSR*, 121). Hacking tries to interpret positively the idea that after paradigm change scientists are living and working in a different world. He constructs a nominalist solution to the new world problem. The fundamental idea of nominalism is, according to Hacking, that there

---

<sup>84</sup> Donellan, for example, appears to share the view (see Donellan 1983, 87).

Meaning Change in the Context of Thomas S. Kuhn's Philosophy

are individuals in the world, but over and above the individuals, there are not any sets, kinds, universals or classes. His solution is that Kuhn's world that does not change is a world of individuals, and the world that changes is the world of kinds of things, which is the world in which scientists work and live. That changes because science needs description for its various tasks of interaction, prediction and explanation. Description, in turn, requires classification, i.e. the grouping of individuals into kinds. (Hacking 1995, 277)

Hacking urges Kuhn to drop talking of natural kinds. As we have seen, Kuhn feels that the notion of natural kind is not well-grounded. Hacking correctly observes that 'natural kind' is not supposed to mean simply something found in nature, but most often it is also associated with a more cosmic sense. Philosophers who argue for the existence of natural kinds tend to speak of the "inner constitution of nature". Their conviction appears to be that the world must have cosmic kinds, or as stated in Plato's metaphor, it must have joints. This can be seen, for example, in selection of examples of natural kinds. Phosphorous and electricity are thought to be natural kinds. More "mundane kinds", such as water, tiger, lemon and heat are also often mentioned, although not mud or dung. Yet even in mundane cases, natural kinds are supposed to pick out more cosmical kinds, such H<sub>2</sub>O, the chromosomal structure of tigers, and the motion of molecules. I previously called Putnam's view of causal theory of reference essentialist because Putnam believes that we can fix a reference via a sample to a natural kind that is characterised by the essence shared by all samples of that kind. Hacking's recommendation is to use the notion of 'scientific kind' to mean simply that a scientific term is used chiefly in some branch of science. Hacking would mainly include terms from more mature science as scientific kinds, but also such terms from common language that have a role in the special field in question. Most importantly, a scientific kind is scientific because of human aspirations and interests. (Hacking 1995, 290-2)

Bird (2003b) has recently endorsed Hacking's interpretation of Kuhn as a kind of nominalist. However, Bird differs from Hacking because he makes Kuhn a nominalist in an epistemological, not a metaphysical sense. That is, Kuhn, in this view, subscribed to the thesis that, although universals exist, we cannot come to

Meaning Change in the Context of Thomas S. Kuhn's Philosophy know of them (Bird 2003b, 692-3). I do agree with both of them that Kuhn opposed conceptual realism. Yet, like Hacking, I take Kuhn's position to be metaphysical. Unlike both of them, I do not think Kuhn ought to be described as a nominalist. In order to understand why, let us first see how Kuhn responds to Hacking.

In his response to Hacking, Kuhn says that he took "immense pleasure" in Hacking's paper. However, he introduces three points of disagreement. One is that he still hopes to be talking of meaning with respect to kind terms. We have already extensively studied Kuhn's meaning theorising, and this comment does not cause much concern because it may be merely a terminological difference. One wants to call something 'meaning', the other does not. What is crucial here is whether the metaphysical views are compatible or not. More interestingly, Kuhn thinks that Hacking's 'scientific kind' is not an appropriate term because what is required is a characterisation of kinds and kind-terms in general. Kuhn makes a reference to his idea of mental module that would make it possible to recognise physical objects, kinds of furniture, of government, of personality and so on. Furthermore, in the book he was planning at the time, which never appeared, Kuhn intended to suggest that characteristics of kinds are "traces to, and on from, the evolution of neural mechanisms for reidentifying what Aristotle called 'substances': things that, between their origin and demise, trace a lifeline through space over time" (*RSS*, 229).

It appears that there is both a point of agreement and disagreement here between Kuhn and Hacking. To start with the agreement, Kuhn's kinds are, to be sure, more general than Hacking's scientific kinds, but I do not think that this is a reason to discount the nominalistic solution. Neither of these philosophers is a realist in relation to natural kinds in the Putnam-Kripke fashion. For example, Kuhn would probably accept Nelson Goodman's notion of relevant kind that includes both natural and artificial kinds and the idea of habituality of kind categories in contrast to absolute or psychological priority. And Nelson Goodman is a nominalist, just as Hacking is. The only reason that Hacking does adopt the term 'scientific kind' is that he thinks that Kuhn is interested only in kind terms in science. Now we see that Kuhn's aim is much wider. Yet, we can discern the proper point of disagreement as well. Kuhn clearly wants to include something other than individuals in his universe.

That is why he writes of 'substances' or kinds in general that give continuity. This indicates that Kuhn is not a full-blooded, or perhaps extreme nominalist, who thinks that objects called by the same name have nothing in common, except that they are called by the same name.

The third point of disagreement brought forward by Kuhn looks even more troubling. Given that he has numerous reasons to oppose Hacking's nominalism, Kuhn tells us of only one. Kuhn wonders how the references of terms like 'force', 'wave front' or 'personality' can be constructed as individuals. Kuhn says that he needs a notion of 'kinds' that will populate the world as well as divide up the pre-existing population. Does he want, then, to subscribe to the view that there are universal substances after all? Nothing that Kuhn says in this context or elsewhere indicates that; quite the contrary, as we have seen. I think it is clear that Kuhn would agree with Hacking that kinds do not exist as such, as being independent of our minds. He does not approve of the Putnam-Kripke doctrine that the essence of samples of kinds defines what kinds they are. But it is also evident that Kuhn is not a nominalist in the sense that kinds are just names.<sup>85</sup>

It is important to take into account Kuhn's craving for something abstract that exists and underlies the naming of objects. As we recall, Kuhn was committed to the existence of concepts as mental beings that were formed by observing similarities and dissimilarities between individual objects. This suggests that Kuhn might be called a conceptualist; conceptualism says that universals exist but only as concepts in the minds of people. Put another way, it may be understood as the doctrine that individuals are real, but universal concepts that classify them are creations of the mind. They are categories that people make in order to recognise and order things in nature. In contrast, I find no justification for the view that Kuhn thought that

---

<sup>85</sup> In relation to the debate on nominalism, it is interesting to note that J. R. Bambrough thinks that Wittgenstein with his notion of family resemblance managed to solve the problem of universals. According to Bambrough, a nominalist claims, for instance, that games have nothing else in common except that they are called games, while a realist thinks that they have something in common, other than that they are games. Wittgenstein said, in turn, that games have nothing in common except that they are games. That is, Wittgenstein denied the joint claim of the nominalist and the realist that there cannot be an objective justification for the application of the word 'game' unless there is an element that is common to all games, or a common relation that all games bear to something that is not a game. Games are, thus, related by a network of overlapping features; the family resemblance makes

Meaning Change in the Context of Thomas S. Kuhn's Philosophy

universals exist as mind-independent entities but that we are not able to tell which they are, i.e. epistemological nominalism. Nowhere do we find speculation on difficulties with getting to know real universals. The situation is rather that concepts constructed by humans do play the role of universals. Taken strictly, Kuhn was then, not an anti-realist in regard of universals as such, because they exist as mental entities. Yet he may be said to be one, because universals do not exist as mind independent. From that point of view, then, Kuhn's emphasis on the existence of conceptual schemes earlier, and mental modules later, starts to make sense. They are attempts to characterise those entities that do the organising. It is, though, noteworthy that Kuhn's earlier quotation of Aristotle's substances and neural mechanism indicates that his conceptualism concerns very fundamental neural structure that makes concept forming and the ordering of objects possible, akin to cognitive science.

The fact that Kuhn refers to David Wiggins's book *Sameness and Substance* in the context of the third counter-argument to Hacking is not without significance either.<sup>86</sup> After all, Wiggins is a self-confessed conceptualist, although a realist as such. His "conceptual realism" is the claim that "in order to single out these things (horses, leaves, sun and stars), we have to deploy upon experience a conceptual scheme which has itself fashioned or formed in such a way as to make *possible* to single them out" (Wiggins 1980, 139). However, Kuhn's reference makes one wonder how carefully Kuhn had read Wiggins's book, and how far he would commit himself to Wiggins's position. That is because Wiggins was also "an essentialist" and his form of conceptualism appears to lean strongly towards realism of an essentialist kind. Wiggins thinks that there is physical necessity. Once a thing has been conceptualised and determined that it is a member of a natural kind, a thing is necessarily of the kind it is. For example, once it is found out that Julius Caser is a man, he is necessarily a man, i.e. has necessarily the certain inner constitution that

---

them games. (Bambrough 1970) What makes this suggestion interesting is Kuhn's commitment to Wittgenstein's family resemblance notion.

<sup>86</sup> After invoking the idea of Aristotle's substances as things that trace lifelines through space and time, Kuhn says in the footnote: "As this sentence suggests, a significant role in the recent development of my ideas has been played by David Wiggins, *Sameness and Substance*." (RSS, 229; footnote 6)

Meaning Change in the Context of Thomas S. Kuhn's Philosophy  
men have. Natural kinds are defined by their extension and deictic-nomological grounding in the Leibniz-Putnam-Kripke fashion that determines the law-like behaviour of things. Wiggins also denies the possibility that the same object could have been conceptualised differently (Wiggins 1980, 101). In Wiggins's conceptualism there thus appears to be very little freedom for conceptualisation. Conceptualisation is more likely just a tool, a mediating method, for organizing things that themselves are necessarily what they are, and therefore, their categories are also necessarily what they are.<sup>87</sup>

There is hardly anything in Kuhn's writings that indicate that he could accept the above view, albeit with the exception of the main idea of conceptualism. Kuhn clearly allows more freedom to conceptualise things than Wiggins does. If we think, as Kuhn says, that objects are classified by their perceived similarities and differences that are not given, then a change in the similarity relations may prompt a change in conceptualisation. Consequently, one central feature of scientific revolutions is precisely that objects are reclassified. The sun was once conceptualised as a planet, but is now seen as a star. Water was categorised as an indecomposable element; now it is seen as a combination of two more elementary substances that can appear in several forms. Further, Kuhn writes: "the transition from Newtonian to Einsteinian mechanics illustrates with particular clarity the scientific revolution as a displacement of the conceptual network through which scientists view the world" (*SSR*, 102). Scientific revolutions can be described more generally as changes in scientific categorisations:

What characterizes scientific revolutions is, thus, change in several of the taxonomic categories prerequisite to scientific descriptions and generalizations. That change, furthermore, is an adjustment not only of criteria relevant to categorization, but also of the way in which given objects and situations are distributed among pre-existing categories. (*RSS*, 30)

---

<sup>87</sup> Wiggins rewrote his book, which resulted in *Sameness and Substance Renewed*, published in 2001. In that treatise Wiggins intends to "correct all the things in the 1980 version that I know to be plain wrong" (ix). Since my interest is in what Kuhn took on board from Wiggins's earlier book, there is no need to consider what were the issues that needed to be corrected.

Interestingly, perhaps Hacking manages to capture some of Kuhn's theoretical insight in a paper that was published more than ten years after their original exchange of ideas (Hacking 2001). In the article, Hacking seeks for a contact point between Aristotle and cognitive science. According to Hacking, although Aristotle's categories were initially meant to be more fundamental and metaphysical than those of cognitive scientists, the idea of module in the conceptual realm, and the reference to domain specific knowledge about several categories, come very close to the list of categories by Aristotle. Naturally, Kuhn's point was to grasp *historically* conditioned categories that structure our ideas about the world.

Finally, it can be admitted that Kuhn's theory - that objects are classified by their resemblance and difference - is not so different from the so-called resemblance nominalism, according to which certain individuals are called by the same name because they resemble each other. The difference is that that process in Kuhn's theory results in abstracted concepts that serve the function of classification. Therefore, Kuhn's conceptualism could also be described as a conglomeration of two types of nominalisms, as classified by Armstrong. It has both a feature of concept nominalism because concepts do the job of realists' universals, and of resemblance nominalism because similarities are called upon to explain how conceptual categories and naming come about (see Armstrong 1978, Ch. 2). Further, if we think about Kuhn's answer to Hacking from this perspective, we may understand it better. Kuhn wants a notion that is not merely tied to the existence of individuals. The fact that some kind terms are noncountable, such as 'force', makes it difficult to conceive of them as individuals. Extreme nominalism is limited because of its concern for individuals and their names. Many kinds of which existence as individuals is at least questionable seem nevertheless to play a role in our thinking. The Kuhnian idea of mental module can accommodate such kinds as concepts, whether their extension is empty or not, i.e. whether they have any real individuals in their extension or not. Thus there is no need for essence or universal substance, even if one thinks that universals exists. They are real, but only in minds as concepts; moreover, they are liable to change, making it impossible to have them as permanent reference points for comparison.

***Kuhn's conceptualism defended***

The explication of Kuhn's metaphysical position above is naturally a totally different question from the evaluation of viability of his view. Before dealing briefly with the question of how feasible Kuhn's conceptualism is, I need to pre-empt criticism of the idea of conceptual scheme from the Davidsonian perspective. Davidson's argument in his "On the Very Idea of a Conceptual Scheme" is that the idea of conceptual scheme is self-defeating. This is because it is supposed to be impossible to provide a description of the reality from a perspective of a scheme that is incommensurable with another scheme, and yet the advocates of conceptual scheme are able to describe incommensurable conceptual schemes in the past.

There is potentially much to say about that argument. It is enough here to outline a case against it to show adequately enough that Davidson's argument is outdated and does not work at least against Kuhn's more developed position. The first thing to remind a reader is that Kuhn's idea of incommensurability changed over the years. Therefore, when talking about incommensurability, we need to be sensitive to the different senses of incommensurability. His later understanding of incommensurability as localised translation failure between the terms of two taxonomies is clearly different and more limited from his earlier characterisation of incommensurability as methodological, observational, and conceptual disparity between paradigms. In regard to Kuhn's later position, Davidson's construction is a straw man. Let us see in what ways this is the case.

The answer that Kuhn, and also Feyerabend (e.g. *RSS*, 203-4; Feyerabend 1996, 265-273) gave to the critics of conceptual scheme on the above Davidsonian ground was that the impossibility of translation that makes theories incommensurable does not imply the impossibility of understanding them. One can, metaphorically speaking, learn another language and become bilingual. A bilingual person may understand two languages and speak them fluently but not be able to give perfect translations. Yet it is possible to try to give an approximate description of "the other world". In fact, this must be something that a historian or an anthropologist studying a culture distant from ours, either in time or place, is doing. Perhaps a more

Meaning Change in the Context of Thomas S. Kuhn's Philosophy

interesting point to notice is that later Kuhn's conceptual schemes are not as global as Davidson implies<sup>88</sup>. It is not that there is one scheme per culture that forms the conception of reality. Kuhn's later conceptual schemes, or mental modules, are more mundane and more local; they are in plural. Nothing that Kuhn says in his later writing prevents a person from having several schemes, perhaps not for the same field or task, but for different tasks of conceptualisation. A consequence of this is that untranslatability is not global and total, but is local and partial, and there may be an overlap in many areas, even if somewhere else we are confronted with untranslatability. Davidson himself accepts the possibility of partial untranslatability. Furthermore, as mentioned in the last chapter, Davidson's and others' talk of conceptual scheme is very abstract and undifferentiated. We have seen that cognitive science has more to say on it that removes the mystery from the idea of conceptual scheme. In fact, it makes it an empirically supported natural entity. Finally, untranslatability, i.e. untranslatability of intension, sense, or so on, may not be as big a problem as has been thought. Although not endorsed in this thesis, the main message of referential solutions with regard to incommensurability is precisely that reference or extension is enough for comparison, even in the absence of an intensional translation.

Discussion on the feasibility of natural kind concept is important because the success of the Putnam-Kripke type of causal theory relies on it. The question whether there are natural kinds is, of course, metaphysical. We just do not know whether nature has such discoverable joints. A realist of an essentialist persuasion has a belief that there are such, and the above mentioned problems are unlikely to persuade him/her of a contrary view. Many philosophers keep explaining progress in the history of science on the basis of natural kind concepts; they say that scientists in the past were referring to and talking of the same kinds as we do, but with less precision.

---

<sup>88</sup> Note that Davidson's characterisation fares better with Quine's understanding of conceptual schemes. Quine frequently writes about conceptual schemes in the singular. He talks about "our adult conceptual scheme" (Quine 1969, 8), "my sort of conceptual scheme" (Quine 1969, 5), "our current conceptual scheme" (Quine 1969, 23; 24), "the conceptual scheme of science" (Quine 1964, 44) etc. Quine, thus, supposes that a person, a community, or a culture has only one scheme. He refers, for instance, to schemes that individuate abstract objects, and refers to the "object-oriented scheme" (Quine 1969, 24).

In any case, I think it is evident that we need kinds in science and beyond science. If kinds - social, natural, or "relevant" - are simply understood as sets or classes in the fashion of Quine, or as concepts, they seem to be indispensable. It is another question whether such kinds can and do share an essence. I am not trying to settle the issue of the existence of natural kinds, but I am merely suggesting that we let empirical facts have more influence on the position we should adopt. That is, we could try an inference to the best explanation: if sciences are discovering "the joints of nature", then supposedly scientists in the course of time are able to agree on the representations of the world, and as a consequence, the systems stabilise and finally become fixed. From the stabilisation of the representations of the world we might infer that the best explanation is that there are something like natural kinds.<sup>89</sup> In other words, if theories converge and stabilise with a certain classification, then the idea that there are joints that science is discovering is a plausible option. Then, of course, we should also decide whether there is one ultimate all-inclusive categorisation in science or several separate categorisations in different fields of science. Resolving that question depends on whether reduction between sciences is possible, which itself is quite a challenging philosophical task. But it is equally important to realise and accept that reasoning to the best explanation may produce the contrary result: if sciences and their systems of classification become more pluralistic, we have reason to become more sceptical of the existence of a privileged system of natural kinds. Ultimately I am thus suggesting that we let the history of science have more influence on our judgment. This is the theme that will be dealt with more in Chapter 7.

However, I wish to make a stronger claim for Kuhn's project of kinds: it is more warranted than its essentialist rival. There is something that both a realist and a conceptualist agree upon. It is arguably unobjectionable that we encounter individual objects of which some are more similar to each other than others. On the basis of their similarity we conceptualise them in kinds. To say, in addition, that some of these resulting classes share an essence, and that there is a universal that explains

---

their classification as kinds, goes much further, however. I think that Kuhn's suggested route is something that we have to start with. After that we may see, in the light of the history of science and of other empirical evidence, whether essentialism is a feasible position and 'essences' needed for explanatory purposes. This is a strategy that reminds us of Ockham's razor: entities should not be multiplied beyond necessity. The acceptance of concept to do the job of universals does not inflate our universe as we accept mental entities, such as ideas or thoughts, anyway. Moreover, in Kuhn's theory, concepts are something that emerge from the empirical realm.

More generally, Kuhn can be taken as emphasising the role of empirical evidence in deciding what type of philosophical view to take. Now, aside from the metaphysical divergence in assumptions between Kuhn and the causal theorists, Kuhn allows determination of reference to essences if that kind of view is compatible with empirical knowledge. I do not agree with Bird, for example, that Kuhn's rejection of the causal theory is his wrong turning in the sense that he turned away from naturalism to a priori reasoning (e.g. Bird 2002, 2004c). The last chapter showed that several parts of Kuhn's philosophy can be seen as receiving an empirical endorsement also *after* Kuhn had made clear that he wanted to reject the causal theory<sup>90</sup>. Specifically, in so far as there is evidence that concepts as actually used by humans do not allow an unambiguous description of objects they refer to, i.e. a description conceived as an essence of the kind, then there is an empirical reason to reject the causal theory. Naturally, it could still be right, but what cannot be said is that it is supported by empirical research on concept use and that a rejection of the causal theory counts as a rejection of naturalism. The case may be, at the end, vice versa. That is, because of its poor empirical support, the acceptance of naturalism prompts the rejection of the causal theory.

If the causal theorists wish to be consistent in their naturalism, they should also submit the concept of essence for empirical testing. If not, the situation is a bit like with logical positivists who wanted to apply the verification principle to every

---

<sup>89</sup> More on possible roles of Inference to the Best Explanation in an argument for scientific realism, see Lipton (1993; 2004).

<sup>90</sup> He made his attack for the first time in "Metaphor in Science" (RSS), based on a conference presentation in 1977.

Meaning Change in the Context of Thomas S. Kuhn's Philosophy

other statement except to the principle of verification itself. Of course, that could not be done because it was not known how the principle itself could have been verified, and for that reason, it had to be taken for granted. As already seen, Shapere observed that the Putnam-Kripke essentialism has a hidden element of apriorism, which seems to be absent in Kuhn's approach. The commitment to the thesis that there are essences and that certain substances have certain essences makes it impossible to accept an argument to the contrary, and the thesis can always be modified in the face of empirical evidence. If cases that are supposed to prove the thesis turn out to be incorrect, an advocate of the thesis can always deny that people have really found essences or broaden the understanding of 'essence' without altering the thesis itself. Consider how Putnam admits that we can imagine that water is not H<sub>2</sub>O, but then say that "it is conceivable but it isn't possible" (Putnam 1975b, 233). Is this not indicative of an apriori commitment that we cannot be sure is really correct? (See Shapere 1982, especially 8-10; 1991, 667-8)

Kuhn's conceptualism is, of course, incompatible with the idea of Lockean real essence. Similarity classes are, therefore, better understood as forming Lockean nominal essences. That is, a kind has a nominal essence if a thing must have certain properties in order to be called a kind. This view does not imply that the objects of kind K need to share essence, just that they are similar in relevant respects that make them kinds. This is to say that similarity classes are metaphysically on a par with nominal essence in their non-essentialism. Yet, crucially, being an anti-essentialist does not imply being an anti-realist ontologically. Kuhn's conceptualism is compatible with minimal ontological realism, i.e. with the belief that there are real objects as they are<sup>91</sup>. What is denied is that there is one true way to represent reality. This position is similar to Dupré's pluralism and his "promiscuous realism". This is the view that there are countless legitimate, objectively grounded ways of classifying objects in the world, depending on our needs and aims (e.g. Dupré's 1993, 18).

---

<sup>91</sup> Kuhn describes himself on one occasion as an "unregenerate realist" (RSS, 203). He also implies that at times nature undermines the security of the professional community (SSR, 169), and that nature has a role to play in shaping our conceptions (ET, 72). Because Kuhn does not commit to a view of what objects there really are or what the essence of objects is, or in short, of what the structure and form of the reality is, this comes close to what stronger ontological realists have mockingly called "fig-leaf realism": Something objectively exists independently of the mental. (Devitt 1984, 23)

Dupré argues further that all systems of classification depend on their function and there is no functionless classification as such. All classifications are good or bad for their purpose. This is to say that the existing classifications in science do not lose any legitimacy if we do not declare them as “approximations of real joints of nature”. There are good reasons for having them and keeping them as long as they are functionally relevant. Neither is there anything that makes categorising easy. Objects are real, and although one can have very curious categories<sup>92</sup>, it is very difficult to make a functioning one for one's purpose. All in all, I believe that Dupré's position accords well with Kuhn. Kuhn might be described as a minimal ontological realist with regard to individual entities or particulars, but as an anti-realist with regard to universals. There are many ways to conceptualise the world but “nature cannot be forced into an arbitrary set of conceptual boxes” (RSS, 159). Nature resists because entities are real.

### ***Limits of extension***

As I have said before in the thesis, I do not think it is correct to maintain that empirical studies in cognitive science have proved that the classical account of concepts - that concepts are determined by a set of sufficient and necessary conditions - cannot work. The advocates of the family resemblance view are right in so far as we are concerned with how people use concepts. We cannot find a set of conditions that apply equally to all instances of the concept. Still, it is possible that

---

<sup>92</sup> Both Lakoff and Wiggins bring forward the case of how an ancient Chinese encyclopaedia classified animals: a) those that belong to the Emperor, (b) embalmed ones, (c) those that are trained, (d) suckling pigs, (e) mermaids, (f) fabulous ones, (g) stray dogs, (h) those that are included in this classification, (i) those that tremble as if they were mad, (j) innumerable ones, (k) those drawn with a very fine camel's hair brush, (l) others, (m) those that have just broken a flower vase, (n) those that resemble flies from a distance.

This is apparently Jorge Luis Borges' invention that Foucault also utilised in his book *The Order of Things*. Interestingly, the writers draw different conclusions from this. According to Lakoff, Borges deals with fantasy and these could not be natural human categories. However, Lakoff thinks that it nevertheless makes the point by giving the impression that a Western reader gets when reading descriptions of non-Western languages and cultures. Wiggins, in turn, says that it is not at all difficult to imagine these categories. The problem is to conceive of how they could be the explanation for anything. (Lakoff 1987, 92; Wiggins 1984, 144-5)

Meaning Change in the Context of Thomas S. Kuhn's Philosophy

we find conceptual cores, and therefore, explicate or improve our usage of concepts. If essentialists are right despite all the scepticism, and if we suppose that we are also able to find out what the essential properties of kinds are, then perhaps we can settle on a conceptual scheme in which each concept can be defined unambiguously. On the other hand, the family resemblance account of concepts offers an alternative theory of extension determination, which is more realistic regarding the actual use of concepts. And that is how I treated the family resemblance notion: as a suggestion of how extension is determined. We need to ask, therefore, whether it can work.

Andersen et al. took the view that Kuhn's account of concept can be interpreted along the lines of Rosch's theory of the graded structure of concepts. That is, there is a prototypic exemplar, and instances that fall under the concept can be graded in their gradual resemblance to the prototype. If an instance is more similar to a specific prototype exemplar than to any other exemplar, it falls under the concept. Furthermore, similarity is always a matter of degree. However, it is also possible to adopt a different interpretation that accommodates the family resemblance idea. That is, we may understand Kuhn's natural families as sets. A set membership criterion can be characterised as an abstracted bundle of properties. An instance is a member of a set if it fulfils enough features of the bundle. What is important is not exactly how much, but that not all the features have to be fulfilled and, crucially, that instances always fall into one or another category. This view can be described as loose intensionalism, to use Bird's terminology, because only a part of extension determining criteria needs to be fulfilled. All instances are equal in their membership, but some instances can be better, i.e. more typical. I prefer this more traditional approach. That way we can avoid, for example, reference to a visual characterisation of the exemplars. Furthermore, the determination of the degree of similarity by an exemplar that is a concrete object strikes me as problematic. It is also easier to deal with the question of comparison via extension and sets than through concrete exemplars.

The immediate problem that the family resemblance account faces is the problem of wide-open texture. That stems from the argument that, since we can always find some resemblance between instances of one concept and another, family

Meaning Change in the Context of Thomas S. Kuhn's Philosophy  
 resemblance does not succeed in limiting the extension of concepts. Thus we need to restrict the extension. Otherwise, we can always extend the class to new instances by focusing on new kinds of similarities and include practically everything in the same class at the end. However, I believe that Andersen's solution (2000) is able to prevent this case from happening. Let us see how.

Andersen's argument is simple. Consider five instances, *abcde*, of a given concept. Concepts possess the features ABCDEFGH according to the following diagram.

a	A	B	C	D				
b		B	C	D	E			
c			C	D	E	F		
d				D	E	F	G	
e					E	F	G	H

This shows how the features of instances extend. Without restrictions there is no limit to the application of the concept because we can take new features I, J etc. of new instances into account, and the series of sorites continues indefinitely.

Andersen's solution in reference to Timothy Williamson (1994) is that such a sorites series can be stopped if it collides with a sorites series that goes in the opposite direction. This can be seen from the following familiar example of ducks and swans. The most common duck, the mallard, in Europe ( $Duck_M$ ) has a short neck (B), a rounded beak (C) and is the colour brown (A). The Chinese duck ( $Duck_C$ ) has a short neck (B), a rounded beak (C) and is the colour white (D):

$Duck_M$	A	B	C	
$Duck_C$		B	C	D

The most common swan has long neck (E), rounded beak (C) and is the colour white (D). However, an Australian swan ( $Swan_A$ ) is black (F):

Swan <sub>C</sub>	C	D	E	
Swan <sub>A</sub>	C	D		F

The duck category cannot be extended in the following way:

Duck <sub>X</sub>	C	D	E	
-------------------	---	---	---	--

That is because the last, Duck<sub>X</sub>, would be a member of swans rather than ducks. The general idea is, thus, that the sorities series stop each other by an excluding property, and boundaries of categories can be kept sharp.

Andersen asks what we can do with intermediate cases, and she gives an answer in reference to Kuhn. Kuhn clearly does not accept that natural families merge into one another (see *SSR*, 45). He writes also that the immediate recognition of objects relies partially on the existence of “empty perceptual space between the families to be discriminated” (*SSR*, 197, ft. 14). It is clear from what is said before in the thesis that Kuhn does not mean that there would be a structure in the world that divides objects naturally into classes. A more appropriate interpretation is that Kuhn requires that there cannot be vague boundaries. We remember Kuhn's words from the last chapter:

The experience of generations has to date confirmed that all observed objects fall into one or another natural family. It has, that is, shown that the entire population of the world can always be divided (though not once and for all) into perceptually discontinuous categories. In the perceptual spaces between these categories are believed to be no objects at all. (*ET*, 285)

In the case of a new discovery that is seen to bring vagueness, we may understand it as a case of the incompleteness of knowledge, which may prompt the construction of a new classification. Andersen et al.'s example of the South American “Screamer” is

Meaning Change in the Context of Thomas S. Kuhn's Philosophy  
a case in point. However, Kuhn may also accept that there are concepts with vague boundaries, but may insist that such cases should not exist in science, or that one should aim at getting rid of vagueness. For example, "tall man" is a vague concept, but it does not have any significance in science.

The emphasis of differentiating features illustrates well Kuhn's point of lexical dependency brought forward also by Sharrock and Read: extensions of terms are mutually related and restricted. Without other similarity sets an extension of a term would be limitless. It is not enough to take into account an individual contrast set ('swan' – 'duck'), but also to consider direct and indirect contrasts in the whole taxonomy in order to limit extension in other directions (e.g. 'Donald Duck'). And a change of similarity and difference features of a term may affect the extension of other terms. Furthermore, in principle, the family resemblance theory does not have an inheritance between the properties of superkind and subkind. Birds might be classified as flying animals, and yet an ostrich without its flying ability is still counted as a bird because of other similarity features. In this case, the family resemblance account differs in an important respect from the classical set theory. It is important to notice that a set does not have to share any single property, because the limitation of sets can be done by different sets of properties. Naturally, in order to be consistent with conceptualism, properties or features ought not to be understood as existing independently. They may be understood as abstractions acquired by observing similarities between individuals. Alternatively, the talk of properties can be taken as a talk of similarities. When we say that a swan is white (X is f), we actually say swan A and swan B resemble each other in certain respects (A resembles B). The idea is, as the above diagrams show, that an instance of a category cannot be more similar to an instance of another. If that is the case, it is an instance of the other.<sup>93</sup>

---

<sup>93</sup> One should not, however, be under impression that a realist about universals has to necessarily think that universals are located in a "Platonic heaven". As David Armstrong has argued, universals can be brought down to earth and think "of a thing's properties as constituents of the thing and think of the properties as universals" (1989, 77). His view is more precisely as follows: "Universals are constituents of states of affairs. Space-time is a conjunction of states of affairs. In that sense universals are 'in' space-time" (1989, 99; see Ch. 5). On the other hand, Armstrong has also written about a particularly interesting form of nominalism, which (of course) avoids postulating anything like a Platonic heaven but, nevertheless, accepts that

## Meaning Change in the Context of Thomas S. Kuhn's Philosophy

As such, the above family resemblance account is not far removed from more traditional descriptive theories of extension. As already mentioned, it is close to the so-called cluster theories that were also inspired by Wittgenstein's philosophy. The difference to the traditional descriptive theories is its insistence on the flexibility of features that instances can have. This also means that it is subject, in principle, to the criticism directed against the traditional descriptive theories. Perhaps the most worrisome has been the fact that it allows changes in extension, and cannot offer the stability of reference sought by the causal theories, and raises, furthermore, the problem of incomparability between theories. Our response to this has to start from something that is already obvious. From a Kuhnian perspective, we refuse to talk of reference to natural kinds because that introduces a metaphysical commitment of universals and the idea of a certain structure of the world. We have to talk only of extension as a set of individuals. Having said that, things start looking a bit different. I defer the full treatment of this problematic to the second last chapter. Yet it is enough to say here that I think that changes in extension are natural and recurrent phenomena. This is something that we should conclude if we take historical data seriously. Furthermore, they do not necessarily cause problems for the comparison of theories since theories may be compared via overlapping sets. More importantly, I think that the significance of point-by-point comparison via reference or extension is hugely overemphasised in the modern philosophy of science, and it is likely to be a gross simplification of the historical process.

---

there are properties. The so-called trope theory is a version of nominalism that conceives properties as particulars, and for that reason does not imply the existence of universals. It postulates 'tropes' that can be understood as 'property individuals' that are qualities located in time and space. The whole world can be constructed from these trope particulars. A version of trope theory called 'bundle theory' appears to be especially interesting. A property of being, for example, white, can be understood as the class of similar tropes. Note that tropes are not understood as being identical to universals. The trope theory whose tropes have also been called 'abstract particulars' and 'property instances', among others, appears to be a theory that is gaining more strength in metaphysics, to the extent that Armstrong said that in his previous work he "underestimated the strength of a tropes + resemblance (+substance-attribute) view" (1989, 120) and that "the philosophy of trope is riding high". Such philosophers as Stout, Campbell and D.C. Williams have developed a kind of trope theory. (See Armstrong 1989, esp. ch. 6; also 1978; for a recent application of a trope theory in the philosophy of science, see Niiniluoto 1999, 29-33.)

## ***Conclusion***

In this chapter I have tried to explain why Kuhn opposed the Putnam-Kripke causal solution with regard to meaning change. The crucial difference between these opposing views can be found in their attitude to natural kinds. First of all, Kuhn was sceptical whether a theory developed for proper names could also be extended to general scientific terms. Furthermore, Putnam, Kripke and others working in that tradition assume that there are natural kinds with essence and that natural kinds together form the structure of the world. This supposition has allowed them to rely on a reference to a universal property that is instantiated by members of a natural kind, which also explains why they belong to the kind in question. Kuhn denies that reference can be fixed to a natural kind because we always need to take into account several sorting properties in naming objects. Moreover, in choosing these properties, it is not given which property is counted as being essential and which accidental. According to Kuhn, the establishment of kind categories is done by us and not given by nature. Several ways to classify are possible. Another point of difference is that Kuhn does not postulate universals that are indispensable for the causal theory. His thinking remains on the more concrete level of individuals, and on their similarities and dissimilarities that determine set membership. Kuhn's example of how the extension of 'planet' has changed is a good example of his thinking.

There are, indeed, reasons to be sceptical of how generally applicable the notion of natural kind is. Rigid designation may not be as effective in differentiating between natural and non-natural kinds as is thought. The analogy between ordinary language terms and scientific terms does not hold. Neither is it easy to find privileged systems of classification in science. I suggested that Kuhn's position is more cautious and more warranted for that reason. It is reasonable to think that individuals are real. The question of whether there is a privileged categorisation that corresponds to the real division of individuals in the world ought to be considered by also taking into account the history of science and other empirical research. If the history of science seems to converge to a certain representation, then it gives support to that view. If not, we should be more careful with the essentialist assumptions. However, Kuhn is not a nominalist but rather a conceptualist. That is, he thinks that there is something

Meaning Change in the Context of Thomas S. Kuhn's Philosophy like a conceptual scheme that organises the data of the world. Previous discussion of cognitive science has made this suggestion empirically plausible. I also argued that Davidson's influential counter-argument is outdated.

I have made clear that I do not think that empirical results from cognitive psychology have proven the classical account of concepts wrong. They may have shown that the application to human concepts is limited. However, it might be possible to find unambiguous definitions of extensions, especially if essentialism were right. For that reason I have studied it as a rival suggestion of how to determine extension. As such, it is relatively near the traditional descriptive theories, with the addition of the flexibility of features that an instance in the extension can have. I also argued that Andersen's solution to the problem of wide-open texture is successful. By using a differentiating relation between categories, we can succeed in limiting extension.

There are at least two important questions that I have not answered yet. One is the question of our ability to know the extension of our terms and those of the past. Can we say, on the basis of our knowledge, that those in the past were wrong about extension, and we are right? How much are we tied to our historical standpoint? The second unanswered question is focused on the one that initially caused a stir in philosophy: how can we compare theories. If we adopt the view above, we can no longer rely on stable natural kinds as points of reference. How then is comparing and choosing between theories possible? Both these questions are taken up in the penultimate chapter. Before that I will engage in a consideration of what meaning change is historically. On this issue it may be said that, in addition to focusing on the relationship between a term and its extension, there is another sense of meaning change that is more appropriate in regard to understanding the thinking of past scientists.

## 6. Meaning change historically

Kuhn was involved in three different professional fields during his life. His first academic education was in physics. From physics he made an abrupt move to the new emerging field in the Anglo-American world, the history of science, prompted by a sudden awakening of interest after being given the opportunity by James B. Conant to teach some history of science courses. His third career move was more gradual. Kuhn's shift to philosophical theorising occurred in degrees as he became acquainted with the philosophical debates of the day. If we exclude what was said of Kuhn's historical rationale in the second chapter, practically all that is said in this thesis has dealt with Kuhn's philosophical aspirations with regard to meaning and meaning change, including also a cognitive-philosophical interpretation of them. Now I want to go back to his earlier involvement with the history of science. My claim here is that at the beginning Kuhn had a rather pure historical interest, not far removed from the traditional concerns of the history of ideas or intellectual history. Yet he lost this interest after becoming more preoccupied by philosophy and, especially, after discovering Wittgenstein's philosophy. Theorising on the history of ideas might have been further developed by the family resemblance notion, but he became more interested in the determination of extension than in the determination of historical ideas. There are others that have, nevertheless, tried to develop a theory of the history of ideas on that basis. Most notably, the representationalist accounts that we saw in Chapter 4 have also received an application in the context of the history of ideas, especially by Nersessian.

In this chapter, I firstly introduce some of Kuhn's early historical themes of interest. They include an examination of Newton's writing, Robert Boyle's chemical thinking and the emergence of the concept of energy conservation, as well as the difference between Aristotle's and modern concepts of speed. I show that Kuhn was deeply interested in the ideas or concepts that the historical figures possessed. Secondly, I argue that this interest implies the question of what it means to possess

Meaning Change in the Context of Thomas S. Kuhn's Philosophy

an idea or a concept, and furthermore, what precisely historical ideas or concepts are in the first place. These are very traditional concerns in the history of ideas and intellectual history, and are separate from Kuhn's later preoccupation with extension. Thirdly, I maintain that, although there have not been satisfactory answers to these questions, the questions cannot be ignored. I explore some possible ways to answer them, after which I construct my suggestion for an answer. My suggestion stems from the tension between two different theories on historical concepts or ideas. Lovejoy's project of unit ideas correctly brings forward the point that all exemplifications of historical concepts must share something in common. Otherwise, from the point of view of history writing, it would not be meaningful to call two representations as representations of the same concept, but it would be more appropriate to take them as instantiations of two different historical concepts. The problem with the unit idea is, however, that it overemphasises continuity and suppresses discontinuity. The capability to describe discontinuity is, in turn, the strength of the proposal by the cognitive history and philosophy of science that takes historical concepts as the family resemblance type. It succeeds in describing both continuity and discontinuity. Yet, the problem is that it allows a situation where two historical representations do not have anything in common. My suggestion is to adopt the basic strategy and tools of the cognitive history and philosophy of science, but to also take Lovejoy's insight on board. First of all, I take concepts as natural beings that are representations in the mind of one or more individuals. We might, if we follow cognitive scientists, call them stereotypes or prototypes. Furthermore, my conclusion is that in order to call several representations as instantiations of the same historical concept, one has to postulate a necessary minimal shared conceptual component, but it is not necessary to think that there is a shared set of features out of this conceptual core. In other words, there is some shared element that is a requirement for conceptual membership, but there is not a full and explicit definition of the historical covering concept. Excluding the minimal component that creates continuity between instantiations, historical concepts undergo constant changes, manifesting simultaneous discontinuity between different exemplifications.

I aim to illustrate my view by considering the development of the concept of 'element' in the 18<sup>th</sup> century. On the basis of the above account, I also give a

Meaning Change in the Context of Thomas S. Kuhn's Philosophy  
mundane interpretation of incommensurability. It may be seen as a practical problem. The meanings of two expressions differ and are described as incommensurable because the implied sets of beliefs under a term differ so significantly that it makes communication and translation difficult, but not necessarily impossible, to achieve. In the course of the chapter I also draft my answers to the questions that were directed against the most famous theoretical project in the history of ideas, i.e. Lovejoy's project - what concepts or ideas are, how they can be identified, and how to reconcile conceptual continuity with conceptual discontinuity. This last question approaches Kuhn's interest in meaning change from a different direction than his preoccupation with extension. We may say that it gives us a purer historical account of what meaning change is.

### ***Kuhn's early affinity with the history of ideas***

At the beginning of his career Kuhn made several, usually short, studies on different historical cases<sup>94</sup>. I will next examine some of them in order to illustrate the nature of his affinity with the history of ideas. In apparently the first published historical paper, published in 1951, Kuhn studies a passage from Newton's "31<sup>st</sup> query" of the *Opticks*. This short text is entirely conjectural in suggesting, on the basis of interpreting sense of the text, that a misprint occurred in translation of Newton's original text. Kuhn's proposal is that, unlike the actual printed text of what Newton says, Newton in fact considered size as well as attractive force to be relevant to determining the power of "menstrua", i.e. dissolving liquid, in this case, of gold and silver. This is an example of a typical textual analysis of a historical work. An article that appeared the following year is more interesting. In his article, "Robert Boyle and Structural Chemistry in the Seventeenth Century", Kuhn engages in interpreting the

---

<sup>94</sup> In fact, the clear majority of Kuhn's publications until *SSR* in 1962, and various texts after it, were reviews of studies of history. I have ignored reviews as unessential and out of the domain of this thesis. I do not either go through all the short historical articles here, but only the relevant ones for the purpose of the thesis. See Hoyningen-Huene (1993, 273-78; 302) for an almost full list of publications. Only *RSS* is missing.

Meaning Change in the Context of Thomas S. Kuhn's Philosophy thinking of Robert Boyle. His target is notably the common conception that Boyle suggested a definition of 'element' that anticipates the one given by Lavoisier, and is thereby a modern one. Kuhn shows how this understanding relies on an omission of a crucial piece of text when the putative definition is given. What Boyle, according to Kuhn, actually tried to do is to argue against the common "chymical" notion of element. That notion stated that there are a small fixed number of unchangeable and unmodifiable elements of which all bodies are made and which are responsible for perceived qualities of bodies. Much of Boyle's work was to prove that any candidate for such an elementary substance fails. Furthermore, the notion of element was in contrast to his chemical thinking based on the mechanical philosophy, according to which all chemical qualities can be derived from the manner in which the corpuscles of bodies are arranged and moved relative to each other. A consequence of this philosophy is that by the sufficient rearrangement of positions and motion one could, in principle, obtain anything from anything. In other words, Boyle believed that transmutation is possible. The crux of the text is that the "modern" notion of element, or any notion of fundamental enduring substance, is in contrast to Boyle's thinking and he, moreover, argued against the existence of such elements. In contrast to what was widely believed, at least still in the 1950s, Boyle could not have introduced a modern (or almost modern) concept of element.

"The Caloric Theory of Adiabatic Compression" appeared in 1958 and brings forward two themes that gain a more important role later in *SSR*. Kuhn studies "the discovery" of a phenomenon that rapidly compressed or expanded gas changes its temperature. For example, compressed gas increases its temperature, which is later said to be due to the work done and was taken subsequently as an example of energy conservation. The same experiments that were thought to give evidence for energy conservation were also understood as supporting the caloric theory of heat. Although this may be paradoxical from a modern perspective, it can be explained, writes Kuhn, by realising that the caloric theory was a well-developed theory, and therefore, difficult to prove wrong. Another point in the paper is that "the discovery" should be understood as a "gradual emergence" rather than a sudden realisation, because various scientists over half a century were involved with various outputs in formulating the adiabatic compression. The latter concern is common to "Energy

Meaning Change in the Context of Thomas S. Kuhn's Philosophy  
Conservation as an Example of Simultaneous Discovery" (in *ET*) published in 1959, where Kuhn examines the emergence of the concept of energy conservation. Kuhn records that twelve scientists in a remarkably short period came to very similar conclusions. For this reason he set out to study the intellectual climate in the two decades before 1850 that made the "discovery" possible. He suggests that the emergence of the concept is due to three experimental and conceptual elements: the "availability of conversion processes", the "concern with engines", and "the philosophy of nature". Throughout the paper, Kuhn is concerned by the emergence of various scientific concepts, such as the concept of universal convertibility, conservation and energy, and work. Yet a further illustration of Kuhn's earlier historical thinking is his "A Function for Thought Experiment" (in *ET*), which appeared just after the first edition of *SSR* in 1964. There, he compared Aristotle's notion of 'speed' to that of modern notions, notably to the one introduced by Galileo in his *Dialogues Concerning Two Chief World Systems*. Kuhn argues that in Aristotle the modern concepts of average and instantaneous speed merged into one, and that his notion was an integral part of his theory of motion and even of the whole physics.

What I want to point out by presenting some of Kuhn's early historical work is that he was interested in textual interpretation, and above all, what scientific concepts or ideas are, and how and when they emerged in history. Much of his early work is concerned with trying to identify and draft a way to the emergence of scientific concepts that scientists came to possess. These concerns I call the traditional fundamental preoccupations in the history of ideas and intellectual history. That is, one of the fundamental tasks in those fields is to identify and define concepts or ideas, explain their birth, trace their influence, and track down their modifications. This reading of Kuhn's early historical works also accords well with Kuhn's self-understanding as a historian. In the 1980s he said of himself:

Probably the thing I do best and certainly the one to which I have devoted most time is climbing from the writing into the mind of dead scientists, figuring out how they thought, why they believed what they did, and how they came to change their minds. My principal efforts have, that is, been directed to what I have sometimes called "*the*

Meaning Change in the Context of Thomas S. Kuhn's Philosophy  
*dynamic interrelationships of pure ideas*". (Kuhn 1983, 27; italics mine)

Furthermore, in his last interview Kuhn says that his ex-student Jed Buchwald "does the sort of *history of analytic ideas* that I do" (*RSS*, 319; italics mine).

In Kuhn's later writings there is a whiff of the same interest. A word of caution, though, is necessary here. Kuhn typically moved on to new problems and did not proceed to develop full-blooded theories. The same applies here. Even though some words can be seen in the light of the history of ideas, they may have pulled Kuhn in developing his theories on family resemblance, taxonomy, and lexicon that later became his main focus. Nevertheless, Kuhn's proclamation in *SSR* that the pre-Einsteinian concept of 'space' was necessarily homogenous, isotropic, and unaffected by the presence of matter, or that the idea of a moving Earth was an nonsensical notion for the pre-Copernicans, seems to communicate the interest in "analytic ideas". (See *SSR*, 149-50) I return to this interpretation at the end of the chapter. Elsewhere Kuhn quite rightly, to my mind, emphasises that the reduction of translation of past texts to identifications of reference of terms is not acceptable. In addition to reference, historians need to report what scientists of the past believed, independent of the truth-values of their beliefs. If we follow, for example, Kitcher's advice and look for a referential equivalent for 'phlogiston' in some contexts where it can be thought to have referred, for example, to oxygen, we leave the text translated in an unintelligible condition. In general, Kuhn spells out clearly that translation based on extensional semantics and truth-value preservation is not enough (*RSS*, 41-2). What must also be translated is intensionalities or senses (*RSS*, 49).

Talk of the definition of historical concepts or intensional meaning is different from considerations of extension or reference. In order to find out what kind of intensional meaning or concept a person has, whatever that exactly turns out to be, does not require knowing whether that meaning or concept refers to anything real in the world, or whether the belief or a set of beliefs communicated in this way is true or false. Let us think of the tripartite distinction between term/word, intension/concept, and reference/extension. Discussion of meaning change addresses the relation between the first two. It is a question of *meaning of term* when 'meaning' is

Meaning Change in the Context of Thomas S. Kuhn's Philosophy understood in its customary way as sense or concept, and a term is taken to denote the sense or the concept. It can be extended to the consideration of reference but that move changes the question from what the intellectual products are that people denoted by their terms to what the objects are that they referred to by their terms. Naturally, extension and intension, as has been emphasised throughout the thesis, can be connected. For example, one can adopt a theory that intension determines the former, which I believe Kuhn eventually did. More precisely, I think that Kuhn's interest in similarity relations took the role of the intensional meaning that determines the extension of terms.

The upshot is that if we are interested in what concepts scientists had or possessed, then we are interested by the same token in something that is different from the question of what their concepts referred to and how. It is one question to ask what the concept of 'element' was that a certain scientific group possessed or had in their mind in 1750, but quite another question to ask what was in the extension of that concept, and yet a different topic to inquire how they determined the extension. If the concept possessed was something like an 'indecomposable fundamental substance found in all bodies and responsible for their qualities', the extension was everything that falls into the categories of four or five putative elementary substances (because they were thought to be such elements), and the determination of that everything was carried out probably in part by observing similarities and in part by deducing them from their causes in bodies, i.e. from the qualities that they were thought to be responsible for. In other words, the respective concerns are the idea or concept in mind or minds, their relation to the world and the way that relation is determined.

### ***Nersessian on the foundations of intellectual history***

Nersessian (1985) describes with particular clarity the problem under consideration here. Nersessian attempts to find an answer to the question 'When did Faraday have his field concept?'. She notes that such a question is customarily seen as an historical

Meaning Change in the Context of Thomas S. Kuhn's Philosophy issue, but a satisfactory answer requires philosophical consideration as well. That is, before we can answer the question of *when*, we must determine *what* Faraday's field concept was. According to Nersessian, this is something that both historians and philosophers of science have given too little thought to in their analyses of the formation and development of scientific concepts.

The answer to the latter question implies, in turn, three separate questions:

1. What was Faraday's conception?
2. What is required for a concept to be a 'field' concept?
3. What does it mean to say that someone 'has' a concept, i.e. what general form does the representation of a concept take? (Nersessian 1985, 175)

Nersessian correctly argues that answering has to start from the last question. In order to say what a certain concept is, we need to be able state the general criteria on which that is evaluated. The fundamental question is, thus, a question of concept possession: What does it mean to possess a certain concept? Nersessian writes in accordance with Kuhn that 'having' a concept does not require that we believe that concept to be true of anything. For example, saying that Faraday had a field concept does not require us to assume that he could substantiate or that Faraday even thought he could substantiate the claims implied by that concept. One can have a concept without anything corresponding to it in the world. (Nersessian 1985, 177; 182)

Furthermore, Nersessian points out the kind of difficult problems that arise if we are not clear on the above main question. A general problem is how successive scientific conceptualisations are related to each other (Nersessian 1992, 8). Many historians attribute a field concept to both Faraday and Einstein. Yet, their thinking and theories were very different. Did they then really "say" or "mean" the same thing by their field concepts? It is problematic to choose the definition of the field concept. If we adopt a modern field concept, then nobody perhaps before Einstein had a field concept, which does not seem to be historically fair. One of Nersessian's main requirements is "to do justice" to the historical data (1991, 680). That is, not to subordinate the historical subjects to our pre-empirical views of language and

Meaning Change in the Context of Thomas S. Kuhn's Philosophy meaning. We may, on the other hand, allow that many scientists had a field concept, but then end up with the view that there are several field concepts. It is not clear why they all are called field concepts, and how they are related to each other. Elsewhere, Nersessian writes that it is possible to trace a pattern of descent for the concept of a field from Faraday to Einstein. Yet Einstein's views are so different from the views of all his predecessors - Faraday, Maxwell and Lorentz - that his cannot be an extension of any of theirs (Nersessian 1998, 160; 1984). And this seems to be a typical case in the history of science. We can find descendants for concepts without being able to show conceptual cumulation. Another example could be the concept of inertia from Galileo to Newton. What this shows, according to Nersessian, is the need to account for change in individual concepts in such a way as to accommodate continuous, yet uncumulative change (1998, 161). While the traditional philosophical approach, writes Nersessian, has viewed conceptual change as static and ahistorical, we need a dynamic and historical analysis of it (1992, 8). Yet, furthermore, we do not find only the descendance of concepts, but totally new concepts, such as 'spin', the disappearance of concepts, such as 'phlogiston', and cases of the partial absorption of concepts, such as 'ether' to 'field' and 'space-time'. In other words, we need an account of what happens in all these historical cases.

Nersessian is concerned about the situation where there is no explicit guidance on the theoretical level of how to individuate and locate concepts in the history of science. What is missing, according to Nersessian, is an explicit metatheoretical notion of what constitutes the meaning of a scientific concept. And even more alarmingly, she feels that this metatheoretical question is "at the core of historical method". (Nersessian 1992, 37) It is clear that this consideration has taken us into the problem field that is most important for the history of ideas and intellectual history: how to describe fairly and accurately the history of human thinking and its products. But, as Nersessian pointed out, the concern of individuating historical concepts implies important philosophical questions as well. This is something that could be adequately described as the philosophy of the history of thought. Yet, I am inclined to agree with Nersessian that the existing philosophical theories have not been too successful when looked at from the point of view of history writing (e.g. Nersessian 1991, 681; 1992, 10). The optimal case would be

such that we succeed in giving a satisfactory answer to the philosophical question that would then enable us to improve and sharpen our historical understanding. This is an ambitious but, nevertheless, a worthy goal to try to reach. And, importantly, because the underlying questions are philosophical by their nature, we are also able to take advantage of any philosophical theorising on the possession of concepts.

### ***Unit ideas and ahistoric concepts***

There are, of course, earlier suggestions on what concepts or ideas are. Arguably, the most famous and influential proposal was Arthur Lovejoy's project of unit ideas<sup>95</sup>. Lovejoy famously compared his unit ideas to elements in chemistry. Like a chemist, a historian of ideas tries to find and differentiate elementary units from heterogeneous aggregates and complexes. They are things that travel throughout time, reappearing in different times and contexts. Lovejoy also made clear that the number of these unit ideas is limited overall, although he never gave an estimation of how many there are. Lovejoy's further specifications on what unit ideas are do not, however, appear to be very useful, for the simple reason that his characterisation becomes very general. (Lovejoy 1965, 7-15; 1948, 9) Nevertheless, Lovejoy at times describes them as specific propositions or principles. This fits well with the description of unit ideas by another classic historian of ideas, George Boas, as declarative statements, assertions of belief and statements of fact or policy (Boas 1953, 4). Thus, Lovejoy's unit ideas much resemble Platonic ideas in a metaphysical

---

<sup>95</sup> Another non-analytic-philosophy tradition that might potentially offer a constructive insight with regard to the problematic in this chapter is the German *Begriffsgeschichte*. According to Melvin Richter, who wrote a book on *Begriffsgeschichte* (Richter 1995) in order to introduce the German conceptual history to the English audience, the choice of concepts as units of analysis in the history of thought distinguishes it from alternative similar methods focusing on other topics (4). *Begriffsgeschichte* makes a reasonable distinction between concepts and words (9). However, it is disappointing that it regards any more detailed definition of concepts as being unimportant and takes the notion to be useful exactly because of its ambiguity (21). It does not improve the situation that sometimes the notions of term and concept appear to be mixed in the usage. For example, Richter talks at times of "meanings of concept", without saying how it differs from meanings of terms, which might be taken as a concept. When saying that the German word '*Bürger*' denoted the citizen of a city in 1700, the citizen of a state in 1800, and someone who was not a proletarian in 1900, it seems we are talking of the denotation of a term, not that of a concept. (46)

Meaning Change in the Context of Thomas S. Kuhn's Philosophy sense, or the Fregean propositions (or perhaps components of propositions) in the sense that they are atomistic, indestructible entities, independent of time and space in a realm of their own, and yet reachable by the inhabitants of the Earth.

Today, the problems with Lovejoy's project are well known. There is no reason to dwell long on them. I will rather merely point out some of the major problems and indicate references to some scholars who have paid attention to them<sup>96</sup>. The first problem has to do with the ontological status of unit ideas. Namely, how conceivable it is to suppose that there are such platonic entities that appear and reappear in history which themselves do not change at all, and furthermore, use these to explain history? The study of history that is tied to a particular time, space, and context does not seem to have much use for such ahistorical entities (e.g. Mink 1968, 1969; Kelley 1990; Richter 1987). Secondly, and not surprisingly, it has not been easy to recognise such unit ideas. In fact, it has been argued that even Lovejoy's prototype idea, Principle of Plenitude, fails to be an atom-like unit idea, and therefore there are no unit ideas to be found in history (Hintikka 1987; see also Spitzer 1944). Thirdly, Lovejoy appears to overemphasise continuity in history. According to Lovejoy, most putative novel ideas are, in fact, rather new combinations or arrangements of unchangeable unit ideas (see Lovejoy 1965, 3-4; 15). The belief that normally there is nothing new in history is an issue that has been difficult to accept. Many commentators think that the history of ideas deals rather with something changing (e.g. Mink 1968, Spitzer 1944, Mandelbaum 1965). A related danger in looking for the same unit ideas is a resulting anachronism projected to historical agents (Mandelbaum 1965). Therefore, Lovejoy's project of unit ideas is thought to have such problems that in his study of the discussion on Lovejoy's *The Great Chain of Being* fifty years after its publication, Daniel Wilson concludes that the atomistic notion of unit idea akin to chemical elements "has been almost uniformly rejected" (Wilson 1987, 204).

Let me be clear on what I think is not under threat here. I think that, unless we change our point of view towards history in a drastic way, it is impossible to

---

<sup>96</sup> For a good survey of the discussion on Lovejoy's *The Great Chain of Being* over the fifty year period, see Wilson (1987).

Meaning Change in the Context of Thomas S. Kuhn's Philosophy

abandon talking of such notions as concept or idea, and of their individuation, changes and influences. In other words, I believe an approach that identifies concepts and ideas is fundamental to the study of the history of ideas and intellectual history. It is just so embedded in historical practice to write of histories, or to just talk of concepts of 'mass', 'evolution', 'liberty', and so on. If we cannot talk of such units, we are in danger of making the past unintelligible. If people did not share concepts, or some kind of units of information within themselves, and at times with us, how can we be able to work out what they thought? Each agent has to be defined as a special case without an assumption of conceptual continuity within us or across other individuals. It is natural to think that people also had concepts in the past that may have sometimes been the same ones as we possess. In order not to exercise this craft at all, we need a much more radical change in the fundamental thinking of our orientation towards the past<sup>97</sup>. I rather think that the real problem is not to dispense with such notions, but to ask how we should understand the talk of concepts and ideas in history. Furthermore, we may adopt a more philosophical point of view and argue that the postulation of concepts is unavoidable. They may be said to serve at least two very essential functions, as Smith and Medin write. We need concepts in order to give our world stability which would be rather chaotic without them. We do not give a name to each individual and take each object as unique, but treat particulars as instances of a concept that we already know. Concepts also allow us to go beyond the immediate information given. Once an instance is placed under a concept, we can infer what attributes it should have. (Smith and Medin 1981, 1)<sup>98</sup> To reiterate, the problem is to say what it is that we possess.

---

<sup>97</sup> Naturally, there are alternatives. For example, Quentin Skinner's theory on intellectual history stems at least partially from the rejection of any entities like ideas, concepts or meanings, and rather focuses on the uses and intentions of historical authors. Skinner thinks that the search for earlier anticipations of later doctrines and the debate on whether a particular doctrine really exists in a particular thinker, or at a particular time, are simply absurd. (Skinner 1969, 10-2; 36-7). Also, Foucault criticises Lovejoy's project in a very similar way in his *Archaeology of Knowledge* (160). Yet the question is whether either of these projects is enough to replace the historical accounts that rely on concepts or ideas. I tend to agree with Keith Thomas that Skinner's project is about the uses of concept, not the history of concept, and as such, it relies in fact on the notion of concept (Thomas 2005, 48).

<sup>98</sup> For more on the functions of concepts, see Thagard (1992, 21-24). Thagard lists ten roles that concepts have in human cognitive psychology.

There are some who have tried to pursue Lovejoy's project further by reformulating the key notion of unit idea. Moltke S Gram and Richard M. Martin defend Lovejoy from Hintikka's (1976) potentially devastating criticism that was intended to show that there are no unit ideas. Gram and Martin argue that there are indeed unit ideas, such as the law of excluded middle, but that they can have numerous formulations. What makes these various formulations instances of a unit idea is the fact that "there are always family resemblances sufficient to justify bringing them all under a common rubric" (Gram and Martin 1980, 510). Nils B. Kvastad writes that a historian of ideas is not "a spiritual chemist breaking up compound into irreducible units", but s/he rather analyses ambiguous terms, being sensitive to similarities and differences. He concludes in a similar fashion to Gram and Martin that the resemblances "can often be elucidated by means of Wittgenstein's so-called theory of 'Family Resemblance'".<sup>99</sup> However, I fail to see how the notions of unit idea and family resemblance could be compatible. The former implies that there is a definite unchangeable core, while the latter says that instances of a concept do not share a set of common features but are related merely by resemblance. We cannot have it both ways.

Nevertheless, I think that there is something attractive in Lovejoy's theory of ideas as units. Let us think of any putative historical concept - an 'atom', for example. What is it that makes it the concept that it is? A persuasive answer is that there is some conceptual core that makes 'atom' the concept it is, and furthermore, determines whether a particular instance is an instance of it or not.<sup>100</sup> Perhaps in this case it is a proposition, a 'minute indivisible portion or thing'. In this way we could recognise the existence of such a concept in ancient Greece, 17<sup>th</sup> century Europe and in 20<sup>th</sup> Century physics. In other words, Lovejoy's project spells out an intuition that in order to be identified as something, there has to be a core meaning that is shared by all that are the "same". We may generalise this conclusion by alleging that if we

---

<sup>99</sup> As quoted by Wilson 1987, 203. Nils Bjorn Kvastad's article, "On Method in the History of Ideas", should be in the *International Review*, 9 (1979). However, after repeated attempts, the librarians at the University of Edinburgh could not trace the article.

<sup>100</sup> Gram and Martin also point this out in their defence of Lovejoy's project against Hintikka's criticism. They argue that without the assumption that there is something like a unit idea, Hintikka

claim that two or more persons were interested in the same issue or thing - 'hybridisation', for example - it implies that there was something that all these people were interested in, in this case, hybridisation. This kind of reasoning leads us easily to the conviction that there has to have been an idea or a concept that everyone shared or possessed; that there are some kind of unit ideas that different people can be interested in and use in different contexts. Perhaps we could think that the existence of the same conceptual component is a minimal condition in order to say that people are or were concerned, possessed, thought, wrote etc. of the same concept. To put it another way, we might contend that the logic of writing the history of the "same concept" presupposes something like unit ideas. Taking all this into account, therefore, I argue that the case for unit ideas can be made, although it can still reasonably be asked whether that case results in satisfactory history writing. However, the postulation of such unit ideas can be seen to derive from the logic of history writing that requires something akin to a conceptual core, rather than their mind independent existence. I come soon to the point regarding in what sense concepts can be said to exist.

Perhaps we could use philosophical theories of possession conditions of concepts to substantiate the view of what it means to possess a certain concept. While the following theory is not created for the purpose of the history of ideas and intellectual history, I do not see why, in principle, it could not offer ingredients for a theory of concepts also in a historical context. Christopher Peacocke has advanced a theory of concepts that uses possession conditions to individuate concepts. The main idea is roughly the following. A concept is determined by an account of the capacity of a thinker who masters the concept to have a propositional attitude to contents containing that concept. The capacity Peacocke is talking about is that of possessing a concept. In other words, to possess a concept a person has to find certain inferences "primitively compelling". Peacocke's example is the concept of 'conjunction' C. A person possessing that concept must find the following transitions primitively compelling:

---

cannot locate environments that an idea P is supposed to have been asserted, assumed, or repudiated (Gram and Martin 1980, 508-9).

p		
q	pCq	pCq
----	----	----
pCq	p	q

Peacocke's theory is quite complex overall, but the main idea of possession conditions is enough for our particular interest here. (Peacocke 1992, 6)<sup>101</sup>

It is important to point out that Peacocke wants clearly to dissociate his concept from mental and prototype theories of concept. His concepts are, just as Lovejoy's ideas are, Platonic entities in a realm of their own, which Peacocke finds indispensable (see Peacocke 1992, 13-4; Ch. 4). He and others working in a similar fashion do really want to keep any anthropocentric connotations at arms length.<sup>102</sup> Another noteworthy issue is that philosophers that have adopted this orientation are rather more interested in what stays the same, than in what changes. Peacocke even claims that the concept in his sense is a requirement in order to describe a case as one of change, rather than as one of replacement, because something has to persist in the change. (See Peacocke 1992, 3; see also Diez 2002, 20). Applied to history, this expresses Lovejoy's intuition of the need for conceptual continuity that manifests

---

<sup>101</sup> Harman has put forward a very similar idea. He also emphasises the role of inferences that are "immediate". According to Harman, the rules of immediate implication and exclusion might be used to characterise meanings of logical constants. For example, the logical conjunction 'and' would be defined as that sentential connective C, such that for any propositions, P and Q,

P,Q logically imply C(P,Q)

C(P,Q) logically implies P

C(P,Q) logically implies Q. (Harman 1986)

<sup>102</sup> See for instance Peacocke 1992 (3, 13). Jose A. Diez has tried to develop a theory for the individuation of scientific concepts that relies on Peacocke's idea of possession conditions. It does become rather complex because he argues that the content of a theoretical concept C of a theory T has five different components: lawful-formal, applicative, observational, operational and folk-ancestry. In itself interesting theory becomes at times rather suggestive through repeated reminders that it is neither "suspicious of socio-psychologism, at least not in any bad sense", nor "dangerously sociologistic" (Diez 2002, 23-4). It gives the feeling that there is some underlying story left untold.

Meaning Change in the Context of Thomas S. Kuhn's Philosophy itself in unit ideas. This is naturally something that a representative of cognitive history and philosophy of science would not agree about, but I believe this basic intuition is correct, as we will soon see.

We might, then, say that while recognising that historical concepts have to have some continuing core, Lovejoy's account of unit ideas, via Peacocke's theory of concept possession, is able to substantiate the intuition and provide a theory of how to individuate concepts. Unfortunately, there are still problems that make this project an unlikely candidate for a theory of historical concepts or ideas. Firstly, a problem with Peacocke's theory is that it operates at such a high level of abstraction - his examples are such concepts as 'red' and 'conjunction' - that it makes it difficult to see whether his theory could actually be used for individuating concepts in the history of science that are much more nuanced and complicated. Diez's (2002) attempt does not seem to make the situation much better either. Further, we saw that some critics of Lovejoy's account thought that it overemphasised continuity and also that Nersessian's account took the problem between continuity and discontinuity as being central in intellectual history. I think that Lovejoy's account or any other project of unit or other ahistoric ideas or concepts, inevitably hides discontinuities in the history of thinking and is, therefore, one-sided. Even though it is important to realise that a meaningful study of a historical concept has to show continuity between different representations, it is not yet enough to have a meaningful conceptual history because discontinuities and changes in thinking are just as interesting and necessary to take into account.

### ***Cognitive history and philosophy of science***

A point of criticism in Lovejoy's theory that I did not discuss further in the last section is that unit ideas are ahistoric and non-natural beings. It is, of course, not possible to give a simple answer to the question of what historical concepts are

---

Perhaps it is just a reflection of deep antipathy, inherited from Frege, Russell, Popper and others,

Meaning Change in the Context of Thomas S. Kuhn's Philosophy ontologically. However, we ought to take seriously the concern expressed by historians that Platonic entities do not come across as being meaningful, although the ontological question is surely secondary from a historian's point of view. Furthermore, the supposition of Platonic entities is a problem if one is inclined to accept the view, as I do, that everything has to be a natural being explainable by sciences. With regard to ontology, my suggestion is to take concepts as mental representations, as Nersessian and Kvastad have argued.<sup>103</sup> For the purposes of history writing, it is better to stick to entities that reside in time and place. More importantly, I fail to see a compelling philosophical reason that makes the acceptance of non-natural entities necessary. Therefore, the preference is to commit only to naturalistic ontology and more generally to naturalism. As a consequence, the existence of any supernatural entities outside the domain of science is rejected. Whatever concepts turn out to be, they have to be subject to natural and scientific explanations.

A rival account to Lovejoy's that adopts such a naturalistic point of view is what Nersessian calls cognitive history and philosophy of science (cognitive HPS). Nersessian offers us an account that does not suffer from the overemphasis of continuity. A reader of this thesis is already generally familiar with the orientation in question, because it is the same overall tradition that Andersen, Barker, Chen and Thagard represent. However, Nersessian adds something more to what has already been said. She appears to extend the cognitive perspective, so that in addition to applications to concept acquisition and extension determination, her perspective also includes theoretical problems in the history of ideas and intellectual history.

The cognitive HPS sees the problem of individuating concepts as part of the wider representational problem, which leads them to rely extensively on cognitive psychology (Nersessian 1992, 37). The cognitive HPS adopts the prototype or probabilistic view of concepts. The general claim is that the representation of a

---

towards anything psychological or sociological.

<sup>103</sup> Gram and Martin appear to continue Lovejoy's tradition in respect of ontology. At least they do not dissociate themselves from Lovejoy's problematic ontological commitments. Kvastad, in turn, says clearly that he does not want to commit to Lovejoy's Platonic ontology, and therefore, not to

Meaning Change in the Context of Thomas S. Kuhn's Philosophy

concept is some sort of measure of a 'central tendency' of the properties of its instances. On the probabilistic view, each feature of a concept has an assigned weight based on the total number of instances that have it. The weight reflects the probability that an instance has that particular feature. Moreover, the more highly weighted features an instance has, the more typical and more similar to other instances of the concept it is. There is no distinction between essential and accidental properties but only a difference in the degree of weight, and the weighting is, moreover, liable to change. The upshot of the suggestion of the cognitive HPS with regard to a new metatheoretical notion in the history of thought is that "the overlapping set of 'similarities' or 'resemblances' makes a concept into a unit, entitles us to call it *the* 'Y', and enables us to write its history". It is, in other words, a family resemblance concept. Furthermore, Nersessian claims that it fits well with analyses of the historical or 'dynamical' dimension of meaning in scientific theories: "It can allow for development, change and continuity in a way the 'classical' conception cannot". A consequence is, for example, that we can say that "there are a number of different concepts of electrical and magnetic action, each of which is a 'field' concept." (Nersessian 1985, 180-1)

Before proceeding any further, we may ask whether Kuhn's development of the family resemblance theory might after all offer us as much as that of Nersessian's, a theory of what the metatheoretical notion in these fields is. I do not think this is so. The reason is that Kuhn's theorising on family resemblance is focused on determining extension, while we are concerned with the possession conditions of concepts. Furthermore, that is something that Nersessian, in her suggestion regarding the probabilistic account of concept, also overlooks. Unfortunately, her suggestion hides the difference between an understanding of instances as objects in the world that fall under a concept, and as other (historical) representations of a master concept that is a representation itself. The probabilistic or prototype account of concept is a summary of features with weight reflecting the probability that an instance has that property. A concept is constructed on the basis of

---

understand ideas as "extramental" concepts. (Kvastad 1977, 158.) In other words, Kvastad's interpretation is that unit ideas should be understood as mental concepts.

Meaning Change in the Context of Thomas S. Kuhn's Philosophy

real physico-material instances, and thus, deals with the application of concept to individual objects in the external world. The question of what features instances of a concept have, and whether there is a unique set of features that characterise all instances, are separate from the question of what it is to *possess* a concept. To possess is to have a certain representation whether or not that representation instantiates anything in the world. How could we assign a weight, let us say, to those features with which ancient scientist characterised the concept of atom? The comparison is not to be made to the world by trying to find out how many instances fulfil this description, but to other representations of the concept of atom, such as in physics in the 20<sup>th</sup> century. Further, it is not clear how we should understand features. As already indicated, it cannot be those features that are born out of observing instances. For example, I may possess a concept without knowing anything about the actual features of instances of that concept. I may have a concept of planet, for example, without having constructed it by observing real planets, nor knowing whether features that I attribute to planets are actually possessed by planets. What is more, there are a lot of abstract concepts that cannot possibly have been constructed by observing their instances, and nor do they have any cognised relation to their putative instances. Naturally, it would be possible to understand concept as a second-order relation to its instances. That is, the extension of a historical concept is all those (historical) representations and not objects that fall under the concept. Nevertheless, this does not change the main point that the question here is of concept possession and representations, without the concern of their relation to the mind-independent world. All in all, without some further qualifications, Nersessian's account is not satisfactory for the purposes of history writing.

Furthermore, Medin and Smith write generally about categorisation that to have a concept of X is to know something about the properties of entities that belong to the class of X, and further, such properties are used in categorisation (8). In relation to the probabilistic account, they say that a representation of a concept, i.e. a summary of features of instances, is used whenever a decision is made of the membership in that concept (62). This seems to suggest that to possess a concept a person has the whole summary description in the mind. But this is implausible. It is not likely that people have a long list of features in their minds when they apply a

Meaning Change in the Context of Thomas S. Kuhn's Philosophy concept. If this is accepted, does it mean that people cannot be said to possess concepts properly that they nevertheless use without effort?

Yet, the most severe problem stems from the logic of history writing. The problem with Nersessian's theory becomes very apparent if we compare it to the one by Lovejoy. One may wonder whether the family resemblance account is in accordance with the pre-empirical intuition of meaning in the context of history, because it allows, in principle, a situation where the "same" concepts do not share any common features. If everything has changed and there is nothing that they share, why would we want to call them the same concepts? Wouldn't it be better just to call them different concepts? If the concept of 'atom' in Ancient Greece is *nothing* like the one in contemporary physics, is there any reason to regard them as variations of the same concept? We should note that we are not interested in the history of terms, but in the history of concepts. The same linguistic appearance should not confuse two different concepts represented by two tokens of the same term as instances of the same concepts. Lovejoy realised this desire for the conceptual core and tried, therefore, to find some elementary units that were, in addition, the most fundamental in the Western intellectual history.

Nersessian's problem is, thus, the opposite of Lovejoy's. Her theory makes the history of concept as history of *concept* questionable because of its strong emphasis on discontinuities. The biggest problem for Lovejoy's unit ideas from a historical point of view is not, to my mind, to find "ideas" like that in history, but that it, on all counts, does not seem to be a very fruitful way to study history. If we have defined this pre-empirical unitary or analytic meaning and point out different contexts where it can be found, we may merely be justifying the view we had in the first place, instead of empirically investigating the thinking of historical agents. By defining 'atom' in such a narrow way as above, we may be able to write a history of "unitary" or "analytic idea" but be able to tell very little of what else people thought in history. Nersessian, on the other hand, can illuminate very well how different thinking was in the past, but she is in danger of putting things together that are conceptually totally dissimilar and disconnected.

On a more general level, this is the problem of continuity and discontinuity in the history of thought. That is, in what circumstances should we talk about conceptual change and in what circumstances of conceptual stability? We are faced with the following dilemma: we may include too little or too much in the conceptual content, and on both accounts we are driven to a problem with the historical description. If we include too little, and thus make our definition of a concept very general, we may find a lot of continuity in history. The result may be that the same concept has appeared and been used in various contexts. But then we run the risk of writing rather uninformative history, as argued above. For example, if we define 'element' as a chemically indecomposable body, we find it both in Lavoisier, who is thought to have initiated a scientific revolution, and in Stahl, who was one of the main architects of the pre-Lavoisier alchemical chemistry. Consider the following example. Stahl makes a distinction in his *Philosophical Principles of Universal Chemistry* between physical and chemical principles and says that the former are such that "a Mixt is really composed" and the latter are such "into which all Bodies are found reducible by the chemical operation hitherto known" (Stahl 1730, 4). The latter definition is strikingly similar to Lavoisier's words in *Method of Chymical Nomenclature* in that "we shall content ourselves here with regarding as simple all the substances which we cannot decompose; all such as we obtain in the last result from chymical analysis" (Lavoisier 1788, 12) or in *Elements of Chemistry* in that "if we apply the term *elements* or *principles of bodies*, to express our idea of the last point which analysis is capable of reaching, we must admit, as elements, all the substances into which we are able to reduce bodies by decomposition" (Lavoisier 1793, xxii). The question is whether it is fair to say that they had the same concept. The other option is to make a definition much more precise and detailed so that it will shed light into differences in their thinking. For example, Lavoisier, unlike Stahl, did not think that the same elements are found in all bodies<sup>104</sup>. If, for instance, that is

---

<sup>104</sup> Note that Stahl also made a distinction between specific and general. "The *generical* is the combination of *Principles* numerically and essentially different ... The *Specific* being unknown to the understanding, depends upon the different figure, number and situation of the *material Principles*" (Stahl 1730, 10). Only in the former sense are elements or principles the same in all bodies: "But as the four *Peripatetic Elements*, howsoever understood, cannot have place if supposed specifically the same in all Subjects; so neither can *Chemical Principles*: for no-one has hitherto

Meaning Change in the Context of Thomas S. Kuhn's Philosophy included in the concept of element, we are able to say that Lavoisier brought a conceptual change. But the disadvantage here is that we may suppress continuity at some level just because there is change at some other level. This is a problem with what Nersessian highlighted through the concept of field. If we adopt a modern definition, then nobody had a field concept before Einstein. In contrast, if we understand 'field' as understood by Faraday, his successors may not have possessed a field concept, but something else. The moral of this discussion is that if we try to give a precise definition of what a concept is, then the recognition of both change and continuity are just as problematic. The postulation of change omits continuity, and the postulation of continuity omits change. And yet it seems that both conceptual continuity and discontinuity are real and important phenomena. The simultaneous description of them does not seem to be easily available, given the tools we have.

### ***Conceptual continuity and discontinuity reconciled***

Now, it is clear that, unlike for Lovejoy, the description of discontinuity is not a problem for Nersessian. She also has an answer to the situation where two representations of "the same concept" do not share anything in common. Nersessian maintains that concepts have a continuing character by being inherited from one group of individuals to another. In the case of several changes that makes two concepts rather, or even totally different, we may still regard them as part of the same historical succession, if we can prove a historical connection from the first version to the last. Nersessian utilises Shapere's idea of "chains of reasoning". That is, reason connects a successor's idea or concept to its predecessor. Scientists who develop "the same concept" are related by a reasoning that leads them to improve, change and modify their predecessors' representations (see Shapere 1982, 21; see also Shapere 2001). We should understand this as the case where successive scientists or scholars are familiar with the conceptual products of their predecessors, i.e. they use their

---

pretended to shew that these *Principles* are specifically the same in all bodies. But if consider'd only as to their *generic qualities*, they may be allow'd in *Compounds*" (Stahl 1730, 4).

Meaning Change in the Context of Thomas S. Kuhn's Philosophy representations to form new representations. The established connection via the minds of scientists is enough to link concepts historically, and thus sameness can be reduced to the continuing, but changing character of the conceptual content.

However, I do not believe Nersessian's solution is enough for what we need, i.e. a historically viable theory for describing conceptual changes. We can accept that linking two representations by scientists proves the case for historical continuity between them, but it is not enough to say that there are two representations of the same concept. A person can study a certain conceptual representation and be inspired to develop something radically different, in which case we would surely not say that the concept that inspired the person is somehow the same as the one that results out of this activity. Again, as Lovejoy's theory implies, the exemplifications of the same concept in history do need to have at least something in common.

The solution to the problem of how to describe historical concepts so that neither continuity nor discontinuity is suppressed can be found by taking the best parts of both accounts. Firstly, let us accept that there has to be a minimal common component before we can categorise two concepts in history as instantiations of the same concept. But let us also take on board Nersessian's and the idea of other cognitive scientists that concept cannot be defined simply by giving it an unambiguous set of features that all instances share. Further, the suggestion that concept can be taken to have a structure where change in the conceptual content can be described as change in the component parts is also worth taking seriously. But note that from the latter commitments, the family resemblance account does not yet necessarily follow. What we can do is to understand that each instance of a historical concept has some minimal, "necessary", shared conceptual component, but not a set beyond the necessary component that would characterise all instances. In other words, something has to be shared between concepts that are the "same", but there is no exhaustive or explicit definition of the covering concept. The minimal component thus guarantees membership, but does not function as a definition of the concept. Definition in any historically illuminating sense is unattainable, because the out-of-core elements of any historical covering concept vary, and as a consequence, no definition can capture the full conceptual content. As already pointed out, I am not

claiming that it is necessarily self-evident what the necessary component of a historical concept is, as its determination may involve subjective evaluation and depend on a specific historical interest. What I am claiming, however, is that postulation of a shared conceptual component between all instantiations of the “same” historical concepts is a (logical) requirement for writing an intelligible history of *concept*.

The fundamental reason for suggesting this is that I believe it is historically the most viable. We can view the concept of ‘atom’ as having a conceptual core, which is something that people who used any exemplification of it had in their minds, but it is impossible to go beyond the minimal definition and draft an exhaustive definition due to the fact that all the other components keep changing.<sup>105</sup> One may ask: “What about a case, should it happen, where there is simply nothing shared between a concept and its distant origin?” I think in such a case we have to make it clear that these two representations are not “the same concepts” any more. Let us imagine that a certain instance I of a historical concept C can be satisfactorily characterised by the following features: A, B, C and D. Further, two other instances I<sub>1</sub> and I<sub>2</sub> can be described respectively as follows: A, B, D, E and A, C, E, F. In this example, the feature A represents the necessary component. It also takes into account other relevant (changing) features (B, C, D, E, F). The suggestion above is thus able to describe both conceptual continuity and discontinuity. A further instance I<sub>3</sub> would not be a member of C, if it takes the form: B, C, D and F. The reason is that it lacks the common necessary component. Whether such cases of discontinuity arise in the historical succession is an issue for historical debate and verification. Nevertheless, now we can understand Nersessian's demand for dynamical concepts that can

---

<sup>105</sup> Papineau and Bird have advanced a theory which postulates an intensional core that stays the same in theory transitions, allowing a descriptive determination of the same reference (see Papineau 1996; Bird 2000, 189-191). Although this theory appears to resemble the one offered in the text, there is a crucial difference between them. Bird's and Papineau's theory is designed to find a minimal intensional content that is true in two or more theoretical contexts (thus) guaranteeing the stability of reference. I am interested in an association (between a term and a feature that characterises it) that stays invariant between different users of concept, irrespective of its truth-value or reference. In other words, Papineau and Bird are not interested in the history of thought *per se*, and therefore their theory is not suitable in the context of this chapter.

Meaning Change in the Context of Thomas S. Kuhn's Philosophy  
change, transform, appear and disappear. Nersessian's requirement makes sense because most of our beliefs that characterise concepts are certainly not static.

There is also an interesting further application that is based on an understanding of concepts as being structured. Thagard has offered a theory of how new concepts can arise. According to his theory, they do not arise from experience or by definition, but by conceptual combination. A new concept is a non-linear and non-definitional amalgam of existing concepts. That is possible because concepts have components that can be combined to form a new concept. Some values or components may be overdriven by others. If we think that a 'feminist' is politically more active than a 'bank teller', then in the concept 'feminist bank teller'<sup>106</sup> we have a contradiction. In that case the political activity of the former takes precedence over the inactivity of the latter. As a result we do not have a contradiction but a new concept that is an unusual combination of qualities. This theory cannot be studied further here but fits well with our discussion of conceptual change and simultaneous continuity and discontinuity in a conceptual change. More importantly, it indicates the way to proceed in formulating a theory of concept emergence and modification. (See Thagard 1984)

However, there are two objections to this account that have to be dealt with. The first asks whether, taking into account that there are innumerable beliefs that can be used to characterise a concept, we are supposed to take all of them as constituting the conceptual content. If the answer is yes, we risk accepting the extremely problematic doctrine of holism<sup>107</sup>. If not, how do we limit the set? The answer is that when talking of a concept or the meaning of a term in this sense, it is meant to include the central or most important beliefs associated with a term or in the conceptual content. The problem with this answer is that it seems to force us to find

---

<sup>106</sup> Thagard's (1984) example.

<sup>107</sup> In the holistic view of content, traditional psychological explanations become problematic. For example, if all beliefs are part of the content, it is hard to see how two people could ever share the same thought, or how they could ever agree or disagree about the same issue, and how anybody could change his/her mind. Arguably, our systems of beliefs are not identical, and hence, our beliefs that are dependent on the entire system have different meanings. Reasoning for a change of mind becomes hard to understand because a certain belief that I first accept, then later reject, receives a different meaning because its rejection brings a change in the system of beliefs. (e.g. Block 1998)

some principle that limits the set. It has to be said that the principle that is out of the question is analyticity. This is not primarily because it is such a problematic principle, as demonstrated convincingly by Quine (see Quine 1964), but because we are certainly not talking of anything that is true, whether in virtue of meaning or otherwise. Rather, we are talking of idea associations that may be assertible if a person knows the concept or the meaning of the term, although they may not be in any way true<sup>108</sup>. Further, we may grant that the notion of central belief is vague because it is hardly possible to draw a sharp line between central and non-central beliefs. Nevertheless, the distinction is meaningful, as much, for example, as that of 'bald'. Admittedly, determining what the central beliefs are involves an element of subjective evaluation. Often they are decided on pragmatic grounds, i.e. those beliefs are highlighted that a person is interested in studying. Yet, it is true that some associated beliefs are more significant than others even though it is hard to agree which exactly. There is also another way to limit what beliefs are taken into account. If the model of frames that stores beliefs to make a prototype is accurate, it gives some criteria for selection. Namely, a frame has a hierarchy (see Chapter 4). For cognitive reasons an appraisal of the number of beliefs, what the beliefs are and in what order they are associated with a term, are obviously limited. It may be an important association of 'dog' that they are furry but much less important that they have a heart, although it may also be possible to deduce the latter indirectly via the postulation that dogs are animals. The latter feature is much less important for the stereotype of dog in this frame structure.

The second problem concerns intersubjectivity. Although not a universally accepted view, many philosophers (especially many of those trained in the analytic tradition) would argue that beliefs are private, which cannot as such be shared by anybody else<sup>109</sup>. How can we guarantee that other people can have the same thoughts as we have and that our thoughts can be subjected to public examination? A popular

---

<sup>108</sup> An interesting theory along these lines is Ned Block's idea of conceptual role semantics. He writes that inferential roles are best understood as narrow contents, i.e. belief contents in a psychological sense, which themselves are never true or false. A contrast is wide contents that have to do with the relation between a belief and the world, and have, therefore, truth-values. (See Block 1993, 1998)

answer is to assume that there are non-psychological propositions that can be shared and whose meaning can be defined truth-functionally. A person is thought to be related to a proposition by a belief and by other so-called propositional attitudes. If we adopt this view, we can begin talking of sets of propositions that terms are related with and that equate privately to beliefs. Similarly, we may talk of a change in the set of propositions that a term is linked with. The existence of propositions is naturally a difficult metaphysical problem although most philosophers appear to accept them in some form. For the naturalistic perspective adopted in the thesis, we do not accept propositions as Fregean non-natural propositions. Another solution might be to postulate some kind of verification procedure that guarantees that people entertain the same beliefs. Let us take a term, a public shared linguistic entity, to imply certain associated assumptions which can be verified by the public use of language. For example, an understanding of the expression 'bird' would require an associated belief that it is feathered, and for this reason, prompt, if asked, the inference 'it has feathers'.<sup>110</sup> I do not want to extend the examination of what possibilities there are for guaranteeing intersubjectivity. Both of these approaches are possible in

---

<sup>109</sup> Cf. Wittgenstein's account of beliefs. He would arguably deny that any sense has been attributed to the idea that beliefs are private and probably contend that philosophical questions about beliefs, for example, should be studied by examining various uses of the word 'belief'.

<sup>110</sup> An example of a theory that uses the intersubjective criteria is Cesare Cozzo's "epistemic theory of meaning". His theory relies on some primitive inferences that a person has to be capable of inducing if s/he can be taken to understand an expression. The crux is that Cozzo's "primitive epistemic property" P, that is at the same time the meaning constitutive property, is such that a person attaches it to an expression E. According to Cozzo, there is no need to give a justification of P, and deviation with regard to P indicates a lack of understanding of E. For example, in order to understand the word 'room' (E), Cozzo writes that a person has to be able to associate the primitive property P<sub>1</sub> to the expression which can be represented as an inferential schema:

X is in a room

P<sub>1</sub> -----

There are walls around X.

In other words, a person has to be able to draw the relevant inferences in order to master the expression. In this case, s/he has to be able to infer from the sentence 'Tom is in a room' that there are walls around Tom. More importantly, a failure to use an expression according to P<sub>1</sub> is a sign of an inadequate understanding. The sense of a word is roughly, then, the set of all primitive epistemic properties one has to attach to that word. (Cozzo 2002, see also Cozzo 1994).

Meaning Change in the Context of Thomas S. Kuhn's Philosophy principle.<sup>111</sup> The intersubjectivity of private beliefs is a problem of its own in philosophy but not a main concern of this thesis.<sup>112</sup>

### ***Meaning change of 'element'***

Kuhn said in his article on Boyle (see above) that Boyle's work was directed against the "chymical" understanding of 'element', a substance that would not be decomposable and transformable. Let us continue the investigation of 'element' and examine what kind of change it later undergoes. An important change in the conceptual development is thought to occur at the end of 18<sup>th</sup> century due to Lavoisier's examinations. Yet there is a sense, as we saw above, in which we could say that the contemporaries of Stahl and Lavoisier all shared the same concept. That is, we may take the concept of 'element' as a kind of unit idea expressing merely something like 'an undecomposable and materially homogenous body'. Therefore, because there is a minimal conceptual content connecting these representations, we may say that in the narrow sense they shared the same concept, and moreover, that it is still used in modern chemistry.

Let us focus our attention, then, on what beliefs changed. We can see that there are many important features that Lavoisier did not attribute to elements that his predecessors did. Firstly, elements were thought to be ultimate constituents of bodies. Secondly, elements, or principles as they were sometimes called, were understood to be responsible for the observable qualities of bodies. An example could be the famous 'phlogiston' that was supposedly responsible for combustion and sometimes also for the firmness of bodies. Thirdly, elements were assumed to be found in all bodies. In general, then, this summarises some of the central features of the concept 'element' before Lavoisier's chemical revolution. A closer examination,

---

<sup>111</sup> A third potential possibility is the position that Hacking sees adopted, for example, by cognitive scientist Medin. That is, concept is more than a set of ideas in the mind of a single individual, but is something less than Platonistic, being something between Frege's associated ideas and *Begriff*. (Hacking 2001, 478-9) This would naturally need much more elaboration.

<sup>112</sup> Cf. Popper's view of intersubjectivity in the philosophy of science (Popper 1997, 45-7).

Meaning Change in the Context of Thomas S. Kuhn's Philosophy however, might naturally bring out some variations in respect of individual philosophers or chemists. As a micro study, it would be possible to proceed, as Perrin (1988) has done in investigating documented responses in the French chemical community over a couple of decades, to Lavoisier's chemical revolution and try to draft an account of the continuous change in the associated beliefs or in the conceptual representation of a community. Nevertheless, these three components can be taken as expressing those features that a person typically had in mind when s/he employed the concept of element at the beginning and mid-18<sup>th</sup> century.

We may think of Lavoisier trying to prove that the bodies that were regarded as elements were not undecomposable. Lavoisier's studies with combustion showed that the prime example of an element earth - diamond - could actually be reduced to air. His further studies with combustion brought him to the famous rejection of phlogiston and gave him evidence for the view that air is not an element, but is composed of two gaseous substances, oxygen and nitrogen. Furthermore, Lavoisier proved, apparently prompted by the experiments of Henry Cavendish, that water is not an elementary substance but is also composed of two "airs". All of these results, combined with the generally confused state of chemical knowledge, gave him the impetus to renew the whole chemical nomenclature. Lavoisier, thus, demonstrated that two elementary substances had turned out to be composable and that they were definitely not the ultimate components of bodies. In general, we may say that Lavoisier abandoned the feature of ultimate constituent and replaced it with a conditional understanding of elements as being so far undecomposable<sup>113</sup>. By the same token he did not think that the same elementary substances can be found in all bodies. Finally and most controversially, Lavoisier abandoned the view that elements are directly responsible for the observable qualities of bodies because, in his view, they combine to produce new chemical substances that are not similar by their qualities to their components. This is controversial because Lavoisier was obviously not free from attributing such powers to some simple substances.<sup>114</sup>

---

<sup>113</sup> On this, see, for example, Oldroyd (1973), Gough (1988) or Siegfried (2002).

<sup>114</sup> To see this, it is enough to think of the name 'oxygen'. Although it was for him an element in the sense that it could not be divided into simpler ones, it also had the property that the older elements had: it caused directly something to happen in bodies. Lavoisier thought that all acids were combined

With regard to meaning or conceptual change, we need first to note that, in addition to the term 'element', the main conceptual content stayed invariant, although many other beliefs changed. Because of the continuity of the conceptual core we can justifiably say that we are talking about instantiations of one concept. But then we can also describe the meaning change of 'element' in 18<sup>th</sup> century chemistry as a change in the set of beliefs associated with that term or in the concept element. What is more, although not essential for conceptual membership, it is not difficult to show that the set of beliefs associated with 'element' after Lavoisier is a successor of the set held by his predecessors. Lavoisier started working with the set of associations constructed by his predecessors in mind and proceeded to change those belief associations. There is a clear historical link.<sup>115</sup> In conclusion, we can say that Lavoisier's concept is a variation of the same concept as the one that preceded it. There is much discontinuity, as we have seen, but also a crucial continuity in the general idea of undecomposability and the focus on the same problematic that his predecessors faced, i.e. what are bodies composed of? I think this is an answer to Nersessian's concern about writing the histories of concepts so that they illustrate both continuity and discontinuity.

### ***Referential change and incommensurability reconsidered***

In light of the above interpretation of meaning and conceptual change, it is appropriate to look again at the problematic passage about referential change by

---

by the combination of non-metallic substances with "eminently respirable air", which he renamed "principe acidifiant" or "principe oxygene". Oxygen was, thus, the substance that acidifies. (Mackie 1935, 217; see also Lavoisier 1790, 24). The same underlying idea is also implicit in the concept 'caloric' because caloric was taken as the principle of heat and expansibility.

<sup>115</sup> In his early career in the 1760s, two of perhaps the most well-known French chemists were Pierre-Joseph Macquer and G.-F. Rouelle. The lectures of the latter were popular in the community of chemists; according to Siegfried, nearly everyone who later became chemically significant in France attended them. Rouelle's doctrine was built on the framework of Stahlian chemistry, but Rouelle himself did not publish. Both Siegfried and a Rouelle-expert, Rhoda Rappaport, think that Macquer was the most influential in spreading Rouelle's view. (Siegfried 2002, 133-4; Rappaport 1960, 77) We know that this was Lavoisier's scientific environment and that, as a young scientist, he also attended Rouelle's lectures. Therefore, we can assume that Lavoisier's scientific thinking at the beginning of his career was Stahlian.

Kuhn and also to consider Kuhn's notion of incommensurability. As we recall, Kuhn suggests that references of 'space', 'time' and 'mass' change in the transition from Newtonian to Einsteinian physics:

The variables and parameters that in the Einsteinian  $E_i$ 's represented spatial position, time, mass, etc. still occur in the  $N_i$ 's and they still represent Einsteinian space, time and, mass. But the physical referents of these Einsteinian concepts are by no means identical with those of the Newtonian concepts that bear the same name. (Newtonian mass is conserved; Einsteinian is convertible with energy. Only at low relative velocities may the two be measured in the same way, and even then they must not be conceived to be the same.) (*SSR*, 101-2)

This is probably one of the most problematic passages in all of Kuhn's texts. We should, nevertheless, find an appropriate interpretation of it although practically no interpretation is without problems, either in terms of the philosophical position or in terms of Kuhn's intended message or terminology. Sankey (1994) advocates the view that Kuhn implicitly accepted wide descriptivism that implies that any theory change allows, in principle, a referential change. The underlying idea is that the sense of a term changes in theory change because it equals the description implied in a theory. This may result in the situation where two senses of the same term are incompatible; therefore, they both cannot be true of the same entity. Bird's (2000, 2002) interpretation is along the same lines with the specification that Kuhn's intensionalism was "thick" and "strict", i.e. intension or sense depend on a wide range of theoretical assumptions that have to be true in order for a term to refer to a property. Both Sankey and Bird appear to think that the idea of two different references is incompatible with realism, which takes 'time', 'mass' and 'space' to refer to one property or entity in both contexts. Furthermore, Kuhn's claim of two references might be taken as an indication of idealism or constructivism, i.e. that there are two separate worlds before and after paradigms. Or if we wish to avoid idealism, then it is still implausible that there exist two similar but separate properties or substances to which respective terms refer. For that reason, Bird (2002, 458-9) concludes that Kuhn must have used "referent" in the above passage in a weak sense to mean something like "internal reference"; i.e. reference is not an entity existing

Meaning Change in the Context of Thomas S. Kuhn's Philosophy independently of the theory, but is the hypothetical entity posited by the theory that might not actually exist. Understood this way, its meaning is not much different from sense or intension.

In fact, it might be possible to retain the idea of two separate references without falling to idealism or constructivism. For example, Field (1973) has suggested that terms may partially denote entities, while in the Einsteinian revolution, terms underwent denotational refinement: Newton's mass partially denoted relativistic mass and proper mass, and it went under refinement in the Einsteinian revolution to denote only the latter. However, it is questionable whether we actually can perceive that such terms as 'time', 'mass' and 'space' refer to some entities or properties. I agree with Bird that Kuhn did not think that these terms have real references in the world. For Kuhn there is no single property of being mass, time, and space there that stays constant in theory changes. The terms may be understood, rather, as denoting something similar to conceptual categories without corresponding references. Interestingly, in the latter part of his career Kuhn defined his position as a Kantian with movable categories (e.g. *RSS*, 104; 207; 264). The difference to Kant is naturally that Kuhn's categories, whether they are called a lexicon or mental module, are historical and subject to modification. Nevertheless, from this "neo-Kantian" perspective, Kuhn's changing 'mass', 'time', and 'space' can be taken as categories that organise experience.

We have above the ingredients for an interpretation of the passage, although it is necessary to reformulate and express it a bit differently. The first step is to think of meaning change as the change between two representations or sets of ideas. The following quotation, already used in Chapter 2, sheds more light on the matter:

The laymen who scoffed at Einstein's general theory of relativity because space could not be "curved" – it was not that sort of thing – were not simply wrong or mistaken. Nor were the mathematicians, physicists, and philosophers who tried to develop a Euclidian version of Einstein's theory. What had previously been meant by space was necessarily flat, homogenous, isotropic, and unaffected by the presence of matter. ... Consider, for another example, the men who called Copernicus mad because he proclaimed that the earth moved. They were not either just wrong or quite wrong. Part of what they

meant by 'earth' was fixed position. Their earth, at least could not be moved. (*SSR*, 149)

We ought to understand that Kuhn is talking about two different conceptual contents here, not of references, and this can be applied also to the quotation above. If Kuhn is historically correct, the concept of space that scientists working in the Newtonian paradigm possessed can be characterised by the following features: flat, homogenous, isotropic, and unaffected by the presence of matter. These beliefs changed in the Einsteinian revolution. Yet, it is likely that they also retained some core idea of space. I believe something like this is implied in many studies that try to study in practice how meaning and thinking have changed over the course of history. We find hardly any technical notion of meaning or concept in most historical studies. And it was a historical study that Kuhn conducted in *SSR*. On the other hand, 'meaning' in this sense could be termed stereotype or prototype because it is about what people would typically believe is true of entities called by a certain name. Notice also that the proposed theory can nominate 'meaning' to proper names because there are certainly beliefs associated even with proper names. That is why we can make sense of the above idea that 'earth' had a different meaning in the Ptolemaic and Copernican paradigms. Many philosophical theories, the causal theory above all, would deny that there is any meaning beyond the individual that a term refers to.

In addition, in order to be consistent with Kuhn's philosophy, we may take these ideas as forming a category that will be used in naming future objects. Secondly, because the ideas between two theories appear to be incompatible, just as Bird and Sankey write, they cannot be true of the same entities. They have to be true of some other entities or not to refer at all. This is to say that it makes sense, in principle, to think that Kuhn had implicitly accepted something like a wide descriptive theory of reference, whether or not he thought that the terms in question actually refer anywhere. It is however important to understand that, if this interpretation is chosen, Kuhn did not consciously postulate or inherit "a descriptive theory". It is rather that it seems to elaborate on something that appears to be implied in the passage under consideration. To put it bluntly, it is merely "a theoretical

Meaning Change in the Context of Thomas S. Kuhn's Philosophy construction" because it is unlikely that Kuhn thought that those terms actually refer: if radically different sets of ideas characterise whatever is called by the same names, this may be expressed in the form that *putative* references have to be different entities. However, for Kuhn, these entities were nothing other than them being putative, i.e. they did not exist. Therefore, it is extremely important to understand that the passage deals with a historical interest regarding the change in thinking of scientists; for this reason, these putative entities are best taken as mental concepts. But this leads one to ask whether it is at all reasonable to talk of references or descriptive theory of reference. Perhaps it would be a better interpretation to understand that the question is rather of the relationship between a term and a concept. It might be better if we use and Kuhn had used 'denotation' here instead of 'reference': i.e. a denotation of the term changed to a different concept, or to the same concept that had undergone a change in some part of it.

Finally, I wish to proceed still further and now look at the notion of incommensurability. My interpretation is something that Kuhn did not explicitly suggest, but it fits well with his philosophical and historical rationale. Let us suppose that there has been a drastic change in scientific thinking, i.e. there has been some kind of scientific revolution. As a consequence, the beliefs of scientists have changed, although scientists often keep using the same terms as earlier, as Kuhn writes. Now the situation is that the same terms are applied in two different ways. Alternatively, the beliefs associated with the same terms are rather different. Let us also suppose that the revolution has been wide enough to cover a large area of interrelated terms. The result is that scientists from two different paradigms use the same vocabulary, even the same sentences, but they imply very different assumptions by them. In that situation it is very difficult in a practical sense to find appropriate translations, because an incident of incomprehension cannot be straightened easily by related vocabulary due to the fact that they imply other different or even incompatible beliefs. It is then, as Kuhn writes, that participants have to become translators. This implies a recognition of differences in their discourse and in those terms and locutions that are used problematically, so that they then can resort to their shared vocabulary in order to achieve comprehension. According to Kuhn, that may result in the situation where one manages to describe "the world" of the other in his

Meaning Change in the Context of Thomas S. Kuhn's Philosophy vocabulary. However, the price to pay may be that the sentences translated are very long and complex, or that they require a great space for the deliberations of the other. Nevertheless, this is something that Kuhn thinks that a historian of science has to do. (SSR, 202-3)

Sharrock and Read appear to emphasise the same point. According to them, Kuhn tried to show that translation between two historical theories is so difficult because assertions associated with the same term differ and the matched sentences would lack the same "feel". Word-for-word translations are often unable to retain "the same detailed set of interconnections". They also use Kuhn's example of the difference in meaning between Newton's and Einstein's 'masses' to point out that respective terms imply different assertions. In addition, Sharrock and Read give a more mundane example that may shed some further light on the issue. They ask us to think of the difference between 'association football' and 'rugby football' or 'American football'. In the latter the expression 'It's over the bar' means 'He scored' but in the former one 'He missed'. In order to find the differences in meaning of these expressions in these contexts we need to discover what kind of assumptions there are in the background. Sometimes a faithful translation of even one sentence would require a large-scale exercise in history and it might take the length of a book to explain what is missed between the two expressions. (Sharrock and Read 2002, 64; 150; 168) To summarise, this indicates that incommensurability can be understood as a practical difficulty (but not as an impossibility) in understanding and achieving comprehension, rather than as a logical impossibility of comparison. In other words, it is an explanation as to why "the proponents of competing paradigms are always *at least slightly* at cross-purposes" (SSR, 148; italics mine).

## **Conclusion**

At the beginning of this chapter I argued that Kuhn was at first preoccupied by very traditional questions regarding the history of ideas. Although we do not find much theorising on the history of ideas later in his writings, there is a fundamental

Meaning Change in the Context of Thomas S. Kuhn's Philosophy

theoretical issue at the heart of this inquest. I agreed with Nersessian that an answer to the question of what it means to possess a concept is of fundamental importance with regard to the history of thought. One of the most influential answers to the question of what the basic theoretical entity in the history of ideas is was given by Lovejoy. He proposed that there are atomistic unit ideas that travel throughout history. Several historians have criticised the theory, but I maintain that Lovejoy's insight is significant. His project implies that, if two concepts are instances of the same historical concept, then there has to be some common core between them. More generally, this may be taken as an expression of something that the logic of history writing requires. One possibility to explicate this notion is to take concepts or ideas as concepts in the fashion of Peacocke. Yet it is true that unit ideas tend to concentrate merely on continuity in history and can be seen, therefore, as one-sided. Nersessian's approach, in turn, compares similarities between different feature combinations and counts different sets as representing the same concept if there is a historical connection between them. The problem with this theory is the opposite to that of Lovejoy's, in that Nersessian emphasises discontinuity to the extent that it is possible that instances of "the same concept" do not have anything in common.

My suggestion is, first of all, to take concepts, in contrast to Lovejoy and in agreement with the cognitive HPS, as natural beings, as psychological or sociopsychological entities. As regards definability, I argued that, historically, the instances of the same concept do need to share a necessary minimal conceptual component, but that, beyond the conceptual core, there is no set of features that is common to all exemplifications of a certain historical covering concept. The minimal component guarantees conceptual membership, but it is not a full and exhaustive definition of the covering concept. Continuity and the description of different representations as the representations of the same concept is made possible via the minimal component, while discontinuity may be described as a change in the out-of-core beliefs.

Furthermore, Nersessian's idea, and that of other cognitive scientists, is to understand concept as a stereotype with a structure, such as a frame, that can be used to describe variation in the concept. In this way we can portray simultaneous

Meaning Change in the Context of Thomas S. Kuhn's Philosophy  
continuity and discontinuity. I illustrated meaning change by a change in the meaning of 'element' or the concept element in the 17<sup>th</sup> and 18<sup>th</sup> century chemistry. Although we may maintain that the core idea of an indecomposable material body has remained invariable, many other central beliefs have altered.

I offered also an interpretation of Kuhn's idea of referential change and incommensurability, illustrated by the transition from Newtonian mechanics to that of Einstein. Referential change emerges from the situation where the relationship between the terms 'space', 'mass', and 'time' and the associated ideas or conceptual representation change. If the ideas that characterise putative entities are incompatible, they cannot, in principle, be true of the same entities and have to, as a consequence, refer to different objects or fail to refer completely. However, it is better to understand that Kuhn was talking about conceptual change and denotation to a concept rather than reference to a mind-independent entity. Furthermore, if the alteration in the set of associated ideas is drastic and wide-encompassing enough, translation becomes hard to achieve because it is difficult to catch all the assumptions implied by the same terms and sentences. Incommensurability is, then, taken as the practical difficulty of achieving comprehension and translation between two different theoretical frameworks.

## 7. Historical perspective

There are still a couple of important questions to raise here in this penultimate chapter before concluding. It is fair to say that the question of comparability brought Kuhn's notions of meaning and meaning change into focus in philosophy. For that reason, it is an issue that needs to be addressed. The standard solution to concentrate on in fixing reference cannot be our Kuhnian solution, because the idea of referring to an invariant universal property or kind is in contrast to Kuhn's thinking. And even though it is true that theories may be compared by their extension composed of individuals, I believe that even this approach would produce an overly simplified image of what actually occurs in science. More importantly, my message is that references are not even necessary in order to have comparability. That is to say that there are many ways to compare theories. Kuhn himself indicated that there are several criteria that may be used in comparison and theory choice. I show that fears of the "anything goes" approach can be abandoned even in the absence of a point-by-point comparison.

This takes us to a foundational issue regarding Kuhn's philosophy. An advocate of a referential solution to the problems of meaning change relies on the idea that the truth values of two propositions, whose concepts refer to the same entity, can be determined, and hence we come to know which proposition is true and which false. Presumably s/he does not wish to relativise truth to a theory, language, or to anything else for that matter. However, this position faces a serious problem, because virtually all propositions in science that have been judged to be true have later turned out to be false. Any proposition that has been accepted could not have been literally true, if truth means correspondence to the mind independent reality. A realist would probably try to salvage the situation by employing the concept of verisimilitude. That is to say, propositions do not need to be exactly true; what is important is that later propositions are nearer to truth. This implies that at the end there is one true description of reality that science is approximating at the moment. In

any case, I think that for a scientific realist, proving an argument for increasing verisimilitude is indispensable. And importantly, the view that science converges to truth could indeed be a correct view, which means that this possibility has to be taken seriously.

Kuhn's philosophy is interpreted as naturalistic in this chapter. I begin the chapter by showing that 'truth' as a non-relativised notion applied in the context of the history of science is a non-natural concept. It is something that is not in the reach of practising scientists by natural means, and therefore, cannot be a criterion for choosing a theory. I will, secondly, take a look at Kuhn's "developmental perspective" on the history of science. This is roughly the following idea: Scientists are part of a certain historical niche. They inherit their predecessors' world view and start reforming it from that point on. The history of science is a dynamic changing process rather than an accumulation from a static foundation. The focus ought to be on the reasons for changes of beliefs rather than on the justification of beliefs as such.

The historical perspective takes us, then, to the issue of comparison. According to Kuhn, scientists, like everybody else, are tied to a certain historical moment and they can only use for evaluation what is accessible to them. One point is that there is clearly not a crucial test, such as falsification. On the contrary, the history of science proves that scientists are epistemologically conservative. Further, there are elements in Kuhn's philosophy that makes his epistemology fit well with coherentism. Kuhn named five criteria that can be used in evaluation that all contribute to achieving greater success in problem-solving: consistency, accuracy, scope, simplicity and fruitfulness. Problem-solving can be understood as a part of the general aim to improve the coherence of theories. That accommodates Kuhn's idea that theory choice is a comparative process because we can take it as a comparison of the degree of coherence between competing theories. Moreover, conservatism in history may be explained by the fact that coherence is easier to maximise by avoiding drastic changes. Epistemological conservatism also appears intuitive if we further assume that there is something like a conceptual scheme or some other system for information organisation, because it is psychologically difficult to make

Meaning Change in the Context of Thomas S. Kuhn's Philosophy comprehensive changes in large systems. However, there also appears to be a problem because the five criteria of evaluation used in theory choice cannot all be connected unproblematically to the concept of coherence. Yet, this is not a problem if they enhance problem-solving and problem-solving can be linked with the concept of coherence, which makes them all at least indirectly related to it.

The fourth major theme in this chapter is progress. Even if scientists could not know what propositions are true, given that we are in the right relation to the world, increasing coherence might take us to truth, as Charles Sanders Peirce indicated, for example. One difficulty of this view is that there are problems in formulating the notion of verisimilitude. My argument, as implied in the earlier chapters, is that the question of whether the idea of convergence *in the history of science* is viable should be decided ultimately on empirical grounds. I will not study here specific arguments for increasing verisimilitude in the history of science, but rather take a look at some of the preconditions that need to hold if those arguments are going to succeed. That is, if within our chosen parameter theories show stability and continuity in the history of science, then it is at least possible to construct an argument for increasing verisimilitude. In contrast, if there is no discernible stabilisation and continuity over time, we cannot hold such a position. Kuhn argued that historically the case is the latter one.

### ***Kuhn's historical perspective***

Sharrock's and Read's Kuhn in their *Kuhn – Philosopher of Scientific Revolution* is a Wittgensteinian therapeutic philosopher who tries to get rid of unsolvable philosophical problems. Even though this description does not quite fit Kuhn for the reason that Kuhn clearly had philosophical ambitions, which Sharrock and Read also note (see 223, note 3), they make a plausible and an interesting claim regarding Kuhn's attitude to science along these lines. Sharrock and Read think that Kuhn's message was that the issues of 'what is really there' or 'whether science represents reality' can only be answered in the context of a scientific controversy itself. These

Meaning Change in the Context of Thomas S. Kuhn's Philosophy questions turn to queries regarding whether this or that paradigm represents reality best – questions that can only be answered by further scientific controversy and scientific work. The question whether science represents reality is not, according to Sharrock and Read, a philosophical question of science, but a “misleadingly abstracted version of specific scientific questions”. (Sharrock and Read 2002, 205) If we then ask the typical questions that an advocate of a referential solution asks, such as ‘What substance remains constant in theory changes?’, then we actually take part in scientific controversies. The problem whether something is, and so always was, a planet or a star, and whether something else is really a compound and not a mixture are questions that science answers. Obviously, the answers to these questions have been different at different times. The upshot is that to try to say what is actually there, and has always been there, is to take part in scientific arguments. For that philosophy does not give any special vantage point. (Sharrock and Read 2002, 57)

I do think that Sharrock's and Read's view is right. The philosophy of science cannot give meaningful answers to such questions independently of science, but is forced to replicate the contemporary science and its results. Arguably, contemporary science has not said the final word on reality and is open to correction. But in that case to judge the history of science from our perspective with some definite conviction can historically only lead to Whig-history mentioned earlier and to an unhealthy bias in interpreting the activity of the past science in terms of the present science. Yet I would not try to force Kuhn into “a conceptual box” of a therapeutic philosopher, but rather look for Kuhn's philosophical argument which nevertheless points to a kind of deflationary view in the philosophy of science. If we take this approach, there is much more to say about Kuhn's specific historical epistemological perspective.

Let us first see what Kuhn said of the notion of truth in the history of science. There are two essays, both published in the 1990s that are of special interest here: “The Road since Structure” and “The Trouble with Historical Philosophy of Science” (in *RSS*). In these, Kuhn presents a familiar criticism of the correspondence theory of truth. According to Kuhn, in “the previous tradition” beliefs were evaluated for their truth or for their probability of being true, where being true meant something like

Meaning Change in the Context of Thomas S. Kuhn's Philosophy  
correspondence to the real or mind independent world. He notes that seldom or never can we carry out such an evaluation directly to reality. (*RSS*, 114) As a proof of impossibility, Kuhn cites what has become known as pessimistic metainduction in philosophy: all past beliefs have turned out to be false; therefore, no belief we currently hold is likely to be true, no matter how strongly we believe in it. (*RSS*, 115)

I think Kuhn's remarks above make a noteworthy point. How could a scientist know whether his belief is true if truth is correspondence to the non-relative reality? It is reasonable to think that none of us is in this privileged position that allows us to tell absolute truths from falsities. Consequently, a scientist cannot know whether his/her beliefs are ultimately true or false in a correspondence sense for the reasons that s/he simply does not have access to reality that would tell us what beliefs are indefeasibly true or false. Being true, therefore, cannot be a criterion to choose between theories in science. And as long as we are concerned with how actual scientists function, we cannot refer to truth as an explanatory notion in their decision on theory choice. That would make a completely non-natural claim involving the access of a privileged being to reality. This may sound as if Kuhn were a sceptic. Yet note that Kuhn does not want to abandon the notion of truth; even less does he think that it would be impossible rationally to compare theories. At one point Kuhn even wants to defend the notions of truth and knowledge from "the excesses of postmodernist movements" (*RSS*, 91).<sup>116</sup>

The impossibility of having access to truth in Kuhn's thinking can be explained by his "historical perspective" (*RSS*, 113), "developmental perspective" (*RSS*, 91), or "developmental view" (*RSS*, 95). Kuhn describes it as follows: The historian picks up a process already under way without resort to the beginning. That perspective provides an analogy for the evaluation of scientific knowledge. For each scientist, beliefs are already in place and provide the basis for an ongoing research, which may result in changes in the body of accepted beliefs. There is no ahistorical

---

<sup>116</sup> It is not, though, clear what Kuhn's favoured notion of truth is. At one point he is inclined to accept "a redundancy theory of truth", but then he says that "the essential function of the concept of truth is to require a choice between acceptance and rejection of a statement" (*RSS*, 99). The first expression hints at a minimalist or deflationary theory, but the latter statement is more compatible with a verificationist or pragmatist definition of truth.

Archimedean platform, "outside of history, outside of time and space" (RSS, 115), available to scientists to be used in evaluations. And so scientific claims are evaluated from "a moving, historically situated, Archimedean platform". No-one is free of the vast amount of accepted beliefs which are changed only when needed:

Creatures born into it [the world] must take it as they find it. They can ... interact with it, altering both it and themselves in the process, and the populated world thus altered is the one that will be found in place by the generation that follows. (RSS, 101-2)

A consequence is that the focus is not on the evaluation of beliefs themselves, but "what's to be evaluated is the desirability of a particular change-of-belief" (RSS, 95-6) or "understanding small incremental *changes* of beliefs" (RSS, 112). On one occasion Kuhn compares the traditional investigation of "the rationality of belief" to his proposal to focus on "the rationality of incremental change of belief" (RSS, 112).

The main point in a historical perspective is clear enough. Both scientists and historians are part of the historical process that began before them. There is no way for the agents to step out of their historical niche and know whether a certain belief or a theory is true in any absolute sense and should, therefore, be accepted because of that. Everyone is bound up with a certain historical situation involving the relevant theoretical body that is used for the evaluation of knowledge, and the evaluation is thus carried out against the body of accepted beliefs. If we want to know why scientists choose certain beliefs or theories, the answer cannot be that they knew that they were true. This means also that any description given by the scientists is tied to a particular historical situation and to the language or, perhaps, 'lexicon' that they use.

That a neutral description is not available is of course a moral of the post-empiricist era after the belief in observation as a neutral judge has been abandoned. However, it is important to note that it is still possible to maintain that, no matter what criteria scientists actually use in choosing theories, and whether they and their theories are culturally infected, the criteria used in theory choice function so well because they make a difference, unbeknown to scientists, between true, or truer, and

Meaning Change in the Context of Thomas S. Kuhn's Philosophy  
false beliefs. For example, "a critical scientific realist" would adopt a position like this (e.g. Niiniluoto 1999, 91). I leave discussion on that for later in the chapter.

It is easy to see that Kuhn's historical perspective is incompatible with an historical explanation that tries to explain changes in history by direct reference to reality, or how things "really" are. We remember how Bird thought that it is not obvious that there has been a shift in extension since the times of Ptolemy's astronomical system. He argued that what has changed is merely what people believed to be the extension. People once called the Sun a planet, but do not do so any more because they had the wrong belief that the Sun behaved like Mars, Venus, etc. in having a large orbit about the centre of the local system. And so "if the Sun is not in the extension of 'planet' now, it never was. People can be mistaken about the extensions just as they can be about references." (Bird 2000, 160-1) The problem with this idea from Kuhn's historical perspective is that it implies that we know what is really in the extension of planet. That is, we have in some way arrived at true beliefs of the world that allow us to form a "natural" category of 'planet' and judge history from that vantage point. Consequently, the crucial question is whether we actually are in a position to know that.<sup>117</sup> We may ask and not only in the Kuhnian framework, in what way could we go beyond our beliefs and beyond "what we think is in the extension"? What if future generations think, just as we think of past generations, that they had certain false beliefs that led them to the wrong categorisation? I think that in the light of the history of science we have to accept that that scenario is at least possible. And if this is so, we should refrain from explaining the history of science from such a metaphysical point of view. If we do not refrain, all that we are doing is creating a Whig-history of our own day that Kuhn objected to so much.

---

<sup>117</sup> Among a number of other distinguished philosophers, Bird advocates externalism in epistemology that rejects the so-called "K-K-principle", i.e. the principle that in order to know I need to know that I know. According to externalism, I can have knowledge even if I am not able to consciously justify my belief should I, for example, have a reliable method in use. If we now overlook any shortcomings of externalism, that position does not help here, if one makes the claim what is "really" in the extension that past scientists missed. That implies that we know that we know, and is inconsistent with externalism. (e.g. Bird 1998, 216-8)

In conclusion, Kuhn's historical perspective wants to equip those who evaluate the history of science only with what is actually available for them. They cannot evaluate the past science by assuming that they know what is definitely true and false. They, as well as scientists of the time, are tied to the conceptions of their time of what is true and false. Truth as correspondence is something that is not applicable in explaining why scientists choose how they choose, at least not at any conscious level. To use truth as an explanatory notion in this context would presuppose an extra-natural access to truth. One way to put this is to say that in contrast to many contemporary philosophers, Kuhn wishes to emphasise the importance of epistemological questions in comparison to metaphysical ones. We cannot fix metaphysics if we do not have some access to reality, and Kuhn argues that that is exactly the case. In addition, Kuhn wants to leave judgement of what certain entities ultimately are to scientists, who are actually engaged by their profession to determine that. It is unlikely that a philosopher or historian could improve such an opinion. Finally, we can see that Kuhn's historical perspective and his deflationary attitude towards metaphysics make him a minimalist and a naturalist<sup>118</sup>. He wants to leave to science as much as possible; in addition, all explanations used are natural. That view is further strengthened if we remember how Kuhn's views on similarity relations, concepts and information organisation can be

---

<sup>118</sup> I am aware that in two of his last writings Kuhn said that historical philosophers, he included, "overemphasised the empirical aspect" (*RSS*, 95) and the conclusion drawn from the historical record "can be derived instead from first principles" (*RSS*, 112). It appears that here Kuhn is taking steps away from naturalism. I think Bird is right in saying that Kuhn desired to argue more philosophically and his work became gradually more philosophical (2002, 2004, 2005). However, I do not take his rejection of the causal theory as a rejection of naturalism, as is made clear in Chapter 5, although his failure to follow research in cognitive science is deplorable. If we look at the context of these expressions we can see that Kuhn tells us that historical philosophers attacked the logical empiricist view on the basis of history, but that it could also be done merely on philosophical grounds, simply by argument. What he sees as important is that we could arrive at a general view of the history of science that would be more difficult to refute. I accept that this was a misguided and incoherent step because otherwise his philosophy stays well within the bounds of naturalistic philosophy.

Why Kuhn wanted to point out this is a different question. I believe that there are two factors that played a role here. Throughout his career Kuhn wanted recognition as a philosopher and this may have been one more attempt to earn it. He probably considered a priori reasoning that is independent of the empirical world profoundly philosophical. Furthermore, Kuhn attempted to distance himself from certain empirical traditions that he had helped to initiate but did not approve of: Kuhn called the Strong Programme of Sociology of Science "an example of deconstruction gone mad" (*RSS*, 110) and an example of "the excesses of postmodernist movement" (*RSS*, 91). Needless to say, the Strong Programme has not always been received favourably among philosophers of science. Attacks on it and associating Kuhn with this empiricist genre surely had an effect on Kuhn's thinking.

Meaning Change in the Context of Thomas S. Kuhn's Philosophy given a characterisation that can be seen to have received empirical support in cognitive science.

### ***Criteria for theory comparison***

Now we come to the question of what ways there are for comparison if we adopt the historical perspective. It is clear that Kuhn does not advocate a referential solution to the problem of comparison<sup>119</sup>. Before drafting a solution that Kuhn brought forward, it is worth noting that referential invariance is not necessary for comparison, even if one wants to stay in the traditional framework. It is possible to compare theories via extension using set theory. Michael Martin argued that even if two terms, 'B' and 'B\*', in two hypotheses,  $B_a$  and  $B^*_a$ , derived from respective theories  $T_1$  and  $T_2$ , have different meanings, they can be shown to contradict each other if the extension of 'B' in  $T_1$  is a subset of the extension of 'B\*' in  $T_2$ . So comparison is possible, even in the absence of the same reference via individuals and their sets. (Martin 1971; 1972)

Nevertheless, comparison via sets is not a solution that Kuhn endorsed. He instead suggested something that he saw would fit the history of science better. In the background there is a recognition that the idea that we can discern a precise point of contact between theories, and have a crucial test to show which theory survives, does not fare well historically. I think that the lesson from the Popper-Lakatos-Kuhn debate is that there is not an unambiguous falsification or any other crucial test to be

---

<sup>119</sup> To complicate matters, Kuhn says in "Theory Change as Structure Change", which is an evaluation of Stegmüller's book on Sneed's formalism, published in 1976, that "Comparing theories ... demands only the identification of reference" (RSS, 190). Yet, as we saw in Chapter 5, Kuhn did not think that identification of reference and the causal theory of reference can offer us a point of comparison with natural kind terms or common nouns. That was because we cannot "trace life-lines of natural families". These statements are not contradictory. It also became clear that Kuhn was initially impressed by the Putnam-Kripke theorising on reference determination and held that, regarding proper names, Putnam and Kripke had proposed an interesting and valuable theory. The above sentence can be thus taken as recognition of the fact that in the case of proper names, comparison is possible via reference, although unfortunately this is not often applicable in science. Kuhn says in the passage that identification is "very difficult" and one may never be absolutely sure of success in it. It is also

Meaning Change in the Context of Thomas S. Kuhn's Philosophy found. Lakatos' already mentioned case was about how scientists did not abandon Newtonian mechanics when their calculations of the motion of Uranus did not match the actual movement. Instead they looked for an ad hoc explanation that saved the original theory (Lakatos and Feyerabend 1999, 68-9). Another example could be the famous abandonment of the phlogiston theory. It was a long and a patchy process involving many experiments over a long period by several scientists<sup>120</sup>. Or think of Joseph Black and the Edinburgh chemists for whom it took a long time to accept Lavoisier's theory, even when the evidence for it was already strong. They nevertheless converted<sup>121</sup>, unlike Joseph Priestley, who held on to the Phlogiston theory for the rest of his life. In addition, we can also mention Ignac Semmelweis who produced a good case for the existence of something like "cadaverous particles", "morbid matter" or "decomposing animal-organic-matter" that is primarily transmitted on hands rather than through air, and he also developed an effective means for preventing transmission. However, it took several decades for medical scientists to come up with a germ theory of child bed fever. Semmelweis' method is sometimes used as an illustration of Popper's hypothetico-deductive method at work, but it certainly does not prove that scientific communities would use a method like that (even if some individuals do), rather the contrary<sup>122</sup>. It would be possible to

---

probable that Kuhn's opposition to the causal theory was still in 1976 in formation and so he could not make a clearer formulation at that point.

<sup>120</sup> For Kuhn's account of this process, see *SSR*, Chapter 6.

<sup>121</sup> See, for example, Perrin (1982) and Kendall (1952) for analyses of the conversion by Black and the chemists in Edinburgh.

<sup>122</sup> There appear to be differing opinions on why Semmelweis' theory did not take root despite its proved practical success. For example, Loudon blames the man himself. According to Loudon, Semmelweis did not publish enough, even though he wrote several letters to eminent obstetricians of the time. In addition, he claims that Semmelweis' difficult temperament had a role to play because he could not stand criticism and for that reason made several personal attacks. Finally, he does admit that Semmelweis' theory may have appeared too simple and too radical with regard to the older theories. (See Loudon 2000, Ch. 7) György Gortvay and Imre Zoltán shift all the blame from the shoulders of Semmelweis in their (probably too) glorifying account of the man and his work. The reason for the resistance to his theories was "the scientific 'conservatism' of his contemporaries", and more generally the fact that his theories were incompatible with the prevailing views (Gortvay and Zoltán 1964, 165-7). Yet another view of the resistance is given by Sherwin B. Nuland. What prevented Semmelweis's studies from gaining prominence was that he did not carry out properly substantiated experiments, never "availed himself" of microscopic means to validate results and never described his results in important medical journals. A further contributing factor was his personality and the political climate of the time. (See Nuland 2003, esp. Ch. 6) It remains a fact that Semmelweis' theories were known by a large number of medical experts but, in spite of this, they were rejected by the medical community.

Meaning Change in the Context of Thomas S. Kuhn's Philosophy continue with examples where the problems with a theory, or good evidence against it, are brushed aside and retained in the old theory. This already shows that an old good theory is not easily abandoned, especially if there is no alternative already available. Kuhn's comment on this is apt: "if any and every failure to fit were ground for theory rejection, all theories ought to be rejected at all times" (*SSR*, 146). However, the main point I want to bring up is that the history of science offers us a picture of epistemologically more conservative scientists than most traditional accounts of theory comparison would allow.

We saw that Kuhn wished to concentrate, not on the evaluation of single propositions, but on changes of beliefs in the system of beliefs. Kuhn argues that in such a change most other beliefs stay untouched, and so the aim is to "incorporate, with minimum disruption, the new claim" (*RSS*, 96). The evaluation of the desirability of a change of belief is comparative. Judgement is based on the comparison of two theories or "two bodies of knowledge" and each is asked which is "*better* for doing whatever it is that scientists do". (*RSS*, 96)<sup>123</sup> Kuhn's view since *SSR* was that scientists try to solve puzzles and the choice between two theories turns, therefore, to the question of whether the suggested alternative manages to solve a puzzle that the old theory could not or whether it can solve more puzzles than the old one. Yet he also offers a more specific characterisation of criteria for the evaluation of theories. According to Kuhn, there is a whole set of apparently intertheoretical, or even interparadigm, criteria used in evaluation: accuracy, consistency, breadth of applicability, simplicity and fruitfulness. In the comparative evaluation of a change of belief we ask: "which of two bodies of beliefs is *more* accurate, displays *fewer* inconsistencies, has a *wider* range of applications, or achieves these goals with the *simpler* machinery" (*RSS*, 114). Kuhn presents on several occasions similar, yet slightly variable lists of criteria that can be used in rational comparison<sup>124</sup>. We can see here that the talk of Kuhn as an outright

---

<sup>123</sup> The idea that comparison is between two or more alternative theories, and is not just an abandonment of the one, is naturally already found in *SSR*. For example: "The decision to reject one paradigm is always simultaneously the decision to accept another, and the judgement leading to that decision involves the comparison of both paradigms with nature *and* with each other" (*SSR*, 77).

<sup>124</sup> In *SSR*, the criteria for comparison are problem-solving success, quantitative precision, novelty prediction capability and aesthetic value between the new and the old theory (152-155). In the

Meaning Change in the Context of Thomas S. Kuhn's Philosophy  
irrationalist is an exaggeration, because he seems to recognise some common theory comparison criteria.<sup>125</sup> Another issue is that the claim that there is no such thing as *the* scientific method does not raise such hostility and panic as when Feyerabend claimed that for the first time. Now when the Kuhn-Popper-debate has cooled, it is easier to recognise that, instead of one supreme method of science, there may be several values or good-making features that can be used in evaluation<sup>126</sup>.

### ***Kuhn and the coherence theory of justification***

We can say that the single most distinguishing feature in Kuhn's theory of theory choice is its conservativeness. That is both a conclusion from his studies of history and a natural consequence of his historical perspective. Further, it is reasonable to assume that the central concept of paradigm was at least partially born out of an observation that scientists commonly tend to stick to their old beliefs and theories even when they are faced with contrary evidence to their beliefs and theories. The theory is an anti-thesis of Popper's falsificationism where one negative result is reason enough to abandon a theory. As Kuhn said, the maxim in revising an old theory is to change it with "minimum disruption". In addition, we may say that falsificationism and other theories of clear-cut rejection have been discredited by the history of science. Let us see what other grounds there are, other than historical, for conservativeness in theory choice.

---

"Postscript" of *SSR* the list is: accuracy, simplicity, fruitfulness and scope (199). And in an essay at the beginning of the 1970s Kuhn gives five criteria for evaluating the adequacy of a theory: accuracy, consistency, scope, simplicity and fruitfulness (*ET*, 322-4).

<sup>125</sup> Note that Hoyningen-Huene points out that there is a subjective element in theory choice because the values on which theory choice is based may be interpreted or "shaped" differently by different individuals. However, it is unclear how differently they can be interpreted, and what the significance of their difference is. Importantly, this does not appear to make theory choice arbitrary because the shared values, however differently shaped, appear to lead to the same theory choices at the end. (See Hoyningen-Huene 1993, sections 4.3c; 7.4b)

<sup>126</sup> For example, Newton-Smith, who is definitely not an irrationalist, nor an anti-realist, recognises eight "good-making features of theories" (1981, 226-2). Bird wishes to add yet two more (Bird 1998, 263-4).

## Meaning Change in the Context of Thomas S. Kuhn's Philosophy

It seems that, yet again, cognitive science can be used in support of Kuhn's view. Paul Thagard writes that to know an area of physics, for example, is to have incorporated a very complicated structure over which one has little or no conscious control. This means that, from a cognitive science perspective, scientists have little choice in whether they are epistemologically conservative in science or not. The system of representation is not easily overthrown. Learning a theory requires adoption of the whole network of frames, or a very large and complex conceptual structure in order to solve problems and handle the flow of information. There is, therefore, a great utility in retaining an old system, which anyway cannot be controlled as a whole voluntarily. This explains why scientists do not abandon an old good way of thinking in the face of a few anomalies. On this basis Thagard formulates his principle of methodological conservatism: "If you have a frame system for domain, it is unreasonable to give it up merely because there is an available plausible alternative frame system". We might yet remind ourselves that in the absence of such an alternative, giving up is not optional but it is practically impossible unless we are prepared to accept that thought "grinds to a halt". (Thagard 1984, 248-9)<sup>127</sup> Note also that we do not need to talk specifically of frame systems. The commitment is only to an interrelational conceptual system. Finally, it is not unreasonable to take the idea of such restricting conceptual structure as an expression of Kuhn's paradigm concept.

The reason for scientists being conservative is, in plain language, that it is just very difficult to change one's thinking in any profound way, even if there were an alternative system available. Unless there is overwhelming evidence to reject the accepted system and choose an alternative, it is natural to be conservative epistemologically. Even in such cases where there is strong evidence in favour of a new theory, many scientists have not given up their old theories, such as in the case of the above-mentioned Priestley. Probably personal factors, such as commitment to a theory and age, as well as sociological factors, may explain why some change quite

---

<sup>127</sup> Bird, who also gives an interpretation of Kuhn by using the tools of cognitive science, appears to be in agreement (see Bird 2005, 122). Bird interpreted Kuhn's World-change thesis as psychological. Because our set of pattern-recognition capacities and habits of mind are so entrenched, changing them is very difficult, and so World-changes are rather rare.

Meaning Change in the Context of Thomas S. Kuhn's Philosophy easily while others hang on to the old theory for as long as they live. This comes close to Kuhn's idea, also known as Planck's principle, that paradigm change requires a generational change.<sup>128</sup> We can discern a grain of truth in that thought, although it is an exaggeration<sup>129</sup>.

Conservatism in theory choice is something that takes us smoothly to coherentism in epistemology. Gilbert Harman, who formulated a version of coherentism, has advanced "a principle of conservatism" that is similar to that of Thagard's: One is justified in continuing fully to accept something in the absence of a special reason not to (Harman 1989, 46). Moreover, Thagard suggests reasonably that Priestley, for instance, never accepted the Oxygen theory because he was the one who had the most elaborate Phlogiston theory and could appreciate its coherence, while he had never studied and used its rival enough to appreciate its coherence. The phlogiston theory was far from perfect, but so was the Oxygen theory. For example, Priestley pointed out that while the weight of phlogiston was never established, neither was the weight of caloric in Lavoisier's theory. (Thagard 1992, 59-60)

All in all, we can say that if we are coherentists in our knowledge evaluations, conservatism in theory choice is well-motivated. Coherentism is a matter of how coherent a set of beliefs are. Or it is a matter, as Bonjour expresses, of how well a body of beliefs "hangs together": "how well its component beliefs fit together, agree or dovetail with each other, so as to produce an organised tightly structured system of beliefs, rather than either a helter-skelter collection or a set of conflicting subsystems" (Bonjour 1985, 93). Suppose that such a highly coherent system is constructed. A person does not have an incentive to change it, should coherence decrease as a consequence. On the contrary, s/he has an incentive not to change it in such a case. And so, in the absence of strong reasons to change, or in the

---

<sup>128</sup> In *SSR* Kuhn quotes approvingly of both Darwin and Planck as they had laid their trust in the younger generation in accepting new revolutionary theories (*SSR*, 151).

<sup>129</sup> C.E. Perrin showed with his comprehensive study of the French chemical community that the majority of scientists converted gradually to Lavoisier's side, some fifteen years after his views started to gain awareness in the community. However, he found support for a weaker version of the Planck hypothesis because there was a sharp age difference between "the early enthusiasts" and "the stubborn resisters" of Lavoisier's theories (121). Some of the established prominent scientists never converted. (Perrin 1988)

absence of an alternative (more coherent) system, or of a change that improves the coherence of the old system, a person should not change his/her beliefs.

This naturally works in the other direction as well: if there is a good reason for a change, i.e. coherence can clearly be increased, then the system ought to be changed. Bonjour writes that achieving a high degree of coherence may require a significant conceptual change. In a case where one or more anomalies appear for which there is no satisfactory explanation available, then devising a new conceptual system may offer an explanation and thus increase overall coherence. I agree with Bonjour when he says that "in this way the progress of theoretical science may be plausibly viewed as a result of the search for greater coherence". (Bonjour 1985, 100)

Moreover, coherentism fits in general with Kuhn's insistence that we should concentrate on the justification for changes of belief, rather than on the justification of beliefs themselves. If our beliefs are holistically justified, i.e. the beliefs in a set mutually justify each other, then we have to understand the whole set as taken and try to improve it rather than to try to find a justification for singular beliefs on a one by one basis; or even less, to start the construction of a new system from scratch in order to look for the justification of beliefs piecemeal. Justification is dependent on such a large scale that it is impossible to proceed on a one by one basis while asking for a complete justification for each belief. The case is rather that when a belief is changed, the rest of the beliefs stay unchanged. For each change one has to ask for a specific justification, and improved coherence counts as such.

But let us take a step back and see why we are preoccupied by coherentism in the first place. The first thing to take note of is that coherentism is a theory of justification. Kuhn thought, as we saw above, that it is impossible for scientists to have access to truth. On this ground he plunged into the classification of values that can be used in theory evaluation by scientists. Kuhn is not alone in this strategy. Bonjour's starting point in his book on coherentism is that because we cannot be immediately and unproblematically accessible to truth, as God would be, then all what we have in hand is justification. (Bonjour 1985, 7) In other words, we do not know whether our beliefs are true or not, but we can know whether they are justified.

There are now, thus, clear indications that Kuhn's historical perspective can be accommodated to a coherentist epistemology and theory of justification. Furthermore, in light of the history of science it appears that scientists are conservative in epistemology and that may indicate, although of course not necessitate, that they are also coherentists. From the cognitive-psychological perspective the only reasonable strategy is to change the webs of belief minimally, which is a natural conclusion from the coherentist epistemology. But if coherentism appears to be our choice in epistemology, we urgently need to clarify further the notion of coherence from the mere intuitive level.

There are a few philosophers that have developed a coherentist epistemological theory. Motivations for developing vary, the most usual probably being the well-known problems with foundationalism that Kuhn also mentions as a rationale for his developmental or historical perspective (*RSS*, 95). My motivation, independent of the fact that it fits Kuhn's philosophy, is that it seems to be both the most reasonable account of how real people actually operate, and also an intelligible normative epistemological theory. I will explicate the notion of coherence with the help of Bonjour, who in his *The Structure of Empirical Knowledge* (Ch. 5) offers the clearest account of the notion that has not in general been adequately explicated in philosophy.<sup>130</sup> Note that I do not underwrite Bonjour's coherentist epistemological theory in its totality; the way he connects justification and truth does not appear to be reasonable. I will discuss this theme in the last section of the chapter.

The customary way to characterise coherence is by one or two conditions: consistency or/and explanatory unity. Consistency means that no coherent set can contain both beliefs that P and not-P. The latter condition states that each belief P is explained by other beliefs in the set. Consistency is also Bonjour's first condition for coherence:

---

<sup>130</sup> Other versions of coherentism are, for example: Keith Lehrer (2000), Paul Thagard (e.g. 1992, Ch. 4), Gilbert Harman (1989) and Nicolas Rescher (1973)

- (1) A system of belief is coherent only if it is logically consistent<sup>131</sup>

A further factor to be considered is a point of contact between components of the set. We need to require that there are some sort of positive connections between beliefs. If they are totally unconnected, we cannot say that the set is very coherent. A natural idea is that connections are inference relations. This means that there should be such relations that a belief or a set of beliefs can serve as the premise(s) of an argument for a further belief. Furthermore, if a system has subsystems, coherence is higher the more they are connected with each other. It is important to notice that both relations between beliefs inside a set and between sets that form a larger system can be understood to be a matter of degree. Bonjour formulates two further conditions for coherence, as follows:

- (2) The coherence of a system of beliefs is increased by the presence of inferential connections between its component beliefs and increased in proportion to the number and strength of such connections.
- (3) The coherence of a system of beliefs is diminished to the extent to which it is divided into subsystems of beliefs which are relatively unconnected to each other by inferential relations.

Bonjour argues that we need to highlight one kind of inferential relation. Explanatory relations, to which Harman and Thagard would reduce the whole notion of coherence, are of special interest. That is because in science and other epistemological tasks the goal is to explain a wide scope of area, including

---

<sup>131</sup> Bonjour points out that coherence should not be equated with logical consistency, i.e. the absence of explicit contradiction. It is a condition, but not enough for the whole characterisation. There may be cases where a consistent system does have a low degree of coherence. By using the notion of probabilistic consistency we may imagine a case where a person holds the belief that P and also the belief that it is extremely improbable that P. The set is consistent, although it would be more coherent if those two beliefs would be dropped altogether. Bonjour concludes from this that, in addition to consistency, probabilistic consistency is another feature to be taken into account: A system of beliefs is coherent in proportion to its degree of probabilistic consistency. I ignore here this specification, but that does not affect in any way the general point adopted here. (Bonjour 1992, 95-6)

Meaning Change in the Context of Thomas S. Kuhn's Philosophy phenomena of different kinds by a relatively small number of explanatory principles. A coherentist system tries to connect an anomalous event or fact by finding inferential connections between it and the rest of the system in the form of looking for an explanation for it. Anomalies threaten the status of the explanatory principles of the system as general and basic, and thus decrease the coherence of a system. Bonjour thinks, therefore, that it is advisable to add one more condition for coherence:

- (4) The coherence of a system of beliefs is decreased in proportion to the presence of unexplained anomalies in the believed content of the system.

This concludes our study of the notion of coherence. There are three criteria to be taken into account in determining the coherence of a system: consistency, the degree of inferential connections and the number of anomalous instances.

Now we come to the crucial part. We have to assess how epistemological coherentism agrees with Kuhn's characterisation of science as a whole, and specifically, how it agrees with the criteria used in theory choice. The feature of science that Kuhn elevated above any other in *SSR* is problem-solving. The ultimate criterion of success of a scientific theory is whether it is successful in problem-solving. It is true that sometimes Kuhn lists problem- or puzzle-solving next to other criteria of evaluation; yet, it is also clear that problem-solving is more important than other criteria. It is something that is the distinguishing mark of the whole scientific period, normal science. Later Kuhn also reiterates his view that puzzle-solving is "what scientists do" (*RSS*, 96).

That science is problem-solving is a conclusion one arrives at by taking a look at history over a long period of time. It is, thus, a metahistorical view expressing what the nature of science is on the whole. If we exclude the problem-solving that can be used to characterise science both on the micro and macro levels, the other criteria that are supposedly used in theory choice are descriptions on the former level only, i.e. they are applicable in the actual situation where scientists choose theories.

## Meaning Change in the Context of Thomas S. Kuhn's Philosophy

In Kuhn's philosophy, the usage of these standards contributes to greater success in problem-solving. For example, Kuhn sees that improved quantitative precision, i.e. accuracy, makes a theory likely to succeed better in problem solving than its older competitors (*SSR*, 153-4).

My suggestion is that problem-solving is a natural component of coherentist epistemology. The connection between coherence and problems (or anomalies) is obvious. Problems, i.e. unexplained phenomena by the machinery of the system, mean that there are less inferential relations between the components of the system or that inferential relations are weak in it, with the consequence that the whole system is less coherent than the system without problems or with fewer problems. A potential difficulty with this suggestion is that Kuhn's criteria - consistency, scope, accuracy, simplicity and sometimes also fruitfulness - do not all appear to correlate directly and unproblematically with a higher degree of coherence. The least problematic is consistency because the concept can be directly explicated by it. However, remembering Bonjour's characterisation of coherence above, both scope and simplicity can be taken as values that enhance the degree of coherence in a system. This is because simpler or more powerful principles are used in explanations and the wider the scope they explain then the less subsystems are needed. Less subsystems means more and stronger explanatory connections, and thus, more coherence in a system. Accuracy might be, then, interpreted as tendency to produce minimally anomalies in the system. That is, in an accurate system there would be an agreement between predictions made and experimental results. It would be a challenging task to link fruitfulness with the concept of coherence. However, it is not a problem if it, or any other unfitting criteria, contributes to greater success in problem-solving and problem-solving, in turn, makes a contribution to achieving a higher degree of coherence. For example, fruitfulness could be interpreted as a promise of future problem-solving capability. In this case, all criteria are linked either directly or indirectly via problem-solving to coherence, which makes Kuhn's philosophy consistently coherentist. Problem-solving is what science is fundamentally, and it is part of the more general search for greater coherence in science.

***Comparison in the history of science***

As already mentioned above, the idea that point-by-point comparison via reference is used in the history of science appears oversimplistic. In practice, there are two types of problem with regard to this idea. First, even if we thought that there were universal substances to which references can be fixed, ontology often changes. In that case, there is no prospect of co-referentiality that would offer a point-by-point referential comparison. A case in point is the chemical revolution by Lavoisier. Entities in the Phlogiston theory are different from those in the Oxygen theory. If these theories are to be rationally compared, there has to be some other way of doing it. Another example is the case of Semmelweis' "cadaveric particles" and the contagion theorists' "miasma" that was transmitted by an air-like infectious disease. 'Cadaveric particles' and 'miasma' are very different kinds of (putative) entities, and both are thought to be non-existent by modern standards. Yet I think we would say that Semmelweis' theory was better and should have been accepted. Furthermore, 'child bed fever' was defined by its symptoms (such as shivering, rapid pulse, high fever, and abdominal pain of peritonitis) at the time (later, by pathological findings and after that, bacteriologically), which almost definitely was often misdescribed and confused with other "fevers". It cannot, therefore, function as a common reference either.

The problem of ontological change applies whenever there are drastic changes in the history of science. Therefore, scientific revolutions may introduce referential discontinuity. A referentialist would naturally make a difference between cases of referential failure and referential success. The situation would often become a complex one where we need to construct cases of partial co-referentiality. Philip Kitcher (1978) has tried to develop a solution on the basis of such referential determination. What is different in his theory compared to many other theories of reference is that he makes the theory of reference context-sensitive, which strikes me as being an improvement. Different tokens of expressions, such as 'phlogiston' and 'dephlogistigated air', may refer in some expressions and uses, and not necessarily only to one entity, but not in others. We pick out those expressions where a term

Meaning Change in the Context of Thomas S. Kuhn's Philosophy refers in one theory, and do the same with another in order to find referential matches between these. In that way he thinks we can form a translation, for instance, between Priestley's and Lavoisier's respective theories of combustion. Kitcher's theory would free us from the rigid theories of reference, which postulate either total referential continuity or total referential discontinuity. In this sense it reminds us of the previously mentioned Field's idea of partial reference (see Field 1973). Kuhn objects to this theory as a theory of translation saying that we need to meaningfully explain the expressions themselves, including all their uses independent of their truth-values. According to Kuhn, we should concentrate on describing how the original text communicated the beliefs of the author, rather than how terms were related to the world. As a concern for the historical interpretation of texts, Kuhn's comment makes sense. Yet I do not think he countered Kitcher on the right issue. Kitcher is suggesting his theory because he wanted to prove that communication and the comparison of truth-values is possible.

No matter whether Kitcher's theory is possible in principle, it is clear that it is historically implausible and implies premises that make it dysfunctional as a theory of theory comparison regarding actual scientists. We may also cast doubt on the strategy that relies unproblematically on the separation between successful and failing references. In describing referring expressions, Kitcher uses metalanguage, i.e. the language of modern science. In fact, he signals it clearly. He says, for example, that "from our perspective Priestley has misdescribed the new gas" (Kitcher 1978, 537). Exactly! But the point is that Priestley did not have "our perspective", nor did Lavoisier. It is not possible to say that Lavoisier knew and showed that 'oxygen' refers but 'phlogiston' does not, or that the latter referred only in some contexts. That was one of the central themes in the scientific controversy of the time. Lavoisier had to use some other type of persuasion to get support for his suggestion. The situation is not any less difficult if it is an open question whether there is some unobservable underlying essence that is assumed to be the common reference. So Lavoisier and his contemporaries could certainly not compare their theories as Kitcher's referential solution implies - that is, by determining the contexts and those terms in the contexts that refer. He seems to be engaged with describing

Meaning Change in the Context of Thomas S. Kuhn's Philosophy  
the history of science as modern science now sees it. What he is not offering us is a theory of theory comparison that could give a historically valuable explanation.

There is also another problem with the idea that the comparison of theories is based on referential matching. We may suppose that scientists know in one way or another that they are referring to the same things or stuff. A simple example would be a Copernican revolution. Let us imagine that there are Ptolemaic scientists and converted Copernican scientists who have a dispute over the place and movement of heavenly bodies. They examine each planet one by one and come to the agreement that they are talking about the same individuals: the sun, the earth, the moon, Mars, and so on. Now, even if we can say that their references match, can we think that it was enough for them to make a choice between theories? I do not think so.

The idea behind the referential solution is that co-referentiality allows the determination of truth-values of claims made of the entities, which enables choosing the theory that makes true claims more. The problem is that practising scientists at the time could arguably not do that. They could not determine whose claims were true and whose false. In fact, the dispute was over that. As proof of how problematic the choosing process is, ponder the fact that the Copernican revolution was a very slow process. It took one hundred years before Copernicus' sun-centred system was generally accepted among astronomers (Kuhn 1985, 227). Kuhn's studies on the revolution made him appeal to other values: in general, to the higher degree of coherence of the Copernican system, and specifically, to its simplicity and potential problem-solving capacity (e.g. Kuhn 1985 11-12; 125-6; 172). Scientists had to use, therefore, some other ways of evaluation, which can also be seen in other cases of scientific change. Moreover, no matter whether it is theoretically possible to construct a case of reference determination, it seems that the motivation for theory change derives from other sources. Lavoisier was disturbed by the confused state of chemical knowledge and nomenclature, as well as by mounting problems with the phlogiston theory. Semmelweis was frustrated with the ineffectiveness of traditional explanations of child bed fever. I think it is more plausible to think that old theories encounter problems, as Kuhn wrote, which leads to the motivation to either revise

Meaning Change in the Context of Thomas S. Kuhn's Philosophy  
them, or sometimes, to look for alternatives to them. What happens before revision, expressed in Kuhn's terminology, is an accumulation of anomalies.

Kuhn's understanding of epistemic evaluation highlights a difference between his perspective and that of the modern causal theorists. Kuhn was interested in how theory change actually occurs in a real historical situation. He paid attention to the actual decisions practicing scientists made. He did not write of comparison in principle or as a possibility if certain non-actual conditions are fulfilled. Referential solution might work in the Copernican revolution if the determination of truth-values was possible. But the problem was precisely that that was beyond the reach of those who affected it.

It would be better to say that the level of coherence is what counts in theory evaluation. The rule is to try to make a theory as coherent as possible and to choose a more coherent one if there is such. Changes in ontology or basic concepts do not constitute a problem because the focus is on coherence, which gives us a common standard for evaluation. The Copernican system was simpler and it answered, for example, the problem of planets' movement and the need for calendar reform (e.g. Kuhn 1985 11-12; 125-6). Lavoisier's reform of chemical nomenclature was certainly an improvement in the search for a more coherent system and his Oxygen theory answered some problems that the Phlogiston theory could not, although it did not answer all the problems. Naturally, it can take time until a higher degree of coherence is fully realised. It may also be the case, for example, that two theories solve different problems. But in that case we need to see how they fare otherwise, and ask which theory forms a more coherent set in some other sense. The other theory may give an explanation but may require acceptance of some non-argued principles. Or perhaps the other has many unconnected beliefs or sets of beliefs, which is a sign of the low degree of inferential relations, and so on. It is not often clear which is better, but the general rule is that a more coherent theory should be chosen. The future scientific work usually makes it explicit which is more coherent. When we remember the rationality debate a couple of decades ago that involved such philosophers as Kuhn, Feyerabend, Popper and Lakatos, the suggestion above leads to an interesting and surprising conclusion. That is, if it is accepted that Kuhn's

Meaning Change in the Context of Thomas S. Kuhn's Philosophy philosophy can be incorporated to the coherentist epistemology, then it seems that Kuhn implicitly accepted that there is something like a rational method of comparison: the method of comparing problem-solving capability and the coherence of theories.

Most theory changes are arguably non-revolutionary that could be described as developments by accretion or accumulation. With Kuhn's terminology this corresponds to the phase of normal science where most of the theoretical body stays the same. Yet, as said above, theory evaluation is comparative in all cases. It is not the case that a belief is just dropped, independent of its role in a theory. In non-revolutionary changes, what is evaluated is a theory before and after putative belief changes. A revolutionary change would be such that a new one replaces most or all of the old theory. The case is usually described so that there is not much connection between an old and a new theory. However, as Thagard and others with the cognition-historical orientation maintain, continuity may be constructed via the mind(s) of scientist(s). We saw above reasons for thinking that a revolutionary change is not possible without there being an alternative theory available. Taking this requirement into account, Thagard describes the process of revolutionary replacement in three stages. First, there are relations between an accepted conceptual network and a group of other concepts. In the second stage, a new network is partially formed in the background that also has links to the connecting set of concepts. He mentions as an example a case of Priestley and Lavoisier in 1777 where, even though they had very different conceptual networks, they shared a lot in terms of experimental techniques and observations. In the third phase, the new conceptual network is developed further and comes to the fore while the old one fades into the background (Thagard 1990, 201-2). Note that the emergence of a new network does not make it impossible to use the old one, even though translation may not be possible between them. Lavoisier could certainly use, understand and argue against the phlogiston theory after his conversion to the oxygen theory. The crucial thing to notice is that the choice to adopt a theory is also a matter of comparative evaluation between two mature theoretical alternatives, and the choice is made on the basis of the overall coherence of the theories.

### ***Convergence or not?***

Evaluation of the coherence of theories was meant to be something that actual scientists have access to. Arguably they do not have access to truth, and therefore, truth cannot have an explanatory value in regard to the actual reasons for the choice of a theory. What scientists are trying to do, according to the suggestion above, is to increase the coherence of their theories. However, we can raise a metaphilosophical question over whether there is something more fundamental that explains the increases of coherence.

The scientific realist is likely to hold that the aim of science is the Truth itself and may say that coherence is merely a symptom of theories becoming truer. S/he might well accept that individual scientists cannot know whether his/her theories and hypotheses are true or false in an absolute sense and yet maintain that science is on a firm path to truth. That is because the ever-increasing degree of coherence unintentionally takes us to truth. We may imagine this as being a kind of invisible hand of truth that guides us in our theory choices. Perhaps scientists use such methods that guarantee the progress in question, or perhaps it is just science as a self-correcting process appropriately related to the world that makes it possible. Bonjour, for example, thinks that the goal of our cognitive endeavours is truth, and the distinguishing character of epistemic justification is its essential or internal relationship to this goal. According to Bonjour, an adequate epistemological theory should be able to show that coherence in one's system of beliefs is likely to lead to correspondence. Adhering to the coherentist standards is thus "truth-conducive". (Bonjour 1985, 7-9)

The first question to be asked is how the notion of 'truth' itself is understood. An easy answer would be to advocate not only a coherence theory of justification but also a coherence theory of truth. Truth would mean something like an ideal fit or ideal coherence. This would link justification to truth inherently: if a theory increases its coherence, it will also be truer by the same token, because truth is identical to

Meaning Change in the Context of Thomas S. Kuhn's Philosophy justification-in-the-long-run. However, the realist naturally understands truth as correspondence to reality, and commitment to a coherence theory of truth is definitely not their choice. It may also be asked whether a coherence theory of truth is really a theory of truth, or merely a restatement of a theory of justification. But that is not an issue for further investigation in this thesis.

Historically, there may not be many philosophers who have tried to connect the coherence theory of justification to the correspondence theory of truth.<sup>132</sup> Nevertheless, an important historical figure that sought to find (at least in some interpretations) a connection between a coherentist justification and a correspondence theory of truth is C. S. Peirce. Peirce understood truth as the limit of inquiry, or as the opinion that a scientific community arrives at in the long run. Ilkka Niiniluoto argues that the limit of inquiry represents, or corresponds to, real things that causally influence the belief formation in a scientific community, and thus, Peirce's characterisation of truth is coextensive with that of the correspondence theory. If that is true, then Peirce did accept the basic idea of the correspondence theory. (See Niiniluoto 2000, 101; 1980) Richard Rorty also thinks that Peirce tried to "bridge the gap" between coherence and correspondence by reducing coherence to correspondence by reanalysing the term 'reality' (Rorty 1994, 447). Richard L. Kirkham, in turn, highlights Peirce's words that "the conception of truth gradually develops ..., reaching the idea of truth as overwhelmingly forced upon the mind in experience as the effect of an independent reality" (Peirce 1934, 394). Kirkham expresses his disappointment that Peirce did not commit to the correspondence theory more strongly because "Peirce's theory of truth is plausible only because it is parasitic on another, hidden theory of truth: truth as correspondence with reality" (Kirkham 2001, 83). All in all, the essential claim to be considered is the scientific realist's contention, which Peirce seemed to accept in some form, that the coherence theory of justification should be connected to the concept of truth as correspondence or/and that an increase of coherence implies progress towards truth. In other words, is the explanation given by the coherence theory of justification just "parasitic" on the correspondence theory of truth?

---

<sup>132</sup> For some suggestions, see Rorty (1994, 447) and Bonjour (1985, 158; note 1).

## Meaning Change in the Context of Thomas S. Kuhn's Philosophy

Let us begin with the normative side. Whether truth as correspondence *should* be the goal of scientific inquiry is dependent on one's philosophical commitment. Many think that it should, but there are a number of philosophers who do not regard it as necessary, Kuhn among them. And if scientists do not need truth as correspondence, or are not able to use it in actual situations of theory choice, I do not see how it necessarily should be the goal of scientific inquiry, except perhaps for motivational reasons. A normative question is, thus, dependent on the values that one accepts in the first place. For that reason, I do not deal with it here any further. I will ask rather whether increasing coherence is compatible with a rising degree of truthlikeness, and further, whether the notion of increasing coherence implies convergence to truth.

I believe that no scientific realist argues that an increase of coherence is an infallible sign of a higher degree of verisimilitude. The case does not change even if we accept Bonjour's "observation requirement", i.e. that any non-a priori belief should be in principle subjected to observational test (Bonjour 1985, 141-4). Even if we suppose that there is a mind independent reality with a determinable structure, it is not *impossible* that we systematically obtain merely false beliefs. There could be many reasons for this. Perhaps there is a sceptic's demon or an evil scientist who feeds false information into our heads, making our system of beliefs internally coherent but fundamentally wrong. Or perhaps part of the reality is inaccessible, or "unobservable", in principle, and cannot be described accurately. Secondly, it may not be discounted that there are several coherent systems that are either equally true or equally far from the truth. Perhaps we are not in a position to describe reality neutrally, and any description always involves something like Kuhn's historical perspective. My message here so far is only that it is presumptuous to think that coherence implies or is an automatic sign of correspondence.

If one wants to connect coherence to correspondence, s/he has to offer an argument for it. Bonjour correctly recognises that if the only rationale for the chosen concept of truth (read: correspondence concept of truth) is an appeal to the related standard of justification, then the argument loses its force. It would be circular to say that a certain standard or epistemic justification is correct because it is truth-

Meaning Change in the Context of Thomas S. Kuhn's Philosophy conducive and that the conception of truth in question is correct because it can connect this way with the suggested standard of justification (Bonjour 1985, 109-10). In other words, an argument for progress towards truth as correspondence has to be motivated independently. However, Bonjour's argument is, disappointingly, reduced to the conviction that a system of beliefs that "(a) remains coherent (and stable) over the long run and (b) continues to satisfy the Observation Requirement is *likely*, to a degree which is proportional to the degree of coherence (and stability) and the longness of the run, to correspond closely to independent reality" (Bonjour 1985, 171; italics mine). I do not think this is quite enough to convince that the account is correct. The account may be reasonable, but he begs the crucial question as to why it is likely that that would be the case. If our descriptions are contingent in the way that Kuhn claims, and there is thus more than one possible highly coherent, and for that reason also relatively stable description of the world, then stability and coherence do not give any guarantee whatsoever that we are closer to truth.

It is clear from practically all that Kuhn wrote that he was against both the correspondence theory of truth and the idea that there is progress towards truth. In *SSR*, Kuhn urges people to abandon the idea that scientific progress has to be defined as a movement towards some goal, and hints that this is a relict as much as the pre-Darwinian evolutionary theories that took evolution to be a goal-directed process planned by God. Instead he proposed to extend the metaphor of evolution to scientific development. (*SSR*, 170-3) In the "Postscript" Kuhn says that there is "no theory-independent way to reconstruct phrases like 'really there'; the notion of a match between the ontology of a theory and its 'real' counterpart in nature now seems to me illusive in principle" (*SSR*, 206). This clearly amounts to a rejection of the correspondence theory of truth. There is not much change either in Kuhn's mind in this sense later, except that he becomes more explicit in philosophical terms. Kuhn writes in 1990<sup>133</sup> that "what is fundamentally at stake is ... the correspondence theory of truth" (*RSS*, 95). And he continues,

---

<sup>133</sup> "The Road Since Structure" (*RSS*), published in 1991.

Nothing about the rationality of the outcome of the current evaluation depends on their, in fact, being true or false. They are simply in place, part of the historical situation with which evaluation is made. ... Justification does not aim at a goal external to the historical situation but simply, in that situation, at improving the tools available for the job at hand. ... Scientific development must be seen as a process driven from behind, not pulled from ahead – as evolution from, rather than evolution toward. (*RSS*, 96; similarly *RSS*, 115)

Note that, as far as problem-solving is concerned, Kuhn motivated it independently; namely, through studies of history<sup>134</sup>. And it is the sole epistemological goal of science that he put forward. Furthermore, he was convinced of the implausibility of the teleological view because of the historical record. Such sheer conviction is not enough to discount the realist's view, but the idea of using historical records as arguments for metahistorical views of science is a laudable one. In order to show that science is converging towards truth, an advocate of that view should, firstly, define the notion of verisimilitude or truthlikeness that is used in those arguments. Secondly, s/he should verify empirically that science is converging.

On the first task, it is clear that early attempts, such as Popper's, failed to make sense of the notion of truthlikeness<sup>135</sup>. Since then, there has been an admirable amount of work carried out around the notion by Ilkka Niiniluoto, Graham Oddie and others. The focus has been on the concept of similarity. The suggested view is that truthlikeness equates to truth plus similarity, and similarity is used for measuring distances from truth. Evaluation of these latter attempts is still ongoing. Unfortunately, to give a verdict on the viability of these complicated arguments goes beyond the scope of this thesis. However, one sceptical remark is in place at this stage. Measuring the distance from a goal requires knowledge of the goal and the point at where the distance is measured from. If we want to know the distance from

---

<sup>134</sup> It is naturally an interesting question whether Kuhn's view of the history of science as problem-solving is actually empirically valid. The most important thing is that his view is open to refutation. Yet, the question of empirical validity is not a concern of this thesis, but it could be a subject for a further study.

<sup>135</sup> This is largely due to the Tichý-Miller refutation of Popper's definition. They showed that Popper's notion is not applicable to the comparison of false theories. In order to use it, the other theory has to be true, which is an absurd assumption with regard to the history of science. (Niiniluoto 2000, 65-8; see also Miller 1974).

Meaning Change in the Context of Thomas S. Kuhn's Philosophy truth, should we then know the truth? I take it as self-evident that that is something that is beyond our reach, at least for the moment. If it were not, then the notion of truthlikeness would lose practically all of its interest. Niiniluoto anticipates this and says that distance is relativised to a certain target, and not to "the whole truth of the whole world" (Niiniluoto 2000, 77). However, it is not clear in what relation such cognitive targets stand with regard to reality itself, which leaves the support for convergent realism as being questionable.

At the moment I would be ready to give the benefit of doubt to those who try to explicate the notion of verisimilitude. But even so, it is not enough if we manage to make sense of the concept. We also need to show that it applies to the history of science. Bonjour sensibly expresses the condition for the case of convergence in the history of science. He writes that the coherentist standards may produce two different types of results in the long run. The system of beliefs might involve constant and relatively wholesale changes over time and thus not approach any stable conception of the world. The other alternative is that the system of beliefs gradually converge on some definite view of the world and thereafter remain relatively stable. The point is that only in the latter case it is *reasonable* to think that the beliefs that stand at the end are true in the sense of correspondence. In the absence of continuity and stability of any kind, i.e. in the case of progress through revolutions and total discontinuity, hitting the truth at some point would be a sudden lucky shot of which we might be wholly unaware. And this scenario is implausible anyway, and so I think we can discount this possibility. In other words, either the history of science settles on a certain conception of the world and remains stable, or it keeps constantly changing. If it converges to a certain view, then it may be the case that it corresponds to the reality. This is not of course necessarily so, as expressed above. If we now forget any challenges offered by the sceptic, one should still be able to say something of the relation between different scientific fields. A certain object can be studied by several scientific fields, which yield rather different results and theories. If there is a reality that can have a unified description, then the sciences that describe it should also be unified. One should tell, therefore, which of sciences is most fundamental, and what kind of relations of reduction there are, because otherwise we are left with several incompatible true descriptions. So, if there is convergence and stability, that would

clearly be a step forward from a realist point of view, but it would then open a problem field on a secondary level (unless convergence had already solved it in some way). Of course there are those, such as Dupré, who think that the project of unified science is doomed to fail (see Dupré, 1993). Then again, if there is constant change in the history of science, it is totally unreasonable to say that there is any empirical-historical evidence for convergence and the increasing correspondence to reality, which makes the whole argument for truth convergence hollow. Thus, the crucial question is whether there is convergence and stability to be detected in the history of science or not.

So is there? One can, of course, focus on many aspects in the search for convergence. If there is no convergence in basic ontology or concepts, then there still might be on a structural level, if appropriately defined. Or if not on the structural level, then perhaps one might try to find it via reference or truth content, and so on. Each case requires both an explication of the notion that convergence is supposed to rely on, and an empirical vindication of the view. On the other hand, if we are arguing for convergence as a kind of ideal final description, i.e. as a view or picture of the world, then there should also be convergence in the basic concepts and ontology. Unsurprisingly, Kuhn argues against that. In *The Copernican Revolution* (1985) Kuhn had already expressed his conviction as follows:

But though achievements of Copernicus and Newton are permanent, the concepts that made those achievements possible are not. Only the list of explicable phenomena grows; there is no similar cumulative process for the explanations themselves. As science progresses, its concepts are repeatedly destroyed and replaced (Kuhn 1985, 264-5)

In addition, beside his overall argument that the history of science comprises scientific revolutions, he is “as a historian ... impressed with the implausibility of the view” that looks for a match between the ontology of a theory and nature. Kuhn says further that:

I can see in their [Newton's and Einstein's mechanics] succession no coherent direction of ontological development. On the contrary, in some important respects ... Einstein's general theory of relativity is closer to Aristotle's than either of them is to Newton's. (*SSR*, 206-7).

Later he talks of vain attempts at “zeroing in on nature's real joints”, and of the need to replace the goal-directed approach with an evolutionary one. The greater closeness of relativistic physics to Aristotelian than to Newtonian is a case that “stand[s] for many” (*RSS*, 206). Kuhn is thus saying in effect that, in general, there is no convergence in basic ontology or in concepts in the history of science.

It is true that Kuhn did not, at the end, offer enough historical argument for his view, which Sharrock and Read (2002) much emphasise. He simply did not study the history of science enough to strongly corroborate his views, which may probably be explained by the fact that he moved ever closer to the non-empirical philosophical argumentation. There are naturally others who have argued against convergent realism on empirical grounds, such as Larry Laudan (1981) and Dupré. Laudan tries to show that “epistemic realism ... is neither supported by, nor has it made sense of, much of the available historical evidence” (Laudan 1981, 20). For example, he comes to the conclusion that there is support for the view that many historically successful theories (he names more than ten of them) are nonreferential in respect of many of their central concepts. He says that anyone who had come to the opposing conclusion “has studied only the more whiggish versions of the history of science”. (Laudan 1981, 33-4) Furthermore, Laudan suggests that the idea that the laws and mechanisms of earlier theories are preserved in theory changes in a “mature” discipline does not withstand historical scrutiny (1981, 47). According to Dupré, the situation is “messy” in biological classification. For example, it is not clear whether the biological basic unit of classification - species - is a kind. The post-Darwinian debate has tipped the balance to the opinion that due to the omnipresence of variation it is questionable whether species share any common internal property. As a consequence of this, many philosophers and biologists treat species as spatially discontinuous individuals. However, there are several competing principles in deciding on the membership of an individual in a particular species. The main

Meaning Change in the Context of Thomas S. Kuhn's Philosophy categories are morphological, evolutionary and pluralistic. Most crucially, there is no way to prove that one of several classifications is privileged. They just work differently for different purposes. That is to say, there is no convergence on the level of basic kinds in biology.

My most important point here is that the question of whether there is convergence in the history of science has to be settled both by the conceptual clarification of such concepts as truth-likeness or verisimilitude and by empirical studies of the history of science. I disagree with Bonjour's idea that the question should be decided by a priori argumentation (Bonjour 1985, 158). Kuhn argues against convergence in respect of basic ontology in science, but I think his empirical data is too thin to corroborate the view. At this point I am ready to leave the case open and wait for the verdict on whether we can find stability and gradual movement to some determinable direction in the history of science. Perhaps we may need to wait on this verdict for a long time. Meanwhile, more empirical research and a bit less historical cherry-picking in support of an intuitively appealing view is needed on both sides.

### ***Finale: Historical-pragmatical view***

An alternative view to scientific realism of the convergence type that does not imply irrationality or that does not make science non-objective can be constructed on the basis of Kuhn's philosophy as it is interpreted in this thesis. Let us start by accepting the historical perspective of science. We are "born into" our systems of beliefs. The evaluation of beliefs is conducted in a historical situation with the means available. What is not possible is to assume, as Cartesians did, that we could search for a secure foundation or start building a system of beliefs from scratch. Instead, the focus is on how we change the beliefs of the existing system. Since the existing conceptual system(s) is the only one there is at each moment, and it serves rather essential functions in our mental life, the default option is to keep it as it is if there is no special reason to change it. Each change has to be motivated by the achieved gain

Meaning Change in the Context of Thomas S. Kuhn's Philosophy that follows. Individual beliefs are changed, and occasionally even whole systems, in order to increase coherence.

The historical perspective comes close to what pragmatists or pragmatically-inclined philosophers have aptly described a replacement of a spectator account of knowledge by an account that makes us "actors in a drama"<sup>136</sup>. We do not evaluate our systems of beliefs outside from a neutral point of view, as if being spectators at some play; rather, we are the ones who are *in* the situation playing, interfering and constructing knowledge. Richard J. Bernstein writes that the conception of man has been distorted by Cartesianism, "not only by the preoccupation with man as knower, but by a certain view of what knowledge is or ought to be – one that is 'incurably contemplative' ". In order to correct this distortion, we need to understand man as an agent that is actively being engaged in various forms of practice. (Bernstein 1971, 7)<sup>137</sup> We are, of course, in relation to "the world" or "nature", not in any neutral one, but in the relation conditioned by each historical moment. The long-term collective activity of a scientific or any other community in fixing our beliefs is given a crucial role in this view. As a consequence, each construction of knowledge is culturally infected or involves a perspective, because that is our starting point as human beings born into the ready-constructed/conceptualised world.

If there is one True description of the world, it can be found only by getting rid of any cultural and contingent elements in our worldview because only in that way it is possible to reach correspondence to mind and human independent reality. This is possible, in principle, if, for example, by increasing coherence we gradually arrive at a certain conception of the world, which would be something like a God's-eye point of view. As stated above, that question needs to be settled by studying what kind of evidence the history of science gives to this possibility. It may also be that there is no such convergence and thus we are unable to arrive at "a view from

---

<sup>136</sup> Bohr reportedly made such a comment (see Niiniluoto 2000, 147-52).

<sup>137</sup> Three classic papers offering a critique of the Cartesian framework from the pragmatist point of view are Peirce's "Questions Concerning Certain Faculties Claimed for Man", "Some Consequences of Four Incapacities", and "Grounds of Validity of the Laws of Logic: Further Consequences of Four Incapacities". The articles are reprinted in *Collected Papers of Charles Sanders Peirce* (Peirce 1934). Note that, as Bernstein says, "Cartesianism" or the "Cartesian framework" is in this context better

Meaning Change in the Context of Thomas S. Kuhn's Philosophy nowhere", even in the long run. This leaves us with the doctrine in which each description of the world inherently has a perspective and is constructed for a specific purpose. There may be, as Dupré argues, plurality of theories that are just as true without any obvious relations of reduction.

This view, therefore, leaves open the possibility that the final description of the world is achievable, but it does not proclaim it. It rather locates us in history as active constructors of knowledge systems with our own limitations of knowing what is "really" true and false. But it implies that we are necessarily in relation to the external world, not in an unmediated way, but via our action-formed concepts. It also offers us a theory of theory comparison. Scientists should choose a more coherent theory. It may happen that two theories seem as coherent with each other. In that case, we should ask, in addition to their coherence, what are we looking for a theory for? With the Kuhnian terms, we would ask what theory is better for doing what scientist do, and focus on their respective ability to solve problems in the desired area.

Kuhn talked in the latter part of his career of being a Kantian with historical moving categories, implying that the world itself, *an sich*, is unreachable for us, and that conceptual categories form the experience for us (e.g. *RSS*, 104). I think the idea of changing mental categories that shape and organise our knowledge of the world is a sensible one, as has been made clear throughout the thesis. However, it would have been better if Kuhn had not said anything of the world *an sich*. There is no need for such a postulation; the idea of conceptual scheme does not imply the distinction between the world *an sich* and the world for us. The latter is the only one we have at the moment. It would have been more consistent with his historical perspective had he stayed silent in respect of the world in itself. As a matter of fact, Kuhn sensibly signals in one of the Kant-references that we might be able to do without the world *an sich* (*RSS*, 207). His historical perspective refrains from metaphysical speculation or taking a standpoint that implies an external point of view. The only meta-view is a historical one that is the result of observing the history of science over a long period

---

understood as an interpretation of Descartes, rather than as a view held by Descartes himself. (see Bernstein 1971, 5)

Meaning Change in the Context of Thomas S. Kuhn's Philosophy of time. We do not really know whether there is a fixed permanent world. If there is and we are in a position to reach it, then the future science presumably takes us there. From our historically situated perspective we cannot tell this. All that we can do is to concentrate on improving our theories of the world, which is a task that needs to be left to scientists, and wait and see what results from this in the long run.

### ***Conclusion***

In this chapter I have studied Kuhn's metahistorical view of science. One of the fundamental claims is that scientist do not have a privileged access to truth. To suppose that they have would be to make a non-naturalistic claim, unwarranted by the history of science. Kuhn suggested that we adopt a historical perspective according to which we are tied to a particular historical situation. We always inherit a ready-made system of beliefs and a ready-made conceptualisation of the world. The whole system cannot be evaluated, and so the evaluation is concentrated on changes to the prevailing system. The maxim is that if there is no particular reason to change any belief, one should retain the old system. Kuhn is, thus, suggesting that conservativeness is a virtue in epistemology. That is also supported by the history of science. There is not something akin to a crucial experiment but scientists tend to stick to the old. Epistemological conservativeness receives further strength from studies in cognitive science. Scientific knowledge is stored in holistically organised systems. All radical changes in the system are practically ruled out if there is no alternative available, and are discouraged even if there is, unless there is a promise of clear epistemological gain.

The consequence of the conclusion that there is no access to truth is, in other words, that we are left with justification. I suggested that Kuhn's philosophy is compatible with coherentism in epistemology. That is, scientists try to increase the coherence of their theories. That fits well with Kuhn's view of science as a problem-solving activity. Unsolved problems make a system less coherent, and solved problems, in turn, increase explanatory power, and, therefore, the overall coherence

Meaning Change in the Context of Thomas S. Kuhn's Philosophy of theories. Many other of Kuhn's criteria may also be linked to coherence. And in any case, we may connect the criteria that may not be linked directly to the concept of coherence indirectly by their contribution to success in problem-solving. A further issue is that the coherentist tends to be conservative in epistemology because changes do often decrease coherence. Coherentism also provides a point for comparison between theories in a way that does not require an agreement between theories in their basic ontology. Scientists should adopt a more coherent theory for the task they are doing. In relation to this, I argued that comparison via references is too simple an account to be a historically realistic theory of theory choice. Reliance on coherentism is historically on firmer ground and gives a rational criterion for theory choice.

I also examined the question of whether increased coherence might lead us to truth. This is the question of whether increasing coherence correlates positively with increasing verisimilitude, or alternatively, whether coherence is explained by a more fundamental notion of truth as correspondence. While this is possible, it does raise further questions. One is to define what truth-likeness or progress towards truth is. Another problem is to prove that such convergence actually occurs in history. Kuhn objected to the idea of convergence saying, for example, that Einsteinian physics is in some respects closer to Aristotelian than to Newtonian physics.

I briefly drafted an alternative view based on Kuhn's historical perspective. This accepts that scientists are historically situated and can only try to increase the coherence of their theories. We are more doers than spectators in the world. That is, we naturally interact with the world and actively construct knowledge from our perspective. This is akin to the pragmatist philosophy. There is no need to assume that the process of science takes us to truth, although it is possible. All that we need are standards for evaluation. If the end result is that there are multiple irreducible as true theories constructed for different purposes and not one True theory of the world, this does not establish a threat to the objectivity and reliability of scientific knowledge. In that situation, we still have scientific knowledge as objective and functioning as science actually takes it anyway.

## 8. Afterwords

Perhaps we can divide philosophers into two kinds. There are those who are very systematic and rigorous but who do not seem to open that many new paths for others to follow. Then there are those who are creative opening new unexpected routes but may lack rigour and be somewhat unsystematic in their work. If this distinction is meaningful, Kuhn undoubtedly falls into the latter category. He chose not to defend default positions in the philosophy of his time but to suggest new ways to proceed. Kuhn is arguably one of the most influential philosophers in the latter part of 20<sup>th</sup> century, both inside and outside professional philosophy, and yet he never succeeded in giving precise formulation on the standards of analytic philosophy to practically any aspect of his theorising. He wrote and developed many issues but always moved on without offering the required explications.

For the above reason he has been an easy target in philosophy, even to the point where he has become one of the apparently necessary bugbears in contemporary analytic philosophy. Michael Devitt, for example, motivated his vigorous defence of scientific realism in *Truth and Realism* - a conference in St. Andrews in June 2004 - with the fact that there really are some anti-realists, such as Kuhn, Feyerabend and Bas Van Fraassen. Their existence is enough to trigger a counteroffensive. Kuhn is undeniably an anti-realist of sorts. I indicated in the thesis that he might be classified as an ontological realist, but also as an epistemological and conceptual anti-realist. The problem is that he has become an unpleasantly convenient figure for the realists' imagination. At times, he appears to serve the role of a straw man just to be destroyed in arguments by many who argue for realism and convergence to truth in science, which appears to be a kind of default position of the philosophy of the day.

Bird, who fortunately does not reduce Kuhn to a straw man, but instead conducts a nuanced and sophisticated study of his philosophy, admits sincerely that

his defence of the Putnam-Kripke position is conducted from a realist perspective (2004a, 90-1). He goes on to say that a criticism that assumes an anti-realist perspective is question-begging because referentialism is a component of scientific realism. This also explains why Bird did not decide to develop Kuhn's own point and try to find an alternative - something that would have produced an inherently anti-realist viewpoint. Furthermore, Bird writes that it is very hard to see the relevance of Kuhn's critique of Putnam, even though the causal theory and essentialism have been put under close and critical scrutiny by analytic philosophers. On the whole, he thinks that Kuhn had "a tin ear" for the analytical philosophical arguments (Bird 2004a, 74-5). Perhaps he had, but a part of the explanation as to why it is so difficult to see the relevance may also come from the perspectives themselves in this debate. My feeling is that the causal theory, or referentialism more generally, has become so popular that practically no-one before Sharrock and Read had tried to understand Kuhn's anti-realistic perspective in this respect. While it is naturally reasonable to defend scientific realism, I think the decision to do so becomes unfortunate if it hides many of the valuable points that Kuhn made. Furthermore, arguments are often in danger of becoming self-serving if one's mind is already made up before the analysis.

Even though Kuhn contributed to the ingredients for misunderstandings so that others often saw him as a kind of philosophical extremist, I do not think he should be reduced merely to the role of a counter pole of scientific realism. One of the aims of this thesis has been to show that Kuhn's philosophy implies elements of interesting theory construction and seeds of thought for serious and relevant criticism with regard to some of the raging philosophical debates of the day. What was just said above applies also to the question of meaning change. To treat meaning change merely as something threatening and something to be opposed is one-sided and conceals many interesting aspects of it. In this thesis I separated three such aspects: philosophical, cognitive-philosophical, and historical. In what follows, I wish to introduce and discuss briefly some possible themes for future research.

In recent research, Kuhn has been seen as a therapeutic philosopher who wants to get rid of philosophical pseudo-problems (Sharrock and Read); as the last logical empiricist (Bird); and as the precursor of cognitive science theorising

Meaning Change in the Context of Thomas S. Kuhn's Philosophy (Andersen, Barker and Chen, and Bird). I rejected both the first and the second interpretations, but endorsed the last one. In addition, the Kuhn of this thesis is a post-empiricist but also a naturalist because of his reliance on historical and cognitive-psychological empirical research. Furthermore, I see him as an anti-realist and as someone to be taken seriously. His conceptual anti-realism or conceptualism and his theory of extension determination via similarity relations form an interpretation that makes him relevant in modern philosophy, most notably in respect of the causal theory and epistemology.

The question of Kuhn's proper context and genesis is one of the themes to which future research may shed more light. As shown at the beginning, there has been a recent move from the earlier emphasis on discontinuity in relation to logical empiricism to descriptions that stress continuity with that tradition. I argued here for discontinuity, but for different reasons than the early views by tracing the origin of Kuhn's meaning theorising to the study of history. I think there is room for an examination of the influences on Kuhn's early thinking from other sources as well, such as Gestalt psychology and Ludvig Fleck's philosophy, which have, admittedly, not been entirely ignored either (e.g. Bird 2000, Ch. 1). My view is that Kuhn moved gradually and thematically towards the main-stream discussion in philosophy (and to the philosophical debates in the first place!), rather than the other way round. If comparison is made to logical empiricism, one needs to make clear whether that is meant purely as an illustration of parallels, or also as a proof that Kuhn was somehow a partaker of the empiricist tradition. In the latter situation, one has to point out what exactly was shared between Kuhn and logical empiricism, and how it separates them from other traditions. For example, it makes sense up to a point to say that Kuhn had (implicitly) adopted a descriptive theory of reference. But this immediately prompts the question of how widely this characterisation applies in philosophy, when we remember that the causal theory and externalism in semantics are rather recent suggestions and hardly successful theories in any pure form. Moreover, one should not ignore that Kuhn's "descriptive theory" was quite a peculiar sort, because objects are picked and classified by similarity relations. To me, this counts as an important unparallel to logical empiricism, establishing a historical connection to later Wittgenstein.

## Meaning Change in the Context of Thomas S. Kuhn's Philosophy

A fresh approach that I have studied with keen interest in this thesis is an application of the cognitive science perspective with regard to Kuhn's philosophy. By highlighting Kuhn's theoretical innovations in the middle and at the end of his career, the advocates of this orientation have managed to point out and develop new aspects in his thinking, such as the idea of knowledge representation, concept definition and concept acquisition. There are also interesting applications to the history of science. I expect that this approach may be able to give us a yet more detailed rational account of what occurs cognitively or psychologically in scientific change. In Kuhn's work, this was left somewhat mysterious because of the emphasis of the Gestalt-type-switch of sudden conversions. It may also be able to give us more historically informative accounts of specific theory changes in the history of science.

Then, of course, Kuhn's ideas themselves may be interpreted in unexpected new ways. I hope to have shown that his stand against the causal theory of reference reveals a proper philosophical disagreement by having argued that Kuhn assumed that similarity relations determine extension and that he was a conceptualist who does not approve of universals as mind-independent entities. Moreover, I also attempted to locate Kuhn in a non-customary way in the epistemological field by connecting his idea of science as problem-solving to the coherentist epistemology. Even if Kuhn was not the most eloquent constructor of philosophical arguments, there are truly interesting issues that are more or less implied in his philosophy. I would not be surprised if further angles and interpretations appear in the near future.

I think that there is (potentially) an entire field of study where activity has lately been minimal. This is something that I call the philosophy of the history of thought, which asks what historical concepts or ideas are, how and why they change, and how to describe the dynamics of concepts and ideas. It seems that historians in the history of ideas, intellectual history, and the history of science are not interested in such questions of a philosophical nature that, nevertheless, can be seen to underlie their work. For example, the question of what 'concept' or 'idea' is often appears to be too abstract in comparison to the practice of history writing. Philosophers, in turn, have various theories of concepts to offer, but it is difficult to apply them to history with any relevance. And neither are philosophers very interested in the philosophy of the history of thought. One reason may be that one of the central theoretical notions

Meaning Change in the Context of Thomas S. Kuhn's Philosophy looks very confusing to an analytic philosopher. From a certain perspective, for example, the notion of historical concept or idea is a conglomeration of what might be called meaning (in the analytic sense) and belief.

I believe that the notion of concept or idea is an important one. Just as Smith and Medin wrote, we need concepts to give stability to our world and to be able to go beyond immediate information. Furthermore, I think we need to also have an account of historically existing and changing concepts. If one caught the insight from the very first page of the thesis that concepts or meanings form the basic ontology of our world, an interest in the dynamics and causes for their appearance and transformation becomes understandable. The question of what makes the entities that structures our world (view) to emerge has a straight epistemological significance: How do we explain the emergence of basic components of our knowledge? What are the reasons and the mechanisms for that process? This concern may also open a fruitful connection to the sociology of knowledge. It is worth elaborating further on this relationship.

If we ask for a reason for meaning change as it was understood in Chapter 6, we are, in effect, asking why certain beliefs have changed. It may be argued that this interest resembles the main object of study in the sociology of knowledge, even though Kuhn himself did not approve of such a field. David Bloor, one of the main theoreticians on the sociology of knowledge, writes that the variation of our ideas of the world is a starting point in the field. The fundamental question is: what are the causes of this variation, and how and why do the ideas change? Furthermore, Bloor says that this study would be impartial with respect to truth and falsity, rationality or irrationality, and success or failure. (Bloor 1991, 5-7) The starting point of the study on meaning change is also concerned with the variation of beliefs. We also want to know why members of a scientific community changed their minds; for example, why did scientists stop believing that elements are found in all bodies? There has to be a mechanism and some causes that explain this change.

A referral to the truth of a belief does not appear to be an explanation because it presupposes that we have already been able to establish that the belief is true. Remembering the pessimistic meta-induction, that would be a daring postulation.

Further, it is not reasonable to give metaphysical explanations to singular events in the history of science. But even if a certain belief were true, we should still explain why people came to accept this belief and abandon another. What was the chain of events that led to that decision? Did somebody have an epistemic access to reality so that s/he could verify that a belief is true? If so, in what way? If not, how did it happen? This also establishes a difference to the sociology of knowledge. The agreement is that the mere supposition of the truth of a belief is not an adequate explanation. But if at some point it turns out to be the case, i.e. if it can be established that a certain scientific belief is true, then it may have explanatory value. This could be the situation that externalists in epistemology and semantics talk about: that the true beliefs about objects are causally effected by the objects themselves. The problem is to establish that such is the case. In this sense, we can use the same *type* of mechanical explanations in both cases but the truth of a belief would bring a further relevant factor to be taken into account in the explanation. On the whole, the study of meaning change shares with the sociology of knowledge both the starting point and a fundamental premise: the former is the task of explaining belief changes and the latter a conviction that the explanations used have to be natural.

I think it is important to realise that the study of meaning change is not necessarily in contrast to scientific realism. It is, in principle, possible to accept that there are certain causal explanations as to how scientists came to accept certain beliefs and maintain that their beliefs are true or truer than those of their predecessors. Judging whether that is the case is another matter. At a more general level, I agree with Paul Thagard that logical, sociological and cognitive explanations can be compatible or even complementary. For example, we may give a sociological account of how Lavoisier's oxygen theory or his revolutionary beliefs spread by various means and reasons to his colleges, which prompted new beliefs and mental representations of them, which in turn led them to appreciate the greater coherence of Lavoisier's theory and thus accept it (Thagard 1994, 637-8). This type of explanation takes all these aspects into account.

It may be said that the step from the era of logical empiricism to the post-empiricist dominance of realism brought a change in how ontological questions

Meaning Change in the Context of Thomas S. Kuhn's Philosophy

relate to epistemological ones. That is, logical empiricism conjoined the question of what exists with the question of how we can know that by making observation the corner stone of all knowing, while the realist philosophy wants to address the questions concerning existence in separation from the questions concerning our capability of knowing. However, looking at this from a historical perspective, such a separation looks puzzling. Arguably, the distinction can be made intellectually, but how about in practice? How can we try to answer what there is without taking into account the position we are in when answering that question? If the history of science can be used as any indicator, then it looks that our answers to ontological questions are distorted by all kind of historical products, such as the web of beliefs, language, and sociological factors. The fact that no answer to ontological question has appeared to be lasting seems to convey the message that epistemology stands in the way of ontology. We are historically situated and, thus, impaired in knowing. In other words, our (in)ability to answer the most fundamental questions has to be taken into account. Kuhn did not go back to the view of logical empiricism that conjoined the two questions, but he may be said to have prioritised the epistemological over the metaphysical.

The problematic whether we are situated inside a frame, language or something else that disqualifies us from answering external questions, or whether there is some direct and unmediated way to get knowledge of reality is an age-old problem and reflects the tension between the historical and the externalist philosophical points of view. I do feel that that the scepticism of the historical perspective in regard to the possibility of our knowing answers (at the moment) to ontological questions is well-grounded. In any case, it is interesting to continue the debate on this basis, and critical realism is at least prepared to engage in such a debate (see Niiniluoto 1999, 91). Such a historical orientation does not need to lead to any subjectivist, idealist, or irrationalist exaggerations, but can offer, in contrast, a reasoned standpoint in philosophy that I tried tentatively and briefly to draft at the end of the penultimate chapter. Of the major philosophical traditions, perhaps pragmatism offers one such reasoned standpoint. What I wish especially to see today, more than forty years from the birth of the historical philosophy of science and the emergence of many departments of the history and philosophy of science, is yet

better integration of historical and philosophical studies. My belief is that in answering the question of whether science really is approximating reality, the history of science has to play a central role. In other words, I wish to see more applications and attempts that try to assess what kind of philosophical view the history of science can accommodate without distortion of the historical data. This has to start with respect to both philosophical argumentation and historical research, and with the decision not to aim to merely vindicate one's view, no matter what the cost.

## Bibliography

- Armstrong, D.M. (1978). *Nominalism and Realism. Universals and Scientific Realism*. Vol. 1. Cambridge: Cambridge University Press.
- - - (1989). *Universals. An Opinionated Introduction*. Boulder: Westview Press.
- Andersen, Hanne (2000). "Kuhn's account of Family Resemblance: A Solution to the Problem of Wide-open Texture". *Erkenntnis* 52, 313-337.
- - - (2001a). *On Kuhn*. Belmont, CA: Wadsworth.
- - - (2001b). "Reference and Resemblance". *Philosophy of Science* 68 (proceedings), 50-61.
- Andersen, Hanne, Barker, Peter and Chen, Xiang (1996). "Kuhn's Mature Philosophy of Science and Cognitive Psychology". *Philosophical Psychology* 9, 347-363.
- Andersen, Hanne and Nersessian, Nancy J. (2000). "Nomic Concepts, Frames, and Conceptual Change". *Philosophy of Science* 67, 224-241.
- Andresen, Jensine (1999). "Crisis and Kuhn". *Isis* 90 (Supplement), 43-67.
- Bambrough, J.R. (1970). "Universals and Family Resemblance". In Richard J. Van Iten (ed.), *The Problem of Universals*. New York: Appleton-Century-Crofts, 250-260.
- Barker, Peter (2001). "Incommensurability and Conceptual Change during the Copernican Revolution". In Howard Sankey & Paul Hoyningen-Huene (eds.), *Incommensurability and Related Matters*. Boston Studies in the Philosophy of Science. Boston: Kluwer, 241-73.
- Barker, Peter, Chen, Xiang, and Andersen, Hanne (2003). "Kuhn on Concept and

Meaning Change in the Context of Thomas S. Kuhn's Philosophy  
Categorisation". In Thomas Nickles (ed.), *Thomas Kuhn*. Cambridge:  
Cambridge University Press, 212-245

Barnes, Barry (1982). *T.S Kuhn and Social Science*. London and Basingstoke:  
Macmillian Press.

Barsalou, Lawrence (1992). "Frames, Concepts, and Conceptual Fields". In Adrienne  
Lehrer & Eva Feder Kittay (eds.), *Frames, Fields, and Contrasts*. Hillsdale,  
New Jersey: Lawrence Erlbaum Associates, 21-75.

Bernstein, Richard J. (1971). *Praxis and Action. Contemporary Philosophies of  
Human Action*. Philadelphia: University of Pennsylvania Press.

Bird, Alexander (1998). *Philosophy of Science*. London and Buffalo: McGill-  
Queen's University Press.

- - - (2000). *Thomas Kuhn*. Chesham: Acumen.

- - - (2002). "Kuhn's Wrong Turning". *Studies in History and Philosophy of  
Science* 33, 443-463.

- - - (2003a). "Three Conservative Kuhns". *Social Epistemology* 17, 125-131.

- - - (2003b). "Kuhn, Nominalism, and Empiricism". *Philosophy of Science* 70,  
690-719.

- - - (2004a). "Kuhn on Reference and Essence". *Philosophia Scientiae* 8, 59-92.

- - - (2004b). "Kuhn and Twentieth Century Philosophy of Science". *Annals of the  
Japan Association for Philosophy of Science* 12, 1-14.

- - - (2004c). "Kuhn, Naturalism, and the Positivist Legacy". *Studies in History  
and Philosophy of Science* 35, 337-356.

- - - (2005). "Naturalising Kuhn". *Proceedings of the Aristotelian Society* 105,  
109-127.

Block, Ned (1993). "Holism, Hyper-analyticity and Hyper-compositionality". *Mind  
and Language* 8, 1-27.

- - - (1998). "Holism, Mental and Semantic". In *Routledge Encyclopedia of  
Philosophy* vol. 4. London: Routledge, 488-493.

- Bloor, David (1991). *Knowledge and Social Imagery* (2<sup>nd</sup> ed.). Chicago: The University of Chicago Press.
- Boas, George (1969). *History of Ideas*. New York: Charles Scribner's Sons.
- Bonjour, Laurence (1985). *The Structure of Empirical Knowledge*. Cambridge, MA: Harvard University Press.
- Caneva, Kenneth L (2000). "Possible Kuhns in the History of Science: Anomalies of Incommensurable Paradigms". *Studies in History and Philosophy of Science* 31, 87-124.
- Carnap, Rudolf (1953). "Testability and Meaning" (reprinted with omissions). In Herbert Feigl and May Brodbeck (eds.), *Readings in the Philosophy of Science*. New York: Appleton-Century-Crofts, 47-93.
- - - (1956). "The Methodological Character of Theoretical Concepts". In Herbert Feigl and Michael Scriven (eds.), *The Foundations of science and the concepts of psychology and psychoanalysis*. Minnesota Studies in the Philosophy of Science Vol. 2. Minneapolis: University of Minnesota Press, 33-76.
- - - (1961). *Der logische Aufbau der Welt: Scheinprobleme in der Philosophie* (2. Aufl.). Hamburg: F. Meiner.
- Chen, Xian, Andersen, Hanne & Barker, Peter. (1998). "Kuhn Theory of Scientific Revolutionss and Cognitive Psychology". *Philosophical Psychology* 11, 5-28.
- Collins, A. M. and Quillian, R. (1969). "Retrieval Time from Semantic Memory". *Journal of Verbal learning and Verbal Behaviour* 8, 240-247.
- Cozzo, Cesare (1994). *Meaning and Argument*. Stockholm: Almqvist & Wiksell International.
- - - (2002). "Does Epistemological Holism Lead to Meaning-Holism".

Meaning Change in the Context of Thomas S. Kuhn's Philosophy  
*Topoi* 21, 25-45.

Davidson, Donald (1984). "On the Very Idea of a Conceptual Scheme". In Donald Davidson, *Inquiries into Truth and Interpretation*. Oxford: Oxford University Press, 183-198.

Devitt, Michael (1981). *Designation*. New York: Columbia University Press.

- - - (1984). *Realism and Truth* (2<sup>nd</sup> ed.). Princeton: Princeton University Press.

Devitt, Michael and Sterelny, Kim (1999). *Language and Reality: An Introduction to the Philosophy of Language* (2<sup>nd</sup> ed.). Oxford: Blackwell.

Diez, Jose A. (2002). "A Program for the Individuation of Scientific Concepts". *Synthese* 130, 13-48.

Donellan, Keith (1983). "Kripke and Putnam on Natural Kind Terms". In Carl Ginet and Sydney Shoemaker (eds). *Knowledge and Mind: Philosophical Essays*. New York: Oxford University Press, 84-104.

Dupré, John M (1993). *The Disorder of Things: Metaphysical Foundations of the Disunity of Science*. Cambridge, MA: Harvard University Press.

- - - (2002). "Is 'Natural Kind' a Natural Kind Term?". *The Monist* 85, 29-49.

Earman, John (1993). "Carnap, Kuhn, and the Philosophy of Scientific Methodology". In Paul Horwich (ed.), *World Changes: Thomas Kuhn and the Nature of Science*. Cambridge, MA and London: MIT Press, 9-37.

Enç, Berent (1976). "Reference of Theoretical terms". *Noûs* 10, 261-281.

English, Jane (1978). "Partial Interpretation and Meaning Change". *The Journal of Philosophy* 75, 57-76.

Field, Hartry (1973). "Theory Change and the Indeterminacy of Reference". *Journal*

Meaning Change in the Context of Thomas S. Kuhn's Philosophy  
*of Philosophy* 70, 462-481.

Fine, Arthur (1975). "How to Compare Theories: Reference and Change". *Noûs* 9, 17-32.

Feyerabend, Paul K. (1965). "Problems of Empiricism". In Robert G. Colodny (Ed.), *Beyond the Edge of Certainty*. Englewood Cliffs: Prentice-Hall, 145-261.

- - - (1981a). "Explanation, Reduction and Empiricism". In Paul K. Feyerabend, *Realism, Rationalism & Scientific Method: Philosophical Papers*, vol. I. Cambridge: Cambridge University Press, 44-97.

- - - (1981b). *Realism, Rationalism & Scientific Method: Philosophical Papers*. Vol. I. Cambridge: Cambridge University Press.

- - - (1996). *Farewell to Reason*. London and New York: Verso.

- - - (1995). *Killing Time: The Autobiography of Paul Feyerabend*. Chicago: University of Chicago Press.

Fodor, Jerry and LePore, Ernst (1992). *Holism: A Shopper's Guide*. Oxford: Blackwell.

Foucault, Michel (1970). *The Order of Things*. New York: Pantheon

- - - (1989). *The Archaeology of Knowledge*. London: Routledge.

Friedman, Michael (2003). "Kuhn and Logical Empiricism". In Thomas Nickles (ed.), *Thomas Kuhn*. Cambridge: Cambridge University Press, 19-45.

Gough, J. B. (1988) "Lavoisier and the Fulfilment of the Stahlian Revolution". *Osiris* 4, 15-33.

Gortvay, György and Zoltán, Imre (1968). *Semmelweis. His Life and Work*. Budapest: Akadémiai Kiadó.

Gram, Moltke S. and Martin, Richard (1980). "The Perils of Plenitude: Hintikka Contra Lovejoy". *Journal of History of Ideas* 41, 497-511.

- Gross, Richard & McIlveen, Rob (2004). *Cognitive Psychology* (2<sup>nd</sup> ed.). London: Hodder & Stoughton.
- Hacking, Ian (1983). *Representing and Intervening*. Cambridge: Cambridge University Press.
- - - (1995). "Working in a New World: The Taxonomic Solution". In Paul Horwich (ed.), *World changes* (2<sup>nd</sup> edn.). Cambridge, MA. and London: The MIT Press, 275-311.
- - - (2001). "Aristotelian Categories and Cognitive Domains". *Synthese* 126, 473-515.
- Harman, Gilbert (1986). "The Meanings of Logical Constants", in Ernst Lepore (ed.), *Truth and Interpretation*. Oxford: Basil Blackwell, 125-34.
- - - (1989). *Change in View: Principles of Reasoning* (2<sup>nd</sup> ed.). Cambridge, MA.: MIT Press.
- - - (1999). *Reasoning, Meaning and Mind*. Oxford: Clarendon Press.
- Hesse, Mary (1974). *Structure of Scientific Theories*. London: Macmillan Press.
- Hintikka, Jaakko (1981). "Gaps in the Great Chain of Being: An Exercise in the Methodology of the History of Ideas". In Simo Knuutila (ed.), *Reforging the Great Chain of Being*. Dordrecht: Reidel Publishing Company, 1-18.
- Hoyningen-Huene, Paul (1993). *Reconstructing Scientific Revolutions*. Chicago: University of Chicago Press.
- Irzik, Gürol and Grünberg, Teo (1995). "Carnap and Kuhn: Arch Enemies or Close Allies?". *British Journal for Philosophy of Science* 46, 285-307.
- Kaplan, David (1970). "Bob and Carol and Ted and Alice". In K.J.J. Hintikka, J.M.E. Moravcsik and Patrick Suppes, *Approaches to Natural Language*. Dordrecht and Boston: D. Reidel Publishing Company, 490-519.

Kelley, Donald (1990). "What is Happening to the History of Ideas". *Journal of History of Ideas* 5, 3-25.

Kendall, James (1952). "The First Chemical Society, the First Chemical Journal, and the Chemical Revolution". Published in two parts in *Proceedings of the Royal Society of Edinburgh* 63A, 346-58 and 385-400.

Kirkham, Richard L. (2001). *Theories of Truth: A Critical Introduction*. Cambridge, MA: MIT Press.

Kitcher, Philip (1978). "Theories, Theorists and Theoretical Change". *The Philosophical Review* 87, 519-547.

Kitcher, Philip and Stanford, Kyle P. (2000). "Refining the Causal Theory of Reference for Natural Kind Terms". *Philosophical Studies* 97, 99-102.

Kripke, Saul (1980). *Naming and Necessity*. Cambridge, MA: Harvard University Press.

Kvstad, Nils B. (1977). "Semantics in the Methodology of the History of Ideas". *Journal of the History of Ideas* 38, 157-174

Kuhn, Thomas (1951). "Newton's "31<sup>st</sup> Query and the Degradation of Gold". *Isis* 42, 296-298.

- - - (1952). "Robert Boyle and Structural Chemistry in the Seventeenth Century". *Isis* 43, 12-36.

- - - (1958). "The Caloric Theory of Adiabatic Compression". *Isis* 49, 132-140.

- - - (1970). *The Structure of Scientific Revolutions* (2<sup>nd</sup> enlarged edn.). Chicago: University of Chicago Press.

- - - (1977a). "Discussion". In Frederick Suppe (ed.), *The Structure of Scientific Theories* (2<sup>nd</sup> edn.). Urbana, Ill.: University of Illinois Press, 500-518.

- - - (1977b). *The Essential Tension. Selected Studies in Scientific Tradition and Change*. Chicago: University of Chicago Press.

Meaning Change in the Context of Thomas S. Kuhn's Philosophy

- - - (1978). *Black-Body Theory and the Quantum Discontinuity, 1894-1912*. Oxford: Oxford Clarendon Press.
- - - (1983). "Reflections on Receiving the John Desmond Bernal Award". *4S Review: Journal of the Society for the Social Studies of Science* 1, 26-30.
- - - (1985). *Copernican Revolution*. New York: MJF Books.
- - - (1990). "Dubbing and Redubbing: The Vulnerability of Rigid Designation". In C. Wade Savage (ed.), *Scientific Theories*. Minnesota Studies in the Philosophy of Science, XIV. Minneapolis: University of Minnesota Press, 298-319.
- - - (1993). "Foreword". In Paul Hoyningen-Huene, *Reconstructing Scientific Revolutions*. Chicago: University of Chicago Press.
- - - (2000). *The Road since Structure*. Ed. by James Conant and John Haugeland. Chicago: University of Chicago Press.

Lakatos, Imre and Feyerabend, Paul K. (1999). "For and Against Method: Including Lakatos's Lectures on Scientific Method and the Lakatos-Feyerabend Correspondence". Ed. by Mattero Motterlini. Chicago: University of Chicago Press.

Lakatos, Imre and Musgrave, Alan (eds.) (1970). *Criticism and the Structure of Knowledge*. Cambridge: Cambridge University Press.

Lakoff, George (1987). *Women, Fire, and Dangerous Things*. Chicago: University of Chicago Press.

Laudan, Larry (1981). "The Confutation of Convergent Realism". *Philosophy of Science* 48, 19-49.

Lavoisier, Antoine (1788). *Method of Chymical Nomenclature*. With Antoine-Francois De Fourceau, Guyton De Morveau, and Claude-Louis Berthollet. Translated by James St. John. London: G. Kearley

- - - (1790). *Elements of Chemistry*. Translated by Robert Kerr. Edinburgh: William Creech.

- Lehrer, Keith (2000). *Theory of Knowledge*. Oxford: Westview Press.
- Lewis, David (1970). "How to Define Theoretical Terms". *The Journal of Philosophy* 13, 427-446.
- Lipton, Peter (2004). *Inference to the Best Explanation* (2<sup>nd</sup> revised edition). London: Routledge.
- - - (1993). "Is the Best Good Enough". *Proceedings of the Aristotelian Society* 93, 89-105.
- Loudon, Irvine (2000). *The Tragedy of Childbed Fever*. Oxford: Oxford University Press.
- Lovejoy, Arthur (1948). *Essays in the History of Ideas*. Baltimore: John Hopkins Press.
- - - (1965). *Great Chain of Being*. New York: Harper & Row Publishers.
- Mackie, Douglas (1935). *Antoinette Lavoisier. The Father of Modern Chemistry*. London: Victor Gollacz ltd.
- Mandelbaum, Maurice (1965). "The History of Ideas, Intellectual History, and the History of Philosophy". *History and Theory* 5, Issue Beiheft 5: The Historiography of the History of Philosophy, 33-66.
- Martin, Michael (1972). "Ontological Variance and Scientific Objectivity". *British Journal for the Philosophy of Science* 23, 252-256.
- - - (1971). "Referential Variance and Scientific Objectivity". *British Journal for the Philosophy of Science* 22, 17-26.
- Miller, David (1974). "Popper's Qualitative Definition of Verisimilitude". *British Journal for the Philosophy of Science* 25, 168-77.

- Mink, Louis (1968). "Change and Causality in the History of Ideas." *Eighteenth Century Studies* 2, Special Issue: Literature and Artistic Change in the Eighteenth Century, 7-25.
- - - (1969). "Reply to Philip Wiener". *Eighteenth Century Studies*. 2, 318-320.
- Minsky, Marvin (1980). "A Framework for Representing Knowledge". In Dieter Metzger (ed.), *Frame Conceptions and Text Understanding*, 1-26.
- Nagel, Ernst (1961). *The Structure of Science*. London: Routledge and Kegan Paul.
- Nersessian, Nancy (1985). "Faraday's Field Concept". In David Gooding, & Frank A. J. L. James (eds.) *Faraday Rediscovered: Essays on the Life and Work of Michael Faraday, 1791-1867*. Basingstoke: Macmillan Press, 377-406.
- - - (1984). *Faraday to Einstein: Constructing Meaning in Scientific Theories*. Dordrecht: Kluwer Academic Publishers.
- - - (1991). "Discussion: The Method to 'Meaning': A Reply to Leplin". *Philosophy of Science* 5, 678-686.
- - - (1992). "How Do Scientists Think?". In R.N. Giere (ed.), *Capturing the Dynamics of Conceptual Change in Science*. Minneapolis, MN: University of Minnesota Press, 3-45.
- - - (1998). "Conceptual Change". In William Bechtel and George Graham (eds.), *A Companion to Cognitive Science*. Malden, MA: Blackwell, 155-166.
- - - (2003). "Kuhn, Conceptual Change, and Cognitive Science". In Thomas Nickles (ed.), *Thomas Kuhn*. Cambridge: Cambridge University Press, 178-212.
- Newton-Smith, William (1981). *The Rationality of Science*. London: Routledge & Kegan Paul.
- Nickles, Thomas (2003). "Normal Science: From Logic to Case-Based and Model-Based Reasoning". In Thomas Nickles (ed.), *Thomas Kuhn*. Cambridge: Cambridge University Press, 142-177.

- Niiniluoto, Ilkka (1999). *Critical Scientific Realism*. Oxford: Oxford UP.
- - - (1980). "Scientific Progress", *Synthese* 45, 427-462.
- Nola, Robert (1980). "Fixing the Reference of Theoretical Terms". *Philosophy of Science* 47, 505-531.
- Nuland, Sherwin B. (2003). *The Doctors' Plague*. New York: W. W. Norton & Company.
- Oldroyd, David R. (1973). "An Examination of G.E. Stahl's Philosophical Principles of Universal Chemistry". *Ambix* 20, 36-52.
- Papineau, David (1979). *Theory and Meaning*. Oxford: Clarendon Press.
- - - (1996). "Theory-Dependent Terms". *Philosophy of Science* 63, 1-20.
- Peacocke, Christopher (1992). *A Study of Concepts*. Cambridge, MA: MIT Press.
- Peirce, Charles Sanders (1934). *Collected Papers of Charles Sanders Peirce*, vol. V, *Pragmatism and Pragmaticism*. Ed. by Charles Harsthorne and Paul Weiss. Cambridge, MA: Harvard University Press.
- Perrin, C. E. (1982). "A Reluctant Catalyst: Joseph Black and the Edinburgh Reception of Lavoisier's Chemistry". *Ambix* 29, 141-176.
- - - (1988). "The Chemical Revolution: Shifts in Guiding Assumptions", in Arthur Donovan, Larry Laudan and Rachel Laudan (eds.), *Scrutinising Science: Empirical Studies of Scientific Change*. Kluwer Academic Publishers, 105-124.
- Popper, Karl R. (1997). *The Logic of Scientific Discovery* (4<sup>th</sup> ed.). London: Routledge.
- Preston, John (2004). "Bird, Kuhn, and Positivism". *Studies in History and*

Meaning Change in the Context of Thomas S. Kuhn's Philosophy

*Philosophy of Science* 35, 327-335.

- - - (1997). *Feyerabend*. Cambridge: Polity.

Putnam, Hilary (1975a). "Explanation and Reference" In Hilary Putnam, *Mind, Language and Reality: Philosophical Papers*, Vol. 2. Cambridge: Cambridge University Press, 196-215.

- - - (1975b). "The Meaning of 'Meaning'". In Hilary Putnam, *Mind, Language and Reality: Philosophical Papers*, Vol. 2. Cambridge: Cambridge University Press, 215-272.

Quine, W. V. (1969). *Ontological Relativity and Other Essays*. New York and London: Columbia University Press.

- - - (1964). "Two Dogmas of Empiricism", in W. V. Quine, *From a Logical Point of View* (2<sup>nd</sup> revised ed.). Cambridge, MA: Harvard University Press.

Rappaport, Rhoda (1960). "G-F Rouelle: An Eighteenth Century Chemist and Teacher". *Chymia* 6, 68-101.

Read, Rupert and Sharrock, Wes (2002). "Thomas Kuhn's Misunderstood Relation to Kripke-Putnam Essentialism." *Journal for General Philosophy of Science* 33,151-58.

Reisch, George A (1991). "Did Kuhn Kill Logical Empiricism?" *Philosophy of Science* 58, 264-277.

Rescher, Nicolas (1973). *The Coherence Theory of Truth*. Oxford: Oxford University Press.

Richter, Melvin (1987). "Begriffsgeschichte and the History of Ideas". *Journal of History of Ideas* 48, 247-263.

- - - (1995). *The History of Social and Political Concepts*. Oxford: Oxford University Press.

Meaning Change in the Context of Thomas S. Kuhn's Philosophy

- Rorty, Richard (1994). "Pragmatism, Davidson and Truth". In Paul Horwich (ed.), *Theories of Truth*. Aldershot: Dartmouth, 443-465.
- Sankey, Howard (1994). *The Incommensurability thesis*. Aldershot: Avebury.
- - - (1997). "Incommensurability: The Current state of Play". *Theoria* 12, 424-45.  
Also at [http://www.hps.unimelb.edu.au/bio\\_hsankey.html](http://www.hps.unimelb.edu.au/bio_hsankey.html)
- - - (2000). "The language of Science: Meaning Variance and Theory Comparison". *Language Sciences* 22, 117-136.
- Scheffler, Israel (1967). *Science and Subjectivity*. Indianapolis: Hackett.
- Schiffer, Stephen (2000). "Pleonastic Fregeanism". In Akihiro Kanamori, *Analytic Philosophy & Logic*, The Proceedings of the Twentieth World Congress of Philosophy. Vol 6. Bowling Green, OH: Philosophy Documentation Center, Bowling Green State University.
- Skinner, Quentin (1969). "Meaning and Understanding in the History of Ideas". *History and Theory* 8, 3-53.
- Siegfried, Robert (2002). *From Elements to Atoms. A History of Chemical Composition*. Philadelphia: American Philosophical Society.
- Searle, John (1997). "Proper Names". In Peter Ludlow (ed.), *Readings in the Philosophy of Language*. Cambridge, MA: Blackwell.
- Shapere, Dudley (1964). "The Structure of Scientific Revolutions". *The Philosophical Review* 73, 383-94.
- - - (1966). "Meaning and Scientific Change". In Robert Colodny (ed.), *Mind and Cosmos: Essay in Contemporary Science and Philosophy*, 41-85.
- - - (1982). "Reason, Reference and the Quest for Knowledge". *Philosophy of Science* 49, 1-23.
- - - (1991). "Leplin on Essentialism". *Philosophy of Science* 58, 655-677.
- - - (2001). "Reasons, Radical Change and Incommensurability in Science". In

Meaning Change in the Context of Thomas S. Kuhn's Philosophy  
Howard, Sankey and Paul Hoyningen-Huene (eds.), *Incommensurability and  
Related Matters*. Boston Studies in the Philosophy of Science. Boston:  
Kluwer, 181-206.

Sharrock, Wes and Read, Rupert (2002). *Kuhn. Philosopher of Scientific  
Revolution*. Cambridge: Polity Press.

Smith, Edward E. and Medin, Douglas L. (1981). *Categories and Concepts*.  
Cambridge, MA: Harvard University Press.

Spitzer, Leo (1944). "Geistgeschichte vs. History of Ideas as Applied to Hitlerism".  
*Journal of History of Ideas* 5, 191-203.

Solso, Robert L. (1995). *Cognitive Psychology* (4<sup>th</sup> ed.). Boston: Allyn and Bacon.

Stahl, G. E. (1730) *Philosophical Principles of Universal Chemistry*. Translated by  
Peter Shaw. London: John Osborn and Thomas Longman.

Sterelny, K (1983). "Natural Kind Terms". *Pacific Philosophical Quarterly* 64, 110-  
125.

Stevenson, Rosemary. J. (1993). *Language, Thought and Representation*. Chichester:  
John Wiley & Sons.

Suppe, Fredrick (ed.) (1977). *The Structure of Scientific Theories* (2<sup>nd</sup> edn.). Urbana  
Ill.: University of Illinois Press.

Thagard, Paul (1984a). "Conceptual Combination and Scientific Discovery". In  
Peter D. Asquith & Philip Kitcher (eds.), *Philosophy of Science Association  
1984, 1*. East Lansing: Philosophy of Science Association, 3-12.

- - - (1984b). "Frames, Knowledge, and Inference". *Synthese* 61, 233-259.

- - - (1990). The Conceptual Structure of the Chemical Revolution. *Philosophy of  
Science* 57, 183-209.

Meaning Change in the Context of Thomas S. Kuhn's Philosophy

- - - (1992). *Conceptual Revolutions*. Princeton: Princeton University Press.
- - - (1994). "Mind, Society, and the Growth of Knowledge". *Philosophy of Science* 61, 629-645.
- - - (2000). *How Scientists Explain Disease*. Princeton: Princeton University Press.

*The Cambridge Dictionary of Philosophy* (2n ed.) (1999). General editor Robert Audi. Cambridge: Cambridge University Press.

Thomas, Keith (2005). "Politics: Looking for Liberty". *The New York Review of Books* 52, no 9.

Wiggins, David (1980). *Sameness and Substance*. Oxford: Basil Blackwell.

- - - (2001). *Sameness and Substance Renewed*. Cambridge: Cambridge University Press.

Williamson, Timothy (1994). *Vagueness*. London: Routledge.

Wilson, Daniel J. (1987). "Lovejoy's The Great Chain of Being after Fifty Years". *Journal of the History of Ideas* 48, 187-206.