‘Sloppy Thinking’: To What Extent Can Philosophy Contribute to the Public Understanding of Science?

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Abstract

This thesis will address two questions: Does philosophy contribute to the 'public understanding of science' (PUoS), and if so, how?

The popular public image of science is one of methodology. Science is a means for making true statements about the world, where we compare hypothesis with observation against the evidence. This then allows for a body of knowledge that guides further advancements and progress. Philosophy, however, seems to be antithetical to this. A popular notion is that philosophy is either what science was, or it deals with objects and ideas so intangible, that they have no real effect in the world. Either it is an outmoded way of doing science, or it is the preserve of armchair academics. In both cases the average person would be forgiven for thinking it had no relevance to them, and especially their ability to understand science. This thesis will look to challenge this relationship. Using hermeneutics, discourse-textual analysis and deconstruction, I present two interpretations of science and philosophy. These two interpretations I will call the 'methodological' and 'historical' approach. The 'methodological' approach is to understand science as a collection of principles or rules that, if followed, will produce true statements about the world. An example of such a principle that intersected both philosophy and science is 'falsification' as understood through the 'problem of demarcation' (PoD). The irrelevance of philosophy to science is fortified by the constant failure to produce fixed rules for what makes one thing scientific and another not. The 'historical' approach is to understand the actions of scientists as historical events. So rather than ask 'what is science?' we might ask, 'what does it mean to act scientifically?' I will argue philosophy can be of use in overcoming the antagonism between understanding a methodological question historically and a historical question methodologically.

Firstly, I give an uncontroversial reading of the PoD, as argued by Karl Popper, who represents the 'methodological' view and oppose this to the 'historical' approach of Paul Feyerabend. Due to the dominance of the interpretation of science as a methodology, I argue that historical critiques, like Feyerabend's, become nonsensical when understood as methodological substitutes. This is what I call the 'received view' of what both Popper and Feyerabend had to say on science. Here, Popper fails to solve the PoD and Feyerabend appears to deny the method, objectivity or rationality of science. Next, using ideas inspired by Heidegger, I reverse those roles by presenting a 'methodological' and 'historical' reading of *The Structure of Scientific Revolutions* by Thomas Kuhn. I develop two types of language, which I call 'about' and 'of' language that map on to the methodological and historical distinctions. Using this method I construct two contradictory readings of the text, but unlike the Popper-Feyerabend antagonism, we see how the historical approach is the more fertile interpretation. One version, which I call the 'strong' reading, has Kuhn as a relativist, irrationalist or anti-science, which is important if this is the 'received view' of Kuhn. This reading carries political weight with 'interest groups' who may wish to undermine the epistemic authority of science. That same reading can be used to discredit Kuhn/ philosophy of science, and by extension philosophy as a worthwhile instrument for understanding science. The other version, which I call the 'weak' reading, has Kuhn as a supporter and defender of science, but it also resolves old philosophical disputes by framing the problem in a different way. This will not only problematize any notion of a dominant interpretation, but it gives good grounds why one cannot be relativist or irrationalist about 'truth'. Thus it defends the epistemic authority of science, and also gives philosophy a valuable role in public thinking about science.

Declaration

I declare that the work in this thesis was carried out in accordance with the regulations of the University of Gloucestershire and is original except where indicated by specific reference in the text. No part of the thesis has been submitted as part of any other academic award. The thesis has not been presented to any other educational institution in the United Kingdom or overseas.

Any views expressed in this thesis are those of the author and in no way represent those of the University.

Signed…………………………………. Date………………………………..

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Chapter One

Introduction: Science, Philosophy and Society

1.1 – Introduction

This thesis places itself at a point of convergence between several issues, emanating from sociology, education, politics, history and philosophy of science. My main concern is with the dialogue between science as a practice and what is necessary for a person to know in order to have a fair understanding of that practice. What concerns me with this dialogue is the apparent irrelevance philosophy has in contributing to this debate in the public sphere. Public understanding of science and much of science education tend to promote science as a 'method', where we can say uncontroversial things like 'knowledge and understanding in science are rooted in evidence'.[1] There is absolutely no denying that science is successful at what it does and it does so because of its methods. On the other hand 'philosophy' appears to make little contribution, since it tirelessly fails to make progress in its subject areas, and worse it has led to all sorts of confused thinking about ideas such as 'truth', 'reason', 'reality', or even 'science'.

Since the fallout of the 'Science Wars' a whole new raft of terms and ideas have filled the public space on what science may or may not be. This normally is attacked under the umbrella term of 'postmodernism' which prima-facie seems to dismiss the central ideas of the Enlightenment from which the scientific attitude came. The core ideas of 'truth', 'objectivity', and 'reason' have been spread too thin or too thick in recent years. From the 'postmodern' mash-ups of New Age mysticism with quantum physics in Fritjof Capra's *The Tao of Physics,* to the ultra empiricism of Hawking and Mlodinow's *The Grand Design*.[2] Whilst these would appear to be at different ends of the 'objectivity' spectrum they share a common metaphysical commitment. For what Capra and those other purveyors of 'postmodern' critiques of science want, is to overcome those positivistic-imperialist views of orthodox science which talk in a language of 'objectivity' and 'truth'. Taking this as a sample of the work

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[1] DfES, *Science: Programme of Study for Key Stage 3 and Attainment Targets* (London: Department for Education and Skills, 2007), p.208

'philosophers' and 'cultural theorists' put out about science gives rise to a kind of 'anti-philosophy' by those more immediately connected with science. This can lead someone like Hawking, without irony, to sub-title his book *New Answers to the Ultimate Questions of Life*, where we are led to page-one where it is claimed, 'philosophy is dead'.[3] The view that Hawking represents is the other end of this 'objectivity' spectrum, where rather than argue that 'objectivity' or 'truth' is a language game, hegemonic structure, or social construct, it is only the scientific way of disclosing the world that counts as true. Thus the only meaningful way to describe reality is objectively.

Part of the originality of this thesis is to argue that these two perspectives share a common source in viewing science as only a method. One view claims there can be no objectivity and the other that there is only objectivity. It is then from this common source that apparently contrary positions can be taken from which philosophy becomes the victim. For we either get the kind of 'anti-philosophy' from the likes of Hawking or we suffer the fallout of the 'postmodernist' abuses of philosophy in science by claiming that due to something like Kuhnian paradigms there can be no objective scientific position. What makes this situation more confusing for a member of the public is that not only can we find specialists who are willing to disagree with the scientific community, but also history shows us that this is how science progresses.[4] For every highly corroborated theory or 'scientific' position we can find a scientist who has 'evidence' to the contrary. This would seem to exceed our maxim of 'knowledge and understanding in science are rooted in evidence' as we have highly trained professionals who seem to disagree over what the evidence is or what it means. Analytical philosophers have tried to contribute to this debate by either working out a formalized account of whatever this method might be or how language, thought, experience and reality all hang together. This acute level of detachment and abstraction has not won many followers from the scientific community who rest assured that their highly abstract problems are at least 'real' or in some proper sense 'scientific'.

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[2] Fritjof Capra, *The Tao of Physics: An Exploration of the Parallels Between Modern Physics and Eastern Mysticism*, 3rd edn. (London: Flamingo, 1983); Stephen Hawking and Leonard Mlodinow, *The Grand Design: New Answers to the Ultimate Questions of Life* (New York: Bantam Press, 2010)
[3] Ibid., p.1

My thesis is to argue for a role for philosophy in the public understanding of science (PUoS). I am not arguing for a positive theory of what science is or that people should follow philosophy *x*, but the practice of philosophizing itself is a public discourse worth having. Part of this thesis will be to argue that before we can ask 'what is science?' (a particular theory), which leads to philosophical problems such as that of 'demarcation', we have to understand the grounds for asking such a question. What I identify as symptomatic of the problems of demarcation is the treating of an essentially historical question as if it were a methodological one. I argue that this is accomplished due to the dominant interpretation of science as method. So any critique of science from a philosophical basis appears to be usurping the methodology of science, as if philosophy could improve on it.

Science necessarily has a philosophical component, but if it is understood as only a methodology, it then covers over the presence of philosophy. Or, in order to become scientific the presence of philosophy has to be diminished until it is not even a question. What do I mean by this? Well first, why would we want someone to know about science? The general idea is that if people understood the scientific method, a number of things might follow: society would improve as people employed evidence-based reasoning in their everyday lives; science could progress as people would understand the need for certain research programmes; and society would progress off the back of those new innovations. This all sounds like a good idea, so how would we achieve this? Taking just the first claim, how would someone follow the methodology of science so their life was enhanced? A core premise to the scientific perspective and, what seems to put it in opposition to the religious perspective, is that, what we believe is true and what is actually true are two totally different things. Most of us would agree with this statement to the point that it seems trivial.[5] To get to this position most, if not all, scientists take up methodological naturalism and empirical-realism. This would require the average member of the public to accept that reality is explainable using only natural forces and that this reality is a real, external, mind independent entity that is knowable. The claim and apparent strength of science is that it is just an extension of commonsense. It should be self-evident that we do not need

[4] For an example see bio-chemist and creationist Michael J. Behe, *Darwin's Black Box: The Biochemical Challenge to Evolution* (New York: Simon and Schuster, 1998)

supernatural forces or that wishful thinking has no bearing on the proceedings of reality. Bronowski writes, ‘the mastery and the greatness of science rests in the end on this, that here the rational and the empirical are knotted together. Science is fact’.[6] Now philosophers get in there and muddy the water by saying stuff like scientists are actually methodological realists, or methodological pluralists, or normative naturalists and so on.[7] Other philosophers deny that there is even a scientific method.[8] Seemingly, if given their day, most philosophers would die of hunger, like Buridan’s ass never being able to make a decision.[9] The ‘over-thinking’ of science by the humanities has led to a range of hostile reactions. There is the ‘sloppy thinking’ in Alan Sokal’s *Beyond the Hoax,* where fashionable discourses within the humanities are argued to act as barriers to valid ‘leftist’ social action – (Sokal writes that empty intellectual fashions promote ‘subjectivist and relativist philosophies that in my view are inconsistent with producing a realistic analysis of society’). [10] Or there is Norman Edmund’s provocatively titled *End the Biggest Educational and Intellectual Blunder in History*.[11] The ‘blunder’ being, ‘the false claim of the non-existence of the scientific method by the Harvard/Conant group’.[12] The ‘Harvard/ Conant group’ consisted of thinkers like James B. Conant, W.V.O. Quine and more importantly for this thesis, Thomas Kuhn.

It would seem that the greatest testament to the efficacy of science is not some philosophical argument, but the fact that stuff works. We have planes, computers, medicine and so on. Connecting the dots, it would seem that from relatively few sensible premises we can derive a world that is ‘out there’ in reality, knowable and explainable without recourse to magic or mystics. Relaxing in the fact that thoughts do not influence reality, we see science continue to work regardless of what people think about it. Yet the perceived influence of metaphysics at work in ‘wishful

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[5] Christopher Norris, *Epistemology: Key Concepts in Philosophy* (London & New York: Continuum, 2005), pp.20-22

[6] Jacob Bronowski, *The Common Sense of Science* (London: Heinemann, 1951), p.99

[7] Howard Sankey, ‘Methodological Pluralism, Normative Naturalism and the Realist Aims of Science’, in *After Popper, Kuhn and Feyerabend: Recent Issues in Theories of Scientific Method,* ed. by Robert Nola and Howard Sankey (Dordrecht: Kluwer Academic Publishers, 2000), pp.211-229 (p.212)

[8] Paul Feyerabend, *Against Method*, 3rd edn. (London: Verso, 2002), p.1

[9] Amartya Sen, *Rationality and Freedom* (Cambridge, MA.: Harvard University Press, 2004), p.229

[10] Alan Sokal, *Beyond the Hoax,: Science Philosophy and Culture* (Oxford: Oxford University Press, 2008), pp.107-108

[11] Norman H. Edmund, *End the Biggest Educational and Intellectual Blunder in History: A $100,000 to Our Top Educational Leaders* (Fort Lauderdale, FL.: Scientific Method Publishing Company, 2005)

thinking' in how mind might influence reality is equally at work in understanding reality as independent of mind. Now I am not arguing that science could be successful with any arbitrary metaphysics, but that its non-recognition is used as an excuse to demote the role of philosophy to a merely linguistic or psychological entity.
Where I step into the debate is on behalf of the general public, not the people practising science, but those charged with the task of understanding it. It will be understood throughout this thesis that those involved with science, as a daily activity, will acquire a tacit understanding of that practice. Whereas, those who do not do science but have to learn about it will have to acquire an explicit form of whatever this tacit element is. The difference between learning about a subject in its explicit form as 'knowledge' (i.e., facts) and its tacit form of 'understanding' (i.e., practised based) is something we do *not* ask the public to consider. What-is-more, I think most scientists and educators do not consider this difference, as they are either unaware or believe it not to be relevant. This is what I mean by absence of philosophy in science. One of my concerns and reasons for writing this thesis is that due to philosophy becoming an increasingly marginalized practice I see a number of things happening. It is either viewed as a lofty subject ripe only for academia, to which the institutionalization and professionalization of the subject have not helped. Here it grows old and stale, a remnant of what classical education use to be, a vocation, benign but valuable like relicts in a museum. Or it is viewed as a shorthand term for anything non-scientific, that alternative medicines and complementary therapies are based on Ancient Eastern wisdom or philosophical, holistic approaches to mind, spirit and body. Here it is viewed as less benign, acting as an agent of deception guiding people into sloppy thinking. Surely, it is only philosophy that could allow one to deny that the scientific method exists, that truth is relative, that science does not progress, or that scientific debates are settled irrationally?

As a rational member of the public, only one option seems sensible, that I side with 'science' and have to concede that any other worldview would be irrational to hold. Here 'philosophy' is just what some Oxbridge dons do or people who mistrust

[12] Ibid., p.xi

conventional science.[13] This position seems even more reasonable considering those who actually are involved with communicating science have adopted philosophical domains and questions as their subject matter. Here, we can explain why people are religious, moral, or why there is something rather than nothing and so on without ever picking up a single book of Aristotle, Spinoza, Kierkegaard or Leibniz.[14] With the ever-increasing explanatory domain of science, I see not only religion but also philosophy being pushed into the margins, and like so many social practices it could one day disappear altogether.

1.2 - The Thesis

I will argue that philosophy is a means not only to protecting the epistemic authority of science but also as a way of combating Sokal's 'sloppy thinking'. I will argue that what has been packaged as science, is an abstracted, metaphysical notion of science as a pure methodology. What people like the 'Harvard/Conant' group have done is pick up on historical inaccuracies in the story of science as only a method. I will argue that in order to understand science as only a methodology, it is necessary to take on a lot of philosophical baggage. This either goes unnoticed or is subtly registered, but due to lack of philosophical ability, it gets subsumed by other discourses that use it for their own socio-political ends. Both of these outcomes I see as anathema to an 'open society' and democracy. We, at the same time, promote 'unthinking' in asking people to accept an idea without scrutinising it and by the same means we supply ammunition to those who wish to combat science as a mode for revealing facts about the world. What I see as the greatest strength of philosophy can also be perceived as its greatest weakness; that is, it can complicate the very simple. As has already been mentioned, philosophers argue over whether science is 'really' a naturalist, realist, or materialist endeavour. This is philosophy and science as represented by a particular group. Philosophy in the sense that I mean it, would try to understand how we could get to a point that we can say a phrase like 'really is' in the first place. This presumes a mind, world, language, and knowledge model of reality that allows scientific and

[13] Later it will be argued that it is a narrow conception of philosophy that is being targeted. On the one side we have science as empirical-realist or methodological naturalism and on the other hand 'abstract philosophy' as belonging to ancient wisdom or alternative/ complementary medicines. See pp.15-16

philosophical debates to take place. The questioning or even denial of this model is not to advocate an ‘anything goes’ free-for-all, but it would actually help explain how science has come to the position it has and why it is so good at what it does. Not only would it help the public confront inherent problems within the ‘science as only method’ conception, but it would also be an antidote to the sloppy thinking of other discourses, including science and philosophy itself. It is only recently that I have come round to this ‘deconstructive’ tendency, and unintentionally have been writing about philosophy as type of deconstruction all the way through my thesis. This is not philosophy as a subject but as a means to understanding how subjects, including philosophy, are established. Norris writes, ‘deconstruction begins with the same gesture of turning reason against itself to bring out its tacit dependence on another, repressed or unrecognized, level of meaning.’[15] If one thinks of philosophy as a subject, it becomes a two and half thousand-year endeavour. Do I look for the answers in Plato, Senaca, St Augustine, Locke, or Hume? When viewed like this, philosophy seems like a detached antiquated substitute for science, if what they were all looking for were ‘facts’ about the world. Rather, if we view philosophy as an activity that they were all engaged in, limited by their socio-historical conditions, we can also view science and philosophy as expressions of that limit. When philosophy becomes absent, we start to think that that limit no longer applies, that any thinking about ‘truth’ is either scientific or what we mean by truth is some wishy-washy metaphor. Understanding what philosophy does and how one does it is not made any easier by viewing it as a benign purely vocational subject. Philosophy is an every day activity not just of thinking, but a way of acting. People act on behalf of philosophical and metaphysical ideas everyday, but it is because it is so ‘everyday’ that we fail to recognize it. Here, I see philosophy like art at its best when it makes the ‘everyday’ seem special, new or unfamiliar, so that we are forced to look at it again.

There can be no scientific argument for why a particular philosophical doctrine should not be advocated over another, and even more so for ethical terms like ‘should’. Even though some philosophers and scientists would disagree with that last statement, I

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[14] For examples of these and popular science writing see, Richard Dawkins, *The God Delusion* (London: Transworld Publishers, 2006); see also chapters 5 and 6 in Lawrence M. Krauss, *A Universe from Nothing: Why is There Something Rather than Nothing* (New York: Free Press, 2012)
[15] Christopher Norris, *Deconstruction: Theory and Practice*, Revised edition (London & New York: Routledge, 1999), p.64

think this is an example of 'sloppy thinking'.[16] To begin with, I will look at the recent history of this debate and how pro-science advocates have represented philosophy, with particular focus on the 'Science Wars'. I will argue that part of the confusion that resulted from the 'Science Wars' was due to a type of incommensurate thinking or dialogue. Whilst some were speaking about science as a historical activity others were discussing science as a pure methodology.[17] This situation becomes even more confusing when one understands a historical critique as a substitute for methodology. So within this thesis, I will outline two 'understandings' or 'readings' of science, one that is methodological and one that is historical.[18] I will argue that not only has the methodological understanding been pushed forward as the 'real' representation of science, but that it is also a philosophical notion which will take philosophy to unpack.[19] One of the conclusions of the methodological reading, or what I will also call the 'strong' reading of science, is something like 'scientism,' which I take metaphysically to be the same or at least parasitic on the fundamentalist religious positions. In contrast to this, I will offer the historical or 'weak' reading of science which is geared to what 'fixes' interpretations of phenomena including the methodological interpretation of science. The role of philosophy in this instance is to show how if we challenge the methodological or strong reading of science that will include things like 'Objectivity', 'Truth' and 'Reason', we are not left with nothing. Thus, the very fact we might think we would be left with 'nothing' is to highlight what is absent in the understanding of science and philosophy. Here, we end up with endless debates about relativism verses absolutism or realism against anti-realism because all we have to deal with is the strong reading. If all we understand science to be is through the strong reading then when someone challenges it, they appear more radical than they actually are.[20] Their 'radicalism' is then either something absurd,

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[16] Sam Harris, *The Moral Landscape: How Science Can Determine Human Values* (London: Transworld Publishers, 2012)

[17] Kitcher has recently discussed this divison in his vision for a pragmatist philosophy of science. Philip Kitcher, 'Toward a Pragmatist Philosophy of Science', *Theoria*, 77 (2013), 185-231 (p.189)

[18] Rorty gave his own version of two types of readings between 'ironists' and 'metaphysicians', where our prior beliefs and philosophy guide what we take to be as acceptable as questions, conclusions and arguments. Richard Rorty, 'Ironists and Metaphysicians', in *The Truth About Truth: De-confusing and Re-constructing the Postmodern World*, ed. by Walter Truett Anderson (New York: Penguin Putnam, 1995), pp.100 -106

[19] Pp.146 -153 in thesis.

[20] Steffano Gattei, *Thomas Kuhn's "Linguistic Turn" and the Legacy of Logical Empiricism: Incommensurability, Rationality and the Search for Truth* (Aldershot: Ashgate Publishing Limited, 2008), p.93

devaluing the role of philosophy, or it is politically desirable to 'interest groups' who may wish to challenge the authority of orthodox science.

As I think historical context is important, I will also be looking at the history of the 'PUoS' movement in the UK and the debates it has raised. It is here that I will pick up on an 'anti-philosophy' motif, but at the same time look at ways philosophy might be relevant. The question 'what is science?' will be crucial to my story, as implicit within it, is part of the methodological narrative that has pushed philosophy 'out of the picture'. For in order to understand what science is, we also have to understand what science is not. Within the philosophy of science this is known as the 'problem of demarcation' (PoD). I will present two orthodox readings of science and philosophy surrounding the PoD. The first will be of Karl Popper and his attempt to answer this question through his principle of falsification. It will look at how the scientific community regarded his contribution and what exactly they thought he was contributing to. In contrast to Popper, I will present one of his students, Paul Feyerabend, his claims against a scientific method and his views on falsification. I will argue that, if you come to these thinkers and ideas through a prior 'strong' understanding, Popper will appear as the heroic de-bunker of superstition through his criteria of testability and Feyerabend comes off as a 'radical'.

Next I reverse this claim by completing a close-text analysis and deconstruction of Thomas Kuhn's ideas in *The Structure of Scientific Revolutions*.[21] Given the importance that Kuhn and *Structure* have had for the development of philosophy of science and the social sciences, I feel that it makes an apt target. I will then offer both a 'strong/ methodological' and 'weak/ historical' reading that are in contradiction with one another. I will also use original concepts inspired from another apparent radical Martin Heidegger. Both of these actions are deliberate. Firstly, what I hope to show with the contradictory readings of Kuhn is that in order to derive either, one must already be reading Kuhn with that understanding of science and philosophy in mind. Secondly, one of those readings creates a useless impotent conception of science made only possible by the presence of a more useless philosophy. Here we are left with a choice if we are to conclude that science does not make progress, is not objective, or operates irrationally. The choices being either give up science for some other cultural narrative with which to explain the world or give up philosophy as a worthwhile endeavour. There is, however, another reading that will make Kuhn and philosophy useful in understanding the question 'what is science?' I do not, however, offer this up as a 'turn' towards pragmatism, as this has been one of the consequences of reading Kuhn in a certain way.[22] Here neo-pragmatists, postmodernists and 'strong' sociologists have appropriated Kuhn to their own ends. Rather, I employ the utility in their reading as an act of philosophy, in not only understanding the arguments but the possibilities for the very position one is arguing from. There is a political-ethical component to this that one reading closes off interpretation, and so comes to define science and philosophy for us. The other opens up ways of reading Kuhn, and at the same time allows us to challenge dominant interpretations. One of the original contributions I will be making is developing my two readings in line with two types of languages, which I have called 'about' and 'of' languages. These have their analogue in the 'strong/methodological' and 'weak/historical' readings. An 'about' language is a discourse, or proposition based. It is simply what something is about. Mathematics is about numbers, quantities and their relationships, bike riding is about a leisure activity, the Highway Code, bio-mechanical processes and so on. An 'of' language is non-propositional and is what allows an 'about' language to form. It is the 'doing' of what an 'about' language refers to. So the language of mathematics is proofing, completing operations, and so on, the language of bike riding is riding a bike. What is unique to the 'of' language is, it is something *only* humans can do as ultimately it has to do with a tacit understanding of our world and practices. One of the conceptual problems this way of thinking may help us with, is whether one understands if an 'about' language precedes an 'of' language or vice-versa.

The purpose of Heidegger, like Kuhn, is rather than to dismiss a person as radical or nonsensical (strong Kuhn), we should engage with what these people wrote and thought and try to make their work relevant to modern problems. As Heidegger is seen as a radical, irrationalist or someone we might not associate with Kuhn let alone the PUoS, it is again the work of the philosopher to show why these things matter. In

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[21] Thomas Kuhn, *The Structure of Scientific Revolutions*, 3rd edn. (Chicago: The University of Chicago Press, 1996). Referred to as *Structure* from now on.

doing so, they show why philosophy matters. I suspect that for the same reasons that we might understand Kuhn a certain way, due to a prior understanding, is also true of 'mainstream' Heidegger. This is not to say that we can let people get away with saying whatever they want and blame the lack of sense on our inability to grasp their level of analysis. If a claim is meant methodologically as it is with the pseudo-sciences or something like 'Intelligent Design', we are at liberty, or even morally obliged to scrutinize those claims. In the last instance, it is not about what Heidegger or Kuhn 'actually' meant, but using the process of philosophy as a means to interpret the problems of the modern world. Hence this thesis is not about Kuhn, Heidegger or another theory/philosophy of science, but is about the role philosophy can play in helping the public understand science and where we might reach absurd conclusions working out why.

Throughout this thesis, I will be drawing on the work of the scientist Richard Feynman.[23] I do so for a number of reasons. Firstly, the 'received view' of Feynman is a no-nonsense scientist and hater of philosophy.[24] I hope to show that the 'philosophy' Feynman disliked was a rather narrow conception of philosophy, and in his pronouncements on science was actually very philosophical. Just as we can relax and not think about what Kuhn wrote as it has been interpreted for us, many scientists adopted Feynman's views without considering what he said or what views he was actually attacking. Here, I take sympathy with the work of Pierre Hadot where philosophy is not just a subject but a 'way of life'.[25] That it is up to us to identify previous thinkers and schools of thoughts as 'models of life' to which we then have to think though for ourselves in line with the tradition of philosophy as a whole.[26] That is 'to take account of all the concrete conditions in which they wrote […] the framework of the school, the very nature of *philosophia*, literary genres, rhetorical rules, dogmatic imperatives, and traditional modes of reasoning.'[27] This is then not to place

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[22] Christopher Norris, *Against Relativism: Philosophy of Science, Deconstruction and Critical Theory* (Oxford: Blackwell Publishers, 1997a), p.82
[23] A web survey of 400 physicists by 'Physics World' had Feynman rank seventh greatest of all time. "Newton Tops PhysicsWeb Poll," *Physics World.Com*, 09 Nov. 1999 <http://physicsworld.com/cws/article/news/1999/nov/29/newton-tops-physicsweb-poll>
[24] See p.105 for examples of Feynman on philosophy
[25] Pierre Hadot, *Philosophy as a Way of Life: Spiritual Exercise from Socrates to Foucault* (Oxford: Blackwell Publishing Ltd, 1995)
[26] Ibid., p.271
[27] Ibid., p.61 [Italics in original]

any one ideal of wisdom above any other, but as ‘a way of living’, making it relevant to our lives and the problems we face. My fear is that if philosophy ceases to be part of public discourse, it fossilizes. We then end up with an understanding of philosophy that it is what people did before the scientific method, and now we can let science interpret the world for us. Philosophical themes can then be subsumed by other discourses that would not be recognized as metaphysical or transcendental. At best, it will just be an endless back and forth about the scientific proof for God, morality, or beauty. At worst, we lose what is human about us, the ability to self-interpret, and instead found our meanings in science that it is some how exterior to us.

1.3 - Is to be Scientific to be Anti-Philosophy of Science?

The context surrounding the relationship between science, philosophy, and the general public, which I am concerned with, is a reaction to the period of academic exchange called the ‘Science Wars’. It was here that the Enlightenment ideals of ‘truth’, ‘objectivity’, and ‘reason’ appeared to be challenged by ‘postmodern’ critics.[28] Indeed, whom we take as our ‘postmodernist’ is worthy of a thesis in itself. The analytic-pragmatist end (e.g., Searle or Rorty) would have someone like Derrida as the protagonist, whereas, from the deconstuctivist position (e.g., Derrida or Norris), someone like Rorty is the perpetrator.[29] The response from ‘science’ can be summarized by E. O. Wilson’s take on this exchange:

> The ongoing fragmentation of knowledge and resulting chaos in philosophy are not reflections of the real world but artefacts of scholarship. The propositions of the original Enlightenment are increasingly favoured by objective evidence, especially from the natural sciences […] Postmodernism is the ultimate polar antithesis of

[28] Barringer labels the ‘sides’ in the science wars as conflict between modernist and post-modernists. Philip S. Barringer, ‘Introduction: The “Science Wars”’, in *After the Science Wars*, ed. by Keith M. Ashman and Philip S. Barringer (London: Routledge, 2001), pp.1-12 (p.4)

[29] I would side with the Derridean analysis of Rorty or Searle as being closer to the ‘postmodernism’ that I am trying to address. It is not that we can not say ‘true’ things but that it is necessary to have a more sophisticated understanding of ‘truth’ if one is to critique it.

> the Enlightenment […] Enlightenment thinkers believe we can know everything, and radical postmodernists believe we can know nothing.[30]

This 'fragmentation of knowledge' and perceived 'chaos in philosophy', are indeed artefacts of scholarship, but have their basis in real world practices. The fragmentation and chaos I would like to draw attention to, are the confusions one can arrive at if insufficient understanding is given to the philosophical issues at stake. My main argument in this thesis will centre on the conclusions one can reach about science, if we conflate or confuse the use of philosophy as a 'methodological' or 'historical' approach. Is science an abstract canon of precepts that are universal and true, which one can derive merely by reason alone, or is science only ever what people do and is subject to a time and place? Fuller and Collier blame philosophers for this confusion, but I argue that it is by dismissing philosophy as merely misguided positivism that we by-pass a potent source of this confusion.[31] A philosophical problem at the heart of PUoS is the PoD, for if we ask people to understand what science is, we are also teaching them what science is not. As a commonsense question, this appears to be intelligible, but as a philosophical project, it has failed to distinguish science from non-science. I will argue that depending on how one thinks about this problem, we may find the source of many of the tensions that became apparent in the 'Science Wars'. We have to ask ourselves, is the PoD a problem of methodology (is it deductive, inductive, verifiable, falsifiable and so on) or is it a historical problem (science is what scientists do at a particular time)? The tensions that I refer to have already been alluded to in what weight of legitimacy we give to ideas such as 'truth' over 'Truth' or 'rationality' over 'Reason'. Philosophy will hopefully show us where metaphysical ideas have come to substitute for the concrete actions of people.

Another tension, between philosophy and science, is the majority of philosophers who appear to be pro-science, and the publicly active scientists who appear to be anti-philosophy.[32] As philosophers are free to comment on science, so too do scientists

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[30] Edward O. Wilson, *Consilience: The Unity of Knowledge* (New York: Vintage Books, 1999), pp.8 - 44

[31] Steve Fuller and James H. Collier, *Philosophy, Rhetoric, and the End of Knowledge: A New Beginning for Science and Technology Studies* (Mahwah, NJ.: Lawrence Erlbaum Associates, 2004), p.8

[32] Sanitt diagnoses an attitude of 'apathy' rather than 'anti-philosophy' among scientists, which does not affect my main point. Nigel Sanitt, 'The Tripod of Science: Communication, Philosophy and

feel free and qualified to comment on philosophy. At the top of most short lists of quotes from scientists and mathematicians that have appeared to speak out publicly against philosophy as means to understanding science is Richard Feynman who said, 'philosophy of science is about as useful to scientists as ornithology is to birds.'[33] One can take this statement a number of ways, but what is significant about Feynman saying it, is that he is regarded by most scientists (especially physicists) as the embodiment of the scientific enterprise. He not only won the Nobel-Prize, made fundamental contributions to science, apparently started the nano-technology revolution, but also solved the *Challenger* space shuttle disaster.[34] Another Nobel-Prize winner, Steven Weinberg, also wrote a chapter simply titled 'Against Philosophy'.[35] Weinberg intentionally contrasts his view with that of one of the 'Conant/Harvard' group, Thomas Kuhn. Here, he concludes that because paradigms cannot be compared to any objective criteria, then one worldview cannot be privileged over another putting shamanism, astrology, and creationism on an equal footing with 'science'.[36] Weinberg does not hesitate to say that science and he are in the business of getting 'closer and closer to objective truth'.[37] Saulson notes that 'Weinberg goes out of his way to contrast his point of view with Thomas Kuhn's position that it was nonsensical to talk about scientific progress taking us 'closer to the truth'.[38] Moving away from Nobel-Prize winners, Stephen Hawking not only diagnoses the death of philosophy, but in *A Brief History of Time*, portrays early attempts at understanding reality as proto-scientific. He calls Kant's the *Critique of Pure Reason* 'very obscure' and reduces the difference between Aristotle, Galileo, and Newton to ideas about inertia.[39] He also gets factual claims about Aristotle's philosophy wrong.[40] Sokal has written extensively about how misguided the

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Education', in *Science Matters: Humanities as Complex Systems* ed. by Maria Burguete and Lui Lam (London: World Scientific Publishing Co., 2008), pp.121-135 (p.125)

[33] Philip Kitcher, 'A Plea for Science Studies', in *A House Built on Sand: Exposing Postmodern Myths About Science*, ed. by N. Koertge (Oxford: Oxford University Press, 1998), pp. 32-56 (p.32)

[34] Lawerence M. Krauss, *Quantum Man: Richard Feynman's Life in Science* (New York: W. W. Norton and Company, 2011), pp. xiv-xvii

[35] Steven Weinberg, *Dreams of a Final Theory* (New York: Pantheon Books, 1992), pp.166-190

[36] Steven Weinberg, 'Physics and History', in *The One Culture? A Conversation about Science*, ed. by Jay A. Labinger and Harry Collins (Chicago: The Chicago University Press, 2001), pp.116-127 (pp.124-125)

[37] Ibid., p.124

[38] Ibid., p.286

[39] Stephen W. Hawking, *A Brief History of Time* (London: Bantam Books, 1998), p.8 & p.17

[40] 'Aristotle believed Empedocle's theory that everything was made out of four elements'. Ibid., p.10 – Aristotle postulated five elements, earth, air, water, fire and aether. This mistake is also replicated in *The Grand Design* (p.51) For further criticism of Hawking's positions see Christopher Norris,

humanities can be, and the constant threat of relativism from ‘sloppy thinking’ which act as a barrier to social progression.[41] Physicist and honoured educator, Lawrence Krauss, received the National Science Board’s ‘Public Service Award’ for ‘his extensive, broad outreach to the public that bridges the chasm between science and popular culture as well as science and public policy.’[42] After a heated exchange with philosopher, David Albert, Krauss spoke publicly deriding philosophy of science as the useless cousin of science. In an interview with *The Atlantic,* he said, ‘people in philosophy feel threatened, and they have every right to feel threatened, because science progresses and philosophy doesn't.’[43] He, like Hawking, sees philosophy as a reactive process to new discoveries in science, and one that adds nothing to the scientist’s work. Lastly, the current heir to Sir David Attenborough’s throne as the communicator of popular science in the UK is Brian Cox. He has publicly expressed his dislike of philosophy on more than one occasion. Early on in the ‘Huw Wheldon Memorial Lecture’ in 2010, Cox tries to define science for his audience, repeating yet again Feynman’s famous statement,

> First to define what science is. Now, this is not easy in a historical context, because, to put it bluntly, vast amounts of drivel have been written about the subject by armies of postmodernist philosophers and journalists. But, I’m going to ignore all this, because I concur absolutely with the quote attributed to the Nobel-Prize winning physicist, Richard Feynman. He said that the philosophy of science is about as useful to scientists as ornithology is to birds.[44]

This quote neatly takes us back to E. O. Wilson’s worries on the postmodernist threat to knowledge and Feynman’s place in the science pantheon.

“Hawking Contra Philosophy”, *Philosophy Now*, 82 (2011) <http://philosophynow.org/issue/82/Hawking_contra_Philosophy> [accessed 17 October 2011]

[41] Sokal, *Beyond the Hoax*, pp.107-108

[42] ‘Public Service Award Recipients 2012’, *National Science Board* <http://www.nsf.gov/nsb/awards/public_recipients.jsp> [accessed 17 Oct 2012]

[43] Ross Anderson, “Has Physics Made Philosophy and Religion Obsolete?”, *The Atlantic*, 23 April 2012 <http://theatlantic.com/technology/archive/2012/04/has-physics-made-philosophy-and-religion-obsolete/256203> [accessed 11 June 2012]

[44] Brian Cox, ‘Huw Wheldon Memorial Lecture: Science – A Challenge to TV Orthodoxy’, *BBC 2 The Royal Television Society*, 02 December 2010

1.4 - Philosophy in the Public Understanding of Science

At first glance, it would seem the public are encouraged by people like Brian Cox, Lawrence Krauss, and Stephen Hawking to ignore what philosophers might have to say about science. Indeed philosophy seems to just get in the way of learning about science and so is an unnecessary hurdle. The first large-scale Governmental reports into the PUoS in the UK reveal in their entirety two uses of the word 'philosophy'. In *The Public Understanding of Science* and *Science and Technology* reports, the word 'philosophy' is used as a synonym for 'theory'.[45] The second use of the word 'philosophy' is in the *Science and Technology - Sixth Report*, where it is discussed in association with 'complementary and alternative medicines'.[46] 'Many conventional medical scientists, while accepting the validity of accumulative empirical observation, believe that those therapeutic disciplines that are based principally on abstract philosophy and not on scientific reasoning and experiment have little place in medicine.'[47] This reasoning appears to be deaf to the idea that 'empiricism' or even 'reason' are actually philosophical ideas. They are as 'abstract' as the therapeutic disciplines they accuse. The failure to include any philosophical aspect of science in both reports and philosophy's alignment with esoteric practices is, I argue, a symptom of a wider confused understanding of philosophy and science. The opening paragraph of chapter 3 from the *Science and Technology - Third Report* says:

> 'Public understanding of science' means the understanding of scientific matters by non-experts. This cannot of course mean a comprehensive knowledge of all branches of science. It may however include understanding of the nature of scientific methods, including the testing of hypotheses by experiment. It may also include awareness of current scientific advances and their implications.[48]

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[45] Walter F. Bodmer, *The Public Understanding of Science* (London: The Royal Society, 1985); House of Lords, *Science and Technology -Third Report* (London: HMSO, 2000), Appendix 4

[46] House of Lords, *Science and Technology - Sixth Report*, para 2.12 – 2.23. Here the term is used in association with complementary and alternative medicines eleven times. Interestingly the term is never used in conjunction with science. It is this general acceptance that science is free or not indebted to philosophy and only the weird or fringe beliefs can be sustained or supported by philosophical notions. This would seem to be either a comment on the perceived usefulness of philosophy to the general public or how well understood philosophy as a practice is.

[47] Ibid., para. 2.17

For *The Public Understanding of Science* report, 'understanding includes not just the facts of science, but also the method and its limitations as well as an appreciation of the practical and social implications.'[49] If there is a use for philosophy, I think it is most effectively deployed in and around such statements. What is a scientific fact? Is there a single, universal scientific method? How do we regard limitations – are they the result of inadequate knowledge, limited cognition or part of a fundamental intractability in nature? How do barriers to scientific research in the past differ from conceptual barriers today? The scope of philosophy of science is as broad as it is deep, but if one is not aware of such issues, 'some understanding of science, its accomplishments and its limitations' hardly seems to be appropriate for informing a general public and creating a genuine understanding of science.[50] Similarly, an over appreciation of the historic-socio-political aspect of science would imply that scientific knowledge could be replaced with sociology of science, which has been a source of antagonism between the natural and social sciences.[51]

Continuing with the social sciences, there are two primary models for understanding how information is communicated within the PUoS, the 'contextual' and 'deficit' models.[52] The contextualist approach suggests that people 'use' knowledge rather than 'have' knowledge as per the 'deficit' model. 'People will pick up the knowledge they need for the task at hand, use it as required, and then put it down again […] it will not be ready to hand when the survey interviewer next asks them if, for example, an electron is bigger than an atom.'[53] Knowing a fact about whether an electron is bigger than an atom is part of what is called the 'deficit' model. Miller argues that there has been a shift from the 'deficit' model, a passive form of transmitting knowledge, to the 'contextual' approach, which is an active mode of meaning generation. Here, the public displays 'local knowledge and an understanding of, and personal interest in, the problems to be solved which are of direct relevance to the

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[48] House of Lords, *Science and Technology – Third Report*, para 3.1
[49] Bodmer, *Public Understanding of Science*, p.6
[50] Ibid.,
[51] For a good example of these exchanges see, *The One Culture? A Conversation about Science*, ed. by Jay A. Labinger and Harry Collins (Chicago: The Chicago University Press, 2001)
[52] Patrick Sturgis and Nick Allum, 'Science in Society: Re-evaluating the Deficit Model of Public Attitudes', *Public Understanding of Science*, 13 (2004), 55-74
[53] Steve Miller, 'Public Understanding of Science at the Crossroads', *Public Understanding of Science*, 10 (2001), 115–120 (p.118)

individual or community.'[54] The development of these two models is an artefact of the developing field of science communication studies, which will be looked at in more detail further on.[55]

What seems to unite the 'deficit' and 'contextual' models is a kind of ethical concern over the usefulness of knowledge. The deficit model says knowing *x* amount of facts equates to scientific understanding, and so your life and society will improve by proxy. The 'contextual' model says people will only use scientific knowledge that is relevant to their everyday lives, and it is by this that we should measure understanding. Either way, 'scientific understanding' is a means-ends driven relationship. Much of what the UK went through in the 1980-90's, as a part of the development of science education and communication studies, was anticipated in America in the 1940-50's. Where this ties into my narrative is that the philosophy and approaches to studying science, that grew out of what Edmund called the 'Harvard/Conant group', is responsible for one of the central figures of this thesis, Thomas Kuhn.[56] It was from the work that came out of the 'Harvard/Conant' group that the perceived threat to science's epistemic authority grew. I believe there is a present threat, but it stems as much from a naïve reading of science as it does from the antagonists of the defenders of 'Reason', such as Gross and Levitt.[57] They argue that intellectual fashions amongst the academic left have discouraged critical thinking about a 'slender body of work'.[58] I will argue that this problem of a lack of critical thinking is endemic to science education, as it does not seek to uncover the very grounds for formulating the rational-critical position it has assumed in order to understand science as only a method. That is, what does it take to understand science unproblematically as *only* a method?

1.5 - The Harvard/Conant Group

James B. Conanat was Harvard President who developed the General Education in Science curriculum. His work on the Manhattan Project convinced him that the public

[54] Miller, 'Science at the Crossroads', p.117
[55] Pp.24-36 in thesis
[56] Edmund, *End The Biggest Educational and Intellectual Blunder in History*, p.xi
[57] Paul R. Gross and Norman Levitt, *Higher Superstition: The Academic Left and its Quarrel with Science* (Baltimore: John Hopkins University Press, 1994)
[58] Ibid., p.239

needed a better understanding of science, that public education was the driving force behind democracy, and that his approach to understanding science was, in part, historical. This 'Harvard' connection is relevant because not only was Conant mentor to Thomas Kuhn, but Willard Van Orman Quine was also a fellow colleague.[59] All of which had an impact on Kuhn's philosophy of science.[60] Conant envisaged a more 'hands on' approach to the public's understanding of science. This was in order to remove any fear of the practice of scientists, to view them as ordinary workmen doing their job. However, the analogy with the 'tradesmen' tends to breakdown when 'Big Science' gets involved. We trust a mechanic with our car because they should be able to do things we cannot or know things we do not, but on the whole it is on the expectation that the car will work better than it did before. Can the same be said of Big Science? Fuller states that Conant himself made this point, that 'the need to identify science with the craft of doing science, which is portrayed as something quite distinct from simply being "well-informed" about the results of science, a state of mind that Conant denigrates as instilling a "merely" critical, outcomes-oriented attitude'.[61]

This division between science as 'knowing facts' and science as 'process' seems to reappear in the UK in the 1970's. *The Rothschild Report* introduced the 'customer-contractor' principle.[62] This basically stated that 'basic research was the province of the universities and research councils […] while applied R & D has 'a practical application as its objective.'[63] It urged the Government to 'resist the view that there is no logical division between pure and applied research, a view that maybe intended to protect the Research Councils from the imaginary ravages of applied R. & D. users.'[64] It was treated as consultative document, where 'almost immediately the government formally accepted

[59] Whilst this thesis is dedicated to a treatment of Kuhn, Quine features heavily in the story of twentith century philosophy of science and the demise of logical positivism. The sorts of things that Quine pioneered such as 'ontological relativity' we find in Kuhn but for the sake of space I have chosen to concentrate on Kuhn. Norris, *Epistemology*, p.7

[60] B. Biddle, 'Putting Pragmatism to Work in the Cold War: Science, Technology and Politics in the Writings of James B. Conant', *Studies in History and Philosophy of Science*, 42 (2011), 552-561 (p.552)

[61] Steve Fuller, 'Being There with Thomas Kuhn: A Parable for Postmodern Times', *History & Theory*, 31 (1992), 241 – 275, (p.265)

[62] The Rothschild Report, *A Framework for Government Research and Development* (London: HMSO, 1971)

[63] Maurice Kogan, Mary Henkel & Steve Hanney, *Government and Research: Thirty Years of Evolution* (Dordrecht: Springer, 2006), p.14 [Quoting the Rothschild Report para.6]

[64] The Rothschild Report, *Framework*, para.25

the idea of the customer-contractor principle'.[65] This was an organizing principle that made a sharp distinction between pure and applied scientific research. The 'customer-contractor' principle regarded pure science as knowledge for knowledge's sake that is aimed at no one individual. Applied science was seen as a response to a specific problem, customer *A* wants *X*, the contractor makes *X* possible, customer *A* pays contractor. Here, the autonomy of scientists was tethered to market forces. Just as Conant wanted to mark science as a craft (practice) out from simply being 'well informed' (knowledge), the 'customer-contractor' principle argued for a division between pure abstract science and the concrete problems of applied science.

1.6 - Scientific Literacy

One of the metaphors deployed in the PUoS and science education is that of 'literacy'. One could equally use phrases such as 'competency', 'proficiency', or 'fluency'. Even though I will refer to 'literacy' throughout this thesis, I do not think the metaphor is appropriate. The implication is that if one is literate you know enough to do something well. The role I wish philosophy to play, conversely, is not aimed at a specific subject matter, but how subjects come to be. The PUoS may be about science but philosophy as an activity is not about a subject but about how we ground knowledge and reason which, if done well, takes in the process of 'grounding' itself. Here, philosophy is not a meta-language that trumps science, but is a supplement to it. The sort of reality that is presupposed in a metaphor like 'literacy' will not escape analysis, so this is another reason why 'literacy' may not be the best choice. John Durant describes scientific literacy as 'knowing how science really works'. [66] What people like Durant and Miller mean by this is that we do not abstract knowledge to a collection of objective statements, or rely on 'methodology-based definitions' of science, but see science as a messy public affair.[67] The 'facts' have to be argued for, not simply observed, since which 'facts' get investigated are a product of social pressures, and what 'facts' get accepted as current knowledge are subject to change. This, however, belies a deeper problem about how one understands such statements with regard to the methodology-based definitions as favoured by science educators.

[65] Kogan, Henkel & Hanney, *Government and Research*, p.14
[66] John Durant, 'What is Scientific Literacy?', in *Science and Culture in Europe*, ed. by John Durant and Jane Gregory (London: The Science Museum, 1993), pp.129-137 (p.134)

For example, the current Key Stage 3 curriculum for science in England states that 'Pupils learn how knowledge and understanding in science are rooted in evidence.'[68] Evidence is always evidence for some theory, which can only be a 'scientific' theory if it is part of some wider worldview. This is only part of a definition that includes, among other things, 'scientific thinking' which uses 'scientific ideas and models to explain phenomena'.[69] This was also the same Key Stage 3 strategy that included 'scientific literacy' as a goal.[70]

Where the 'literacy' metaphor also comes unstuck is in its implication for competence. If one is grammatically literate, one has learnt the rules of grammar, sounding phonics, syntax structure and so on. We are then free to read any amount of literature we like and even pass comment on the literacy of others. When we say public understanding should be a form of literacy, is the same true for science? The implication is that 'understanding' is as uniform as learning the rules of grammar or sounding phonemes. What one scientist understands as scientific so does another, and by following the simple rules of scientific method we can converse and understand the world scientifically. We might forgive those who are ill-educated (illiterate) and understand the world in terms of mystical forces or phenomena that defy current scientific consensus. Yet, without too much effort we can find Cambridge professors of physics who do not say that elementary particle physics are fundamental to understanding reality, but theology. Theology is the 'deepest level of understanding'.[71] There are biochemists who, on the 'evidence', hold the Earth to be around five thousand years old and evolution is a myth.[72] Moving away from a 'religious' orientation, we can find Nobel-Prize winners of physics who argue for telepathy and 'memory' in water.[73] Can we say these people do not know the 'rules'

[67] Miller, 'Science at the Crossroads', pp.118-119.
[68] DfE, 'Science: Programme of Study', in *The Statutory Programme of Study for Key Stages 3 and 4 Science* (London: HMSO, 2011) <http://www.education.gov.uk/schools/teachingandlearning/curriculum/secondary/b00198831/science/ks3/programme> [Accessed 13 Feb. 2012]
[69] Ibid.,
[70] DfES, 'Literacy in Science' in *Key Stage 3 National Strategy* (London: Department for Education and Skill, 2002)
[71] John Polkinghorne, *Science and Creation: The Search for Understanding* (London: SPCK, 1988), p.1
[72] Duane T. Gish, *Evolution: The Fossils Say No!* (San Diego: Creation-Life Publishers, 1979)
[73] Brain Josephson the recipient of the Nobel Prize in Quackpottery – GrrlScientists, 'Noble Prize in Quackpottery: Physics', *The Guardian*, 9th Oct. 2012

of science or its grammar? We cannot accuse them of being illiterate, and yet we have this nagging feeling that what they say is not scientific. Or is it? What this should demonstrate is that understanding, unlike literacy, is not an either/or. Literacy is linked to the things one is supposed to be literate of, but understanding and science are linked to the world and our place in it. Hence, we can find scientists who are technically literate but do not hold the scientific 'worldview'. The place I have for philosophy is aimed at this worldview and how we come to hold it. A worldview comes from somewhere, as do our own understandings, but it is our prior understanding of the world that gets in the way of seeing how philosophy might be relevant.

1.7 - Rationale and Methodology of Thesis

In this thesis, I am not arguing for another philosophy of science, I do not make the case for a better abstraction of the scientific method or prescribe how science should be done.[74] Rather, I am arguing that philosophy as a method or approach needs to be included to help make our worldviews present to us. This would also be to reflect on what one holds as a philosophical doctrine as well, be it the empirical-positivistic position or transcendental idealism. What I hope to achieve is echoed by Rudolf Carnap's *Wissenschaftslogik*, where he was not concerned with explaining or justifying scientific knowledge by exhibiting some ultimate basis for it. Rather, he was concerned with developing a new role for philosophy (via the empirical sciences), that would contribute to scientific progress, whilst at the same time, avoiding all the traditional philosophical disputes which, for the logical positivists, were a serious obstacle to progress in both science and scientific philosophy.[75] My purpose is not to advance science or scientific philosophy but improve public comprehension of scientific issues. I will argue how the 'methodological' conception of science and all its associated philosophical content have come to dominate how we think about science, no matter whether it is internal or external to scientific discourse.

<http//:guardian.co.uk/science/grrlscientist/2012/oct/09/nobel-prize-quackpottery-physics> [accessed Jan 3 2013]

[74] Or as what Feyerabend says in *Against Method* 'my intention is not to replace one set of general rules by another such set: my intention is, rather, to convince the reader that *all methodologies, even the most obvious ones, have their limits*.' Feyerabend, *Against Method*, p.23 [Italics in original]

[75] Michael Friedman, 'Carnap on Theoretical Terms: Structuralism without metaphysics', *Synthese*, 180 (2011), 249-263 (p.259)

I will offer instead an alternative interpretation of science as ‘historical’, which also interprets terms such as ‘truth’, ‘reason’, and ‘knowledge’ as historical objects. My point will not be to show one is true and the other false, as this would still be dealing in a methodological framework, but to merely show the errors that occur if we were to view one interpretation through another. That is, what happens if we treat an ostensibly historical question as if it were a methodological one. Here I will argue not only that this is the source of a lot of philosophical problems, but that this entire exercise of thinking in ‘methodological’ and ‘historical’ modes is to be philosophical, which is what I want the PUoS to address. That is, with the changes in emphasis in what our investigation is trying to achieve changes the investigation itself. So, I would not just want ‘science’ to be thought about as a ‘methodological’ and ‘historical’ practice, but philosophy itself.

This is why I would not want to say that one particular ‘philosophy’ is a priori correct, but rather to have the ability to turn a subject against itself and to see where the faults lie. Just as with PUoS, I do not want people to accept a second-hand account of what science is. Similarly, I do not want people to accept a second-hand account of what philosophy is either. This, I will demonstrate through the PoD as traditionally understood as a methodological problem, but also what can happen if we understand it as a historical problem. I will then attempt to invert this trajectory by developing two contradictory readings of Thomas Kuhn’s *Structure*. Through ‘methodological’ and ‘historical’ readings, I will illustrate the problems and solutions one can derive. As I must practice what I preach, I have decided to include as part of my methodology conceptual tools, inspired from a notoriously obscure philosopher who has been marginalized as one of those purveyors of ‘sloppy thinking’, Martin Heidegger. For good measure, I also include another exponent of deconstruction and widely understood ‘peddler’ of ‘postmoderist drivel’, Jacques Derrida. By using the tools they helped develop, I hope to show that we do not just ‘read’ or ‘understand’ a text as the PUoS would wish us to do, but we always ‘read’ or ‘understand’ something ‘as’ something. A very good example of this is in the introduction to Kripke’s *Wittgenstein on Rules and Private Language*, where he writes,

> In the following, I am largely trying to present Wittgenstein’s argument, or, more accurately, that set of problems and arguments which I personally have gotten out of reading Wittgenstein […] so the

> present paper should be thought of as expounding neither 'Wittgenstein's' argument nor Kripke's: rather Wittgenstein's argument as it struck Kripke, as it presented a problem for him.[76]

What this shows is that the problems we are struck by are not latent in the text, as we would all come away with the same problems or understanding, but we carry a set of a priori beliefs about how things are or should be. For Kripke, it was the genuine paradox of rule following and what this meant for language that was important, even though he knew Wittgenstein would probably disagree.

By deconstructing and reconstructing two versions of Kuhn's influential *Structure,* I will present two contradictory readings and the problems the 'strong' and 'weak' reader will be 'struck' by. By showing that a certain interpretation is not inevitable and indeed may even be undesirable, this will be an illustration of the role I would want philosophy to have in PUoS.[77] As this thesis is not about Heidegger or Kuhn but how we can use philosophers to clarify problems or dissolve them, there will only be a surface engagement with Heideggerian themes. Where relevant, I take up a Feyerabendian position on the role of philosophy as an ethical-political concern in education. I do not want to argue over what they 'really' meant as might be done in a typical philosophical academic thesis, but how can we make them useful for a modern age that is sadly lacking philosophers.

Next I am going to elaborate on the themes mentioned above with a closer look at the history of the PUoS, the social scientific work surrounding science communication and the central question to PUoS 'what is science?' We will see how this has informed decisions from governmental policy to how public scientific literacy is measured. It also harbours the implicit notion that once we are deemed to know what science is, we can then tell what science is not. This becomes problematic when repeated attempts to define science through methodological criteria have failed. This failure is then levelled at the discipline where we see philosophy being particularly

[76] Saul A. Kripke, *Wittgenstein on Rules and Private Language* (Cambridge, MA.: Harvard University Press, 1982), p.5

[77] It has been noted to me that my thesis has a Hegelian format of thesis, anti-thesis and synthesis.

inept at solving its own problems rather than any inherent problem with the way we think about science.

Chapter Two

The Public Understanding of Science

2.1 - A Brief History of the Public Understanding of Science (PUoS)

The relationship the public has to science is not new, but as a part of social policy it is relatively recent. With 'interest groups' raising doubts over climate change or pressure to reform science curriculums it has been noted there is, even within developed countries such as the USA, a 'growing anti-science streak on the American right.'[1] In 2011, the Chief Scientific Advisor to the UK government, John Beddington, declared that we should be 'grossly intolerant of pseudo-science, the building up of what purports to be science by the cherry-picking of the facts and the failure to use scientific evidence and the failure to use scientific method.'[2] Whilst I agree that pseudo-science masquerading as science proper should not be tolerated, I think Beddington's criticism is misplaced. Beddington is forwarding what I will later be discussing as the methodological conception of science. This version of science has no need of philosophy as it works off very simple principles, such as facts, evidence, and method. The stance that I will contrast this with is the historical approach which tells us how we come to see something 'as' evidence, what sort of metaphysics must be presumed in order to have 'facts', and where the idea of 'method' as distinct from its history has come from.

I will argue that the methodological view is open to abuse because it does not acknowledge the philosophical content of its own ideas. So one may be able to induce inductive pessimism over scientific progress or the available evidence because we have this detached view of how science works. To this end, unwarranted shadows can be cast over issues such as climate change that can use philosophical 'sleights of hand' to forward such causes.[3] With the advent of the Internet, information, as well as

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[1] Editorial, "Science Scorned", *Nature,* 467 (7312), Sept. (2010), p.133 <http://www.nature.com/nature/journal/v467/n7312/full/467133a.html> [accessed 20 August 2011]
[2] Stuart Allan, 'Introduction: Science Journalism in a Digital Age', *Journalism*, 12 (2011), 771-777 (p.773)
[3] See global warming sceptic and climatologist, Patrick Michaels, *Meltdown: The Predictable Distortions of Global Warming by Scientists, Politicians, and the Media* (Washington: Cato Institute, 2004); *Climate of Extremes: Global Warming Science They Don't Want You to Know* (Washington: Cato Institute, 2009)

misinformation, is easy to propagate with special interest groups lobbying governments and big corporations for change.[4] Now, it is as important as ever to make sure the public do not just know some science but understand it, which for me, means understanding what is at risk if we only know the methodological interpretation of science. To get at the centrality of this problem, I will next look at the recent developments in the PUoS movements at academic and governmental levels within the UK. The distinction between science as a method (something abstract) and science as a practice (lived activity) will be highlighted by the debates being had over science as pure or applied research.

In order to see where the PUoS schemes came from and what they were designed to address, it is important to spend some time looking at the pre-PUoS landscape of 1970's Britain. Governmental science policy and history of science have generally gone hand-in-hand, with this field of study opening up during the 1970's. Historians such as Roy MacLeod, followed shortly by the establishment of the 'Science Policy Research Unit' (SPRU) at Sussex University, were at the forefront of developing this dialogue.[5] The historical links between science and cultural attitudes towards development and innovation in science were what dominated the political landscape on science, funding and public engagement around this time. The activities of scientists and how funds were allocated were thought to be protected by the 'Haldane Principle'. The idea was to keep the machinations of politicians and the autonomy of scientists in research councils separate.[6] Here, scientists dictated what directions their research should take rather than social or political pressures. This has received varied criticisms in later years, in so far as advancing technologies, greater social dependence on scientific innovation and the creation of new fields of research have eroded the old distinctions between applied, basic and pure science.[7] In 1971, two

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[4] For an extreme example of this see Barbara H.Peterson, "German E-Coli Outbreak: Accident or Planned "Genocide" for Organics?", *Farm wars*, June 2012, http://www.farmwars.info/?p=6236 [accessed 7 August 2012]

[5] David Edgerton, "The 'Haldane Principle' and Other Invented Traditions in Science Policy", *History and Policy,* July. 2009 <http://www.historyandpolicy.org/papers/policy-paper-88.html#S6> [accessed 12 May 2012]

[6] The Haldane Report, *Report of the Machinery of Government Committee under the Chairmanship of Viscount Haldane of Cloan* (London: HMSO, 1918). It is worth noting that the phrase 'the Haldane Principle' was never used by that report.

[7] David Edgerton argues that the term is a modern invention as a re-telling of the past in order to support political decisions about funding in the present. David Edgerton, "The 'Haldane Principle' and

reports were issued on the links between the nature of scientific activity and organization of funding. Both started from very different positions on the relationship between fundamental and applied research, which unsurprisingly, and came to two very different conclusions. *The Dainton Report* 'rejected a distinction between pure and applied research because of the interdependence of the two for progress'.[8] *The Rothschild Report* urged that the 'Government resist the view that there is no logical division between pure and applied research' suggesting that this view is 'intended to protect the Research Councils from the imaginary ravages of applied R. & D. users.'[9] Both reports were treated as consultative documents where 'almost immediately the government formally accepted the idea of the customer-contractor principle' as laid out in *The Rothschild Report*.[10] This new approach to organizing scientific activity provoked significant opposition from scientists within academia.[11] The main complaint, as highlighted by *The Dainton Report*, was that applied and fundamental research could not be separated as activities. There were many perceived weaknesses with *The Rothschild Report*, such as unforeseen areas of policy development outside of governmental settings; or how fragile scientific institutions actually were, whilst being valid questions, will not concern us.[12] Even though there is a historical and political dimension to how these divisions have been made, my concern is with the possible division between pure/basic/theoretical science on the one hand and applied/R&D/practical science on the other, and how this has been played out in PUoS schemes.[13] For it is also within this simple division there is an implied form of the PoD in how we tell one type of science from another.

Pure science we might characterize as intrigue or wonder, the simple desire to know or do something for its own sake, regardless of its ramifications or practical worth.

Other Invented Traditions in Science Policy", *History and Policy*, July. 2009 <http://www.historyandpolicy.org/papers/policy-paper-88.html#S6> [accessed 12 May 2012]

[8] Maurice Kogan, Mary Henkel & Steve Hanney, *Government and Research: Thirty Years of Evolution* (Dordrecht: Springer, 2006), p.14 [In footnote]

[9] The Rothschild Report, *A Framework for Government Research and Development* (London: HMSO, 1971), para.25

[10] Kogan, Henkel & Hanney, *Government and Research*, p.14

[11] Philip Gummett, *Scientists in Whitehall* (Manchester: Manchester University Press, 1980), pp.195-213

[12] Kogan, Henkel & Hanney, *Government and Research*, pp.15-16

[13] For example Margret Thatcher's training as a chemist and her subsequant affilication with Hayekian economics influenced her social policies on how science should be treated and the propagation of 'Thatcherism'. Jon Agar, "Thatcher, Scientist", *Notes and Records of the Royal Society*, 65 (2011), 215

Applied science is the application of an already pre-existing science. Here, knowledge is certain enough that it can be applied to research problems. Application is always for something and normally aimed at developing effectiveness, efficiency or productivity. This divide between pure and applied science in pre-PUoS Britain is an attempt at demarcation between types of science. These distinctions are not new and go all the way back to Ancient Greece in the differences between 'praxis' and 'theoria'. Theory was never an end in itself but always in the service of practice.[14] Whilst this was a meditative exercise for the Ancient Greeks, the dichotomy between theory and practice has never left us.

It was in 1980 that the 'Royal Society for the Arts' published their manifesto *Education for Capability*.[15] The 'manifesto stated that the education and training process gives too much emphasis to analysis, criticism and the acquisition of knowledge and not enough to problem-solving, doing, making and organising.'[16] The Royal Society did not become involved with the PUoS until 1982, where it grew out of a committee report on the state of science education in England and Wales for the 11-18's.[17] From this, the recommendation was taken up that a small working group be established to investigate the ways in which the public understanding of science might be enhanced. This resulted in the 1985 *Royal Society Report* titled 'The Public Understanding of Science'. This was the beginning in a surge of interest in the PUoS in Britain.[18] That project highlighted these concerns in its opening paragraph:

– 232 <http://rsnr.royalsocietypublishing.org/content/early/2011/05/13/rsnr.2010.0096.full.pdf+html> [accessed 29 September 2012]

[14] Pierre Hadot, *Philosophy as a Way of Life: Spiritual Exercise from Socrates to Foucault* (Oxford: Blackwell Publishing Ltd, 1995), p.60

[15] The Royal Society for the Encouragement of Arts, Manufacture and Commerce, *The Education for Capability Manifesto* (London: RSA, 1980)

[16] 'Education for Capability: Details of the Royal Society of Arts Recognition Scheme', *Industrial and Commercial Training*, 13 (1981), 14-21 (p.14) <http://www.emeraldinsight.com/journals.htm?articleid=1675053&show=abstract> [accessed 28 July 2011]

[17] Walter Bodmer, "Public Understanding of Science, The BA, The Royal Society and COPUS", *Notes and Records of the Royal Society*, 64 (2010), 151-161 (p.152) <http://rsnr.royalsocietypublishing.org/content/early/2010/06/16/rsnr.2010.0035.full> [accessed October 13 2011]

[18] Jane Gregory and Steve Miller, *Science in Public: Communication, Culture and Credibility* (Cambridge: Basic Books, 1998), p.9. Consequently the academic journal *Public Understanding of Science* was 'founded as a direct result outcome of the 1985 Royal Society report.' Bodmer, 'The BA, The Royal Society', p.158

> Science and technology play a major role in most aspects of our daily lives both at home and at work. Our industry and thus our national prosperity depend on them. Almost all public policy issues have scientific or technological implications. Everybody, therefore, needs *some* understanding of science, its accomplishments and its limitations. Many personal decisions, for example about diet, vaccination, personal hygiene or safety at home and at work, would be helped by *some* understanding of the underlying science. Understanding includes not just the facts of science, but also the method and its limitations as well as an appreciation of the practical and social implications.[19]

'Public' was given a five level definition, ranging from the everyday individual to those in charge of policy formation. 'Understanding' meant 'comprehension of the nature of scientific activity and enquiry, and not just knowledge of some facts.'[20] Finally, 'science' was given a broad definition to include technology, engineering, mathematics and all aspects of applied science. From this report, it was concluded that scientists need to learn how to communicate with the public more effectively. From this initial mobilization towards a PUoS, the 'Committee for the Public Understanding of Science' (COPUS) was established. Their role was to promote research on and engagement with science and the public. In 1988, a nation-wide survey was conducted to establish how scientifically 'literate' the British public were. The results suggested that the levels of scientific literacy between the UK and United States were similar. '10 percent or less of those questioned were scientifically literate, depending on the exact definition used'.[21] The importance of definitions in social surveys is paramount, which really should tell us more about the research method than the intelligence of people. On other criteria such as 'civic scientific literacy', Sturgis and Allum state that 'not more than one-quarter of the European and US public qualify as scientifically literate.'[22] The term 'scientific literacy' came out of the same era as

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[19] Walter F. Bodmer, *The Public Understanding of Science* (London: The Royal Society, 1985), p.6 [My Italics. Also referred to also as the Bodmer Report]
[20] Ibid.,
[21] Steve Miller, 'Public Understanding of Science at the Crossroads', *Public Understanding of Science*, 10 (2001), 115–120 (p.116)
For analysis of the 1988 survey see, John R. Durant, Geoffrey A. Evans, and Geoffrey P. Thomas, 'The Public Understanding of Science', *Nature*, 340 (1989), 11-14
[22] Patrick Sturgis and Nick Allum, 'Science in Society: Re-evaluating the Deficit Model of Public Attitudes', *Public Understanding of Science*, 13 (2004), 55-74 (p.56)

James B. Conant's educational reforms in America, the phrase being coined by Paul DeHart Hurd.[23] The metaphor of 'literacy' has been interpreted in many ways, John Durant defines scientific literacy as 'what the general public ought to know about science'.[24] Yet that 'ought' does not come from a scientific standpoint, but could be considered ethical, political or philosophical.[25] In 1993, another white paper entitled 'Realizing Our Potential' re-emphasized the role and importance of science, engineering and technology for a prosperous country.[26] The implication of 'literacy' was that the more one is scientifically literate the more in favour of 'science' one will be. The rationale seems reasonable enough. Bodmer stated that a better PUoS was not desirable because it would produce the 'right' decision but because 'decisions made in the light of adequate understanding of the issues are likely to be better than decisions made in the absence of such understanding.'[27] However, it is quickly passed over both what 'understanding' and 'science' are. It would seem that given 'adequate understanding', whatever this maybe, social progress would follow.[28] Yet as already mentioned, we can find well-respected scientists in positions of high expertise, who by the standards of their communities or from a strict methodological point of view, do not arrive at the 'right' decision. There are two points here: firstly, that social progress is not necessarily the same as scientific progress, and secondly, that 'literacy' as a guiding metaphor for 'understanding' appears flawed.[29]

In 1996 the 'British Social Attitudes Survey' was conducted to establish how well the PUoS scheme had performed in raising the British public's understanding of science. The 'survey indicated little change in scientific literacy, other than increased

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[23] Rüdiger C. Laugksch, 'Scientific Literacy: A Conceptual Overview', *Science Education*, 84 (2000), 71-94 (p.72)
[24] John Durant, 'What is Scientific Literacy?', in *Science and Culture in Europe*, ed. by John Durant and Jane Gregory (London: The Science Museum, 1993), pp.129-137 (p.129)
[25] The metaphor of 'literacy' as an adjunct to scientific understanding will be considered later on - pp. 94-98 & pp.193-194 in thesis
[26] *Realising our potential: A Strategy for Science, Engineering and Technology* (London: HMSO, 1993) This report still acknowledged the Rothschild customer-contractor model for managing science, Ibid., para. 1.18, no.9
[27] Bodmer, 'The BA, The Royal Society', p.154
[28] Alain Touraine, 'The Crisis of Progress', in *Resistance to New Technology: Nuclear Power, Information Technology, and Biotechnology* ed. by Martin Bauer, (Cambridge: Cambridge University Press, 1995), pp. 45–56
[29] Geoff Evans and John Durant, 'The Relationship Between Knowledge and Attitudes in the Public Understanding of Science in Britain', *Public Understanding of Science*, 4 (1995), 57-74 (p.57)
[29] Bodmer, 'The BA, The Royal Society', p.154

recognition of the initials DNA.'[30] This apparent continued scientific illiteracy, plus a sceptical attitudes towards scientific progress equating to social progress, motivated the *House of Lords Select Committee's Report* on 'Science and Technology'.[31] This report highlighted the general unease with which the public treated scientific issues, and not without due reasons; from the destructiveness of nuclear energy, to the pre-clinical trialed administration of thalidomide, to agricultural use of DDT. Some may argue that the knee-jerk reaction to scientific controversy is to err on the side of caution, which more recently saw parents willingly withdraw their children from taking the MMR vaccine. Yet, would it be illiterate to question the judgement of experts who have been wrong in the past?

What is implicit in the literacy metaphor is the ability or competency to do something. As science is portrayed as a methodology that distills knowledge into facts, to 'understand' science is to grasp both what that methodology is and the factual knowledge it generates. One also implies the other. To know something is to also to why we know it. This is part of what is called the 'deficit' model of communication. It refers to a deficit in information on any subject, and it is an assumption that this deficit shapes public reaction and attitudes. Another assumption is that in the absence of scientific understanding people intuit folk knowledge, unfounded beliefs and superstition about how things are. It is said that this ignorance leads to a state of scepticism, hostility or fear of the unknown, which is politically powerful enough to block certain research programs. In contrast to this model is the 'contextual' model, which has been favoured by the humanities due to its inclusion of surrounding socio-historical factors as a means to evaluating scientific understanding. Popular thinking about science, including philosophy, tends to fall into one of these camps. Knowing how one thinks about a problem is halfway to understanding why it is a problem, and so I will spend some time unpacking both these models and their relevance.

2.2 - The Deficit Model

The general structure of the deficit model is that it is a top-down process where the informed scientific community passes on information about science. Firstly, it is

[30] Miller, 'Science at the Crossroads', p.116

assumed that scientific knowledge is sufficient for scientific understanding, and secondly, that this will result in a positive attitude towards science. The implication being that if one is mistrustful of science then one has failed to understand it. Sturgis and Allum say the research indicates a 'robust but not especially strong positive correlation between "textbook" scientific knowledge and favourability of attitude towards science'.[32] This kind of top-down feeding of information and resultant increase in positive attitude towards science share some similarities with a model of mind and knowledge transfer that Karl Popper criticized. The 'Bucket Theory of Mind' was his attempt to illustrate why inductivist models of our experience of nature could not lead to scientific knowledge.[33] More recently, Gross has picked up on the asymmetry of this model with the public as passive knowledge receivers and the scientific community as active and inherently valuable. He also raises the point that the 'contextual' model is symmetrical in the interaction between science and the public. Here, a public has to be persuaded of the value of science and has to be reconstructed in terms of social, ethical, and political agendas, so scientific understanding is co-created, as it is the integration of personal experience and scientific knowledge.[34] Other research from social studies of science has shown that the underlying communication models that predicate the deficit model between experts and the public are often incompatible or are determined to a large extent by context.[35]

An assumption within the deficit model is that it presupposes that, given 'some understanding' of science, we will all arrive at the same conclusions. This quest for some ideal language where, all things being equal, we would all arrive at the same conclusions, or at least agree which was the best conclusion, is an idealized outcome of the methodological approach. As Bodmer wrote 'decisions made in the light of adequate understanding of the issues are likely to be better than decisions made in the

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[31] House of Lords, *Science and Technology* (London: HMSO, 2000)
[32] Sturgis and Allum, 'Science and Society', p.57
[33] Karl Popper, *Objective Knowledge: An Evolutionary Approach* (Oxford: Oxford University Press, 1972), p.456
[34] Alan G. Gross, 'The Roles of Rhetoric in the Public Understanding of Science', *Public Understanding of Science*, 3 (1994), 3–23 (p.6)
[35] Sharon Macdonald, 'Authorising Science: Public Understanding of Science in Museums', in *Misunderstanding Science? The Public Reconstruction of Science and Technology*, ed. by Alan Irwin and Brian Wynne (Cambridge: Cambridge University Press, 1996), pp.152-169, also see, Camille

absence of such understanding.'[36] This idea seems to bear a resemblance to Habermas's theory of communicative action, where human action, understanding and rationality are grounded in linguistic structure.[37] For Habermas, communicative rationality is communication that is 'oriented to achieving, sustaining and reviewing consensus – and indeed a consensus that rests on the intersubjective recognition of criticisable validity claims.'[38] It is essentially an ideal language where given that everyone will share the same goals, such as, believing true statements, we would all arrive at satisfactory consensual conclusions about the world. Thus, the deficit model and the purpose of public scientific literacy seem to be geared towards the same model of world, language and knowledge. What then do we say to those scientists who are well aware of bias, false inferences and the like, but do not come to the same consensual agreement as the rest of their community? Indeed, where those scientists have turned out to be correct, it would seem to challenge this idea that science has some privileged access to the structure of reality. The ability to understand science in this way, along with the consequences of its denial, are rooted in a particular way of understanding the world and science, which is where I see philosophy being of use. Here, we are being asked to suppress our everyday notions, which are already fraught with bias, to invert our worldview for whole new set of biases. For some, this plays out as 'scientism', where given the methods and assumptions of science we can be taught how to experience the world.[39] Feynman had a problem with this conception of science, in that what science teaches (methodological) is somehow separable from how we experience the world (historical). He said, if 'someone says science teaches such and such, he is using the word incorrectly. Science doesn't teach it; experience teaches it.'[40] It is this experience we are being asked to give over to an underlying metaphysical notion of science.

Limoges, 'Expert knowledge and Decision-Making in Controversy Contexts', *Public Understanding of Science*, 2 (1993), 417–426 (p.417)

[36] Bodmer, 'The BA, The Royal Society', p.154

[37] Jürgen Habermas, *The Theory of Communicative Action: Reason and the Rationalisation of Society*, Vol.1, trans. by Thomas A. McCarthy (Boston, Mass.: Beacon Press, 1988a), p.xiv

[38] Jürgen Habermas, *On the Logic of the Social Sciences*, trans. by Shierry Weber Nicholson and Jerry A. Stark (Cambridge, Mass.: The MIT Press, 1988b), p.17

[39] A recent example of this is from Steven Pinker who tries to defend scientism and how the humanities suffer from placing philosophical, historical or political criteria above science or 'philistine indifference to science'. Steven Pinker, "Science is not your Enemy: An Impassioned Plea to Neglected Novelists, Embattled Professors, and Tenure-less Historians", *New Republic*, Aug. 6 2013 <http://www.newrepublic.com/article/114127/science-not-enemy-humanities> [accessed 17 Aug 2013]

[40] Richard Feynman, 'What is Science?', in *The Pleasure of Finding Things Out: The Best Short Works of Richard P. Feynman*, ed. by Jeffery Robbins (London: Penguin Books, 2001), pp.171-188 (p.187)

The significant problems that the deficit model had faced, led to an alternative approach that claimed,

> [S]cientific facts and their public assimilation were not as unproblematic as the deficit model had assumed. Studies by Wynne and Irwin showed the importance of social context and lay knowledge as playing a significant part in how science was used by members of the public interpretation was not an unambiguous process.[41]

The search for an unambiguous language with which to describe nature or reality was a central tenet of the logical positivists, but this idea that we simply read off objective statements from nature, is challenged by the history and development of science itself. This will have some significance for us as I will hold this view in opposition to the 'methodological' perspective that has been discussed so far.

2.3 - The Contextualist Model

Under this model, knowledge of science is not a list of 'facts' but types of understanding within varying practical and social contexts. Wynne states that 'three elements of public understanding have to be expressly related: the formal contents of scientific knowledge; the methods and processes of science; and its forms of institutional embedding, patronage, organization, and control.'[42] We may bring in 'expertise' as part of institutional embedding, in that, the public's trust in experts is 'mediated by knowledge of the institutional arrangements under which expertise is authorized.'[43] This raises questions about what grounds one can legitimately claim expertise? Within PUoS studies, however, this rift between the deficit and contextualist models for assessing public understanding appears to be as binary as the difference between quantitative and qualitative research. Ironically, the very debate about whether scientific understanding can be measured by qualitative or quantitative

[41] Miller, 'Science at the Crossroads', p.117
[42] Brian Wynne, 'Public Understanding of Science Research: New Horizons or Hall of Mirrors?', *Public Understanding of Science*, 1 (1992), 37-43 (p.37)
[43] Sturgis and Allum, 'Science and Society', p.58

means appears to be a reflexive comment on what it is to be a science and the maturity of the science performing it.[44]

A context that shall not be taken up in this thesis, but does contribute to the public understanding and attitude towards science, is the role the media play in presenting 'scientific' stories and representing the image of science. It tends to be only 'news worthy' stories about science that make it into the popular press, either some great achievement, disastrous outcome or conflicting information that is socially important.[45] Very rarely does the hum-drum of scientific practice make it into the headlines, unless that practice is something that goes against the popular public image, such as publishing fraudulent research. With media reports on apparent controversies within science, conflicting interpretations of data and the heroic achievements of science, along with popular science writing, and mind, body and spirituality publications, all make for very conflicting messages about science. However, even within the contextual models, there seems to be a need for a reduction to a particular cause. Goldacre, on the issues of good and bad science, apportions some blame to 'humanities graduates for dumbing-down science in the popular media, [where] those journalists create a parody of science that they then write about'.[46] This appears to be echoing C. P. Snow's division in *The Two Cultures* between science and the humanities. Goldacre also points out a number of known cognitive biases that influence how we reason. However, looking through the history of science, we could equally accuse past scientists of believing silly things, but during the time they lived, they would have been justified in their beliefs. So this begs the question that we cannot appeal to studies of cognitive biases to know what is scientific or rational, as we use those very terms to arrive at our conclusions. That is, we work out what is rational using reason, and what is reasonable is set by historical context. Most of us are, however, not asked to work out what reason or science is. We are instead bombarded with images, textbooks, documentaries, and so on as examples of what science does or cannot do. From this mixture of messages, Erickson says a state of 'ambivalence can be identified in almost all public representations of science in

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[44] Ibid., p.59

[45] One piece of research concluded we 'do not find a significant direct relationship between television viewing and negative attitudes towards science'. Anthony Dudo et al, 'Science on Television in the 21st Century: Recent Trends in Portrayals and Their Contributions to Public Attitudes Toward Science', *Communication Research*, 38 (2011), 754-777 (p.754)

contemporary society.'[47] One image of science we are asked to adopt is of the lone genius, advancing knowledge, going from great discovery to great discovery in an endless upward trajectory of progress. Yet, at the same time, the public meets with tales and images of scientific failure, false predictions and disastrous applications of science to nature. This cycle of expectation and disappointment, it is argued, brings about a state of ambivalence, if not hostility towards science. What was realized in the contextualist model was that science and the public do not exist in separate vacuums, but are involved in a dialogue that feedback on one another. Scientific knowledge does not just pour from the laboratories into the lives of individuals, but 'science' is represented in many forms in many places.[48] The realization of the social aspect of science and this two-way dialogue became the main agenda of the House of Lords *Science and Technology Report*. In this report, unlike the Royal Society's 1985 version, the arguments about public ignorance and media distortion of science are 'almost totally absent' if not repudiated.[49] It was noted that the implied deficit model at the heart of the 1985 report was 'condescending' in tone and 'outmoded and potentially disastrous.'[50]

The contextualist model does not escape criticism though. Aside from being too reliant on qualitative methods, Gross also argues that 'rhetoric and rhetorical analysis play major roles'.[51] He says that the root metaphor of the deficit model is 'scientific sufficiency and public deficiency', whereas in the contextualist model it is 'interaction'.[52] Whilst in the deficit model, 'understanding' is a wholly cognitive act, in the contextualist model it is partly social, ethical and political. Here, we might start getting into murky waters if by the above we think that science is rhetoric, or rhetorical analysis does the same thing as scientific analysis. If science as a practice is open to rhetorical analysis and we understand science as *only* a text, it is only a short step to be in the field of literary criticism or cultural studies. My objection would be

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46 Ben Goldacre, *Bad Science* (London: Fourth Estate, 2009), pp. 224-225

47 Mark Erickson, *Science, Culture and Society: Understanding Science in the 21st Century* (Cambridge: Polity Press, 2005), p.220

48 The scientist and philosopher Ludwick Fleck argued for 'thought collectives' that were either internal to science as 'esoteric' or external to it as 'exoteric'. Ludwick Fleck, *Genesis and Development of Scientific Fact*, trans. by T. J. Tren & R. K. Merton (Chicago: University of Chicago Press, 1981), p.105

49 Miller, 'Science at the Crossroads', p.117

50 House of Lords, *Science and Technology*, p.140

51 Gross, 'Roles of Rhetoric', p.8

52 Ibid., p.6

that the paradoxes one might arrive at by poor philosophical analysis could be construed as desirable. We see this with applications of modern science to religious discourse 'as if' they were about the same thing. This leads someone like Tipler to treat Christianity as a 'possibly true theory of physical reality'.[53] Moreover, we may only be able to treat 'truth' 'through a language of paradox' or 'philosophy' as 'a pre-text for critics to indulge any kind of whimsical, free-wheeling or 'creative' commentary that happens to take their fancy.'[54] Paradoxically, I argue, philosophical ideas are present where they appear to be most absent. By this, I mean when we have given over to a single interpretation, where no other version or way of thinking seems meaningful. Here is where I see philosophy as desirable as a part of the PUoS that forces us to look again at commonsense notions. Given this, there is a sense that we are not being asked to think about science but a version of science, a particular methodological interpretation. To instruct or teach what science is is also to give some indication of what science is not by omission, inference or implication. Even those who will never touch a philosophy book cannot escape this logic. As with the scientific method, we can only really offer a positive account, for saying what something is not is a much longer list than saying what something is. Also, by calling something 'science' we pick up on the negative space that there must be something that is not-science or non-science.

This general tension at the heart of PUoS is called the 'problem of demarcation' (PoD). This will be our route into philosophy as a useful component of science education. Not as a means of devising some universal criteria for telling science from non-science, but by deconstructing the very problem of defining science itself. We may intuitively feel that there must be something called science as people study, teach it, and above all, it makes the modern world possible. Yet, the constant failure to resolve the PoD has highlighted a deficiency, in either our problem or our method. I will argue that because the PoD is classically framed in a methodological manner, it ends up deconstructing itself with only one outlet, which many scientists have picked up on, that philosophy is useless, especially to science. Yet, even those who have attempted the PoD and failed have by virtue of how they approached the problem

[53] Frank J. Tipler, *The Physics of Christianity* (London/New York: Doubleday, 2007), p.267
[54] Christopher Norris, *The Truth about Postmodernism* (Oxford: Blackwell Publishers,1993), p.206; *Deconstruction: Theory and Practice*, Revised edition (London & New York: 1991), p.138

have remained in high regard by the scientific community. Whilst those who attempted to illustrate the naivety of this problem have been marginalized by those 'anti-philosophy' or 'philosophy as postmodern' scientists.[55] I think this is the case because the methodological approach, whilst incomplete or insufficient, due to it mirroring in form the subject it looks to preserve it is thus deemed worthy. On the other hand, the historical approach does not mirror its subject in form (however possibly more rigorous), but still looks to preserve science by showing how a methodological interpretation is inconsistent with the actual practices of scientists. These attempts, however, have been construed as 'attacks' on the authority of science and so must be avoided, if not derided, as much as possible.

2.4 - Problems with the Problem of Demarcation

Put bluntly, the PoD is 'how do we tell science from non-science?' In part, it is what the PUoS sought to address, as by knowing what science is, it might prevent people from using 'alternative' modes of reasoning in their everyday lives. It is no secret that all attempts at answering this question by formulating the scientific method as a set of abstract principles have failed. We can conclude either this project failed because the problem is too complex or the methodology was incorrect. The alternative is too unthinkable, that the truths of science are contingent upon social factors or ultimately the logic and structure of science is unknowable. I will argue that the project failed and the unthinkable alternatives become present because we are treating a historical question in a methodological way. We always see reality 'as' something, which is historical and never just pure reality, which is the methodological abstraction. Just as evidence is always for something and never just 'evidence'. It is uncovering what allows us to see something 'as' something that is the hallmark of scientific innovation and revolution, which is to say ontological. 'Ontological' here means the study of what there is or can be said to exist, but just how one comes to these questions is open to philosophical analysis of the methodological/ historical kind. So, when we see the Sun are we looking at the same Sun as the ancients? Do we see the Sun as a fiery sky God or as a burning star? It is the covering-over of this 'as' that is the hallmark of efficient modern science. For example, Aristotle saw a falling object 'as' an expression of its nature, Newton saw it 'as' an

[55] This will be evidenced in chapters 3 and 4 where Karl Popper is treated as the 'scientists' philosopher' and Paul Feyerabend as the 'radical postmodernist'. Pp.44-93 in thesis

expression of mechanical forces, and Einstein saw it 'as' an expression of curved space-time. Once we have settled on a confident explanation, the 'as' disappears and we just see a falling object. So, for me the question 'what is science' is about ontology. Yet, if we treat it as a methodological question, we look to epistemology or linguistics to derive its 'what-ness' as an object. In doing so, we hit an impasse as the discipline fails to account for itself, in that, what does it mean for something to be scientific is itself a non-scientific question. Fuller and Collier say that 'the demarcation project's failure only shows that attempts to study science scientifically, as the philosophers have wanted to do, often result in science deconstructing its identity.'[56] It is when scientists are forced to question the meaning of the objects they are studying, measuring, and quantifying that science begins to move forward. Indeed, are those same objects real? This questioning of the reality of objects or methods by which science studies the world is part of a 'crisis' science undergoes, where it will depart from what it traditionally held to be 'scientific'.

Why is this important? I am arguing that the PUoS, which also includes education curriculums, presents science through the methodological interpretation, and in doing so, I think that people are being asked to understand a metaphysical conception of science. The other interpretation that I will offer is the 'historical' notion of science. The problem for me is that, whilst people are being asked to understand a metaphysical version of science so we can meaningfully say things like know the 'mind of God' or treat Christianity as a theory of physical reality, it is this interpretation that comes to dominate the discussion.[57] The problem is not that religion should be exempt from scientific analysis but that it is all too easy to arrive at the dichotomous outcome of science as *only* objective and religion/ belief as *only* subjective, if we can *only* understand either as methodological statements about reality. What is more, because the public, unlike scientists, are not involved with science on an everyday level, when they do run into a problem with this representation of science, there may be less intellectually honest people waiting to offer an answer. For instance, it is very easy to arrive at meta-inductive pessimism or scepticism about the scientific process if we take it as only a method. So, for example, one might think that because what we have believed in the past was false so too the likelihood is the things we believe now are false. If I was even more dishonest I

[56] Steve Fuller and James H. Collier, *Philosophy, Rhetoric, and the End of Knowledge: A New Beginning for Science and Technology Studies* (Mahwah, NJ.: Lawrence Erlbaum Associates, 2004), p.88

could present research papers on why you should not believe research papers.[58] For some, this is enough to understand that science does not have all the answers and that 'alternative' ways-of-knowing might just have access to truths regular science does not. Here I am referring to what is known as a pseudo-science, be it crystal healing, homeopathy, anti-vaccination programs, Creationist science and so on. What is wrong with this is that these 'alternative' ways-of-knowing are basing their claims on a model of reality that also includes 'Truth' as a part of its methodological content. As they are seeking to replicate science, they set up a practice that is metaphysically parasitic on the methodological conception of science, which also includes its notion of 'Truth'.[59] This is one form of demarcation. However, as it requires some heavy philosophical lifting we would rather accept notions of 'Truth' and 'Reason' as defined by the popular image of science. What this does is leave a gap for 'nonsense' or 'uncritical thinking', which is in danger of not being addressed. For not only can the methodological conception of science not answer it (failure of demarcation criteria), but the very mode of analysis that will show it up as being nonsense (philosophy) is itself be derided as nonsense.[60] This is the lumping together of all alternative discourses that deal in paradoxes or that complicate commonsense notions as unnecessary.

Next, I will briefly consider the 'other' that science has been and often is defined against. Within the literature this has been called, 'fringe science', 'non-science', 'cargo-cult science', 'proto-science', 'junk science', 'pathological science', 'voodoo science', and 'pseudoscience'.[61] All have slightly different uses and meanings, but all are seen in

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[57] Stephen W. Hawking, *A Brief History of Time* (London: Bantam Books, 1998), p.191
[58] John P. A. Ioannidis, "Why Most Published Research Findings Are False", *PLoS Medicine*, 2 (2005), <e124 http://www.ncbi.nlm.nih.gov/pmc/articles/PMC1182327/> [accessed 13 May 2012]
[59] Helga Nowotny, 'Science and Its Critics: Reflections on Anti-Science', in *Counter Movements in the Sciences*, ed. by Helga Nowotny and Hilary Rose (Dordrecht: Reidel, 1979), pp.1-26 (p.15)
[60] Norris suggests something similar in his observation that the views of analytical philosophers and purist deconstructors on Derrida appear to converge. Norris, *Deconstruction: Theory and Practice*, p.147
[61] Steven I. Dutch, 'Notes on the Nature of Fringe Science', *Journal of Geological Education*, 30 (1982), 6-13;
Paul Feyerabend, *Against Method*, 3rd edn. (London: Verso, 2002), p.34;
Richard P. Feynman, *Surely You're Joking Mr Feynman: Adventures of a Curious Character* (London: Vintage Books, 1992), p.346;
Dirk Hartmann, 'Protoscience and Reconstruction,' *Journal of General Philosophy of Science*, 27 (1996), 55-69;
Peter W. Huber, *Galileo's Revenge: Junk Science in the Courtroom* (New York: Basic Books, 2001); Irving Langmuir, 'Pathological Science', *Physics Today*, 42 (1989) 36–48;
Robert L. Park, *Voodoo Science: The Road from Foolishness to Fraud* (Oxford: Oxford University Press, 2002);

opposition to science proper.[62] I will be using the terms ‘pseudo-science’ and ‘non-science’ as representatives of the ‘other’. The question is how can something like non-science persist in the modern world? We have already seen the purpose of the PUoS which is to limit the effect pseudo-science has on the general populace, but we have also seen that it takes scientific understanding to be a matter of ‘literacy’. So, how come we find scientists who are experts in their fields but hold ‘scientific’ views contrary to that of the scientific community? For example, Arthur Ernest Wilder-Smith was a Professor of Pharmacology and a Fellow of the Royal Society of Chemistry. He gained three doctorates in organic chemistry and pharmacology and published more than seventy scientific papers. Yet, he believed that dinosaurs and humans co-existed due to a ‘literal’ understanding of Genesis. In reference to the alleged human footprints found alongside dinosaur footprints in Glen Rose Texas, he said, ‘if these footprints are really human – and there exists no experimental reasons to question this assumption […] then we have struck upon yet another considerable difficulty with dating methods’.[63] Can it simply be that in that one instance Wilder-Smith, for all his doctorates, training and tenured professorship, ceased being scientific? Surely there are lots of things that we have no experimental reasons to question, but does that make them likely?

What makes me more qualified than Wilder-Smith to say that he is wrong or at least engaged in poor scientific reasoning?[64] An original distinction I will develop in this thesis is that philosophy along with all other commentating subjects are involved in a language ‘about’ science. But when scientists actually do science, they are no longer limited to this ‘about’ language but are involved in creating the language ‘of’ science. It is when they stop and reflect on what they are doing, which has to relate to other disparate subjects, that they then take up this third-person perspective to talk *about* science. I will develop this distinction more fully later on.[65] This distinction, however, is a tricky one as it breaks with the methodological conception of science in that, the activity is non-propositional, pre-linguistic and pre-epistemological. It is prior to

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Karl R. Popper, *Conjectures and Refutations: The Growth of Scientific Knowledge* (London: Routledge, 2004), p.44

[62] For example Kitaeff writes about the legal contexts of ‘junk science’. Jack Kitaeff, *Malingering, Lies, and Junk in the Courtroom* (New York: Cambria Press, 2007)

[63] A. E. Wilder-Smith, *The Natural Sciences Know Nothing of Evolution* (San Diego, CA: Masters Books, 1981), p.119

[64] On a simliar point about how background understanding guides what we take to be legitmate claims was Saussure’s dealing with Genevan spiritualist Mlle Smith who claimed to channel sanskrit as well as martian. Christopher Norris, *Against Relativism: Philosophy of Science, Deconstruction and Critical Theory* (Oxford: Blackwell Publishers, 1997a), pp.8-9

knowledge. Through a methodological conception such statements appear non-sensical, and so we feel obliged to dismiss them and the practices that give rise to them, which we are free to do, but the problem of telling science from non-science or expert from crank still remains. This problem goes back to at least Ancient Greece, as illustrated in the Platonic dialogue *Charmides*. In this dialogue, Socrates is discussing the virtue of temperance and its associated virtue sophrosyne. Socrates makes the point that in order to judge a real physician from a quack, one has to be qualified to tell the difference. If wisdom is the difference between knowing what you do know and knowing what you do not know, as mediated by temperance and sophrosyne, then no one would ever make a mistake. But, as people do make mistakes, Socrates concluded that the science (of determining who knows what) is impossible.[66] In keeping with our intuitive binaries, Socrates has knowledge and non-knowledge, knowing what we know and what we do not know. [67] This is similar to the form of science and non-science, which like non-knowledge, is potentially an infinite category. By 'infinite', I mean what remains unknown or yet to be scientific is an indeterminate amount.

'Infinity' like 'knowledge' is one of those nebulous, philosophical words. The ultimate form of both we see rooted in mathematics. To know something as concrete and certain as the outcome of 2 + 2, or the non-outcome of the series $N + 1$, is what we aspire to when we use those words. Yet, we forget that those practices came from somewhere, with a beginning, that meant something different than it does today. Mathematics is the methodological interpretation writ large and because of these metaphysical overlaps with knowledge, truth and even beauty, it finds deep parallels in fields such as theology. Polkinghorne uses the conclusions of Gödel's incompleteness theorem to state that, 'truth always exceeds what can be proved' ('Truth', here being used unproblematically).[68]

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[65] Pp.138-145 in thesis
[66] Thomas M. Tuozzo, *Plato's Charmides: Positive Elenchus in a "Socratic" Dialogue* (New York: Cambridge University Press, 2011), pp.251-252
[67] Ibid., p.251
[68] John Polkinghorne, *Science and Creation: The Search for Understanding* (London: SPCK, 1988), p.82. It is Polkinghorne's blending of scientific, mathematical and religious discourse by playing on the uncertainty that Gödel's theorem implies for knowledge and the excesses of 'truth' beyond what we can know is used as a justification for the divine. Yet as Derrida tried to show it is only by through a logically rigourous reading using formal methods of bivalant logic that we can bring into question those very same ideas. Christopher Norris, 'Derrida's *Positions* Thirty Years On: Introduction to the Second English language Edition' in *Positions*, trans. by Alan Bass, 2nd edn. (London: Continuum, 2002), pp. vi – xlii (pp.xxxi -xxxiv).

It is this prioritizing of one interpretation into the domain of another (methodological over historical) I take to be the sustaining power of many of the problems in philosophy of science. Possibly, the greatest enemy of Enlightenment ideals is the relativism of truth. In this thesis, this will come from the conflation of a historical interpretation as a substitute for a methodological approach. For my purposes, I do not wish to say that history can play the part of methodology. It is in keeping this historical-methodological division in mind that we will be able to open up a text in interpretation, by which we may get a more productive notion of science. This is only done, however, by rigorously holding to the text itself in logical consistency, conceptual rigour, modes of truth conditions, and so on.[69] Fuller and Collier have described this distinction between methodology and the historical approach as the 'divergence between the words and deeds of scientists.'[70] 'Words' being the realm of discourse based philosophy and 'deeds' as the historical acts of people. For Fuller and Collier, the inability to articulate this distinction in research methods 'absent an explicit normative stance [that] have resulted in the much ballyhooed "relativism" of STS research.'[71] 'Truth' may be too grand a place to start, but my worry is that if we dismiss philosophy as an antiquated practice, it does not cease to be but simply disappears. By this, I mean, the philosophical component (whatever that might be) of thought and language is simply absorbed by other discourses. Normally, we see this in political or religious discourses. To have philosophy out in the open not only makes our thinking more rigorous, but it would ultimately defend science against alternative discourses that exploit the methodological gap between what the textbooks say science is and what scientists actually do. Yet, if a certain proportion of the analytical schools get their way, the methodological interpretation will dominate PUoS where philosophy is regarded as a hindrance:

> It [philosophy] may be a handicap when one turns to scientific texts […] One can learn physics without ever reading Galileo, Newton or Einstein, and study biology without reading a line of Darwin. What matters are the factual and theoretical arguments these authors offer, not the words they

[69] It is the idea that the text somehow has its own internal 'meaning/logic' that is incommenusrable with any attenpt of objective analysis. Derrida was trying to argue against this with 'supplementarity'. Christopher Norris, *Deconstruction: Theory and Practice* (London and New York: Routledge, 1999), pp.151-152

[70] Fuller and Collier, *Philosophy, Rhetoric, and the End of Knowledge*, p.7

> used. Besides, their ideas may have been radically modified or even overturned by subsequent developments in their disciplines. Furthermore, scientists' personal qualities and extra-scientific beliefs are irrelevant for the evaluation of their theories.[72]

However, for the above statement to make sense, we already have to be schooled in a way of thinking that allows the truth of facts to be separate from their origins. Philosophy, as I understand it however, is not trying to do the job of science but open up ways of thinking *about* it. Next, I will be outlining two attempts at the PoD, but as philosophically presented from the inverse of my view. In that, I will treat Karl Popper's attempt and ultimate failure to give a methodological account of science as heroic. I will argue that as Karl Popper's own metaphysical view of science resonated with most scientists and analytical philosophers that he was seen as justified in his attempt. I will contrast this with one of Popper's protégés, the 'radical' Paul Feyerabend, and his anarchistic view, which argues for the lack of any unified scientific method. As he drew mainly from historical accounts and made comparisons to the scientific world from religion and myth, his project can be perceived to be at odds with the Enlightenment ideals it appears to challenge.

Chapter Three

Karl Popper and Falsification

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[71] Ibid.,

[72] Alan Sokal and Jean Bricmont, *Intellectual Impostures Postmodern Intellectuals' Abuse of Science* (London: Profile Books, 1998), p.196

3.1 - Karl Popper

Hopefully it should be clear that, even before we get to discuss better ways of communicating science to the public, we have to know what it is we are discussing. The 'received view' that science is a method grounded in evidence and observation is uncontroversial. The inference being there are practices not based in evidence or observation, which would make them non-science. The standard account of the difference between these two positions is whether the claim can be 'tested' or 'falsified'. Karl Popper was one of the originators of this position, but I will argue that because his attempt was 1) intellectually appealing, 2) methodology driven and, 3) Popper himself was a contributor to science (theoretical physics) that his attempt is still looked upon favourably by science educators and scientists.
Before I give a standard account of Karl Popper's 'falsificationism', I would first like to give a brief background to the history and context of how the criterion of falsification came about. I will then discuss the possible reasons why Popper is regarded as the 'scientist's philosopher', in that he is aligned more with scientists than he is with philosophers.

Karl Popper's attempt at a methodological principle, by which one could demarcate science from non-science, was preceded as a project by logical positivism and the 'Vienna Circle'. Their main aim was to formulate a criterion of meaning based on verifiability by which an unambiguous language could be constructed. The hope was that, after weeding all the metaphysical statements out of language, what remained could be reduced to protocol statements, which then could be subject to logical analysis. The logical positivists were an amalgamation of two 'circles' of thinkers, made up of philosophers, mathematicians, logicians and scientists. The 'Vienna Circle', and the later 'Berlin Circle', were groups of thinkers who shared a common ideology about the purpose and role of philosophy and science.[1] These shared ideals

[1] Members of the 'Vienna Circle' included, Rudolf Carnap, Hans Hahn, Otto Neurath, Phillip Frank, and Moritz Schlick. Freidrich Stadler, 'The Vienna Circle: Context, Profile and Development', in *The Cambridge Companion to Logical Empiricism*, ed. by Alan Richardson and Thomas Uebel (Cambridge: Cambridge University Press, 2007), pp.13-40 (p.15). The 'Berlin Circle' included Hans Reichenbach, Carl Gustav Hemple, David Hilbert, and Richard von Mises. Nikolay Milkov, 'The Berlin Group and the Vienna Circle: Affinities and Divergences', in *The Berlin Group and the*

came about from events such as, the rise of National Socialism or the lack of progress from Hegelian or Kantian philosophy when compared to the 'new physics'. Their commitment to the elimination of metaphysics and the propagation of empiricism was a challenge to the German romantic idealism and Christian worldview that dominated much of Europe at that time.[2] There was an aspiration amongst the logical positivists to a logical-empirical description of reality that came from science but was seemingly marred by the traditions of philosophy. After the disbandment of the Vienna and Berlin 'Circles' a third 'Circle' was established, called the 'Kraft Circle', amongst whose founding members was Paul Feyerabend.[3]

The elimination of metaphysics from science was the first of the Circle's aims as set out in *The Scientific Conception of the World View*; this was to be achieved by the method of protocol statements. Protocol statements are 'statements needing no further justification and serving as foundation for all the remaining statements of science' or which 'refer to the given, and describe directly given experience and phenomena, i.e. the simplest states of which knowledge can be had.'[4] From these protocol statements it was thought that the whole of science could be unified in its methods and descriptions of reality, and there would be a foundation of certain knowledge from which all other scientific knowledge could be assembled. The unity of science project was led by Otto Neurath and its vision for science was that the 'meaning of every scientific statement and every scientific concept, of whatever branch of science, must be analysable step by step to concepts of a common basic type, 'referring to the given itself''.[5] Neurath proposed a 26-volume encyclopaedia, later to be called *Foundations of the Unity of Science*, which would deal with various aspects of the philosophy of science, ideally leading to a consensus and unity on science and the scientific

Philosophy of Logical Empiricism ed. by Nikolay Milkov and Volker Peckhaus (Dordrecht: Springer, 2013), pp.3-32 (pp.8-9)

[2] Thomas L. Akehurst, *The Culture Politics of Analytical Philosophy: Britishness and the Spectre of Europe* (London: Continuum, 2010), p.131

[3] Feyerabend was granted a scholarship to study under Wittgenstein but he died before Feyerabend could meet him. He then took supervision under Karl Popper at the London School of Economics. Paul Feyerabend, *Science in a Free Society* (London: New Left Books, 1978), pp.114-15

[4] Rudolf Carnap, 'Protocol Statements and the Formal Mode of Speech', in *Essential Readings in Logical Positivism*, ed. by Oswald Hanfling (Oxford: Basil Blackwell, 1981), pp.150-160 (p.153)

[5] Ibid., p. 104

method.[6] This idea was discussed at the 'First International Congress for the Unity of Science' held in Paris in 1935, in which the congress voted on its approval. One of those who attended the Paris congress, but opposed Neurath's idea for such a project, was Karl Popper. Described by Neurath as 'the Official Opposition' of the Vienna Circle, Popper was a member but had radically different ideas as to how science operated and proceeded.[7]

3.2 - Popper and the Logical Positivists

A central problem that the logical positivists faced was how facts related to theories. It was Hume who first showed the problems of basing knowledge on inductive reasoning. Universal laws of science require the future to be the same as the past, but we cannot arrive at this conclusion by experience alone. Just because we have always experienced something to be the case does not mean the same has to be true of future events. An assumption that was being worked with is that we start with pure experience and then extrapolate to theories about reality. If knowledge, however, cannot be grounded in experience, what is the basis of scientific knowledge? Russell stated the severity of this problem when he wrote, 'induction is an independent logical principle, incapable of being inferred either from experience or from other logical principles, and that without this principle science is impossible.'[8] Hume's problem challenged the doctrine of empiricism, where we cannot simply observe 'facts' from nature and invent theories that explain them. This objection tested the empirical basis of science. Modern science still retains some notions of inductivism but these have moved into statistical and probabilistic analysis.[9]

The idea that a statement which could not be verified (or verified in principle) was meaningless, or that all metaphysics should be exorcised from scientific language, was not what concerned Popper. For the logical positivists a statement such as, 'God

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[6] Otto Neurath, Rudolf Carnap, and Charles Morris, eds. *Foundations of the Unity of Science: Toward an International Encyclopaedia of Unified Science*, 4th edn. (Chicago: University of Chicago Press, 1971)
[7] Bryan Magee, *Popper* (London: Fontana Press, 1985), p.11
[8] Bertrand Russell, *History of Western Philosophy* (London: Routledge Classics, 2004), p.700
[9] Roberto Festa, 'Bayesian Inductive Logic, Verisimilitude and Statistics', in *Philosophy of Statistics*, ed. by Prasanta S. Bandyopadhyay and Malcolm R. Forster (Oxford: Elsevier, 2011), pp.473-492 (p.474)

exists', is a meaningless metaphysical claim. Popper, however, said this was a meaningful statement (which could potentially be true), but because there is no possible way to prove or disprove such a claim, it was not a *scientific statement*. Moreover, Popper believed metaphysical statements to be necessary to the scientific programme,

> In using this term [metaphysical research programme] I wish to draw attention to the fact that in almost every phase of the development of science we are under the sway of metaphysical - that is, untestable - ideas; ideas which not only determine what problems of explanation we shall choose to attack, but also what kinds of answers we shall consider as fitting or satisfactory or acceptable, and as improvements of, or advances on, earlier answers.[10]

Another major difference between Popper and the logical positivists was that he never offered a criterion of meaning, such as the 'verification principle'. Popper argued that it was a major philosophical error to start with terms and their definitions.[11] The notion that we must define all our terms before we can have a coherent discussion is itself demonstrably incoherent, since every time one defines a term one has to introduce a new term and so on. For the physicist Richard Feynman, the learning of scientific terms and their uses was 'not science'.[12] For him it only told you about what people called things, the limits of human imagination and nothing about nature. This is where the idea of a protocol-language floundered, as its proponants became tied up in establishing the meaning of a statement, who said it, or what mode of language the statement is expressed in. For Popper, discussion had to make use of weakly defined vague terms. For we have an everyday notion of something like 'energy', but it also has a technical meaning which is more precise, even though the term 'energy' itself has no one single definition. Feynman explained that, for a physicist when they make

[10] Karl Popper, *Realism and the Aim of Science: From The Postscript to The Logic of Scientific Discovery*, ed. by W. W. Bartley III (London: Routledge, 1985), p.161
[11] Magee, *Popper*, p.49
[12] Richard Feynman, 'What is Science?', in *The Pleasure of Finding Things Out: The Best Short Works of Richard P. Feynman*, ed. by Jeffery Robbins (London: Penguin Books, 2001), pp.171-188 (p.177)

a calculation about ‘energy’ they are not describing a mechanism or something concrete, but are in fact invoking a mathematical principle.[13]

Popper, unlike the logical positivists, who sought ostensive definitions, wanted to substitute the importance of meaning in science with that of description. If a student were to ask, ‘what is a gene?’ a biologist might say a gene is a ‘locatable region of genomic sequence, corresponding to a unit of inheritance.’[14] But Popper would say that the real answer to the question, ‘what is a gene?’ is the name scientists give to a locatable region of genomic sequence that corresponds to a unit of inheritance. The term ‘gene’ is shorthand for a long description given meaning by the surrounding theoretical context. Popper is not saying one should never define the terms under use, for there are instances in which more detailed definitions are required. This would be true of the less precise sciences, where words tend to inculcate their meaning, such as ‘dysfunctional’ or ‘excessive’, as used in neurology or psychiatry. What Popper did, by challenging induction as a logical basis for method, is to show that ‘facts’ or ‘observations’ cannot precede the theories that make those things facts or an observation relevant. We always come to observation with some theory, which means ‘that observations, and even more so observation statements and statements of experimental results, are always *interpretations* of the facts observed; that they are *interpretations in the light of theories*.’[15]

In the place of the ‘verification principle’, Popper offered an alternative methodological basis, that of the criterion of falsification. Popper acknowledged that falsification comes in several types. At the level of logic, Popper was a weak or naive falsificationist, in that he did not think we should give up on a theory if we find a falsifying instance. But, at the level of methodology, he was a strong falsificationsist, in that, if a theory made highly novel, bold, complex and precise predictions, which turned out to be true, then we should accept that theory over one that does not. How Popper overcame the problem of induction was to place theories first prior to facts and argue that, we deduce from theories what is to be expected and then use those

[13] Richard Feynman, *Six Not-So-Easy Pieces: Einstein’s Relativity, Symmetry and Space-Time* (London: Penguin Books, 1999), p.29
[14] Helen Pearson, ‘Genetics: What is a Gene?’, *Nature*, 441 (2006), 398-401
[15] Karl Popper, *The Logic of Scientific Discovery* (London: Routledge Classic, 2002a), p.90 [In footnote – Italics in original]

predictions as objective tests of the theory. The scientific method for Popper was *deductive* not inductive.

3.3 - Logical and Methodological Falsification

Falsification is the idea that a theory can be shown to be false by an appeal to the results and observations of an experiment. There is a simple logical relationship that seems to give falsification commonsense appeal. Even if we assume we have access to 'true' statements about reality, how can we arrive at a universal law from a limited set of observations? How do we get 'all swans are white' from 'every swan I have seen is white'? Luckily, by using singular observation statements we can logically deduce the falsity of a universal law or principle. So all we have to do is observe one non-white swan, to show that the universal claim 'all swans are white' is false. No amount of positive, supporting observations can prove the universality of a general statement, but all it takes is one counter observation to conclusively disprove it. The logic is simple, a theory or hypothesis counts as genuinely scientific 'if there exists a logically possible observation statement or set of observation statements that are inconsistent with it, that is, which, if established as true, it would falsify the hypothesis.'[16] Methodology, is however, prone to error so we may not take falsifying instances as being totally conclusive. For instance, what I thought was a black swan might have been a Canadian goose. As a conclusion, falsification then, is not possible at the methodological level and we should not seek it, for if we did, and then kept modifying our theory to fit the observation, Popper believes we would have stopped being scientific. Rather, we should seek refuting instances and anomalies of observation, but not any falsifiable hypothesis will do. Popper argued, that the means of falsification must be by the inability to make novel, bold and detailed predictions. A general falsifiable statement is not as good as a precise one. The logical positivists struggled with the term 'verification' because what it meant to be a verifying statement in the absence of a theory was hard to say. Also, when a strict logical rule was applied, its logical consequences seem to run counter to our intuitions about what should count as a verifying instance. A demonstration of this was Hempel's 'raven

[16] Alan F. Chalmers, *What is this thing called Science?* 3rd edn. (Maidenhead: Open University Press, 2010), p.62

paradox'.[17] Another problem with the term 'verify' was that to verify something was to remove doubt about its truth value, but in what sense does it mean to talk about verifying something that cannot be doubted?[18] On this score Popper is persuaded that the term to 'falsify' is less problematic that the term to 'verify'.

For Popper scientific knowledge is a set of provisional hypotheses that are offered in an attempt to describe or account for the behaviour of some aspect of reality. It was 'the permanent right to challenge this authority [dominant theory] being one of the things which […] marks off an intellectual procedure as being 'scientific' at all'.[19] If there is no logically possible observation one could make in order to disprove a claim then for Popper it is not science, but may still be meaningful. Conversely, if a statement is true by definition, then this is no longer a theory as it cannot be falsified. Popper demanded that scientific propositions be falsifiable because without the logical possibility of ruling certain options out, one can never learn anything new about the world.

Unfalsifiable statements do not conflict with any states of affairs, so we cannot determine whether they are true or not – where as a statement that can be tested and shown to be false means there is one less statement we have to consider about the world. The claim that 'all mammals have hair' is not only falsifiable, but also potentially gives us more information about mammals. Where as 'all triangles have three sides' is true because of the term 'triangle', and so tells us nothing new. Popper argued that there were theories that superficially appeared to have all the features of scientific hypotheses but were in fact impostors, for they are unfalsifiable and so should be rejected as explanations. Popper singled out the Marxist theory of

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[17] Carl Hempel tried to give a general account of science by saying that scientific laws were derived by our experience of regularities and causation. This took the logical form of 'all As are Bs' and a statement of the form 'Ax' then we can conclude 'Bx'. However, this is also the same as saying, 'all non-Bs are not A'. If we start from a known universal such as 'all snow is white' and qualify this with the statement that, 'anything that is non-white is not snow'. Then every thing we see that is non-white such as red apples, brown chairs and so on is a confirming instance that 'all snow is white'. This is not a problem of logic but a problem posed by logic. As a methodology it takes an instance of snow being white as being equivalent in confirmation as a chair being brown, which we intuitively feel is not the same. Carl G. Hempel, 'Studies in the Logic of Confirmation (I.)', *Mind*, 54 (1945), 1-26

[18] Moritz Schlick raises this point about existential propositions saying that asking about the 'reality of an experience has meaning only where this reality can be meaningfully *doubted*'. Moritz Schlick, 'Positivism and Realism', in *The Philosophy of Science*, ed. by Richard Boyd, Phillip Gaspers, and J. D. Trout (Cambridge, Mass: MIT Press, 1991), pp.37-54 (p.49) [Italics in original]

[19] Stephen Toulmin, 'Does the Distinction Between Normal Science and Revolutionary Science Hold Water?', in *Criticisms and the Growth of Knowledge*, ed. by Imre Lakatos & Alan Musgrave (Cambridge: Cambridge University Press, 1999), pp.39-48 (p.40)

historicism as an example of pseudo-science, but would later also target psychoanalysis.[20] He attacked what he saw as pseudoscientific theories, especially in the light of the rise of National Socialism, which for Popper used Platonic and Hegelian ideas as justification for human rights atrocities.[21] The idea that one could predict the future of society by looking at 'patterns' from history, and that what happens could not have happened any other way, was unconvincing to Popper. Popper, motivated by the rise of fascism, places a weight of blame at the feet of Plato and then Hegel for the neglect of reason. Here, he is in complete sympathy with the logical positivists and as a philosopher starts to add to the growing scepticism and cynicism over philosophy as a form of natural scientific enquiry. He accuses Hegel of bringing about a 'renaissance of tribalism' and calls his teachings a 'bombastic and hysterical Platonism' that has led to the totalitarian dictatorships of the twentieth century.[22] It was not just Marxist and Hegelian ideas that came under Popperian fire, he also objected to the theories of psychoanalysis by Freud and Adler. For example, there was no observation of human behaviour that could be made that would be in conflict with Adler's theory of 'inferiority complex'.[23]

3.4 - Degrees of Falsifiability

As a maxim, Popper stated theories that are easier to falsify by reason of their increased content and predictive power should be preferred to less falsifiable ones. For we can reduce the falsifying content of a theory until we either reach mundane observations, pleonasms or tautologies. Popper himself thought that the scientist should be imaginative in their conjectures when trying to solve a problem:

> I can therefore gladly admit that falsificationists like myself much prefer an attempt to solve an interesting problem by a bold conjecture, *even (and especially) if it soon turns out to be false*, to any recital of a

[20] Karl Popper, *The Poverty of Historicism* (London: Routledge, 1957), pp.77-158; *Conjectures and Refutations: The Growth of Scientific Knowledge* (London: Routledge & Kegan Paul, 1969), p.49
[21] It must be noted that Popper's treatment of Hegel and Plato was deemed unscholarly at the time. Popper borrowing from Schopenhauer's interpretation of Hegel. Jon Stewart, *The Hegel Myths and Legends* (Evanston, Ill.: Northwestern University Press, 1996), p.102
[22] Karl Popper, *The Open Society and Its Enemies*: New One Volume Edition (Princeton: Princeton University Press, 2013), pp.246-247. The animosity toward Hegel is clear calling him a 'clown'. Ibid., p.247
[23] Popper, *Conjectures and Refutations*, p.49

> sequence of irrelevant truisms. We prefer this because we believe that this is the way in which we can learn from our mistakes; and that in finding that our conjecture was false we shall have learnt much about the truth, and shall have got nearer the truth.[24]

This emphasis on picking out the mistakes is a part of a wider epistemic position called ‘evolutionary epistemology’. Deduction and falsification are powerful weapons for ‘killing-off’ redundant ideas. Here the scientific method is akin to the natural selection process, which would eventually leave us with only the ‘fittest’ theories and explanations for phenomena. Popper argued that part of good scientific practice was that theories should be daring and imaginative. Not only to make them more falsifiable, but *if* they withstood the test of falsifiability, it made them more likely to be true. As long as the ideas were rejected after falsification, scientific knowledge could progress, because otherwise there would be no reason to accept one idea over another. It was this methodological claim that underpins falsificationism, but in the spirit of falsification, this is not what we see when we look to the history of science. Here, we might find non-scientific reasons for why a scientist might continue to support a theory, even after the weight of evidence is against it. For example, cosmologist Fred Hoyle continued to back the ‘steady state’ model of the universe, even when evidence mounted in favour of the ‘Big Bang’ explanation because a universe with a beginning was philosophically troubling, as beginnings imply causes. To accommodate the growing number of observations that supported the Big Bang model, Hoyle invented a theoretical field that could create matter in line with the laws of energy conservation (C-Fields) this model being called ‘quasi steady state’ (QSS) cosmology.[25] If an expanding universe was philosophically troubling for Hoyle, there appears to be even more cause for concern today with most of the Universe and the means for its accelerated expansion unaccounted for. In their place we have ad-hoc explanations in ‘dark matter’ and ‘dark energy’. As NASA says, ‘theorists still don't know what the correct explanation is, but they have given the solution a name.’[26] Whilst Popper seem to dislike the sophistry of philosophers, his explorations in scientific reasoning brought him into contact with one of philosophy’s most slippery

[24] Ibid., p.231
[25] Jane Gregory, *Fred Hoyle’s Universe* (Oxford: Oxford University Press, 2005), p.334

concepts, 'truth'. Popper was wary about claiming 'truth' as this led to philosophical problems about essentialism, so he thought it was better to remain agnostic on the 'truth' of a statement and at best say it was 'truth-like', or had 'verisimilitude'. He wrote, 'we are seekers for truth but we are not its possessors.' [27] This I take to mean we can look for truth and recognise it but we can never have definite acquisition of it. The great scientific breakthroughs came about by jumping beyond the known observations and facts to the deeper structures of nature. Here, for Popper, there was no 'method' of discovery, but 'justification' of discovery was subject to highly rigorous procedures.[28]

Popper holds that falsification is the main principle by which scientists work and that highly predictive, bold conjectures, are the currency of good theories. Scientists, 'whether they realize it or not, this [falsification] is the rationale of what they do, and accounts for the way human knowledge develops.'[29] Yet if we look to history for examples we see this may not be so. Fred Hoyle said, 'a theory that is too predictive has a high risk of being prematurely and unjustly invalidated, since the exceptions that prove the rule are always the ones discovered first.'[30] In this case, Hoyle's theory could not be tested, and the predictions it did make were part of a newly developing field that was still not well understood. Since it is hard to know what one is falsifying in a new science, and falsifying instances are so hard to come by in cosmology, it would not seem to be the best criterion to operate by.[31] A Popperian may then say I am not giving a falsifying example, but merely illustrating the psychological dispositions of scientists. This would be the same as saying, we describe what scientists think they are doing, instead of what they are actually doing. How is one then to falsify Popper's theory of scientific knowledge? It would seem one might have to provide an alternative to inductive and deductive logic as a methodological basis for science. The catch here is that what have been offered as alternative accounts for

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[26] NASA, "Dark Energy, Dark Matter", *NASA Science: Astrophysics* <http://science.nasa.gov/astrophysics/focus-areas/what-is-dark-energy/> [accessed 14 Jan 2013]
[27] Popper, *Conjectures and Refutations*, p.47
[28] Popper, *Scientific Discovery*, p.8
[29] Magee, *Popper*, pp.30-31
[30] Gregory, *Hoyle's Universe*, p.334
[31] George Gale, "Cosmology: Methodological Debates in the 1930s and 1940s", *The Stanford Encyclopaedia of Philosophy*, 2013, ed. by Edward N. Zalta < http://plato.stanford.edu/entries/cosmology-30s/#SteStaCos> [accessed 12 March 2013]

thinking about science's development, have been taken to be substitutes for these methodological procedures.

3.5 - Falsification of What?

Once theory *X* has been posited, the fallibility of *X* can be logically deduced by observations *Y*. Yet, as Popper argued, all observations are theory-ladened and hence fallible. Consequently, the clash between *X* and *Y* does not logically demand that *X* is always at fault. All that logically follows from this situation is that we know either one or both are wrong. When observation and experimental evidence challenge the predictions of some established theory, it can conceivably be that the evidence is in error and not the theory. The observation that I saw a man levitate would confound the known scientific laws about humans and gravity. It is such a well-confirmed theory that humans cannot levitate, it invariably passes as fact. Every so often there are claims that humans have been observed to levitate. Do these claims falsify all that has been scientifically established in the past? What is more likely, given the evidence in favour of the theory that humans cannot levitate? In this situation we are in luck, as there is a massive weight of corroborated evidence in favour of one theory that helps us determine which is the *more likely*. In a situation, however, where both competing theories share equally compelling evidence or disharmonious claims between the facts and theory, the logic of the situation cannot help us decide. Observation alone cannot lead to the falsification of the theory. Here, Lakatos says that that there are only theories. We do not possess facts only 'factual propositions', which means, any clash between theory and factual propositions are actually inconsistencies not falsifications.[32] Feyerabend wrote, 'the right method must not contain any rules that make us choose between theories *on the basis of falsification*. Rather, its rules must enable us to choose between theories which we have already tested *and which are falsified*.'[33] Feyerabend offered instead a principle of 'counter-induction' as a way of thinking about the scientific method, in that, if what one thinks is probably the case then they should consider the opposite to be true. By use of historical examples, Feyerabend showed how an appeal to any neat principle, that conveniently explained

[32] Imre Lakatos, 'Falsification and the Methodology of Scientific Research Programmes', in *Criticisms and the Growth of Knowledge*, ed. by Imre Lakatos & Alan Musgrave (Cambridge: Cambridge University Press, 1999), pp.91-198 (p.99)

a lot of scientific phenomena, did so because it is presented that way and so we should be suspicious of it. This for Feyerabend was a way of challenging the everyday link between observation and language.[34]

The situation of determining the universality of the colour of swans is seen as rather simplistic, when one considers that the majority of modern science deals with unobservable entities. A simple universal statement is easy to falsify in a singular instance, but this is very rarely the case with modern scientific claims. Furthermore, as technology progresses and becomes evermore advanced, a theory under experimental tests will need to be augmented by additional assumptions about the theories behind the instruments being used. So not only is the theory under scrutiny, but also the experimental conditions of the test. It is conceivable that a prediction that is met with negative observations does not necessarily mean the theory is at fault. It is conceivable that this error has its origins in the equipment, the setting up of the experiment, a faulty analysis of initial conditions, and so on. An intricate theory cannot be conclusively falsified, as some part of the complex web that makes up the 'confirmation' of the theory could be in error and not the theory itself. It only becomes conclusively falsified if we assume every other part of the experiment; its auxiliary theories and assumptions are true and accurate. This criticism is also known as underdetermination of scientific theory by evidence or the 'Duhem-Quine thesis', which plays on the unknown variables or the uncertainty of a situation. It states that, we cannot know which assumptions to hold as true given that some of those assumptions will determine what we take as evidence for them in the first place.[35] Similarly for any given set of observations there are potentially an infinite amount of theories that are compatible with what is observed.[36]

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[33] Feyerabend, *Against Method*, pp.50-51 [Italics in original]
[34] Ibid., p.52-53. 'Counter-inductivism' can also be construed as a swipe at 'Popperianism'. Popper argued that one should always be critical and open to refutation but he appeared to not take criticisms or refutation very well. The intellectual persuasiveness of 'falsification', its use as a solution to the problem of inductivism and the dogmatic stance with which it was held was for Feyerabend a signpost that it must be wrong.
[35] Lakatos, 'Falsification and the Methodology', p.100-101
[36] Pierre Duhem thought that one could actually test an isolated phenomenon in physics but not in disciplines that dealt with multiple interacting variables. Willard Van Orman Quine proposed something a bit stronger and extended this scepticism to all human knowledge. Donald Gillies, 'The Duhem Thesis and the Quine Thesis', in *Philosophy of Science: The Central Issues*, ed. by Martin Curd and J. A. Cover (New York: Norton, 1998), pp.302-319

Strong falsificationism cannot be how science proceeds either, as there are many instances of observation not meeting theory. This may be summed up as provisional falsification not equating to conclusive falsification. The understanding that Popper's falsificationism meant chucking away any theory, as long as it has been falsified, is a caricature of his position. Even Popper claims his own students misunderstood him on this point.[37] Popper, in stressing the importance of falsification as a criterion of demarcation, has also been portrayed as elevating certain features that make claims falsifiable over others. Popper seems to advocate boldness, daring and imagination in theory formulation over conservative, safe estimates, because banal observations such as 'water boils' are not as informative or as falsifiable as the claim, 'pure water boils at 100°C in an open vessel at sea-level atmospheric pressure'. Yet boldness, daring, and imagination, as a feature of falsifiability, are not useful in and of themselves for the growth of scientific knowledge. The imaginative and highly falsifiable claim that 'all stars are made of yoghurt', whilst eliminating one claim from the set of observations about 'stars' and 'yoghurt', does not do us any favours as there are lots of things stars are not made of. Instead, Popper thought we should be looking at the falsification of established, safe conjectures as a route to genuine scientific advancement, whilst remaining agnostic on the yet-to-be falsified nature of other, more daring speculations. The wild claim that 'I am immortal', whilst easy to falsify, will have nowhere near the same impact on the scientific landscape as the falsification of the law of energy conservation, even though my immortality would imply its contravention.

Popper was also aware that falsificationism could be used to eliminate very healthy theories simply because all observations did not match all the theory's predictions. Currently there is a huge discrepancy between the theories of general relativity and quantum mechanics, since the theories work at two very different levels. Whilst the two theories by themselves are hugely successful, they seem to contradict each other when either is scaled up or down. This would imply that either one or both of them is wrong and so we must look for a better explanation. But should we scrap two of science's most successful theories just because they do not fit absolutely with reality? A more considered approach is to say, that because we know the theory is not

[37] Karl Popper, 'Replies to my Critics', in *The Philosophy of Karl Popper*, part 2 vol.14, ed. by Paul A.

completely correct it then compels us to find a better, more comprehensive theory. Popper argues that a strong metaphysical notion of truth or adherence to first-order logic is one of the problems that verificationism suffers from. Where something either is or is not the case. Popper argues that this type of justification cannot be applied to theory selection and testing when trying to allocate the best description of reality. Popper thinks it is not useful to regard a theory as either true or false, but to think in terms of degrees of truth, or better or worse approximations to the truth.[38] Because of the everyday and metaphysical association with the word 'true', Popper wanted to use the word 'verisimilitude' instead.[39] Popper has 'verisimilitude' on a continuum; at one end we have 'true', but as we can never be certain or prove anything beyond doubt (as it must be falsifiable and open to doubt) it is unobtainable. At the other end we have 'false' and as we can never have conclusive falsification, as there must be room for doubt, so is equally unobtainable. On this continuum of verisimilitude some statements are truer than others. As Popper believes he is expressing verisimilitude, and not truth, it has its analogue in 'fuzzy logic'. Here certainty comes in degrees of probability, but in substituting a term like 'truth' with 'verisimilitude', metaphysically, does nothing for what that term expresses. What is it that gives 'verisimilitude' verisimilitude? Here we can search for 'truth' as a satisfactory explanation of the aims of science, if by 'truth' we mean the set of all true propositions. Even though, in principle this is unobtainable, we are allowed to admit false statements as approximations, as long as they are not too false or that their falsity content is greater than their truth content. Here the 'falsificationist settles for progress rather than truth.'[40] So we see that Popper's falsificationism allows us to hold theories that are false, but as long as they are not too false.[41] This was seen as a retreat from falsification as a hard methodological principle to it being a normative prescription of what scientist should do, rather than what they do do. This would appear to deviate from the Popperian philosophy of providing a description of what scientists were 'really' doing rather than a psychological account of what scientists believe they were doing. We seem to have reduced falsification to a heuristic rather

Schilpp (La Salle, Il: Open Court, 1974), p.999-1013

[38] Popper, *Conjectures and Refutations*, p.314

[39] Ibid., p. 307

[40] Chalmers, *What is this thing*, p.86

[41] Popper, *Conjectures and Refutations*, p.318

than a criterion of science.[42] We intuitively think that scientific statements should be falsifiable, but in reality some of them are not. Here we seem to be getting at an ‘ethics’ of science, where we argue what science ‘ought’ to be rather than what it ‘is’.

The sophistication and complexity of Popper’s philosophy tends to be overlooked in favour of a mainstream account of falsification. We may find that because he apparently offered a methodological account of the scientific method, one that is intellectually seductive (too easy to understand), and seem to rally against the pseudo-intellectual claims of philosophers, psychoanalysts and Marxists, Popper tends to be lauded as the scientists’ philosopher. As Susan Haack says,

> It should come as no great surprise that some of Popper’s most famous admirers seem not to have fully understood the philosophy of science they so enthusiastically endorsed […] what they found most attractive about Popper’s ideas was, I suspect, his romantic picture of the scientist as a dedicated seeker after truth, making bold, imaginative conjectures, fearlessly testing them, frankly acknowledging when they are falsified, and heroically starting over.[43]

It is almost irrelevant to ask what Popper *actually* meant as this in itself implies metaphysics of language, world and mind. But the influence of Popper is undeniable as we can see a definite role played by Popper as the ‘scientist’s philosopher’. I will argue that he gets more of a voice as a ‘philosopher of science’, because scientists understood Popper through their own worldview of science, as being a purely methodological endeavour. I will not attempt to point out all the complexities of Popper’s philosophy, but just show that science for scientists, or a general understanding of science, tends to be equated with methodology. Philosophical accounts that give validity to that claim tend to be looked upon more favourably, than

[42] Feyerabend makes the point that one only gets heuristics ‘after the fact’. That only once messy imperfect scientific practice has made a contribution can it then be re-told as a beautiful, simple or elegant extrapolation of a previous position. Feyerabend, after quoting a sympathetic statement from Niels Bohr on the nature of inquiry, then says, ‘science is never a completed process, therefore it is always ‘before’ the event. Hence simplicity, elegance, or consistency is *never* necessary conditions of (scientific) practice.’ Feyerabend, *Against Method,* p.8 [In footnote - Italics in original]
[43] Susan Haack, “Just Say “No” to Logical Negativism”, *Evidence & Evidentiary Procedure eJournal,* 17 Aug. 2012 <http://www.papers.ssrn.com/sol13/papers.cfm?abstract_id=2126805> [accessed 14 November 2012]

those that do not. In opposition to this view, I place the historical account. I will be looking at the reception and attitudes towards Feyerabend in the next chapter as an example of this.

3.6 - Popper, Idol

The online *Stanford Encyclopaedia of Philosophy* says,

> [I]n the modern technological and highly-specialised world scientists are rarely aware of the work of philosophers; it is virtually unprecedented to find them queuing up, as they have done in Popper's case, to testify to the enormously practical beneficial impact which that philosophical work has had upon their own.[44]

The Nobel-Prize winner for medicine, Sir Peter Medawar, has publicly declared Popper to be 'incomparably the greatest philosopher of science there has ever been.'[45] Jacques Monod, also a Nobel-Prize winner for medicine, wrote the foreword to the French version of *The Logic of Scientific Discovery*.[46] Sir John Eccles, another Nobel-Prize winner, wrote, 'my own scientific life since 1945 owes much to my conversation (if I may call it so) to Popper's teaching'.[47] The cosmologist Hermann Bondi was a great advocate of Popper's works. Even the anti-philosopher Hawking references Popper in *A Brief History of Time* as an upholder of good scientific reasoning.[48] There may be an elevated sense of scientific kudos for Popper as his forays into science were not only as part of developing an abstract methodological

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[44] Stephen Thornton, "Karl Popper", *The Stanford Encyclopaedia of Philosophy*, 2011, ed. by Edward N. Zalta <http://plato.stanford.edu.entries/popper/> [Accessed 13 November 2012]

[45] Magee, *Popper*, p.9

[46] Karl Popper, *La Logique de la Découverte Scientifique*, trans. by Nicole Thyssen-Rutten and Philippe Devaux (Paris: Payot, 1973)

[47] John C. Eccles, 'The World of Objective Knowledge', in *The Philosophy of Karl Popper*, part 1 vol. 14, ed. by Paul A. Schilpp (La Salle, Il: Open Court, 1974), pp.349-370 (p.349). In the same paper Eccles writes that 'the utmost importance for scientists, and especially for the leaders of scientific research, to be illuminated and guided by a *theory of scientific method*.' Ibid., p.350 [My emphasis]

[48] Hermann Bondi and Clive W. Kilmister, 'The Impact of Logik der Forschung', *British Journal for the Philosophy of Science*, 10 (1959-60), 55-57, 'Sir Karl Popper – On His Eightieth Birthday', *Foundations of Physics*, 12 (1982), 821-823, Stephen Hawking, *A Brief History of Time: From the Big Bang to Black Holes* (New York: Bantam Books, 1989), p.12

account of science, but he also contributed to the 'new' physics.[49] Revered physicists such as Bohr, Einstein, and Schrödinger attended his lectures and entered into extended debate with him.[50] It is not just amongst scientists that Popper has cemented his reputation, 'Popper towered above circle [Vienna Circle] philosophers – old and young alike. He had Schlick's erudition, and was acquiring his lucidity. He has Carnap's logical precision, and intellectual agility to add. He had Neurath's imagination, and ingeniously translated it into methodology.'[51] For Victor Kraft, Popper came to replace Wittgenstein as the Circle's major influence and it was by his opposition that the Vienna Circle progressed.[52] It was his attacks on the positivist's ideology that he forced Circle scholars to advance their own arguments.

Popper's influence did not just end after the collapse of logical positivism. His accounts of the scientific method are still with us, almost naturalized within scientific education. The hypothetico-deductive model of forming hypothesis, constructing an experiment, making observations, and testing the hypothesis, is now common practice as a representation of the 'scientific method'. The key assumption being that hypothesis is distinct from observation, which can be falsified.[53] The Key Stage 3 curriculum for science in the UK states that a key concept of science is that theories should be falsifiable or that they 'at least in principle, be tested by observation and/or experiment.'[54] For me the PUoS should contain an explicit acknowledgement and understanding of the philosophical issues at stake, which may be one of the barriers to understanding science. Science textbooks and curriculums, however, tend to be

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[49] Karl Popper, *Quantum Theory and the Schism in Physics: From the Postscript to the Logic of Scientific Discovery*, ed. by W. W. Bartley III (London: Routledge, 1992), pp.27-29. Now known as the 'Popper Experiment' which has been tested. See, Yoon-Ho Kim and Yanhua Shih, 'Experimental Realization of Popper's Experiment: Violation of the Uncertainty Principle?', *Foundations of Physics*, 29 (1999), 1849-1861

[50] Karl R. Popper, *Unended: An Intellectual Biography* (London: Routledge Classics, 2002b), pp.148-151, Popper also accredits himself with the honour of being the one to convince Einstein that his view on the relationship between subjectivity and probabilistic interpretation of quantum mechanics were wrong. Popper, *Quantum Theory and the Schism in Physics*, p.4 [In footnote no.10]

[51] Malachi Haim Hacohen, *Karl Popper The Formative Years 1902-1945: Politics and Philosophy in Interwar Vienna* (Cambridge: Cambridge University Press, 2000), p.210

[52] Victor Kraft, 'Popper and the Vienna Circle', in *The Philosophy of Karl Popper*, part 1 vol. 14, ed. by Paul A. Schilpp (La Salle, Il.: Open Court, 1974), pp. 185-204 (p. 200)

[53] Sir Peter Medawar criticised this structure of investigation leading to 'knowledge' as he claimed its presentation in scientific papers gave a false impression of how conclusions are arrived at, that experiments are very rarely that linear. Peter Medawar, 'Is the Scientific Paper a Fraud?', *The Listener*, 12 Sept. 1963, 377-8

[54] DfES, *Science: Programme of Study for Key Stage 3 and Attainment Targets* (London: Department for Education and Skills, 2007), p.208

written or at least advised on by scientists. This would be unproblematic if science were straightforwardly methodological, but the failure of Popper and the logical positivists would imply that it is not. If those charged with writing educational texts on science have somehow demarcated philosophical from scientific thought, it is not done on a point of method. Yet we get scientists making statements about science, as if it were unproblematic. Astrophysicist, Sir Malcolm Longair, writes for Key Stage 3 students of science that, 'knowledge and understanding in science are rooted in evidence.'[55] Yet we may ask, do we not need knowledge and understanding of the thing we are investigating in order to know what counts as evidence in the first place? On this view, evidence is rooted in scientific knowledge and understanding, which would be a circular argument if, we could only appeal to methodological accounts of how we come to know things in the first place.

Karl Popper is often contrasted with the 'Vienna Circle', but in their dislike of 'pretentious wisdom' was 'very much at one with the Vienna Circle and with its spiritual father, Bertrand Russell.'[56] There are very good cultural and historical reasons why Popper disliked 'philosophy'. With the rise of belief in the destiny of nations as embodied by National Socialism, or the corrupting influence of political ideology on science as witnessed by the Lysenko affair, Popper felt none of these could be combated by appeals to traditional philosophy. In fact, it was with philosophical thinking that he thought the problem lay. On another point, Popper in contrast to Feyerabend, can be deemed 'more rational' in his rejection of Nazism. This in retrospect can be interpreted as moral and rational expediency, that one was seduced by ideology (Feyerabend) whereas the other saw right through it (Popper). This dislike of philosophy is one that is echoed by many contemporary scientists and is implied in the Government's *Science and Technology Sixth Report*.[57] I do not want to argue that all philosophical contributions are valid, but I think that the 'philosophy' Popper, the logical positivists, and certain scientists dislike, is a philosophy of a particular kind. Indeed, in order to show that some philosophical contributions are better than others, one has to know how philosophical activity is performed. In line with my thesis, Putnam in response to Popper stressed the importance of scientific

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[55] Ibid.,
[56] Karl Popper, 'Intellectual Autobiography' in *The Philosophy of Karl Popper*, part 1 vol. 14, ed. by Paul A. Schilpp (La Salle, Il: Open Court, 1974), pp.3-181 (p.70)

practice. He says, 'what Popper consistently fails to see is that *practice is primary*: ideas are not just an end in themselves […] The primary importance of ideas is that they guide practice, that they structure whole forms of life.'[58] This criticism, I think, can be made in terms of the historical-methodological distinction I will be making. Putnam also picks up Popper on his anti-philosophy streak. '[T]he failure to see the primacy of practice leads Popper to some rather reactionary political conclusions.' Popper, thus, on the grounds of falsification a priori rules out Marxism or psycho-analysis as being able to make 'true' statements. The irony being, Putnam notes, that this is done 'in the name of an anti-a priori philosophy of knowledge!'[59]

Popper makes it clear he has a particular idea of what metaphysics he dislikes; the features associated with philosophical theories, and not scientific ones. In *Metaphysics and Criticizability* he lists five theories that he believes exemplify philosophical doctrine, determinism, idealism, irrationalism, voluntarism and nihilism.[60] He argues that whilst these theses are irrefutable, he believes them to be false. He first thinks we can distinguish between three types of theory: logical/ mathematical, empirical/ scientific, and philosophical/ metaphysical.[61] He gives no criteria for how they should be grouped and thus assumes for the rest of the paper that those distinctions are unproblematic. Popper's reasoning behind why he can state that those philosophical doctrines are irrefutable, but false, is because he can treat them as if they were designed to answer specific problems, as a scientific theory might. Popper says, 'every *rational* theory, no matter whether scientific or philosophical, is rational in so far as it tries to *solve certain problems*.'[62] The binary Popper establishes is; either a philosophical theory tries to answer something specific and to this end can be tested or it is irrational. Yet, if we start with Popper's conclusion, can we ask what makes something a problem in the first place? Popper offers the idea that where belief and reality meet, there will be problems generated by the mismatch, and so the belief then has to be altered to accommodate for what is observed. The conclusion being that

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[57] House of Lords, *Science and Technology - Sixth Report*, para. 2.12 - 2.23
[58] Hilary Putnam, 'The Corroboration of Theories', in *The Philosophy of Karl Popper*, part 1 vol. 14, ed. by Paul A. Schilpp (La Salle, Il: Open Court, 1974), pp.221-240 (p.238) [Italics in original]
[59] Ibid.,
[60] Karl Popper, 'Metaphysics and Criticizability', in *A Pocket Popper*, ed. by David Miller (Oxford: Fontana Paperbacks, 1983), pp.209-219 (pp.210-11)
[61] Ibid., p.214
[62] Ibid., p.216

philosophical theories generate philosophical problems. The hope was that science would slowly unpick the problems of philosophy, by having a greater correspondence with reality, and thus do away with great swathes of 'pretentious wisdom'. What Popper does not consider is that this whole way of looking at reality, the relations between reality, truth, knowledge and science, even if all is provisional, is itself metaphysical. It was this tension that Wittgenstein's *Tractatus* brought into view. But as Popper took the logical method to be central to Wittgenstein's achievements, this then galvanized his views on 'philosophy'. In response to Wittgenstein's conclusions, such that the most meaningful propositions in language cannot be stated in any unambiguous way, Popper says,

> All the metaphysical nonsense against which Bacon, Hume, Kant, and Russell have fought for centuries may now comfortably settle down, and even frankly admit that it is nonsense. (Heidegger does so) For now we have a new kind of nonsense at our disposal, nonsense that communicates thoughts whose truth is unassailable and definitive; in other words, deeply significant nonsense […] the anti-metaphysical theory of meaning in Wittgenstein's *Tractatus*, far from helping to combat metaphysical dogmatism and ocular philosophy, represents a re-inforced dogmatism that opens wide the door to the enemy, deeply significant metaphysical nonsense, and throws out by the same door, the best friend, that is to say, scientific hypothesis.[63]

As metaphysics is so pervasive in language and ideas, when we are tempted to dismiss ideas as 'nonsense' because they appear to clash with commonsense notions about reality, it may be because our metaphysics have gone wrong not the rationality of the person stating it. Yet, as the most belligerent metaphysics lie at the core of 'commonsense' it is very hard not to take this as just how things are. I would think every person holds a view of 'how things are' and as Popper's philosophy comes from a position of persuasive 'commonsense', I doubt many see their error. For example,

[63] Karl Popper, 'Notes', in *A Pocket Popper*, ed. by David Miller (Oxford: Fontana Paperbacks, 1983), pp.379-461 (pp.404-405)

the scientist John Eccles thought there was much to Popper's three-world view.[64] There is the first world of things, the second world of subjective experiences and the third world of objective knowledge. Once we take this to be true the problem is then, how do these three worlds communicate? How do we pass from dead symbols on a page to my interpretation and understanding of them, which result in objective knowledge?[65] Eccles is not alone in supporting such a view. Physicist Roger Penrose, in his 'objective reduction theory of consciousness' uses Popper's three worlds to structure the relationship of the internal mind, external world and objective truth.[66] Penrose, though, is stuck with the same problem as Eccles, as each of the three worlds appears to imply knowledge of the other. 'There is a seemingly paradoxical aspect to these correspondences, where each world seems to 'emerge' from but a *tiny* part of the one which precedes it.'[67] Again what is viewed as unproblematic is the metaphysical view of an internal mind, external world, and truth being a correspondence between the two, resulting in a repository of objective knowledge. Neither considers the philosophical basis for such a view by asking, what is it that enables us to speak meaningfully about reality like this?

Whilst not directly related to Popper the project of establishing phenomena on methodological grounds is highly appealing to other subjects. The search for 'Artificial Intelligence' (AI) began by an appeal to propositional logic and rule following, but soon found that 'meaning' is not reducible to either. Currently the some of the best attempts lie in 'information theory', that uses probability and statistical methods for trying to simulate intelligence. Whilst it has not succeeded in replicating 'human intelligence', it has had wider application in 'intelligent' search engines. The irony of this approach is that 'information', in its technical sense, is meaningless to humans. Such that a 'search term' can be broken down to bits and processed probabilistically, through certain sets of syntactical and grammatical rules, inference rules, and decision trees. Search-engines do not find search terms by understanding the meaning of the phrase typed, but by a series of heuristics and

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[64] Eccles, 'The World of Objective Knowledge', pp.359-61
[65] Harry Collins has also tried to tackle this problem via his notion of strings and tacit knowledge but still fails to acknowledge that one is starting from a metaphysical position when we assume knowledge can be objective or meaning separate from language. Harry M. Collins, *Tacit and Explicit Knowledge* (Chicago: Chicago University Press, 2010), p.45
[66] Roger Penrose, *Shadows of the Mind* (London: Vintage, 1995), pp.411-20
[67] Ibid., p.418

algorithms, in an essentially statistical way. The project of replicating human intelligence has been criticised by Dreyfus and more recently by Chomsky, amongst others.[68] However, while their critiques appear to be justified by the problems that AI theorists and scientists face, Marvin Minsky, a leading cognitive scientists in the field of AI accused philosophers, such as Dreyfus, for misunderstanding their work and that they 'should be ignored.'[69] Here again we see the meddling philosopher needlessly complicating serious scientific work. The sorts of problems I am raising here, in that the methodological is seen as superior to any other approach, I will develop later on with regard to ways of reading Kuhn.[70] For now all I want to do is highlight the leeway methodological accounts of science and phenomena get by scientists, as it tends to fit with their worldview. A philosophical basis, such as Popper's that reinforces this worldview receives far less criticism by scientists, than a historical account of the same thing. The sorts of solutions or criticisms that may be offered, as with Dreyfus, will seem quite unpalatable to the scientist as it will question the sorts of metaphysical foundations one has or is presuming in order to establish something like 'human intelligence'.

Next, I will offer in contrast to Popper's methodological account of science, an explanation of his protégé Paul Feyerabend. I will loosely identify Feyerabend's approach as 'historical', as not only does he argue against a methodological conception of science, but unlike Popper, he argues from historical examples and not abstract philosophical principles. As Feyerabend's account challenges certain ideas about the methodological account of science, the correspondence notion of truth and the possibility of objective rational knowledge, ideas held sacred by the scientific community, he is treated with hostility. I will first offer a historical-biographical account of Feyerabend and then provide a reading of Feyerabend that is typical of some scientists and analytical philosophers. That is, it will be a fairly 'literal' reading of Feyerabend, treated with a minimal amount of charity.

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68 Hubert Dreyfus, *What Computer's Can't Do* (New York: MIT Press, 1979), Yarden Katz, "Noam Chomsky on Where Artificial Intelligence Went Wrong", *The Atlantic*, 1 November 2012 <http://theatlantic.com/technology/archive/2012/11/noam-chomsky-on-where-artifical-intelligence-went-wrong/261637/?single_page=true> [accessed 19 Nov 2012]

69 Daniel Crevier, *AI: The Tumultuous Search for Artificial Intelligence* (New York: Basic Books, 1993), p.143

70 See pp.146-244 in thesis comparing a 'strong' versus 'weak' way to read Kuhn.

Chapter Four

Paul Feyerabend: Against Method

4.1- Paul Feyerabend

Before discussing Feyerabend's ideas, and how they have been received, I would like to start with a small biography. The reason for this is that 'philosophy' does not occur in a vacuum, but tends to be a reaction to something. The Feyerabend that is read at face-value can be bracketed with the cultural relativists and postmodernists, who appear intent on denying the epistemic authority of science. Those who have placed Feyerabend in this camp have been described as the 'anti anti-science' movement.[1] Here, Feyerabend is interpreted as either not understanding the positivistic accounts of science, or denying that a methodology is possible. What is more, it would appear that the later Feyerabend recants his alignment with the relativists, deconstructionists and social constructivists, due to the explanatory power of the scientific method.[2] A quick look, however, at his biography will show that to start from these assumptions is unfair. After the biographical account, I will attempt to render a reading of Feyerabend that is familiar to the 'anti' anti-science movement, which calls for a 'literal' and uncharitable reading of his ideas.[3]

Paul Feyerabend, like Wittgenstein, Popper, and many of the 'Vienna Circle', was Austrian but practised in the Anglo-American traditions. This movement of 'Continental' philosophers out of Europe was no coincidence, but the result of Nazi activity. Yet there is a claim that the sort of 'cultural relativism' frequently levelled at Feyerabend, is an offshoot of post-Kantian, 'Continental' philosphy. Whilst we might be able to identify some of the features that earmark the 'Continental' tradition that the logical/empirical positivists disliked so much, there is equally an argument that

[1] Ullica Segerstråle, 'Anti-Antiscience: A Phenomena in Search of an Explanation', *in Beyond the Science Wars: The Missing Discourse Between Science and Society*, ed. by Ullica Segerstråle (New York: State University of New York Press, 2000), pp.101-122 (p.93)
[2] Paul Feyerabend, 'Atoms and Consciousness', *Common Knowledge*, 1 (1992) (1), 28-32 quoted in Paul Gross and Norman Levitt, *Higher Superstition: The Academic Left and its Quarrel with Science* (Baltimore: John Hopkins University Press, 1994), p.49; Alan Sokal, *Beyond the Hoax: Science Philosophy and Culture* (Oxford: Oxford University Press, 2008), p.197 [In footnote]

suggests that those very features can be found in the American pragmatic, Rortian, Quinean philosophies of the Anglo-American traditions.[4] Whilst I could also add Wittgenstein or Kuhn to this list, this for me, is beside the point. For it is not useful to play a blame game on which tradition or philosophy is inferior. Rather, if we can find the source of this inferiority and what then could these two 'traditions' learn from each other? Apart from rehearsing the arguments about why cultural relativism cannot apply to scientific truths, it gives us a handy excuse not to 'get-to-grips' with Feyerabendian philosophy. Instead, if we can just use him as a shorthand term for 'postmoderism' or 'anti-science' philosophy, everyone knows where they stand.

Feyerabend studied physics and then philosophy, adhering to a highly conventional positivistic outlook, that science was the basis of all knowledge, and that non-empirical claims were either illogical or meaningless.[5] Feyerabend and Popper met at the Alpbach seminars. Feyerabend seemed to enjoy Popper's disregard for authority and tradition, and his ability to clarify philosophical problems.[6] Feyerabend claims that Popper's views were not new to him, saying deductivism was being offered as an alternative to inductivism by his later supervisor Victor Kraft, a once member of the 'Vienna Circle'. Feyerabend also noticed similarities in thought between Popper and Wittgenstein, both being influences on him.[7] The 'Kraft Circle' named after his supervisor Victor Kraft, of which Feyerabend was a founding member, was a meeting of scholars who wanted to discuss 'philosophical problems in a non-metaphysical manner and with special reference to the findings of the sciences'.[8] It was after one of Wittgenstein's lectures that Feyerabend said, 'when he left we still did not know whether or not there was an external world'.[9] This one quote seems to dig at the heart of traditional philosophical problems and the unsatisfactory responses of positivism.

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[3] I place 'literal' in inverted commas as the term is problematic for it implies an already accepted metaphysical view of reality. This critique will be developed further in chapter 7.
[4] Christopher Norris, *Epistemology: Key Concepts in Philosophy* (London & New York: Continuum, 2005), pp.84-86
[5] Paul K. Feyerabend, *Killing Time: The Autobiography of Paul Feyerabend* (Chicago: University of Chicago Press, 1995a), p.68
[6] Paul Feyerabend, *Science in a Free Society* (London: New Left Books, 1978), p.115
[7] Ibid., p.116
[8] Paul K. Feyerabend, 'Herbert Feigl: A Biographical Sketch', in *Mind, Matter, and Method: Essays in Philosophy and Science in Honour of Herbert Feigl*, ed. by Paul K. Feyerabend & Grover Maxwell (Minneapolis: University of Minnesota Press, 1966), pp.3-16 (pp.3-4)
[9] Ibid., p.4

Whilst Feyerabend was studying physics at the University of Vienna he became interested in the work of physicist Felix Ehrenhaft.[10] Indeed, Ehrenhaft's professional life almost serves as a template for Feyerabend's later thinking on science and scientific rationality. In 1910, Ehrenhaft was in a dispute with Robert Millikan concerning the explanation of the observed effects concerning the 'oil drop' experiment. Whilst the history of science has sided with Millikan, it is reported that it was Ehrenhaft who was doing the better science. 'Hand-written notebooks show that Millikan discarded 59% of the drops, as they did not provide support for this hypothesis of the elementary electrical charge.'[11] Instead of investigating results that failed to meet his theory, he simply discarded them as failures.[12] Whilst this controversy was enough for Millikan to be over looked for the Nobel-Prize, it is claimed that the majority of chemistry textbooks in the USA do not contain reference to this episode in its history.[13]

It was at the 1949 Alpbach meeting that Ehrenhaft, who was known for being a critic of orthodoxy, took part in a lively debate with the group, which had a lasting effect on Feyerabend. By using the results of physical experiments, Ehrenhaft was able to challenge the empiricist's stance that we simply observe and do not interpret. Using the same oil-drop experiments Ehrenhaft demonstrated new phenomena, 'which cannot be interpreted at all in the customary manner.'[14] As Feyerabend and his colleague were not prepared to give up their orthodox explanations, persuasion had to come from elsewhere. He realised, in retrospect, the strategies he and his fellow empiricists had to resort to in order to counter Ehrenhaft's experimental results were themselves non-empirical. 'Only much later did Ehrenhaft's lesson sink in and our attitude at the time of the entire profession provided me then with an excellent illustration of the nature of scientific rationality.'[15] Feyerabend swapped from physics to philosophy as his doctoral subject. The exact reasons for this are speculative, but part of the 'uncharitable' reading of Feyerabend in this section will suggest that

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[10] Eric Oberheim, *Feyerabend's Philosophy* (Berlin: Walter de Gruyter Gmbl & Co, 2006), p.116
[11] Ibid., p.117
[12] Mansoor Niaz, 'The Oil Drop Experiment: A Rational Reconstruction of the Millikan-Ehrenhaft Controversy and it Implications for Chemistry Textbooks', *Journal of Research in Science Teaching,* 37 (2000), 480-508 (p.490)
[13] Ibid., p.499-501
[14] Oberheim, *Feyerabend's Philosophy*, p.118
[15] Feyerabend, *Free Society*, p.111

proper science was too difficult for Feyerabend and so he had to settle for philosophy.[16] Moreover, Feyerabend had already shown greater interest in the later Wittgenstein, having published several papers after reading *Philosophical Investigations* in manuscript form.[17] Whilst originally going to study with Wittgenstein at Cambridge, Feyerabend's plans had to change after Wittgenstein died. From here, he then decided to study under Karl Popper. As Popper's ideas were not too dissimilar to that of the 'Kraft Circle', Feyerabend easily assimilated the principle of falsification. He considered himself a weak falsificationist, in that, scientists should defend their theories in the face of falsifying instances and that scientific theories can start out as being untestable. However, as Popper developed his normative epistemology, working from reasoned principles to how science is done, Feyerabend thought this way of looking at things 'may be out of touch with reality, [and] with scientific practice.'[18]

Feyerabend increasingly looked towards the history of science as a source for understanding how science is conducted. His approach, becoming evermore historical, allowed him to challenge the ontological foundations of methodological claims. This was done by a number of routes, but it was this aspect that the concept of 'incommensurability' drew out the most. He says, 'a theory is incommensurable with another if its ontological consequences are incompatible with the ontological consequences of the latter.'[19] This comes about by attempting to interpret fundamental theories through a strong realism, as if they described genuine aspects of reality. A prerequisite for Feyerabend's incommensurability thesis to work is that 'the interpretation of an observation language is determined by the theories which we use to explain what we observe, and it changes as soon as these theories change.'[20] So in this respect, a theory for Feyerabend is, 'an ontological framework and thus

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[16] The online 'Stanford Encyclopaedia of Philosophy' states in parenthesis amongst the biographical information '(the philosopher of science as failed scientist?)'. John Preston, "Paul Feyerabend", *Stanford Encyclopaedia of Philosophy*, winter 2012, ed. Edward N. Zalta <http://plato.stanford.edu/archives/win2012/entries/feyerabend/ > [accessed 15 November 2012]. Part of Feyerabend's scientific credentials is that he held a Masters degree in astronomy.
[17] Feyerabend, *Free Society*, p.114
[18] Feyerabend, *Killing Time*, p.90
[19] P. K. Feyerabend, *Realism, Rationalism, and Scientific Method: Philosophical Papers Vol. 1* (Cambridge: Cambridge University Press, 1985), p.xi
[20] Paul Feyerabend, *Against Method*, 4th edn. (London: Verson, 2010), p.219

practically irrefutable within its own extent of empirical application.'[21] Feyerabend tries to illustrate the ontological character of his argument when he states, 'major discoveries, I said are not like the discovery of America, where the general nature of the discovered object is already known.'[22] Instead, discoveries are inextricably linked with the overcoming of the old observation language, which is achieved by an 'anything goes' ethic of practice. With the movement away from a normative account of science to how the ontological foundations of science are revealed by historical changes in science, we see his departure from Popperian thought. We cannot start from reasoned principles to how science is done. We cannot provide a methodological account of what is, essentially, a historical act. Arguably Feyerabend's most famous work, *Against Method* is in itself a model of Feyerabend's argument. It is not a linear book or an extended essay, but a collage of thoughts, the culmination of a number of encounters.[23] It is not even one book. In its four editions it shifts focus, theme and authorial position.[24] From the discussions with physicist Carl Friedrich von Weizsäcker about the foundations of quantum mechanics, to conversations with Kuhn regarding the idea of incommensurability, to the increased cultural diversity of students accepted to Berkeley, all have acted on Feyerabend's conception of science.[25] This enabled Feyerabend to realize 'the discrepancy between abstract normative thinking about science (including his own up until that point), and the actual, complex, and context-dependent practice of science.'[26] Theory and experience emerge together rather than theoretical statements being tested against a separate reality. These divisions of methodology, history, epistemology and ontology will be central to developing the two versions of Kuhn later on.

[21] Jeu-Jenq Yuann, 'A Naturalistic Approach to Scientific Methodology: A Comparative Study of O. Neurath and P. Feyerabend', in *Naturalized Epistemology and Philosophy of Science* ed. by Chienkuo Michael Mi and Ruey-Lijn Chen (New York: Rodopi Philosophical Studies, 2007), pp.171-197 (p.193)
[22] Feyerabend, *Killing Time*, p.92
[23] Ibid., p.139
[24] Hacking claims that Feyerabend wrote three different books under the same title. Ian Hacking, 'Paul Feyerabend: Humanist', *Common Knowledge*, 3 (1994), 23-28
[25] Paul Feyerabend, *Against Method*, 3rd edn. (London: Verso, 2002), pp.276–279. Feyerabend acknowledges Kuhn's influence during the period 1960-61 where he remarks that since meeting Kuhn he has looked at 'science in a new way ever since.' Paul Feyerabend, 'Consilations for the Specialist', in *Criticisms and the Growth of Knowledge*, ed. by Imre Lakatos & Alan Musgrave (Cambridge: Cambridge University Press, 1999c), pp.197-230 (p.197)

4.2 - Against Method

Next, I will give an outline of Feyerabend's argument in *Against Method*, which will be followed by an analysis of the reception and reactions Feyerabend's work has received. I will argue that some of the more radical criticisms of Feyerabend are a result of his historical account being treated as a methodological response to the progress of science. A common trend is that to derive the 'radical' Feyerabend he has to be read 'literally' and with minimal charity.[27] For example, Sokal writes, 'despite the title of one of his books, *Farewell to Reason*, he never became entirely and openly an irrationalist.'[28] If Sokal thinks Feyerabend wants to 'literally' say farewell to the use of reasoned argument or rationality, he has almost completely missed the point of that book, which is to wave goodbye to a particular philosophical notion of 'Reason'. Pigliucci pays Feyerabend even less respect with a heading, 'Rebel with a Feeble Cause: The Ranting of Paul Feyerabend'.[29] As Feyerabend's writing lends itself more to European 'Continental' philosophy, it should come as no surprise that his work received a better reception in Europe than in the English speaking world. Hacking, in the introduction to the fourth edition of *Against Method*, quotes Largeault claiming his review to be 'far more perceptive than most of the English-language reviews at the time.'[30] This lack of charity or overly naïve interpretation I will call the 'strong' reading. This will be developed more fully with the work of Kuhn in chapter 7. The 'strong' and 'weak' readings are hermeneutic devices that make up part of the deconstructive process. That is, we do not just read a text but we interpret. The 'strong' and 'weak' readings reveal the basis for our interpretations or what meanings we pull from the text. Here, I hope to show that how we already come to the text will influence what reading we are likely to elicit. Equally, it is a caricature or 'strong'

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[26] Paul Hoyningen-Huene, 'Paul K. Feyerabend: An Obituary', in *The Worst Enemy of Science? Essays in memory of Paul Feyerabend*, ed. by John Preston, Gonzalo Munévar and David Lamb (Oxford: Oxford University Press, 2000), pp.3-15 (p.6)

[27] Davidson states the principle of charity as the need to 'make maximum sense of the words and thoughts of others when we interpret in a way that optimizes agreement.' Donald Davidson, 'On the Very Idea of a Conceptual Scheme', *Proceedings and Addresses of the American Philosophical Association*, 47 (1973-74), 5-20 (p.19). Unfortunately 'agreement' in this sense is over the very faculties we are using to make the claim in the first place. I, however, take 'principle of charity' to mean that when an educated and otherwise rational person says something that appears outrageous or nonsensical then maybe we have not understood the point.

[28] Sokal, *Beyond the Hoax*, p.197

[29] Massimo Pigliucci, *Nonsense on Stilts: How to Tell Science from Bunk* (Chicago: The University of Chicago Press, 2010), p.258

[30] Feyerabend, *Against Method*, p. vii

reading of deconstructivism that suggests there are infinite meanings to a text and one interpretation cannot be privileged over another. Deconstructivism, however, actually looks to uncover the basis that make such claims intelligible. It is usually by the sheer force of rigour and logical coherence that either reveal such statements to be rooted in a particular metaphysics, which have not been accounted for in the original statement, or the statement 'deconstructs' itself under the weight of its own argument. So, for example, the inability to come up with a scientific method for telling science from non-science means we may have to look harder at what grounds a methdological statement or equally why in its absence we do not end up with ultra-relativism.

Arguably the opposite or absence of 'deconstructivism' is in inheriting a 'received view'. Here, people like Feyerabend or Kuhn have their work already interpreted for us and we are simply asked to build on to it. Segerstråle notes this with regard to the 'anti' anti-science movements' attitude towards Kuhn, 'Gross and Levitt seem to believe that the meaning of Kuhn's book was somehow fixed once and for all, and others did not have the right to "incorrectly" interpret it!'[31] I take this as analogous to the task being asked of the public in PUoS, where science has been interpreted through a methodological worldview, that then limits further interpretation. The 'weak' reading will not only aid in diagnosing 'sloppy thinking' but also defend the epistemic authority of science against pseudo-scientific claims. I use the terms 'weak' and 'strong' in reference to their commitments to ignoring philosophy and metaphysics.

In *Against Method*, Feyerabend makes the case for there being no universal, ahistorical method of science, that contains the standards that all sciences should aspire to, if they wish to be considered scientific. It should be understood that Feyerabend is not providing a criterion of demarcation as such, but is offering an argument for why such a proposition is impossible in the first place. Feyerabend makes the case for science being of no particular authority or of any privileged position in knowledge acquisition, either in its logic or its methods. He claims that when one looks at the history of science and that of theory change, there can be found no singular method, principle or rule within the history of scientific practice which

[31] Segerstråle, 'Anti-Antiscience', p.94

has not been violated at some point in the advancing of scientific knowledge. Feyerabend is not critiquing science per se, but is critiquing the abstract rationalism and philosophy of science that supports such a worldview. Feyerabend describes his famous phrase 'anything goes' as 'the terrified exclamation of a rationalist who takes a closer look at history.'[32] This I take to mean, if one views science as a unified, systematic process, that adheres to strict methodological rules, then on historical reflection, we will have no explanation for how science proceeds. In absence of an explanation we may be forced to conclude that either, science does not progress or there is no such thing as 'science'. Feyerabend uses the 'Copernican Revolution' as a case study of how there is no singular methodology within the advancement of scientific knowledge. Typically, in the scientific narrative of this period of controversy between Galileo and the Church, Galileo is portrayed as the hero and champion of Reason, and the Church the dogmatic holder of unscientific belief. The Galileo case is crucial for Feyerabend, since he is questioning a certain idea of scientific progress and of paradigmatic conceptual change. Feyerabend portrays Galileo as making full use of rhetoric, propaganda, and various epistemological slights-of-hand, in order to support a heliocentric cosmology. He also looked to diminish the importance of empirical argument by suggesting those non-scientific influences such as, aesthetics, personal bias and social factors, have a far more decisive role in the history of science than the rationalist or empiricists admit.[33] As Feyerabend saw it, there is no scientific method, and if science has no method, then there is no particular way of investigating nature to attribute science's success to. There is no reason to say 'science' is the best way of acquiring knowledge. The strong reader understands these claims as there being no difference between religion and science, where one can arbitrarily replace one tradition with another. It is this strong relativism that worries the defenders of science, but paradoxically, we also see it active in the beliefs of pseudo-scientists, who would be their real opponents and not a philosophy that is committed to the history of how science is actually practised. Refutation here though is not by way of empirical proof, as that is what Feyerabend is rejecting, but can be done by sound philosophical argumentation. When we do look to

[32] Feyerabend, *Against Method*, p. xvii

[33] Chalmers (1985; 1986; 2010) has raised criticisms with Feyerabend's accounts of the Galileo episode but also stated that even on his 'corrected' interpretation of history Feyerabend's case against method can still stand. Alan F. Chalmers, *What is this thing called Science?* 3rd edn. (Maidenhead: Open University Press, 2010), p.161

the history of science we find that as Popper expressed, it is a negative process of conjecture and refutation. In that we know what is false, but not what is true. Feyerabend goes a step further and suggests that in the establishing of certain 'successful' theories, rather than just appealing to logical deductivism, all sorts of non-scientific elements will be used to justify its superiority. So where Feyerabend suggests that, 'the show has been rigged in its [science's] favour', rather than understand this as a statement about the methodological arbitrariness of science, we understand this as how the success of science has no fundamental basis in its methodologies.[34]

Feyerabend's campaign against the idea of science as a universal method was to produce the response that there is no method. Scientists should simply follow their own subjective wishes. Feyerabend summarises the argument as,

> [S]cience is an essentially anarchic enterprise: theoretical anarchism is more humanitarian and more likely to encourage progress than its law-and-order alternatives […] this is shown by an examination of historical episodes and by an abstract analysis of the relation between ideas and action. The only principle that does not inhibit progress is: anything goes.[35]

Here Feyerabend sets up his argument as a choice between rationalistic accounts of scientific methodology, which would be a barrier to scientific progress, or an open free-for-all, that should not be constrained by methodological considerations. This is more than just the division that Popper understood between 'discovery' and 'justification'. For Feyerabend, to 'discover' something is to already have justified it as a sensible option. The world already contextualizes the claim allowing it to be entertained as a possibility. A way to accommodate this claim within the standard philosophical accounts, is to say that the 'context of 'discovery' and 'justification' develop historically in parallel'.[36] Feyerabend, however, is saying something slightly more unsettling for the standard account that, in discovering a phenomenon, we spell

[34] Feyerabend, *Free Society*, p.102
[35] Feyerabend, *Against Method*, p.5
[36] Nigel Warburton, *Philosophy: The Basic Readings* (London: Routledge, 1999), p.274

out the grounds for its justification; 'scientific practice does not contain two contexts moving side-by-side' but is a 'complicated mixture of procedures.'[37] It is thought that anyone can discover something, but it is only science that justifies it. However, it is that justification that needs to be over-ruled in order for science to progress. For, the scientific 'fact' that has to be overturned, would also have been justified, else it would not have been a prior 'fact'. If we identify 'justification' with 'methodological prescription', and 'discovery' with 'historical description', as is the received view, Feyerabend believes we are already thinking about the order of influence in the wrong way. It is more apt to think about 'justification' or 'methodological prescription' as what is historically descriptive. That is to say, any totalizing account of justification is a historical description of what counts as methodology at that point. 'Discovery' is where the 'real' methodology lies, in that, scientists are breaking with the traditions and norms of scientific conduct in order to put forward a new theory, or phenomena. Here, is where Feyerabend put forward 'methodological anarchism' as an alternative to this 'received view', which means, scientists, in practice, *could* be doing anything.[38] The fact that it contains the word 'methodology' is a bit misleading. For Feyerabend is not offering a methodology or a meta-methodology, but a way of thinking about science.[39] Do we derive scientific knowledge by following abstract principles divined by Reason, or is it only in the light of historical outcomes that we then judge what is 'scientific', which then can be further distilled into abstract principles. 'Reason', here, is also a part of that historical outcome. For example, once we have the historical move from Newton to Einstein, we can then retrace the moves in terms of the inevitability of our present situation. Feyerabend writes,

> [T]he idea that science can, and should, be run according to fixed and universal laws is both unrealistic and pernicious [and is] detrimental to science for it neglects the complex physical and historical conditions

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[37] Feyerabend, *Against Method*, p.149

[38] The strong reader would put emphasis on the 'anything' as if all discourses were the same. The 'weak' reader emphasises the 'could' in acknowledgement of the historical contingency of how actual research proceeds.

[39] Nola and Sankey put Feyerabend as somewhere between 'Protagorean relativism and dialectical interactionism in which principles and practice inform one another.' Robert Nola and Howard Sankey, 'A Selective Survey of Theories of Scientific Method', in *After Popper, Kuhn and Feyerabend: Recent Issues in Theories of Scientific Method* ed. by Robert Nola and Howard Sankey (Dordrecht: Kluwer Academic Publishers, 2001), p.34

> which influence scientific change [and] makes science less adaptable and more dogmatic.[40]

Some philosophers of science have argued for a middle ground between methodological anarchism and dogmatism, but Feyerabend's point is that, in arguing for one methodological approach over another, by what standards are we making this decision?[41] This problem alludes to what Feyerabend called the 'incommensurability thesis'. The literature has the 'incommensurability theses' of Feyerabend and Kuhn as being different. For Feyerabend, it was a 'worldview' that was shown up as being incommensurable. For Kuhn, it was his attempt to reduce these 'worldviews' to matters of language or psychology that distinguished him from Feyerabend.[42] This, however, is also a product of the 'strong/weak' reading, as one can only derive these conclusions if one understands these as the logical outcomes of the philosophies offered. I will not discuss Feyerabend's 'incommensurability thesis' at any great length, as the same points will be brought to bear in the discussion on Kuhn.[43] Needless to say, Feyerabend developed the idea from Bohr and Einstein. Even though Feyerabend's popularization of 'incommensurability' led to accusations of radicalisation and irrationality about science, it was actually the ideas of scientists he was looking to develop.[44] One result for the person who construes scientific claims through the 'received view', is to have the rationality of science challenged. It would seem unless there are some meta-standards for judging changes in standards, then those changes cannot be construed in a non-relativist way. This then implies that all standards are relative to the historical time under which they are being investigated.

[40] Feyerabend, *Against Method*, p.295

[41] Larry Laudan, *Progress and Its Problems: Towards a Theory of Scientific Growth* (London: University of California Press, 1977), p.141, *Science and Values: The Aims of Science and Their Role in Scientific Debate* (London: University of California Press, 1984), p.20

[42] Feyerabend and Hacking give criticism to Kuhn and a 'strong' account of the 'incommensurability thesis'. A strong reading would allow one to 'produce a theory of incommensurability that would suit linguists and cognitive scientists.' Feyerabend, *Against Method*, p.xii

[43] See pp.174-184 & pp.234-244 for comparisons of Kuhn's incommmensurability.

[44] Einstein uses the term 'incommensurable' in his 1949 autobiographical notes, Albert Einstein, 'Autobiographical Notes', in *Albert Einstein: Philosopher-Scientist* ed. by P. Schilpp (La Salle, Il.: Open Court, 1949a), pp.3-95 (p.23). Einstein also appears to state a form of methodological relativism that Feyerabend and Kuhn developed, 'the external conditions which are set for [the scientist] by the facts of experience do not permit him to let himself be too much restricted, in the construction of his conceptual world, by the adherence to an epistemological system. He therefore must appear to the systematic epistemologist as a type of unscrupulous opportunist.' Albert Einstein, 'Remarks Concerning the Essays Brought Together in this Co-operative Volume', in *Albert Einstein: Philosopher-Scientist* ed. by P. Schilpp (La Salle, Il.: Open Court, 1949b), pp.665-688 (p.683-84)

What disturbed Worrall, and many others, is the implication behind the idea that 'if no principles of evaluation stay fixed, then there is no 'objective viewpoint' from which one can show that progress occurred […] however this is dressed up, it is relativism.'[45]

One of the legacies of Feyerabend's philosophy of science is the proliferation of relativism and irrationalism about science. This has been summed up in Feyerabend's famous slogan 'anything goes'. Hacking notes that this 'aphorism is often taken to be anti-science, a sort of New Age waffle', but in that 'anything', he was not ruling out the multifarious methods of science, only stating that we are not limited to them.[46] Feyerabend himself addresses this accusation. In *Against Method*, he writes a postscript discussing the criticisms of relativism, and in *Farewell to Reason* he devotes the first chapter to analysing different forms of relativism. Feyerabend insists it is out of a desire to understand cultural diversity that we get relativism.[47] The whole of *Farewell to Reason* is aimed at discrediting a certain philosophical notion of 'Reason' and its associated rationality, which claims that by denying rationality in science, we then have to concede scientific progress is irrational. The conclusion being, we must then be thinking about 'rationality' in the wrong way. In *Three Dialogues on Knowledge*, Feyerabend tries to show that 'relativism' is a relation between dogmatic worldviews.[48] In *Science in a Free Society*, he devotes a chapter entitled 'Conversations with Illiterates' to these misinterpretations of his work.[49] Feyerabend does not help himself though, as his style of writing much like his philosophical ideas, are tinged with anarchism. In that same chapter he chastises his critics for not being able to distinguish between playfulness, irony, and argument by reductio ad absurdum. By using 'rationalist' criteria, Feyerabend sought to show that those same rationalists would have to admit that science has developed in a fashion that is either, contrary to their standards or in a manner that they would have to characterise as irrational. So the 'reductio' is that, by the same standards that the rationalists wanted to defend the methodology of science, they would be forced to

[45] John Worrall, 'The Value of a Fixed Methodology', *British Journal for the Philosophy of Science*, 39 (1988), 263-75 (p.274)
[46] Feyerabend, *Against Method*, p.xiii
[47] Paul Feyerabend, *Farewell to Reason* (London: Verso, 1987), p.19
[48] Paul Feyerabend, *Three Dialogues of Knowledge* (Oxford: Blackwell Publishers, 1995b)
[49] Feyerabend, *Free Society*, pp.125-218

drop it as irrational.[50] Farrell points out that Feyerabend's 'reductio ad absurdum' structure was completely missed by those who reviewed *Against Method*. Feyerabend used 'reductio ad absurdum' and 'modus tollens' reasoning, precisely because rationalists have to accept them. In his epistemological anarchism he wanted to demonstrate that one could use rationalist rules without submitting to them.[51] This approach has partly inspired the methodology behind this thesis. Whilst, methodologically, I utilise Heideggerian notions in interpreting Kuhn's work later on, I would say ethically my commitment is much closer to Feyerabend. That is, Feyerabend seemed more committed to using philosophy and scrutinizing how science is interpreted as a means to political and social progress than Kuhn does as a philosopher of science.[52]

4.3 - Against a Universal Neutral Observation Language

One of Feyerabend's main points in *Against Method* was the idea that there is no neutral observation language with which one can judge the claims of a theory. There is no meta-language outside of the context within which a theory is proposed and meaningful. There is no background reality with which one can test a theory, as those background assumptions, which we call 'reality', are as much theory-laden as the theory under question. It was this new relationship between theory and experience that saw Feyerabend depart from the positivist and Popperian philosophy of science. Both Lakatos and Feyerabend suggest that science is not an autonomous form of reasoning, but is inseparable from the larger body of human thought and inquiry. If so, then the questions of truth and falsity, or correct and incorrect understanding are not uniquely empirical. Many meaningful questions cannot be settled empirically, not only in practice but also in principle. For,

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[50] Munévar quoted in Farrell (2003) complained that 'it should be an embarrassment to the profession that many reviews were completely unable to see the structure of this simple reductio'. So the damning reviews by Newton-Smith (1981) and Laudan (1993) were failures to see this almost sarcastic attempt at developing science along rationalist lines. Robert P. Farrell, *Feyerabend and Scientific Values: Tightrope-Walking Rationality* (Dordrecht: Kluwer Academic Publishers, 2003), p.12

[51] Ibid., p.12

[52] It could be argued that Feyerabend's work is Heideggerian. In his final book *Conquest of Abundance* he makes explicit reference to Being, projection and 'being towards the world'. Paul Feyerabend, *Conquest of Abundance: A Tale of Abstraction Versus the Richness of Being* (Chicago: The University of Chicago Press, 1999c), pp.121-124. It is not noted anywhere that Feyerabend ever read Heidegger but he did turn down a meeting with him. Paul Hoyningen-Huene, 'Paul K. Feyerabend: An Obituary',

> [n]o theory ever agrees with all the facts in its domain, yet it is not always the theory that is to blame. Facts are constituted by older ideologies, and a clash between facts and theories may be proof of progress. It is also a first step in our attempt to find principles implicit in familiar observational notions.[53]

A lot of Feyerabend's work here had direct implications for philosophical notions such as 'rationality', 'objectivity', and 'truth', ideals held dear by scientists. The idea that science somehow has a privileged language with which it describes nature is directly challenged by Feyerabend's social and historical locating of science. In *Against Method*, he tries to illustrate this point by way of the 'Copernican Revolution', in which it is argued that not only is what is observed shaped by the ideologies of the time, but what is constituted as rational is also governed by non-scientific factors. Feyerabend argues through his example, that what Galileo was attempting, could indeed be deemed 'irrational' by the standards of the day and 'in part, even now'.[54] The incongruent experience of a moving Earth, a lack of empirical evidence (or the evidence that could also be explained by other geo-centric models), and the theoretical inadequacy of the Copernican model all indicate that Galileo was not acting out of rational consideration of the evidence at hand. On the counterintuitive flip-side, it is argued that the Church's position was closer to what we would recognise as being scientific, by today's standards, and was also the more responsible position of the two viewpoints.[55] The point Feyerabend is making here is that 'rationality' cannot be deemed as part of the scientific method, as what we consider rational is only subject to the standards and assumptions of its location in history and culture.

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p.7. Arguably it was Feyerbend's involvement with Nazism that led to his pluralist and anti-dogmatic views on science. Oberheim, *Feyerabend's Philosophy*, p.18

[53] Feyerabend, *Against Method*, pp.5-6

[54] Ibid., p125

[55] 'The Church was not ready to change just because somebody had produced some vague guesses. It wanted *proof* – scientific proof in scientific matters. Here it acted no differently from modern scientific institutions: universities, schools and even research institutes in various countries wait a long time before they incorporate new ideas into their curricula. But there was as yet no convincing proof of the Copernican doctrine. Hence Galileo was advised to teach the Copernican theory *as a hypothesis*; he was forbidden to teach it as a *truth*.' Ibid., p.132 [Italics in original]

Feyerabend's example of the 'Copernican Revolution' was also good ground for showing where other systematic attempts at a demarcation criterion had also gone wrong. Feyerabend became a vocal critic of Popperian ideas and saw Popper's attempt to codify the activities of science into scientific and non-scientific statements, by way of falsification, as another ruse to straitjacket inquiry. Feyerabend in-keeping with his anarchistic ethic argued that,

> [t]here are circumstances when it is advisable to introduce, elaborate, and defend ad hoc hypotheses, or hypotheses which contradict well-established and generally accepted experimental results, or hypotheses whose content is smaller than the content of the existing and empirically adequate alternative, or self inconsistent hypotheses, and so on.[56]

As there was no empirical reason to favour the Copernican over the Ptolemaic system, one could only evaluate these cosmologies on non-empirical grounds, such as metaphysical implications, aesthetics of the mechanics, or maths involved, simplicity of explanation, and so on.[57] Feyerabend accuses Galileo of going against near all of Popper's criteria for what we should consider or defend science to be. As the rotation of the stars is relative to a frame of reference on Earth and vice-versa, any attempt at establishing which body is in motion with limited data, technology and certain background assumptions, one could easily conclude that the motion of the Earth had been 'falsified' by observation. All that is claimed is that science cannot be reduced to a single principle such as falsification, but must also recognise that it is part of a wider picture in which 'non-scientific' elements also have to be considered.

What has been discussed so far is a brief outline of some of Feyerabend's key philosophical points. A methodological response to a historical question is what Feyerabend generated in his critique of science, rather than a realisation that the historical approach called into question the methodological conception of scientific practice. Some of this misunderstanding has to be levelled at Feyerabend's writing

[56] Ibid., pp.14-15

style, which also raises another issue. From the 'received view' it would seem that the inability or refusal to write a clear positive account of how things are is to concede that one cannot state their position in clear terms. That one resorts to hiding behind language and literary devices instead of being 'up front'. This attitude also belies a deeper problem about how one understands how arguments can work from metaphor or other such literary devices, which deal with 'truth', but do not claim it. What I will also attempt to show in chapters 7 and 8 is how this order of 'literal' and 'figurative/ metaphorical' can be inverted. Next I will give a 'strong' analysis of Feyerabend in line with those responses that saw Feyerabend as 'anti-science'.[58] This is to prime the reader as to what can be expected in the chapters on Kuhn, who in many ways suffered the same fate.

4.4 - Against Feyerabend

In the following analysis of Feyerabend's work, I am writing with a 'literal' and uncharitable interpretation in mind.[59] It will be philosophically naïve, in that, it will ignore the implications of its own position. This I call the 'strong' reading, as it has such a strong commitment to the metaphysical position it starts from, that it fails to acknowledge it. Someone who is philosophically naïve might take the term 'truth' to relate to some form of representationalism or correspondence between a statement about the world and the world itself. The strong reader starts here. Yet 'correspondence' already presupposes a metaphysical relationship between language, knowledge and truth. The challenge is to be able to separate methodological and historical notions of 'Truth', but the abstract reality with which the PUoS encourages

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[57] 'Modern comparisons of Copernican and Ptolemaic predictions 'with the facts', i.e. with 19th and 20th century calculations, show, furthermore, that empirical predictions were not improved and actually became worse when competing systems are restricted to the same number of parameters.' Ibid., p.145

[58] Feyerabend was a keen defender of science as his philosophical work was an attempt to expose the weaknesses in assuming a wholly empirical or rational view of science. This, however, did not stop the epitaph of 'anti-science philosopher' being placed against his name in the obituary column of the New York Times. Wolfgang Saxon, "Paul K. Feyerabend, 70, Anti-Science Philosopher", *New York Times*, 8 March 1994 <http://wwwnytimes.com/1994/03/08/us/paul-k-feyreabend-70-anti-science-philosopher.html> [accessed 13 June 2011] – if nothing else Feyerebend's life long dependency (from the age of 21) on painkillers show his commitment to the veracity of modern medicine.

[59] In the preface to the 1st edition of *Against Method* Feyerabend tells us he will from time to time be writing in 'a rather ironical vein'. The preface to the 3rd edition acknowledges that he is using 'paradoxical formulations' and so a 'literal' reading of the summary of his argument may not be the correct approach. Feyerabend, *Against Method*, p.xvii & p.xxviii

us to think about science means we always tend to think about 'truth' as purely methodological. Next I will present this 'strong' analysis.

Against Method, as well as much of Feyerabend's other writings, contain numerous ambiguous or confused statements, which often either end in violent attacks on science, or have deep running implications for the enterprise of science as a whole.[60] Some of the confusion in Feyerabend is that his vision is simultaneously philosophical, historical, and political, so that judgements of facts get mixed up with judgements of value. When Feyerabend says, 'the idea that science can, and should, be run according to fixed and universal rules, is both unrealistic and pernicious' he is criticizing at length the 'fixed and universal rules' through which earlier philosophers thought that they could express the essence of the scientific method.[61] While there may be no such thing as the 'scientific method', this does not necessitate the non-existence or the impossibility of developing of certain rules, heuristics and procedures, with an approximate degree of validity, on the basis of previous experiences. If Feyerabend's aim in *Against Method* is to illustrate through historical examples, the limitations of any general and universal codification of the scientific method, one could reasonably say he had achieved his aim. However, one could argue he goes much further and, depending on which edition of *Against Method* you read, a different Feyerabend presents himself. Here the inconsistency of authorial position may indicate a weakness of commitment in his argument.

Possibly one of the most infamous and quoted lines in *Against Method* is, 'all methodologies have their limitations and the only 'rule' that survives is 'anything goes.'[62] Depending on how we read this statement, it is either trivially true or just false. 'All methodologies have their limitations' is not a revelation to any scientist, but then what is implied is that *any* method is as good as *any* other, which is blatantly false unless qualified by the statement 'as long as it produces true statements'. There is not one way to drive a car but there are better, more suitable approaches if one wishes not to crash. All methods of driving, as with investigating nature, are not

[60] Larry Laudan, *Beyond Positivism and Relativism: Theory, Method and Evidence* (Boulder: Westview Press, 1996), p.107
[61] Feyerabend, *Against Method*, p.295
[62] Ibid., p.5

equally reliable. In the second edition of *Against Method*, it appears that Feyerabend tries to defend a literal interpretation of the 'anything goes' approach,

> A naïve anarchist says (a) that both absolute rules and context-dependent rules have their limits and infers (b) that all rules and standards are worthless and should be given up. Most reviewers regard me as a naïve anarchist in this sense […][But] while I agree with (a) I do not agree with (b). I argue that all rules have their limits and that there is no comprehensive 'rationality', I do not argue that we should proceed without rules and standards.[63]

Feyerabend only suggests what these 'rules and standards' might be; they should not be a set of general rules or standards, but should be contextual and are to supplement those absolute rules, but do not replace them. A problem here is that Feyerabend does not commit himself to what these 'rules and standards' are and so, unless they are constrained by some notion of rationality or truth correspondence, one arrives quite easily at a type of ultra relativism. It is also very easy to make criticisms when one cannot offer an alternative, and to some this appears to be the bread and butter of philosophers of science.[64] Laudan expresses this worry about relativism, when he reads Feyerabend and Kuhn as propounding a view of the irrationality of science when it comes to deciding between fundamental competing theories. Laudan couples the Feyerabendian/ Kuhnian irrationality of science with the relativism of cultural theory, to arrive at a picture that puts scientific claims equal to those of magicians or shamans.[65] However, when Feyerabend comes down from the level of abstraction and does address concrete problems in the history of science, he frequently mixes reasonable observations with strange sounding suggestions,

> The first step in our criticism of customary concepts and customary reactions is to step outside the circle and either invent a new

[63] Ibid., p.231

[64] Professor of mathematics and physics Mark Perakh says the value of philosophy of science is in its ability to entertain. 'I doubt […] the philosophy of science have ever produced even a minute amount of anything that could be helpful for a scientist, be he/she physicist, biologist, geologist'. Mark Perakh, "Philosopher Ruse as an Entertainer", *Talk Reason,* 24 Dec. 2010 <http://www.talkreason.org/articles/ruse.cfm> [accessed 16 Feb 2013]

[65] Laudan, *Progress and its Problems*, p.3

> conceptual system, for example a new theory, that clashes with the most carefully established observational results and confounds the most plausible theoretical principles, or to import such a system from outside science, from religion, from mythology, from the ideas of incompetents, or the ramblings of madmen.[66]

One could defend these assertions by invoking the classical distinction between the contexts of 'discovery' and 'justification'. As for Popper, the process of inventing theories or discovering phenomena has no codification, all methods and means are acceptable.[67] However, once we have our theory or idea, its justification has to be rational, even if what counts as rational cannot be wholly codified. As with Feyerabend's example of Galileo, where it is argued that his actions could be deemed wholly irrational, how did we get to a stage where Galileo's arguments won out against the Church? Surely, not because he was better at rhetoric or the greater showman? One factor that Feyerabend seems to overlook is that outside of the social and philosophical aspects of the Galilean controversy, he and his antagonists shared a lot of ideas in common. Both sides agreed that you could describe planetary motion and from these descriptions you could deduce certain observable facts. Ptolemy, Brahe, and Copernicus along with many other astronomers, believed you could map out the heavens. Galileo pointed out trivial facts that his opponents had no option but to admit true, such as lamps seem larger than they really are when viewed from a distance at night, and that Venus appears smaller in the light of day than at night. It was from simple agreed facts, which followed from rational argument that telescopic data was allowed to be submitted as evidence, and began to turn the tide in favour of Galileo. The change is gradual, slow and messy – to borrow a Quinean idea, each claim in the web of science is revisable in the light of new evidence, be it the methodology, standards of conduct, theories or facts. As each part is modified it can then be judged against the remaining parts of the web to see if the case holds water. What does not happen is that the entire web is changed at once. For those same standards of rationality, science, theories and facts only make sense in relation to one

[66] Feyerabend, *Against Method*, pp.52-3

[67] Apocryphally August Kekulé was inspired to the molecular shape of benzene after a dream he had, whilst a huge array of other scientific discoveries come about by mere chance or accident. For a list of accidental discoveries just in medicine including Kekulé see, Roberts M. Royston, *Serendipity: Accidental Discoveries in Science* (New York: Wiley, 1989)

another, not in their absence. It would be impossible to form an objective consensus on what works, have a sense of progression, or knowledge acquisition, if we had to replace the entire onto-epistemological network each time. Feyerabend, however, seems to deny this distinction between the context of 'discovery' and 'justification' by suggesting that division is not as clear as has been made out in traditional epistemology.[68] The rationalist wants to say that the history of science, context of discovery and justification, is not piecemeal, but evolutionary. Growing evidence and research affects how many prior theories could or could not be justified. With this said, this distinction has always been there, for if it were not, the justification of theories would be unconstrained and here we would have an 'anything goes' situation. All of Feyerabend's use of historical data would appear self-authenticating, when we consider the reasons for its use. Feyerabend wants to say that, the history of science (HOS) trumps the validity of philosophy of science (POS). Yet, as the HOS entails looking at the POS (here the POS is an explication of the notions of philosophical/ historical ideas such as rationality, truth, objectivity and so on), how can we write a HOS that does not implicitly entail that POS, which the HOS is supposed to be critiquing? That is to say, if on the HOS account POS is discredited, what is the point of looking at past scientific texts, that would have been written and organized by those same discredited philosophical concepts? How exactly is the historian supposed to test his model if the 'historical data' is composed under an opposing POS? As Newton-Smith says, 'an attack on a particular aspect of method presupposes method.'[69]

In the introduction to the Chinese 3rd edition of *Against Method*, Feyerabend lays out the argument against appeals to 'non-scientific' behaviour in the establishing of knowledge or 'success'. He says, ''non-scientific' procedures cannot be pushed aside by argument', the use of the term 'non-scientific' assumes the existence of something that is scientific, which appears contrary to Feyerabend's position.[70] He argues against the assertion 'science is always successful', which on a literal reading no one would agree with, and so is a strawman figure to argue against and, as he observes,

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[68] W. H. Newton-Smith, *The Rationality of Science* (Boston: Routledge & Kegan Paul, 1981), p126
[69] Ibid., p.134
[70] Feyerabend, *Against Method*, p.2

‘there are lots of failures’.[71] Science does, however, count its failures and so in establishing that Newtonian physics does not adequately explain the world of the very small or the very fast we have, in a certain sense, a success. We ‘know’ something, be it a weakness in a theory or underdetermination in the evidence. The same cannot be said of a failure in ‘non-scientific’ procedure, the prediction of countless Armageddon’s for the devoted ‘non-scientist’ does not show a weakness in the process that led to failure in prediction. The idea that the world, one day, will end, is compatible with many observations, myths, and stories – and its failure to be realized in any actual way, can be saved by ad-hoc interpretation. Even though ad-hoc arguments have been used in the advancement of scientific ideas, overtime, further observation, evidence and theory remove the ad-hoc component, whereas in non-scientific ideas they remain stagnant.

Feyerabend, in the same introduction, makes the trivial observation that scientists do not know ahead of time what will make it to the level of knowledge. But from this states of affairs, he then generalizes, ‘there exists no special way of weight scientific promises either – scientists are no better off than anybody else in these matters, they only know more details.’[72] Again, on a literal reading, of course you cannot guarantee the success of an idea, theory or prediction ahead of time, but you can have levels of confidence in an idea, some things, given certain assumptions, are more likely than others. It is these details that are crucial for knowing why we prefer theory *a* over theory *b*. Scientists are not better than anybody else in these matters, with regard to the power to see into the future (apart from those non-scientists who claim to be able to see into the future), but their methodology does give them the advantage. Louis Pasteur was accused of being lucky in many of his discoveries, to which he replied, ‘[I]n the field of observation, chance favours only the prepared mind’, and as Wolpert remarks, ‘it is not by chance that it is great scientists who have the luck.’[73] The rationalist will think there are very good reasons why Pasteur made the discoveries he did and not a blacksmith or a baker, or indeed why it took two thousand years for Aristotle’s simplified theory of motion to be challenged. It was not questioned by a

[71] Ibid.,
[72] Ibid.,
[73] Morton A. Meyers, *Happy Accidents: Serendipity in Modern Medical Breakthroughs* (New York: Arcade Publishing, 2007), p.7; Lewis Wolpert, *The Unnatural Nature of Science* (London: Faber & Faber Ltd, 1993), p.81

farmer or soldier, people equally exposed to falling objects, but by Galileo. Galileo was looking at the same phenomena everyone else saw, but it took him, with his approach to investigating nature, to question a two-millennium old assumption.

The presenting of science as equivalent with non-science, because both fail at some level, seems to be trying to lay the foundations for the ‘anything goes’ ethic. Once Feyerabend established his ‘anything goes’ idea he frequently draws comparisons between science and religious myth. ‘Newton reigned for more than 150 years, Einstein briefly introduced a more liberal point of view only to be succeeded by the Copenhagen Interpretation. The similarities between science and myth are indeed astonishing.’[74] It is true, to some extent, that the ‘Copenhagen Interpretation’ of quantum mechanics was accepted dogmatically by fellow physicists, after Niels Bohr and Werner Heisenberg showed that the very act of observing or measuring a quantum event changes the ‘reality’ of that event. However, if Feyerabend wants to draw analogies between myth and science, he should give examples of where myths have altered due to their incongruity with observational findings, or ‘suggest how experiments aimed at discriminating between earlier and later versions of the myth’, helped societies to evolve.[75] If there are similarities between science and myth, it is at the superficial level of how language is used. We see the same techniques at play in the New-Age mysticism writings on the links between spirituality and quantum physics.[76]

The interpretation that I have presented above is what I have called the ‘strong’ reading. It is a particularly uncharitable or ‘literal’ reading of a philosopher, who in this case was Feyerabend. A more nuanced reading of Feyerabend saw him turn away from the positivist conceptions of truth and knowledge, and in its place offer a more historically informed diagnosis of those same problems offered up by the positivists/empiricists. Yet his ‘reductio ad absurdum’ structured argument in *Against Method*, his playfulness with language, and his affiliation with Wittgenstein makes it easy to lump him in with those ‘Continental’ philosophers of science who would undermine the authority of science for making truth-claims. At its core, what I have

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[74] Ibid., p.298
[75] Sokal, *Beyond the Hoax*, p.201
[76] Jeremy Bernstein, *Science Observed: Essays Out of My Mind* (New York: Basic Books, 1982), p.335

identified, is the confusion between treating a historical question methodologically and a methodological question historically. The 'strong' reading only submits to ideas, philosophical or otherwise, as being treated methodologically and any deviation is an attack on this approach. The 'weak' reading, which I only hint at in his biographical sketch, highlights both possibilities and presents philosophy with a greater role to play in the dialogue between science, education and philosophy.

4.5 – Conclusion

In conclusion to the first section of this thesis, what I have attempted to argue is that the PUoS, which is mainly written, organized and delivered by scientists, has a distinct lack of philosophical engagement with the ideas being used. What is more, many of the publicly outspoken scientists, who are highly credible in their own fields, have dismissed philosophy or ridiculed it in some way. I can restate no better formulation of this position than the comments in the House of Lords *Science and Technology Sixth Report*: 'many conventional medical scientists, while accepting the validity of accumulative empirical observation, believe that those therapeutic disciplines that are based principally on abstract philosophy and not on scientific reasoning and experiment have little place in medicine.'[77] Here we have a clear binary choice between practices based on 'accumulative empirical observation' deemed as scientific, and practices based on 'abstract philosophy', which are not. It is not even considered that the position of empiricism is itself an 'abstract' philosophy, that is supplemented with more than just scientific reasoning. What I have argued for in this first section is that science tends to be known via its representation as a method, which allows us to collect facts about the world. What the sociology of scientific knowledge and PUoS studies have shown is that, knowledge of 'facts' is itself not a great measure of scientific understanding. The explicit assumption in the 'House of Lords' reports on science, technology and society is that with greater knowledge of science, the more in favour of science people will be, which will spill over into their personal lives, allowing them to make better decisions. However, social research has shown that the opposite is happening. The more people know

[77] House of Lords, *Science and Technology*, para. 2.17

about science the less they are likely to equate it with social progress.[78] With the mechanisation of industry, jobs and skills have been lost. With the proliferation of nuclear energy, the threat of total war and environmental disasters has increased. With the over reliance on antibiotics and drug therapy we have seen the creation of stronger, more deadly viruses, and the doping of the human condition. The defence here is that this is not what science *is* but how science is *used*.[79] All of which then draws us closer to our central question of a PUoS, 'what is science?'[80]

It is thinking that science is other than what people do, which keeps it anchored as a methodology. Here we work from the abstraction of methodology to real world cases. My main route to exposing this tradition in thinking about science has been through the failed attempts to demarcate science from non-science. Here, I have looked at Popper's use of falsification as a principle of demarcation. I argued that Popper's attempt to give philosophical validity to a methodological conception of science, as well as his personal contributions to science, has him elevated above most philosophers of science, as the 'scientists' philosopher'. Yet as we see for a number of reasons, 'falsification' fails and can be treated, at best, as a heuristic of scientific practice. 'Falsification' can then be thought of alongside other heuristics, such as, simplicity, beauty, consilience, explanatory power and so on. What this and other failures to demarcate science from non-science on a methodological basis should tell us is, not that there is no difference between religion, myth and science, but that we are thinking about the problem in the wrong way. One could reasonably conclude that if we cannot demarcate science from non-science on a methodological basis, which is part of the rhetoric of the 'anti-science' movements, then maybe there is no difference between magic and physics.[81] If the public is not philosophically 'literate', then these

[78] Alain Touraine, 'The Crisis of Progress', in *Resistance to New Technology: Nuclear Power, Information Technology, and Biotechnology* ed. by Martin Bauer, (Cambridge: Cambridge University Press, 1995), pp. 45–56; Geoff Evans and John Durant, 'The Relationship Between Knowledge and Attitudes in the Public Understanding of Science in Britain', *Public Understanding of Science*, 4 (1995), 57-74 (p.57)

[79] Also called the 'neutrality argument'. Ziauddin Sardan, *Thomas Kuhn and the Science Wars* (Cambridge: Icon Books, 2000), p.14

[80] Feyerabend's critique of science is cashed out, he claims, by there being no single governmental policy on science. 'Government agencies no longer finance 'science', they finance particular projects'. Feyerabend, *Against Method*, p.xii

[81] As the social, political and philosophical are almost inseparable it only takes a charismatic leader to espouse strong philosophical objections to mainstream accounts of 'how things are' to win over rational people. If people are not trained or skilled in philosophical thinking it can be very hard to recognise bias, assumption or spurious metaphysics in alternative positions. For example the 'Tammy

alternatives can be easily entertained as genuine possibilities. It is from such a position that I think ‘anti-science’ ideologies can take hold, regardless of their political motives. This, however, is not a recommendation to replace science with philosophy. For even philosophy suffers from the above criticism, when it seeks the epistemological clout of science in an area such as ‘rationality’ or ‘consciousness’. This was one of Feyerabend’s targets, but such projects still persist today in philosophy and science.[82]

In contrast to Popper, I offered Feyerabend, the protégé of Popper as someone who trained in science, was closely associated with empiricism as a philosophical position and understood the Popperian criticisms of logical positivism. Feyerabend though represents the alternative approach to philosophy of science, by thinking about it not as a question of methodology, but as a historical process. This approach requires adjusting our philosophical lenses and seeing exactly what is abstract in thinking about science as a methodology in the first place. As this approach questions and critiques the ‘science as method’ conception of science, which is also a direct challenge to its philosophical basis as a particular way of finding out facts, it tends to provoke a hostile defence. My use of Feyerabend was to illustrate a number of things, how a historical approach gets us to reevaluate ideas such as ‘Truth’, ‘Reason’ and ‘Objectivity’. That a methodological response to a historical question produces confused interpretations, and that regardless of what someone writes we are free to interpret in a number of ways, that will also highlight our own philosophical biases. I also use Feyerabend as he stands as useful bridge between the ‘analytic’ and ‘Continental’ traditions. For a simple distinction, we could consider the analytic tradition being dominated by the ‘spirit’ – an abstract, metaphysical project to know the underlying principles of Reason, Knowledge and Truth. On the other hand, the ‘Continental’ tradition is dominated by ‘life’, starting from people and our

Kitzmiller et al. v. Dover Area School District case,’ in which the scientific credibility of Intelligent Design (ID) was being argued. I believe this was mainly argued on a methodological premise – is the supernatural hypothesis testable? Do organisms show irreducible complexity? The lack of peer review in ID. “Decision of the Court,” Kitzmiller v. Dover Area School District, *The Talk Origins Archive*, 31 Dec 2005, Sect. E4. <http://www.talkorgins.org/faqs/dover/kitzmiller_v_dover_decision.html> [Accessed 12 Dec 2012]

[82] The search for replicating human intelligence in the Artificial Intelligence (AI) project is one such example that has hit many problems at the scientific and philosophical levels. An overview of these problems within AI cognitive science is given in Mark H. Bickhard and Loren Terveen, *Foundational*

experiences of the world who are engaged in practices that maintain rationality, knowledge and truth. The critique I give of Feyerabend in the last section with an unsophisticated, literal, uncharitable reading of Feyerabend I called the 'strong' reading. With the strong reading the individual will not only miss the most important points of an argument, but that it will also downgrade philosophy as a mode of inquiry. If we come away from Feyerabend thinking he is a relativist, irrationalist, anti-scientific radical, then we may assume that it takes a philosopher and not a scientist to reach such conclusions about science.

To show the direction I wish to proceed in, there are two bibliographical events that are instructive. The first is that in the 'Library of Living Philosophers' series, a notable absentee in the large two-volume tome for Karl Popper is one of his main critics, Feyerabend. The reasons for his absence are not clear, but one can assume he was either not asked or he did not want to contribute. Considering his vocal criticism of Popper and his professional association with most of the contributors such as, Popper, Lakatos, Kraft, Feigl, Maxwell, Aggassi, and Kuhn, it would seem strange that he was not asked. However, what I find more interesting is the reason for the book series, as the name suggests and is noted in the introduction. It is to ask philosophers direct questions while they are still alive to avoid 'interminable controversies'.[83] Schilpp, paraphrasing Schiller, in response to the idea that one has to necessarily interpret a work of philosophy, says,

> If, in desperation, he decides that all of the interpreters are probably wrong and the only thing for him to do is go back to the original writings of the philosopher himself and then make his own decision – uninfluenced (as if that were possible) by the interpretation of anyone else – the result is not that he has actually come to the meaning of the original philosopher himself, but rather that he has set up one more interpretation, which may differ to a greater or lesser degree from the

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Issues in Artificial Intelligence and Cognitive Science: Impasse and Solution (Amsterdam: North-Holland, 1995)

[83] Paul A. Schilpp, 'Introduction', in *The Philosophy of Karl Popper*, part 1 vol. 14, ed. by Paul A. Schilpp (La Salle, Il.: Open Court, 1974), p.vii

interpretations already existing. It is clear that in this direction lies chaos.[84]

I quote this passage in full because it illustrates a number of points I wish to develop. There is an irony in that this quote should be placed in a large work dedicated to Popper who had developed the idea that no observation, like the reading of a text, is 'pure'. We come to phenomena with it already interpreted for us. Feyerabend and Kuhn were developing this idea, but it was also an a priori metaphysics that allows us to think that if each of us comes away with our own interpretation, what is to stop us descending into a chaos of competing claims? The answer lies in history and culture. Only certain interpretations are possible for each person, or at least meaningfully possible. What is more, the aim of the 'Living Philosophers' series fails, as can be seen from the controversy that raged when Popper (as with most philosophers) was still alive. It is not in the overcoming of interpretation, as we cannot help but interpret, but in the knowing why and how one has arrived at their interpretation and the resulting problems that come forth from it. Hence, merely asking Popper what he 'meant' is irrelevant if one is already pre-disposed to think and act a certain way. If this were not the case there would be no controversies, we would simply explain our positions and agree with whoever was correct.

The second bibliographic event, and moving the discussion away from Feyerabend and on to Kuhn, is the published work coming from the 1965 'International Colloquium in the Philosophy of Science'. This later became a 4 volume work, out of which came *Criticism and the Growth of Knowledge*. Popper chaired the debate and the importance of which is signaled by Thomas Kuhn stating in the first paper of the

[84] Ibid., p.viii. Also so see Schiller's review of and reply to Morris (leading American advocate of the 'Unity of Science Movement' and close associate of the logical positivists. It was from this movement that Kuhn was asked to contribute to *the International Encyclopaedia of Unified Sciences* where he published an early draft of *Structure*) over his article 'Must Philosophers Disagree?' The conflict is typical of the time, Morris defending a strict logical interpretation of language and meaning, and Schiller finding fault with doctrine founded on logic. Pre-empting much of Feyerabend and Kuhn's work, Schiller says that 'what is truly deplorable, and has been so since Plato's day is the preposterous interpretation philosophers have sought to put upon a legitimate scientific procedure […] Dewey's greatest discovery, and the core of his specific doctrine, is surely that of the need for constant reconstruction of beliefs. It is for this reason that science is progressive, and no truth is absolute. But how is this insight compatible with the old ideal of pinning down for all eternity the meaning of every idea by an exact "analysis"?' F .C. S. Schiller, '"Must Pragmatists Disagree?" Reply to Morris's Review', in *The Chicago School of Pragmatism Vol. 4,* ed. by John R. Shook (Bristol: Thoemmes Press, 2000), pp.51-56 (p.55)

book that he will compare his views on scientific development as laid out in *Structure* with those of Popper.[85] The other two keynote speakers of Lakatos and Feyerabend had to re-write their responses to Kuhn, and all other contributions are re-addressed to Kuhn, and not Popper.[86] We may also see a failing in the 'Living Philosophers' series ethos of simply asking a philosopher 'what he means', for even Feyerabend reads Kuhn as propounding a 'theory of science' of a sociological or psychological nature.[87] That is, he is treating, what I think is actually a historical argument methodologically. These two events highlight what is most important for me, that language, belief and phenomena come already interpreted, that is, already meaningful and what interpretation we come to, is open to philosophical analysis. The beliefs and ways-of-thinking that we carry with us will shape what interpretation we form and the nature of the problems that will follow from them. Merely asking someone what they mean in everyday discourse is usually sufficient to find out what they meant, but when the discourse is challenging the foundations upon which common acceptance of language-meaning transference, truth or rationality are based, it would seem just asking is not enough. What is more, the very way we hold these ideas, the general direction of influence they have on us and what we take to be primary in eliciting descriptions of 'reality' will also determine how we understand those ideas.

Probably the most important and widely read text in the philosophy of science is Thomas Kuhn's *The Structure of Scientific Revolutions*.[88] What I allude to in my treatment of Popper and Feyerabend, I will develop further with Kuhn as the central figure. It will be argued that Kuhn can be read in at least two contradictory ways. Two polar versions can be constructed; one version is 'strong', which leads to bizarre conclusions, and devalues philosophical participation in the arena of understanding science. It is this version of philosophy I believe scientists and some philosophers of

[85] Thomas Kuhn, 'The Logic of Discovery or the Psychology of Research', in *Criticism and Growth of Knowledge*, ed. by Imre Lakatos and Alan Musgrave (Cambridge: Cambridge University Press, 1999a), pp.1 - 24 (p.1)
[86] This idea of interpreting events can also be seen in why that whole Colloquium happened. Sardar writes, 'the purpose of the Colloquium was to bring Kuhn up against the combined might and criticism of the British philosophers.' Sardar, *Thomas Kuhn and the Science Wars*, p.35. Whereas Fuller states that the Colloquium was 'officially devoted to the implication of Karl Popper's philosophy of science.' Steve Fuller, *Thomas Kuhn: A Philosophical History For Our Times* (Chicago: The University of Chicago Press, 2000), p.288
[87] Feyerabend, 'Consilations for the Specialist', p.197-198

science debate over when they omit philosophy from public forums in discussing science. The other version is 'weak', which requires a certain level of philosophical 'literacy', but gives philosophy a genuine role as a monitor of scientific and philosophical discourse. In the case of Kuhn, mainstream thinking about science and philosophy, I will argue, it is the 'strong' version that has taken root.[89] What is more, as strong versions will be parasitic on one another, the 'other' that stands in opposition to 'strong' accounts of science and philosophy of science, are given more credibility by our overlooking the weak interpretation. For example, relativist notions of truth can only be sustained if one works off a metaphysics that sustains absolutist notions of truth. One can only get a form of idealism present in 'alternative' therapies, if one is willing to entertaining the hard realism of absolutist scientific explanation. All of which is just a confusion of philosophy as 'realism' cannot be proved nor be demonstrated, it is just a position one holds as a framework for how subject, object, mind and reality all hang together. This confusion, I argue, leads to greater public misunderstanding over scientific matters. In addition to using Kuhn, I also make use of ideas inspired by Martin Heidegger in formulating my 'strong – weak' distinctions. Again, I will not be asking what Heidegger *really* meant by his philosophy, just how they can be used to clarify or possibly resolve certain problems. Heidegger, like Feyerabend, is too easy to misunderstand as being anti-science, irrational or relativist. But like Feyerabend's counter-inductivism, if the conclusion is too easy to come too then maybe there is a problem with our thinking. Whether one agrees or disagrees with my 'strong-weak' readings of Kuhn, or opposes my use of Heidegger, one can only really do so on philosophical grounds.

To conclude this section, the direction and aim of the remainder of this thesis can be summed up by Feyerabend's character *B* in *Three Dialogues of Knowledge*. In response to the incongruity between the ideal cases philosophy deals with and the messiness of reality, character *A* asks,

[88] Feyerabend also read *Structure* in draft form and conversed with Kuhn. He is the first to be named in the acknowledgments section of that book, his comments having been the 'most far-reaching and decisive'. Kuhn, *Structure,* p.xiv
[89] I believe it is this Feyerabend is alluding to when he says, 'it [Structure] also encouraged lots of trash.' Feyerabend, *Against Method,* p. ix

What becomes of the philosophy of science if we assume such an attitude?

B: It withers away and is replaced by history and a philosophically sophisticated science that can take care of itself. Unfortunately the situation today is very different, though there are signs for hope here and there. What we have is a philosophically unsophisticated science that wants to take over the place of religion and theology had before, a scientifically unsophisticated philosophy that praises it and is praised by the scientists in turn, a cowardly religion that has ceased to be a world view.[90]

[90] Feyerabend, *Three Dialogues of Knowledge*, pp. 87-88

Chapter Five

The Role of Philosophy

5.1 - The Effects of Philosophical 'Literacy' or Different Worldviews?

The PUoS is thinking about science. It does not require the public to be scientists, nor perform scientific duties. I argue that it is in this thinking 'about' science that philosophy can play a role. 'Philosophy', however, is fighting an up-hill battle. From the failure to demarcate science from non-science, to the rise in anti-scientific thinking challenging ideas such as 'truth' or 'reason', all of which can only re-enforce the subject's invalidity.[1] In the previous chapter, we saw how the more radical sounding ideas of Feyerabend have concerned scientists enough to write on the subject. Here, as a response to the 'Science Wars' and the proliferation of relativism left in its wake, we have scientists reminding the general public of the irrelevance of philosophy to science. Kitcher remarks how books such as *The Unnatural Nature of Science* or *Higher Superstition: The Academic Left and its Quarrel with Science* 'make it plain that distinguished scientists find large portions of the work done in the name of science studies ignorant, confused and damaging.'[2] Sokal has accused the academic left of using the tactics of postmodernism within science studies that devalue terms like 'truth', 'objectivity' or 'rationality', so that any meaningful critique of society becomes impossible.[3] Ever since Quine in *Two Dogmas* delivered a hefty blow to the rationalist project for how language and the world relate, it as been but a short step to the types of meaning holism and ontological relativity that signify 'postmodernist' thinking on science.[4] Here there has been a kind of implicit assumption amongst those defenders of science in the 'Science Wars' exchange. That science treated under the Anglo-American traditions of logical positivism and

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[1] The conservative right of academia tends to view such statements with what has been described by Dennett as a 'deepity'. This is 'a claim that appears profound but is in fact a superficial equivocation.' Dan Lloyd, 'Popping the Thought Balloon', in *Dennett's Philosophy: A Comprehensive Assessment*, ed. by Don Ross, Andrew Brook and David Thompson (Cambridge, Mass.: MIT Press, 2000), pp.169-218 (p.171)

[2] Philip Kitcher, 'A Plea for Science Studies', in *A House Built on Sand: Exposing Postmodern Myths About Science*, ed. by Noretta Koertge (Oxford: Oxford University Press, 1998), pp.32-56 (p.32)

[3] Alan Sokal, *Beyond the Hoax: Science Philosophy and Culture* (Oxford: Oxford University Press, 2008), p.107

empiricism are useful adjuncts to science proper, whereas, those positions on science, truth and objectivity, emanating from the 'continent' are pernicious and ultimately useless as a means to progress, if all truth claims are relative. Whilst the case against 'relativitsm' seems a fair one, the attributing of its source in 'Continental' rather than Anglo-American pragmatist traditions seems less deserving.[5]

The sort of philosophical practice that has been regarded as the 'flagship' for postmodernist 'sloppy thinking' is 'deconstructivism'. Putnam describes deconstructive analysis as the 'move from relativism to nihilism.'[6] His problem is that 'instead of offering a formula which is supposed to tell us what truth is, deconstructionists announce that the notion of truth is incoherent, part of "metaphysics of presence"'.[7] This quote is a reference to Derrida who, for Putnam, can only offer metaphysical discontent as evidence for 'Truth's' incoherence.[8] It is a move, however, to go from metaphysical discontent with a specific notion of 'Truth', to saying we cannot make true statements. Derrida was not stupid and could see the incoherence of saying nothing is true, in the sense that I should deny the existence of events, objects or people, but the sort of evidence Putnam requires is not the sort that can be given in a formula. Hence, Derrida offers rigorous deconstructive arguments to try and undermine the metaphysical assumptions that lead to thinking of 'Truth' as an ostensive definition, or even its negation.[9] This sort of misunderstanding is not uncommon in academia, so there would appear to be even greater jeopardy with regard to public understanding, where 'philosophy' is relegated to coffee-house talk. For Ayer, clear thinking was part of 'good prose' and this is to be contrasted with 'the sort of nonsense we get from Germany and now also from France.'[10] Whilst a certain conception of philosophy aligns itself with the sciences, it, like science, has become a victim of its own success. Where science has the pseudo-sciences, philosophy has

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4 Christopher Norris, *Against Relativism: Philosophy of Science, Deconstruction and Critical Theory* (Oxford: Blackwell Publishers, 1997a), p.43

5 An example of this is the over-investment in the power of lanaguge to 'literally' change reality. See Richard Rorty, *Contingency, Irony and Solidarity* (Cambridge: Cambridge University Press, 1989); *Objectivity, Realtivism and Truth* (Cambridge: Cambridge University Press, 1991)

6 Hilary Putnam, *Renewing Philosophy* (Cambridge, Mass.: Harvard University Press, 1993), p.72

7 Ibid.,

8 Ibid.,

9 See Derrida's essay 'Supplement of the Copula' and 'White Mythology' for an example of his treatment of 'truth'.

10 Quoted in Hans-Johann Glock, *What is Analytic Philosophy* (Cambridge: Cambridge University Press, 2008), p.170

what I would call 'bad' philosophy. 'Bad' philosophy is, for me, to ignore the limitations of ones own method. That if one is epistemologically rigorous and force analysis to the point of aporia or self-contradiction, these conclusions can point to the gap between thought and the 'logic' of the text or the inadequaye formulation of the problem.[11] Instead, however, we try and close over this gap by further analysis or re-telling from first principles, be it logic or meta-metaphysics. Rather than acknowledging the limitations of ones own method and what this should tell us about our practices, the invested interests of professionalized and institutionalized philosophy continue unabated with endless research articles. Most, normally stating something quite unoriginal or tediously dry in exposition, to the point that 'some great minds have lamented that 90% of contemporary philosophy, including analytic philosophy, is bullshit.'[12] For the sorts of scientists I have been discussing, they would raise this to about 99.9%. For them, the solution is simple, philosophy has nothing to do with science and where it does get involved it appears to muddy or obfuscate thought.[13] No amount of public misconception of what Plato said will affect the truths of scientific endeavour. This is the place I wish to target, as it is not thinking about philosophy as a subject that I want people to be involved with, but developing philosophy as a kind of cognitive tool-kit to be able to spot when metaphysical claims are made. For this thesis it is the ability to tell a methodological statement from a historical one, and the implications of confusing the two when thinking about science. Rather than do away with philosophy we must defend the values of good philosophical analysis because if we do not interpret the world, someone will interpret it for us. By this I mean, without access to values such as consensus, rigour or criticism,

> we would still be labouring against massive odds of doctrinal imposition and entrenched dogmatic beliefs. More to the point, we should possess no means by which to criticize such false or groundless

[11] Christopher Norris, *Deconstruction: Theory and Practice*, Revised edition (London & New York: Routledge, 1991), p.105
[12] Pascal Engel, 'Bad Analytic Philosophy', *Dialectica*, 66 (2012) (1), 1-4 (p.2)
[13] See 'Against Philosophy' in Steven Weinberg, *Dreams of a Final Theory* (New York: Pantheon Books, 1992); in reply see Wesley C. Salmon, *Casualty and Explanation* (Oxford: Oxford University Press, 1998), pp.387-391

> beliefs and thus attain a better understanding of the forces that had worked to keep them in place.[14]

Once we are in the socio-political domain the sort of philosophical analysis Sokal or Putnam has in mind with 'deconstruction' appears impotent in concrete cases. How can we address issues of poverty if nothing is true or how can we develop social health care policy if 'science' or 'health' are just social constructs or 'texts'? This position works if we assume this is what the postmoderists or literary theorists are saying. Equally, it works as a handy opponent to re-enforce the scientific worldview against. Whether this is a result of not knowing enough about philosophy, or part of an incommensurable worldview is hard, if not impossible to say. Does Putnam miss Derrida's point due to not reading enough Derrida or because he works within a tradition that is 'incommensurable' with Derrida's conclusions, as he is 'disposed' to read him a certain way? Of course it is possible Putnam has just not read enough Derrida or is simply presenting a strawman opponent. This 'incommensurability' of traditions that Putnam and Derrida represent, however, it could be argued, remain so by 'a product of selective hindsight or a failure to perceive the large areas of shared concern which tend to be obscured by differences of idiom and localized professional ethos.'[15] This selectivity or failure of perception is what we all come with. For even before we have committed thought to paper our ideas are based in the world, as this is where they come from. Just as Putnam can (mis)-interpret Derrida in a certain way, 'science' too, as a public discourse is also open to interpretation. 'Texts are in and of the world because they lend themselves to strategies of reading whose intent is always part of a struggle for interpretative power.'[16] The analysis I wish to present of Kuhn is to illustrate this struggle and how it does not just stop with philosophers or philosophy as a subject, but applies to other discourses such as understanding science. As part of my original contribution to knowledge I wish to use ideas inspired by Heidegger to render readings of Kuhn as an example of this 'disposition' towards one view or another. What-is-more, I will show that in understanding the basis of our interpretations, we are then in a position to resolve conflicting issues which otherwise

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[14] Christopher Norris, *The Truth about Postmodernism* (Oxford: Blackwell Publishers, 1993), p.301
[15] Norris, *Against Relativism*, p.56
[16] Norris, *Deconstruction: Theory and Practice*, p.88

may remain latent in our metaphysics or act as device of persuasion when considering 'alternative' modes of thought.

So far I have been arguing that there is a mode of investigation that a certain philosophical tradition favours, where methodological abstraction becomes the only meaningful way of dealing with science.[17] Yet, when the epistemic certainty and principles of science were turned back on itself, in the form of the 'problem of demarcation', it only led to failure of those very same principles. Instead of concluding that we may have understood the problem incorrectly, it was easier to blame the tools. Rather, if we think of science and philosophy as historical practices, we may understand why the methodological approach to defining science failed. As science is taught as a 'method', I believe it is for this reason that some scientists think they see the impotency of philosophy. Multiple attempts at defining science from non-science in point of principle have failed and each time we may consider the tool too blunt an instrument. Yet, if like Feyerabend we claim there is no singular method, or that we might understand our relationship to science better, if taken from a historical perspective, one is accused of trying to undermine the methodological principles of science which clearly do work. [18]

This whole situation is a muddle about philosophy and philosophical thinking. To start with, the naturalized, realist, empiricist's position, with which science is most often associated, is itself a philosophical position. There is no way of 'testing' to see if one has adopted the correct position. To think that any historical analysis is a threat to methodological ideals, such as truth, reason or objectivity or to hold oneself in relation to reality, so that realism and empiricism seem 'obvious', is also to presuppose a certain metaphysics. From a PUoS point of view, if certain metaphysical or philosophical components become naturalized into thinking about science, so that we no longer see them, all kinds of alternative practices and belief systems can be

[17] Two notable examples being a view from the 'absolute conception of the world' in Bernard Williams, *Descartes: The Project of Pure Inquiry* (Harmondsworth: Penguin, 1978); or a view from 'objectivity' in Thomas Nagel, *The View from Nowhere* (Oxford: Oxford University Press, 1986)

[18] Fuller says that 'several philosophers have noticed that Feyerabend presupposes the positivists' rather limited conception of rationality and so that he appears more outrageous than he really is.' Steve Fuller, 'Being There with Thomas Kuhn: A Parable for Postmodern Times', *History & Theory*, 31(1992), 241-275 (p.252) [In footnote 39]. See also, Arne Naess, 'Paul Feyerabend – A Green Hero?', in *Beyond Reason: Essays on the Philosophy of Paul Feyerabend*, ed. by Gonzalo Munévar (Dordrecht: Kluwer Academic Publishers, 1991), pp.403-416

established that are parasitic on those naturalized components. For example, realism gives credence to anti-realism, objectivity gives rise to subjectivity, rationality defines irrationality, and the binary structures (logics of exclusion) that allow for these options to seem reasonable. This is not to say one has to give up real objects, truth or reason, but a certain philosophical notion about them. For to treat something 'as if' it were real, is indistinguishable from it actually being real.[19] The central themes of this thesis are distinguishing the methodological and historical approaches to science, and how philosophy can help us make sense of current claims in science for the PUoS. These are best illustrated when scientists attempt this themselves, which I will turn to next.

5.2 - The Smolin-Susskind Debate

An interesting example of the distinction I have been making and how philosophy is used by scientists, is given in a published e-mail exchange between two highly respected theoretical physicists, Lee Smolin and Leonard Susskind. The blog that published the e-mail exchange says that it is a debate by physicists for physicists, yet surrounding the technical details of the science there are fundamental opposing philosophical views of what makes something scientific.[20] I will not attempt to comment on the science, but pull from the debate the distinctions I have been making thus far. The main crux of the debate is that Smolin argues that there is a version of the 'Anthropic Principle' (AP) in Steven Weinberg's theory of galaxy formation. Smolin argues that the AP is unfalsifiable and therefore, not science or at least good science. Smolin takes a hard Popperian stance. Susskind replies, saying the assumption of the AP in Weinberg's theory is not a philosophical matter but a product of common sense appeal. Smolin points out that 'commonsense' is local to the people thinking it and so it is just a circular confirmation of the principle. The possible

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[19] Wolpert gives a good example of this casual attitude towards philosophy and its unimportance, '[m]y own position, philosophically, is that of a common-sense realist […] I know that philosophically my position may be indefensible, but – and this is crucial – holding my position will not have made one iota of difference to the nature of scientific investigations or scientific theories. *It is irrelevant*.' Lewis Wolpert, *The Unnatural Nature of Science* (London: Faber and Faber, 1993), p.106 [My italics] Wolpert was also on the House of Lords committee for the *Science and Technology Sixth Report* discussed on pp.14-15 in thesis

[20] Lee Smolin and Leonard Susskind, "Smolin vs. Susskind: The Anthropic Principle", *Edge: The Third Culture*, 18 Aug, 2004, <http://www.edge.org/3rd_culture/smolin_susskind04/smolin_susskind.html> [accessed 09 Dec 2012]

universe in which I think *x* is the universe that is geared towards human life. Depending on what scientists take to be relevant in a theory of galaxy formation they can adjust assumptions about prior probability distributions and so on, so the AP lends no explanatory weight. It is a description and not explanation. Smolin says, 'if a large body of our colleagues feels comfortable believing a theory that cannot be proved wrong, then the progress of science could get stuck.'[21] In its place Smolin was offering a version of natural selection of universes, where black holes act as the wombs of universes. He claims his theory not only does not need the AP, but is falsifiable. Susskind counters Smolin by claiming we still do not know what black holes are. Theoretically a black hole is indistinguishable from an elementary particle. Susskind then poses an ontological question, 'should we treat every particle as a black hole?' Smolin in his final reply has at the heart of his response Popperian science in mind, 'the more vulnerable a theory is, the better science it is' and we must 'carefully distinguish conjectures from the actual results.' Susskind attacks Smolin's central piece of logic, implying he is a philosopher and not a scientist and gives a tongue-in-cheek admonishment of an appeal to authority, namely Popper, a philosopher and not a scientist. Susskind then debates over what Smolin means by 'unfalsifiable', such that a claim cannot be falsified in principle or due to physical or epistemological constraints? Susskind then makes a telling remark about his view of science, that an idea is not worth pursuing unless it has drawn the criticism of unfalsifiability. He then gives examples from psychology, physics, and biology of once previously thought unfalsifiable ideas, which now are accepted as common place. Susskind also comments that for social reasons physicist will turn a blind eye to theory-evidence, if it does not 'fit' with their own pet theories. Susskind says that falsifiability tells us nothing about the way the world is, if science is only falsification. He references the creationism debate saying that two conjectures, one stating that the universe was created 6,000 years ago ready-made with the design of an old universe, is as unfalsifiable as saying it did not begin this way. Smolin's position is easy to locate as a Popperian and therefore offering a methodological definition of science. Susskind puts his cards on the table by saying, 'good scientific methodology is not an abstract set of rules dictated by philosophers. It is conditioned by, and determined by, the

[21] Ibid.,

science itself and the scientists who create the science […] let's not put the cart before the horse. Science is the horse that pulls the cart of philosophy.'[22]

Susskind, knowingly or not, has done a number of things. Firstly by giving historical examples of where falsification has not worked as a principle for telling science from non-science, he is adopting a non-methodological definition of science. That is, science is what scientists do, good and bad. He takes Smolin's criteria of falsification to mean 'observable' falsification, such that we cannot witness universe formations in black holes, like we cannot witness evolution. This rehearses the old debates of empiricism and positivism, that one can simply observe facts to prove or falsify a theory. Susskind, however, after giving a critique that could have come from Feyerabend or Kuhn, then aligns the methodological approach of defining science as 'philosophy'. Yet his analysis is not scientific by concluding science defines science or science is what scientists do. This could easily be a sociological statement about boundary definitions or a philosophical statement about the limits of a methodological definition.

This is one example of the methodological versus historical conception of science as understood by scientists. This exchange, however, mostly revolved around the ironing out of exactly what is meant by falsification and its implications for scientific claims. This is old territory for philosophers of science, but another more explicit statement of how science is *more than* its methodology is given by Richard Feynman. For it is all too easy to say 'philosophy of science is as useful to scientists as ornithology is to birds', but that is precisely the point, philosophy is not science, it is thinking *about* science.

5.3 - Richard Feynman on Science

Richard Feynman is noted and quoted for his dislike of philosophy.[23] This is the 'received view' of Feynman, but was Feynman so anti-philosophy? In a 1966 address

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[22] Ibid.,

[23] Science educator Brain Cox quotes Feynman on the uselessness of philosophy of science. Brian Cox, 'Huw Wheldon Memorial Lecture: Science – A Challenge to TV Orthodoxy', *BBC 2 The Royal Television Society*, 02 December 2010. In an edited work from the 'Feynman Lectures', now considered textbook classic on relativistic physics Feynman dedicates a whole section to 'relativism' and

at the 'National Science Teachers Association', Feynman gave a talk titled, 'What is Science?' In this talk he makes a distinction between knowing (as an epistemic act) and understanding (as a lived relation or tacit awareness). He goes on to dismiss all the methodological criteria for what science is taught to be. This I take to be an example of how we are not committed to one interpretation of science and that any the 'philosophy' that scientists despise must be cut from a very thin cloth. Feynman states that teaching textbooks on science are the 'watered-down and mixed-up words of Francis Bacon'.[24] Quoting William Harvey, he critiques the Baconian model saying that one cannot merely observe, but a judgement is involved about what to pay attention too. He takes this, however, to be an example of how philosophy seeks a methodological description of science and has failed. 'And so what science is, is not what the philosophers have said it is and certainly not what the teacher editions [textbook] say it is.'[25] Feynman, in order to get at the spirit of what he is trying to say about science, quotes a children's poem about a toad asking a centipede how he runs. The centipede, in the act of trying to work out how he runs, falls over confused not knowing how to stand. Feynman likens the question 'what is science?' to the centipede, in that, in any explication of what science 'is' is more confusing than 'doing' science.

Feynman states that he 'dislikes philosophical exposition' and recognizes the 'difficulty of the subject', so instead he chooses to describe how he learned science.[26] In fact, what he does is present what science means to him. Feynman goes on to give a series of personal anecdotes of where 'science' is present for him. He then stops himself halfway through an anecdote on the relationship between mathematical symbols and their meanings to consider what they mean in regards to science. He states that learning the meanings of scientific concepts is *not* science. The learning of what words and concepts mean is the act of teaching science but *not* doing it. 'It is not

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'philosophers'. His treatment is equally scathing and facetious aimed at anonymous philosophers of science calling them 'cocktail party philsophers'. Richard Feynamn, *Six Not-So-Easy Pieces: Einstein's Relativity, Symmetry and Space-Time* (London: Penguin Books, 1999), p.73. In the pre-amble to an interview with 'Omni Magazine' Feynman is described as talking about 'what he knows and loves best – physics – and what he loves least, philosophy.' Richard Feynman, 'The Smartest Man in the World', in *The Pleasure of Finding Things Out: The Best Short Works of Richard P. Feynman*, ed. by Jeffery Robbins (London: Penguin Books, 2001), pp.189-204 (p.189)

[24] Richard Feynman, 'What is Science?', in *The Pleasure of Finding Things Out: The Best Short Works of Richard P. Feynman*, ed. by Jeffery Robbins (London: Penguin Books, 2001), pp.171-188 (p.173)

[25] Ibid.,

science to know how to change Centigrade to Fahrenheit.'[27] It is a sufficient but not necessary condition of science. He describes concepts and words as the tools of science, but says they are *not* science itself, and that merely learning the meaning of words will ultimately get you nowhere in science, as all you will only learn about the limit of people's imaginations. Feynman gives an analysis of a child's textbook lesson on 'energy', which verges on a critique of metaphysical notions in physics. Feynman tells us that the scientific idea of 'energy' is so difficult to get right that any everyday use of it will derive incorrect inferences or deductions. The problem is that we use 'energy' in a way that is tautologous with its definition. We may say that energy is 'in' a moving object, but it is not 'what' makes it move. Yet if we wind up a clockwork toy we may say that it is the energy in the springs that are making it move.[28] Feynman suggests that if you cannot re-describe the concept 'energy' without using the word 'energy', or a synonym of energy, you are then only learning definitions and have learnt nothing about science. He hints that science tells us about the relationships where energy is found but not what it is.

Feynman offers a pet theory for how science got started, but by coming at science from the 'doing' side, rather than an abstract definition, his analysis takes on a historical aspect. He says that humans got to a stage where worldviews could be passed on without losing too much information due to our efficiency with language.[29] In this inheritable worldview, profitable as well as 'mistaken ideas' can be passed on. Science for Feynman is, 'to find out *ab initiio* (sic), again from experience, what the situation is, rather than trusting the experience of the past in the form in which it was passed down.'[30] Feynman, in attempting a definition of science, also then states that science is not its form. To say science is 'this or that method' is one of the ways science develops, but is itself not science. To copy or imitate form is not to be doing science, and from this observation Feynman says he can also re-define science as, 'the belief in the ignorance of experts. When someone says science teaches such and such,

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[26] Ibid., p.174
[27] Ibid., p.177
[28] Ibid., p.179
[29] Feynman calls it a 'memory for the race' rather than 'worldview'. Ibid., p.185
[30] Ibid., [Italics in original] 'ab initiio' (sic) refers in science to the starting from first principles. This comes very close to a Husserlian phenomenology, where we 'neither make nor go on accepting any judgements […] that I have not derived from evidence' based on the first principles of experience. Husserl quoted in Juha Himanka, 'Husserl's Argumentation for the Pre-Copernican View of the Earth', *The Review of Metaphysics*, 58 (2005), 621-644 (p.625)

he is using the word incorrectly. Science doesn't teach it; experience teaches it.'[31] In a sense, it takes learning (scientific training) to be able to conflate 'experience' with 'science' as 'true reality'. To which Feynman suggests a kind of intellectual agnosticism, where science is the skill to,

> '[P]ass on the accumulated wisdom, plus the wisdom that it might not be wisdom […] to teach both to accept and reject the past with a kind of balance that takes considerable skill. Science alone of all the subjects contains within itself the lesson of the danger of belief in the infallibility of the greatest teachers of the preceding generation.'[32]

In these two brief descriptions on what science is by scientists I have tried to show that the methodological interpretation can be legitimately challenged as to whether it is the dominant interpretation over the historical. Ideas, such as starting from experience rather than trusting the experiences that has been handed down to us, or learning from the episodes of past as to what is 'unfalsifiable', are not necessarily unscientific, but just an elevation of this historical aspect.

Before I continue with these methodological/historical discourses about science I would like to restate why I believe philosophy to be important to understanding science. I will then explain Thomas Kuhn's role in my thesis and how I intend to develop those distinctions I have made thus far.

5.4 - The Situation so Far

As human knowledge is not genetically inheritable, the further we press on with science's investigation the more there is for the average person to know about. But as has already been argued, simply *knowing* facts about science is not the equivalent to *understanding* science. The direction someone is heading makes more sense if one understands where they have come from. Present scientific achievements need to be understood in terms of past scientific activity. Yet it could be argued that we tend to

[31] Feynman, 'What is Science?', p.187
[32] Ibid., p.188

do the opposite. We understand past scientific achievements in terms of present scientific activity, as if it were the same thing.

Today, the current landscape of physics seems to bear a close resemblance, in some respects, to that of pre-twentieth century physics, in that metaphysical and theoretical ideas abound. The luminiferous aether of eighteenth and nineteenth century physics has made way for ideas such as dark energy or multi-dimensional universes. This is not to say they are a replacement, wrong or unscientific, but only that they bear the philosophical and metaphysical hallmarks that irritated the logical positivists. The inability to replace our current theory of gravity, as described by general relativity, is also another example of how science does not proceed by falsification.[33] Closer to the day-to-day physics of mechanics or calculus appear more concrete in character, but still operate within certain metaphysical assumptions, such as inertia, causation, or space-time geometry. The problem is not that science contains metaphysical ideas, as arguably it could not operate without them, but that if lacking suitable training in the use of metaphysics or with the wholesale neglect of philosophy, we can get into all sorts of theoretical, philosophical or linguistic quagmires. From a public point of view, the understanding of science can only be impoverished by the attitude that philosophy is irrelevant or even worse, antithetical to science.[34] This is not to say all philosophical contributions to science are valid, but the placing of methodology as primary to science's historical development is to read events off as either having an invalid contribution, which we label as 'mystical' or 'pseudo-scientific', or any valid contribution as 'prophetic' or 'genius'.[35]

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[33] Nobel-Prize winning cosmologist Adam Reiss in an interview explains that 'physicists are conservative. We don't want to throw away our theory of gravity when we might be able to patch it up'. Victoria Jaggard, "Physics Nobel Explainer: Why is Expanding Universe Accelerating?", *National Geographic,* 4 Oct. 2011 <http://news.nationalgeographic.com/new/2011/10/111004-nobel-prize-physics-universe-expansion-what-is-dark-energy-science/> [accessed 20 Nov 2012]

[34] A current example of how philosophy suffers from its perceived image of 'uselessness' is by Australian MP Jamie Briggs. He wrote an article highlighting examples of 'waste' in Governmental academic research funding where philosophy featured prominantly. Jamie Briggs, "Ending More of Labor's Waster", *Labor = Waste: Highlighting Labor's Waste, Inefficiency and Mismanagement*, 5 Sept. 2013 <http://laborwaste.com.au/post/60308301521/ending-more-of-labors-waste#.Ui8CbT_9WeY> [accessed 9 September 2013]

[35] Peter Medawar's review of Teilhard de Chardin *The Phenomenon of Man* is a good example of this. Medawar rips into Teilhard de Chardin for making woolly statements about evolution in mystical language. *The Phenomenon of Man* was written in French, in the 1930's with a foreword by Julian Huxley, the co-developer of the modern synthesis in biology. Given the historical context in which it was written and with the support of one of the foremost scientists of evolutionary theory, is Medawar reading *The Phenomenon of Man* as a modern scientific critique of 1960's evolutionary theory? Peter Medawar, *The Strange Case of the Spotted Mice and Other Classic Essays on Science* (Oxford: Oxford University Press, 1996), pp.1-11. Chardin is also credited with predicting the Internet in the form of the

Neither science nor philosophy has the monopoly on metaphysics, but it is a mistake to think one is freer of them than others. A dogmatic belief in this has been called 'scientism'.[36] I do not take scientism to be an isolated school of thought, but is part of a continuum of epistemological claims. At the one end, there is the denial that science says anything about reality and at the other, it is only science that can tell us what things are real or meaningful. Ryle used the phrase 'poison-pen effect', which is a rhetorical device where a writer invokes the authority of science to show up the illusionary nature of experiential knowledge.[37] Science tells us that solid objects are actually mostly empty space at the sub-atomic level. If we take this to mean our experiences of 'hardness' are some how figments of our imagination, and what is 'real' is the weak electrical forces of an object pushing against those of my hand, we are starting to replace lived reality with an abstract description of it. Another publicly outspoken anti-philosophy scientist is Nobel-Prize winner, Steven Weinberg. He sees philosophy only as a tool for clarifying the meaning of words.[38] This is to place philosophy's role as purely semantic or linguistic. Philosophy, for Weinberg, has no bearing upon what he believes he is doing. Science, for him, is 'the slow and uncertain progress of physical theories toward an ultimate culture-free form that is the way it is because this is the way the world is'.[39] The metaphysics that allows the idea of 'ultimate culture-free' descriptions of reality to make sense to Weinberg is the inverse of the metaphysics that can have reality as a creation of the mind or pure linguistic construct. With philosophy demoted to the role of dictionary, we may struggle to give a non-methodological account of why such non-realist interpretations are not valid. Yet, from the constant failure of the methodological account and its parasitism on realist/objectivist metaphysics, it is this relationship by which

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'noosphere', which is either prophetic or a kind of genius. See, Patrice Flichy, *The Internet Imaginaire* (Cambridge, Mass: MIT Press, 2007), pp.111-113

[36] Sorell identifies new and old scientism. Tom Sorell, *Scientism: Philosophy and the Infatuation with Science* (London: Routledge, 1991), pp.24-40. The 'new scientism' is also called 'philosophism' in the literature, Michael Lynch, 'Scientism and Philosophism: Comment on 'Kinder, Gentler Science Wars' by Gabriel Stolzenberg, *Social Studies of Science*, 34 (2004), 93-98

[37] Gilbert Ryle, 'The World of Science and the Everyday World', in *Dilemmas: The Tanner Lectures 1953*, ed. by Gilbert Ryle (Cambridge: Cambridge University Press, 1954), pp.77-89

[38] Steven Weinberg, 'Peace at Last?', in *The One Culture? A Conversation about Science*, ed. by Jay Labinger and Harry Collins (Chicago: University of Chicago Press, 2001), pp.238-242 (p.240)

[39] Ibid., p.238

‘alternative’ explanations gain their potency.[40] It is between the poles of science as its methodological guise and science as unable to say anything about reality through its historical guise, that I find Kuhn as a useful mediator, to whom I turn next.

5.5 - Thomas Kuhn

Kuhn holds a unique position for this thesis, as he was a trained scientist who then moved into the history and philosophy of science, and his work has dramatically divided opinion over what he means or what the most important aspects of his work are. Kuhn started out as a scientist, who approached the subject of history of science and philosophy as an ‘outsider’. His main book, *The Structure of Scientific Revolutions,* as an explanation for scientific change, divided academic opinion on what is important about it.[41] As an explanation or model for scientific change, it has also crossed over and influenced disciplines outside of the natural sciences. For the purposes of this thesis I offer Kuhn’s work in relation to the PUoS as: 1) to offer an alternative to the methodological interpretation of science, and by showing how this is constructed demonstrate 2) which ‘Kuhn’ one derives in *Structure* is a result of the relationship one has to the text. By this I mean, whether one is struck by the methodological or historical aspects of his book are, in part, a result of a prior understanding of those same ideas. How this relates to the PUoS is that which ‘Kuhn’ we enlist to help us understand what science is, will also determine the utility of the philosophy and science involved. There is a more general point here that the same effect is at work in the PUoS itself, in how it frames what science is and how scientists have perceived the role of philosophy.

I will not be considering all of Kuhn’s work, but center on *Structure* as one of the most important and influential books in the philosophy of science.[42] The fact that it is

[40] Popular New-Age interpretations of quantum physics in Capra (1983; 1991), Zukav (1989), utilise the metaphysics that Weinberg assumes. However, this is not to say they are doing the same thing at the level of practice. Peter Woit writes, ‘bootstrap philosophy, despite its complete failure as a physical theory, lives on as part of an embarrassing New Age cult, with its followers refusing to acknowledge what has happened. Peter Woit, *Not Even Wrong: The Failure of String Theory and the Search for Unity in Physical law* (New York: Basic Books, 2007), p.145
[41] Thomas Kuhn, *The Structure of Scientifc Revolutions*, 3rd edn. (Chicago: The University of Chicago Press, 1996) [Also referred to as *Structure*]
[42] In the seven-year period between 1976 – 83 *Structure* was reported to be the twentieth century’s most frequently cited book. Eugene Garfield, ‘A Different Sort of Great Books List: The 50 Twentieth-

so influential is one of the reasons it has been chosen. It may seem an obvious thing to say, but the reasons for its considered importance, I will argue, are the result of the interpretations that have been ascribed to it. By this I mean, the interpretations are not the result of what one knows about science, history or philosophy as a topic, but the philosophical baggage one already possess and brings to bear on those subjects.[43]

Kuhn started out as a scientist, training and eventually receiving a doctorate in physics. He then taught an undergraduate class for humanities studies in the history of science, as devised by Harvard President James B. Conant.[44] Kuhn was at Harvard whilst Conant was its President and Willard Quine was an active faculty member.[45] Conant was important for Kuhn and the development of academic and public engagement with science, which again was motivated by surrounding socio-political events.[46] To combat public fears of science (nuclear research programmes), Conant highlighted the role of 'seeing' science happen. He thought that opening up laboratories to the general public would remove any mystique and maybe develop a sense of respect for what scientists do, rather than 'arousing public suspicions'.[47] But the idea that one merely 'sees' science happen is akin to the deficit model's 'knowing'

Century Works Most Cited in *The Arts & Humanities Citation Index*, 1976-1983', *Current Comments*, (16) 20 April 1987. Fuller says, '*The Structure of Scientific Revolutions* is one of the most influential books of this century.' Fuller, 'Being There', p. 241. John Naughton states that the Kuhnian term 'paradigm shift' appears in 18,300 of the books marketed by Amazon and *Structure* is one of the most cited academic books of all time. To date it has sold 1.4 million copies, which for an academic book is 'Harry Potter-scale numbers.' John Naughton, "Thomas Kuhn: The Man Who Changed the Way the World Looked at Science", *The Guardian*, 19 Aug. 2012 <http://guardian.co.uk/science/2012/aug/19/Thomas-kuhn-structure-scientific-revolutions> [accessed 28 Oct 2012]

43 James Pinkerton, an ex-Reagan and Bush administration aide, Fox News panellist, and political columnist is an example of the relevance of philosophy and the importance of understanding ideas. He has described how reading Kuhn and his idea of paradigm change influenced his decisions on educational spending policy. He re-describes the movements in US political educational reform in the language of Kuhn. His own take-away message from Kuhn was that *Structure* was about 'change'. Pinkerton's reading is based on a similarity of language and not ideas, yet this was enough for him to implement 'school choice' reform as part of the 'No Child left Behind' legislation. James P. Pinkerton, "School Choice: The Paradigm Shift – Thomas Kuhn and the Structure of the Education Revolution", *The American Conservative*, 27 Aug. 2012 <http://theamericanconservative.com/articles/how-school-choice-became-inevitable/> [accessed 12 December 2012]

44 N. M. Swerdlow, 'An Essay on Thomas Kuhn's First Scientific Revolution, *The Copernican Revolution*', *Proceedings of the American Philosophical Society*, 148 (2004), 64-120 (p.67)

45 Also called the 'Harvard/ Conant' group on pp.17-18 in thesis

46 For example, in America the context of World War II, the perceived political threat of communism and the destructive power of nuclear energy had a significant impact on how science was understood in America. George Reisch, "The Paranoid Style in American History of Science", *Theoria*, 27 (2012) <http://www.ehu.es/ojs/inde.php/THEORIA/issue/current> [accessed 28 October 2012]

47 Steve Fuller, *Thomas Kuhn: A Philosophical History For Our Times* (Chicago: University of Chicago Press, 2001), p.223

facts. Yaron Ezrahi describes Conant's 'seeing is believing' empiricism as being part of a wider 'visual culture'.[48] For it is not enough to be presented with science but it is how the public is involved in the justification of scientific matters that count.

Conant found the historical analysis of science far more revealing than the philosophical, which for him and Kuhn meant, 'analytical philosophy'.[49] Conant in *On Understanding Science* shows the value of the historical approach by saying the answer to the question 'what is science?' is unanswerable in analytical terms (methodological), but can be attempted in historical terms.[50] Conant's take on science is just as sophisticated as Kuhn's and in a particularly revealing passage says, science 'almost by definition, I would say, moves ahead.'[51] As Swerdlow comments, 'we live in a scientific age not in virtue of what we know, but what we do. Science must advance to stay alive, must advance to meet the definition of science.'[52] Yet even in Conant's time his courses received wide criticism as a method for teaching about science.[53] Out of Conant's curriculum came Kuhn's first book, *The Copernican Revolution*, which had a number of important developments. Instead of treating periods of historical scientific development as only movements between conceptual schemes, Kuhn thought that the accompanying shifts in intellectual development were equally fundamental.[54] Thus these groupings of 'conceptual schemes' can be treated as exemplars for how people conducted science, but within these conceptual schemes it tells us nothing of how we move from one to the other.

From the ideas seeded in *The Copernican Revolution* Kuhn went on to write *Structure*. This, like his previous work, was influenced more by mainland European

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[48] Yaron Ezrahi, *The Descent of Icarus: Science and the Transformation of Contemporary Democracy* (Cambridge, Mass: Harvard University Press, 1990), pp.67-96
[49] Fuller, *Thomas Kuhn*, p.223
[50] James B. Conant, *On Understanding Science, A Historical Approach* (New Haven: Yale University Press, 1948), pp.24-25. In contrast Sardar says Conant's historical studies of science were 'strictly internal'. If by this he means internal to science studies that dealt in methodological terms I take this as another example of how our relationship to the text affects our understanding of events, for it would appear to contradict Conant's own thinking. Ziauddin Sardar, *Thomas Kuhn and the Science Wars* (Cambridge: Icon Books, 2000), p.24
[51] Conant, *On Understanding Science*, p.25
[52] Swerdlow, 'An Essay on Thomas Kuhn', p.70
[53] James B. Conant, *The Growth of the Experimental Sciences: An Experiment in General Education. Progress Report on the Use of the Case Method in Teaching the Principles of the Tactics and Strategy of Science* (Cambridge, Mass.: Harvard University Press, 1949), pp. 5-10
[54] Thomas Kuhn, *The Copernican Revolution: Planetary Astronomy in the Development of Western Thought* (Cambridge, Mass.: Harvard University Press, 1957), pp. vii-viii

scholars such as Ernst Cassirer, Emile Meyerson, Léon Brunschvicg, Hélène Metzger, Anneliese Maier, E. J. Dijksterhuis, Ludwig Fleck and Alexandre Koyré, than Anglo-American scholars.[55] Kuhn had exposed himself to antiquated scientific thinking where he began to situate this former knowledge as a historian rather than as a scientist.[56] This move slowly began to erode his student 'textbook' worldview of science.[57] It is with the publication of *Structure* that Kuhn's thoughts on science and scientific change began to polarize opinion. On the one hand, it was thought Kuhn was rejecting traditional ideals such as truth, rationality and objectivity, long held sacred in analytical philosophy and science. This apparent rejection was then enthusiastically taken up by more radical thinkers, associated with pragmatism, the strong programmes of sociology, and 'postmodernism', as a new kind of relativism. This relativism could then be wielded against any pronouncements of authority or superiority by the sciences as an epistemic enterprise.[58] On the other hand, Kuhn's emphasis on the historical, the scientific community, and practice reinforced an emerging trend toward social and cultural history of science. Here Kuhn addressed directly what scientists were doing and tried to situate their actions and thoughts within the knowledge and problems of their time, rather than reflect upon them from the present with what we now 'know'.

5.6 - *The Structure of Scientific Revolutions*

It is a trivial observation to say that Kuhn has influenced subjects outside of history of science. What is less benign is to ask why the same book has created such a divide in academic opinion?[59] What I will try to show in my analysis of *Structure* is that, those

[55] Kuhn, *Structure*, p.viii. Friedman argues Kuhn was more influenced by scholars in the European traditions than by any developments in Anglo-American logical positivism. Michael Friedman, 'Kuhn and Logical Empiricism', in *Thomas Kuhn* ed. by T, Nickles, (Cambridge: Cambridge University Press, 2003), pp. 19-44.
[56] Swerdlow anecdotally states that Kuhn regarded himself as a physicist finding his way in the history of science. Swerdlow, 'An Essay on Thomas Kuhn', p.75
[57] Kuhn, *Structure*, p. vii
[58] For further commentary on this see Michael Friedman, 'On the Sociology of Scientific Knowledge and Its Philosophical Agenda', *Studies in History and Philosophy of Science*, 29 (1998), 239–271
[59] Fuller has argued that interpretations of Kuhn come about by indulging 'in shameless Kuhnotropistry.' He cites his own work as evidence for this and confesses, 'this essay is partly in atonement for my errant ways.' Fuller, 'Being There', p.242. Others have claimed the tensions in Kuhn between subjective and objective knowledge are what foster these alternative readings. Alan F. Chalmers, *What is this thing called Science?* 3rd edn. (Maidenhead: Open University Press, 2010), pp.125-129. Ian Hacking in his introductory essay to the fourth edition says simply 'just because *Structure* is such a great book it can be read in endless ways and put to many uses.' Ian Hacking,

who take ‘methodology’ to be key to understanding Kuhn can then derive a Kuhn that is a relativist, irrationalist, anti-realist or any other of the more radical positions ascribed to him. However, if one takes the ‘historical’ aspect to be key, one may escape those radical positions, without giving up a ‘sensible’ understanding of philosophy and science.[60] That is, we may preserve the special epistemic authority of science and find a useful role for philosophy. I will be mainly analyzing *Structure* in its three editions, but taking the 1996 edition as my main source.[61] Despite Kuhn’s own attempts to spell out what he means in *Structure* I find it more useful to look at the reactions to his text, rather than any appeal to what Kuhn ‘actually’ meant. Kuhn himself appreciated his detractors more than those that wished to build on what they thought he was saying.[62] So, put another way, Kuhn has more to say than is written in *Structure*, which displaces the author as having any privileged position on its content.[63]

A number of things will be argued in chapters 7 and 8: 1) If Kuhn is read as a methodology of science, where we take the linguistic or epistemological to be the driving force of science, one can derive a ‘radical’ Kuhn or what I will call a ‘strong’ version.[64] 2) If Kuhn is read as a historical explanation of science, we can derive a ‘moderate’ Kuhn as a ‘weak’ version, and in doing so 3) justify a role for philosophy showing that this type of philosophical activity contributes, rather than detracts from a public understanding of science.[65] Patrick Heelan also used the terms ‘strong’ and

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‘Introductory Essay’, in *The Structure of Scientific Revolutions*, 50th Anniversary Edn. (Chicago: The University of Chicago Press, 2012), pp.vii – xxxviii (p.viii)

[60] Kitcher gives this dual interpretation in his brief sketch of Kuhn but fails to explain why he ‘listens’ to one interpretation over another. Philip Kitcher, ‘Toward a Pragmatist Philosophy of Science’, *Theoria*, 77 (2013), 185-231 (p.201)

[61] A fourth edition was released in 2012 with an introductory essay by Ian Hacking.

[62] ‘It is doubtful that there has ever been another academic who has met the greatness thrust upon him with such apparent ingratitude.’ Fuller, ‘Being There’, p.242. In an interview Kuhn also said, ‘I’ve often: said I’m much fonder of my critics than my fans.’ John Horgan, ‘Profile: Reluctant Revolutionary: Thomas S. Kuhn Unleashed Paradigm on the World’, *Scientific American*, 264 (1991), 40-49 (p.49)

[63] Pigliucci expresses this problem in regards to what Kuhn ‘actually’ meant, ‘So which one was it? Was Kuhn a relativist or did he accept the idea that science does make progress?’ Massimo Pigliucci, *Nonsense on Stilts: How to Tell Science from Bunk* (Chicago: The University of Chicago Press, 2010), p.267

[64] I could identify ‘strong’ with what Caputo calls the ‘classical’ reading. *Deconstruction in a Nutshell: A Conversation with Jacques Derrida*, ed. by John D. Caputo (New York: Fordham University Press, 1997), p.76

[65] This idea of reading Kuhn in a number of potential ways is not new. Bird offers three potential versions of Kuhn as a conservative, going against the received view of Kuhn as a radical. Alexander J. Bird, ‘Three conservative Kuhns’, *Social Epistemology*, 17 (2/3) (2003), 125-131. Pinch gives two

‘weak’ in his hermeneutic reading of science.[66] For me ‘strong’ and ‘weak’ refer to the metaphysical commitment we have to our ideas when interpreting the text. The ‘strong’ version refuses to accept the basis of its own position, whereas the 'weak' version becomes a kind of positive, self-undermining practice, that we see with deconstructivism. The term pairings of ‘strong’ and ‘methodological’ and ‘weak’ and ‘historical’ can be used interchangeably. Within the ‘strong’ reading this inability to acknowledge the possibility for its own origins produces a scale of methodological positions,

> that runs from slightly fretful realists anxious to defend their stance by making certain seemingly minor (though often in fact highly damaging) concessions, via defenders of a middle ground approach […] to fully-fledged anti-realist arguments or out-and-out descriptivist/constructivist programs in the Rortian or Goodmanian ‘ways of worldmaking’ mode.[67]

In the ‘Huw Wheldon Memorial lectures’, Brian Cox states, in reference to explanatory power of science, that ‘as long as you accept that evidence is more important than opinion, then this is a statement of the obvious’.[68] The methodological approach is straightforward on this matter. We look at the theory and the observations and see if they are supported. Yet from a historical perspective we see that ‘evidence’ is an ambiguous term. This is exactly what is at stake between the physicists Smolin and Susskind.[69] For example, over the last twenty years the ‘hockey stick’ controversy over global warming has not only been fuelled by the ambiguities in methods of data collection, data coverage, statistical analysis, and modeled assumptions, but also over

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polemical versions of Kuhn one conservative the other radical. His reading is closer to mine, however, he only pursues the concept of ‘paradigm’ where one meaning is language based such that paradigms are quantifiable by citations and sociometric measurements. The other meaning of paradigm ‘emphasises the integration of, and nature of cognitive and social activity in science.’ Trevor Pinch, ‘Kuhn – The Conservative and Radical Interpretations: Are Some Mertonians ‘Kuhnians’ and Some Kuhnians ‘Mertonians’?’, *Social Studies of Science*, 27 (1997), 465-482 (p.466)

[66] Timothy J. Stapleton, ‘Introduction’, in *The Question of Hermeneutics: Essays in Honour of Joseph J. Kockelmans*, ed. by T. J. Stapleton (Dorecht: Kluwer Academic Publishers), pp.1-16 (p.11)

[67] Christopher Norris, ‘Pots, Kettles and Shades of Black: Analytic Philosophy Versus Postmodernism’, *Revue Internationale de Philosophie*, 1 (2010), 9 – 37 (pp.22-23)

[68] Brian Cox, ‘Huw Wheldon Memorial Lecture: Science – A Challenge to TV Orthodoxy’, *BBC 2 The Royal Television Society*, 02 December 2010

[69] Pp.103-105 in thesis.

accusations of data plagiarism.[70] Cox argues for the authority of the 'peer reviewed' system, yet scientists can and do get things wrong, and with the added pressure of having to publish in order to secure funding, the scientist's hand may be forced even more.[71] Going back over history we can see how theory dictates what gets picked out as evidence. Both Aristotle and Galileo 'saw' the retrograde orbit of Mercury across the night sky, but evidence in both these contexts cannot be taken in isolation of the theory that allows them to recognize the motion of Mercury.[72] If we do, it allows us to think that Aristotle and modern astronomers are both involved in the same activity. This was Kuhn's major breakthrough in reading Aristotle, not as a Newtonian, but as an Aristotelian.[73] Yet, the message Cox delivered, and is central to the methodological perspective, is the unequivocation of scientific terms like 'evidence', but could as easily be replaced with 'true' or 'rational'. Why this becomes problematic for the PUoS is that, like Kuhn, how does one solve or address any of the methodological problems one may come up against when presented with the history of science?

It was after reading Heidegger that I was inspired with the idea of how one may go about understanding those different aspects of Kuhn mentioned so far. It must be stated that it is Heidegger inspired ideas that are used in this thesis, and not Heideggerian ideas themselves. It was from my reading of Heidegger that I developed conceptual tools for separating out the methodological and historical elements of Kuhn. There is also a more general reason I have picked Heidegger, as with the 'strong' Kuhn, he has acquired many detractors (rightly or wrongly), so it is with acknowledging his influence that we see how philosophy can be part of a practice of synthesis and adaptation. As with the strong reading, it is too easy to dismiss someone as radical or aiming to obfuscate, rather than make the effort to retrieve some sense of intelligibility in their work.

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[70] Michael E. Mann, *The Hockey Stick and the Climate Wars: Dispatches from the Front Lines* (New York: Columbia University Press, 2012)

[71] Kitcher notes the problems with a 'credit economy' in science. Philip Kitcher, 'Toward a Pragmatist Philosophy of Science', *Theoria*, 77 (2013), 185-231 (pp.203-206)

[72] Feyerabend's challenge to Ernst Nagel's model of explanation by reduction is generally regarded as the first time that a formal logical model has been out manoeuvred by an historical approach, to wit Feyerabend's critiques had to be assimilated into the general account of reductionism. See, Paul Feyerabend, 'Explanation, Reduction and Empiricism', in *Minnesota Studies in the Philosophy of Science: Scientific Explanation, Space and Time*, ed. by H. Feigl & G. Maxwell, 3 (1962), pp.28-97; Kenneth F. Schaffner, 'Approaches to Reduction', *Philosophy of Science*, 34 (1967), 137-147

[73] Thomas Kuhn, *The Road Since Structure: Philosophical Essays 1970-1993*, ed. by James Conant and John Haugeland (Chicago: The University of Chicago Press, 2000), p.16

5.7 - Martin Heidegger: Introduction

As previously stated I am not so much concerned with what Kuhn 'actually' meant but more with what interpretations can be developed and how we reflect on the origins of those interpretations.[74] Do they offer fruitful interpretations of science or do they result in doctrines that cannot be self-sustained, such relativism, scientism or irrationalism? Another philosopher, who arguably has divided more academic opinion than Thomas Kuhn, is Martin Heidegger. Whilst Heidegger's philosophy of science is fairly underdeveloped in *Being and Time*, it is through his brief description of how science progresses and some of the ideas expressed in *Being and Time* that brought about the genesis of this thesis.[75]

Science, for Heidegger, means something different to the everyday notion of science that we might use. For Heidegger, any area of thought that can be thematized into objects for investigation is a science.[76] However, what Heidegger, and more to the point what Kuhn meant by 'science' is irrelevant, as this is what will be up for grabs by how we read them. As Kuhn was not a trained philosopher and was writing *Structure* with ideas that were new to him, I think there are many philosophical themes in the text either not fully expressed or understood. Some have gained momentum, whilst others have been overlooked, which may be another reason why *Structure* has achieved the status it has, due to the many philosophical threads one can pick out of his text.[77] There is the naive psychologism, radical meaning variance of

[74] Fuller writes, 'because *Structure* is not a perfect text, and does not cloak its imperfections in jargon, it invites further participation by the reader to correct its flaws and complete its argument.' Fuller, 'Being There', p.259

[75] Whilst 'time' is central issue for Heidegger its importance in Kuhn but will only be discussed indirectly via its metaphysical inclusion in the methodological approach and its implied structure in the historical reading. Kuhn's own agenda was with 'Whiggism' in the history of science. Paul Hoyningen-Huene (1992; 2012) has argued for the 'philosophical element' in the historiography of Kuhn or any historiography. Paul Hoyningen-Huene, 'The Interrelations Between the Philosophy, History, and Sociology of Science in Thomas Kuhn's Theory of Scientific Development', *British Journal for the Philosophy of Science*, 42 (1992), 487-501; 'Philosophical Elements in Thomas Kuhn's Historiography of Science', *Theoria*, 75 (2012), 281-292.

[76] Martin Heidegger, *Being and Time*, trans. by J. Macquarrie & E. Robinson (New York: Harper & Row, 1962), p.7

[77] The first extensive critical review of *Structure* was by Dudley Shapere, in *The Philosophical Review*, 73 (1964), July (3), 383 - 394 who was already looking critically at the logical positivists' attempt to analyse language and its use in science. Both Kuhn and Shapere were students at Harvard. Shapere was championing the idea that one can only understand science by a historically sensitive analysis of ordinary language and by what scientists meant in particular contexts, see Dudley Shapere, 'Meaning and Scientific Change', in *Mind and Cosmos: Essays in Contemporary Science and Philosophy*, ed. by

terms, the incommensurability of paradigms, or the challenge to the positivist's conception of science, to name a few philosophical issues.[78] The dominance of the methodological interpretation can be seen in Marletta, where he identifies no less than three types of incommensurability in *Structure*, but also stating that Kuhn's thesis is one of methodological incommensurability.[79] The other types of Incommensurability, as followed by Hoyningen-Huene and Sankey, are semantic and ontological.[80] Marletta chooses to avoid ontological relativism as it 'is a complex position which involves not only the thesis of incommensurability, but also the structure of paradigms and the refutation of the correspondence theory of truth.'[81] I will argue that if Kuhn is read through my 'strong' version, one will come to similar conclusions as Marletta. Here ontological relativity in science becomes very hard to defend, if one wants to save other notions such as 'objective truth'. On the other hand, the 'weak' version will not only show how one arrives at the 'strong' interpretation, but also why something like 'objective truth' does not have to be risked.

As my use of Heidegger is heuristic in understanding Kuhn my argument does not lie with what Heidegger 'meant' either. For Heidegger can be viewed as one of those guilty of paving the way for those radical postmodern interpretations of science that

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R. Colodny (Pittsburgh: University of Pittsburgh Press, 1966), pp.41-85; 'Mathematical Ideals and Metaphysical Concepts', *The Philosophical Review*, 69 (1960), 376-385. In Shapere's review of *Structure* he argues that Toulmin had already made most of Kuhn's points. Shapere cites two of Toulmin's books that predate Kuhn's writing, Stephen Toulmin *The Philosophy of Science: An Introduction* (London: Hutchinson University Library, 1953); *Foresight and Understanding: An Enquiry into the Aims of Science* (Bloomington: Indiana University Press, 1961). Fuller speculates that Kuhn's general account in *Structure* and 'innocence of any formal logical apparatus' made it an easy target for critiquing all the inadequacies with a philosophy that does not support the positivistic tradition. Fuller, 'Being There', p.243. Conversely, Kuhn was aligned with the positivistic tradition by the British anti-positivists such as Popper, who saw Kuhn as reducing the essence of the scientific enterprise to puzzle-solving, a process resembling the logical articulation and confirmation of theories.

[78] Kuhn's association with Carnapian logical positivism is argued by George Reisch, 'Did Kuhn Kill Logical Empiricism?', *Philosophy of Science*, 58 (1991), pp.264-277, John Earman, 'Carnap, Kuhn, and the Philosophy of Scientific Methodology', in *World Changes: Thomas Kuhn and the Nature of Science* ed. by Paul Horwich, (Cambridge, Mass.: The MIT Press, 1993), pp.9-36, Michael Friedman, *Reconsidering Logical Positivism* (Cambridge: Cambridge University Press, 1996). Elsewhere Kuhn admits to being a late comer to hermeneutics and the subsequent effect it had on his view of science. Thomas Kuhn, *The Essential Tension: Selected Studies in Scientifc Tradition and Change* (Chicago: The University of Chicago Press, 1977), p.xiii

[79] Marco Marletta, 'Truth and Historicism in Kuhn's Thesis of Methodological Incommensurability', *Kairos: Journal of Philosophy and Science*, 6 (2013), 91-110

[80] Paul Hoyningen-Huene and Howard Sankey (eds.) *Incommensurability and Related Matters* (Dordrecht: Kluwer Academic Publishers, 2001)

[81] Marletta, 'Truth and Historicism', p.92

this thesis looks to discount.[82] To see, however, how Heidegger's thoughts led to my use of him in understanding Kuhn, it will be instructive to the reader to know how his general philosophy sits with regards to science. In chapter 6, there will be a more detailed exposition of the central terms under use in the construction of my 'strong' and 'weak' readings.

For me, a 'philosophy' that is merely a relentless debate about the existence of 'reality' seems unsatisfying as an activity. It would seem more useful to be able to understand what makes such debates intelligible, rather than trying to achieve any real answer to such grand philosophical questions. It will be through Heideggerian inspired ideas that will allow me to create and analyze 'strong' and 'weak' readings of Kuhn. The 'strong' reading deals with a very narrow conception of philosophy and philosophical terms, whereas the 'weak' reading will allow us to expose metaphysical fixations and thus dissolve any antagonistic dualisms that come out of the 'strong' reading. One of these antagonisms is between the 'realist' and 'anti-realist' philosophies of science. Due to lack of space and its only inferred presence in *Structure*, I will not address this debate at any length. Suffice to say that to presuppose realism, which is probably the most commonsense position to take-up, is also to take up implicit metaphysical notions about how experience and language relate to the world. The denial of this is to make ones-self an anti-realist, idealist or instrumentalist. Yet why do they have to be the only other options? In the strictest terms I am a causal-realist, but I have no way of 'proving' this position. For this is to then fall into methodological discourse, which has to presuppose metaphysics that then become sustenance for anti-realist or subjectivist positions. What I am attempting here is to expose the possibilities for just such arguments, which means by force of rigour, evidence and logical argument we can side with the best case, which are all standards of the scientific enterprise.

One of Heidegger's main ideas was the division between being and beings and the priority of 'ontology'. His claim was that we tend to understand our being in terms of beings, that is, the awareness that we are is understood in term of objects, which are

[82] Here we might object to Heidegger's aesthetic reading of Kant or his grand narrative for technology and Western metaphysics. Norris, *The Truth about Postmodernism*, p.242; *Against Relativism*, p.19

themselves not self-aware. For Heidegger, this notion of being is ontologically structured, which is how we relate to own understanding of our history, from which ways-of-being arise. One way-of -being is that developed by the sciences where we try to reduce the understanding of ourselves to neurons firing or an assembly of matter. Where it gets tricky is that he wished to reverse this relationship. Being is not reducible to beings. Beings are only beings because we have being, which also involves the ability to interpret oneself and the cultural history that supports it. In the absence of our being beings would not exist. The tethering of being, beings and existence is crucial for Heidegger. Now to ask what being is, whilst a legitimate question, brings home the extent to which we are 'methodologically' inclined. This is exaggerated more so if Heidegger is treated with naivety, as to what is being expressed.

As it is only people that can encounter objects and can situate things as objects, these acts are worked out beforehand in our tacit knowledge of what 'existence' is (due to us having an awareness of being). However, if we were to vanish, as Heidegger writes, 'what would vanish from the world would be the capacity to uncover entities as existing and as the entities as they are.'[83] Without the world of people, objects neither do nor do not exist, because existence in any meaningful sense requires a world.[84] It is absurd to think that in the absence of people objects would literally disappear, but what would would be our understanding of them, which includes what it is to designate something as a being. As we are in a world, we can reflect upon the possibility of us not being in it and so when one says something like, 'neither does nor does not exist', it appears as a paradox. So it is pointless to talk about electrons in the absence of the practices and ways of being that give meaning to their existence. Heidegger's ontological difference may then be put another way. Being is 'a criterion for meaningfulness' and beings are 'the meaningful thing itself'.[85] This now seem less mystical, but the problems one may have in understanding such statements, without descending into relativism or anti-realism, is where I see an overlap with Kuhn. Another example of how we may be disposed to read something as a methodological

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[83] Stephen Mulhall, *Routledge Philosophy Guidebook to Heidegger and 'Being and Time'*, 2nd edn. (London: Routledge, 2005), p.98
[84] Richard Sembera, *Rephrasing Heidegger: A Companion to Being and Time* (Ottawa: University of Ottawa Press, 2007), p.43

statement is through the logic of something like ‘does and does not exist’. Methodologically we can understand this as objects ‘literally’ existing for one person, but not for another, from which it would follow that there is no objective notion of truth. Understanding a claim as paradox in this sense makes all claims equal, where we get the problems of irrealism or radical incommensurability.[86] Many philosophers and sociologists have used this more radical sounding version to argue against the idea of scientific progress, the authority of scientific claims or show the irrationality of science.[87] Not only has this confusion resulted in a number of spurious ideas about how science is thought to function, but also has resulted in unhelpful readings of philosophers like Kuhn and Feyerabend.[88]

5.8 - Summary

To restate my problem: depending on what my philosophical disposition is or what my worldview is like, not only am I pre-disposed to read Kuhn in a certain way, but I can also find vindication for my beliefs in that text. But surely a selective reading of any person can result in variety of interpretations, a kind of underdetermination by reading. This is true, but as philosophy is not science we are not looking to ‘prove’ a position but expose the possibilities for any such interpretation and thus show some positions to be more tenable than others. Hence, recourse to methodological arguments will only take you so far. For any decent pseudoscience can compete on

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[85] Ibid., p.45

[86] This is kind of conceptual problem is at the centre of Goodman’s ‘many-worlds’ and Davidson and Quine’s critique of it. This debate remains at the level of language and logical description and can be transposed on to Kuhn’s reading of science. To ‘conclude with Goodman that either there is no world at all or else we live in more than one world, or to conclude with Davidson that the phenomena of equivalent descriptions, which we have recognized in science since the end of the nineteenth century, somehow involves a logical contradiction.’ Putnam, *Renewing Philosophy*, p.119

[87] This has to be qualified by what various authors have meant by the term ‘irrational’. Some have aimed at the purely social dimension in which rationality and truth are negotiated constructs; see Barry Barnes and David Bloor, *Relativism, Rationalism and the Sociology of Knowledge* (Oxford: Blackwell Publishers, 1981). Kuhn can be understood as using the term in a similar way, in that what comes to count as ‘science’ is itself not subject to logical analysis. Others have confused the discussion in thinking that if there is an absence of ‘Reason’ that science is not rational. The mistaken argument is that Kuhn was making the claim that science operates in the absence of any justifying cause or belief. John Watkins, ‘Against ‘Normal Science’’, in *Criticisms and Growth of Knowledge*, ed. by Imre Lakatos & Alan Musgrave (Cambridge: Cambridge University Press, 1970), pp.25-38

[88] Alan Musgrave considers the idea that Kuhn ‘the revolutionary never existed – but then it was necessary to invent him’. Alan Musgrave, ‘Kuhn’s Second Thoughts’, in *Paradigms and Revolutions: Applications and Appraisals of Thomas Kuhn’s Philosophy of Science*, ed. by Gary Gutting (London: University of Notre Dame Press, 1980), pp.39-53 (p.51). More explicit denial of Kuhn’s controversial

those grounds by equivocating over terms such as 'evidence' or 'truth'. More to the point, this is what someone like Kuhn seems to be advocating. Yet on a more subtle reading we can bring into doubt both the dominance of the methodological interpretation and the authority of a 'received' understanding of Kuhn.

So why Kuhn? In the literature we do not find the same fuss being made about the logical positivists, empiricists or Popperians, as any attempt to provide a methodological account of truth, or statement-object reference, I would argue, fits with the 'worldview' of most scientists. When 'verificationism' failed, it was replaced with falsification, when that failed it was demoted to a heuristic, at which point it was replaced with structuralism and metaphysical naturalism.[89] Although I am not arguing from an instrumentalist position, I do agree with Bas van Fraassen that any theory of science that is based on formal logic or linguistic analysis cannot work. It is this same 'worldview' that is extended to reading Kuhn that generates the problems one finds. Bas van Fraassen writes, '[p]erhaps the worst consequence of the syntactic approach was the way it focussed attention on philosophically irrelevant technical questions […] solutions to purely self-generated problems, and philosophically irrelevant.'[90] Whilst van Fraassen is typically held up as a representative of 'constructive empiricism', his attempt to move the debate from an endless cycle between realism and anti-realism is still taken as a methodological claim that then becomes embroiled in another dispute about whether 'unobservable' is different to 'unobserved'.[91] Another philosopher who has tried moving the debate on, with some success, bringing the traditions of 'analytical' and 'Continental' philosophy closer together, is Michael Friedman. Friedman has tried to reconcile Carnap's neutralism with the metaphysics of naturalism, here he argues that Carnap's project of using the methods of formal mathematical logic are to embed observable and theoretical phenomena in the ontology of an appropriate mathematical structure. Here we are not concerned with whether objects exist, but whether there exists a corresponding mathematical logical

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readings of science are found in Kuhn, *Structure*, pp.74-210, 'Reflections on my Critics', pp.231–78; *The Essential Tension*, pp. xxi-xxiii and p.293.

[89] There is also a third position between realism and instrumentalism called 'neutralism'. Stathis Psillos, *Scientific Realism: How Science Tracks Truth* (London and New York: Routledge, 1999), pp.38-67. Neutralism comes from Carnap who was a supporter of Kuhn's project as evidence in correspondence between them. George A. Reisch, 'Did Kuhn Kill Logical Empiricism', *Philosophy of Science*, 58 (1991), 264 - 277

[90] Bas van Fraassen, *The Scientific Image* (Oxford: Clarendon Press, 1980), p.56

structure that supports the scientific claim. Friedman tells us that the ontology of these structures is to be found in 'the ongoing practice of modern physics itself.'[92] This ultimate concern for ontology is what maybe links Carnap and Kuhn, rather than the surface similarities with language.[93]

By using Heidegger, I hope to open ways of reading Kuhn and by developing concepts inspired by Heideggerian philosophy, look to see why someone might read Kuhn in the 'strong' as opposed to the 'weak' sense. Next, I will be discussing how the two readings of Kuhn will be constructed and defining those Heideggerian inspired ideas that I will be using in generating those versions. Then, I will go on to give two polar accounts of Kuhn's position in *Structure* that can be read either, as a methodological or historical account. The strong version will argue for the issues of knowledge and language as primary to how science functions. The weak version will argue for historical act as primary. The ability to know which version one is interpreting requires philosophical training, and it is this role that I see for philosophy in a public forum. Moreover, the adjustment in perspective that the methodological and historical accounts require can be brought to bear on the 'problem of demarcation'. This can be expressed best as the difference between the questions 'what is science?' verses 'what does it mean to be scientific?

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[91] Paul Churchland, 'The Ontological Status of Observables: In Praise of the Superempirical Virtues', in *Images of Science: Essays on Realism and Empiricism*, ed. by P. Churchland and P. Hooker (Chicago: The University of Chicago Press, 1985), pp.35-47 (pp.39-40)

[92] Michael Friedman, 'Carnap on Theoretical Terms: Structuralism without Metaphysics', *Synthese*, 180 (2011), 249-263 (p.261)

[93] Kuhn says this in the 'Afterword' to *World Changes*, '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. Part of my embarrassment results from my sense that responsibility required that I know my target better, but there is more.' Thomas Kuhn, 'Afterword', in

Chapter Six

Heidegger's Influence

6.1 – My Route to Heidegger

Before I engage with Heidegger, I will first spell out his methodological importance to my thesis and exactly what I find useful about his work. I will then acknowledge the debates and critical reception surrounding Heidegger's philosophy. Finally, I will address those concepts in Heidegger's work that I have taken and developed as my own for reading and interpreting Kuhn.[1]

Autobiographically, in reflecting on my journey into Heidegger, I realized that I was guilty of the same accusations I have brought against the 'non-literate' scientists and PUoS in their dislike or ignorance of philosophy. Originally my use of Heidegger was to illustrate what happens when philosophy 'gets out of hand'. He was going to be used as an example of the 'postmodern drivel' that passes for philosophy, with seeming contradictions, paradoxes, pseudo-mystical language, and so on. For this is how I came to Heidegger, with prior expectations of radicalism, mysticism and general obscurantism. It was then in taking the time to make sense of Heidegger, knowing that he is frequently cited as one of the 20th century's most influential thinkers, and immodestly asking that maybe he is not all to blame?[2] The breakthrough came when I realized I was not defending what philosophers of any age 'actually' meant, but in reading, making sense of, applying to modern problems and assessing those implication, we have the act of philosophy as a practice. This is what I was defending and trying to promote in a PUoS. An educational process that seemed to be

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World Changes: Thomas Kuhn and the Nature of Science ed. by Paul Horwich (Cambridge, Mass.: MIT Press, 1993), pp.311-341 (p.313)

[1] The pairing of Heidegger and Kuhn is not original in itself. See, Joseph Rouse, *Knowledge and Power: Towards a Political Philosophy of Science* (Ithaca and London: Cornell University Press, 1987); and 'Kuhn's Philosophy of Scientific Practice', in *Thomas Kuhn* ed. By T. Nickles (Cambridge: Cambridge University Press, 2002), pp.101-121. Glazebrook also attempts a bridging of Kuhn and Heidegger but concentrates on Heidegger's analysis of science as the 'mathematical projection of nature'. What Heidegger identifies as 'rigour' Glazebrook tries to relate to Kuhn's notion of scientific commitment to a 'paradigm'. Trish Glazebrook, *Heidegger's Philosophy of Science* (New York: Fordham University Press, 2000), pp.88 – 95

[2] Richard Wolin, 'Preface', in *The Heidegger Controversy: A Critical Reader*, ed. by Richard Wolin (Boston, Mass.: The MIT Press, 1993), pp.vi-viii (p.vii)

deficient in philosophical awareness, rather than demanding that everyone read the same books or follow a certain philosophy. It was also in retrospect that I understood how I came to Heidegger and had already dismissed his work out of hand, which I had accused scientists of, in not bothering to understand Kuhn or the philosophy of science in a charitable way. As I had come to his work already interpreted for me as 'nonsense', which struck me as analogous to what I was arguing against in the PUoS. For I had been arguing that 'science' was already interpreted for people and the means to interpreting through philosophical thinking had been airbrushed out of the picture. So, a simple question like 'what is science?' presupposes a 'what is' that directs our thinking in a particular direction. I even found this present in scientists' own understanding of other scientists. Richard Feynman has been typically held up as pro-science, anti-philosophy scientist, which he is explicit about to a degree, but it appears to be against a very narrow conception of what philosophy is. In promoting his 'philosophy of ignorance', as Feynman calls it he writes, 'hence, what is not surrounded by uncertainty cannot be truth'.[3] These do not seem the words of someone vehemently against philosophical thinking.

It was from reading Heidegger that I have mined some useful conceptual tools. These not only helped me think about why I had that initial response to Heidegger, but it also gave me a means to read *Structure* and understand the divisions in thought it has caused.[4] Also, in reading the philosophers that had been influenced by Heidegger or took issue with him, I noticed similar projects in academics defending philosophers from a particular pre-critical attitude towards their work. By 'pre-critical', I mean an understanding inherited by virtue of the life one has happened to have led, which for me, was 'Heidegger as Nazi', without ever having read Heidegger or had him explained to me. 'Pre-critical' may also include a knowledge that is second-hand as filtered by a tradition, which may make us pre-disposed to read a philosopher a certain way. This, however, does not mean we cannot overcome the traditions we come from, if we allow philosophy to work as a kind of self-undermining practice. So, like my defending a certain reading of Kuhn as being more credible than a radical 'postmodernist' position, I see other academics battling to keep credible readings of

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3 Richard P. Feynman, *Don't You Have Time to Think?* (London: Penguin Books, 2006), pp.xi-xiv
4 Thomas Kuhn, *The Structure of Scientific Revolution*, 3rd edn. (Chicago: The University of Chicago Press, 1996)

other philosophers. Here I have great sympathy with the works of philosophers like Michael Friedman, Christopher Norris, and Pierre Hadot, who look to clarify thought and use philosophy as means to combat ‘sloppy thinking’, not propagate it.[5]

What I found most instructive in Heidegger and *Being and Time,* for reading Kuhn, is the conceptual separation of what I have been calling the historical and methodological perspectives. It was only through reading Heidegger and understanding the consideration we should give to the historical account, such that we always start with being, which is the being of a living person, who is not just alive, but living in a time, with customs, ideas, beliefs, and so on. What I also took from Heidegger is how traditional philosophy has always sought to suppress the priority of being in favour of an abstract account of beings, which I have been calling the methodological view. Where we are able to view the world and ourselves as existing like ‘things’, neutral, indifferent, timeless, and from this state-of-affairs try and work out the abstract rules for all existence or reality. This key notion is expressed in Heideggerian language as ‘ontological difference’.[6] What this mean for us, is that what I have termed the ‘strong’ reading, is to be aligned with the ‘methodological’ perspective, or to start from beings (things). What I have termed the ‘weak’ reading, can be aligned with ‘historical’ perspective, or that of being. As this might sound a little vague it can be understood, as Sembera defines it, as a ‘criterion for meaningfulness’, which is bound to historical-cultural context.[7]

It must be re-stated that I am not offering a theory of science or a philosophy of science but offering a heuristic role for the concepts I have derived from Heidegger’s philosophy. Where philosophy ‘gets out of hand’, is where it sees itself as a competing or completing the methodology of science, as an equivalent discourse. I do not want to offer a ‘Heideggerian’ account of Kuhn or science but only take what is

[5] Christopher Norris, ‘Deconstruction as Not Just a “Kind of Writing”: Derrida and the Claim of Reason’, in *Re-Drawing the Lines: Analytic Philosophy, Deconstruction, and Literary Theory* ed. by R. W. Dasenbrock (Minneapolis, Minn.: University of Minnesota Press, 1989), pp.189-203; *The Truth About Postmodernism* (Oxford: Blackwell Publishers, 1993); Pierre Hadot, *Philosophy as a Way of Life: Spiritual Exercise from Socrates to Foucault* (Oxford: Blackwell Publishing Ltd, 1995); Michael Friedman, *The Dynamics of Reason* (Stanford: CSLI Publications, 2001); ‘History and Philosophy of Science in a New Key’, *Isis,* 99 (1), March (2008), 125-134

[6] Martin Heidegger, *Being and Time*, trans. by J. Macquarrie & E. Robinson (New York: Harper & Row, 1962), p.12

relevant for my purposes, that is, we can either begin with an abstract account that includes people or we can begin with people who create abstract accounts. Nothing mysterious or grandiose, but it helps us consider the implications for our conclusions if we understand where we have started from in our interpretations. These same starting points will also inform ones approach and understanding as to the point and value of philosophy. The sorts of questions asked in philosophy '101' of the type seen in the Platonic dialogues and the sorts of issues considered as 'problems' in philosophy, seem to be a million miles away from the real world. Does anyone really doubt that the chair they are sat on is real? If this is what people take philosophy to be, then it is indeed almost useless as an activity. If, however, this is the study of a subject to show how thinking and problems are constrained by social-historical factors, then it is of use. If we are looking for reasons how a question like 'is the chair real?' even becomes a meaningful problem, we are half way to finding the answer. If, however, we are actually just trying find out if the chair is real or why we can doubt such a proposition, then not much is going to get illuminated, or as the saying goes 'the game is not worth the candle'. What is worth the candle is the new interpretations and understandings that come to bear, not just in what philosophers 'meant', but the activity of philosophy itself, which then opens up news ways of thinking. In my case it has been to develop ideas inspired by Heidegger and construct ways of reading Kuhn and why one reading tends to dominate. The effects of a dominant interpretation means it takes over until it is understood as just fact, which means we are having things interpreted for us, which while not always bad, is a slippery slope to 'thinking' being thought for us. Where on completion philosophy disappears. If we have means for 'thought' to undermine itself, philosophy will always be relevant in creating and producing new ways of understanding. This procedure of getting 'thought' to undermine itself is part of the tradition of deconstruction. Whilst I will not draw exclusively on literary theory or literary criticism, I do find parallel conclusions in the deconstructivist literature, such as literal-metaphor inversion. This will be explained as I construct the two readings.

With the methodological and historical distinction in mind, I wish to reconsider the responses to the PoD. A PUoS that understands the question 'what is science?' from a

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[7] Richard Sembera, *Rephrasing Heidegger: A Companion to Being and Time* (Ottawa: University of

methodological standpoint, will only encounter further philosophical pseudo-problems, either in the logic by which science proceeds or how unfalsifiable we are allowing our assumptions to be.[8] Rather, if we take up the historical stance and ask 'what does it mean to be acting scientifically?' such that all actions of scientists are embedded in a given culture and location in time, we find that such problems do not arise. What is more, if we wish to push further with the interpretation, it is in the historical analysis that we find the conditions for such philosophical problems. For science is whatever scientists do; as Susskind concisely puts it, methodology 'is conditioned by, and determined by, the science itself and the scientists who create the science.'[9] Or as Feynman says, 'to find out *ab initiio* (sic), again from experience, what the situation is, rather than trusting the experience of the past in the form in which it was passed down.'[10] These, however, tend to occur only at times of revolutionary science where science is 'inevitably dependent upon philosophical questioning'.[11] This is not to say that the methodological approach is wrong, for it is definitely the best way of organizing routine scientific practice, but asking and understanding what science *is* is not doing science. We only constrain our own ability to interpret if we give credence to only one view, which tends to think about science as it is organized, that is, methodologically.

Next I will briefly consider the reception of Kuhn's *Structure*. It is in the reception and criticisms it received that we may also start to uncover the conditions for what I have called the 'strong' and 'weak' readings.

6.2 - From Kuhn to Heidegger

What I will develop as the 'strong' reading in chapter 7 can be recognized by a number of characteristics, which conclude that Kuhn's leading ideas, were either

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Ottawa Press, 2007), p.45

[8] As seen with the Smolin-Susskind debate. See pp.103-105 in thesis

[9] Lee Smolin and Leonard Susskind, "Smolin vs. Susskind: The Anthropic Principle", *Edge: The Third Culture*, 18 Aug, 2004, <http://www.edge.org/3rd_culture/smolin_susskind04/smolin_susskind.html> [accessed 09 Dec 2012]

[10] Richard Feynman, 'What is Science?' in *The Pleasure of Finding Things Out: The Best Short Works of Richard P. Feynman*, ed. by Jeffery Robbins (London: Penguin Books, 2001), pp.171-188 (p.185) [Italics in original]

[11] Joseph Rouse, 'Heidegger on Science and Naturalism,' *Division I Faculty Publication*, 36 (2005), 1-22 (p.6) <http//:wesscholar.wesleyan.edu/div1facpubs/36> [accessed 8 February 2012]

absurd, contradictory, trivial or just wrong. Shapere, in the first review of *Structure*, was highly critical of the idea of 'paradigms' and their inability to be formally expressed.[12] Scheffler gives an eleven-point critique of Kuhn's ideas and argues against the metaphors of psychological, political and religious conversion.[13] Indeed, it is not hard to see why Kuhn generated these responses. For he worked at a time when industrialized 'Big-Science' was becoming the standard model for practicing scientists.

Not only was *Structure* not well received by scientists, but also by fellow philosophers of science, describing it as 'unoriginal, dry and confused.'[14] After this first round of academic exchange, a more considered voice was raised in his defense. Though there is vagueness with Kuhn's terminology and use, what was taken to be the most persuasive parts of *Structure* were considered both 'obvious and insignificant'.[15] To read *Structure* as a methodological statement about science would prompt these responses. If we take methodology as only being able to tell us what we can know with any certainty, given certain methodological constraints – it is then implicitly tied to epistemology. Once our discussion starts with epistemology, we are then on a sliding scale of how knowledge makes contact with the world. The stronger the reading of Kuhn, the more bizarre the conclusions that can be offered. It was this image of Kuhn that people either rallied against or endorsed, but did so from a pre-existing tradition. In Britain, Kuhn's rejection of falsification as a means of scientific progression was regarded as an anti-positivistic challenge. On the other hand, with Kuhn's endorsement from Carnap, one can read his efforts completely in line with the logical positivists, such as an informal development of the relativized a priori.[16] For others, Kuhn's apparent relativistic take on the truth of paradigms is widely applicable.[17] Still today, Kuhn the radical, relativist or the irrationalist can be found in

[12] Dudley Shapere, 'The Structure of Scientific Revolutions', *The Philosophical Review*, 73 (1964), July (3), 383 - 394 (p.393)
[13] Israel Scheffler, *Science and Subjectivity*, 2nd edn. (Indianapolis: Hackett Publishing Company, 1982), pp.125 – 138
[14] Ziauddin Sardar, *Thomas Kuhn and the Science Wars* (Cambridge: Icon Books, 2000), p.32
[15] Richard J. Bernstein, *Beyond Objectivism and Relativism: Science, Hermeneutics and Praxis* (Philadelphia: University of Pennsylvania Press, 1983), p.51
[16] Steffano Gattei, *Thomas Kuhn's "Linguistic Turn" and the Legacy of Logical Empiricism: Incommensurability, Rationality and the Search for Truth* (Aldershot: Ashgate Publishing Limited, 2008), p.179
[17] For differing degrees of this compare, Wolfgang Stegmüller, *The Structure and Dynamics of Theories* (New York: Springer, 1976); Gerald Doppelt, 'Kuhn's Epistemological Relativism: An Interpretation and Defense', *Inquiry*, 21 (1978), 33-86; Ian G. Barbour, 'Paradigms in Science and Religion', in *Paradigms and Revolutions: Applications and Appraisals of Thomas Kuhn's Philosophy*

academia and deployed by groups who may have an vested interest in undermining scientific authority.[18] For with not much effort or training in philosophy, one can take from Kuhn a face-value reading of a concept like ‘paradigm’. This can then be translated ‘into a range of roughly equivalent ideas which exploit its [paradigm’s] more extreme cultural-relativist implications while ignoring (or dismissing) those aspects of his theory that resist such assimilative treatment.’[19] Ultimately it is not the concepts that get exploited, but the people who uncritically take on second-hand accounts of what Kuhn ‘meant’.

Structure was, in part, written out of a reaction to what Kuhn saw as an anachronistic reading of past thinkers and scientists. The story of science as told by ‘textbook’ science education and by a certain philosophical tradition, gave the impression that science was a linear progression, from one great discovery to the next, usually by individuals gaining closer access to the ‘Truth’.[20] Kuhn’s own ideas on the development of science were galvanized when he studied the European schools of thought, that had a tradition of treating ideas in their own historical context. Kuhn begins *Structure* by framing some of his ideas within established neo-Kantian traditions of people like Koyré and Meyerson.[21] Kuhn, himself, notes that there are ‘philosophical implications’ for *Structure*’s historical orientation, but those implications are double edged.[22] He first claims that any scepticism detected in *Structure* is directed towards a ‘philosophical attitude’ rather than ‘any one of its fully

of Science, ed. by Gary Gutting (Notre Dame and London: University of Notre Dame Press, 1980), p.236

[18] David Penny calls Kuhn ‘one of the two leading anti-‘science’ writers of the twentieth century.’ David Penny, “Biology and Physics Envy”, *EMBO Reports*, 6 (2005), 511 <http://www.ncbi.nlm.nih.gov/pmc/articles/PMC1369106/> [accessed 7 Oct 2012]. Elsewhere Kuhn has been used to deny or challenge global warming claims, particularly by global warming sceptic and climatologist Patrick Michaels. See, *Meltdown: The Predictable Distortions of Global Warming by Scientists, Politicians, and the Media* (Washington: Cato Institute, 2004), p.221; *Climate of Extremes: Global Warming Science They Don’t Want You to Know* (Washington: Cato Institute, 2009), p.39; and Ross McKitrick ‘Bias in Peer Review Process: A Cautionary and Personal Account’ in *Climate Coup: Global Warming’s Invasion of Our Government and Our Lives*, ed. by Patrick J. Michaels (Washington: Cato Institute, 2011), pp.69 - 96 (p.69). The majority of this work being generated by the Cato Institute, an independent ‘think tank’ that is privately funded.

[19] Christopher Norris, *Against Relativism: Philosophy of Science, Deconstruction and Critical Theory* (Oxford: Blackwell Publishers, 1997a), p.82

[20] Kuhn, *Structure*, p.vii

[21] Ibid., p.viii. Norris notes Koyré’s and Bachelard’s bridging effect between the philosophical traditions, which my treatment of Kuhn will attempt to echo. Christopher Norris, *Epistemology: Key Concepts in Philosophy* (London & New York: Continuum, 2005), pp.80-91

[22] Kuhn, *Structure*, p.xii

articulated expressions.'[23] What is not articulated, and which I hope to elaborate on, is the 'philosophical attitudes' of scientists and the metaphysics underlying those commonsense notions of 'Truth', 'Reason', and 'Objectivity', rather than trying to undermine or prove any one philosophical, historical or scientific account.

From a mixture of Kuhn's lack of philosophical training, and working between Anglo-American and European traditions from the history of science, we get a tension between the methodological and historical in *Structure*. The 'weak' reader of Kuhn will tend to empathize with the historical view more, where *Structure* is understood as revealing science to be a historical phenomenon that does not conform to its methodological descriptions. Mary Hesse, for example, in her review of *Structure* said that, 'one can find many ideas typically ascribed to Kuhn and *Structure* in well-known scholarly work', which range from philosophy of science to historiography to sociology of scientific knowledge.[24] Hesse claims that *Structure*'s main contribution is the 'historical pattern' it draws, but I think that his 'pattern' can be different for each of us due to our prior understanding of ideas or the interpretations we inherit.

Whilst the use of Heidegger for me is purely heuristic in reading Kuhn, I will still have to acknowledge the reception and problems Heidegger has generated. If this were a thesis on Heidegger it is arguable whether I could achieve my aim in opening up the interpretations of science, or at least recognizing why we have the ones we do. For 'meaning' in Heidegger can be to return 'back to its proper, self-identical core' in being.[25] I would say, though it is not my aim to prove it here, that just like Kuhn, Heidegger too suffers from a 'strong' and 'weak' reading. I will only allude to this in the background literature and the variety of responses he has received, before staking out those concepts that I will be applying to *Structure*.

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[23] Ibid.,

[24] Juan V. Mayoral, 'Five Decades of *Structure*: A Retrospective View', *Theoria*, 27 (2012), 261-280 (p.262); Mary Hesse, 'Review of The Structure of Scientific Revolutions', *Isis*, 54 (1963), 286-287

[25] Christopher Norris, *Deconstruction: Theory and Practice*, Revised edition (London & New York: Routledge, 1991), p.70

6.3 - The Critical Reception of Heidegger

It could be viewed as poor methodology to use an example that is regarded as being more complex, unintelligible, or obscure than the thing I am trying to illuminate. The worry is that I become 'lost' in Heidegger or slip unaware into Heideggerian-neologisms, assuming they make sense.[26] Just the language alone is daunting as Mulhull writes Heidegger's German rendered into English makes for a 'tortured intensity of prose'.[27] Yet we do not stop at something just because it is hard. In looking at where Heidegger came from, when he was writing, who he was addressing, why he uses the phrases he does and the subsequent path his work took into the English speaking world, a story can be told that makes sense of the reputation he has gathered. It was from this reputation that Heidegger had figured in my pre-critical understanding of him. For example, Edwards takes Heidegger to task on his apparent failure to grasp Russell's 'existence is not a predicate' argument.[28] Yet, any time spent reading Heiegger and his surrounding works, we see that he not only was well acquainted with the problem in Aristotle, but also in Kant's original formulation. Moreover, logic was a major component of Heidegger's doctoral thesis, along with mathematics and medieval history.[29] So, regardless of whether he is right or wrong, a simple dismissal of Heidegger and his ability to grasp the logic of the argument seems a little premature. This leads Edwards to say that Heidegger's faulty understanding of being is a 'pseudo-inquiry and his quest a non-starter.'[30] Edwards' attitude is not uncommon in the literature. John Searle says, 'most philosophers in the Anglo-American tradition seem to think that Heidegger was an obscurantist muddle-head at best or an unregenerated Nazi at worst.'[31] Heidegger's Nazism tends to be the 'elephant in the room', but for my purposes his political and moral convictions play

[26] 'Being' for Herman Philipse is 'a methodologically muddled blend of hackneyed cultural and feeble conceptual analysis' in Taylor Carman, 'On Making Sense (and Nonsense) of Heidegger', *Philosophy and Phenomenology Research*, 63 (2001), 561-572 (p.561). Mehta tells us that Heidegger 'cannot be interpreted or truly understood, only used.' J. L. Mehta, *The Philosophy of Martin Heidegger* (London: Harper and Row, 1971), p.6 [In footnote]
[27] Stephen Mulhall, *Routledge Philosophy Guidebook to Heidegger and 'Being and Time'*, 2nd edn. (London: Routledge, 2005), p.viii
[28] Paul Edwards, *Heidegger's Confusions* (Amherst, NY.: Prometheus Books, 2004), pp.38-40
[29] John van Buren, *The Young Heidegger: Rumour of the Hidden King* (Bloomington: Indiana University Press, 1994), p.53
[30] Edwards, *Heidegger's Confusions*, p.37
[31] John R. Searle, 'The Limits of Phenomenology', in *Heidegger, Coping and Cognitive Science: Essays in Honour of Hubert L. Dreyfus*, ed. by Mark Wrathall and Jeff Malpas (Cambridge, Mass.: MIT Press, 2000), pp.71-92 (p.71)

no more of a role than Feyerabend's past involvement with Nazism did. What is more it goes to show that no person is above their cultural-historical situation. Blattner says that Heidegger's 'turn',

> away from traditional philosophy and its logical methods makes Heidegger almost unintelligible to mainstream academic philosophers trained in traditional philosophy, especially those reared on formal logic and the generally empiricist epistemology that is standard fare in the world of English-speaking philosophy.[32]

Wrathall says, 'philosophers with analytical sensibilities often see Heidegger as a throw-back to the bad old days of metaphysical (i.e., unscientific) speculation – an appearance only heightened by his seeming inability or refusal to make clear, logical, analytical arguments.[33] Heidegger's reception as a champion of anti-science, speculative, big question philosophy has been attributed to Werner Brock, who first published the English translations of Heidegger's work.[34] Heidegger can be considered problematic for a certain population due to his assertion that 'being' is not based in propositional, logical, or representational modes. Most Heidegger scholars refer back to him either as part of early or later bodies of work. The early Heidegger, under influence from Husserl, argued for philosophy as an 'absolute' science, whereas the later Heidegger rejected this position as absurd.[35] Heidegger, near the end of his career, turned to art and poetry, which has been regarded as belonging to his 'mystical' turn.[36]

For all the negative criticism of Heidegger (rightly or wrongly), he is still massively influential in his own right, as a thinker. His ideas impacting way beyond philosophy into fields such as, architectural theory, cultural criticism, literary criticism, theology, psychotherapy, and cognitive science. Just to show that Heidegger does not escape from the same sorts of intepretation problems found in Kuhn, without getting too involved in Heidegger scholarship, we see in Glazebrook's reading of Heidegger's

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[32] William Blattner, *Heidegger's Being and Time: A Readers Guide* (London: Continuum International Publishing Group, 2006), p.175
[33] Mark Wrathall, *How to Read: Heidegger* (London: Granta Books, 2005), p.2
[34] Ibid., p.3
[35] Vincenzo Crupi, 'Reframing Heidegger and Science', *Human Studies*, 26 (2003), 133-139 (p.134)

thoughts on science 'as a philosophy of science.'[37] Whereas, William J. Richardson argues that Heidegger 'offers no philosophy of science'.[38] Indeed, 'on the longest day he ever lived, Heidegger could never be called a philosopher of science.'[39] This division of reading extends throughout the literature on Heidegger, from the emphasis he placed on practice over theory, or the body over the social, to his conception of 'truth'.[40] Blattner gives a two reading account of Heidegger's rejection of the realist-idealist debate. One reading he calls 'deflationary' and the other the 'robust' reading.[41] Whilst the reasons for these interpretations could occupy a thesis in itself, I am just letting it be known that any recourse to what Heidegger 'actually' meant is not relevant for me.

6.4 - The Post-Positivistic Philosophy of Science

Where Heidegger and Kuhn have shared ground is in the post-positivist philosophy of science, through works of philosophers like Caputo, Heelan, Kockelmans and Rouse.[42] This relationship and direction of influences are not necessarily in chronological order either.[43] At the same time there have been substantial works on Heidegger that fail to include a single essay on his thoughts about science.[44] More

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[36] John D. Caputo, *The Mystical Element in Heidegger's Thought* (Ohio: Ohio University Press, 1990)
[37] Trish Glazebrook, *Heidegger's Philosophy of Science* (New York: Fordham University Press, 2000), p.13
[38] William J. Richardson, 'Heidegger's Critique of Science', *The New Scholasticism*, 42 (1968), 511-36 (p.534)
[39] Ibid., p.511
[40] Hubert L. Dreyfus, 'Holism and Hermeneutics', *The Review of Metaphysics*, 34 (1980), 3 – 23 (p.11); Diego Marconi, 'On The Mind Dependence of Truth', *Erkenntnis*, 65 (2006), 301-318 (p.302) Harry Collins, 'The New Orthodoxy: Humans, Animals, Heidegger and Dreyfus', in *After Cognitivism: A Reassessment of Cognitive Science and Philosophy*, ed. by Karl Leidlmair (Dordrecht: Springer, 2009), pp.75-85 (pp.79-80)
[41] Blattner, *Heidegger's 'Being and Time'*, p.114
[42] Among other notable scholars John D. Caputo, 'Heidegger's Philosophy of Science: The Two Essences of Science', in *Heidegger on Science*, ed. by Trish Gazebrook (Albany: State University of New York Press, 2012), pp.261-280; Patrick Heelan, 'Towards a Hermeneutics of Science', *Journal of the British Society of Phenomenology*, 3 (1972), 85-93; Joseph J. Kockelmans, 'Phenomenology and the Theory of Science', *International Studies in Philosophy*, 7 (1975), 207-208, *Heidegger in Science* (Washington: University Press of America,1985); Joseph Rouse, 'Kuhn, Heidegger and Scientific Realism', *Man and World*, 14 (1991), 269-290
[43] It has been argued that *Structure* 'helped explain the undeveloped notion of rationality in *Being and Time* and in Merleau-Ponty's *Phenomenology of Perception*.' John D. Caputo, *Radical Hermeneutics: Repetition, Deconstruction, and the Hermeneutic Project* (Bloomington: Indiana University Press, 1987), p.214
[44] Hubert L. Dreyfus and H. Hall, *Heidegger: A Critical Reader* (Oxford: Blackwell, 1992); Charles B. Guignon, *Cambridge Companion to Heidegger* (Cambridge: Cambridge University Press, 1993)

recently, the work of Ginev has explored the theme I wish to take up with Kuhn in addressing two accounts of how he can be read. His analysis is exclusively of Kuhn's notions of 'normal science'. The two readings he calls 'the tradition-bound and framework reading of normal science', which 'are not only two opposite philosophical interpretations of Kuhn's ideas, but two conflicting tendencies in Kuhn's thoughts.'[45] Elsewhere, Ginev has identified two general readings for interpreting science, one he calls 'knowledge construction', which can be loosely associated with my strong reading, and 'constitution of meaning', which would fit with my weak reading.[46] Ginev through his take on Kuhn's 'normal science' gives form to my two readings of Kuhn. He says, 'the paradigm of science-as-practice as opposed to the paradigm of science-as-knowledge was initiated precisely by Kuhn's elaborations on the notion of normal science.'[47] It is by using Heidegger that I wish to push these divisions further throughout Kuhn.

One of the methodological legacies Heidegger left the philosophical community was the project of 'deconstruction'. This for Heidegger was a way of doing philosophy and we find in post-positivistic philosophy of science and sociology of science this general theme of separating knowledge from the practices that constitute the meaning for knowledge. In De Vries similar ontological account of Wilfred Sellars' work, writes 'the idea that some form of scientific image of the world is possible independently of the framework of persons, which could then be "joined" to it, has lost sight of the fact that science is primarily a human activity.'[48] I see my methodological/ historical divide as fitting those categories, with knowledge on the one hand and practice/ human activity on the other. This is a means to yield interpretations that may help us think about science and philosophy. So those that have already given a Heidegger-Kuhn account become more instructive, once we acknowledge the traditions those readings come from. For example, Dreyfus gives a particular 'Anglo-American' reading of Heidegger, but also draws the comparison

[45] Dimitri Ginev, 'Re-reading Normal Science', *Crítica*, 35 (2003), 65-89 (p.73); These distinctions are further developed in *The Context of Constitution: Beyond the Edge of Epistemological Justification* (Dordrecht: Springer, 2006), pp.110-123
[46] Dimitri Ginev, *The Tenets of Cognitive Existentialism* (Ohio: Ohio University Press, 2011), p.xix-xxiv
[47] Ginev, 'Re-reading Normal Science', p.67
[48] Willem A. de Vries, 'Ontology and the Completeness of Sellars's Two Images', *Humana Mente: International Journal of Philosophical Studies*, 21 (2012), 1-18 (p.13)

with Kuhn. He says 'Heidegger provides a sophisticated account of science which, like Kuhn's, emphasizes the role of scientific skills and theory in producing data, but, unlike Kuhn's, still leaves room for scientific realism.'[49] I am not sure that it is obvious Kuhn does not leave room for scientific realism, or that Heidegger was arguing for realism in scientific claims. It is in how someone can understand the claim that Heidegger or Kuhn was neither realist nor anti-realist that we find one of the uses of philosophy. It should not be our first reaction to dismiss it as nonsense until it has either collapsed under the weight of its own claim, or the rhetorical devices that allow a claim to pose as meaningful or profound are turned inwards in an effort to undermine itself. The fact that Heidegger never achieved his aim of answering the question concerning the meaning of being is not relevant for me. In his effort, however, to answer this question, he developed a number of methodological tools and conceptual devices, that I will modify and apply to Kuhn, presenting ways to understand him. Whilst this could be a general claim, I hope to show that how one understands Kuhn will also impact on how one regards the worth of philosophy for science, and more importantly, PUoS. Next, I will describe the concepts I will be using as inspired by Heidegger. I will first explain what I have taken to be of relevance from Heidegger and how I have adapted them for my own purposes for constructing a 'strong' and 'weak' reading.

6.5 - Ontological Difference: Methodological and Historical Interpretations

A crucial distinction in *Being and Time* is the difference between beings and being.[95] In Heidegger's language the sciences make up a practice that is ontical, if it investigates beings, whereas an investigation that considers 'being' is ontological. This difference between beings and being is known as 'ontological difference'. To regard something as a 'being' (object) it requires us to take up an abstract relationship with it, that we might view it in terms of properties, categories, or attributes. The ability to take up either an ontical or ontological relationship with the world and ourselves is expressed by 'Dasein'.[50] The putting of 'beings' first we can align with

[49] Hubert L. Dreyfus, Being-In-The-World: A Commentary on Heidegger's Being and Time, Division I (London: The MIT Press, 1999), p.82

[95] I will not be capitalizing 'being' as this tends to imply something special or metaphysical about it.

[50] Dreyfus notes that there have been problems in the conceptualisation of 'Dasein' in other philosopher's work, such as, an extension of Husserlain phenomenology in which the conscious subject

the methodological interpretation of science, or what I have also called the 'strong' reading. Being, on the other hand, is always the being of a person. So, to be a scientist is to be involved in a way of acting or conducting oneself. As this happens in a world that is historical, such that the world informs or makes intelligible (criterion of meaningfulness) ways of acting, the being of a scientist is connected to history. This we can align with the historical interpretation or what I have also called the 'weak' reading. Whilst my interpretation of Kuhn does not depend on ontological difference in the sense that Heidegger means it, 'being' and 'beings' are handy distinctions with which to organize the following concepts around.

6.6 - Fundamental and Regional Ontology

In Heidegger's philosophy being is ontologically fundamental and is primary to beings, which occupy regional ontology. Fundamental ontology is rooted in ontological difference, that is, the awareness of being. Another way of expressing fundamental ontology is how we relate to own understanding of our history, from which ways-of-being (ways of acting towards the world) arise. For me, fundamental ontology can be aligned with something like 'tacit understanding', where we already have a sense of our world, which is beyond explicit formalization. Regional ontology is what traditional philosophy has normally taken to be ontology, in that it refers to 'what there is', and seeks explicit formalization of it. As the strong reader starts from this as a meaningful statement, its origins are in epistemology and language. For me, it will be this methodological view that situates regional ontology 'as if' it were fundamental ontology and thus looks to ground its own account as being fundamental to the activity of science. Here, we can only account for the activity and progress of science in terms of the reality it describes. What is more, for the strong reader we only get something like the historical account because 'truth' only finds its 'real' expression under the methodological account. This has been one of the criticisms of the sociology of scientific knowledge, in that, how can a second-order activity be 'more true' than the first order-activity it looks to critique? How can the constructivist take on scientific truth be 'more true' than the facts it looks to undermine? Ultimately,

is central. Dreyfus claims that Sartre and Føllesdal make the mistake of portraying Dasein as an autonomous individual subject whereas Haugeland argues for a Dasein as a collective term. Dreyfus, *Being-in-the-World*, p.14

because the strong reader starts from a position of regional ontology, it will leave unacknowledged a metaphysics that makes sense of its epistemology, that is, the presumed model of how language, knowledge and reality all relate.

The weak reader starts from a position of fundamental ontology or tacit awareness, which requires a world for this to be the case. It will be through tracing the historical developments of science that we see how science cannot be grounded in regional ontology or methodology, as it is this that comes under challenge when scientific breakthroughs are made. We see in the history of science that every methodological rule for conducting science is broken or ignored at some point. We also see how the background assumptions (what was taken to be fundamental to scientific description) are challenged in revolutionary science and become open to scientific inquiry (become part of regional ontology). Yet, science can be conducted in the absence of these supposedly fundamental ideas, as it is the experiences and explanations of the past that scientists are looking to overcome in generating new knowledge. Heidegger argued that, systematized knowledge seeks to cover over the ontological origins of its own practices, which has extended to the very question concerning the meaning of being.[51] The similarity I would like to draw from this is that, the 'strong' reading seeks to establish itself as the 'real' version of science, where knowledge is a by-product of following simple methodological rules, such as checking theory with observation against the evidence. As we are starting at the level of regional ontology and epistemology, we can re-describe events as the product of the incommensurability of theories, meaning variance of terms, or changes in psychological perspective. This, in turn, has given rise to the research agenda for the past thirty years in sociology of science, philosophy of science, and science and technology studies.

Two more Heideggerian concepts that stem from ontological difference are those of 'categorical' and 'lived' space.

6.7 - Categorical and Lived Space

There are two types of space that can be discerned in *Being and Time*. When Heidegger speaks of 'being-in-the-world', we might think that this means a spatial location, such as tea in a mug, but this is to be thinking 'methodologically'. Because we have ontological difference and tea mugs (as far as we know) do not, we exist in a different way to all objects. We are in the world in a unique way. Yet, it is perfectly possible to treat people 'as if' they did exist in this way. We may treat people through taxonomic, ethnic or gender categories, in order to say 'what' they are. In order to frame a person as an object or a 'what', science represents people in 'categorical space'. 'Categorical space' is the abstract notion of space, which can take the form of co-ordinates on a map, geometrical representation or topological description. According to Heidegger, we do not primarily experience the world in this way, but it has been the quest of science and traditional philosophy to understand our existence in exactly these terms. This is to understand ourselves and the world around us, in terms of 'beings', part of which, is to understand abstract categorical space as being more fundamental than real, lived experienced space. 'World' here also takes on a specific meaning, such that we might refer to 'nature' or the planet Earth. To understand a person as occupying categorical space is to have them like the tea in the mug. Here they have properties, which exist in a specific location and time. Yet, to understand 'in' in this sense is part of the tradition of epistemology, in which we can categorize and quantify the world around us. When we do make scientific statements they are expressed as part of categorical space. It is the methodological view of science that takes this way of expressing the world as primary, as it is necessary to the organization of science.

The other way we can understand the 'in' of 'being-in-the-world' is not as an object in a physical location, but as a part of 'lived spaced'. [52] When we say 'I am in a bad mood', 'in trouble', or 'in love' the 'in' does not refer to a spatial location. Since this is not a spatial location, the 'world' is not one either. This may be a tricky idea to grasp, the world as a non-thing, but these two ways of understanding the world are crucial for my interpretation of Kuhn. The 'world', in the ontological sense for

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[51] Heidegger, *Being and Time*, p.15

Heidegger, was bound to 'being-in-a-world'.[53] It is only people that have a world as people exist in a way that no other entity does. 'World' in this sense refers to a way-of-life or activity, such that we might talk about the academic, financial or sporting world. The 'world' here does not refer to the planet Earth or 'nature'. To repeat, we exist in a way different to all other things due to ontological difference. We differ by virtue of the fact that our own existence is a question or concern for us. This can only be recognized in light of a world that structures our ways-of-being towards it. So the world, in this sense, is not a thing but a system of relations which we come to understand by our involvement with it.

The methodological view around which we organize science has brought about a particularly successful way of understanding the world, culminating in things like 'objectivity'. It is in opposition to this that all other ways of understanding the world have been placed. So any statement about being 'in love' becomes a subjective statement referring to an internal psychological condition. Yet, Heidegger would want to say that these expressions of 'being in love' or 'being in turmoil' refer, ultimately, back to our being (a fear of death), which is a lived relation between the world and our awareness of being. Here 'in' is not a thing inside another thing, but refers to our familiarity with the world or the world as we 'feel' it. Being in love or in exile refers to a world that makes these possibilities real for us and indeed other possibilities unlikely, for they are historically and culturally contingent.[54] How I am as a human is thus a part of 'lived space', so to be an ancient Egyptian Pharaoh was a real possibility 3,000 years ago, but it refers to a specific way of life that is now no longer meaningfully possible. 'Lived space', however, is made up of activities and practices, by people's involvement with the world that structures actions as meaningful.[55] Now other possibilities exist for ways-of-being, such as our digital extended selves that would have been a meaningless pre-Internet. Due to 'lived space' having a historical and cultural component, I place it with the historical view in contrast to the categorical space/ methodological view. It is the categorical/ methodological that tells us 'what' we are and it is the lived/ historical that tells us 'who' we are. The problem for Heidegger was that people often misunderstand themselves in terms of the world

[52] Ibid., pp.53-54
[53] Ibid., p.12
[54] Ibid., p.144

or they mistake the 'what' for a 'who'.[56] Who I am is what I am. Yet, Heidegger wanted to say that it is precisely the other way around. We first have a sense of who we are as we are born into a world that is already interpreted as meaningful for us and we then learn to see it as a 'what' or something more abstract. To place the categorical/ methodological as more important than lived/ historical, as a ways of experiencing the world, is to confuse an ontic description with an ontological condition. That is, as I imply a world, I can understand myself in terms of categories, properties, and attributes. This is an ontic statement. To say that my world fell apart after my dog died refers back to myself and how I am in my world. Not only can we confuse what I am as part of who I am, but the ways the 'world' is conceived in both these descriptions also become confused. There is the 'world' as a thing or object (categorical/methodological) and there is the 'world' as a non-thing or system of relations, that structure our experiences of who we are. Theses two ways of understanding the world are significant for our readings of Kuhn, for they can be related to how we understand terms like 'paradigm' or 'world'. For Kuhn, a description of the world that involves categories, properties, and attributes is called a 'paradigm'. Yet, due to our tendency to understand the world in the methodological sense, we mistake 'paradigm' (what nature is) for the world (a non-thing) when reading Kuhn. To put it another way, the strong reader will collapse the ideas of paradigm (regional ontology) onto world (fundamental ontology), where as the weak reader will not. For the weak reader, the 'world' is what we have a tacit understanding of as part of fundamental ontology, whereas 'paradigm' is what our regional ontology is. Paradigms tell us 'what there is' at any one point in history. The world, however, is not a thing, but something that gives structure to our interactions with other beings. Where the strong reader collapses these distinctions, the weak reader keeps them separate. The very fact that how we describe reality has to change in order for knowledge to advance means there is never a perfect one to one mapping of what the world is and what our paradigms say about it. Some paradigms used to contain phlogiston and aether, but now they contain dark energy and matter. It is not that to understand nature or oneself in terms of a paradigm/ regional ontology is wrong, as we are human, bipedal, mammals and without such a mode of inquiry science could not be performed. It is that to *only* think about ourselves and the world in such a way

[55] Sembera, *Rephrasing Heidegger*, p.45

is a mistake. That is to say it may be the best way for *doing* science but not necessarily the best way for thinking *about* it.

'Lived space' is what pre-exists 'categorical space', as we are always already in a world which we tacitly understand. We come into a world that is already meaningful, but metaphysically the strong reader wishes to place the origins of that meaning or intelligibility in categorical space. Raymond Tallis gives an account of these two notions of space in discussing 'facts', and also the conditions for either a strong or weak reading depending on the emphasis we use. Whilst we use the term 'fact' as a shorthand for 'what there is' we are actually committing to a metaphysical idea of 'what there is'. He says a fact is 'not something like an object that is simply 'there''.[57] A 'fact', he says, is dependent on how we notice the world and how we choose to divide it up. So even on an everyday level my bedroom has the possibility for a number of facts, but that possibility is constrained by the world I occupy, or what I am allowed to acknowledge as being 'there'. Some get confused here between social and natural facts, arguing that a chair might not be a chair in another culture, but neither can deny it as an object. It is this way of thinking about things that comes from the methodological interpretation. For in order to see just an 'object' one has to abstract the chair (something meaningful) into categorical space, so in the absence of culture or the vagaries of language we can objectively state there is at least one fact. Tallis says 'facts are the progeny of a three-in-a-bed between my consciousness, my language (and the habits of noticing and dividing dictated by my language), and whatever is intrinsically there, independent of my awareness and descriptive habits.'[58] Depending on which element in this tripartite we choose to emphasize we then situate ourselves as a 'strong' or 'weak' reader. For the strong reader it is our consciousness of what is intrinsically there (our consciousness is about something) that guides our language. The weak reader starts with our habits of noticing, that is informed and made meaningful by the world, which enables us to abstract the ways we are in the world so we can have something like 'consciousness', or make a statement like 'intrinsically there'. It is these same approaches that can either make sense or a nonsense of a question like 'what is science?' As we saw with Popper or any

[56] Dreyfus, *Being-in-the-World*, p.42
[57] Raymond Tallis, *The Kingdom of Infinite Space: A Portrait of Your Head* (London: Atlantic Books, 2008), p.263

philosophical criterion of demarcation, there is the implosion of science as it tries to account for itself methodologically. It tries to give the 'what there is' of a practice whose aim is to say 'what there is'. Heidegger expresses this implosion as 'no science can know from itself about its own fulfilled form of knowing. We cannot reflect on physics, as a science, with the help of the procedures of physics.'[59]

There are additional Heideggerian concepts that I will need to describe, and which I have already alluded to, which are those of the 'ready' and 'present-to-hand'. I will then explain these distinctions through the fundamental difference that I call 'about' and 'of' languages.

6.8 - 'About' and 'Of' languages

For Heidegger how we get at the structure of the world and fundamental ontology is through how we relate and use the world around us. Being is a kind of activity of existing. Existing and being in this sense implies a world (a non-thing). There are for Heidegger two ways of relating to the world that reveals its structure, the 'present' and 'ready-to-hand'. Heidegger argues the origin of theorizing for the scientific enterprise is in the present-to-hand. It is an abstract relationship that allows us to distance ourselves from the world and to reflect upon it as a thing 'as if' we were absent from it, or existing in it like everything else. This is the essence of the methodological approach, that the world can be reducible to objects, categories, properties, and so on. Once we have a completely faithful description of the world, we have scientific reality, which tells us what there is. What the strong reader does not acknowledge is that this is a metaphysical view. 'Reality' *is* somehow this abstraction. The other way of relating is called the 'ready-to-hand'. For Heidegger the 'ready-to-hand' is our ordinary state of being. It describes how we are when we are involved or absorbed into the world in our everyday lives. Normally, we are active in the world, either trying to achieve something, get somewhere, or do something. In these instances the things we are involved with (our worlds) become transparent to us, we are not aware we are using or occupied with it. Here we are not inspecting the world,

[58] Ibid.,
[59] Glazebrook quoting from Heidegger's paper, 'Die Bedrohung der Wissenschaft' (1937) in Glazebrook, *Heidegger's Philosophy of Science*, p.216

as with the present-to-hand, but are busy using it. It is only when our involvement with the world is disrupted, such that something does not work as expected, do we then take up the ‘present-to-hand’ and abstract the problem. The more controversial point in Heidegger is not just that we have these two modes of relating, but that the ‘ready-to-hand’ is more fundamental than the ‘present-to-hand’.

This is because both have their source in our being, but only in the ready-to-hand is this origin made visible through the analysis of human being. In the present-to-hand, I treat myself as though I were an object, and thus my own being remains hidden. In the ready-to-hand, on the contrary, the difference between my being and the beings, that I relate too is part of the relation itself. This is why the majority of *Being and Time* is to show that there is a difference between the present-to-hand, and the ready-to-hand, and that the latter has priority over the former.

Again, one mode of relationship is to abstract and the other is to be involved. In order to get to a point of abstracting we have to presuppose we are already in a world doing stuff. It is just that this is obscured in the present-to-hand and can lead to the interminable debates, for example, about whether reality is real or not. For Heidegger, at least, to have a relation to something always presupposes a ‘reality’ of some kind or other, whatever that reality might be.

To align our concepts, on the one side we have the ‘methodological/regional ontology/categorical/present-to-hand’, and on the other we have the ‘historical/fundamental ontology/lived/ready-to-hand’, all of which is a tension between being and beings. From understanding these ideas I have developed them as part of my original contribution to knowledge, in what I have called ‘about’ and ‘of’ languages. These will help me draw out the distinctions I wish to make, which are inspired by Heidegger, but do not use his vocabulary.

An ‘about’ language we can align with this first group of ideas (methodological/regional ontology/categorical/present-to-hand). It is simply a language that is *about* something else. For example, mathematics is *about* quantities and their relationships, classical physics is *about* objects and motion, and football is *about* a game or sport. This language is always proposition/ discourse based; it is what science aspires too, that is, science is *about* making true statements *about* reality. Here there are already three fairly vague terms such as, ‘science’, ‘true’ and ‘reality’ and yet I doubt scientists would disagree with this description. This agreement coming from whatever we are talking about being already meaningful. That is, we always experience 'reality' or science' *as* something, it is already *about* something. This is what gives our practices certainty and makes 'regional ontology' its domain of description.[60] So the 'about' language of classical physics is about classical sized objects, which exhibit a number of properties given the ways we are allowed to examine them. Objects that travel slower than light speed, and are more massive than elementary particles, can then be described in terms of things that are 'about' mechanical forces or constant mass. Other words for an 'about' language are epistemology and knowledge. Once we are at the stage of having developed an about language we are fairly certain in our knowledge and what is in the world. This is what science, in its methodological guise, aims at and what the traditional philosophy of science wants to uncover. How do language, knowledge and reality hang together so that we have these successful descriptions of nature? In addition to this, we have what I have called an 'of' language. An 'of' language is how an 'about' language is realized in the first place. Just as 'world' for Heidegger could be a non-thing or non-propositional, so too is an 'of' language. An 'of' language cannot be written, spoken or abstracted into symbols, but is simply done. Once we begin to talk about an 'of' language we have taken up an 'about' language. Whilst this might be problematic for discussing an 'of' language, if one can keep this conceptual division in mind, then what follows will be all the more easier. So the language *of* mathematics is proofing, calculating, completing operations and so on, in measuring and recording one is performing the language *of* classical physics and the language *of* football takes place on the football field. So before we can talk about 'mechanical forces' or 'constant mass', as meaningful objects belonging to classical physics, we have to be involved in an 'of' language that allows those ways of talking and acting to be meaningful. Science presupposes a person doing the science. For example, 'even logic presupposes the disposition of the logician to attend to the purely formal relations of concepts or propositions'.[61] It is the activity of logicians or how logicians are, that in

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[60] It is only as part of a regional ontology do we see something as an 'object'. This has troubled scientists in discussions about 'existence'. Mathematical physicist Paul Davies ponders the 'reality' of laws for we 'observe the things, not the laws' themselves. Paul Davies, *The Mind of God: The Scientific Basis for a Rational World* (New York: Touchstone, 1993), p.84

[61] Michael Gelven, *A Commentary on Heidegger's Being and Time*, revised edition (Illinois: Northern Illinois University Press, 1989), p.11

some way, inform what logic is going to be. It is the methodological view that abstracts to saying, it is the rules of logic that says how logic is, 'as if' it were world-less. 'Of' languages are meaningful actions, produced by being concurrent with an 'about' language. For example, one can only 'drive' in a framework that makes driving meaningful, which requires a world (non-thing).

To elaborate further, consider the apocryphal story of the invention of the game of rugby football. It is said that William Webb Ellis whilst playing football caught the ball and ran with it. In the context of the game of football this act was not 'about' football, it did not make sense. It does, however, in the language 'of' football, since the latter is playing football. It takes place in lived-space with other people. It is non-propositional. FIFA, UEFA or FA rules and the like are all 'about' football but are not football themselves. All 'about' languages are propositional or discourse based. For anyone who has ever played football it was not impossible to pick up the ball, it was just not permitted or meaningful given the rules they were asked to play by. In a situation where someone handles the ball who is not the goal-keeper or referee, the 'about' of football is challenged by a new 'of' language. Here, we can either change the rules of football to include such acts or we invent a separate game, which then develops its own 'about' language. Now we start to see how two 'about' languages quickly become disparate. One cannot scrum in football nor keep goal in rugby. What stops us being locked into whatever game we are playing and allows change to occur is that both parties would have a tacit understanding of what 'games' or 'sport' are, by being part of the same world.

We are primarily active in the world by 'doing' (of) and not involved in theorizing or abstracting the world (about).[62] So the very possibility of having a regional ontology (about) is based in something more fundamental, the world, which necessarily implies activity (of).[63] One cannot have an 'about' language without the 'of', but an 'of' language only makes sense, retrospectively, in light of the 'about'. This first order of influence is the weak reader's understanding, where we get the knowledge, methods and beliefs of science, without first having those activities that allow us to form them.

[62] Bhaskar talks about ideologies 'for' and 'about' science, which serve a similar function. Roy Bhaskar, *Reclaiming Reality: A Critical Introduction to Contemporary Philosophy* (Oxford: Routledge, 2011), p.22

The second order of influence is taken as primary by the strong reader, that is, we are only able to discuss the activities of the past and what people do because of what our language and knowledge express about reality as vindicated by science. For the weak reader the methodological perspective is actually a comment about science, at given point in history. The language *of* science, which metaphysically is what the methodological aspect aims at, is in the doing of science, which is a historical practice. The strong reader, however, collapses these distinctions and understands methodology as the language *of* science.

Ontologically, an analysis of what it means to be (of/fundamental) is prior to knowing what things there are (about/ regional). This corresponds in the shift from asking 'what is science?' to 'what does it mean to be scientific?' From the former question we will get the traditional answers that most philosophers of science have given, such that values, virtues, principles and methods are specified as a priori conditions, if certain functions are to be performed. This tells us what science is 'about' at any particular point in history, but not how it got there. In the latter question we are asking, what were scientists doing (of) in order to later abstract a methodological narrative (about). Put another way, what it means to be scientific is prior to what science is.[64]

So in order of influence, the weak reader understands that the 'of' comes before the 'about' and the strong reader understands this as the opposite. So, a typical strong version of science would be: who I am is *only* possible because of what I am (atoms, electrical forces, a mammal). So the efforts of scientists to reduce something like my enjoyment of music or art to an ECG scan or evolutionary adaptation, is a re-enforcement of this order.[65] If science is to function as science it has to utilize its methodological interpretation. What I am arguing is that this is only one interpretation

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[63] Heidegger, *Being and Time*, p.31

[64] Ginev argues something similar when he says that the three major readings of science, scientific realism, semantic realism and constructive empiricism all imply essentialism. They start with the 'is' of 'what is science?' Scientific realism posits scientific objects as 'things' and are reified by science's theories and experiments. This Ginev says is a version of naturalist essentialism. For semantic realists it is the 'semantic interpretations (models) of theory's conceptual structure' and the reification is of the mathematical structure. Here Ginev argues we are led to a form of Platonic mathematical essentialism. Ginev, *The Tenets of Cognitive Existentialism*, p.24

[65] Tallis calls the attempt to describe consciousness as the seat of happenings in the absence of the person living it as 'neruomythology'. Tallis, *The Kingdom of Infinite Space*, pp.160-161

of science. However, as it is so dominant as an interpretation, any attempt to challenge it from a philosophical perspective is met as a challenge to the idea that things cannot be true, real, objective or rational, i.e., those things used by the methodological approach.[66] Of course there may be some who do intend this to be the case, but it is only by understanding the philosophical content can one counter such claims. We find such spurious ideas in 'New Age' writings on science or Creationist science ideology. The paradox is that they require the fact that regular science works so well in order to legitimize their own competing claims. What is more, the fact that the methodological view is so prominent as an understanding of science, to the point where it is believed to be necessary for science to function, pseudosciences also adopt this view which, knowingly or not, come with the same metaphysics. So if we take 'reality' to be what our 'about' languages are 'about' and what makes our statements true is by these corresponding to the way the world really is (as a thing), such that there is no difference between our 'about' and 'of' language, we invite philosophical problems. Whilst this hardly ever stops science from being practiced, it allows for rhetorical devices to be used in questioning the epistemic authority of science (if we understand this to be what science is). So we may be able to deploy meta-inductive scepticism about progress in science, which is intuitive enough for most people to understand, but sufficiently philosophically complex that most people cannot contest it. We also find that competing claims in pseudoscience require the fact that orthodox science works so well in order to give meaning to its own discourses. This may be evidenced by the plethora of New Age literature that have a titled pre-fixed with 'quantum'.[67] The term 'quantum' is now shorthand, not only for cutting-edge science, which has allowed the modern world to take shape, it is also a legitimate claim in saying the world is not as we perceive it, and suitably complicated that not many people understand it.[68] It would not be so bad if those propagating these ideas

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[66] Kuhn acknowledges this in 'Reflections on my Critics'. In addressing the charge of relativism and irrationality, Kuhn explains that he does not know what work the term 'irrational' is doing as a criticism. If by this we mean 'unjustified belief', then he says, to argue for it as a part of science is 'not only absurd but vaguely obscene […] to suppose, instead, that we possess criteria of rationality which are independent of our understanding of the essentials of the scientific process is to open the door to cloud-cuckoo land.' Thomas Kuhn, 'Reflections on my Critics', in *Criticism and Growth of Knowledge*, ed. by Imre Lakatos and Alan Musgrave (Cambridge: Cambridge University Press, 1970), pp.231-278 (p.264)

[67] Deepak Chopra, *Quantum Healing: Exploring the Frontiers of Mind/Body Medicine* (London: Bantam Books, 2010)

[68] This makes 'quantum' the perfect rhetorical device for saying whatever you want. But for however 'mystical' or 'Eastern' some like Deepak Chopra thinks it is to ask 'how things really are' is to be

were demonstrably untrustworthy or caddish but people such as Deepak Chopra are trained medical doctors and hold positions as university lecturers. The relevance of this is how 'expertise' and 'science' are viewed by a public who many not be asked to understand the subtleties and complexities of science that philosophy brings with it. What I will want to say on our weak reading is that terms like 'quantum', as used by someone like Chopra, is not the limit of a metaphor but language that talks of 'how things really are' is.

In the following two readings of Kuhn, I will continue to be using the distinctions applied and defined so far. For the 'strong reading' I will make synonymous with the methodological approach, that is, to put an 'about' language first. The 'weak reading', which will be synonymous with the historical approach will put the 'of' language first. In the two readings, I will reconstruct Kuhn's concepts of paradigms, normal/ revolutionary science, and incommensurability to present two polarized interpretations of *Structure*. One will be the basis for an answer to the traditional 'what is science?' question. The other will act as grounding for understanding why the question 'what does it mean to be scientific?' should precede it. The hope is that the weak version will present us with a better way of reading Kuhn, but also the exercise itself will give purpose to philosophy as a public activity. By analyzing what it means to be scientific, it is this understanding of science that one could not be made aware of if one only studied science as a subject or collection of facts, in ignorance of its philosophical content. 'Good' philosophy seems less about giving answers and more about problematizing the answers we already have. It is this methodologically centered reading of Kuhn I hope to problematize, by not only showing how one arrives at something like the strong reading, but by also offering an alternative. Hopefully, one should notice that we have a complete inversion of chapters 3 and 4 in chapters 7 and 8. In chapter 7 by using the same metaphysical commitments of Popper (science as methodology) we can render a 'radical' Kuhn who, like a 'radical' Feyerabend, is easy to dismiss or mock and with it, a certain conception of philosophy. This has the dual function of producing a 'ready-made' strawman from which we can support even more spurious readings of science, epistemology and

involved with a certain kind of metaphysics. In this instance probably the neo-Kantian variety that enabled such things as the 'Copenhagen Interpretation' of quantum mechanics. What is more, the very idea of 'mind' in Chopra's 'mind-body' view is a Cartesian hang-up. Ibid., p.102

metaphysics and denigrating the worth of philosophy as a public discourse for understanding science. I argue this is because Kuhn's statements on the development of science, through its various stages, are taken as methodological prescriptions. Unlike Popper, this Kuhn is not elevated to the status of the scientists' philosopher, even though at a metaphysical level he and Popper are committed to the same things. Where Popper wanted to downplay the role of metaphysics and any supporting philosophy, by identifying scientific statements that pass the test of falsifability he, in turn, galvanized a metaphysical worldview and a supporting philosophy that make a nonsense of alternative explanations. That is, if we presuppose science is its methodology (where 'of' and 'about' are the same) when we received a historical critique of methodology and its supporting metaphysics/philosophy, we may feel inclined to interpret this critique as an alternative or substitute for methodology. Here we are led to accusations of relativism, irrationalism, or subjectivism about science. So, inadvertently a Popperian conception of science and its relationship to the world not only plays up the pseudo-problems of philosophy, which it looked to conquer, but it downplays the usefulness of philosophy in clarifying problems, which it sought to do, by giving the individual a lack of options in thinking about science.

In chapter 8 I give a more considered reading, as argued for with Feyerabend, where we can derive a Kuhn that is not 'radical', explains why something like a metaphysical commitment to 'science as methodology' does not work, and gives philosophy a valuable role in understanding a subject. This version opens the interpretations of science up, rather than pinning the individual down so they are forced to accept historical claims as methodological. Whilst this is the general thrust of what I am doing, these two readings are not addressed directly towards Popper or Feyerabend, but what they represent under these two readings.

To re-state a point, this is not a thesis about Kuhn's ideas in relation to Heidegger, but are about a Heideggerian inspired analysis of Kuhn. Through my use of the concepts laid out here I hope to not only show that 'understanding' is a product of how one comes to a text (a prior understanding of ideas) but that there are better and worse ways of reading Kuhn. At its best philosophy can help us dissolve the old antagonistisms of historical thought, but at its worst it limits us to one interpretation of those ideas which then turns back on itself, so that we give up on the enterprise of philosophy altogether. Heidegger notes an expression of this problem with thinking about science,

> A fog still surrounds the essence of modern science. That fog, however, is not produced by individual investigators and scholars in the sciences. It is not produced by man at all. It arises from the region of what is most thought-provoking – that we are still not thinking; none of us, including me who speaks to you, me first of all.'[69]

With this in mind I proceed to the next section.

Chapter Seven

[69] Martin Heidegger, 'What Calls For Thinking?', in *Martin Heidegger: Basic Writings*, ed. by David Farrell Krell (London: Routledge & Kegan Paul, 1978), pp.345-367 (p.355)

The Strong Reading

7.1- Kuhn and *Structure* – The Strong Reading

In order to begin our strong reading of Kuhn and *Structure*, I will first set its context. In order to derive the strong version not much work is needed, which may also explain its proliferation. Kuhn, as with Feyerabend in chapter 4, can be read prima-facie, 'literally', without much philosophical sophistication. I would hazard to say it is a 'commonsense' reading, but 'commonsense', like 'experience', or 'reality', covers over a range of philosophical problems, which we no longer have to engage with, once they are removed from our discourses as problematic. The term itself has a history that tells us more about what is completely uncommon to our experiences. For example, Aristotle thought 'commonsense' was a principle that governed our senses to be found in animals, as well as humans, but later developments of 'commonsense' have it as a uniform response to reality.[1] What is out in the world is literally common to our senses. The term is intricately linked to philosophical issues, such as mind, epistemology, ontology and metaphysics, yet we use it in an everyday sense. The strong reader will forgo this and assume that each of us comes to *Structure* in the same way.

As I am trying to construct an unsophisticated, 'commonsense,' reading of Kuhn, it will appeal to the methodological perspective of philosophy and it will understand any historical claims as a substitute for methodology. It will mistake historical critique for methodological prescription. I hope to show that a 'traditional' interpretation can account for the stronger and hence more speculative, or radical conclusions of Kuhn's work. This is not to argue that this version of Kuhn is 'wrong' but that once we start limiting ourselves to one interpretation, that does not defend the epistemic authority of science, it at once also devalues philosophy as a practice.[2] Here we may be

[1] Dorothea Frede, 'The Cognitive Role of *Phantasia* in Aristotle', in *Essays on Aristotle's De Anima* ed. by Martha C. Nussbaum and Amelie Oksenberg Rorty (Oxford: Oxford University Press, 1992), pp.279-296 (p.284)

[2] As Alexander George says, 'one's judgements about the relevance of the considerations Kuhn points to will be determined by one's position on the very issue on which these considerations are meant to bear. Kuhn's work consists, then, not so much in a refutation of a certain Viennese approach to science as in a decision to proceed differently.' Alexander George, 'Opening the Door to Cloud-Cuckoo-Land: Hempel and Kuhn on Rationality', *Journal of the History of Analytical Philosophy*, 1 (2012),

ethically inclined to side with the weak over the strong version. For once we are unaware of how we arrive at an understanding we are open to intellectual abuse, be it scientifically, politically or religiously.

7.2 - Framing the Strong Reading

The strong version will take up a methodological stance as primary. Here the ‘about’ language takes precedence over the ‘of’ language. Just to frame the strong reading, here are some commonsense assumptions about our starting point. We exist in a world and science is the finding out of facts about reality from the ideas we have about it. We may simplify this relationship to the dichotomy of theory to fact, this being a central preoccupation with the empiricists, positivists and rationalists. Science is the collection of facts about the world, how theory and observation interact, and philosophy of science is to uncover the hidden mechanisms by which this is possible. Science is about how reality really is, not how we think it ought to be. We all have an understanding of the difference between knowledge and belief and the problems these can bring if we confuse the two. Nahin states that the pre-science of someone like Descartes was more in the tradition of Aristotle than any science we know today. For him Aristotle and Descartes were involved in wishful thinking about reality, that is, ‘physics the way we *think* it should be, rather than what experiment *shows* it to be, arguments that today seem ludicrous.’[3] Rather, we should always go with what observation and evidence tell us, over what we want to be the case. To view observation and evidence as ‘world-less’, such that one can simply compare observation, theory and evidence, would make Aristotle and Descartes appear ‘ludicrous’ by today’s scientific standards. Carl Hempel articulates this ‘world-less’ methodological commitment when he says,

> The philosophy of science is regarded as concerned exclusively with the logical and systematic aspects of sound scientific theorizing and of the knowledge claims it yields. On this view, the psychological,

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<http://jhaponline.org/journals/jhap/article/view/1301> [accessed 16 Nov 2012] This ‘to proceed differently’ is for me the adjustment between the historical and methodological approach.

[3] Paul J. Nahin, *When Least is Best: How Mathematicians Discovered Many Clever Ways to Make Things as Small (or as Large) as Possible* (Princeton, NJ.: Princeton University Press, 2004), p.105 [Italics in original]

> sociological, and historical facets of science as a human enterprise are irrelevant to the philosophy of science, much as the genetic and psychological aspects of human reasoning are held to be irrelevant to pure logic, which is concerned only with questions of the deductive validity of inferences, logical truth and falsity, consistency, provability, definability, and the like.[4]

We could re-describe this as a division between objective and subjective knowledge. That the subjective elements of humans, their biographies, histories and beliefs have no bearing on whether a mechanism, inference or deduction is true or not. Kuhn, traditionally, has been understood as disregarding this relationship or split between subjective/objective, analytic/synthetic and with it the picture of what science is and its relationship to philosophy of science. For example, George argues that Kuhn's dilemma is whether any historical case-study could force one to give up the idea that a description of what scientists actually do is grounds enough for saying we cannot conclude what they ought to do.[5] This dissolving of the contexts of 'discovery' and 'justification' is almost unavoidable, but how one arrives at this conclusion from his words, can be framed as a strong or weak understanding of the ideas contained within.[6] For this reason the aims and approaches of chapters 7 and 8 fit well within the methodologies of deconstruction and hermeneutics.

A strong reading frames all inquiry through 'about' languages. The inquiring mind of a person wants to discover truths about an indifferent external reality. The hope being that we can define exactly or describe the external world through a formal language (mathematics, logic, and science). A one-to-one correspondence between these languages and reality, in this model, is what we call 'truth'. Truth is about the meeting

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[4] Carl Hempel, 'Scientific Rationality: Analytic vs. Pragmatic Perspectives', in *Introductory Readings in the Philosophy of Science*, ed. by Robert Hollinger, David A. Kline and E. D. Klemke (New York: Prometheus Books, 1988), pp.292-304 (p.293). Kuhn directly responded to Hempel 'his work does nothing for me at all when I work on, say, the history of thermodynamics or of the quantum theory.' Thomas Kuhn, *The Essential Tension: Selected Studies in Scientifc Tradition and Change* (Chicago: The University of Chicago Press, 1977), p.12

[5] George, 'Opening the Door to Cloud-Cuckoo-Land', p.1

[6] The 'inevitability' of this conclusion merits that only so many interpretations are possible. For if there were the lack of constraint on 'truth', 'objectivity' or 'reason' that the strong reading of Kuhn implies the very medium contradicts the message. As Derrida writes, '[a deconstructive reading] must always aim at a certain relationship, unperceived by the writer, between what he commands and what he does

up of discourse with world. Again, there is something very plausible about this. What is at stake in this model is how we know our methods of inquiry are reliable, removing the subjective content of our psyches, and the influence of culture or beliefs as bias? Slowly, 'science' has condensed as our finest example of inquiry into the true nature of reality. Here we have to fight our experiences, as the world is very different to how think it is. It is not intuitive to think that the earth is revolving or we all came from single-celled organisms. Science is the removal of 'mind' from our intuitive knowledge of the world, hence why for Aristotelians their belief that the world does not spin is just plain wrong. A view of reality that has the earth spinning and rotating around the Sun is closer to how things really are. What this strong view passes over is that any attempt to say what there 'really' is, or give a representation that has absolute one-to-one correspondence between discourse and the world, is a metaphysical consideration.

It is itself a worldview that puts the extremely powerful methods and techniques of science as primary or that allows science to function, as a science.[7] As part of those methods and techniques it can generate and revise propositions in an objective sense, for they are concretely *about* something. If this is all we take science to be we can easily fall in to the trap of thinking that the 'about' language is fundamental and that all inquiry and understanding is grounded in epistemology or discourse.[8] The theorizing that this particular stance allows cannot account for itself in terms of the metaphysics it supports. Where we start from dualisms such as, subject-object, internal-external, and work outwards. The friction that is generated by this underlying tension becomes visible in our understanding when we conclude that Kuhn is arguing for relativism, irrationality, anti-realism, anti-science or that people can 'literally' live

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not command of the patterns of the language that he uses.' Jacques Derrida, *Of Grammatology*, trans. by Gayatri Chakravorty Spivak (Baltimore: The John Hopkins University Press, 1997), p.158

[7] Fuller notes, 'social scientists were attracted to Kuhn's book precisely because it seemed to provide a blueprint for how a community of inquirers can constitute themselves as a science, regardless of their subject matter.' Steve Fuller, 'Being There with Thomas Kuhn: A Parable for Postmodern Times', *History & Theory*, 31 (1992), 241-275 (p.269)

[8] Chalmers accuses Kuhn of confusing objective and subjective knowledge in explaining paradigm change. He says 'statements do not have properties in the sense that physical objects do, and spelling out the mode of existence of such linguistic objects, as well as other social constructions such as methodological rules and mathematical systems, is a tricky philosophical business.' Alan F. Chalmers, *What is This Thing Called Science?* 3rd edn. (Maidenhead: The Open University Press, 2010), p.127

in different worlds.[9] The term 'literally' will also be significant, as in the strong reading 'literalism' is what we are aiming for. A = A, paradigm equals world, our 'about' and 'of' languages are the same. They all seek a literal one-to-one correspondence, which the history of science seems to deny us. On the weak reading, though it may sound contentious and contrary to commonsense, these models are in a sense just metaphors, and there can be no literal tracing of world by discourse or collapsing an 'of' language into an 'about'.

With the strong version, what we should find is that we derive a Kuhn that appears to be stating something either banally trivial, or wildly speculative. Both are predicated on the grounds that either epistemology or language is the driving force of science. The observation that Kuhn can be read more than one way is not novel and itself seems a truism. Pinch, for example, notes that Kuhn's 'paradigm' has been taken by the mainstream to mean either the sum cognitive output of a field or the impact of that knowledge on surrounding social activity.[10] Pinch claims that this can be identified in the American and British traditions of sociology of science, where the cognitive and social activities of science are kept separate. My interpretation, however, hopefully goes beyond the descriptive into not only analyzing why we get certain readings, but also an ethical sense of which we should prefer. As the claims of the strong reading can range from the banal to the absurd, it will be argued that the resultant conclusions are aimed at the apparent processes that divulged them, philosophy. So in the case of philosophy of science, a statement that amounts to a triviality such as 'different people have different opinions', shows the redundancy of the practice. And where we derive seeming absurdities, such as science being irrational or jettisoning objectivity for relativism, philosophy appears destructive or a nuisance. Bernstein suggests that these misreadings of Kuhn exist for good reasons as it 'has currency in its continued propagation for certain interest groups.'[11] It is, however, by the same means that these

[9] For Newton-Smith, this strong position is what is most signficant about Kuhn's work. W. H. Newton-Smith, *The Rationality of Science* (London: Routledge & Kegan Paul, 1981), p.103

[10] Trevor Pinch, 'Kuhn – The Conservative and Radical Interpretations: Are Some Mertonians 'Kuhnians' and Some Kuhnians 'Mertonians'?', *Social Studies of Science*, 27 (1997), 465-482 (p.466). Pinch lists a number of theorists that have made the division between 'paradigm' as cognitive content and 'paradigm' as social activity. Hagstrom (1965), Crane (1969; 1972), Mullins (1973), Gilbert (1976), Law (1976), Edge & Mulkay (1976), and Small (1977), ibid., p.467

[11] Richard J. Bernstein, *Beyond Objectivism and Relativism: Science, Hermeneutics and Praxis* (Philadelphia: University of Pennsylvania Press, 1983), p.150

readings can be problematized and thus render them ineffective as weapons of rhetoric or persuasion.

In order to see why the 'strong' reading may seem commonsense, it will be useful to look briefly at the history of *Structure* and the impact made by Kuhn. Dudley Shapere was the first person to give an extensive review of *Structure* and put Kuhn on the academic radar. The review portrayed Kuhn as representing an anti-positivistic theory of science.[12] Fuller speculates that this was an exercise in displaying the power of sophisticated philosophical investigation against the 'wild-eyed claims of historians overly impressed with the remoteness of the past.'[13] This 'historicist turn', with notable members such as Norwood Hanson, could be regarded as a 'revolt against positivism'.[14] Shapere saw the future of philosophical scientific inquiry as represented in Frederick Suppe's book *The Structure of Scientific Theories*.[15] This told the impressive story of the rise of the 'received view' of analytic philosophy over neo-Kantian doctrines. In the 'Afterword' to the second edition, Suppe predicted that the influence of Kuhn was winding down and that the future of historicist philosophy of science belonged to Toulmin and Shapere.[16] It would seem strange if Kuhn were 'anti-positivist' he should seek the help of Carnap, the leading positivist, and even stranger to enter into correspondence over the development of *Structure*.[17] Another bibliographic anomaly is that Larry Laudan, the torch bearer for 'historicism', did not receive a single comment in Suppe's massive 800 page opus, publishing the second edition of *The Structure of Scientific Theories* in the same year as Laudan's *Progress and its Problems*.[18] Laudan went on to develop the area of sociology of scientific knowledge (SSK). This became known as the 'Strong Programme' to distinguish it

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[12] Dudley Shapere, 'The Structure of Scientific Revolutions', *The Philosophical Review,* 73 (1964), July (3), 383 – 394

[13] Fuller, 'Being There', p.244

[14] Ibid., p.243 – For examples of Hanson's work and the unwelcome reception the 'historicist turn' received see, Norwood R. Hanson, 'Scientists and Logicians: A Confrontation', *Science,* 138 (1962), 1311-1314; 'The Irrelevance of History of Science to Philosophy of Science', in *What I Do Not Believe and Other Essays,* ed. by S. Toulmin & H. Woolf (Dordrecht: Reidel, 1971), pp. 274-287

[15] Frederick Suppe (ed.) *The Structure of Scientific Theories,* 2nd edn. (Illinois: Illini Books edition, 1977)

[16] Ibid., pp.631-649 – Suppe's makes it clear that the 'worldview' perspective is on the way out making way for an objective realist account of scientific theories and the objects they refer to. Suppe, however, tentatively concedes 'I wonder if we really can give a philosophical argument in favor of this [realism]. Is it not simpler just to say, 'well, this is the best thing to do?' Ibid., p.598

[17] George A. Reisch, 'Did Kuhn Kill Logical Empiricism?', *Philosophy of Science,* 58 (1991), 264-277

[18] Larry Laudan, *Progress and Its Problems: Towards a Theory of Scientific Growth* (Berkeley: University of California Press, 1977)

from other methodologies, such as Karl Mannheim's sociology of knowledge, who argued that the rational methodology of the natural sciences precluded it from social explanation. It was in the 1970's that the 'Edinburgh School' emerged, bringing together the studies of history, philosophy and sociology of science, the three main areas Kuhn brought together in *Structure*.[19] A largely constructivist agenda arose from this melting pot which adopted Kuhn as a member.[20] More recently, introductory texts to the sociology of science or postmodernism, represent Kuhn's philosophy of science, less as one of anti-positivism and more one of full-blown relativism.[21] Kuhn, as a relativist, may now be the 'received view' of his work. For example, Merchant states in the opening to her article, *The Theoretical Structure of Ecological Revolutions* that in assessing the approaches to theories of environmental histories she 'accepts the relativist stance set forth in the first edition of his [Kuhn] book'.[22] I do not have a problem with people using a relativist framework for investigating various approaches, but I do disagree with using the authority of Kuhn to give extra academic and theoretical credence to an argument, by assuming relativism as a given.[23] Feyerabend notes this pre-critical adoption of what Kuhn 'meant' by philosophers when he says,

> Kuhn's ideas are interesting but, alas, they are much too vague to give rise to anything but lots of hot air. If you don't believe me, look at the literature. Never before has the literature on the philosophy of science been invaded by so many creeps and incompetents. Kuhn encourages

[19] We may also include the 'Bath School' as an affiliate with notable members being Harry Collins, Trevor Pinch and Robert Evans.
[20] Barry Barnes, *T.S. Kuhn and the Social Sciences* (Oxford: Oxford University Press, 1982)
[21] Michael Erickson, *Science, Culture and Society: Understanding Science in the 21st Century* (Cambridge: Polity Press, 2005), p.71; Ziauddin Sardar, *Thomas Kuhn and the Science Wars* (Cambridge: Icon Books, 2000), p.32
[22] Carolyn Merchant, 'The Theoretical Structure of Ecological Revolutions', *Environmental Review*, 11 (1987), 265-274 (p.265) – Merchant caveats her claim to the first edition as in the postscript to the second and third editions Kuhn spells out why he is not a relativist and those that think he is are mistaken. More recently Merchant has used 'Kuhn's perspective' not declaring him a relativist but as giving a 'perspective internal to the working of science and the community of scientists.' – *Ecological Revolutions: Nature, Gender, and Science in New England*, 2nd edn. (Chapel Hill: University of North Carolina Press, 2010), p.3
[23] If we trace the history of Kuhn's work and the reception, critiquing and assimilation of *Structure* in the literature we find it difficult to deny its place in the narrative of historicist philosophy of science over the positivistic tradition. Take Lakatos and Musgrave's *Criticism and the Growth of Knowledge*, for what started out being a festschrift to Popper, is now received and taught as a collection of responses to Kuhn.

> people who have no idea why a stone falls to the ground to talk with assurance about scientific method.[24]

If we follow an uncritical positing of relativism we can ultimately arrive at a place that science does not make objective true statements about the world or is just one competing form of knowledge. This, in conjunction with the failure of demarcation, would appear to be a situation that could only be made possible by the presence of philosophy. It then takes ‘commonsense’ scientists, such as Sokal and Hawking, to come along and tell postmodernist and constructivist philosophers that what they are doing is wrong. Yet, the means to resolving these problems do not come from outside philosophy, but from within. This is where philosophy shows its true value to public discourse. We can uncover the metaphysical requirements for relativism or anti-realism, such that we turn philosophy back upon itself without it consuming itself entirely.

As I do not have the room for every aspect of Kuhn’s philosophy, the aspects that I do address are to be illustrative of: 1) how any reading is predicated on a prior understanding, which itself contains philosophical notions. 2) By using ideas inspired by Heidegger to show how we can arrive at ‘strong’ and ‘weak’ readings, and 3) in doing both 1 and 2 not only to bring to the fore the value of philosophy for thinking about science in the PUoS, but alsoto show that we have good reasons for preferring one reading over another. So when we read in Hawking that ‘philosophy is dead’ or Weinberg aims at a ‘culture free description of how reality is’ in science we know when scientific opinion has moved over to metaphysical or even mythical speculation.[25] What is more, we may be able to state the very conditions for why a scientist could entertain those ideas as meaningful in the first place.

So that the reader may be able to follow the developments I wish to problematize, I will next give a brief overview of *Structure*. This will be followed by a strong reading

[24] Paul K. Feyerabend, ‘How to Defend Society from Science’, in *P. K. Feyerabend: Knowledge, Science and Relativism*, Philosophical Papers Vol. 3, ed. by John Preston (Cambridge: Cambridge University Press, 1999b), pp.181-191 (p.185)

[25] Stephen Hawking and Leonard Mlodinow, *The Grand Design: New Answers to the Ultimate Questions of Life* (New York: Bantam Press, 2010), p.1; Steven Weinberg, ‘Peace at Last?’, in *The One Culture? A Conversation about Science*, ed. by Jay Labinger and Harry Collins (Chicago: University of Chicago Press, 2001), pp.238-242 (p.238)

of the central concepts of 'paradigm', 'normal' and 'revolutionary' science, and 'incommensurability'. The 'elephant in the room' with Kuhn is whether he is a realist or anti-realist. As I do not have the room to address this concern it is hoped that by the end of this thesis one will realize that this problem is a result of the strong reading and that the weak reading is a means to dissolve such metaphysical antagonisms.

7.3- The General Structure of *Structure*

Just so we can locate ideas when they are being discussed, I will give a brief overview of the structure of *Structure*. Kuhn's argument follows the layout of his chapter headings, in that we move from pre-normal to normal science via incommensurability of theories, to the establishing of paradigms and the breaking of paradigms by anomaly and crisis. Pre-paradigm, there are numerous competing explanations and perspectives for 'what there is', and one, for whatever reason, draws more attention than others. Once the fundamentals of this new perspective are worked out it begins to extend to new phenomena. We could loosely call this a 'paradigm'. In the presence of a paradigm 'normal science' takes place, which acts as an expression of the paradigm. Normal science is concerned with 'puzzle-solving', and describes the majority of practices scientists are involved with.[26] Normal science recognizes, identifies itself with, and is based on the paradigm, so much so, that its main theories, experiments, research values and metaphysical assumptions are given a coherence that allow further cumulative puzzle-solving to occur.[27] Normal science, however, seeks to replicate itself and give a full explication of its domain. In doing so, it accumulates anomalies, which for the most part it suppresses, re-describes or ignores, until the paradigm, which supports the 'normal science', can no longer make sense of those anomalies and remain coherent in their presence. The paradigm enters a state of 'crisis', where the scientist is forced to ask fundamental questions about the paradigm. As it is the paradigm that tells scientists 'what there is', this can no longer be trusted and so we return to the maelstrom of competing theories and perspectives from which the enterprise started. When one theory gains ascendancy over the others and settles

[26] Thomas Kuhn, *The Structure of Scientific Revolution*, 3rd edn. (Chicago: The University of Chicago Press, 1996), pp.36-40

[27] Paradigms tend take on the name of their sponsor, for example, Newtonian or Einsteinian physics as a paradigm. The use of single name tends to hide the multiple teams of researchers and individuals present in constructing and maintaining that paradigm.

as the new paradigm, we can be said to have undergone a ‘revolution’. The problem then arises that, as the paradigm is in dispute, what does the scientist then appeal to in order to guide research? If the paradigm had previously contextualized evidence, observation, theory or what was reasonable to assume, what are we left with? Moreover, what was considered ‘unscientific’ by the old paradigm, might well be part of our ‘new science’ in the prevailing paradigm.

This is, more or less, the pattern for scientific development Kuhn presents us with. In his concluding remarks to the introduction he potentially signposts us with a warning,

> [M]any of my generalizations are about the sociology of social psychology of scientists; yet at least a few of my conclusions belong traditionally to logic or epistemology […] can anything more than profound confusion be indicated by this admixture of diverse fields and concerns?[28]

The warning could possibly be that if one thinks that just because the generalizations are about sociology, psychology, or have consequences for logic or epistemology, that if one takes science to progress by these means, then only confusion will result. With this said I look at the strong reading of ‘paradigms’ next.

<u>7.4 - Paradigms</u>

One of the central concepts in *Structure* is what Kuhn calls ‘paradigms’. This is crucial to an interpretation of his book, in what one understands them to be. From this it dictates how they change and how science relates to them.[29] After fifty years of

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[28] Kuhn, *Structure*, pp.8-9 – Kuhn’s statements about psychology, perception, religious conversion and cognition are not just metaphorical or analogous for the strong reader but they have literal import for how scientists ‘see’ the world. Koffka, Fechner, Helmholtz and Wundt’s psychological criticism of philosophical doctrines of epistemology laid the foundations for a naturalistic reading of the perceptual experiences Kuhn describes in *Structure*. Gary Hatfield, ‘Koffka and Kohler, and the “Crisis” in Psychology’, *Studies in History and Philosophy of Biological and Biomedical Science*, (2011), 1-10, (p.2) <http://lchc.ucsd.edu/MCA/Mail/xmcamail.2012_04.dir/pdf6vwCNNTidm.pdf> [accessed 25 March 2013]

[29] It is argued that many of the central ideas in Kuhn were already floating about in philosophy of science. Sardar, *Thomas Kuhn*, p.32. Merton, de Saussure, and Toulmin, all predate Kuhn in the use of ‘paradigm’. Raymond Firth, ‘An Appraisal of Modern Social Anthropology,’ *Annual Review of Anthropology*, 4 (1975), 1-25 (p.15); Fuller, ‘Being There’, p.255

analysis, one still struggles to find a definitive definition of paradigm, but it seems to be the case that it is easier to say what one 'does' rather than what one 'is'. This is born out by the fact that multiple definitions and uses of paradigm can be found throughout *Structure*, which will remain a problem for our strong interpreter.[30] Kuhn himself in the postscript to the revised editions acknowledges this problem with 'paradigms'. Not, that is, the idea but in their articulation, 'several of the key difficulties of my original text cluster about the concept of a paradigm […] I suggest the desirability of disentangling that concept from the notion of a scientific community.'[31] His own re-articulation of the concept of paradigm leads him to present them as having two major uses – the first is sociological, in that, it refers to the practices of a given community, the second is paradigm as 'exemplary past achievement'.[32] As these distinctions were not clear in the first edition and a wealth of literature was evoked by these misgivings, many of which, Kuhn himself did not agree with, he then claims that paradigms are, 'the most novel and least understood aspect of this book.'[33] Kuhn's own disagreement with those interpretations of what he was saying is either a testament to the radical nature of those ideas or misunderstanding is easier than he supposed.[34] If a paradigm is both a set of beliefs within a community of practitioners and is also an historical event the paradigm identifies with, what then is its content? One of the problems that are present with the strong interpretation is that in asking for a definition of a paradigm, we are making the same mistake as in asking for an exact definition of science. It would seem for Kuhn that a paradigm is everything to science.[35] The extensiveness, however, of what a paradigm is will either have to include everything and thus explain nothing, or at least account for the success of one paradigm over another, without recourse to explaining the paradigm's success in terms of what scientists are doing, which is working within

[30] Masterman points out that the term 'paradigm' has twenty-two different uses and meanings in the first edition of *Structure*. They can generally be put into three groups, 1) beliefs, 2) exemplars, and 3) the artefacts of science. She also notes that these variations are not necessarily inconsistent with one another but may act as elucidatory. Margaret Masterman, 'The Nature of a Paradigm', in *Criticisms and the Growth of Knowledge*, ed. by Imre Lakatos and Alan Musgrave (Cambridge: Cambridge University Press, 1999), pp.59 - 90 (p.65)
[31] Kuhn, *Structure*, p.174
[32] Ibid., p.175
[33] Ibid., p.187 – He does however go on to change his mind and claim that incommensurability was the most novel part of *Structure*. Thomas Kuhn, 'Afterwords', in *World Changes: Thomas Kuhn and the Nature of Science*, ed. by Paul Horwich (Cambridge, Mass.: The MIT Press, 1993), pp.311-314 (pp.314-315)
[34] Kuhn, *Structure*, pp.185-186

a paradigm. Sidestepping this initial problem, we can identify a paradigm by its operation in a 'mature' science. Again, Kuhn does not define this but uses it synonymously with the term 'normal science'. In *Criticism and the Growth of Knowledge* he says that the transition from immature or proto-science to science is marked by the 'abandonment of critical discourse' and we only take up critical engagement with the paradigm once the mature or normal science has entered into a state of crisis.[36] The claim that routine science 'abandons critical discourse' is a hard idea for the strong reader to take without concluding either, 1) that such a practice would be irrational, or 2) it would not be a science.

What is a paradigm under a strong reading? How does this affect our understanding of Kuhn or his vision for the development of science? The first definition we are provided with is found in the 'preface' to *Structure*; a paradigm is a, 'universally recognized scientific achievements that for a time provide model problems and solutions to a community of practitioners.'[37] As we have seen Kuhn can mean 'paradigm' in more than one way, and for the strong reader, it will be problematic that Kuhn does not stick to this definition of paradigm. For its meaning becomes so loose that 'anything that allows science to accomplish anything can be a part of (or somehow involved in) a paradigm.'[38] Shapere notes that the concept of 'paradigm' bears a large responsibility for how we see scientific development in Kuhn, and that if we are to make sense of Kuhn's account of scientific change, 'paradigm' must contain significant explanatory power. It is this explanatory power that is lacking as a result of the sheer width of the term paradigm, as paradigms are not rules or theories for saying such and such is correct, but are more 'global' from which, rules and theories are abstracted and deployed in research.[39]

What the strong reader expects of a 'paradigm' is to know what it is 'about', so that we can look at two or more paradigms and see what is common between them. In

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[35] Shapere in his review of *Structure* was one of the first to criticise the vague notion of paradigm, Shapere, 'Structure', p.385

[36] Thomas Kuhn, 'The Logic of Discovery or the Psychology of Research', in *Criticism and Growth of Knowledge*, ed. by Imre Lakatos and Alan Musgrave (Cambridge: Cambridge University Press, 1999a), pp.1 - 24 (p.6)

[37] Kuhn, *Structure*, p. x

[38] Shapere, 'Structure', p.385

[39] Kuhn, *Structure*, p.43

looking for those similar properties, there is the hope that we can distill a methodological account such as, rational choice theory between paradigms. At the very least, we will want to keep the distinction between 'context of discovery' (psychological or social) and 'context of justification' (methodology) alive. If we can deduce normative rules that are common to paradigms we can save science from the charges of irrationality or relativism.[40] If we concede that historical research shows that there exist guiding factors, held by the majority of scientists over a certain period of time, and it is those guiding factors that have allowed science to progress, what then are those guiding factors? If what is common to those scientists over time is bound up in the term 'paradigm', which enable scientists from different backgrounds and disciplines to share research and compare evidence or theories – can we distill those overlapping features into a coherent logic of discovery or underlying philosophy of science?

What we find, when we look towards Kuhn's concept of paradigm as a methodological suggestion for how science proceeds, is a muddle of answers. Kuhn says that 'tests and theories must proceed from within one or another paradigm-based tradition' and yet paradigms cannot be formally expressed. When the historian or philosopher of science tries to unveil the underlying structure that scientists follow they find that, 'phrased in just that way, or in any other way he can imagine, they [rules or general beliefs constituting the paradigm] would almost certainly have been rejected by some members of the group he studies.'[41] The fact that some people can reject part of the paradigm, and that for Kuhn paradigms are open to 'direct inspection', that then allows the scientist, historian or philosopher to agree that a paradigm exists, 'without agreeing on, or even attempting to produce, a full *interpretation* or *rationalization* of it.'[42] More recently, those that have taken the idea of 'paradigm' as a methodological suggestion have sought to ground it in the

[40] Shapere, 'Structure', p.392-393; 'The Paradigm Concept', in *Reason and the Search for Knowledge: Investigations in the Philosophy of Science*, ed. by Robert S. Cohen and Marx W. Wartofsky (Dordrecht: Reidel Publishing Company, 1984), pp.49-57 (p.50-51). It is not just for those people who responded to Kuhn but those who interpret the interpreters of Kuhn. Harvey Siegel, 'Meiland on Scheffler, 'Kuhn and Objectivity in Science', *Philosophy of Science*, 43 (1976), 441-448 (p.445)
[41] Kuhn, *Structure*, p.146 & p.44
[42] Ibid., p.44 [Italics in original]

cognitive sciences.[43] Here scientific development works by way of a 'group-licensed way of seeing', so that similarities between a proposed puzzle solution and the paradigm correspond.[44] The Kuhnians involved in the 'psychological turn' take this literally as a matter of cognitive processing, such as how I might distinguish similarities between family members, smells or tastes.[45] Eventually what is sought is a tacit knowing that is acquired by frequent encounters with the paradigm's exemplars and the potential puzzle-solutions they allow.[46] Here we are faced with the problem of how the 'tacit knowledge' in the scientist's head relates or influences the outside paradigm or how the paradigm becomes embedded in the scientist's psyche.[47] The shortcomings of the methodological suggestion of 'paradigm as shared exemplars' are raised by Nickles, in that, the exemplar Newton gives in the *Principia* are not the exemplars found in twentieth century physics textbooks.[48] The exemplars have changed in response to more modern puzzle-solving. At the heart of the cognitive science approach or 'psychological turn', however, still lies a representational model of the world to mind. Here scientists see 'stimuli' from which we abstract to forms of reasoning or cognitive habits located in the brain, which call to the senses different data sets or experiences.[49] Notice that 'stimuli' is as abstract as the term 'evidence' or 'observation', but it is what our 'experiences' are about. This fails to acknowledge that we do not encounter 'stimuli', but an event that is always already meaningful for us.

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[43] Bird calls Kuhn's use of psychology 'evidential' whereas Hanson's was 'illustrative'. It is a strong reading that has Kuhn rooting paradigms in psychology. Alexander Bird, 'What Can Cognitive Science Tell Us About Scientific Revolutions?', *Theoria,* (2012) 27, 293-321, (p.294)
[44] Kuhn, *Structure*, p.189
[45] Bird, 'What Can Cognitive Science Tell Us', p295
[46] Here Harry Collins has run into theoretical problems with the notion of tacit knowledge and how it relates to inherently social activities called the 'socialisation problem'. Harry Collins, *Tacit and Explicit Knowledge* (University of Chicago Press: Chicago, 2010), p.7
[47] One solution to this 'puzzle' has been the development of 'analogical reasoning' or 'case-based reasoning'. See Thomas Nickels, 'Normal Science: From Logic to Case-based and Model-based Reasoning', in *Thomas Kuhn,* ed. by T. Nickles (Cambridge: Cambridge University Press, 2003), pp.142-177
[48] Bird, 'What Can Cognitive Science Tell Us', p.298 – This criticism seems to equivocate how science is taught (textbooks - methodological) with how it is done (practice - historical).
[49] Bird in support of the naturalised psychological interpretation of Kuhn gives an argument from authority, as it is used in other fields of research, but he also adds that his conclusions are in need of 'further empirical confirmation.' Ibid., p.301. How does this statement fit in with this interpretation of Kuhn, for if the challenge is then, 'Bird only sees Kuhn's theory of Paradigms in terms of cognitive science because he is working within a paradigm that makes this interpretation possible. What would 'further empirical confirmation' mean here?

Returning to paradigms as described in *Structure*. Kuhn makes it clear that a mature or 'normal science' cannot operate in the absence of a paradigm. It is only by normal science that we know a paradigm or that a paradigm even makes sense, yet a paradigm does not require shared agreement of its interpretation, nor a fully explicit set of rules in order for it to guide research.[50] Indeed 'the existence of a paradigm need not imply any full set of rules to exist.'[51] Moreover, Kuhn believes paradigms not only '*could* determine normal science without the intervention of discoverable rules' but 'actually do operate in this manner.'[52] If the existence of a paradigm is conceivably non-reducible to a set of axiomatic rules, it would appear that paradigms are necessary yet inexpressible, all encompassing yet revisable, tacitly knowable, yet not explicitly known.[53] The effect of paradigms is to be found at the 'conceptual, theoretical, instrumental and methodological' levels functioning to instruct the scientist 'what both the world and his science are like'.[54] When expressed as such, how can science, the epitome of human rationalization and objectivity come from anything like a paradigm? What is then the explanatory scientific worth of such a vague concept? How can a paradigm be open to direct inspection when scientific 'facts' can only be observed through a paradigm? The polysemy of this term would be a problem, if it were a methodological suggestion for how science progresses. It would seem akin to saying the word 'magic' as part of a scientific explanation. As with Feynman, we have already seen the problems that arise if one wants to know what science is through the definition of words. As with his example of 'energy', philosophers tend to use or understand the term 'paradigm' tautologically so nothing new is learnt, or it then becomes very hard to articulate.[55] A few of the criticisms the term paradigm incurred may reveal this propensity. James B. Conant wrote to Kuhn expressing his worries that he would be understood as the guy who 'grabbed on to the word 'paradigm' and used it as a magic verbal wand to explain everything.'[56] Shapere found it 'mysterious', 'vague', and 'ambiguous', whereas Wisdom thought that is was

[50] Kuhn, *Structure*, p.44
[51] Ibid.,
[52] Ibid., p.46 [Italics in original]
[53] The different uses and meanings of the term 'paradigm' can be found on, Ibid., pp.11; 25; 27-28; 37; 44; 56; 62; 92; 103; 105; 109-110; 113; 125; 151-152; 158. For their different interpretations see Vasso Kindi, 'Kuhn's Paradigms', in *Kuhn's The Structure of Scientific Revolutions Revisited*, ed. by Vasso Kindi and Theodore Arabatzis (Oxford: Routledge, 2012), pp.91-111 (p.92)
[54] Kuhn, *Structure*, p.42
[55] p.102 in thesis.

‘not easy to say what it means’, and for Scheffler, ‘the adoption of a new scientific theory is an intuitive, mystical affair, a matter for psychological description primarily rather than for logical or methodological codification.’[57] For Cedarbaum, a paradigm is ‘an axiom system and a model (in the technical sense) for that system.’[58] If ‘paradigm’ is understood in the methodological sense, it will be construed through the normal scientific worldview, which is to say it must be ‘about’ something. In science a term is the shorthand for a long description. Here we have seen philosophers and sociologists busily trying to find out ‘what’ a paradigm is or how science is structured.[59] Shapere says, ‘one reason why, in particular cases, identification of “the paradigm” is so difficult: not just because it is hard to see, but because looking for the guiding elements in scientific activity is not like looking for a unitary entity that either is there or is not.’[60] Kuhn’s initial attempt to prevent such confusion led to him enlisting the help of Wittgenstein’s ‘family resemblance’s’, but unfortunately Wittgenstein, like Kuhn is also open to interpretation.[61] Hoyningen-Huene gives a detailed exposition of the changing nature and role of paradigm for Kuhn. He notes that in the second edition of *Structure* Kuhn tries to disambiguate the term paradigm by introducing the notion of the more analytically sounding ‘disciplinary matrix’. For Hoyningen-Huene, it was this that marked the beginning of a period of confusion, which ultimately led to Kuhn abandoning the term ‘disciplinary matrix’ altogether.[62] The inclusion of ‘disciplinary matrix’, which includes the socialization of scientists into ‘seeing’ similarities, the analogy drawn with Wittgenstein’s ‘family

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[56] Letter to Kuhn, June 5th, 1961 – cited in Daniel G. Cedarbaum, ‘Paradigms’, *Studies in the History and Philosophy of Science*, 14 (1983), 173-213 (p.173)

[57] Shapere, ‘Structure’, p.393; John O. Wisdom, ‘The Nature of Normal Science’, in *The Philosophy of Karl Popper*, part 2 vol. 14, ed. by Paul A. Schilpp (La Salle, Il.: Open Court, 1974), pp.820-842 (p.832); Israel Scheffler, *Science and Subjectivity* (Indianapolis: Bobbs – Merrill, 1972), p.18

[58] Cedarbaum, ‘Paradigms’, p.204. Cedarbaum is elsewhere accused of misunderstanding Kuhn’s concept of paradigm. Brad Wray, *Kuhn’s Evolutionary Social Epistemology* (Cambridge: Cambridge University Press, 2011), p.56

[59] This has led to scientometric criteria. This could be group membership, social network analysis or citation frequency. Marc de May, *The Cognitive Paradigm* (Chicago: The University of Chicago Press, 1992), pp.124-131

[60] Shapere, ‘Structure’, p.388

[61] Peter Winch’s reading of Wittgenstein impact on how Wittgenstein is understood in the social sciences. Peter Winch, *The Idea of a Social Science and its Relationship to Philosophy* (London: Routledge and Kegan Paul, 1958). Winch’s ideas caused a lot of controversy which can be divided in a strong-weak reading of his work according to whether one thinks socially embedded actions stem from beliefs/knowledge or vice-versa. For an example of this see Peter Winch’s exchange with Ian Jarvie in ‘Understanding and Explanation in Sociology and Socil Anthropology: Comment and Reply’, in *Explanation in the Behavioural Sciences: Confrontations*, ed. by Robert Borger and Frank Cioffi (Cambridge: Cambridge University Press, 1970), pp.231-270

resemblances', plus a leaning to a more linguistically focused analysis, would push the strong reader to think that these are all methodological suggestions for linguistic, sociological or psychological investigation.[63] The use of 'family resemblances', for Shapere, is inappropriate. As Kuhn's idea of paradigm does not necessitate any common property that guides scientific procedure, in the same way that our inability to define what is common to all games implies that games must derive from a unified abstract idea. As the rules that guide normal research are abstracted from the paradigm, there would seem no possible way to compile a set of rules for investigating the paradigm in order to inspect it.[64] This objection has been leveled at the vagueness of the concept, which for Masterman is just the result of a crude analogy.[65]

One way of 'inspecting' paradigms has been through 'scientometrics'. Derek de Solla Price, who popularized the term 'Big Science', has taken to studying the 'life cycles' of sciences based on Kuhnian categories. One scientometric indicator of where a field is in the Kuhnian life cycle is called 'Price's Index,' which measures the obsolescence rate of journal articles in terms of diminishing citation patterns. A high obsolescence rate means a paradigm is performing at top puzzle-solving levels.[66] Such that a contemporary physics journal would not publish anything on 19th century Newtonian mechanics, as it would appear there is nothing left to discover about it. This would now be the domain of history of science or some such supporting fields. Fuller claims that work in 'scientometrics' 'rests on simplifying assumptions about the international political economy of science that cannot help but contribute to the image of science as a self-contained, self-sufficient enterprise.' He also argues that scientometrics takes the science policy maker as evaluator not prescriber, as if science had its own 'natural

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[62] Paul Hoyningen-Huene, *Reconstructing Scientific Revolutions* (Chicago: University of Chicago Press, 1993), p.132

[63] Pinch points out that Kuhn can be read as dividing the social from the intellectual when restating and operationalizing 'paradigm' in the later editions of *Structure*. A 'disciplinary matrix' has four components. Some of which are purely epistemological whereas others social. Pinch identifies this separation of intellectual and social aspects as the 'conservative' interpretation of Kuhn. Pinch, 'Kuhn – The Conservative and Radical Interpretations', p.466

[64] Fuller argues that Kuhn was looking to generalise Polanyi's account of scientific authority which 'meant that he had to detach that account from the local squabbles in which Polanyi first developed it. However, this move, in turn, created a free-floating legitimating narrative--a "myth," properly speaking--that could be used by any discipline in need of boosting its status: quite the opposite of what either Polanyi or Kuhn would have wanted.' Fuller, 'Being There', p. 269

[65] Masterman, 'The Nature of a Paradigm', pp.80-81

[66] de Mey, *The Cognitive Paradigm*, pp. 111-170

trajectory', and their job is to diagnose when it veers off course.[67] The British rival to scientometrics can been seen as arising out of Mulkay's early writings on Kuhn, where he was considering how the notion of paradigm may be used in empirical research. He suggests that 'it is in the latter sense of intellectual prescriptions, or technical and cognitive norms, that Kuhn's notion of paradigm is most important for the sociological study of scientific innovation.'[68]

If a paradigm governs what theories are successful, what counts as evidence, or as rational, we can then be drawn into a debate about explicitly defining 'success', 'evidence', or 'reason' independent of the activities that require them or make them meaningful.[69] But can we give an account of 'evidence' in the absence of a theory? Arguably, evidence is 'about' a theory and that is what makes it evidence and not just some random observation. Equally, success, which the sciences definitely achieve, can always be gained by ad hoc modifications; indeed Kuhn tells us that through normal science a paradigm is maintained by 'numerous articulations and *ad hoc* modifications of their theory in order to eliminate any apparent conflict.'[70] If the success of science is explained in terms of the success of paradigms, which is accomplished by adjusting observation and theory to suit, then science takes on a twisted façade, a decidedly anti-scientific enterprise. Possible problems to consider for the strong reader are ones of meta-methodology, such as trying to discover a 'logic' of scientific discovery, a rational choice algorithm for deciding between paradigms or even a structure of successful theories that may remain stable across paradigms. The current attempts to flesh this out has been through structural realism, which still raises questions about whether mathematical structures are lost in

[67] Fuller, 'Being There', pp.270-271

[68] Michael J. Mulkay, 'Some Suggestions for Sociological Research', *Science Studies,* 1 (1971), 207-13, (p.207)

[69] One might argue for the social readings of Kuhn here as an antidote. Social theorists like Barnes, however, ultimately make the problems of a socio-cognitive analysis of science epistemological/ linguistic and not ontological. Barnes says 'technique, theory and language of observation were intelligible only in terms of each other.' Barry Barnes, *Scientific Knowledge and Sociological Theory* (London: Routledge Kegan Paul, 1974), p.50. Here one also has to understand whether Barnes means 'intelligible' as being synonymous with 'rational'? The debate of rationality as intelligibility is part of what made up the Strong Programme of sociology of science. What keeps this as a strong reading is that one cannot abstract the knowledge production of science from the social activities that give them meaning, sociologist then used this conceptual framework to inform empirical research on how science 'works'. This is part of what Pinch calls the 'radical interpretation'. Pinch, 'Kuhn – The Conservative and Radical Interpretations', p.473

[70] Kuhn, *Structure,* p.78 [Italics in original]

paradigmatic theory change?[71] Those who look to the logico-mathematical structure of theories continue the debate within the purest of 'about' languages. What Kuhn called 'paradigms' can be rearticulated as 'research programmes', 'research-traditions', or 'worldviews'.[72] For the strong reader, we are still trying to produce an abstract methodological account of how we interact with the world as a thing. This could be through logic, the structure of theories, or cognitive perception, which is then to make 'knowledge' about our worldviews the problem. Laudan's meta-inductive pessimism argument, about the reality of the objects that our theories describe, is a major threat for the methodological understanding of science.[73] Here, philosophers have pushed realism back to the essential parts of theory structure or come up with new models for reference between types and tokens.[74] Yet 'there is no consensus among those defending standard realism in the face of theory change.'[75] The discussions about reference and referent are 'about' the objects of our regional ontologies, does *A* refer to a real or fictitious *A*? These arguments overlook the very grounds for making such claims, that our words and experiences are already about something.

Stanford gives a useful contribution to this debate with his 'unconceived alternatives'. This states that throughout history scientists have failed to conceive alternative theories that are equally well-confirmed to the theories of the day by the available evidence and, crucially, that such alternatives eventually were conceived and adopted by some section of the scientific community.[76] For me this is an indication of why certain ideas were held over others due to what could be meaningfully said at any one point.

It is not just knowledge of how our best theories connect to reality that is the problem. Kuhn's linguistic and psychological 'turns' have generated a wealth of research into

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[71] Kyle P. Stanford, 'Pyrrhic victories for Scientific Realism', *Journal of Philosophy*, 11 (2003), 551–572 (pp.570-572); Anjan Chakravartty, 'Structuralism as a Form of Scientific Realism', *International Studies in Philosophy of Science*, 18 (2004), 151–171 (p.164)

[72] Lakatos, 'Falsification and the Methodology of Scientific Research Programmes', p.177; Laudan, *Progress and its Problems*, pp.70-120; Suppes, *Structure*, p.631

[73] Larry Laudan, 'A Confutation of Convergent Realism', *Philosophy of Science*, 48 (1981), 19–49 (p.45)

[74] Phillip Kitcher, *The Advancement of Science* (Oxford: Oxford University Press, 1993)

[75] James Ladyman, 'Structural Realism', *The Stanford Encyclopaedia of Philosophy*, Summer 2009, ed. by Edward N. Zalta <http://plato.stanford.edu/archives/sum2009/entries/structural-realism/>[accessed 18 April 2013]

how paradigms might be cognitive structures.[77] This psychological interpretation is understandable, in that, Kuhn does use concepts from psychology to demonstrate his points. Another characteristic of the strong reading is whether one takes Kuhn's comparisons as 'literal'. Kuhn talks about conversion experiences and the throwing of gestalt switches when one finally succumbs to the dominant paradigm.[78] The language of religious experience or cognitive switches implies that the act of being persuaded lies outside of evidence or compelling reason. If one does not become religious based on evidence or by reasoned argument – it would suggest a type of faith or evincing by rhetoric.[79] Kuhn goes on to draw the similarities between scientific crisis and paradigm change with that of political revolution.[80] The implication is that the success or failure of a paradigm is brought about by the satisfying or disenfranchising of its members, and from the chaos of political ambiguity parties will collect around central ideas or values that should constitute the new politics. Opposition groups will remain loyal to the old values and visions of society, but 'once this polarization has occurred, *political recourse fails*.'[81] Once divided on central ideological issues, there is no political standard that one can appeal too for the efficacy of institutional reform or societal structure. The two parties can no longer debate on any agreed or shared ground. For example, if one party believed that money should no longer play a role in the new system of exchange, how then would they debate on issues that implicitly relied on concepts such as, capital, credit, or taxation? As the reforms are outside of a common discourse, the parties then have to fall back on to 'techniques of mass

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[76] Kyle P. Stanford, *Exceeding Our Grasp: Science, History and the Problem of Unconcieved Alternatives* (Oxford: Oxford University Press, 2006)

[77] Bird minimising the linguistic element of Kuhn suggests that we look at 'discoveries and theories in cognitive science that are not conceptual in focus, for example the work on analogy, case based reasoning, cognitive habits, and quasi-intuitive connections'. Bird, 'What Can Cognitive Science Tell Us About Scientific Revolutions?', p.318

[78] Kuhn, *Structure*, pp.19; 115, 151-153; 158, 202-204 (for Conversion) and pp.63; 85; 111-118; 150; 188 (for Gestalt) - p.122 combines both in the phrase 'scales falling from the eyes' likening the gestalt switch to Saul to Paul's 'road to Damascus experience'.

[79] The locus for Kuhn's use of social metaphors and analogies in the revolutions of science may be traced to Toulmin who challenged the positivist idea that values were emotionally based and not open to rational argument. Instead of 'scientific rationality' and 'moral rhetoric' Toulmin suggested that both discourses were equally rational and rhetorical. That we can rationally investigate what is really good or bad and contest these claims as claims are contested in science. Stephen Toulmin, *The Place of Reason in Ethics* (Cambridge: Cambridge University Press, 1950), pp.84-85. He also limits logical treatment of non-formal statements which require 'experience, insight and judgement' rather than pure mathematical computation in *The Uses of Argument* (Cambridge: Cambridge University Press, 1958), p.173

[80] Kuhn, *Structure*, pp.92-94

[81] Ibid., p.93 [Italics in original]

persuasion, often including force.'[82] Kuhn presses on with the politico-scientific comparison, in that, when a scientist argues in favor of their paradigm, she can only do so from within the confines of that paradigm. They may only use evidence, logic, observation or a rationale that their paradigm allows. As there is no higher set of rules from which one can judge between 'paradigms', each person's argument becomes self-referential. This can be likened back to Wittgenstein's 'games' and asking what is common to all games in and of their structure. For example, what counts as 'castling' in my game only applies to chess and does not have its equivalent in tennis. Here the arguments for one paradigm over another 'cannot be made logically or even probabilistically compelling' for those who refuse to accept the premises of the opposing paradigm.[83] All attempts at converting the conflicting parties can be understood by the strong reader as acts of persuasion. The term 'persuasion' carries with it a sense of manipulation by an external force. Kuhn's referencing of Orwell's *1984* in how the outcomes of paradigmatic crisis are made to look like natural progression can, for the strong reader, put the impetus for scientific movement in rhetoric or a kind of 'brain washing' by historical manipulation.[84]

Part of this historical manipulation is what has come to be called 'Whig history', where we tell the story of the present as an inevitable outcome of the past.[85] A logical conclusion for the strong reader where evidence, logic and observation cannot be appealed to in preferring one paradigm to another implies that scientists are not occupying the same reality. Kuhn does not shy away from drawing the conclusion that scientists from different paradigms 'live in different worlds'.[86] It is this claim, more than most, that have led contemporary philosophers of science to label Kuhn a 'relativist', 'subjectivist', or 'irrationalist.' Yet, Kuhn tells us that scientists cannot just be slaves to the paradigm because science would never progress. Rather they need to balance an 'essential tension' as a professional.[87] A commitment to both tradition

[82] Ibid.,
[83] Ibid.,
[84] Ibid., p.167 – Also see George Reisch, "The Paranoid Style in American History of Science", *Theoria*, 27 (2012) <http://www.ehu.es/ojs/inde.php/THEORIA/issue/current> [accessed 28 October 2012]
[85] Nickels, 'Normal Science', p.171 – Kuhn cites the 'Plank effect' as a means to how ideas gain historical ascendancy. This is where those who dogmatically hold on to the old paradigm simply die out and those coming through schooled in the new paradigm take over. Kuhn, *Structure*, p.151
[86] Ibid., p.193
[87] Ibid., p.79

and innovation, or 'conservative and innovative imperatives'.[88] This for the strong reader, is a statement of epistemology, which can be accessed through social psychology and organization theory. So for the strong reader, if science is its paradigm and paradigms tells us 'what there is', there is a horribly circular relationship between knowledge, language and reality. We only know what reality is, through our perception of it, as described by our language. However, what we perceive is pre-determined by the paradigm, which is what we appeal too in order to find out 'what there is'. The strong reader collapses the distinctions between paradigm and world into the same thing. They are all 'about' each other. The problems this understanding produces will be discussed under the heading of 'incommensurability'.[89] The concept 'paradigm' is closely connected to the concept of 'normal science', and this will be the next concept to be given a strong interpretation. The counterpart to 'normal science' is 'revolutionary science' via 'crisis', which will also be developed.

7.5 - Normal and Revolutionary Science[90]

Normal science is not the same as a paradigm, but it does allow normal science to function, which in turn re-enforces what the paradigm is. People who practice normal science are committed to sharing the same rules and standards as each other. The consensus this sharing derives is what stabilizes the endeavor of normal science, allowing that research tradition to continue. In opposition to normal or mature science, there is immature science. An immature science is said to be pre-paradigmatic. It is immature as it has not agreed upon a single research tradition and is pre-paradigmatic as it is lacking consensus across the domain.[91] Here disagreement is at a fundamental level. What should count as an assumption, what should be taken as meaningful, what needs to be explained, and so on. Normal science, which is the result of paradigmatic dominance, is a 'relatively late acquisition in the course of

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[88] Fred D'Agostino, 'Naturalizing the Essential Tension', *Synthese*, 162 (2008), 275 – 308 (p.276)
[89] The strong version on pp.175-185 in thesis to be compared with its weak counterpart on pp.235-245.
[90] Fuller suggests that Kuhn's choice of terms is reverting back to a 'crypto-Aristotelian distinction between 'natural' and 'violent' motion, as captured in the normal/ revolutionary science contrast.' Fuller, 'Being There', p.256
[91] This inability to agree does not prohibit agreement and so even in the pre-paradigmatic space of science there are elements of paradigms that allow for discussion and debate. Kuhn, *Structure*, p.179

scientific development.'[92] During a science's fledgling years it proceeds without a paradigm to guide it. It is, however, the difference in development between that pre-paradigmatic history and where the subject is now that reveals the importance of 'normal science.' The early developmental stages of a science are characterized by a state of intense discussion and debate over fundamentals that precede work under a single paradigm. Kuhn qualifies this with the development of optics. '[N]o period between remote antiquity and the end of the seventeenth century exhibited a single generally accepted view about the nature of light.'[93] Pre-Newtonian paradigm, the scope of possible disagreement, minor consensus or lack of collective progress, is for Kuhn illustrative of a tradition working without a 'normal science'. It is the type of agreement that can be brought about by working under a single paradigm is that allows an immature practice to become a science proper. Kuhn, like Popper, uses examples from Ancient Greece to illustrate his points, but their conclusions for the strong reader are antithetical.

This is best exemplified by the exchange in *Criticism and the Growth of Knowledge*. Here, Kuhn was attacking the simplistic view that science progresses by the repeated attempts to falsify fundamental assumptions by way of bold conjectures. Kuhn's argument is that what Popper attributes to normal scientific research, is actually the reserve of crisis and revolutionary science. Indeed, science would get nowhere if a paradigm was constantly in dispute, which for Kuhn is evidence of there being no paradigm. Instead, Kuhn suggests once a paradigm is up and running it is down to normal science to maintain it and rather discussing the fundamental constituents of the paradigm, scientists become involved with 'puzzle solving'.[94]

The intricate and detailed investigations that paradigms permit in normal science explain, for Kuhn, science's productivity, which is galvanized when 'its members take the foundations of their field for granted.'[95] For Popper, science is demarcated from non-science by a methodological commitment to falsification, which is characteristic of critical rational discourse. For Kuhn, the evolution of an immature science to a

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[92] Thomas Kuhn, 'The Function of Dogma in Scientific Research', in *Scientific Change*, ed. by A. C. Crombie, (London: Heinemann, 1963), pp.347-369 (p.353)
[93] Kuhn, *Structure*, p.12
[94] Ibid., pp.35-42
[95] Ibid., p.178

mature one is when critical discourse has been completely abandoned and replaced by consensus over the fundamentals.[96] The strong reader, who is methodologically orientated, will take Kuhn's critique of Popper and falsification as either limiting the role of rationality, or offering in its place, a methodological substitute in consensus formation, which can be explained by rhetoric or psychological dispositions. This depiction of Kuhn's 'normal science' makes it closer to a cult than a science, with a closed society of followers, not open and free investigation. Above all, it is with the abandonment of critical discourse that science seemingly becomes an irrational activity and not the personification of 'Reason' that it is thought to be.[97] Kuhn develops the idea of normal science further in the relationship of puzzle to puzzle-solver. Here, we take 'puzzle' in the everyday sense of the word. The paradigm under which a normal science operates instructs what can be taken as a legitimate puzzle or not. The normal science of Newtonian physics would allow a principle like 'action-at-a-distance' to be legitimate, whereas within Einsteinian physics it would not.[98] One cannot be working in an Einsteinian paradigm and still accept faster-than-light phenomena. So, if a puzzle cannot be stated in recognizable terms, which the paradigm permits, it can be seen as either not scientific or having no proper scientific solution.

As many such problems are untranslatable in terms of normal scientific research they are then regarded as too complex, poorly formulated, or too vague to warrant scientific investigation. Normal science, if performed well, will convince the puzzle-solver that there is 'nothing to see', as it were, in the foundations of that science and

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[96] Kuhn, 'The Logic of Discovery or the Psychology of Research', p.6

[97] Fuller argues that that the Popperians were following an eighteenth century 'Enlightenment' concept of reason when critiquing Kuhn's work, so they saw him as an irrationalist for valorising 'the unreflective practices of normal science over the reflective ones of normal science'. Fuller, 'Being There', p.251. Fuller draws the distinction between eighteenth (Enlightenment) and nineteenth century (Positivistic) conceptions of 'reason' which, he argues, Kuhn periodically conflates. If one can pull apart these two concepts, I think this compliments my strong - weak reading of Kuhn. It is generally regarded as the positivistic notion of reason that Kuhn was challenging with his philosophy of science, however, 'normal science' for Kuhn was based on positivistic principles, where an accumulation of knowledge and steady 'progress' is made. It is because of the normative positivistic practices of normal science that it is able to enter into a state of crisis where Kuhn then switches to the 'Enlightenment' form of 'reason'. Ibid., p.251

[98] Mary Hesse examines the conceptual problems with 'action-at-a-distance' noted during Newton's time. She says that Leibniz thought that this principle was 'occult' and as it did not adhere to the mechanistic Cartesian model. Newton's response was that if mass and acceleration are known one can work out the attractive force from the laws of motion. Gravity as 'action-at-a-distance' was simply a description of phenomena and not an 'occult quality'. Mary B. Hesse, 'Action at a Distance in Classical Physics', *Isis*, 46 (1955), 337-353 (pp.339-340)

instead re-direct their attention to surface problems. It is with these sorts of puzzles that most scientists are involved with. Such is their commitment to the footings of that science, and the authenticity of their puzzles, that they may be prepared to dedicate a whole professional life or equally risk academic isolation in pursuing a pseudo-puzzle. For example, Brian Josephson shared the Nobel-Prize for his theoretical work in superconductivity, which led to the discovery of a new phenomena that now bears his name (Josephson Effect), along with several applications of it (Josephson Junctions).[99] He then 'quit mainstream physics and became preoccupied with paranormal phenomena.'[100] In a booklet for the Royal Mail, for their one hundredth anniversary of the Nobel-Prize stamp collection, he wrote, 'quantum theory is now being fruitfully combined with theories of information and computation. These developments may lead to an explanation of processes still not understood within conventional science such as telepathy'.[101] The reaction from the physics community was unanimous in condemning such ideas. Yet as Strogratz points out, 'a hundred years ago, no one would have believed that electrons could synchronize by the billions and pass through impenetrable barriers.'[102] In the puzzle-solving analogy the normal scientific community not only regards Josephson as being a bad puzzle-solver, as his puzzle does not fit that of accepted normal practice. They have actively tried to limit his ability to put forward new puzzles, as they would appear to be counter-instances to the accepted paradigm.[103] Yet Josephson, 'by the rules' is among the best puzzle-solvers for winning the Nobel-Prize, and with that distinction he remains

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[99] The work that won him the Nobel Prize was complete prior to his being awarded a PhD. He was 33 when receiving the Nobel Prize, equal fourth youngest of all time.
[100] Steven Stogratz, *Sync: The Emerging Science of Spontaneous Order* (London: Penguin Books, 2004), p.150
[101] Ibid., p.151
[102] Ibid., p.152
[103] The maintenance of a paradigm is not limited to just practicing scientists but also to supporting institutes such as education and popular media. Josephson being named as the recipient of the Nobel Prize in Quackpottery – GrrlScientists, 'Noble Prize in Quackpottery: Physics,' *The Guardian*, 9th Oct. 2012 <http//:guardian.co.uk/science/grrlscientist/2012/oct/09/nobel-prize-quackpottery-physics> [accessed Jan 3 2013]. Of equal note is the Noble Prize winner Julian Schwinger who had heated exchanges with editors of journals after papers were refused publication on the subject of 'cold fusion'. As a result he resigned from the board of the 'American Physical Society'. He said of this time that, 'peer review based on rigid preconception is censorship.' Jagdish Mehra and Kimball A. Milton, *Climbing the Mountain: The Scientific Biography of Julian Schwinger* (Oxford: Oxford University Press, 2000), pp.550-552. More recently Andrea Rossi has proposed that 'cold fusion' is possible and has secured patents over a cold fusion reactor. Whilst some still see this as a hoax, Steven B. Krivit, "Report 3: Scientific Analysis*", New Energy Time Magazine*, 37 (2011), July 30 <http://newenergytimes.com/v2/news/2011/37/NET370.shtml> [accessed 15 Sept 2012]. Others are taking seriously the implications for this claim, Christos Stremmenos, 'A Detailed Qualitative

emeritus Professor of Physics at Cambridge University. Using our science as 'method' or 'knowledge' description then, can we really say Josephson does not understand what science is as he is using ideas that contravene currently agreed normal scientific practice. Cases like Josephson show that normal science 'does not aim at novelties of fact or theory and, when successful, finds none.'[104] Normal science, if done well, should not produce anomalies or data to the contrary. It should not call forth new phenomena or aim at the production of new theories. It should only direct itself 'to the articulation of those phenomena and theories that the paradigm already supplies.'[105] Yet, the almost esoteric level of investigation that normal science conducts, weakens the joints of its own paradigm by constantly coming into conflict with anomaly, through its routine practices, that eventually lead to its displacement. This constant conflict with anomaly can, if not managed well, develop into a state of crisis and one which eventually leads to a revolution and replacement of part or all of the hosting paradigm. An anomaly is 'the recognition that nature has somehow violated the paradigm-induced expectations that govern normal science.'[106] Anomalies are found, not by 'testing' (as this is not the aim of normal science), but by repeated failure to solve a puzzle.[107]

For the strong reader the term 'puzzle-solving' is deliberately used over 'testing'. The analogy being that one completes a jigsaw or crossword, but does not test it. Puzzles only really tell us about puzzle makers and our ability to agree over proposed solutions. They do not tell us why we have puzzles or why puzzles should be solvable. It is only when we reach the stage of puzzles, when many puzzle-solvers have had a go and agree that the puzzle is not genuine and something more fundamental is amiss, do we then get some kind of testing. The puzzle-solvers then go away and look at all the old completed puzzles to see where they might have gone wrong. This is where testing of the rules of puzzle-solving occur and it is not until most major anomalies can be accounted for by a new theory do we start to usher in a new paradigm. The new paradigm will make sense of the old missing pieces of the previous paradigm. For Popper, where 'paradigm' was linked to our historical

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Approach to the Cold Fusion Nuclear Reactions of H/Ni', *Journal of Nuclear Physics*, May (2011), <http://www.journal-of-nuclear-physics.com/?m=201105> [accessed 13 Sept 2012]
[104] Kuhn, *Structure*, p.52
[105] Ibid., p.24
[106] Ibid., pp. 52-53

perception of reality this relationship was still a 'logical' one. In his understanding of 'paradigm' as dominant theory and his 'framework' metaphor as 'mutually untranslatable languages', he kept the discussion fixed as a methodological suggestion.[108] That is, we are still dealing with a language-knowledge relationship. Popper concluded that Kuhn's description of normal science depicted the world of applied science, not pure science.[109] That Kuhn's formulation was predicated on relativised knowledge and ultimately was a threat to how science was conducted. Popper, however, did agree that science was confined to a 'framework' and that we were, in a sense, prisoners to this framework, but unlike Kuhn, we were able to critically assess the dimensions of this framework.[110] Watkins puts it stronger still, 'normal science (in which there is not really any testing of theories), is genuine science; Extraordinary science [revolutionary science] (in which genuine testing of theories does occur) is so abnormal, so different from genuine science, that it can hardly be called a science at all.'[111] Watkins, building on Popper's interpretation, takes Kuhn's description to be either psychological or sociological in nature, leaving the testing of theories within normal science as impossible.[112] Watkins then goes on to compare Kuhn's concept of 'normal science' with that of a theologian trying to gloss over the inconsistencies in two passages of the Bible.[113]

Kuhn, arguing against Popper, that falsification cannot be the mark of what makes one thing a science and another not. He takes as one of his marks the development of the geo/ heliocentric models of the solar system, but elsewhere he uses the examples of astrology. Both made predictions that failed, both accounted for those failures within terms of the system that made them. Here Kuhn is saying that failed predictions can amount to the falsification of a theory or, what is more likely, and more routinely practiced, is it can be overlooked or explained by additional factors. Kuhn says, 'of the two criteria, testing and puzzle-solving, the latter is at once the less equivocal and the more fundamental' to science.[114] Here again, we notice that Kuhn is

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[107] Ibid., pp.35-42; pp.144-145
[108] Karl Popper, 'Normal Science and its Dangers', in *Criticisms and Growth of Knowledge*, ed. by Imre Lakatos and Alan Musgrave (Cambridge: Cambridge University Press, 1999a), pp.51-58 (p.56)
[109] Ibid., p.53
[110] Ibid., p.56
[111] Watkins, 'Against 'Normal Science'', p. 29
[112] Ibid., p.28
[113] Ibid., p.33
[114] Kuhn, 'The Logic of Discovery or the Psychology of Research', p.7

not just using ‘puzzle’ as synonym for ‘testing’. Since Kuhn is arguing for the principle of ‘puzzle-solving’ over ‘falsification’, along with pretensions to indoctrination within the dominant paradigm, science takes on the guise of a dogmatic belief system, closer to a religion than what we would ordinarily call science. Likewise, in comparing what scientists do to a professional puzzle or crossword solver, implies there is no necessary connection between their solutions and what there really is.[115] With these two descriptions combined, ‘science’ appears to be closed, insular, cult-like, narrow-sighted, and against the strongest protests of the Popperians, not looking to prove itself wrong. If anomalies are always being overlooked and falsification is only a last resort, if the paradigm fails to support itself – how then is science different to a religion? How is religious belief different to scientific fact? Is its’ rationale based on the same ground as the meta-physical and pseudo-scientific systems it looks to debunk, replace or explain? Here we can understand religious or political discourse and normal science as being ‘about’ the same sort of thing, that is, maintaining dominance as a belief system. To take Kuhn’s conception of normal science, as part of a logic of science, we either get serious problems with science as a practice or a conception of science that can be used to justify ‘fringe’ practices, that then become associated with ‘abstract philosophy.’[116]

So how does one move from a normal science, a tradition that is insular, blinkered, non-seeking of novelty and lacks testing, to a place of ‘revolutionary’ science? The preceding paradigm has to have entered into a state of crisis, so much so, that the rules, puzzles and solutions as promised by the ruling paradigm constantly fail. This then leads scientists to look for alternative perspectives with which to explain these failures. How a paradigm gets itself into this state of crisis is not just a failure of

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[115] Solutions to puzzles from within the strong reading argue that we can only ‘solve’ puzzles by empirical adequacy according to the structure of theories. Here we do away with absolute notions of ‘truth’ and instead only have how parts of a theory relate to one another. In the work of Sneed (1971; 1976) and Stegmüller (1976) they make the distinction between ‘core theory’ and ‘expanded core theory’. ‘Core theory’ is for example Newton’s second law from which all his other laws of motion are derivable, which would be the core theory ‘expanded’. Thomas S. Kuhn, *The Road Since Structure: Philosophical Essays 1970-1993,* ed. by James Conant and John Haugeland (Chicago: The University of Chicago Press, 2000), pp.185-186.

[116] Hanegraaff links the publication of *Structure* with New Age beliefs that science as traditionally conceptualised as a reductionist paradigm is to be replaced by more holistic ones. Wouter J. Hanegraaff, *New Age Religion and Western Culture: Esotericism in the Mirror of Secular Thought* (New York: New York University Press, 1998), p62. This conception of science as just one practice among many each with its own paradigm is what I imagine Wolpert is objecting to in the House of

theory against observation. In Kuhn's example of the 'Copernican Revolution' as a paradigmatic crisis, he points to additional factors as driving this change. It was the case that planetary movements, eclipses and equinoxes were meeting with the discrepancy of observation, but the reasons people were looking for accuracy in the first place was so that reliable calendars could be made. It was the social pressure of calendar reform, such as knowing when the date for Easter fell, that partly drove the development of the celestial models, and partly, why so much attention was placed in one area where 'the break down first occurs'.[117] For Kuhn, a new paradigm is not a re-articulation of an old paradigm, but a 'reconstruction of the field from new fundamentals.'[118] For the strong reader, 'fundamental' here is understood as fundamental ontology.[119] The weak reader understands any ability to articulate a field begins with its regional ontology. For even if we are re-stating something that is fundamental to a science, such as what type of space-time geometry we believe to be the case, this is still regional ontology. Yet, as the strong reader discerns no difference between 'paradigm' (what we say about the world) and the world itself (as a non-entity) this reconstitution of ontology is understood as fundamental. It is with 'crisis' that the conflation between regional and fundamental ontology creates greater problems. It is with the paradigmatic shifts in knowledge that the methodological description of science becomes endangered. Here, the strong reader has no recourse to a methodological principle by which the truth of one paradigm can be assessed over another. This then also endangers those faculties and ideas that allow us to perform science in first place, such as reason, objectivity and truth.

The 'transition' from one paradigm to another and the subsequent changing of how one views the field has been of significant interest to readers of Kuhn. In particular, his analysis of how or why one would choose between competing paradigms. On a strong reading, revolutions in science, to follow Kuhn's language, are conversion experiences, or gestalt switches. The term 'revolution' itself has political connotations. A revolution being a violent upheaval of norms and traditions brought about, not by consensus, reasoned argument, or appeal to evidence, but as an

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Lords report. House of Lords, *Science and Technology - Sixth Report* (London: HMSO, 2000), para. 2.17

[117] Kuhn, *Structure*, p.69

[118] Ibid., p.85

[119] Ibid., p.40

expression of dissatisfaction with the current status-quo and that change can only be brought about by force. Once the revolution is underway, why or how one is converted can lie in factors outside of the discourse of science. So that one becomes a proponent of a particular party of science for non-scientific reasons.[120] The idea of conversion, or understanding by gestalt switch, implies an instantaneous shift in worldviews. The idea that scientific change between paradigms is a forced affair by way of rhetoric or literal visual-field alteration, with small appeal to evidence or scientific reasoning due to the paradigms controlling influence, means that competing paradigms are often 'incommensurable' with each other.[121]

This is where I would like to look at the strong understanding of 'world' as the difference between 'world', as the planet we literally inhabit, and 'world' as non-entity that structures our experiences. It is in this the first sense that the strong reader will proceed with Kuhn. The 'world' is what our knowledge and experiences are 'about' and science as a methodological approach is 'about' the reality of this world. This is a purely discursive notion of world with definite properties and qualities. Scientists, like the layman, live in this world as an object that occupies space. The implication of 'world change', if taken literally, pushes forward the agenda of relativism as two people can now occupy two different conceptions of world and with it two different 'about' languages.[122] Due to their language/ knowledge being about two different things, it is a short step to radical 'incommensurability'. What is more, any non-literal way our strong reader can conceive of 'world', in paying Kuhn poetic license, will be limited by the methodological representation of it. Hence, Masterman finds Kuhn's concepts vague or crude.[123] Here we still have to explain how the 'world', however non-literal, changes with scientific knowledge and how this might be achieved.[124]

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[120] Kuhn references the role of sun worshiping in Kepler's conversion to Copernicanism. Kuhn also cites the role of biography and personality in whether one follows a paradigm or not. Ibid., p.153
[121] Kuhn, *Structure,* p.4
[122] This has been referred to as the 'new-world problem'. Juan V. Mayoral, 'Five Decades of *Structure*: A Retrospective View', *Theoria*, 27 (2012), 261-280, (p.276)
[123] Masterman, 'The Nature of a Paradigm', pp.80-81
[124] Hoyningen-Huene gives three causes for misunderstanding of Kuhnian 'incommensurability'. First is due to simultaneous use of it by Kuhn and Feyerabend and how they have differed over time. His second and third reasons are tied to my strong and weak readings. The second reason points to this kind of 'pre-understanding' that this confusion may come from. For 'in a particular conception of incommensurability all the essential elements of the respective philosophical position are present in concentrated form […] an understanding of a certain conception of incommensurability presupposes an

7.6 - Incommensurability

In the failure to give a complete account of the scientific enterprise, by a ‘logic’ of discovery, or that science works by a number of methodological principles, such as falsification, this leaves our strong interpreter of Kuhn with yet another problem. It is obvious that science is one of the most successful endeavors ever taken up by humans and yet we cannot adequately account for its success by its own methods. For if science is progressing in knowledge, becoming wider and deeper in its understanding of nature, and its advances are objectified by the development of better and more sophisticated technology, why cannot science give an account of itself? Some may then be forced to jump from the observation that science is unable to account for its own success, such as a theory of successful theories, to the conclusion that either ‘paradigms’ are some mystical organizing property, or, science’s success is based on more than just methodology. The latter has been of great interest to philosophy, sociology and cultural studies, but still suffers from the strong-weak distinction that I have been arguing for.[125] A strong form of this ‘more than just methodology’ that constitutes science’s success can be expressed in the dualisms of absolutism/objectivism/realism versus relativism/ subjectivism/anti-realism. This debate has been going on in philosophy, sociology and history of science for some time and does not show signs of ending anytime soon. Kitcher recognizes the futile tennis match that is scepticism about scientific progress and argues for the establishment of a serious historical epistemology. He says this is, ‘not needed merely to free us from the nagging of annoying skeptics [but] it can also play a powerful elucidatory role in domains of knowledge where our understanding of what the practitioners say and do is cloudy or incomplete.’[126] What he singles out as historical

overall understanding of the respective philosophical position.’ The third reason is he treats incommensurability as a ‘methodological problem’ which is in line with my strong reading. Paul Hoyningen-Huene, ‘Kuhn’s Conception of Incommensurability’, *Studies in History and Philosophy of Science*, 21 (3) (1990), 481–492 (p.482)

[125] Pinch gives a possible account of where this has been the case for sociology of science. Sociology of science has read Kuhn both as a normative account of science and as an interpretative tool. But rather than critically engaging with Kuhn’s work they have either taken Kuhn’s account to be a criterion for the structure of science or how scientists as actors negotiate meanings within a paradigm. Due to the hermeneutic or interpretative methodology being so pertinent to sociological problems, it has thus framed scientific knowledge as a social problem by way of epistemology or linguistics. Pinch, ‘Kuhn – The Conservative and Radical Interpretations’, pp.472-473

[126] Philip Kitcher, ‘Epistemology Without History is Blind’, *Erkenntnis*, 75 (2011), 505-524 (p.514)

epistemology, I expand to public discourses of philosophy, where we may not necessarily just look at scientists, but also at what sceptics say and do.

A bridging point from our discussion of 'paradigms' that will carry with it, on a strong reading, the deeper philosophical problems of 'progress', 'objectivity' and 'rationality', is in chapter IX of *Structure*, where Kuhn writes,

> [A]s the problems change, so, often, does the standard that distinguishes a real scientific solution from a mere metaphysical speculation, word game, or mathematical play. The normal-scientific tradition that emerges from a scientific revolution is not only incompatible but often actually incommensurable with that which has gone before.[127]

Kuhn appears to be saying that two paradigms are not only mutually 'incompatible', but also 'incommensurable'. Much has been made of this argument, that due to no objective third position, two paradigms cannot be evaluated for their comparative truth content.[128] As what was once a genuine scientific puzzle or assumption in one paradigm, such as classical Euclidean geometry for Newtonian physics, is no longer a part of the puzzles or background assumptions of a different paradigm, such as, the non-Euclidean geometry of Einsteinian physics. The problems that the background assumptions raised for Newtonian physics, such as absolute space-time, now no longer exist in the new paradigm, but have changed to include different phenomena or predictions, that could not have been considered under the old paradigm. Moreover, with the retention of terms from one paradigm to the next we are faced with the concern of meaning variance over time. The problem then becomes, even if a new paradigm does incorporate a lot of the previous paradigm, do the concepts and terms still mean the same thing?[129] Hacking expresses the linguistic-epistemological

[127] Kuhn, *Structure*, p.103
[128] Allen Franklin argues for theory neutral procedures to decide between paradigms but what measurements are deemed as relevant and how they are made stem from already having accepted both paradigms. Allen Franklin 'Are Paradigms Incommensurable?', *The British Journal for the Philosophy of Science*, 35 (1984), 57-60 (p.58)
[129] Dreyfus gives a similar account of science if we take interpretation of language as our prime mover. If we put language first and accept that there is no third position from which to judge the objective truth of one paradigm over another, we can set up the difference between natural science and social science as not being one of true or false statements but the 'confrontation of incommensurable holistic

significance of this for physicists when he writes that 'Newton's term 'mass' may not even mean what it does in Einstein's relativistic physics'.[130] So how do scientists know that they are measuring or talking about the same thing? Here, we start to venture into the realms of linguistic relativity and radical meaning variance. Where in order to understand another person's language or culture we must first maximize what is obviously true or false, and from this we can then begin to understand what is common to experience. This view of interpretation by radical translation, where we put language first, can also be seen in the work of Geertz.[131] For any sociology or philosophy of science that makes language or epistemology fundamental to the limits of the scientific process, we then can mistake what science is 'about' (things that can be said with a high probability) for its 'of' language (what can be said). For if only language constrains what can be known or how we know it, ideas central to science then become a 'language construct like any other concept.'[132] To start with language or knowledge as an expression of language in the strong reading, is to metaphysically fence oneself in. To begin with either is to start at the end of a process or chain of reasoning, not the beginning.[133] What I mean by this is that to get to a stage where we can treat cultural, sociological or philosophical terms in the abstract, is to begin from a place where those terms already make sense to us. So, for the sorts of analysis and investigating where we can abstract the world into 'things', so it is 'about' language or knowledge production, is to take up a highly specialized relationship with the world. So to understand reality in relation to Einsteinian physics so that 'change' is *about* entropy or thermodynamics takes years of training. This way of viewing things is highly contrived, but it is the strong reader who inverts this as to understand that

interpretations'. Hubert Dreyfus, 'Holism and Hermeneutics', *The Review of Metaphysics*, 34 (1980), 3-23 (p.4). Dreyfus also argues that through Føllesdal, Rorty's interpretation fits with Quine and Davidson's development of 'radical translation'. Thus we are still dealing with how language contacts with the world if the world is what changes. Ibid., p.4

[130] Ian Hacking, 'Introduction', in *Scientific Revolutions*, ed. by Ian Hacking (Oxford: Oxford University Press, 1981), pp.1-5 (p.3)

[131] Geertz looks to gain access to foreign conceptual worlds by a semiotic approach to culture. The 'worlds' that cultures come from also made Geertz a proponant of relativism for understanding anthropological claims. Clifford Geertz, *The Interpretation of Cultures* (New York: Basic Books, 1973); 'Distinguished Lecture: Anti Anti-Relativism', *American Anthropologist*, 86 (2), June (1984), 263-278

[132] Erickson, *Science, Culture and Society*, p.67

[133] The work of Fleck, whom Kuhn was heavily influenced by, can be read as making 'thought' central to scientific progress. Kuhn, *Structure*, p.ix. Fleck argued for the centrality of 'thought communities' to the operation of science, where communities form 'around any work of the mind, such as a dogma of faith, a scientific idea, or an artistic musing'. Ludwick Fleck, *Genesis and Development of Scientific Fact* trans. by T. J. Tren & R. K. Merton, foreword by T. S. Kuhn (Chicago: University of Chicago Press, 1981), p.105

our ways of being-in-the-world ('of' languages) emanates from our 'about' languages and are not the basis for them. To consider the methodological stance as primary, is to place an 'about' language prior to an 'of' language, which leaves the person with little room for understanding how statements about science can be anything other than methodological.

If we place the source of incommensurability at the level of language, be it linguistic or textual, it can be argued that there is no objective third position from which to evaluate the problems of language. What is at stake in such an interpretation is that science, as well as philosophy, becomes in Rorty's words, just another 'kind of writing'.[134] Here the claims of science can be placed alongside other literary genres or tropes. The simple one-to-one correspondence of science texts can thus be restated in socio-political or economic terms as a form of classic bourgeois realist interpretation, that occurs only through habit rather than any necessary relationship.[135] Here the misunderstanding of the weak position can set philosophers such as Derrida, Heidegger, or Feyerabend up as 'postmodern', in that their suggestions are interpreted as being methodological substitutes for 'truth', 'real' or 'objective'. If this is the case, not much can be done with these as ideas, as a notion of 'truth' that tries to undermine methodological propriety or logical form cannot be used in any positive sense for making claims about the world. For that reason, it is possible to write that 'Heidegger and Derrida are emblematic figures who not only do not solve problems, they do not *have* arguments or theses.'[136]

As words like 'true' have their methodological expression as being 'about' something, which are always part of a process that make these notions known or meaningful. Yet, we by-pass this process and begin from an abstract conception of how language, mind, knowledge and world all hang together. If our language is always about something, to then bring that 'something' into doubt challenges this relationship. Rather, if we understand that the language 'of' communication is to be in a world that allows for meaningful statements to be expressed, such that our language already

[134] Richard Rorty, 'Philosophy as a Kind of Writing: An Essay on Derrida', *New Literary History*, 10 (1978), 141- 160 (p.143)

[135] Just within the field of literary criticism there has been significant resistance to any ideas of a 'rhetoric of infintized textual 'freeplay'' or a severance of experience and language. Christopher Norris, *The Truth About Postmoderism* (Oxford: Blackwell Publishers, 1993), p.197

refers to a world that has a history and our ideas are culturally bound to it. So as with Wittgenstein's 'private language' argument, our language can never just be 'about' something as it comes from a lived process (an 'of' language), which is itself non-linguistic. This argument will be developed more as part of the weak reading of 'incommensurability'.[137] I will do this by placing the level of incommensurability in and around the problems of language and knowledge ('about'), rather than paying attention to the conditions and practices that give debates about theories of meaning or truth their intelligibility ('of'). Here, if we understand that incommensuability occurs as a result of a mismatch between our 'about' languages, we then have space to re-position what our language or knowledge is about. However, if like the strong reader, we think that incommensurability is a product of our 'of' languages, all those unpleasant logical problems to do with 'truth' arise. Yet, the very fact we are able to have the debate should make the even the strongest reader sceptical. That is, if strong incommensurability were really a problem we would not even be able to discuss it, let alone argue that certain positions are untenable.

Kuhn states that we have to accept that when a paradigm changes the standards governing 'permissible problems, concepts and explanations' also change.[138] The idea is that along with the paradigm, the contents of the paradigm change as well. Its concepts, methods, phenomena, what counts as evidence, and so on, all change. The terms may stay the same, but their meanings can change. In order to make sense of paradigm change, and what gives science the illusion of a fluid, incremental trajectory through time is that,

> [P]ast 'paradigms' are reinterpreted by current 'paradigms' to appear as if researchers of the past had been involved in the same projects as current researchers. Current historians of science read the past texts of science from the perspective of their own dominant paradigm thus,

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136 Rorty, 'Philosophy as a Kind of Writing', p.143 [Italics in original]
137 Pp.235-245 in thesis.
138 Kuhn, *Structure,* p.106

> they view past research as less competent attempts to disclose the same world.[139]

This idea takes up an argument in *Structure* that is aimed at the positivistic view of science, where scientific knowledge is progressive and accumulative. The stronger implication is that later sciences cannot be derived from earlier ones, as the two do not and cannot share the 'same world'. The two worldviews are 'incommensurable'. Much has been made of this idea, almost to the point where it dominates the theme of *Structure*, as if the whole book were an argument for 'incommensurability', but in fact it is only mentioned about a dozen times.[140] Strong Incommensurability as a central idea follows nicely from chapters, such as, 'Revolutions as Changes of World View'.[141] For if language and how we perceive the world are so tightly knit, something like a paradigm shift alters what the literal-world is like for us. Here Kuhn attempts to give a psychological account of why the world can take on different appearances, to different people, at different times.[142] With this he is questioning the methodological attempt to produce a neutral observation language, when data/ evidence would appear not to be fixed, if the phenomena the paradigm permits can be so radically different in interpretation. If an object is given meaning by a theory and a theory is given meaning in relation to a paradigm, there is a sense in which people from different paradigms 'see' different things. How literal and far one takes this conclusion is a part of the strong interpretation.[143] A strong reading of Kuhn takes both 'incommensurability' and 'world change' as a doctrine for relativism or a form of subjectivism that would make the scientific enterprise irrational or even nihilistic. The worry here is that scientific truths are not transcendental, but paradigm relative.

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[139] Brent Dean Robbins, 'A Reading of Kuhn in Light of Heidegger as a Response to Hoeller's Critique of Giorgi', *Janus Head*, 1 (1998) <http//:www.janushead.org/JHSummer98/BrentRobbins.cfm [accessed 17 July 2012]

[140] Later Kuhn changes his position on this moving the focus from 'paradigms' to 'incommensurability' as being of central importance to *Structure*. Kuhn, 'Afterword', pp.314-315

[141] Kuhn, *Structure*, pp.111-135

[142] A point to be made here is that people who accuse Kuhn of being a relativist or irrationalist neglect to see that even on a strong reading, he is trying to use the 'science' of psychology to explain elements of scientific development at the subjective level.

[143] Rorty's 'radical translation' thesis states that interpretation is a translation between two or more theories, which for Rorty meant there was no essential difference between the natural and social sciences. Richard Rorty, *Philosophy and the Mirror of Nature* (Princeton, NJ.: Princeton University Press, 1979), p. 352. Norris also notes how Rorty is propozing a 'literalized verison of Kuhn's arguably

Kuhn does not give a definition of 'incommensurability' in *Structure*, but in order to derive the sorts of problems that a strong reading encounters we can take incommensurability to mean two things that are 'incomparable' or 'incompatible'. This incomparability is not just of paradigms, but everything they contain, theories, beliefs, metaphysics, methods, standards for evidence and rationale.[144] Kuhn introduces the concept early on in *Structure* 'what differentiated these various schools was not one or another failure of method (they were all 'scientific') but what we shall come to call their incommensurable ways of seeing the world and of practicing science in it.'[145] Later on in his discussion, when he analyses the nature and the necessity of scientific revolutions, he says 'the normal-scientific tradition that emerges from a scientific revolution is not only incompatible but often actually incommensurable with what has gone before'.[146] Kuhn inquires as to why proponents of competing paradigms 'may hope to convert the other to his way of seeing his science and its problems [but] neither may hope to prove his case'.[147] He singles out three reasons why 'the proponents of competing paradigms must fail to make complete contact with each other's viewpoints.'[148] We may already note the 'religious' overtones of the use of the word 'convert' – that choices between paradigms are not based on evidence, but more on what seems like a form of revelation or blind faith. 'In the first place, the proponents of competing paradigms will often disagree about the list of problems that any candidate for paradigm must resolve. Their standards or their definitions of science are not the same'.[149] '[Yet] more is involved than the incommensurability of standards'.[150] Thus, for example, to make the transition from Newton's to Einstein's world, 'the whole conceptual web whose strands are space, time, matter, force, and so on, had to be shifted and laid

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metaphoric claim that 'the world changes' with every major shift of scientific paradigms.' Christopher Norris, *Epistemology: Key Concepts in Philosophy* (London & New York: Continuum, 2005), p.170

[144] If we see Kuhn's description as a theory about paradigms framing beliefs from which our practices derive we still have to explain how those beliefs originate and are maintained. Quine expresses this idea, 'hypotheses in various fields of inquiry may tend to receive their confirmation from different kinds of investigation, but this should in no way conflict with our seeing them all as hypotheses. We talk of framing hypotheses. Actually we inherit the main ones, growing up as we do in a going culture. The continuity of belief is due to the retention, at each particular time of most beliefs.' Willard van Orman Quine and Joseph S. Ullian, *The Web of Belief* (New York: Random House, 1978), p.81

[145] Kuhn, *Structure*, p.4

[146] Ibid., p.103

[147] Ibid., p.148

[148] Ibid.,

[149] Ibid.,

[150] Ibid.,

down again on nature whole.'[151] In a revealing statement about the disparities between these worlds Kuhn says,

> In a sense that I am unable to explicate further, the proponents of competing paradigms practice their trades in *different worlds*. One contains constrained bodies that fall slowly, the other pendulums that repeat their motions again and again. In one, solutions are compounds, the other mixtures. One is embedded in a flat, the other in a curved, matrix of space. Practicing in different worlds, the two groups of scientists see different things when they look from the same point in the same direction […] Equally, that is why, before they can hope to communicate fully, one group or the other must experience the conversion that we have been calling a paradigm shift. Just because it is a transition between incommensurables, the transition between competing paradigms cannot be made a step at a time, forced by logic and neutral experience. Like the gestalt switch, it must occur all at once (though not necessarily in an instant) or not at all.[152]

The strong reader will take Kuhn's notion of 'different worlds' as literal, that is to say, there will be two sets of languages and knowledge, that are about two different worlds. Our knowledge of the world is tied up with our beliefs and language through which we represent the world, but due to our occupying two different spaces consensual agreement is almost impossible. Their paradigms are *about* two different things. An incommensurability that is framed as this means that person *a* and person *b* can literally experience the world in two contradictory ways because the world is different for them.[153] There can be no mutual point of reference over which to agree about things. Hoyningen-Huene has a sense of this strong reading with the

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[151] Ibid., p. 149
[152] Ibid., p. 150 [My Italics]
[153] A strong-weak reading of Wittgenstein would be that you can either think that the *Tractatus* is an argument for the limitation of language, logical representation and how philosophy and all other languages are necessarily second-order activities and so we must persist with logic. Or that because the majority of meaningful human life cannot be logically expressed there is necessarily more to 'understanding' than logical form. So when Wittgenstein says, 'the world of the happy man is a different one from that of the unhappy man', we can take this as a form of incommensurability. Ludwig Wittgenstein, *Tractatus Logico-Philosophicus*, trans. by D. F. Pears and B. F. McGuinness (London: Routledge Classics, 2001), p.87

methodological conception of incommensurability. He says, 'to gain knowledge about the subjects of world constitution means to treat them *as objects belonging to one's own world*, and this implies the use of substantial parts of ones own idea of reality.'[154] The problem then is that one cannot get at a general theory of world independent of 'ones own idea of reality'.[155] A possible source of this linguistic bind can be traced to Wittgenstein, but by the same token, what elements one stresses in his philosophy also producs a strong/ weak interpretation.[156] The strong readers can just as easily situate their position as the result of this or that language-game. So their inability to express, compare, or validate the representational worldview can be by either a relativist account of truth or by investigating further the methodological relationship between language, knowledge and world. If we take incommensurability to be a mismatch between two points, each with its own subject-object language representation, then to map one point onto the other we would have to account for 'meaning variance'. A strong reading has Kuhn as advocating 'radical meaning variance' (RMV). For if all the terms of a theory derive their meaning in relation to one another from within the theory, the change in meaning of any one term can give rise to a change in all terms. So, if Einstein and Newton were describing different things or their terms had different meanings, then the theories are not in competition with each other as they are not theories *about* the same thing. This sort of conclusion is directly against the traditional view of science as an evolutionary progession of theories. Newton-Smith, argues for RMV in Kuhn, and goes to great lengths to show that it is irrational and a hindrance to scientific Enquirer.[157] RMV also has implication

[154] Hoyningen-Huene, 'Kuhn's Conception of Incommensurability', p.492 [Italics in original]
[155] Ibid., - Hoyningen-Huene suffering the logical conclusions of this strong reading resigns himself to saying, 'I must admit that I don't know what do to in this methodological situation' Ibid.,
[156] Wittgenstein writes, 'for doubt can exist only where a question exists, a question only where an answer exists, and an answer only where something *can be said*.'Wittgenstein, *Tractatus*, p.88 [Italics in original]. This 'what can be said' is either the beginning of inquiry such as with the logical positivsts or philosophically tells us about the game we are entering into when we take up a logico-propositional view of reality. The deeper claims of the *Tractatus* which come in the last portion of the book (weakly read) are aimed at an ontology of understanding. I do not have the room to develop this argument but there is tension between 'spirit' such that a universal logic connects our thought, language and understanding of the world, and 'life' in that the majority of what is meaningful and makes us human is inexpressible in logical form. It is the limitation of what is overtly expressible that possibly points to a pre-theoretical or pre-logical origin. 'The facts contribute only to setting the problem, not to its solution.' Ibid., p.88. At one point Wittgenstein gives a highly 'Heideggerian' aphorism on the role of philosophy, 'there really is a sense in which philosophy can talk about the self in a non-psychological way. What brings the self into philosophy is the fact that 'the world is my world.' The philosophical self is not the human being, not the human body, or the human soul, with which psychology deals, but rather the metaphysical subject, the limit of the world – not a part of it.' Ibid., p.70.
[157] Newton-Smith, *The Rationality of Science*, p.10; pp.102-124

for the objects that the meanings of terms are supposed to refer to, in that, either people are talking about different objects, in which case, they cannot be compared. Or the objects about which we talk change in accordance with the changes in their meaning. These ideas are only operational at the level of regional ontology, in that, the objects of a domain are different or change only as much as we are able to describe or justify them in our 'about' languages. So, for Newton, 'mass' was about a constant quantity, where as for Einstein, it was about a relative quantity. Their paradigms were about different physics, which enable us to interpret the world differently. What the strong reader does not pay attention to is the 'of' language that allows us to recognize the paradigm and thus re-evaluate it. The 'of' language comes from being in the world, where we can then start to make statements about it through this specialized way of viewing it, that I have called the methodological stance. However, the strong reader regards paradigm and world as identical. So the world we have tacit understanding of (non-propositional/ non-literal), is the same as the world our thoughts, ideas and language are about (propositional/ literal). So when terms change their meanings, the objects to which those terms refer must change also.

This particularly linguistic take on Kuhn is evident in Newton-Smith. He concerns himself with meaning change of scientific terms across paradigms. He cites Kordig's arguments against RMV, that if communication between two paradigms is impossible, then RMV would be methodologically undesirable for scientists. Newton-Smith notes that just because a thesis is methodologically undesirable does not mean it is false. He says that '[RMV] does rule out thesis of verisimilitude, and this ought to prompt us to consider an alternative approach to the meaning of terms within scientific theories which does not give rise to [RMV].'[158] Reading Kuhn through the 'linguistic turn' makes it hard to see how he was not advocating a form of RMV, yet at the level of commonsense, it seems unbelievable that two scientists, from different cultures or historical periods, cannot/did not see the same thing. This tie between language and perception is strengthened from Kuhn's reference to reading Whorf in the preface to *Structure*.[159] This, along with the meaning-holism that was being developed by Quine that was taking affect in the philosophy of science during that time period, may act as

[158] Ibid., p.158
[159] Kuhn, *Structure*, p.viii

a bridge to linguistic-cognitive relativism and social constructivism promised in the strong reading.

Incommensurability limits our ability to share a world. Here we are trapped inside our own paradigms, forms of life, or language games. As Kuhn says, 'practicing in different worlds, the two groups of scientists see different things when they look from the same point in the same direction.'[160] If we cannot compare theories or appeal to evidence or logic to discern between competing paradigms, what then can be those guiding factors? If we are not choosing between Einstein and Newton on the grounds that Einstein explains more and is more accurate than Newton, then on what grounds do we appeal? There would appear to be no rational reason for choosing one paradigm over another due to the inability to compare. Polkinghorne claims that 'Kuhn does not flinch from drawing [this] conclusion', and that he over emphases the role of social factors and personal psychology to the extent of 'proclaiming the efficacy of scientific mob rule.'[161] Those 'interest groups' that proliferate the strong reading actively encourage the irrationality of science or the intellectual weight of Kuhn's association with it. Here they use 'paradigm' and 'incommensurability' as just one more rhetorical tool with which to deny evidence-based research, in areas that have socio-political import, such as climate change studies.[162] This undermining does not just apply to the natural sciences, but also our accounts of history. Chapman says 'in the context of the culture wars […] historical revisionism is any attempt to revise historical understanding through political and ideological dishonesty.'[163] This too can be a powerful rhetorical tool if the questioning of how things 'really happened' is tied to ideological notions, such as national identity. This intellectual dishonesty can be very hard to discern from legitimate critiques of Enlightenment ideals, if one only has a methodological understanding of how science and its associated terms work. So from the relativism detected in the strong reading of Kuhn we can form a critique of

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[160] Ibid., p.150

[161] John Polkinghorne, *One World: The Interaction of Science and Theology* (London: SPCK, 1986), p.13. Kitcher argues that Kuhn's representation of science as a 'club' in which membership means everyone agreeing on a certain doctrine is a 'hopeless caricature'. Philip Kitcher, *Abusing Science: The Case Against Creationism*, (Cambridge, Mass.: MIT Press, 1982), p.168. 'Nevertheless, the caricature has become commonly accepted as a faithful representation, thereby lending support to the Creationists' claims that their views are arrogantly disregarded.' Ibid.,

[162] Patrick Michaels and Robert C. Balling Jr, *Climate of Extremes: Global Warming Science they Don't Want You to Know* (Washington, DC.: Cato Institute, 2009), p.39

'how things really happened', which for some is the work of 'moral relativism, postmodern philosophy, and a "politically correct" agenda'.[164]

7.7 - Introducing the Weak Reading

In opposition to the strong reading, which puts the methodological conception of science first, and from which we derive an account of reality, I will offer the weak reading. As the 'strong' reading seems to appeal more to commonsense, I will make more of the concepts I presented in chapter 6, to distinguish the weak reading by. Science has to view things through the methodological lens, as this is part of how it works. It has to abstract the world into 'things' in order to get to the level of normal science, which permits the acute level of inquiry needed. This is all very well for 'doing' science, but a PUoS is not about 'doing' science, but understanding and talking 'about' science. This does not require the methodological stance, but what I have called the 'historical approach'. Putting his more radical sounding conclusions to one side, Heidegger warned that if we only understand life in terms of the methodological stance we not only then begin to interpret ourselves in those terms, but we take it to be the fundamental description of what it is to be human.[165] Science then becomes the only genuine form of understanding – not only for comprehending nature but our own existence. This can be harmful in a number of respects. Where we understand science, which is a human historical practice, in terms of its abstract metaphysical cousin, Ww also create the means to demote the role of philosophy as a way of thinking about science. To give people only the tools to think about science as a methodological procedure is to limit our ability to interpret it and to think in general. To put the question 'what is science?' ahead of 'what does it mean to be scientific?' limits the role philosophy can play and also gives us a metaphysical template by which to think about ourselves. The problem then amounts to an ethical and political one, where if we can only discuss science (and by proxy philosophy) as part of a methodological view, which then dominates what we can say about terms, such as, 'real', 'truth', or 'reason', we have cut ourselves off from whole area of enquiry and

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[163] Roger Chapman, *Culture Wars: An Encyclopaedia of Issues, Viewpoints and Voices*, Vol. 1 (New York: M.E. Sharpe, Inc., 2010), p.470
[164] Ibid.,
[165] Martin Heidegger, 'The Question Concerning Technology', in *Martin Heidegger: Basic Writings*, ed. by David Farrell Krell (London: Routledge & Kegan Paul, 1978), pp.283-318 (p.285)

intellectual rigour. Thus to hold a view of reality that is not based upon the empiricist's metaphysics, when put against this dominant methodological view, ends up appearing absurd or just wrong.[166]

The philosophical notions we have dealt with so far such as truth, objectivity, and reason, are all things we would normally associate with science. What the historical perspective does is argue that these are all unquestioned assumptions we have conceptually built upon, by placing them as the basis for our methodological view. The historical approach will try and show that what the methodological view calls fundamental, is actually a regional ontology. Kuhn's description of scientific change, when understood through a strong reading, presents the problems of what we assign the term 'real' to, or what 'progress' means, and indeed, 'what' science is progressing towards? For now most scientists in their normal state can just accept that science works and avoid the deep philosophical issues, which again, is fine for doing science, but I would say completely inadequate for understanding what it means to be scientific. The important point to be made here is that a public understanding of science (and philosophy) can be easily up-ended if science as a methodological abstraction is shown to be problematic. If we think this problem is a result of the methodological interpretation, and we have no other conceptual tools for thinking about science with, we then have to rely on science as a self-sustaining definition or give up on philosophy as a method. The developing and gathering of these 'conceptual tools' is not part of the scientific process, but belongs to philosophy.

In the next chapter, I will construct the weak reading of Kuhn, again looking at the same concepts, but doing so from the historical approach. Here the concepts laid out in chapter 6 will be more explicitly applied as this is the harder version to understand, if one is already coming to it as a strong reader. What I want to show is that interpretation is not just a matter of reading but it involves a philosophical

[166] An idea that was not explicitly discussed was whether Kuhn is a realist or anti-realist. Again, what is 'real' is normally an expression of our methodological understanding. The degree to which scientists engage with 'philosophy' is also a comment on the training they receive as well as the revolutionary/normal state of their field. For example, see Bohr's conversation with Einstein about the possibility of describing quantum 'reality'. Niels Bohr, 'Discussion with Einstein on Epistemological Problems in Atomic Physics', in *Albert Einstein: Philosopher-Scientist*, ed. by P. A. Schlipp (La Salle: Open Court, 1969), pp.199-241

understanding. The weak reading is more useful for a PUoS than the strong reading, in that it protects the epistemic authority of science, and it also gives a role for philosophy as a part of public discourse in talking ‘about’ science.

Chapter Eight

The Weak Reading

8.1 - Kuhn and *Structure* – Recap of the Strong Reading

In the last chapter it was argued, that without much effort, we could derive a reading of Kuhn that has him as a relativist, subjectivist, or irrationalist about science. It was understanding Kuhn through a methodological perspective, that I have called the ‘strong’ reading, which situates epistemology in the reader’s mind as the driving force behind Kuhn’s take on science. As science is always ‘about’ something, we take this ‘about’ as undifferentiating and universal. Science is about making true or at least not false statements about the world. The strong reader understands this ‘about’ as fundamental to science. The ‘weak’ reader, on the other hand, understands this as a metaphysical belief about the organization of science. For in order to get to this stage where the ‘world’ or ‘reality’ is a universal entity that all scientists describe objectively, we have to have already engaged with a model of ‘reality’ that has knowledge, language and world hanging together in a particular way. Here our knowledge and language are always ‘about’ the world, that is sensible and intelligible to us, yet how we make knowledge or language about a particular thing is not contained within the ‘about’ language itself.

As our language is always ‘about’ something, for the weak reader, what we see or understand that ‘something’ as is a historical question. In opposition to the ‘strong’ reading I will offer its weak counterpart, the ‘historical approach’. This reading will challenge the strong reader, but if they cannot accept the starting premise, it will appear to make the strong reading all the more credible. In doing so, it will also make the weak reading seem all the more unnecessary. That is, if we understand an essentially historical question or critique as being a methodological substitute, it produces highly counter-intuitive and nonsensical answers, which can then be projected back at the practice that created them, that is to say, philosophy. This produces such questions as how a second-order activity, like philosophy, could know more about the ‘world’ than a first-order activity, like science? If a philosopher or cultural theorist is understood as doing the same job as a scientist or their claims are taken to be achieving the same sorts of thing, it is this issue that defenders of science have rallied against. Bricmont and Sokal highlight what they believe to be such claims from the humanities, where ‘the natural world has a small or non-existent role in the construction of scientific knowledge.’[1] If we take such claims as read, what value is a subject that can suggest that, ‘what there is’ plays little or no role in science? Bricmont and Sokal state outright that their problem with such claims is one of epistemology and methodology, which is vindication for the strong reader that this is where the problems lie.[2] Whether arguments in philosophy of science or sociology of scientific knowledge are meant as a means to more reliable knowledge than science, is why we should study such claims in depth and not accept received readings. Part of this confusion, I argue, comes from an understanding that is already based in terms and phenomena that are already interpreted for us. We come to a ‘text’ with pre-critical ideas about how the world is, but those ideas and ways of understanding come to us already made meaningful by activities that pre-exist us. This pre-critical understanding is bound up in benign terms like ‘commonsense’, ‘reality’, or ‘literal’. Norris on Derrida makes a similar point. Concepts like those mentioned ‘will always have been ‘worked’ or elaborated in advance by the discourse of philosophic reason.’[3]

It is the weak reading that will help problematize a ‘received view’ of Kuhn, and with it, expose the basis for producing such a view by inverting the elements of the ‘strong’ reading.

8.2 - The Weak Reading

The strong reader starts with regional ontology. Science is always already ‘about’ things. As paradigms tells us what the world is like, and knowledge/ language are the starting points for the strong reader, these then share an identity with the world. For if our theories, experiments, evidence, and observations are always ‘about’ the world,

[1] Bricmont, Jean and Alan Sokal, ‘Science and Sociology of Science: Beyond War and Peace’, in *The One Culture? A Conversation about Science*, ed. by Jay Labinger and Harry Collins (Chicago: University of Chicago Press, 2001), pp.27-47 (p.28)
p.28. They quote passages from Harry Collins, Bruno Latour, Barry Barnes, David Bloor and Stanley Aronowitz.
[2] Ibid.,
[3] Christopher Norris, *Deconstruction: Theory and Practice,* Revised edition (London & New York: Routledge, 1991), p.142

and the paradigm dictates how we do this, they are then also 'about' the paradigm. If all we have is an 'about' language, in explaining the kind of problems that traditional philosophy of science throws up, we are left with only a few ways to interpret such problems.

The weak reader can also begin with regional ontology or what our science is 'about', but they do so on the understanding that how we come to situate something as a proper scientific object is historical. For Newtonian theories to be *about* absolute space-time or constant mass, the 'world' had to allow such possibilities to exist for Newtonians. For the problems and issues of Newtonian physics to be experienced as a 'problem', one has to be involved with the language 'of' Newtonain physics. This applies to how science was conducted and permitted those 'abouts' of Newtonian physics to be thought of as meaningful. This is to bring in extra-scientific factors such as theology, Ancient Greek metaphysics and so on. For the weak reading to be conducted something has to take place before regional ontology, before we can abstract and our theories and language as about something. The weak reader understands the conditions for making the methodological view as the basis for producing a historical account. This is achieved by placing, what I have called an 'about' language, as either equivalent or prior to what I have called an 'of' language. An 'of' language is pre-theoretical and pre/ non-propositional. Any way of examining or describing something is necessarily 'about' something. Yet, the conditions for such a state of certainty to be achieved, one has to already be engaged in practices, made meaningful by the world, that do not originate in those discursive practices. For example, the rules of football are *about* football. The playing *of* football is football. The activity has to precede what our thoughts and language will be about. It is starting with the 'about' language, however, where most of philosophy and commonsense thinking finds itself. From which we try and recapture this 'of' non-discursive element. A way forward from here is that because our 'about' and 'of' languages cannot be the same, it is the role of philosophy to problematize our models of reality or language where these distinctions have been conflated.

However, in order to write about an 'of' language I have to take up an 'about' language. That is to take up a methodological stance to articulate something that is non-methodological. Just because I have to go via an 'about' language to articulate what an ‘of’ language is, does not make the ‘about’ language more fundamental or important.[4] It is this same problem that I think occurs in talking about and understanding what science is. For any method for disclosing what there is is already dominated by the methodological interpretation of science. I have argued that this is what we see with the problem of demarcation (PoD). One cannot give a methodological account of what makes something a science. Yet, because an ‘of’ language is pre-theoretical and can only be expressed through an ‘about’ language, it is very hard to not re-interpret this in terms of other ‘about’ languages. So where the weak reader will want to say, that to see the world as ‘geo-centric’ is a statement about belonging to a world that allowed this way of seeing it to be meaningful, the strong reader still regards this as expressing something about psychology or language. As Dalston says, for example, ‘in the end even scientific ‘ways-of-seeing’ are ultimately, consciously or unconsciously, psychological’.[5]

Whilst it might not be obvious that *Structure* can be understood as an argument for the primacy of ‘of’ languages, it is from taking ‘about’ languages or regional ontology as our starting point that the more pernicious readings can be sustained.[6] By exposing the metaphysical basis of something like the ‘strong’ reading, we cannot only defend science from those ‘interest groups’ who may wish to hijack a certain reading of Kuhn, but also find a role for philosophy in recognizing ‘sloppy thinking’ and not promoting it. In looking to undermine or defend science, texts ‘lend themselves to strategies of reading whose intent is always part of a struggle for interpretative power.’[7] Whoever can lay claim to the authentic meaning of Kuhn gets to say what he means. So when Kuhn writes, ‘though the world does not change with the change of a paradigm, the scientist afterward works in a different world. Nevertheless, I am convinced that we must learn to make sense of statements that at least resemble these’, we have to ask ‘how’ and ‘who’ is performing this interpretative service so we can already understand Kuhn as a relativist or not.[8] What makes the weak reading

[4] Bhaskar talks about ideologies ‘for’ and ‘about’ science which serve a similar function. Roy Bhaskar, *Reclaiming Reality: A Critical Introduction to Contemporary Philosophy* (Oxford: Routledge, 2011), p.22

[5] Lorraine J. Dalston, ‘On Scientific Observations’, *Isis*, 99 (2008), 99-110 (p.110)

[6] Kuhn only mentions ‘ontology’ a couple of times. Thomas Kuhn, *The Structure of Scientific Revolutions*, 3rd edn. (Chicago: The University of Chicago Press, 1996), p.40 & p.184

[7] Christopher Norris, *Deconstruction: Theory and Practice* (London & New York: Routledge, 1991), p.88

[8] Kuhn, *Structure*, p.121

conducive to PUoS is that it understands the activities of scientists as historical and what it means to be scientific as a historical act.[9] This can be directly related to the weak reading, in that, one could potentially trace the available possible interpretations of a philosopher by the surrounding historical debates. So, in giving the public another means to interpret science and opening up its possible meanings, they are not conceptually confined to only a methodological understanding. So when a project like demarcation is shown to be faulty by 'alternative' practices, the public have recourse to explain such failures, and not be led to the conclusion that there are no differences between science and non-science as epistemic enterprises.

This approach from the historical angle is not new. Friedman has argued that science itself is tied to the historical context within which the surrounding debates of science contribute to those paradigmatic shifts.[10] The PUoS and the historical approach, however, are both parts of thinking *about* science and not doing it. It is in thinking about science that we might find there is more than just one approach and indeed it might reveal why the methodological perspective is so dominant in the discourses on science. What-is-more, if we only identify 'philosophical' thinking with the ability to produce an abstract methodological account of science, we start to lose some of the more critical aspects of philosophy as a practice. In my attempt to give a polarized account of Kuhn, it may help to see where already existing attempts to deconstruct Kuhn have drawn their lines. Fuller, for example, identifies 'relativistic' and 'universalist' readings of *Structure*.[11] My point, however, is not to produce just another reading of Kuhn as a part of Kuhn scholarship, but that what is detectable in Kuhn is symptomatic of a tendency in the philosophy of science, its dialogue with the natural sciences and its implications for PUoS. That if we read Kuhn or certain critiques of science as *only* methodological suggestions, we begin to confuse how

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[9] Kuhn does not explicitly state hermeneutics as a method in *Structure* but later acknowledges its presence. Thomas Kuhn, *The Essential Tension: Selected Studies in a Scientific Tradition and Change* (Chicago: Chicago University Press, 1977), p.xiii. This focus on the historical Hacking calls the historical a priori. Ian Hacking, *Historical Ontology* (Cambridge, MA: Harvard University Press, 2002), p.5

[10] Michael Friedman, *The Dynamics of Reason* (Stanford: CSLI Publications, 2001); 'Einstein, Kant and the Relativised A Priori', in *Constituting Objectivity: Transcendental perspectives on Modern Physics*, ed. by Michel Bitbol, Pierre Kersberg, Jean Petitot (Dordrecht: Springer, 2009), pp.253-268 (p.254)

[11] Steve Fuller, 'Being there with Thomas Kuhn: A Parable for Postmodern Times', *History & Theory*, 31(3) (1992), 241 – 275 (p.273). Fuller claims that neither 'relativism' nor 'universalism' takes up the central aim of *Structure* which could be termed a 'didactic macrohistory of science'. Ibid., p.273. That

philosophy relates to science and we end up with the relativism some readers accuse Kuhn of. The weak reader's goal is to uncover the source of this confusion, and by doing so, removing the metaphysical basis for such interpretations, be they are positive or negative. This at once dissolves any parasitism a pseudo-science might have in relation to genuine science, or reveal the inherent problems within something like the PoD, and show the value of philosophy in the process of doing so.

The following reading will also try to problematize the 'literal/ metaphor' order in Kuhn. As an indication as to how philosophy may help us make sense of misplaced metaphor, or show us how we are committed to a metaphysical notion, I will briefly examine the notion of public 'literacy'. For up until now I have been re-using the metaphor of 'literacy' for PUoS and philosophy. Yet it would seem problematic. To be literate, either grammatically or technologically, is to suggest one can do something proficiently by way of understanding rules and how those rules are applied. This metaphor has its source in the methodological way of thinking. Rather, 'literacy' or 'rule application' only means something because we are involved in the activities of communication.[12] As Wittgenstein shows in his 'rule following paradox' there is no 'rule' for rule application. For whatever the rule is, any action can be brought into accord or conflict with it at the same time.[13] A rule is what one does with it.[14] Not to delve too deep, there is an internal contradiction to the metaphor if we consider 'literacy' in its methodological guise. To be literate is to know how to apply the rules of written and spoken grammar. Once we are proficient at this the individual can continue to read and write in new contexts by applying those same rules, and we would say they are literate. To be literate is to be literate in a language whose main function is to be understood or communicate. By knowing the 'rules' we have a standardized way of achieving this. Yet, in English there are no 'hard' rules of grammar. There are general rules, but in each instance there will be an exception when they are broken. There is no rule for knowing when a rule is to be broken in

is, a drawing of general lessons from the unfolding of our collective cognitive development, that we can prepare for a future which will be unlike the present by looking at episodes from the past.

[12] Ginev uses the term 'phronesis-rationality'. Dimitri Ginev, 'Re-Reading Normal Science', *Crítica,* 35 (2003), 65-89 (p.78)

[13] Ludwig Wittgenstein, *Philosophical Investigations,* trans. by G. E. M. Anscombe, 2nd edn (Oxford: Basil Blackwell, 1986), p.81 (para. 199)

[14] Ibid., para. 201

order to make sense.[15] Part of language is that people will appropriate words, change spellings and give different pronunciations, but this is all part of living language. Yet there is no methodological imperative to say when we should break with tradition or the conventional rules in order for communication to occur. We say illiteracy is the ignorance of rule application or the illegitimate breaking of rules, but if the point of language is to communicate, we may need to break rules in order to be understood. Yet, if we take language to be part of some Austinian 'speech-act' performance, whatever obstructs communication is deviant to language.[16] 'Literacy' is the learning of these communicative rules in performative speech acts yet, it also appears to be in antagonism with making oneself understood, which 'literacy' is supposed to aid.

8.3 - Framing the Weak Reading

The weak reading suggests that if *Structure* is understood as a theory of methodology, with its roots in either epistemology or linguistics, it produces either banal truisms or radical conclusions. Questions that are integral to the PUoS, such as 'what is science?', if taken as methodological question, end up either not being able to definitely say what science is (PoD) or we blame the tools by which the question came about (philosophy). Rather, the weak reading says that to start from a point of methodology is already to begin from a contrived position. To start from questions of epistemology or linguistics and how they make contact with the 'world', or to presuppose a commonsense realism which allows objects to be abstracted into 'about' languages, is for the weak reader, the end of a process not the beginning of inquiry. Admittedly, this is where scientific *thinking* begins but historically and philosophically we have had to come a long way to make this abstract situation appear routine.[17] For it is science that establishes the 'about' in most investigative empirical questions, but when the 'about' is directed back on to science we find ourselves in an unhelpful loop where it then begins to deconstruct itself. Here philosophy is seen as doubly useless, as it not only has problematized what seems like a sensible and important question, but it allows for counter-intuitive responses to be taken seriously.

[15] There are two ways of 'making sense' here. Through the 'about' language, which will be to 'make sense', according to the rules of grammar. Then there is 'making sense' through the language of communication, which may well be meaningless according to grammatical rules.
[16] Norris, *Deconstruction: Theory and Practice*, p.112
[17] 'Thinking' may start here but scientific practice is always on going.

Terms such as 'true' or 'reality' are overlooked as not being methodologically necessary in science but act more as heuristics. Weinberg says, the reality of the laws of nature are as 'real' as rocks to him. He says this is 'not a philosophical argument, but rather a personal report, that my experience of the laws of nature in my work as a physicist has the same qualities that in the case of rocks make me say that rocks are real.'[18] What follows from this, for Weinberg, is that philosophers can help clarify terms such as 'real' and 'true', but 'they have no business telling us [scientists] not to use them.'[19] The weak reader would object to this on the grounds that it does not matter what we call something, be it 'real' or 'true', but it is the fact that we already know what these terms mean and apply them liberally that should puzzle us.[20] To start with a debate about the use of a term, as Weinberg argues, is to begin at the end of a long process of inculcated philosophical thought. Part of what I will argue under the weak reading is that the methodological understanding of terms like 'true' or 'real' is the naturalized use of metaphor. Where we have confused a metaphysical abstraction for something more concrete.

The weak reading starts with people and what they do. This means to be alive in an active culture and society. What will come to the fore in the weak reading is how, unlike the strong reader who starts with what our language/knowledge is 'about', they will begin with the 'of' language. An 'of' language is how we are in the world, it is our practices, and it is how we are involved with things. Without an 'of' language no 'about' languages can be formed. Something like the PoD tries to reverse this by stating what science *is* in terms of what science is *about*. For science is about falsification, but it also about a number of other things that can be brought into conflict with any singular principle. Science, rather, is what scientists do, which frustratingly for philosophers of science, is not governed by a logo-centric idea of what science is. So in looking at what people have historically done 'we are not studying an object, but *a process*'.[21] This is where the weak reader finds themselves at

[18] Steve Weinberg, 'Peace at Last?', in *The One Culture? A Conversation about Science*, ed. by Jay Labinger and Harry Collins (Chicago: University of Chicago Press, 2001), pp.238-242 (p.240)
[19] Ibid.,
[20] Heelan offers the term 'hermeneutic realism' that means science constructs knowledge of the world through a series of interpretative technologies, such as where a needle on meter is and what that means. Patrick Heelan, 'Natural Science as a Hermeneutic of Instrumentation', *Philosophy of Science*, 50 (1983a), 181-204; 'Perception as a Hermeneutical Act', *Review of Metaphysics*, 37 (1983b), 61-76
[21] Michael Gelven, *A Commentary on Heidegger's Being and Time*, revised edition (Illinois: Northern Illinois University Press, 1989), p. 24 [My italics]

the beginning of *Structure*, in the difference between the traditional methodological conceptions of science and how science transforms as a historical process. It is only when the reality of objects postulated in theories, or the veracity of the theories themselves are highlighted as problematic (beyond puzzle-solving) through crisis, that the paradigm of normal science is revealed to be grounded in something other than its methodological principles. Feynman suggested a type of wisdom to know when to suspend any commitment to past-accepted explanations.[22] In crisis, the regional ontology of normal science and its constituting paradigm become 'visible'. The paradigm moves from its 'of' language in normal science to now being about the paradigm itself, through the new language 'of' revolutionary science. So the language of the Newtonian paradigm is 'about' Newtonian physics and objects. The language 'of' the Newtonian paradigm is to practice physics in such a way that helps us generate the theories and objects it is about.[23] This way of doing physics was highly successful, but eventually enough anomalies built up with what Newtonian physics was 'about' (i.e., faster than light speeds, infinite space-time, constant mass, and so on) that how physics was done began to change in order to resolve these problems. In order to do this, rather than continue within the 'of' language of Newtonian physics, we have to turn the search lights back onto the paradigm to see where we are potentially going wrong. We now have a new way of doing physics. Famously it was the Einsteinian paradigm that replaced the Newtonian one, and so it was from the language 'of' Einsteinian physics (i.e., doing physics in such a way that did not require those problematic Newtonian assumptions) that the 'new physics' is now about something different. Scientists, thus, take up a new perspective of re-constituting the paradigm as an object of inquiry, where we can now attend to the shortcomings of whatever it is we are looking to replace. We learn from history that when paradigms are in a revolutionary or unstable state, knowledge is very difficult to produce. Yet, this difficulty hardly ever results in the raising of questions over fundamental as opposed to regional ontology. As science is a derived activity, scientists can 'attend carefully to entities without having to inquire into their being',

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[22] Richard Feynman, What is Science?', in *The Pleasure of Finding Things Out: The Best Short Works of Richard P. Feynman*, ed. by Jeffery Robbins (London: Penguin Books, 2001), pp.171-188 (p.188)
[23] One must be careful not to undertstand such statements as a methdological claim, in that, the Newtonian paradigm generates 'reality'.

which is the preferred state for knowledge production.[24] For it is precisely this questioning of being that is prominent in the revolutionary states of science.

Just as ideas may already be interpreted for us, a Kuhnian term like 'paradigm' is now fairly common. It is used within fields such as business management, but they still seem to carry a particular understanding of what it is.[25] Kuhn, does not write in the language of 'about' and 'of' languages, but terms like 'paradigm', 'normal science', 'revolutionary science', 'crisis', and 'incommensurability', are all ways of doing science.[26] They all reference an 'of' language from what they are 'about'. The weak reader understands the nominal notion of 'science' as 'textbook science', which we might identify with 'normal science' as made possible through a paradigm. This science when investigated as a 'thing' can be typically broken down into methodological principles. Likewise, when we view 'revolutionary science', we view it from our current position, which more often than not, is during the 'peace time' of normal science. All of the current successful languages of science are 'about' something and how we get to that 'something' is re-told as part of the methodological story of science. Here our current paradigm partly instructs how that story goes, but this story is not binding. In order to progress and uncover the problems with our current paradigm, such as the incompatibility of relativistic and quantum physics, we need a new paradigm from which we can view the errors of our current ways of doing science. We need to turn it into an 'about', which requires some as yet unpracticed 'of' language. I take this to be the problem that Stanford wishes to answer with his 'unconceived alternatives'.[27]

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[24] Rouse, 'Heidegger on Science and Naturalism', p.6

[25] Jay Kandampully, 'Paradigm Shifters in Retailing', in *Service Management: The New Paradigm in Retailing*, ed. by Jay Kandampully (New York: Springer, 2012), pp.285-306 – Even though is a completely borrowed phrase it makes no reference to its origin or original meaning. Alan Price attributes the term 'paradigm' to Kuhn and defines it as 'a framework of thinking – consisting of exciting, modern ideas which would replace the stale and ineffective prescriptions of personnel management.' Alan Price, *Human Resource Management in a Business Context*, 3rd edn. (London: Thomson Learning, 2007), p.41

[26] Kuhn, *Structure*, pp.174 – 182. Dreyfus identifies Heidegger's term '*vorhabe*' with Kuhn's term 'disciplinary matrix'. '*Vorhabe*' is the totality of cultural practices that make us who we are such as the skills to become a scientist and thus be able to judge what are the relevant facts and experiences. Hubert Dreyfus, 'Holism and Hermeneutics', *The Review of Metaphysics*, 34 (1980), 3 – 23 (p.10). Dreyfus acknowledges in the footnotes that this is a 'forced reading' of *Being and Time* but 'is true to what Heidegger is trying to get at'. Ibid., [In footnote]

[27] Kyle Stanford, *Exceeding Our Grasp: Science, History and the Problem of Unconceived Alternatives* (Oxford: Oxford University Press, 2006)

If we think of the periods of 'normal' and 'revolutionary' science rather not as distinct methodological entities, but as comments on what it means to be acting scientifically at a given time, this will help reveal the depth available in Kuhn. Even when scientists engage with the humanities in their studies of science their conclusions and understanding are still comments on what it means to be scientific. This is the case, even when for the most part, their concerns do not extend past the epistemological or methodological.[28]

8.4 - A General Structure of *Structure*

Language and knowledge for Kuhn are essentially historical. Wherever one takes as their starting point in time concepts, ideas and values are presupposed. The importance of history for Kuhn is shown in the 'Introduction' and first chapter, where he outlines 'A Role for History'.[29] The periods of history Kuhn wants to take as his exemplars for scientific change are the 'Copernican Revolution', the development of the theories of oxygen and electricity, the instrumental application of x-rays, and the superseding of Newtonian by Einsteinian physics. In looking at the historical activities of scientists, Kuhn is attempting a re-evaluation of the historical development of science. By spelling out the different stages of scientific revolution and what is necessary for scientific crisis, the weak reader understands that science is more than just its methodology. The strong reader wants to account for this 'more than' by methodological means, be it through cognitive science or the structures of theories. The weak reader wants to understand this as a problem of how those 'about' languages are grounded in an 'of' language. So what precedes the establishment of knowledge are activities that allow for ways of talking and doing science to be meaningful. Kuhn, however, never explicitly states the resolution to his problem, only a number of ways it may be tackled.

For the weak reader 'paradigms' are not things, but are what structure our experiences or what might be a system of relations. They are discursive attempts at describing the

[28] David Mermin reflects on his exchanges with sociologists of science and in his essay 'Conversing Seriously with Sociologists' suggests three rules so misunderstandings between disciplines are limited. David N. Mermin, 'Conversing Seriously with Sociologists', in *The One Culture? A Conversation about Science,* ed. by Jay Labinger and Harry Collins (Chicago: University of Chicago Press, 2001), pp.83-98 (pp.97-98)

'literal' world, but they are only possible due to being grounded in the 'non-literal' world with which we are tacitly engaged. Paradigms tell scientists what their activities are 'about', but it is the strong reader who then collapses this distinction on to the world, as if the paradigm and world were the same thing.

Paradigms can be thought of as the meanings and interpretations that are possible or available to us in interpreting and giving an account of the 'literal' world. What is crucial to the weak reading is that paradigms are not the world, they are what enable us to see something 'as' something.[30] So both Aristotle and Newton 'saw' falling objects, but they saw them 'as' different phenomena due to their paradigms, which are intrinsically connected to the time in which they lived. In periods of normal science paradigms can stand in for the world, as they are so good at telling us what 'reality' is like or what to expect. However, because they are not the world and it is in times of crisis that this becomes apparent, research can still be conducted in the absence of a ruling paradigm.[31] As we are first and foremost always already in and part of the 'non-literal' world, we have a tacit understanding of it that guides our practices in trying to express it in a 'literal' sense. Paradigms legitimize certain ways of acting or ideas as possible, which is what regulates the methodological content. So only certain things can count as evidence, observation, theory, justification, explanation and so on, but what the history of science shows us is that these do change. What we must note is that just because a paradigm provides the possible interpretations for a phenomenon that does not mean 'any' interpretation is possible or equally likely. Strictly speaking, I do not experience the world 'as' rotating, but that does not mean I can live my life as a 21st century geo-centrist. Of course I am free to believe the earth is not rotating, but that belief is in no way meaningful for the time I happen to be living in. I cannot override the possibilities given to me by my culture and history.[32] It is this that worries the opponents of relativism, that if truth is not grounded in simple one-to-one

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29 Kuhn, *Structure*, pp.1-9

30 Devitt and Sterelny express this as a problem for the constructivists in that they blur the distinction between theories of the world and the world itself. Devitt Michael and Kim Sterelny, *Language and Reality: An Introduction to the Philosophy of Language* (Oxford: Blackwell Publishers, 1999), p.253

31 Ibid., p.44

32 This accusation of open relativism is present in Sartre's reading of Heidegger, which focused on the subjective. For Sartre there was no essential 'human nature'. It was only real as far as people acted and man's freedom to act was rooted in subjectivity, in the 'I think' being the only possible basis for ethics. Having no essential human nature and letting subjectivity guide being opens the door to endless self-

correspondence what is to stop the astrologer's 'truth' being equivalent to the astronomer's 'truth'? What is to stop infinite interpretations of texts or continual underdetermination by evidence? This observation also applies to the strong-weak reading, that any particular interpretation of Kuhn is itself a product of history, which is why I argue for the presence of philosophy as a type of constructive self-undermining practice.[33]

Kuhn sees the historical dimension to the development of thought as crucial for understanding how thought changes. He suggests that the popular 'textbook' picture of science is 'the piecemeal process by which these items have been added, singly and in combination, to the ever growing stockpile that constitutes scientific technique and knowledge'.[34] And that this view is only tenable on the grounds that one is unaware of how science develops. If we regard Kuhn as presenting a theory for scientific change, in the sense that he is providing some overarching philosophy of science so that we might investigate how science is 'really' done, is to concentrate on the normative, prescriptive aspects of Kuhn's writing. If this becomes what is important to Kuhn he then suffers from a need to explain why scientific revolutions and paradigms change, rather than just show that they do.[35] For example, he determines there to be three foci for normal factual scientific investigation so that, there are in principle three types of phenomena about which a new theory maybe developed or how all crisis will close in one of three ways, all of which seems arbitrary.[36] Rather, if we read Kuhn as trying to undermine the traditional, normative, grand philosophical approach to science by taking the pre-theoretical aspects more seriously, such as, paradigms as meaning fixing, tacit understanding, or the ontological character of his general description, the normative interpretation seems less problematic. This pre-theoretical element is part of what I have called an 'of' language.

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interpretation. Martin Heidegger, 'Letter on Humanism', in *Martin Heidegger: Basic Writings*, ed. by David Farrell Krell (London: Routledge & Kegan Paul, 1978), pp.189-242 (pp.190-191)

[33] Golinski says in regard to Fuller's 'Cold War' interpretation of Kuhn, 'I see aspects of the ideological context of the times as having shaped the readings of Kuhn's work in sometimes contradictory and paradoxical ways, and often against his own inclination.' Jan Golinski, 'Thomas Kuhn and Interdisciplinary Conversation: Why Historians and Philosophers of Science Stopped Talking to One Another', in *Integrating History and Philosophy of Science: Problems and Aspects*, ed. by Seymour Mauskopf and Tad Schmaltz (New York: Springer, 2012), pp.13-28 (p.14)

[34] Kuhn, *Structure*, pp.1-2

[35] Ibid., p.207

8.5 - Paradigm

Kuhn notes in the 'postscript' to *Structure* that 'several of the key difficulties of my original text cluster about the concept of paradigm'. He then tries to give a separation of paradigm from scientific community.[37] He suggests that this may be done by thinking of paradigms in two ways, that 'on the one hand, it stands for the entire constellation of beliefs, values, techniques, and so on shared by the members of a given community', Kuhn identifies this with the sociological interpretation.[38] On the other hand, paradigm can be thought of as 'one sort of element in that constellation, the concrete puzzle-solutions which, employed as models or examples, can replace explicit rules as a basis for the solution of the remaining puzzles of normal science.'[39] With these two distinctions Kuhn tries to pin point the location for much of the early confusion over his text.[40] Definition one is paradigm as a definite set of objects, beliefs, techniques and values in which people share. These are open to inspection via historical, sociological or psychological study. By 'scrutinizing the behavior of a given community's members' one may be able to identify the paradigm at work.[41] This could be viewed as paradigm writ large. Kuhn gives definition two as 'exemplary past achievements' or 'shared example'.[42] This can be viewed as paradigm writ small. Kuhn, then says, 'philosophically, at least, this second sense of 'paradigm' is the deeper of the two'.[43] Pulling Kuhn's two definitions apart, the first definition can be taken as just that, where it is the 'entire constellation of beliefs, values, techniques, and so on shared by the members of a given community'.[44] By looking at what people do, say and believe, we can study the paradigm under which they practiced. Unfortunately for Kuhn, in giving his second definition he says by focusing on 'one sort of element in that constellation' namely, 'exemplary past

[36] Ibid., p.25; p.97; p.84
[37] Ibid., p.174
[38] Ibid., p.175 – The role of the social study of science in *Structure* seems to be that if one can describe the implicit normative structure of science one is better placed to know how to govern it. 'Reflections on my Critics', in *Criticisms and Growth of Knowledge*, ed. by Imre Lakatos and Alan Musgrave (Cambridge: Cambridge University Press, 1999), pp.231-278 (p.237)
[39] Kuhn, *Structure*, p.175
[40] Ginev argued that in Kuhn's attempt to clarify the notion of paradigm and normal science by re-articulating it as a disciplinary matrix then led to a tendency to think of paradigm and normal science in epistemological terms. Rather we should think about paradigm as 'science-as-practice' rather than 'science-as-knowledge'. Ginev, 'Re-reading Normal Science', p.67
[41] Kuhn, *Structure*, p.176
[42] Ibid., p.175; p.187
[43] Ibid., p.175

achievements' 'exemplars', or 'shared examples' as replacing the rules for puzzle-solutions, he then wants to redefine paradigm as a 'disciplinary matrix.'[45] This has the effect of throwing attention back onto paradigm as understood through the first definition. Where if we look at what scientists do, say and believe we can work out what the structure of the paradigm is.[46] The weak reader will realize that paradigms as 'exemplar' is the way out of our circular definition of paradigm. Where a paradigm is what people practice and what people practice is the paradigm.[47] What we have to reflect on is why a specific period of scientific activity is significant. Paradigms do not command full consent, but they provide enough structure that something like normal science can occur.[48] Yet, there is enough room for debate, doubt and challenge that means the paradigm is never fully secure. Zammito says that because of a paradigm's tie to normal science, something only becomes paradigmatic if it allows 'ongoing, and fruitful practice; hence, it is a *historical* concept'.[49] When scientists receive their training it will always be through a paradigm, otherwise there would be nothing to train for. The paradigm will have already picked out what is significant for the scientist to study and how it should be done. The empirical content of theory and explanation are given meaning by the paradigm.[50] In this respect the paradigm tells the scientist what the (literal) world and their science is 'about'. Yet whilst this 'about' language would make it seem that we literally experience the world, we always experience it *as* something. In the sciences this is normally as 'nature'. The world in the 'non-literal' sense (system of relations – 'of' language) is what allows us to entertain notions such as the 'literal' world in the abstract. Any paradigm is rooted in this sense of the world through our 'of' languages, which we may call the practices of scientists.

Science represented as a combination of Baconian or Popperian principles is a caricature of what science is, by eliding over what it is that scientists actually do. The

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[44] Ibid.,
[45] Ibid., p.175, p. 182, p.187
[46] Kuhn's original use of paradigm exceeds what he tries to achieve with 'disciplinary matrix' and in realizing its inadequacy stops using it after 1969. Paul Hoyningen-Huene, *Reconstructing Scientific Revolutions* (Chicago: University of Chicago Press, 1993), p.143
[47] Ibid., p.176 – Kuhn notes the circularity of the definition but also finds it unproblematic.
[48] Ibid., p.186
[49] John H. Zammito, *A Nice Derangements of Epistemes: Post-Positivism in the Study of Science from Quine to Latour* (Chicago: University of Chicago Press, 2004), p.56 [Italics in original]
[50] Kuhn, *Structure*, p.188

standard strong interpretation says we have to learn to disregard our inclination to make sense of the world, to overlook the world as it appears to us. Science has done such a good job at isolating properties and abstracting the world into equations, laws and theories, we now take what is ‘everyday’ to us, such as meaning or values, to not be part of the ‘real’ world but ‘literally’ in our heads. The world as described by science is made up of matter in various stages of motion and entropy. Again this interpretation would be a problem if it were the only available one. What the strong reader takes to be fundamental, such as the paradigm of elementary particle physics, is actually a regional ontology, an ‘about’ language that tells us what the aggregate parts of atoms are about. This, however, is rooted in the ‘of’ language of those people that practice science. It is the strong reader who sees the ‘about’ language (science as knowledge) as being more fundamental to reality than our ‘of’ language (science as process). In placing the ‘about’ language as being more fundamental than the ‘of’ language, we mistake a metaphysical conception of science for a lived activity. This is akin to Heidegger’s complaint of misplacing regional ontology as fundamental ontology.[51] Whilst this is not so much a problem for scientists, as they will have a tacit understanding of what science is, it does however, severely limit a public understanding of science for non-scientists, as they will only be presented with its methodological interpretation.

As noted above I am using the term ‘world’ in two senses, ‘literal’ and ‘non-literal’. Just to clarify his distinction. We can all agree that we share the same planet (literal), but the ‘non-literal’ world of an Amazonian tribal chief is different to the world of a Hong-Kong business executive. If we understand this distinction as both pertaining to the ‘literal’ world, such that Hong-Kong and the Amazon are different geographical places, containing two different peoples, this observation seems trivial. Going the other way, if we understand ‘world change’, not as the change in the ‘non-literal’ world, where new meanings and possibilities for interpretation and ways of practicing science open up. Rather, ‘world change’ here is understood as the morphing of the ‘literal’ world, where we literally see different objects. From this we get the kind of strong incommensurability that troubles the opponents of relativism. Strong incommensurability is to confuse the first situation for the second. The difference

[51] Martin Heidegger, *The Basic Problems of Phenomenology*, trans. by Albert Hofstadter, revised

between these two types of 'world change' can be found in Kuhn, when he says 'though the world does not change with the change of a paradigm, the scientist afterward works in a different world. Nevertheless, I am convinced that we must learn to make sense of statements that at least resemble these.'[52] The strong and the weak reading both make sense of this statement, but it is the weak reading that I will argue is more the useful. For science to 'work', which for me means to bring about a state of normal science, the paradigm has to remain hidden or invisible. It is only when the paradigm goes into crisis do we see that our core concepts as being contingent and our assumptions as opaque. What the paradigm tells us should be methodologically fundamental to explanation (quantum or relativistic reality) is actually exposed as deficient for such a task. When Kuhn says 'none of these crisis-promoting subjects has yet produced a viable alternate to the traditional epistemological paradigm' the strong reader understands this as inevitable.[53] There can be no viable alternatives, for what would we ground that knowledge on? The weak reader understands that to achieve the methodological abstract version of science, we first have to understand science in its 'of' form.

We do not normally deal in 'knowledge', but as this is what science does the scientist has to take up different modes of relating to the world. Methodologically it is useful to have science only as an 'about' language. Here we break science down into 'things', such as, testing, falsification, observation, prediction and so on. Yet, historically we find that this is not all there is to what scientists do and in those episodes of paradigmatic crisis our methodological notion of 'knowledge' becomes challenged. Here we then start to scrutinize what our concepts and terms are actually 'about'? Are they real? Are they true as long as they are useful? This is where the average philosopher of science joins the debate. Yet despite the recurrent crises that science undergoes this is not enough for scientists to challenge the epistemological paradigm through which knowledge discovery and justification are framed. The strong reader wants to maintain this separation where as the weak reader will want to say such a division is a product of the methodological understanding. So, just because a historical reading of methodology problematizes 'discovery' and 'justification', is not

edition (Bloomington: Indiana University Press, 1982), p.165

[52] Kuhn, *Structure*, p.121

[53] Ibid.,

to say, 'discovery' and 'justification' do not occur. Shimony, however, conflates this historical rendering of methodology for methodology itself when he says Kuhn denies science the possibility for 'ultimate justification' in its practices or results.[54]

A paradigm is a particularly potent interpretation of 'reality'. Its potency, paradoxically, is felt by how absent it is. Paradigms, like being for Heidegger, are at their most effective when they are invisible. Here we look straight through the paradigm and just see objective 'reality'. We do not question 'reality'. We just get on with things. The language 'of' the paradigm helps it remain invisible and so we only see what our theories and observations are 'about', yet we know that these can and do change. It is this sense of scepticism in how knowledge is challenged that scientists retain the term 'theory' for all scientific statements. For example, whilst the effect of gravity is very real, what gravity 'is' is still unknown and so we only have theories about gravity. Gravity used to be 'about' mechanical forces, waves, vortices, but is now 'about' grooves in space-time, or possibly tightly bundled strings.[55] Feynman states the relationship of the epistemic fecundity ('about') of a subject to its practices ('of') in a letter about a quantum gravity conference, where he wrote, 'I am learning nothing. Because there are no experiments, this field is not an active one, so few of the best men are doing work in it'.[56] It is this idea that where a field becomes inactive because people do not know how to proceed, is for the weak reader, the difference between a science operating under a paradigm and one without. Unlike the strong reader, the weak reader understands that a paradigm is an articulation of the world not the world itself. Thus where our 'about' languages change in relation to how science is conducted Kuhn expresses this ability of the scientist to strike a balance between tradition and innovation in science or what he calls an 'essential tension'.[57] Feynman expresses this as the ability to,

[54] Abner Shimony, 'Comments on Two Epistemological Theses of Thomas Kuhn', in *Essays in Memory of Imre Lakatos,* ed. by R.S. Cohen, P. K. Feyerabend & M. W. Wartofsky (Dordrecht: Reidel, 1976), pp.569-588 (p. 570)
[55] Lee Smolin, *The Trouble With Physics: The Rise of String Theory and the Fall of Science and What Comes Next* (New York: First Mariner Books, 2007), p.106
[56] Richard Feynman, *What Do You Care What People Think?: Further Adventures of a Curious Character* (London: Penguin Books, 2007), p.91
[57] Kuhn, *Structure*, p.79

> [p]ass on the accumulated wisdom, plus the wisdom that it might not be wisdom […] to teach both to accept and reject the past with a kind of balance that takes considerable skill.[58]

It is this skill that one learns from being part of the 'non-literal' world of the scientist. It is the tacit understanding one obtains from being involved in a practice. Understanding, in the 'tacit' sense, is not possessing knowledge, but is a way of acting. Again, we could express this difference as, knowledge is always *about* something, so it is explicit, whereas tacit understanding is part of the language *of* knowledge. These would be practices which fore-structure knowledge that come with being in the world. Kuhn's own inclusion of tacit knowledge and intuition in *Structure* appears to be aimed at overcoming the location of epistemology as the source of scientific change.[59]

To call what is tacit in some sense 'knowledge' means it must be explicable, which appears to be an oxymoron.[60] Just as one learns to swim or ride a bicycle without recourse to a general theory of what one is doing, we acquire our ideals and beliefs about science by being socialized into them and not by learning general theories or forming beliefs about them. The way we are with regards to science, politics, eating rituals or whatever, embody a whole cultural interpretation of what it means to be a human being. There is a cultural self-interpretation implicit within our shared practices and paradigms. Our interpretation here is very close to Dreyfus, when he writes, 'this inherited background of practices cannot be spelled out in a theory because (1) the background is so pervasive we cannot make it an object of analysis, and (2) the practices involve skills.'[61] In the same way, Kuhn argues that the cognitive content of science is not to be found in theories or facts, but in acquiring a 'way of seeing'.[62] If one understands this as a type of Austinian performative act it can then be traceable to how beliefs and language tie up. Understanding, for the weak reader, however, is not something we possess in our heads (facts, beliefs, propositions) but something we do (meaningful activity). Just as the student learns how to apply

[58] Feynman, 'What is Science?', p.188
[59] Kuhn, *Structure*, pp.191-198
[60] Harry Collins, *Tacit and Explicit Knowledge* (University of Chicago Press: Chicago, 2010)
[61] Hubert Dreyfus, 'Holism and Hermeneutics', *The Review of Metaphysics*, 34 (1980), 3 – 23 (p.7)
[62] Kuhn, *Structure*, pp.187-189

exemplars to new situations he reveals his understanding of it, not by what propositions he believes to be true or false about a class of problems, but by acting in accordance with the accepted rules of the paradigm.

Most, if not all, non-scientists do not occupy such a rigorous framework as a paradigm.[63] The looser sense of paradigm is 'worldview', but worldviews do not tend to go into crisis, they have no such analogous practice or aim as 'normal science'. As scientific worldviews are parasitic on scientific paradigms the perceived clashes can be re-interpreted through the methodological approach.[64] For example, Intelligent Design Creationist (IDC) scientists interpret the lack of intermediate fossils 'as' evidence for the falsity of evolutionary theory.[65] They do not see it 'as' evidence that fossils are hard to preserve.[66] As IDC requires a number of negative observations to be true, such that there are no intermediate fossils, or the breeding of one kind will never produce another kind, it meets with general observation. As there is little or no application for these observations, a part from trivially re-describing what we see, it allows us to hold these beliefs without ever having to change them. As Helga Nowotny says, 'the pseudo-sciences aspire to become scientific', but in its aspiration the IDC worldview gets its meaning from the veracity of the evolutionary paradigm it seeks to replace.[67] There are no IDC scientists busy trying to disprove Egyptian creation myths. Similarly we see with episodes such as the Lysenko affair or the Nazi rejection of 'Jewish physics' that their actions only become meaningful due to the paradigmatic presence they are looking to overcome.[68]

[63] To view the world as a Newtonian or Einsteinian takes considerable effort and training as it is at odds with our everyday experience. Andrea A. Di Sessa, 'Unlearning Aristotelian Physics: A Study of Knowledge-Based Learning', *Cognitive Science*, 6 (1982), 37-75

[64] We find this with the religion verses science debates. Barbour tries to narrow the gap between perceived differences in religious and scientific paradigms. Ian G. Barbour, *Myths, Models and Paradigms: A Comparative Study of Science and Religion* (New York: Harper and Row, 1976)

[65] Philip Kitcher, *Abusing Science: The Case Against Creationism*, (Cambridge, Mass.: MIT Press, 1982)

[66] Indeed we might say one cannot offer evidence in favour of a negative proposition, but in the past, it could be argued, scientists have done just this. It was that lack of perpetual motion machines that Helmholtz took as inspiration for his work on the principle of energy conservation. Fabio Bevilacqua, 'Helmholtz Ueber die Erhaltung der Kraft: The Emergence of a Theoretical Physicist', in *Hermann von Helmholtzand the Foundations of Nineteeth-Century Science*, ed. by David Cahan (London/ Berkeley: University of California Press, 1993), pp.291-233 (p.297)

[67] Helga Nowotny, 'Science and Its Critics: Reflections on Anti-Science', in *Counter Movements in the Sciences*, ed. by Helga Nowotny and Hilary Rose (Dordrecht: Reidel, 1979), pp.1-26 (p.15)

[68] R.G.A. Dolby, *Uncertain Knowledge: An Image of Science for a Changing World* (Cambridge: Cambridge University Press, 1996), p.86

In *Structure*, the scientist's engagement with exemplars provides the examples of the paradigm, however, they come to the exemplars in a world where they are already meaningful. They do not have to establish its meaning as a new phenomenon but only integrate it into further articulations of the paradigm. Kuhn gives three exemplars of eighteenth century mechanics. He says if the student has understood they will be able to see a family resemblance to a singular principle of 'vis viva' or 'actual descent equals potential ascent'.[69] Kuhn makes the point that such a principle only makes sense if one can recognize what the problems or applications are, which means knowing 'something, prior to the law, about the situations that nature does and does not present.'[70] This knowing something 'prior to the law' with reference to which the law can make sense appears to be more than knowledge produced by the paradigm, but a tacit understanding. This can also be understood in how he criticizes the traditional models of learning, such as the presentation of words and nature together.[71] It is the privileging of the methodological approach that tells us that Aristotle, Newton and Einstein all just saw objects fall ('about' language). Yet, as we always see an object 'as' something, that 'as' comes from our lived relationship ('of' language) with the world. This privileging allows for objective statements to be made, so we can be confident when we say Aristotle, Einstein, as well as myself, all see objects fall. Why 'falling' is significant in each case is different however. For Aristotle objects falling are 'about' natural dispositions, for Newton they are 'about' mechanical forces, for Einstein they are 'about' the curvature of space-time warped by mass. Going back a step further, why they should want to explain falling objects is more different still. The methodological stance likes to speak of 'observation', but in this word is an implied normative sense of light, vision and optics. All three were markedly different in the 'Ancient world', so what it was to 'observe', let alone what they were observing, has little continuity with its modern scientific use.[72]

Paradigms are synonymous with the normal science that they allow. They work most effectively when no one is questioning its fundamental assumptions, as paradigms at their best appear to be the same as the 'literal' world. Once we no longer see the

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[69] Kuhn, *Structure*, p.191
[70] Ibid.,
[71] Ibid.,
[72] David C. Lindberg, *Theories of Vision from al-Kindi to Kepler* (Chicago: The University of Chicago Press, 1976), pp.11-14

paradigm, but just objective reality we can get to work as a normal scientist. The precise sort of work a paradigm allows is a direct result of the scientist working fluently in the language of the paradigm. Another term for language of the paradigm is normal science or 'puzzle-solving'. Whatever those puzzles might be are what the paradigm is 'about'. However, remembering that the 'about' is founded in and preceded by the 'of' language, is how we get change in science. In times of normal science a paradigm is concretely 'about' something that we investigate. Here epistemology is explicit. How we investigate and situate what our investigations are about is through the languages of normal science. This will be whatever is agreed as standard normal scientific practice at the time. In times of crisis, where we begin to doubt what it is that we are investigating or the objects that the paradigm allows to meaningfully exist, the 'about/ of' relationship becomes re-structured. Once we have suitable reason to doubt the veracity of the paradigm, we can re-cast our ways of doing science. So now our investigations are 'about' the paradigm itself. We can do this because the paradigm and normal science are not 'science' itself, for this takes place in a world even in the absence of a paradigm. Science does not cease to occur, but is just practiced differently from how it once was. With the dissolving of a paradigm we lose its normal science, but not science altogether for science is not its 'about' language. Normally this displacement is not so violent and unsettling, for if a rival paradigm offers new insights, we can quickly resume normal science.[73] It is in the cases where there are no rival theories, or too many to choose from, that we experience the absence of a paradigm. This is marked by highly conflicting views about how to proceed with research or what exactly needs investigating. This may lead to people abandoning the project or it being carried on, but as a softer or pseudo-science, that finds less and less application to the world. If, like the strong reader, we equate paradigm with the world in both its 'literal' and 'non-literal' sense, all disputes are settled epistemologically or linguistically. How do we know the objects as described under one paradigm are the same as the objects described under another paradigm? If Kuhn is understood as presenting a theory of scientific change, where we cannot appeal to some meta-paradigm for methodological rules, the challenge then is leveled straight at epistemology or language. The consequences of this being a form of radical meaning variance, relativism, or ultra scepticism about our ability to

[73] Kuhn, *Structure*, p.21

discover facts which, depending on your religious or political outlook, may be a desirable thing.[74]

The weak view of paradigms makes a distinction between 'puzzles' and 'problems'. Paradigms inform us of what are or are not legitimate puzzles to be solved. 'Problems' are deficiencies with the paradigm that come about by anomaly accumulation. Unlike a puzzle, 'problems' have no solutions from within the paradigm. They would need to be re-articulated through another paradigm so they can be converted into a puzzle. Here they are given meaning by being in relation to all other puzzles. This tends to be a retrospective activity, for at the time it is hard to know what phenomena are connected. For example, prior to the Big-Bang paradigm the hiss of static on radios was not regarded as a problem or puzzle for steady-state theorists. Gribbin notes that papers published as early as 1948 posited a background radiation signal of a Big Bang, but no radio astronomers saw it worthy of attention.[75] This theoretical prediction was completely forgotten until the 1960's, where Penzias and Wilson picked up an unexplainable hiss on their radio telescope.[76] Even here neither was looking for this radiation signal. Given the potential enormity of the discovery, as we understand it now, maybe we can assume this prediction of cosmic background radiation was just not a legitimate puzzle for radio astronomers.

The idea that scientific knowledge is gained from a repeated exposure to problems that then allow the scientist to apply those rules and theories, is for Kuhn, to misplace 'the cognitive content of science.'[77] That working out of solutions to puzzles is through analogical reasoning, rather than deducing from general principles.[78] General

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[74] Bricmont and Sokal argue against two types of relativism that are the result of the epistemological challenges that a strong reading of 'truth' brings about. They are 'cognitive (or epistemic) relativism, and methodological relativism.' Bricmont and Sokal, *The One Culture?*, p.38; also see, chapter 4 in Alan Sokal and Jean Bricmont, *Intellectual Impostures: Postmodern Intellectuals' Abuse of Science* (London: Profile Books, 1998); Alan Sokal, *Beyond the Hoax: Science Philosophy and Culture* (Oxford: Oxford University Press, 2008), pp.171-228

[75] John Gribbin, 'Cosmic Luck: The Discovery of the Cosmic Microwave Background was an Accident', *New Scientist*, 80 (1978) (1), p.262 (p.262)

[76] Ibid.,

[77] Kuhn, *Structure*, p.188

[78] Leonard Nash, who was also Kuhn's co-lecturer, proposed this method of exposing students of science to similar idealised cases. In *The Use of Historical Cases in Science Teaching* (1952), Nash identified two delusions that students suffer from, which he termed 'the cult of the fact' and 'the cult of the method'. The answer that Nash proposed was 'the case approach' which involved presenting students with edited versions of classic experimentalist writings that will enable them to follow or re-

principles, as Rouse puts it, 'are useful as relatively compact expressions, but the understanding they express is embedded within the disaggregated ability to grasp various situations in those terms.'[79] The weak reader appreciates how history and paradigms are linked in such a way that controversies of one age are re-told as a teleology of scientific progression. Exemplars, as models for the paradigm, are given to students who are taught to see similarities between puzzles where it is known that those exemplars are applicable. We view exemplars through normal science, which means, it has been distilled from its original 'about/of' relationship where it was less a puzzle and more a problem. It would seem more than chance that two people would independently come up with infinitesimal calculus or a theory of evolution if old paradigms were not under threat forcing people to conceive of alternatives.[80] What the science student experiences, however, is not a world where we are being pushed to consider alternatives. So taking something like 'Newtonian forces' these are already a part of our world, but with their Einsteinian modifications. Through experimentation students are taught how to 'see' forces in action. Yet, for Newton and those prior to him the world did not contain forces, as the movement of objects could be explained in terms of natural dispositions. The way that modern students of science come to know about 'forces' is not the same way that Newton did, as they do not occupy the same 'non-literal' world. The 'of/ about' relationship is different to Newton's. The language of science for pre-Newtonians was conducted in such a way that its 'about' content could include things like God, faster-than-light actions, mechanistic forces through aether and so on. Whist we still might be able to say they are statements 'about' reality, there is no way of conducting science that would allow us to preserve these as part of our 'about' language and still be acting scientifically in the modern sense. This, for Fuller, is displayed in the idea of internal/ external histories of science.[81]

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enact the experiment in question. Steve Fuller, *Thomas Kuhn: A Philosophical History For Our Times* (Chicago: The University of Chicago Press, 2000), pp.183-184

[79] Joseph Rouse, 'Kuhn's Philosophy of Scientific Practice', in *Thomas Kuhn* ed. by T. Nickles (Cambridge: Cambridge University Press, 2002), pp.101-121 (p.108). Elsewhere within the hermeneutic phenomenological tradition this sentiment can be expressed as our understanding be it scientific or religious, is always already involved in a horizon of projected possibilities. Ginev expresses this as 'a horizons of understanding the world as possibilities that can be actualized'. Dimitri Ginev, *The Tenets of Cognitive Existentialism* (Ohio: Ohio University Press, 2011), p.164

[80] Jerry D. Moore, *Visions of Culture: An Introduction to Anthropological Theories and Theorists*, 2nd edn. (Walnut Creek, CA.: AltaMira Press, 2004), p.70

The facts and knowledge, internal to science, are separate from the social and cultural factors that made the work originally meaningful. So today one does not need to know about Newton's theological or metaphysical beliefs in order to study his mechanics. External history is closer to what sociologists and historians of science study, but for me they still contain strong and weak readings of events by what 'events' we deem as relevant. For Fuller, this is evidenced by the fact that after a period scientific work becomes 'canonized' as being integral to a paradigm. Scientists can then relax and forget about the social and cultural implications by which the problems were first posed.[82] For example, prior to the establishment of quantum mechanics philosophy played a large role in debates about the 'reality' of tiny events, but once we have 'good reason' to accept the reality of such claims, the actuality of the event about which the philosophizing occurred seems unimportant. We see this attitude in contemporary scientists, for example, Steven Weinberg skates over the issue by saying, 'the choice of scientific question and the method of approach may depend on all sorts of extrascientific influences, but the correct answer when we find it is what it is because that is the way the world is.'[83] He has also criticized Werner Heisenberg over his 'philosophical' pronouncements in obscuring scientific claims. He then backs up this accusation of sloppy thinking by picking out technical mistakes of Heisenberg's in the German Nuclear weapons programme.[84]

Kuhn tells us that puzzles always exist within normal science because a paradigm is never fully able to resolve all its research problems. 'The very few that have ever seem to do so (e.g., geometric optics) have shortly ceased to yield research problems

[81] Fuller, *Thomas Kuhn*, p.184
[82] Ibid., p.185
[83] Steve Weinberg, 'Sokal's Hoax', *New York Review of Books*, 8 Aug. 1996
[84] Ibid., Weinberg maybe overlooking the fact that Heisenberg was working within a political ideology where physics could be 'Aryan' or 'Jewish' and the associated pressures of promoting one or the other. Mark Walker, *Nazi Science: Myth, truth, and the German Atomic Bomb* (New York: Harper Collins, 1995), pp.263-268. An interesting historical point is that Steven Weinberg's encounter with philosopher Max Back at Cornell, as an undergraduate, left him very dissatisfied with what philosophers took as real. Richard Feynman was also a member of staff at Cornell University whilst Max Black was lecturing there. Arguably Richard Feynman's dislike for philosophy as a source of confusion over 'relativistic' claims can be found in his exposure to Max Black's ideas. Feynman saw the philosopher's intrusion into relativistic physics as such a problem that he devotes a section to it in an otherwise introductory science text on classical and relativistic physics. Richard Feynman, *Six Not-So-Easy Pieces: Einstein's Relativity, Symmetry and Space-Time* (London: Penguin Books, 1999), pp.73-77.

at all and have instead become tools for engineering.'[85] It is when we can engage with the paradigm or a part of the paradigm in this way that normal science gets going. I think there is room for useful analogy here between a law ($f = ma$) as a tool and the tool as a physical object (hammer).[86] A hammer becomes a hammer in its use, but it is also in its use that the hammer removes itself from our attention.[87] It is only when something goes wrong with the hammer or our situation of hammering changes that we are forced to look at it again. We then cease to see it as a hammer, but as an object with properties and qualities. Similarly, for the engineer, laws like $f = ma$ are completely functional for all macro and sub-luminal phenomena. Just as a carpenter may pick up a hammer for a specific job, so an engineer may call upon a specific law of motion for completing a task. It is only in situations where forces are not obvious, such as in the dynamics of non-Newtonian liquids, that we may be forced to reflect upon the situation in which we are using it. Is the situation at fault? Is my application incorrect? Or have we found an exception to the law? In times of 'normal science' we may overlook the anomaly if it persists, but when conflict between the world and paradigm is unavoidable and is preventing further research we may have to take up the challenge of re-writing those laws.[88] The 'law' like the 'hammer' reveals understanding in the way that it is used. If a research scientists were trying to determine current resistance with $f = ma$ it would soon be pointed out that it is the scientist who is at fault, and not the equation. Likewise, a carpenter who was trying to saw a piece of wood in half with a hammer would be picked up on their craftsmanship. A hammer only has only so many possibilities 'as' a hammer due to the world we live in. The same is true of 'evidence' and 'observation' in that the paradigm situates certain possibilities as meaningful. Just because there are possible scientific alternatives that we now know about, does not mean that people in the past were free to choose between them or whatever the unthinkable alternative is for us now. We are not free to understand the world in anyway we choose, no more than we

[85] Kuhn, *Structure*, p.79 - In Heidegger's discussion of the 'present' and 'ready-to-hand' he relates this to how we are with tools. Heidegger, *Being and Time*, pp.73-74
[86] This interpretation is part of the hermeneutic realist tradition in phenomenology, where scientific practices are a part of 'readable technologies' that are always already embedded in a relationship of practices. Ginev *The Tenets of Cognitive Existentialism*, p.xxiv
[87] Heidegger, *Being and Time*, p.68
[88] Kuhn writes, 'as in manufacture so in science - retooling is an extravagance to be reserved for the occasions that demands it.' Kuhn, *Structure*, p.76

are free to see a hammer any other way than a hammer, due to the immersion in the 'non-literal' world, that structures how we experience 'reality'.

The 'group-licensed way of seeing' that Kuhn talks about, as an explanation of the paradigm, is for the weak reader a comment on the 'non-literal' world, and has nothing to do with psychology or linguistics.[89] Paradigms are spaces for meaning. They allow certain thoughts, ideas and actions to be meaningful in respect to the 'literal' world and give meaning to practices that look to say things about the 'literal' world. Paradigms are not just a gathering of beliefs or methods, but they are what allow those beliefs and methods to be exercised as intelligible and rational. A paradigm is not just an exemplar, but it comes to be illuminated by the success of exemplars. A paradigm is not just that which can be in crisis, although its potential for crisis is what gives revolutions their axis.[90] As Rouse comments, 'scientists use paradigms rather than believing them' and that paradigms 'should not be understood as beliefs (even tacit beliefs) agreed upon by community members', but rather as ways of acting.[91] It appears hard to justify actions, as Rouse has done, if the person does not believe what they are doing, is in some way correct. This can be hard to accept where the truth or certainty of our actions does not come from the truth of our beliefs or the certainty of our knowledge. What this points to is that we already know how to act in the world, even in the absence of certain knowledge, which most our knowledge is anyway. As Wittgenstein says, the truth of a statement is the test of my understanding of that statement, which is, how we act in the world given what is allowed to be said.[92]

Due to the paradigm (about) being non-identical with the world (of) science can be practiced in the absence of a total description. Science here may not continue as 'normal science', but takes up a different way of being practiced. Typically when we discuss science and the sort of science portrayed in PUoS we are illustrating 'normal

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[89] Ibid., p.186
[90] Dreyfus seems to be saying something similar when he argues that background practices or paradigms do not 'consist in a belief system, a system of rules, or in formalized procedures; indeed, it seems the background does not consist in representations at all.' Dreyfus, 'Holism and Hermeneutics', p.9
[91] Rouse, 'Kuhn's Philosophy of Scientific Practice', p.107
[92] Ludwig Wittgenstein, *On Certainty*, trans. by Denis Paul and G.E.M. Anscombe (New York: Harper and Row, 1969), para.80

science'. 'Normal science' is synonymous with the language of the paradigm. It is also 'about' the paradigm to the extent that it generates research puzzles. The more powerful the paradigm the more diminished the distinction between the 'about/of' becomes. So where a paradigm is at its most totalizing, the normal science and puzzles generated appear to be 'about' the 'literal' world, if not identical to it. From this the scientist pushes further to expand the identity between paradigm and the 'literal' world. In so doing, we start to accumulate anomalies. The sort of precise levels of inquiry the paradigms promise and allow eventually bring about their own demise. Here, it starts to become apparent that what we had been calling 'forces', 'aether', or meaningless 'static hiss', actually belong to the paradigm and are not natural categories of the world. So our ways of practicing science bring about a change in what we think the 'literal' world is about, for it is not what our paradigms say it is about. 'Crisis' is the point where 'normal science' and 'puzzles', as defined by the paradigm, cease to be legitimate and we move from speaking *about* the world, through the language *of* the paradigm, to turning science onto the paradigm itself. The objects that we thought were in the world cease to generate puzzles. Instead we get problems which directly illuminate the paradigm, whereas puzzles are only seen through the paradigm. Metaphysically, we move from the commitment that we are investigating the world through the language of the paradigm to investigating the paradigm itself – we realize that it is not the world that our science is 'about' but the paradigm itself.

Up until now the examples for paradigmatic shifts have been rather large universal ones, such as the movement from Ptolemaic to Copernican planetary systems. In *Structure* Kuhn tells us they do not need to be that dramatic and might only concern a small field or tiny shift in application. The revolution in our understanding of x-rays demanded a new look at the regional ontology of electromagnetism and radiation, but it added very little to fields that did not utilize x-rays.[93] So it could be thought of as new piece of knowledge or instrumental application, but not something that requires people to address the fundamentals of their field, which governs how they practice science. Practices that cannot agree about the regional ontology of their subject will never get to a stage of normal science through which a paradigm may enter into crisis.

[93] Kuhn, *Structure*, p.93

The inability to agree on the regional ontology of a subject is also connected to the subject's ability to produce research puzzles. At the heart of the social sciences, there is a disagreement about whether the prime methodology should be quantitative or qualitative. Both aim to make valid scientific inferences, but what constitutes a valid inference changes depending on whether one is dealing with general causal inferences or how meaning is structured or negotiated in social actions.[94] We find that there is an underdetermination of possible meanings when there is no dominant paradigm, such as in sociology, psychology or economics, and its overdetermination when there is only one possible means of interpreting phenomena. For example, prior to the agreed theory of combustion there were many competing theories as the evidence underdetermined the phenomenon. Once the oxygen theory of combustion gained ascendance we find it hard to interpret combustion in any other way. To give the reverse of this situation, prior to the development of non-Euclidean geometry, all phenomena could be treated as objects described by Euclidean geometry. So much so, that Kant developed his categories of mind on this assumption.[95] Now we know that whatever geometry we choose to describe the world through is not dictated to us by what we see. The paradigm of Euclidean geometry no longer overdetermines the actions of objects or the structure or reality.

When we say things like how we describe the world is not dependent on what we see it can be easy to lapse into the strong position, concluding that we cannot make true statements about the world. Kuhn resists talk of 'Truth' when dealing with paradigm change. Revolutionary, as opposed to normal science, is just a different way of producing an 'about' language, by having a different 'of' language. To have these two views of science such that the historical/ weak view concentrates on the 'of' language, whereas the methodological/ strong view concentrates on the 'about' language, is not itself incorrect. What is incorrect is to prioritize these interpretations, so that the weak follows from the strong, or devalue the process that gives meaning to these distinctions. Of course we can say sensible objective things about the world through this methodological mode, but what the weak reader takes this to mean is that the highly accurate abstractions we get from this mode are the product of a world that is

[94] Gary Goertz and James Mahoney, *A Tale of Two Cultures: Qualitative and Quantitative Research in the Social Sciences* (Princeton, NJ.: Princeton University Press, 2012), p.4
[95] Friedman, 'Einstein, Kant and the Relativised A Priori', pp.256-257

already articulated by a paradigm. The 'non literal' world situates phenomena as already meaningful so we can just get on with the business of investigating it, 'as if' it were the 'literal' world.[96] By taking the view that the world has only one mode of presentation through the methodological stance, this can result in interpreting human beings as just another object in a world of objects. The ability to reflect and interpret oneself as an object is necessary to science and knowledge production. We refer to this perspective as being 'objective', but there is nothing necessary about it. For the scientific attitude of objectivity is still only an attitude. No doubt the attitude of mind by which one sees the objects in the world as objective has to be the best attitude for *doing* science, but it is not necessarily the best attitude for thinking *about* science.

As a paradigm is not the world it allows room for creativity and change. Once the paradigm has gone from being the language of the science to being just another thing science is about, the scientist can begin new work. In the absence of the paradigm to guide research, where certain possibilities had been ruled out, previously unimaginable meanings become viable. Here, though, there has to be historical continuity. New meanings can only be considered because knowledge and scientists are historical and their commitments to the (metaphysical) 'literal' world are bound to the (historical) 'non-literal' world. We are not free to consider any meaning we want. For example, Aristotle could not have considered quantum mechanics any more than we can consider whatever the alternative to all our best theories of gravity or quantum mechanics are.[97] As Kuhn writes, 'scientists can agree that Newton, Lavoisier, Maxwell, or Einstein has produced an apparently permanent solution to a group of outstanding problems and still disagree, sometimes without being aware of it, about the particular abstract characteristics that make those solutions permanent.'[98] The nature of this disagreement might be about how we moved from the 'about' of crisis solution to the more robust and transparent 'of' language of normal science. The

[96] Feyerabend draws a similar distinction but his takes in whole traditions, but like Heidegger he says these points of view differ by 'intention, not fact'. The 'theoretical tradition' like the 'present-at-hand' seeks to produce objective, fact-like statements, but to get to this point we have to acknowledge that people are active in using, creating and maintaining those statements. This is the 'historical tradition' and it focuses on use and interpretation and has its analogue with the 'ready-at-hand'. However, Feyerabend calls the maintaining of these two world views side-by-side a 'false consciousness' which promotes the 'theoretical tradition' over the 'historical'. Paul Feyerabend, *Farewell to Reason* (London: Verso, 1987), p.126

[97] Kuhn later writes that paradigms cannot be characterised by shared beliefs only that scientists accept a sufficient amount of problem solutions. Kuhn, *The Essential Tension*, pp. xviii – xix.

asymmetry between a paradigm and the world means scientists ‘can agree in their *identification* of a paradigm without agreeing on, or even attempting to produce, a full *interpretation* or *rationalization* of it.[99] Lack of a standard interpretation or of an agreed reduction to rules will not prevent a paradigm from guiding research’.[100] There has been a large weight of significance attached to the word ‘rational’ and ‘irrational’ in academia, at least since the publication of *Structure*. The implication being, that if we cannot provide a full and explicit account of what it is to act scientifically, we then have no justifiable grounds for certain actions or beliefs. As the strong reader has Kuhn challenging the rational enterprise of science, due to a dogmatic obedience to paradigms or inability to compare the contents of its paradigms, we are stuck for a justification of its activities. The problem with ‘rationality’ is two-fold: like ‘truth’ its definition is self-referring, since we arrive at what is ‘rational’ via a process that is itself rational. The term presupposes the method by which we will arrive at our conclusion. Secondly, if we use a metaphysics that has action stemming from belief then this is the equivalent of putting knowledge prior to tacit-understanding or an ‘about’ before an ‘of’. If we could articulate what makes beliefs true and how knowledge refers to the world, we would then have concrete grounds for the actions and beliefs we form. Yet we do not just see the world or reality and make beliefs about it. We always see it ‘as’ something. We find the world has already been rationalized for us by a paradigm so we can make sense of statements that appear to refer to a ‘literal’ world. Similarly, we can just talk and argue about ‘truth’ and ‘rationality’ as if they were sensible things.

Due to theories extending to ever more unobservable phenomena, philosophers and scientists have had to take up a more considered position, since we are using representations of what we believe there is. Here, mathematicians and theoretical physicists are split. Some have no commitment to the reality of the equation outside of its ability to predict and control phenomena, whereas others take mathematics to be

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[98] Kuhn, *Structure*, p. 44

[99] The different ways ‘rationality’ became problematic can be seen in the diverse fields that attempted to address it. There were the Popperian economic and social theorists of Ralf Dahrendorf and Hans Albert against the Frankfurt school, most notable Theodor Adorno and Jurgen Habermas, see Adorno et al, *The Positivistic Dispute in German Sociology*, trans. by Glyn Adey and David Frisby (London, Heinemann Educational Books, 1976). In Britain the rationality debate concerned whether cross-cultural, universal forms of reasoning could exist, the main protagonists. See *Rationality* ed. by Bryan Wilson (Oxford: Basil Blackwell, 1970)

[100] Kuhn, *Structure*, p.44 [Italics in original]

'literally' the language of nature.[101] The success of mathematical predictions, such as the Higgs Boson, seems to give us good reason to think that there is some deep connection between reality and mathematics, but this is an imposition of the methodological ideal.[102] The history of science shows how we not only re-tell the aesthetics of a practice (i.e., what scientists regarded as a beautiful or simple explanation), but current mathematical predictions, such as 'string theory', seem to run counter to what was acceptable as a decent mathematical explanation. Rather than being led by an aesthetic sense of mathematical truth, such that we should seek simplicity in our explanations, we are fleshing out all sorts of theories as the paradigm for fundamental reality is weakening.[103] Where epistemology has progressed, moving from categorical statements to more probabilistic ones, this has not dampened the philosophical debates over the existence of scientific objects. Probabilistic arguments, however, are statements about our paradigms. For what is probable can only be conceived by what is possible, and that is limited by how we are allowed to interpret the world.

8.6 - Normal and Revolutionary Science

History, for Kuhn, enables us to understand what science is and how it proceeds. Kuhn suggests that the science we ordinarily speak about is 'normal science' and it is part of the job of the paradigm to tell the story of that science, in the light of present knowledge.[104] This has come to be known as Whig history. Fuller express at least three types of history in the history of science, as 'Whig,' 'Prig' and 'Tory'.[105] All of which have to do with how we regard our current position and how we got here. All of these come with teleology, history as told by the winners, losers, or the neutral historian. The weak reader is aware of this, but also that however we think about

[101] Sadri Hassani, *Mathematical Physics: A Modern Introduction to Its Foundations* (New York: Springer, 1999), p.v

[102] Arianna Borrelli, 'The Case of the Composite Higgs: The Model as a "Rosetta Stone"', in Contemporary High-Energy Physics, *Studies in History and Philosophy of Science Part B: Studies in History and Philosophy of Modern Physics*, 43 (3), Aug. (2012), 195-214

[103] Smolin, *The Trouble With Physics*, pp.151-154

[104] Ginev argues that there is another way to understand 'normal science' in a totally non-reducible form. He says that in Kuhn's writing he gradually starts referring to normal science as a research tradition based on practical experience where initiation for the scientist is training in esoteric practices such as experimentation. This Ginev claims 'does not belong to post-positivistic epistemology. It is rather congruent with views and postures of philosophical hermeneutics.' Ginev, 'Re-Reading Normal Science', p.71

ourselves or the subjects and activities we are engaged with is itself historical. As this has to do with thinking about science and not doing it, it leaves the actual practice relatively free to proceed. As non-scientists however we want to re-describe past events will not affect what science is now. What would affect current science is by scientists working off a historical interpretation of an assumed phenomenon, such as what 'consciousness' or 'intelligence' is.

Yet as PUoS is involved with thinking about science, not doing it, it is crucial that this element be highlighted as part of interpreting science. For example, if we understand science only in its methodological guise we can re-tell the story of science as shifts in knowledge, as achieved by great white European men. By adhering to a few simple methodological principles debates were settled by looking at the evidence. Equally we can re-tell the story of those achievements by philosophers, as contributions to current disciplines, but arguably we can only do that because of how they contributed to the way we think about practices and their relationship to metaphysics. For example, Quine writes about how Aristotle, Plato, Descartes, Leibniz, Locke, Berkeley, Hume, and Kant were all 'actually' scientists, even when it was called 'natural philosophy'. 'All these luminaries and others whom we revere as great philosophers were scientists in search of an organized conception of reality.'[106] This is to re-tell the story of past thinkers 'as if' they were sharing something in common with current philosophers, scientists, or our commonsense notions about 'reality'. The bits that were 'true' stuck it out and found their way into productive discourses and all the unusable speculative stuff is now what we call philosophy. We could say this is to get the situation precisely the wrong way around. Thinkers of the past do not think like current day scientists because they were engaged in some unified 'search of an organized conception of reality'. Rather, people today, including scientists, think like they do, can entertain a notion about what it means to be involved in a search for an organized conception of reality because of what philosophers of the past did. In the first instance, our current state is inevitable. In the second instance, our current state is contingent. Had Descartes been Buddhist would modern epistemology be saturated with the mind/body problem? Would the Cartesian 'I' be so problematic? The very possibilities about how we relate to history are tied up in our philosophy and

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[105] Fuller, *Thomas Kuhn,* p.24

metaphysics. Take Gardner's 'worldmaking' which extends Goodman's 'many-worlds' idea.[107] He argues that some of us are blessed with the ability to look past our current state of knowledge and intuit a completely new conception of the world, such as Newton or Einstein. Yet at the same time he is forced to say how they are still products of their time. Einstein sought a unified picture of reality which was argued against by Gödel, Bohr, and Heisenberg, amongst others.[108] So, contra Quine, it was an organized conception of reality inherited from Aristotle, that the world should be knowable or unified in any deep sense, rather than Einstein having any greater insight into the structure of reality. Gardner wants to place this 'worldmaking' ability at the level of psychology. That some how these select few were '"pretuned" to make telling discoveries'.[109] Firstly, we are not free to make *any* world because we are born into a world that pre-exists us, with a history, culture, society, language that fixes a notion of reality for us. So as great as Newton was his thoughts on alchemy tend to be forgotten, along with the connection his physics had to theology. Secondly, Goodman and Gardner worked together at Harvard.[110] There, Gardner was exposed to ideas from Goodman and Piaget, that then allowed him to make sense of 'multiple intelligence theories'. Thirdly, if the world can only be described a number of ways, if enough people produce total descriptions, there would be left a collection of descriptions that 'work'. This is part of an evolutionary epistemology, but gives the illusion of individuals being deeply connected to reality. Fourthly, developmental psychology is part of an unstable paradigm. The same observations (depending what you count as an observation) have multiple explanations. We take it that we know what competence or intelligence at mathematics or music is, yet both develop and change along with what it is to be good at them because the 'non-literal' world in which their meaning is situated changes. What 'music' and 'maths' were 'about' changed and these changes were preceded by how those disciplines were practiced. The above examples are to show the affect of how we construe metaphysical ideas

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[106] Willard V. Quine, *Theories and Things* (Cambridge, Mass.: Harvard University Press, 1981), p.191
[107] Paul Hoyningen-Huene talks about Kuhn's 'plurality-of-phenomenal-worlds thesis' which echoes Goodman's 'many-worlds thesis'. Hoyningen-Huene, *Reconstructing Scientific Revolutions*, pp.36-38
[108] Howard Gardner, 'Gifted Worldmakers', in *The Truth About Truth: De-confusing and Re-constructing the Postmodern World*, ed. by Walter Truett Anderson (New York: Penguin Putnam, 1995) pp.182-188 (p.187)
[109] Ibid.,
[110] John White, 'Illusionary Intelligences?', in *New Philosophies of Learning*, ed. by Ruth Cigman and Andrew Davis (Oxford: Wiley-Blackwell, 2009), pp.239-259 (p.250)

such as, history, knowledge, or world, which then effect what becomes sensible or reasonable to assume.

For normal science to take place, its fundamental principles cannot be questioned. It is not that it is impossible to question them, but more that there is no need to when normal science is functioning correctly. Here we have a 'stable horizon of expectations, anticipations, and orientation in the research process.'[111] The point of normal science is that it guides research by highlighting puzzles pertinant to that paradigm, but it also produces anomalies when the puzzles cannot be totally solved. The coherence and consistency of nature do not need to be re-examined every time an anomaly occurs but can be ignored. A strong interpretation of normal science is that all those taking part are in agreement over the fundamentals of their field. They are blinded by the paradigm to which normal science is a slave.[112] Kuhn says that those doing normal science do not engage with controversy over the fundamentals nor look for new phenomena or novel theories. A lack of controversy or novelty seeking activity, however, is not inconsistent with disagreement. Whether in agreement or disagreement normal science is not directed towards scrutinizing the fundamentals of the field, but is more a 'mop-up' job.[113]

In the chapters 'The Nature of Normal Science' and 'Normal Science as Puzzle-solving', Kuhn attempts to describe the sort of person that would engage with normal science or devote their life to a puzzle.[114] Normal science, for Kuhn, presupposes the disposition of scientist as puzzle-solver. That they attend only to the formal concepts and theories of a given paradigm. Normal science also presupposes the willingness of the puzzle-solver to give up a host of possible puzzles, for interest in a particular set of questions. Whatever 'puzzle' a scientist occupies themselves with, they have to believe that whatever the solution, it will actually tells them something about reality. The strong reader takes the term 'puzzle' as the metaphor, whereas, for the weak reader any talk of interrogating nature or investigating things as they 'really' are is the

[111] Ginev, *The Tenets of Cognitive Existentialism*, p.167
[112] Karl Popper, 'Normal Science and its Dangers', in *Criticisms and Growth of Knowledge*, ed. by Imre Lakatos and Alan Musgrave (Cambridge: Cambridge University Press, 1999a), pp.51-58 (pp.55-56)
[113] Kuhn, *Structure*, p.24
[114] Ibid., pp.23-42

metaphor.[115] Equally, any sense that world might be of our own construction, as with Goodman, or we are somehow involved with proving the existence of the external world, is also a metaphor. Puzzles are a part of normal science, but because paradigms are not identical with the world, puzzles cannot be either. The puzzles tell us something about the paradigm, which in turn tells us something about the 'non-literal' world that makes them possible. It is in working to make these metaphors 'literal' that we uncover their instability. For the strong reader, a term like 'puzzle' in *Structure* is a metaphor, that science or philosophy of science has to use metaphorical language in order to express what science is really doing. For the weak reader, any expression of what there 'really is' is the metaphor.[116] Derrida is probably one the finest exponents of this point through his essays 'Supplement of the Copula' and 'White Mythology' he presents the argument that any term that appears to denote categorical states always has to start off as a metaphor.[117] As Norris remarks,

> [T]here could be no accounting either for science or for the history and philosophy of science, were it not for (1) the existence of a real-world, mind independent domain of which knowledge is none the less attainable; (2) the capacity to 'rectify' naïve or hitherto unnoticed metaphors, analogies, anthropomorphisms, etc; and (3) the resultant possibility of advancing 'from an inefficient trope-concept that is poorly constructed, to an operative tropic-concept that is more refined and more powerful in a given field and at a determined phase of the scientific process' ('White Mythology', p.264).[118]

For the weak reader, expressions or ambitions of stating what reality is really like are based on a realist model. This model is so intuitive it would appear to be a prerequisite for truth or objectivity. Instead, the weak reader understands that the realist model is an equally contrived metaphysics that allows statements such as there

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[115] This is not meant in the strong Nietzchean 'all concepts are sublimated metaphors' way. The sort of claim levelled at Derrida, which invites interpretations of anti-realism or cultural relativism. Christopher Norris, *New Idols of the Cave: On the Limits of Anti-Realism* (Manchester: Manchester University Press, 1997b), p.78

[116] Kuhn elaborates on this idea in 'Metaphor in Science', in *Metaphor and Thought*, ed. by A. Ortony (Cambridge: Cambridge University Press, 1979), pp.409–19

[117] Jacques Derrida, 'The Supplement of the Copula' and 'White Mythology' in *Margins of Philosophy*, trans. by Alan Bass (Chicago: University of Chicago Press, 1982), pp.175-205 & pp.207-271

[118] Norris, *Idols of the Cave*, p.88

‘really is’ to seem like commonsense. To talk about what there ‘really is’ is to have some sense of the ‘literal’ in term-object correspondence. It is this quest to make paradigms and world literally the same thing that spurs most scientists on. In doing so, what one is trying to achieve is a paradigmatic description that is so totalizing we are unable to interpret it any other way. However, because ‘literal’ requires a certain model of truth, language and world as being already meaningful before we can apply it, we have to presuppose ‘what there is’. It would seem that the metaphor of ‘literalism’ is self-supporting. This is what enables the weak reader to say that there is no literal meaning but the ‘literal’ is meaningful.[119] As long as these sorts of terms have meaning science will ‘always [be] directed ahead toward possibilities it cannot yet fully grasp or articulate’.[120] As long as metaphors such as ‘Truth’ or ‘Reality’ are what guide practice, any attempt to prove what there ‘really’ is or how we really know something, will always throws up more problems and puzzles. Which luckily for scientists is what they do.

Normal and revolutionary sciences are ways of doing science. What is scientifically meaningful in either case is also historical. For example, grounded in a pre-scientific world, stargazing could be astrological. Those same objective actions of looking up and ‘observing’ in a post-Einsteinian world are still meaningful. The western world still supports an astrological interpretation, but as with IDC it is now dependent on modern paradigms for its meaning where new information may alter practices, give more accurate planetary orbits or demote planets to dwarf planets. Now, unlike ancient Babylon, legal disclaimers have to be given in acting on astrologer’s advice.[121] It would seem any practice that is anachronistic receives its meaning from the dominant paradigm that it appears to be challenging. The reason for Kuhn why practices like astronomy and cosmology have flourished and astrology is all but dead as a ‘science’, is that in the absence of a dominant paradigm no new normal science can be established and so it stops producing puzzles. From here there is no way of

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[119] Norris, *Deconstruction: Theory and Practice*, p.66
[120] Joseph Rouse, ‘Heidegger on Science and Naturalism’, *Division I Faculty Publication*, 36 (2005), 1-22 (p.6) <http//:wesscholar.wesleyan.edu/div1facpubs/36> [accessed 8 February 2012]
[121] The name of the Astrological Association’s (AA) research journal is, ‘Correlation: The Journal of Research into Astrology’. The name seems to be fitting in that the harder sciences deal with causation rather than correlation. A disclaimer to the AA says ‘Contributors are at liberty to express such opinions or employ or advocate such astrological techniques as they wish for readers to consider and question. They should also seek qualified professional advice before they act upon them in their lives.’ <http://www.astrologicalassociation.com/pages/articles/index.php> [accessed 19 Feb 2013]

knowing how research should proceed.[122] Astrology has a regional ontology, but it is in no way fixed or agreed upon and so multiple theories, explanations, ways of doing astrology prevail. It does not have the same chance of going into crisis as a practice that has developed a normal science. This process of stability, crisis and re-formulation of what there is ontologically, is a potential point of departure between Kuhn and Heidegger's take on science. Rouse points out, 'where Kuhn and Heidegger diverged was that Kuhn endorsed this closing off of ontological inquiry, whereas Heidegger did not'.[123] The weak reader will not be so quick to conclude this. Kuhn states that paradigms are not only constitutive of science but of nature also.[124] The fact that we always see reality 'as' something that is open to change and that change alters our worlds does not mean the 'literal' world changes. 'World change' is not located in epistemology as this is what goes into crisis. '[A] pendulum is not a falling stone, nor is oxygen dephlogisticated air'.[125] Interpretation 'can only articulate a paradigm, not correct it.'[126] Kuhn's rejection of the 'methodological stereotype of falsification' does not mean scientists do not reject theories. As he writes, the 'act of judgment that leads scientists to reject a previously accepted theory is always based upon more than a comparison of that theory with the world.'[127] Naïve falsification relies on the literalism of the 'world' metaphor. Where we take an observation and compare it to reality. Rather we only see something as a worthy observation and know what to compare it to because of the world as framed by a paradigm. As the scientists will gain a tacit understanding of how their practice relates to the world they start to exercise professional judgement about when to relax the methodological rules for doing science. Feynman described this as the ability to 'pass on the accumulated wisdom, plus the wisdom that it might not be wisdom'.[128]

One of the worries the strong reader has with Kuhn's stance on paradigm, normal science and anomaly is that, any anomaly is potentially a counter instance to the paradigm.[129] What this means for the strong reader is that any failure to meet the

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[122] Kuhn, 'The Logic of Discovery or the Psychology of Research', p.9
[123] Rouse, 'Heidegger on Science and Naturalism', p.6
[124] Kuhn, *Structure*, p.110
[125] Ibid., p.121
[126] Ibid., p.122
[127] Ibid., p.77
[128] Feynman, 'What is Science?', p.188
[129] Kuhn, *Structure*, p.82

expectations of the paradigm is another reason to doubt it. The scientist may then be at liberty to rethink the paradigm to explain why these anomalies occurred. What this can lead to is the Duhem-Quine underdetermination by evidence thesis. Where any ‘observation’ is for or against an infinite amount of competing explanations.[130] If this were the case, normal science and paradigm formation would be impossible and science would not happen. Yet what this perspective misses out, which the weak reader takes on board, is the historical and cultural aspect. For the weak reader, there are not infinite competing interpretations of ‘anomaly’, but a selection that are historically possible and meaningful. For example, Aristotle could not have posed gravitational field equations for explaining why objects fall for in his ‘non-literal’ world this option was not a meaningful possibility. The fact is we can reflect on what Aristotle did and believed because gravitational field equations are now a meaningful part of our present ‘of’ languages, where we are free to re-interpret his beliefs into a current ‘about’ language. That is, we can re-state his ‘about/of’ relationship in terms of current ‘about/of’ languages in modern science. As Rouse says, ‘philosophical preoccupation with testing and evaluating hypotheses betrays a retrospective emphasis upon the certification of knowledge. That emphasis contrasts with the prospective orientation of scientific research toward the extension of understanding.’[131] The aim of science is always forward looking, but it can only make sense of itself by where it has come from. From the present we can guess at the trajectory of technology or research, as we can see where it came from and where it could possibly go, but we do this as a part of normal science. Whilst some are firmly as a result of previous research, such as there was no hope of Aristotle or Newton posing the question of the Higgs boson, some are more esoteric and vague. The importance of historical continuity is to ask whether the puzzle meant the same thing to them? As much as the Higgs boson was not a meaningful possibility for Aristotle, so too, we find it hard, if not impossible, to imagine what the alternatives to our current best theories might be.[132]

[130] This too has strong and weak forms, from whether we take this to mean no claim is irrefutable or no claim is wholly falsified or confirmed, or given a certain schema only certain theories can be considered. Larry Laudan, ‘Grünbaum on the ‘Duhemian Argument’, in *Can Theories be Refuted? Essays on the Duhem-Quine Thesis*, ed. by Sarah G. Harding (Dordrecht: Reidel Publishing Company, 1976), pp.155-161 (pp.158-159)
[131] Rouse, ‘Kuhn’s Philosophy of Scientific Practice’, pp.7-8
[132] Stanford, *Exceeding Our Grasp*, p.22

Normal scientific anomalies remain at the level of epistemology. They are puzzles that are unsolved, but the rules for puzzle solutions remain unquestioned. Data that is unclear or vague is not a counter instance of the paradigm, but is partly an articulation of the paradigm. This is where the ability to contextualize a claim or anomaly meaningfully comes in. Collecting and recognizing evidence counter to a paradigm presupposes a deep understanding of the paradigm in question. Anomaly recognition, as opposed to counter-paradigm evidence, involves the more general skill of noticing that something somewhere is not yet adequately understood or has been dealt with in an improper manner.[133] To understand what the problem is, rather than just that there is one, is to have already gone a long way toward resolving it. Of this situation Kuhn says, 'assimilating a new sort of fact demands a more than additive adjustment of theory, and until that adjustment is completed – until the scientist has learned to see nature in a different way - the new fact is not quite a scientific fact at all.'[134] A larger more pervasive anomaly suggests not merely that this particular phenomenon is not understood, but that whatever causes it, and whichever situations it might manifest itself, are not reliably understood either. Under these circumstances, a science may enter a state of crisis, in which the intelligibility, reliability, and significance of its practices and achievements come into question.[135] Heidegger himself cites these very conditions as the basis for science.[136]

A further distinction must be made that crisis and uncertainty in the paradigm are not the same thing. Scientists frequently accommodate uncertainty into their work, because either the phenomenon they are working with is too complex or any variable that produces uncertainty will hopefully be resolved by further articulation of the paradigm. Crisis results only when scientists become uncertain about how to advance, which theories and models are reliable, which background assumptions need to be

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[133] The 'faster-than-light' neutrinos 'detected' at the INFN-Gran Sasso laboratory in Italy (2011) would be a good example of this type of anomaly spotting as opposed to counter-instances to the paradigm of relativity.
[134] Kuhn, *Structure*, p.53
[135] Fuller has argued that when Kuhn moves from 'normal science' to 'revolutionary science' he also moves from nineteenth century 'positivistic' form of reason to an 'eighteenth century 'Enlightenment' form of reason by which science is conducted. Fuller, 'Being There', p.251. Ginev calls the making explicit of implicit assumptions that occur in times of scientific crisis 'hidden normativity'. Ginev, 'Re-Reading Normal Science', p.85
[136] Heidegger, *Being and Time*, pp.7-8

checked or indeed what the problem is?[137] Take for example the debate between 'inflationary' and 'steady-state' models of the universe. Everyone could agree that certain observations had been made. What those observations meant, however, was what had to be fought over. For Hoyle, to imply that the universe had a beginning was not scientific and echoed religious ideals which he was strongly against.[138] We now know the universe had a beginning, but what this means is still contentious. Was it preceded by other 'Big Bangs' or is it part of a multiverse where each act of creation is unique? The now observed accelerated expansion of the universe forces scientists to posit things like dark matter and energy, but these are place-holder names in the absence of an explanation.[139] Taking the example of 'inflationary' and 'steady-state' universe models we might say 'crisis' is when communication 'of' the paradigm breaks down and communication 'about' the paradigm takes off. To speak as a 'steady-state' proponent, I speak and think in the language 'of' my paradigm. This includes philosophical, aesthetic, social and scientific reasons why a 'steady-state' universe is preferable to an 'inflationary' one. Perfectly within the rules of science, Hoyle invented a theoretical field that could create matter in line with the laws of energy conservation (C-Fields), this model being called quasi steady-state (QSS) cosmology.[140] He would thus talk in terms of C-Fields which were not accepted as legitimate phenomena by opposing cosmologists. He died never changing his views. Those however that were 'converts' moved from talking and thinking in the language *of* their paradigm to reflecting, thinking and talking *about* the paradigm. They take up a new relationship with it, a kind of shift from first to third-person, where the regional ontology of the paradigm is now apparent to them.[141] Whilst Hoyle and his contemporaries were at odds over whether the universe was expanding or not they both could agree it was science they were doing which is 'about' making true statements about 'reality'. A difference between the weak and strong reader is that a

[137] Fuller's reading of Kuhn has 'normal science' as the most desirable state of science. Fuller, 'Being There', p.257
[138] Jane Gregory, *Fred Hoyle's Universe* (Oxford: Oxford University Press, 2005), pp.70-71
[139] As NASA say, 'Theorists still don't know what the correct explanation is, but they have given the solution a name. It is called dark energy.' 'Dark Energy, Dark Matter', *NASA Science: Astrophysics*, <http://science.nasa.gov/astrophysics/focus-areas/what-is-dark-energy/> [accessed 14 Jan 2013]
[140] Gregory, *Fred Hoyle's Universe*, p.334
[141] Michael Friedman developed a similar idea that science works with a single common tradition of cultural exchange that is not trapped to anyone Carnapian framework. This allows the new possibilities of paradigm articulation to take place before the revolution. Friedman, *Dynamics of Reason*, p.60

weak reader understands that the ‘about’ language is secondary to the ‘of’ whereas the strong reader understands the opposite.

Though we tend to think in big, bold examples, as they are easier to picture, crisis and paradigm shifts for Kuhn could be any size.[142] McMullin draws the distinction between ‘shallow’ and ‘deep revolutions’.[143] Shallow revolutions leave much of the theory and supporting concepts intact, such as Kuhn’s example of x-rays, which only applied to the use of equipment, whereas, something like space-time geometry was a deep revolution, as fundamental ideas were challenged. McMullin argues that it was in the ‘deep’ sense of paradigm change, that most philosophers of science based their critiques of Kuhn’s account of paradigm change. Kuhn himself states that revolutions in one tradition will not necessarily extend to others.[144] One of the main problems was seen to be how proponents of different paradigms could proceed with research if both were committed to fundamentally different accounts of the ‘literal’ world. How could they agree between themselves which experimental data or theoretical models to consider? What background assumptions to test, or which methodologies to use if there was no objective third position from which to assess those things. Agreement and rational discussion occur because the paradigm and our discourses are not the ‘literal’ world. Communication and discussion shifts from the language ‘of’ the paradigm to discussion ‘about’ the paradigm.

Kuhn’s distinctions between normal science, crisis, and revolution are often translated as definite episodes of the development of scientific disciplines. Instead normal and revolutionary science can be thought of as ways of doing science, rather than stages of a linear process. That would involve having a particular model of time, history and knowledge change in mind. In relation to what this means for the ‘methodological view’ of science, we are forced to question the philosophical notions of ‘Progress,’ ‘Truth,’ and ‘Reality’. If paradigms do not aim ‘towards’ anything they cannot share a common direction and so in this sense cannot be linear.[145] There is the metaphor of ‘revolution’ as something that ‘revolves’, as opposed to something that ‘revolts’,

[142] Kuhn, *Structure*, p.92
[143] Ernan McMullin, ‘Rationality and Paradigm Change in Science’, in *World Changes: Thomas Kuhn and the Nature of Science*, ed. by Paul Horwich (Cambridge, Mass.: The MIT Press, 1993), pp.55-78 (pp.59-60)
[144] Kuhn, *Structure*, p.50

where we may get discursively stuck.[146] Potentially this is another useful metaphor in how we understand whether revolutions 'progress' towards anything or not. Rather, if we understand science always has to be a 'normal science' before it can descend into crisis we are always, philosophically, starting from the same spot in scientific advances. This is what sets the potential for the next 'spin' to then ultimately return to the same stage of normal science and anomaly accumulation. Another possible problem with the 'revolution' metaphor is that we tend to think of revolutions as being sudden, violent upheavals of norms and power relationships. As Kuhn writes, 'these transitions to maturity have seldom been so sudden or so unequivocal as my necessarily schematic discussion may have implied.'[147] Indeed, the linear order of the chapters in *Structure* adds to the understanding that revolutionary progression is incremental and orderly with a kind of implied motion forward. If we use rotation as our metaphor the implied motion becomes circular, which does not intuit notions of progression but rather change or reoccurrence. Again one may be free to imply a notion of mythic time on to this schema, such that patterns of science repeat themselves throughout history. For the weak reader, however, this is to only highlight the metaphysics we bring to bear on interpreting a practice. To give 'time' any sense of teleology, be it linearity or cyclicality, is to be working with a notion of time or history already interpreted for us.

The 'invisibility' of revolutions comes by way of the various ways we can relate and re-tell history.[148] If told completely through the replacing paradigm revolutions and controversies can be forgotten altogether by what makes it into science textbooks, courses, and how philosophy of science tells its own stories.[149] For example, Feynman not only being a 'literal' visionary in the field of quantum electrodynamics (he invented diagrams with which to visualize quantum interactions) he is heralded as being the father of nano-technlogy. In two transcribed lectures by Feynman he is cited as supposedly predicting or anticipating the nanotechnology age. In the editor's

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145 Ibid., pp.160-173

146 Fuller and Collier argue that Kuhn's description of the movement of science implies a 'cyclical history' rather than 'linear progress'. Steve Fuller, and James H. Collier, *Philosophy, Rhetoric, and the End of Knowledge: A New Beginning for Science and Technology Studies* (Mahwah, NJ.: Lawrence Erlbaum Associates, 2004), p.6

147 Kuhn, *Structure*, p.21

148 Ibid., pp.136-143

149 Ibid., pp.144-159

preamble he calls Feynman the 'father of nanotechnology'[150] The second talk the editor declares Feynman a 'Nostradamus of computer science' and him being responsible for the 'current revolution in nanotechnology.'[151] A comprehensive biography of Feynman calls him the 'intellectual father' for nanotechnologists.[152] However much of the work conducted in nanotechnology does not attribute or cite Feynman as an influence before or during the 1980's. Renewed interest in his talks, however, occurred in the 1990's due to the currency the term 'nano-technology' was gaining. Toumey argues that Feynman's association with the birth of nanotechnology fifty years ahead of its time is a retroactive account that gives nanotechnology an already written history that uses the prestige of Feynman's name to give it greater legitimacy.[153]

The choice of normal or revolutionary science may occupy the history books, in that, great lengthy periods of equilibrium in science are punctuated with these violent upheavals of foundational knowledge, but the weak reader does not see this as a choice of either/ or. They can coexist. Some scientists may start with the fundamentals of their field, explicitly formulating the background postulates, and altering or removing long held principles. Others will continue with the humdrum laboratory procedures as part of larger research projects, setting and solving puzzles in the ways that are familiar. This mundane work tends to get overlooked, as it does not fit the heroic narratives so well, nor capture the public imagination as much. We like to deal in singular instances of great scientific achievements, rather than whole research communities of very ordinary and slight achievements. For example, the solving of 'Fermat's Last Theorem' can be told as the lone 6 years of work Andrew Wiles completed.[154] Equally it could be told as the collective 358 years worth of mathematical development it took for Wiles to have the tools to solve it. McMullin's has argued that many commentators of Kuhn have associated paradigm change with

[150] Richard Feynman, 'There's Plenty of Room at the Bottom', in *The Pleasure of Finding Things Out: The Best Short Works of Richard P. Feynman,* ed. by Jeffery Robbins (London: Penguin Books, 2001), pp.117-139 (p.117)
[151] Richard Feynman, 'Computing Machines in the Future', in *The Pleasure of Finding Things Out: The Best Short Works of Richard P. Feynman*, ed. by Jeffery Robbins (London: Penguin Books, 2001), pp.27-52 (p.27)
[152] James Gleick, *Genius: Richard Feynman and Modern Physics* (London: Abacas, 1994), p.14
[153] Chris Toumey, 'Reading Feynman into Nanotechnology: A Text for a New Science', *Techné,* 13 (2008), 133-168
[154] Simon Singh, *Fermat's Last Theorem* (London: Fourth Estate, 1998), p.263

large-scale theory change. Bechtel and Rheinberger argue that within biology the movement from cytology to modern cell biology came about, not due to theory change, but improvements in technology, allowing scientist to ask different questions.[155] It was the technology that allowed cell biologists to go about their work differently, to ask different questions, to explore new possibilities. This is completely within the boundaries of Kuhn's description of science as he gives four possible levels for normal scientific commitment, anomaly and crisis.[156] Again, one cannot foresee ahead of time how a new piece of technology will affect any one field, so whether a revolution goes from 'shallow' to 'deep' or vice-versa is uncertain. This uncertainty though is what was at stake in the policy issues of pure versus applied science funding. From the esoteric equation of Einstein's matter-energy equivalence and Rutherford's internal workings of the atom we have the kernel for sustainable energy, which is one of the main applied research paradigms of today's science. Neither had power stations in mind but this is where it has led us.

What is thus taken to be a 'revolution' or 'paradigm shift' is always a matter of retrospective interpretation. Whether a new discovery, theory or application amounts to a revolutionary science rather than a new expression of the paradigm is one of the things a PUoS would get people to think about. For instance, many great scientific breakthroughs occur as the unintentional result of an earlier project. So how could one know ahead of time whether a current ordinary piece of research, that fits normal science perfectly, does not have revolutionary applications elsewhere?

Philosophically, the hermeneutics someone applies to a discovery can create greater or lesser impact than others depending, again, where in time someone is viewing it from. Kuhn explicitly expresses the importance of hermeneutics to his philosophy of science, after *Structure* was written.[157] The standard mathematical-physicist's interpretation is that the special theory of relativity is a very successful attempt to work out the implications of Maxwell's electrodynamics. This was only accomplished

[155] William Bechtel, 'Integrating Science by Creating New Disciplines: The Case of Cell Biology', *Biology and Philosophy*, 8 (1993), 277-299; Hans-Jörg Rheinberger, 'Experimental Complexity in Biology: Some Epistemological and Historical Remarks', *Philosophy of Science*, 64 (1997), 245 - 254
[156] The four levels are 'conceptual, theoretical, instrumental and methodological'. Kuhn, *Structure*, p.42
[157] Kuhn, *The Essential Tension*, p.xiii

through previous work done by Poincaré and Lorentz, which again was only part of a long series of investigations into light and electromagnetism, whereas many popular accounts regard Einstein's work as a revolutionary reconstruction of classical physics in a Goliath one-man effort.[158] Using Feynman again, he shared his Nobel-Prize with two other physicists. The short biopic that introduces many of his books states that 'he all but rebuilt the theory of quantum electrodynamics.'[159] This is rephrased elsewhere as 'rebuilt the theory of quantum electrodynamics and high-energy physics.'[160] This seems to downplay the shared aspect of his achievements. Co-Nobel Prize winner Julian Swinger criticized Feynman's methods, such as Feynman diagrams, which he believed inhibited understanding of field equations. He thought they seduced students into pictographic representations of particles, rather than dealing with the paradoxes of the field equations. It was Sin-Itiro Tomonaga, along with his students, who discovered the 'renormalization' method for canceling out infinities in QED theory, the thing that Feynman is famous for.[161] Even Feynman himself recognized how the story of science is told with a kind of censored history. In his preamble to a lecture, in which he describes the theory that won him the Nobel-Prize, he gives an abridged version of scientific discoveries leading up to the problem he solved. He says,

> What I have just outlined is what I call a "physicist's history of physics," which is never correct. What I am telling you is a sort of conventionalized myth-story that the physicists tell to their students, and those students tell to their students, and it is not necessarily related to actual historical development, which I do not really know![162]

When methodological advancements are being understood in separation from their historical context, which allowed them to take place, we start to get problems with our

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[158] Ginev makes the point that even with mathematical physics the area is not disclosed by 'imposing a ready-made formalism, but through the modification and revisions of mathematical tools.' Ginev, *The Tenets of Cognitive Existentialism*, p.14. There are no idealised scientific objects that resist change because in order to idealise a scientific object it needs to meaningfully be a part of practices that allow idealisation to take place. They are not separate to it. Ibid.,

[159] Richard Feynman, *QED: The Strange Theory of Light and Matter* (London: Penguin Books, 1990), p.i; *Six Not-So-Easy-Pieces*, p.i; *The Pleasure of Finding Things Out*, p.i; *Don't You Have Time To Think?*, p.i

[160] Richard Feynman, *What Do You Care What People Think?: Further Adventures of a Curious Character* (London: Penguin Books, 2007), p.i

[161] Silvan S. Schweber, *QED And The Men Who Made It: Dyson, Feynman, Schwinger, and Tomonaga* (Princeton, NJ.: Princeton University Press, 1994), pp.365-367 & pp.268-270

interpretations. For example, one of Einstein's contributions to modern physics was to change Newtonian mass from a constant quantity to a relative quantity. As the term 'mass' is retained, but its meaning altered, we get possible ambiguities in paradigm articulation. The interpretive question then becomes whether one emphasizes the similarities and continuities or the differences and discontinuities? For the history of science can equally be told as episodes of discontinuity, which for some this is more preferable.[163] This is compatible with the weak reading as discontinuity is leveled at either epistemology or regional ontology. The strong understanding of this would be to mistake metaphorical world for paradigm, or impose a particular metaphysics of time, in that scientific ideas can be discontinuous with their own culture or history, giving them a kind of transcendental truth.[164] In reality, physicists do not struggle to talk meaningfully about Newtonian or Einsteinian masses. What would be a problem is if one could only converse in the language 'of' either paradigm. Currently in the Einsteinian paradigm we know what the Newtonian mass means and as we can never return to a period pre-Einstein it does not make sense to talk about what mass means the other way around. We can perform thought experiments given assumptions *x*, *y*, and *z* in a Newtonian world, but this mode of thought would not even be possible without the prior paradigm shift. Currently, due to the dominance of relativistic physics Newtonian problems have now become part of the 'about' language of Einsteinian physics. By this I mean Newtonian descriptions are now just regarded as rough approximation of Einstein's equations.[165]

Breakthroughs in science are not the result of dogmatically following 'falsification' or any other methodological principle but by a scientific attitude, expressed by Kuhn as the 'essential tension'.[166] Kuhn also notes that those who have contributed paradigm-

[162] Feynman, *QED*, p.6
[163] Kuhn wrote about quantum 'discontinuity'. Thomas S. Kuhn, *Black Body Theory and the Quantum Discontinuity 1894 – 1912* (Oxford: Clarendon Press, 1978); Gaston Bachelard, *The New Scientific Spirit*, trans. by A. Goldhammer (Boston, MA.: Beacon Press, 1985)
[164] Croca has argued that a principle called 'Eurhythmy' is what has been measured across history with regard to articulating a law of 'least action'. He argues that Heron in the first century measured it and as well as De Broglie in the twenty-first century. José R. Croca, 'The Principle of Eurhythmy', in *Special Science and the Unity of Science*, ed. by O. Pombo, J. M. Torres, J. Symons and S. Rahman (London/ New York: Springer, 2012), pp.19-52. This does not consider that 'observation' in Ancient Greece meant having a completely different understanding of vision and light than in modern day Europe.
[165] Kuhn, *Structure*, p.99
[166] Ibid., p.79

defining work either tend to be very young or very new to the field.[167] Being very new to the field is almost a truism, for whilst a science is still in its premature phase, it is easier to make fundamental contributions than when a science is fully mature and in its normal science phase. Jones and Weinberg note that scientific creativity is not so much dependent on age, but the level of maturity the science is at.[168] These types of studies are difficult to derive anything concrete from as they rely of inductive inferences. Who is to say how future sciences are going to be practiced or even which sciences will persist?

The 'tension' referred to by Kuhn as a prerequisite for paradigm shifts is a tension between a commitment to a tradition and the potential for the tradition to undermine itself.[169] We can restate this as our 'about' languages not being fixed by our 'of' languages. It is, however, in thinking that what our sciences are about determines how we do science or that paradigm and world are identical, thus lead the strong reader to some unwanted conclusions about the possibility for scientific change. To which I turn next.

8.7 - Incommensurability

For the strong reader squaring the idea that scientific progress is possible given that scientists can literally live in different worlds is a tricky one. It is this 'world change' that had led many to accuse Kuhn of being a relativist, subjectivist or irrationalist about science. 'Incommensurability' as an idea was not original to Kuhn, its form and use by Lewis and Feyerabend predate Kuhn's.[170] For Popper 'incommensurability' was linked to total political regimes where one could not judge the standards of

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[167] Ibid., p.89

[168] Benjamin F. Jones and Bruce A. Weinberg, 'Age Dynamics in Scientific Creativity', *Proceedings of the National Academy of Sciences of the United States of America*, Nov. 7 (2011), 1-5 <http://www.pnas.org/content/early/2011/11/03/1102895108.full.pdf+html> [accessed 16 May 2013]

[169] In Kuhn's language it is the tension between 'conservative and innovative imperatives' Kuhn, *The Essential Tension*, p.227; or the scientist's commitment to 'professional skills' and 'professional ideology'. Thomas Kuhn, 'The Function of Dogma in Scientific Research', in *Scientific Change*, ed. by A. C. Crombie, (London: Heinemann, 1963), pp.347-369 (p.369). The fact that paradigms allow normal science to function to such an esoteric level means scientists can engage with the most abstract problems which ultimately bring the paradigm into disrepute which is key to genuine innovation. Ibid., p.365

[170] Ziauddin Sardan, *Thomas Kuhn and the Science Wars* (Cambridge: Icon Books, 2000), p.32; Steffano Gattei, *Thomas Kuhn's "Linguistic Turn" and the Legacy of Logical Empiricism:*

claims. Kuhn does not help himself by using political metaphors or Orwellian remarks in his discussion.[171] Hollinger argues that the threat of incommensurability was born of the paranoia of Cold War America and the threat of totalitarian Marxism.[172] Others have argued a meta-incommensurability between terminology, philosophy and language in the development of Kuhn's own ideas due to them being borrowed ideas.[173] While some take the incommensurability of 'worlds' to be 'plainly metaphorical,' others have argued against a Whorfian style literalism in how words change what we perceive.[174] Yet 'incommensurablity's' origin is in mathematics. Gattei says that 'incommensurability' is seldom discussed in its mathematical form.[175] Most attempts to resolve 'incommensurability' either aim at semantic variance or try to show why literal 'world change' is either self-defeating or disastrous for any coherent notion of truth or realism.[176] These critiques are epistemological, linguistic or psychological. The weak reader, on the contrary, understands that any talk of 'world change' is part of the activities of scientists and how they apply their trade.[177] If there is confusion, it partly results from taking Kuhn's analogies and metaphors literally, such as religious conversion, political revolution, Gestalt switches, or visual field experiments.[178] As Kuhn himself says,

> In *The Structure of Scientific Revolutions*, particularly chap.10, I repeatedly insist that members of different scientific communities live in different worlds and that scientific revolutions change the worlds in which scientists work. I would now want to say that members of different communities are presented with different data by the same

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Incommensurability, Rationality and the Search for Truth (Aldershot: Ashgate Publishing Limited, 2008), p.73

171 Kuhn, *Structure*, pp.92-94 & p.167

172 David A. Hollinger, 'Free Enterprise and Free Inquiry: The Emergence of Laissez-Faire Communitarianism in the Ideology of Science in the United States', *New Literary History*, 21 (1990), 897-919; 'Science as a Weapon in *Kulturkämpfe* in the United States During and After World War II', *Isis*, 86 (1995), 440-454

173 Babette E. Babich, 'Kuhn's Paradigm as a Parable for the Cold War: Incommensurability and its Discontents from Fuller's Tale of Harvard to Fleck's Unsung Lvov', *Social Epistemology: A Journal of Knowledge, Culture and Policy*, 17 (2003) (2), 99 – 109 (p.99)

174 Richard Boyd, 'Constructivism, Realism, and the Philosophical Method', in *Inference, Explanation and Other Frustrations: Essays in the Philosophy of Science*, ed. by John Earman (Oxford: University of California Press, 1992), pp.131-198 (p.176); Devitt and Sterelny, *Language and Reality*, p.225

175 Gattei, *Thomas Kuhn's "Linguistic Turn"*, pp.74-75

176 Michael A. Bishop, 'Why the Semantic Incommensurability Thesis is Self-Defeating', *Philosophical Studies*, 63 (1991), 343-356

177 Kuhn, *Structure*, p.112 & p.150

178 Ibid.,

> stimuli. Notice, however, that that change does not make phrases like "a different world" inappropriate. The given world, whether everyday or scientific, is not a world of stimuli.[179]

The strong reader understands Kuhn as forwarding a 'literal' change in the world, such that we are talking about different realities. It is this understanding that is the challenge to realism, truth and the authority of science, which gives a certain interpretation of Kuhn political currency.[180] Yet, the weak reader takes this description as suggesting a change in our 'of' languages or the change of our 'non-literal' worlds. Similar to my 'of' language or 'non-literal' world, Rouse introduces the concept of 'work-world'.[181] This is to explain differences in worldviews, in that, scientists 'see' things differently in regard to the things they do. By 'work-world' Rouse means the forms of life we refer to when someone refers to the 'financial' or 'academic' world. The members will more than likely hold different beliefs, but the crucial differences are in how they act in their worlds.[182] A paradigm that is stable enough to produce a normal science or consensual normative practices makes it easier to distinguish between a good and bad practitioner. So a builder whose houses kept collapsing would be deemed a bad builder, but a conceptual artist whose job is to break conventions would challenge the idea of demarcation in art. 'World change' through the 'strong-weak' distinction is also a good example of the literal-metaphor inversion that might be at work in our understanding. What would it mean to take the term 'world change' as literal and what would the consequence of it be? For the strong reader, the literal world and paradigm are identical and so when the paradigm changes scientists are literally working in different worlds.[183] Since we live in those worlds some of us would live in *world a* and some of us in *world b*, the puzzle is then, how can we communicate with each other or produce reliable knowledge if our language and knowledge is representational of the different worlds around us? The

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[179] Kuhn, *The Essential Tension*, p.309
[180] Devitt and Sterelny, *Language and Reality*, p.253
[181] Rouse, 'Kuhn's Philosophy of Scientific Practice', p.113
[182] Dreyfus tries to make this distinction available in Wittgenstein as well as in Heidegger. He places 'world difference' in 'what people *do* and *say*, not what they *believe*.' Dreyfus, 'Holism and Hermeneutics', p.12 [Italics in original]. 'Incommensurability' for Dreyfus is resolved by what either Wittgenstein calls 'finding ones way about' or Heidegger calls 'finding a footing'. This means that we can understand another culture or theory even if we do not believe it to be true or correct because we come to learn how to share '*know-how and discriminations* rather than arriving at agreement concerning which *assumptions and beliefs* are true.' Ibid., [Italics in original]

weak reading suggests this way of talking is the metaphor. To understand the world 'literally' as a planet and how we experience that planet changes with paradigm articulation is useful for science, but it is an abstraction or metaphor based along a string of notions that are metaphysical. Rather, it is what allows such talk of 'possible worlds' or object-language relationships to be meaningful and make it seem like a genuine problem.

Instead, the weak reader understands the 'non-literal' world as changing, which refers to how people adapt and change their practices for constituting the 'literal' world. What we think our practices are 'about' is tied to our 'of' language. So the language of geo-centric astronomy is different to the language of helio-centric astronomy. This will inform what their observations are about, so one person will 'see' circular orbits and the other will 'see' elliptical orbits (this does not mean 'things' will literally change in the sky above them). The problem occurs if one thinks that our 'of' languages (historical practices) come from our 'about' languages (methodological view). Here it is easy to think that because science is always about 'reality' and making true statements that this must guide our practices. Just as Quine re-told what philosophers were doing in terms of the statements current practices hold to be meaningful, the weak reader understands that to casually use a term such as 'reality' is a statement about the practices from where those ideas come from.[184] To place an 'about' language before an 'of' is part of traditional philosophy. Such that we might ask 'what is a chair?' and list all those things chairs are about and then try and see what is common to them all. Transcendentally, we might think that they are all 'about' an ideal form of chair. Yet to start with 'chair' is to already understand the thing you are interrogating. 'Chair' comes to us through our 'of' languages of sitting down, in a culture where sitting down is significant. If we did not sit down 'chairs' would not be meaningful to us as they are now. They would not be about sitting down but something else.

If 'paradigm' and 'world' or 'about' and 'of' languages were identical, methodological incommensurability would be a real problem. People would not understand each other and the relativism of truth claims would halt the practice of

[183] Kuhn, *Structure*, p.111; p.117; p.118; p.121

science. For the weak reader 'incommensurability' is a matter of not being able to appreciate or see the point of what another person is doing, rather than saying there is no truth at all.[185] The strong version is to think that the 'of/about' are the same or that the 'about' precedes the 'of'. This means that some notion of objective reality informs how we investigate it. Here we simply 'observe' or compare the evidence. Yet, because someone else's paradigm tells them that the world is another way their science is 'about' something completely different. To have incommensurability at the level of knowledge content or indeterminacy of language translation is to make either epistemology or linguistics the focus of incommensurability.

Feyerabend took this to be the most uninteresting version of the problem as it reflects a philosopher's problem and does not address how objects are constituted in knowledge.[186] Old ideas can be re-articulated through a modern paradigm so as to either look irrational, stupid or profoundly deep and relevant. Either way, both are a rationalization of past events through a modern understanding of the world. To read Aristotle as if he were trying to accomplish the same sorts of activities as a modern physicist is to not appreciate Aristotle on his own terms. Likewise, to read Newton as if he had Einsteinian approximations in mind is to not read him fairly either. As Newtonian physics holds more-or-less true for our world and Aristotelian physics does not, we can then interpret one as being mistaken and the other as pointing towards a 'timeless' or transcendental 'Truth'. The 'Truth' that this understanding is referring to is that of our about languages, in that we can objectively say, planets travel in ellipses or that matter and energy are equivalent. Incommensurability comes from the belief that our tacit understanding of 'of' languages is derived from the objective sphere of 'about' languages. The weak reader understands the reverse to be true. In order to have an objective notion of what there 'really' is has to come from practices that make sense of those ways of talking and acting.

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[184] Quine, *Theories and Things*, p.191

[185] Dolby writes, 'If we took the problem to its logical extreme, none of us could ever precisely understand the utterances of another, because each of us has had a distinct personal history. That is an unhelpful view.' Dolby, *Uncertain Knowledge*, p.145. Fuller elaborates on this position by saying that '*the very practice of arguing* will have made one accustomed to the other's position.' But in order for two people to disagree or argue they must share something in common over which they dispute. Fuller, 'Being There', p.253 [Italics in original] The dispute itself has to make sense. Wittgenstein said, 'it is what human beings *say* that is true and false; and they agree in the *language* they use. This is not agreement in opinions but in form of life.' Wittgenstein, *Philosophical Investigations*, para.241

It is said Newton gave the first expression of the theory of relativity as a corollary of his second law, but the significance this has for modern physics as opposed to eighteenth century scientists is that the possible meanings it had for them was limited by their ‘non-literal’ world.[187] It would not have been news to seamen or train passengers that unless one looked at a fixed point outside the ship/train one could not tell if they were moving (as long as it was a uniform straight line). Yet this only became significant when investigations into the phenomena of electricity, electromagnetism and light picked up and the assumptions of Newtonian mechanics started producing ‘nonsensical’ answers, such as the earth’s rotation as having zero velocity.[188] Feynman makes a similar point about why philosophers in Newton’s time were not making a fuss about such claims, unlike ‘cocktail-party philosophers’ who argue all is relative.[189] His answers is that those Newtonian claims were not significantly meaningful, because until Maxwell had developed his equations it was not thought that one could measure velocity without reference to some stationary ‘outside’ object.[190] The ‘work-world’ of Newton did not take issue with mass invariance or relative motion, rather it is the later developments that then reinterpret Newton’s work-world in terms of more modern problems that make these observations significant. The methodological habit though is to represent such claims as being ‘about’ the same thing, ‘reality’. If, however, one does not do the philosophical work, as with the strong reader, we find seemingly relativistic or anti-realist claims in Kuhn. This task is even easier given the ‘received view’ of the radicals such as Feyerabend or Heidegger. As Heidegger writes,

> [We cannot] say that the Galilean doctrine of freely falling bodies is true and that Aristotle’s teachings, that light bodies strive upward, is false; for the Greek understanding of the essence of body and place and of the relation between the two rests upon a different interpretation of entities and hence conditions a correspondingly different kind of seeing and questioning of natural events. No one would presume to

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[186] Paul Feyerabend, *Conquest of Abundance: A Tale of Abstraction Versus the Richness of Being* (Chicago: The University of Chicago Press, 1999a), p.268

[187] Feynman, *Six Not-So-Easy Pieces*, p.50

[188] Ibid., p.53

[189] Ibid., p.73

> maintain that Shakespeare's poetry is more advanced than that of Aeschylus. It is still more impossible to say that the modern understanding of whatever is, is more correct than the Greeks.[191]

The weak reader understands that both Italians and the Greeks saw the same objective 'fact' (a falling object) but that 'fact' had different significance within their non-literal worldviews. Worldviews ('of' language) cannot be judged by merely looking at the literal-world and its contents, as what the literal-world ('about') is is determined in advance.[192]

The distinction I have been making so far through the methodological/ historical or 'about'/'of' languages, I take to be similar to a distinction made by Michael Friedman. He argues one should give due significance to the parallel developments in philosophy of science alongside the advancements in scientific knowledge. One can remove any sense of incommensurability between the Newtonian and Einsteinian paradigms if one considers not only the 'about' debates but also the 'of' languages that made those debates meaningful.[193] He develops this idea in *Dynamics of Reason* where he says there are two levels of language in scientific progress, the first level is internal to science, a part of the process of normal science and which constitute the rules of a given linguistic framework within which scientists proceed. The second is external to science and come to the fore when normal science undergoes a revolution and the previously agreed rules for its linguistic framework come under question. Here we are involved in the process of replacing one linguistic system with another, and it is this activity that is not wholly based within the methodological view. Friedman argues that this second language that supports the transition of paradigms and helps to establish them, is of a mainly philosophical nature. Thus, the mechanical natural philosophy of the seventeenth century was not based upon its empirical success or mathematical prowess 'but by the inspiring philosophical vision of a radically new approach to the understanding of nature self-consciously crafted by

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[190] Ibid., p.75

[191] Martin Heidegger, 'The Age of the World Picture', in *The Question Concerning Technology and Other Essays*, trans. by William Lovitt (New York: Harper and Row, 1977), pp.115-154 (p.117)

[192] Ibid., pp.266-267

[193] Michael Friedman, 'History and Philosophy of Science in a New Key', *Isis*, 99 (2008), 125-134 (p.129)

Descartes and Galileo against the backdrop of medieval Scholasticism.'[194] For Einstein's theory of special relativity was mathematically and empirically equivalent to the Lorenz-Fitzgerald theory of aether, yet fundamentally different in that one preserves the classical Euclidean space-time geometry and aether and the other does not.[195] Friedman argues that Einstein's main contribution was a conceptual one, in that, what space-time geometry we chose to describe an event with is of our own free choice and neither nature nor our minds forces us to chose. Yet, this only became a possibility because of nineteenth century debates between Kantianism and empiricism in the philosophy of geometry.[196]

If, however, we ignore the historical context, and deal solely in an 'about' language we can look at causal theories of reference. These are a derivative of practical normative uses of language in that they locate common causes.[197] Understood this way language becomes an 'interaction with the world rather than as a formal structure of meanings connected to the world only indirectly.'[198] From the strong position, it seems almost impossible how something like science could even get started considering the methodological problems with induction, scepticism or incommensurability. The weak reader does not dismiss methodological problems but recognizes them as problems of philosophy in the guise of scientific prescriptions. As Rouse writes, 'the greatest danger in science was not error, which is more readily correctable by further inquiry, but the emptiness of assertions closed off from genuine accountability to entities.'[199] If we place the activities of our 'of' languages as belonging to what our descriptive languages are 'about' something like radical meaning variance (RMV) can become a real issue.

A general observation and criticism of the strong reading is why do we not get the sorts of incommensurability the strong reading proposes? [200] Why can we

[194] Friedman, *The Dynamics of Reason*, p.23
[195] Michael Friedman, *Reconsidering Logical Positivism* (Cambridge: Cambridge University Press, 1999), p.22
[196] Ibid., p.6
[197] Rouse, 'Heidegger on Science and Naturalism', p.9
[198] Ibid.,
[199] Ibid.,
[200] Kuhn points out that meaning change or term variance 'names a problem rather than an isolable phenomenon'. Kuhn, *The Essential Tension*, p. xxii. This suggests that incommensurability is more of a philosophical problem than an empirical or scientific one. Pinch argues that those who work within the

communicate across paradigms or convert to different worldviews? I argue it was Kuhn's 'linguistic turn' in focusing on lexicon content and language learning that has made 'incommensurability' in its strong guise more natural in reading Kuhn.[201] The problems that Kordig and Newton-Smith addressed in RMV hinge around the meaning of terms and how they change. The strong account of Kuhn's incommensurability may arise by making all knowledge and understanding a problem about epistemological. Where understanding another person is just a question of theoretical knowledge.[202] The linguistic and psychological 'turns' of Kuhn, as read by philosophers such as Rorty, Quine, or Bird, also make a strong reading easier to locate. One of the problems with the linguistic interpretation is that we then can be drawn on philosophical issues such as 'indeterminacy' versus 'non-determinacy' of language.[203] A stronger result still of placing incommensurability at the level of language is that we can make all truth claims the result of formal expressions of grammatical and syntactic structures, which can be analyzed as just another 'kind of writing'.[204] Here we are forced to look for explanations of meaning variance, how we know what people mean, and how this relates to an external reality. At the level of regional ontology with which these questions deal, we can get into further problems by asking about whether objects, that were once believed to exist, now suddenly do not.[205] Was 'phlogiston' true as long as it was maintained within the dominant theories and beliefs? The problem is that if meanings are not fixed to objects then both can change with no necessary connection or dependence on one another. The

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'conservative interpretation' of Kuhn are 'reluctant to discuss the incommensurability problem at all' and instead take up a 'laisser-faire realism'. Trevor Pinch, 'Kuhn – The Conservative and Radical Interpretations: Are Some Mertonians 'Kuhnians' and Some Kuhnians 'Mertonians'?' *Social Studies of Science*, 27 (1997), 465-482 (p.477)

[201] In the postscript to *Structure* Kuhn describes incommensurability as a break down in communication due in part to different neural programming and different linguistic traditions. Ibid., p.201. In *World Changes* Kuhn regrets drawing the comparison with language translation and instead opts for language learning. Kuhn, 'Afterwords', p.324

[202] Carl R. Kordig, 'Feyerabend and Radical Meaning Variance', *Noûs*, 4 (1970), 399-404; William Newton-Smith, *The Rationality of Science* (London: Routledge and Kegan Paul, 1981), pp.148-182. Dreyfus calls this position 'theoretical holism' and is to be contrasted to his view of Heidegger's position called 'practical holism'. Dreyfus, 'Holism and Hermeneutics', p.7

[203] Roth argues that even philosophers sympathetic with Quine have misunderstood his position, such as Føllesdal and Davidson. Paul A. Roth, 'Paradox and Indeterminacy', *The Journal of Philosophy*, 75 (1978), 347-367 (p.347); Dreyfus blames the prominence of indeterminacy in hermeneutic phenomenology on Gadamer where indeterminacy and non-determinacy come to mean the same thing. Dreyfus, 'Holism and Hermeneutics', p.12

[204] Richard Rorty, 'Philosophy as a Kind of Writing: An Essay on Derrida', *New Literary History*, 10 (1978), 141- 160 (p.143)

mistake is to think about meanings as if they were 'facts' as if it is something you can be right or wrong about. As if 'meanings' were some how in the world connected to objects.

As most would want to defend science from irrationalism or relativism the strong reader tries to make sense of this situation. This forces someone like Putnam to posit the 'No Miracles' argument or when Quine writes, 'science is itself a continuation of commonsense […] the scientist is indistinguishable from the common man in his sense of evidence, except that the scientist is more careful.' [206] It is this kind of liberal notion of 'commonsense' that I would want philosophy to complicate in a PUoS. For appealing to commonsense tells us nothing, as 'commonsense' is what limits our ability to interpret phenomena in both the real and abstract. Not only did every scientist and natural philosopher use their commonsense to infer or deduce what was true or reasonable in their time, but what phenomena that now exist, such as electrons, are completely counter to our notions of commonsense. Other ideas that appear commonsense to us are 'truth' and 'reality'. Not only is there an intellectual payoff in understanding how terms like 'true' or 'real' are not encapsulated within scientific methodology, but that there is an ethical, political element to understanding how these terms can be appropriated by particular 'interest groups'. For it is commonsense that we do not 'see' evolution happening, but in what sense would it matter if we could for we always see the world 'as' something. It is in understanding how we ground such claims that we can find a source of resistance, even if one were not scientifically trained.

To sum up, the weak reader's 'incommensurability' is not a product of epistemology or linguistics but how we act in the world. To make incommensurability a product of knowledge or language is to, either make us prisoners of our languages, or realize the absurdity of the claim and make philosophy as a practice redundant. Another result of

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[205] Sankey argues for a progression of relativism in Kuhn from conceptual and rationality relativism to ontological relativism. Howard Sankey, 'Kuhn's Ontological Relativism', *Science and Education*, 9 (2000a), 59-75 (pp.59-60)

[206] William van Orman Quine, *The Ways of Paradox and Other Essays* (Cambridge: Harvard University Press, 1976), p.233. This is not an uncommon description of science but it ignores that fact that 'evidence' is always evidence 'for' something. If we depend on a simplistic notion of evidence as a methodological principle we are back with Popperian falsification but what contextualises something as evidence is paradigm dependent. In the absence of a paradigm there is no 'evidence'.

the strong reading of incommensurability is the sort of metaphysics one is using to support such a claim. This is tricky enough when it is conducted at the level of observable claims, such as falling objects, but when we discuss the provisionality of terms ascribed to unobservable entities, the problem escalates. It would appear that if one is for the incommensurability of objects, in what our languages are ‘about’, then one is against realism. Similarly, one must also be against a correspondence theory of truth, which leaves science wide open to attack. For the sake of space I have decided not to develop a strong/weak analysis of the possibility of realism/ anti-realism in Kuhn and what the alternatives might be. Needless to say, due to ‘realism’ being the commonsense, default position, it is only through self-reflection of the process of writing this thesis that this assumed commonsense may not necessarily be a good thing. For it is by positing a pre-critical idea that we can elide over any subtle difference between a methodological and historical point of view, which ultimately may be the source of our problems and not in trying to give proof of realism over anti-realism.

Chapter 9

Conclusion

9.1 – Concluding Remarks

Part of the reasoning behind producing a strong and weak version of the same text is to show that what one understands philosophy to be, is itself based on a prior understanding of how one comes to philosophy. This may range from a firm belief that philosophy of science contributes nothing and only further obfuscates matters to trying to uncover the meta-conditions by which we call one thing a science and another not. My position is that philosophy is neither of these, though it can be about them. I do not want to fall into the same trap of trying to define what philosophy is, but I think it should be understandable, practical and do this by problematizing the 'everyday'. It should not be an extension of commonsense, but should take such ideas as the basis for generating discussion and debate, for there is a lot of political weight in terms such as 'commonsense', 'truth', and 'real'. Philosophy, as I see it in PUoS, would not just be limited to thinking about science, but would be a skill applicable to any area where we are asked to think about something. I have argued that this skill can be misunderstood by a number of means. The question may not make sense, it may be poorly practiced, or certain 'interest groups' have a stake in limiting philosophy's scope to either a dominant interpretation of texts or even philosophy itself. From this, we can then either blame philosophy as a practice for creating such problems, or limit its use by way of recycled interpretations or as Weinberg suggests to help define terms such as 'true' or 'real'.[1]

I doubt the average member of the public struggles to use these terms. What is more interesting is finding out why we do not. What I have tried to show with my strong-weak reading is that on a surface reading of Kuhn, which is supported by the surrounding literature, we can derive a Kuhn that is relativist, subjectivist, irrationalist or anti-science. For some this is just what Kuhn is and has become the dominant reading of him. Others less philosophically trained will follow in the wake of such readings and assimilate this as the meaning of the text. This I take to be equivalent to unproblematically using words like ‘true’ and ‘real’. The weak reading was designed to challenge such a ‘commonsense’ understanding and in doing so open up ‘philosophy’ and ‘science’ to a more productive dialogue, in relation to public understanding. This tradition of taking opposing views, such as understanding the generative forces of the ‘strong’ and ‘weak’ readings, can really only achieve an effect by taking on board those ‘same professionally motivated conflict of interests’ that motivate debates such as the ‘science wars’.[2] Here my project echoes those such as Koyré’s, in attempting to present a way forward outside of those benign exchanges between philosophy and science.

Whether one agrees or not with my method, analysis or interpretation, is part of the point, because to do so is to be thinking philosophically, but to be aware of this is as important as to be doing it. The lack of awareness of what makes interpretations possible is something I am equally guilty of. Prior to my reading of the ‘Continental’ and post-positivistic philosophers of science I was firmly in the strong camp understanding Kuhn as a relativist. It was the realization that I was thinking about an historical question in a methodological way that was the inspiration behind this thesis.

Another difficult conceptual point that I take to be a decent example of the themes addressed in this section is the metaphor-literal inversion. Firstly, as someone who came from the strong camp, I think in retrospect, if I had read such a claim elsewhere I would have taken this as another reason why philosophy is useless. How can something not be literally true or not the case? I was not only troubled by this fact, but I problematized this proposition myself, where I started to find myself agreeing with philosophers I had been told were bad, obtuse, or hard-to-read. It was in reading Heidegger and seeing a similarity with Kuhn or a way to understand him that seemed to be more in the spirit of philosophy than writing another thesis on what Heidegger or Kuhn ‘meant’. Another philosopher I struggled with as a member of the strong camp was Derrida, yet I found myself not only agreeing with his analysis of the

[1] Steven Weinberg, ‘Peace at Last?’, in *The One Culture? A Conversation about Science*, ed. by Jay Labinger and Harry Collins (Chicago: University of Chicago Press, 2001), pp. 238 – 242 (p.240)

metaphor-truth inversion, but also discovered that my thinking was Derridean in challenging the received interpretation of a text. My main source of ammunition for challenging the methodological interpretation of science is the history of science, which also suffers from the same problem. It is in trying to get out of the habit of looking for the 'correct' account that philosophy teaches its lessons, but it also has to be understood that not just any account will do. So take the commonly understood purveyor of the 'anything goes' ethic, Paul Feyerbend. It was he who emphasized the role rhetoric played in the historical development of science. His analysis of Galileo's use of rhetoric to hide the gaps in his argument implies, however, that Feyerabend aligns himself closer to the positivists than he is normally portrayed in the literature. The inquisitors were the ones who used methodical analytical criticisms and it was Galileo who fudged the issue. However, in the end, Galileo seems to come into agreement more with modern physics than the Church. So the 'Feyerabendian moral, then, is that the positivists mistake rhetoric for method, but method doesn't win, either: *reality* does – and that's something that transcends both crafty rhetoric *and* rigorous method.'[3] There does have to be a certain type of rigor to philosophical analysis, but such that we can understand a subject not by what it is, but by what it means. It is for this reason that I think that a question like the PoD can never succeed on purely methodological grounds. We have to answer the question what does it mean to be scientific before we get to the question what is science?

What I have called the strong and weak readings can be also be thought about as the difference between the methodological and historical stance. I have argued that these are two possible ways people could interpret a text and come away with two very different understandings. One shows Kuhn to be a relativist, subjectivist, irrationalist about science, the upshot of which makes scientists an insular, ad-hoc modifying community who are not interested in the correctness of ideas or describing reality, but in protecting their theories from falsification. This would appear to clash with our everyday notion of science as experienced through technology or modern medicine and so would feel justified in ignoring any discipline where this is considered a sensible option. Likewise, if our aim is to undermine scientific authority we can use

[2] Christopher Norris, *Epistemology: Key Concepts in Philosophy* (London & New York: Continuum, 2005), p.82

the academic weight of Kuhn and *Structure* to 'show' that science is a paradigm relative practice and therefore the knowledge it elicits is relative too. 'Interest groups' that may wish to bring about scepticism over global warming, vaccination or evolution can do so by appealing to the strong Kuhn. When we compare philosophy to science the picture looks even bleaker. Philosophy is stuck on the same questions, making no progress, whereas science has constructed Internet forums for these ivory tower debates to be played out in. There appears to be almost an absurdity to discussing realism or truth, in that, we have to presuppose what it is we are going to be sceptical over. A debate about truth or the existence of an external reality would make no sense unless we already in some way tacitly understood what those terms meant. Just as there is bad science there is also bad philosophy. I am not advocating that any philosophical tradition is better than none, but where philosophy appears to be absent we are at liberty to do the most work, for this is where the tyranny of thought creeps in.[4] As previously stated, I am not so much concerned for the activity of science as that takes care of itself, but how we think about science. The problem I originally started with is what I take to be central to a PUoS, 'what is science?' The implication of this question is that we can also tell what science is not. Depending on how we understand this question, whether it is a matter of methodology or history, comes from a prior understanding, which either exposes or hides its philosophical content. I tried to parallel this with two readings of Kuhn and in doing so, illustrate a type of methodology.

To demonstrate what I meant by how the philosophical component of a text either exposes or hides itself, I used the central concepts found in *Structure*. For example, 'incommensurability' for the strong reader will either highlight epistemology, linguistics or regional ontology as being the source of incommensurability. For them, the 'literal' world and 'paradigm' are the same thing and the difficulty then becomes how do we understand scientific advances, if paradigm shifts literally change the world we live in? This, coupled with the apparent denial of scientific progression, can

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[3] Steve Fuller, 'Being There with Thomas Kuhn: A Parable for Postmodern Times', *History & Theory*, 31(3) (1992), 241 – 275 (p.256)

[4] Feyerabend uses the expression 'tyranny of truth' and 'tyranny of science' to the same ends. I simply mean that we forget we always experience the world 'as' something and never just as pure reality. Philosophy here is looking at the basis of what makes our interpretations possible and what is at risk if we forget it. Paul Feyerabend, *Farewell to Reason* (London: Verso, 1987), p.247; *The Tyranny of Science*, ed. by Eric Oberheim (London: Polity, 2011)

lead to either a rather bleak outlook for science as an epistemic authority, or we passover Kuhn as an episode in the philosophy of science as another reason why we do not consider philosophy as a useful enterprise.[5] Again, it is the strong reader who thinks that if we are denying the metaphysical possibility of attaining ultimate 'Truth' we are also denying 'truth', or that all interpretations become equal. What we might want to say is that 'truth' is a by-product of activities, which once organized meaningfully, we can then abstract to some philosophical notion of 'Truth'. Weinberg commenting on Kuhn's remark about the difficulty in stating a phrase such as 'closer and closer to the truth' says, 'all this is woodworm to scientists like myself, who think the task of science is to bring us closer and closer to objective truth.'[6] I too wish to defend science, but this dismissal is too easy and ultimately not in keeping with the scientific ethos. Rather, very good arguments can be made why such statements are problematic, which do not just apply to science but any discipline or pseudo-science wishing to fill its shoes. It is in those periods of revolutionary science when the paradigm goes from 'of' to the 'about', that the metaphysics of 'truth' can come up for argument, which was not uncommon to the likes of Bohr, Einstein and Heisenberg. 'Truth' as a metaphysical idea, however, is not a requirement for doing science, since we do not know how or what reality is, or if it means anything at all. 'Truth' might be a functional ideal that enables scientists to practice, but as Feynman said we have to balance this with an understanding that we might be wrong. Here, again, we may feel inclined to let the methodological incarnation of 'Truth' trump the historical notions of 'truth', but alas we have to consider the direction of influence. If discourse and reality mapped perfectly we could have a sense of 'Truth', but as Wittgenstein says, it is only facts and statements that can be true or false not the world itself as it is not a thing or object.[7] We always view the world 'as' something, as a collection of certain 'facts' or 'states of affairs', which come to us as an understanding predetermined by the world. Heidegger, like Kuhn, argues that the views of Aristotle and Galileo cannot be compared point for point, which is the

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[5] Benson and Stangroom raise the ambiguities in interpretation and possible outcomes of reading Kuhn. 'Kuhn's ideas, whatever his intent, and even he came to regret their original formulation, did just come to be associated with various kinds of anti-science radicalism' Ophelia Benson and Jeremy Stangroom, *Why Truth Matters* (London: Continuum, 2006), pp.39-40

[6]Steve Weinberg, 'Physics and History', in *The One Culture? A Conversation about Science*, ed. by Jay A. Labinger and Harry Collins (Chicago: The Chicago University Press, 2001), pp. 116 - 127 (p.124)

[7] Ludwig Wittgenstein, *Tractatus Logico-Philosophicus*, trans. by D. F. Pears and B. F. McGuinness, London: Routledge Classics, 2001), p.5 (para.1.1)

mathematical meaning of incommensurability. If one accepts that Aristotle and Galileo inhabited different worlds in the sense that their 'of' languages were different then this allows for different possibilities for interpretation. If, however, we think that they both occupied the same 'literal world' where their 'about' language was aimed at the same reality we can assess Aristotle as wrong and Galileo as more correct.

When we read the great natural philosophers, such as Plato, Bacon, Newton or Kant there is a distinct modern feel to their problems and ways of talking about them. The strong reader wishes to say this is because they think like us, whereas the weak reader wants to say, it is because of them we think like we do. It is through the methodological stance that Newton comes to us first and foremost as a physicist. It is the historical stance that also raises the importance of theology, alchemy and philosophy in his thinking. The stuff Newton did that has relevance to our modern world, such as his work on optics or laws of motion, get distilled from the theological and philosophical reasons for why he came to the conclusions he did. This seems to be most critical in science education where we do not hear of Newton the philosopher, theologian or alchemist, or if we do it is passed off as non-scientific. If we go back to when science and philosophy were both covered by the term 'natural philosophy', there was no distinction between what Leibniz, Descartes or Newton were doing, it was at the same time science, philosophy and theology. Hadot goes back even further by making a distinction between modern and ancient philosophy. His distinction seems to complements my 'about/ of' languages. He says that modern philosophy is 'first and foremost a discourse'. It is about discourse development and exegesis.[8] Ancient philosophy on the other hand is, 'a way *of* life', 'form *of* life', 'the art *of* living', 'an exercised practised'.[9] It seems to be the aim of the methodological understanding to keep the 'about' language as primary, where as the historical view is concerned with the conditions for bringing such a view about. Tracking the history of philosophy, we see from the end of the eighteenth century onwards philosophy became the preserve of the university, which gave rise to the professional philosopher. There were exceptional cases where philosophy grew outside of the University, such as with Schopenhauer and Nietzsche, but in the main, Universities remained places of

[8] Pierre Hadot, *Philosophy as a Way of Life: Spiritual Exercise from Socrates to Foucault* (Oxford: Blackwell Publishing Ltd, 1995), p.271
[9] Ibid., p.271; p.272; p.273 [My emphasis]

instruction for Kantianism or Hegelianism.[10] Schopenhauer, as well as Hadot, points out the dangers of Universities being thought of as having ownership over philosophical discourse and practice.[11]

It was in the early twentieth century that science and professional philosophy became distinct. The logical positivists, who were made up of scientists, philosophers and mathematicians, seeing the success of Einstein and the new physics, wanted to unify the sciences in a uniquely philosophical task, by looking at what was special about scientific knowledge. The blurring of what was pertinent to philosophy or to science can be seen in the responses to Carnap's original dissertation proposal where Max Wien believed the topic to be too philosophical and Bruno Bauch thought it to be pure physics.[12] Science and philosophy, however, have never been totally separated. The re-telling of 'controversy' as part of the normal scientific method, rather than any deep unresolved philosophical issues, can be seen in episodes of history. For example, when potentially unnerving claims were being made about the compatibility of causal determinism with quantum indeterminacy, this did not move many scientists to ponder the metaphysical implications behind freewill. The Kuhnian explanation for this would be that such questions are too fundamental and so remain philosophical, as opposed to workable, in a normal scientific arena. In order for neat mathematical formalisms to be made one has to remain agnostic on the deeper implications of what one is doing. At the time of the logical positivists, biology was being dismissed as an immature science and unable to achieve the unity of physics due to fundamental disputes between 'mechanism' and 'vitalism'.[13] If we accept that science is comprised of normal and revolutionary science, we can see that normal science has no need of philosophical questions whereas revolutionary science is over-run with them.

The importance of philosophy to thinking about science as a socio-cultural product is an ethical, as well as political concern. Where the effects of a poor economy or the marketization of education have pushed people to equate knowledge with 'capital', it

[10] Ibid., p.271
[11] Ibid.,
[12] Mauro Murzi, "Rudolf Carnap (1891-1970)", *Internet Encyclopaedia of Philosophy*, 12 Apr. 2001, <http://www.iep.utm.edu/carnap/>[accessed 12 Jan 2013]
[13] G. E. Allen, 'Mechanism, Vitalism and Organicism in Late Nineteenth and Twentieth-Century Biology: The Importance of Historical Context', *Studies in History and Philosophy of Biological and Biomedical Sciences*, 36 (2005), 261-283

appears natural that this should fit in with thinking about science. This ‘about’ domain though is what philosophy does, and further still, it tries to problematize it by asking for the conditions by which such thinking can come about. However, if we do not want people to think ‘about’ thinking we can wheel out a certain representation of Kuhn or Feynman in order to stigmatize any such attempt.

Yet, as history shows, even scientists cannot escape their own culture, where language and ideas are inherited. It is by looking at the episodes in history, where the practiced notion of science seems to grate with the methodological ideal. Where trained scientists have entertained the notion of ‘Aryan physics,’ ‘lysenkoism,’ or ‘Creationist science’ as geuine possibilities. Here it seems to be more than just a matter of looking at the evidence. Yet how one understands this is open to abuse by forwarding one interpretation as ‘commonsense’ over another. This was my aim with Kuhn, in producing two contradictory readings of the same text. The ‘strong’ reading produces problems from which a certain way of thinking gives us an ‘out’. Here relativism, scepticism, irrationalism or the uselessness of philosophy can be easy to accept, if it is only the ‘strong’ interpretation that is made known to us. It was the ‘weak’ reading and philosophy that act as a kind of therapy or exercise that does not seek to solve these problems, but show us how they come to exist by virtue of our understanding. It is when philosophy appears useless or absent that ‘thought’ is in the greatest danger. To understand philosophy as only an academic pursuit is to create a gap in public discourse in which a market of competing ideologies will eagerly take its place.

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