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THE FERTILITY OF THEORIES

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Signature

Date

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Abstract

In addition to empirical adequacy and compatibility with other current theories, scientific theories are commonly judged on three criteria – simplicity, elegance, and fertility. Fertility has received comparatively little attention in the philosophical literature.

A definition of a certain sort of fertility, called P-fertility, proposed by Ernan McMullin, is that it consists in the capacity of a theory to be successfully modified over time to explain new experimental data or theoretical insights. McMullin made the major claim that he has a novel and perhaps the sole argument for Scientific Realism. His argument involves two strands (i) theories must be considered diachronically and it is an historical fact that long standing successful scientific theories are P-fertile, and (ii) the correct explanation of this fact is that these theories reflect the realities of a mind-independent world.

A rebuttal of McMullin's position given in the literature is considered and rejected. His argument therefore requires further consideration. The plausible first strand of McMullin's argument is accepted for the purposes of discussion, and thus the observation requires explanation, either along McMullin's own lines or otherwise.

The concept of diachronicity and the implications of accepting a diachronic view of scientific theories are considered. The identity of theory across time can be understood both from a Realist and an Anti-realist perspective via the concept of significant claims in the successive versions of the long standing successful theories. This defuses a possible objection to McMullin's argument, namely that by assuming diachronicity he begs the question against the Anti-realist. Explanations of the conjunction of success and P-fertility are examined from the perspective of Scientific Realism and the major current Anti-realist stances – Entity Realism, Structural Realism, Instrumentalism, and Internal Realism.

To justify the second strand of McMullin's argument, a notion of the approximate truth or of the verisimilitude of theories is required. *Inter alia* it is argued that a distinction must be made between the approximate truth of a scientific theory and that of a simple assertion or a simple narrative. The concepts of the approximate truth of scientific theories and their verisimilitude are explored and some serious difficulties are identified. First, it is difficult to accommodate differences in respect as well as in degree in delineating the nature of an approximately true theory. Second, it is difficult to give a satisfactory account of the metric used to assess the verisimilitude of theories. It is argued that in any case no version of these concepts can adequately support the second strand of McMullin's thesis. This is because, at best, approximate truth and verisimilitude can only support a pragmatic claim – the improved empirical adequacy of successive versions of the long standing theory. In contrast, McMullin's thesis requires that successor versions generally are better theories. Third, there is an intractable theory dependent weighting problem posed by the open ended nature of scientific theories in contrast with the closed narratives describing idealized models.

The role of the approximate truth of scientific theories is explored, within the frameworks of Realism and Anti-realism, with regard to the possible responses to the existence of two highly successful, well corroborated, but incompatible theories – general relativity and quantum mechanics. It is suggested that Scientific Realism itself, not only McMullin's argument for Scientific Realism, requires the notion of approximate truth or verisimilitude of theories.

Putnam's Internal Realism is considered, and, if as I suggest, no adequate account of the concepts of the approximate truth or verisimilitude of scientific theories can be given, Internal Realism (which need not draw on these concepts because of its denial that there is a unique correct description of the world) is more plausible than the full blooded Scientific Realism advocated by McMullin, despite granting the claim of the historical observation of the conjunction between long standing successful theories and their P-fertility.

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CHAPTER 1 THE FERTILITY OF THEORIES

It is a question of fact, whether the perceptions of the senses be produced by external objects, resembling them: How shall this question be determined? By experience surely; as all other questions of a like nature. But here experience is, and must be entirely silent. The mind has never anything present to it but the perceptions, and cannot possibly reach any experience of their connection with objects. The supposition of such a connection is, therefore, without any foundation in reasoning.

David Hume¹

What, in brief, does science tell us about the world? This question has been a crucial one for philosophers since the time of Hume, who was the first to defend a phenomenalist ontology which would deny an intelligible structure to nature, and therefore by implication refuse any sort of realist view of science

Ernan Mc Mullin²

1.1 Introduction

The debate between Scientific Realism and Scientific Anti-realism is partly about the reality of the unobservable entities³ postulated by scientific theories. Aspects of the debate are also about the truth or approximate truth of scientific theories. On the one hand contemporary science has enjoyed remarkable success, both in making predictions and in giving rise to major technological advances. On the

¹ Hume, D. 1999 *An Enquiry Concerning Human Understanding* T.L. Beauchamp (Ed) Oxford: Oxford University Press 12.12.

² McMullin E. 1970 'The History and Philosophy of Science: A Taxonomy' *Minnesota Studies in the Philosophy of Science* R. Stuewer (Ed) Volume 5 Minneapolis: University of Minnesota Press p. 45.

³ It should not be thought that these are merely huge populations of a small number of esoteric fundamental building blocks like electrons or quarks. The vast majority of the Earth's living creatures are bacteria or other microscopic organisms. Not merely by number, but also by mass, these creatures outweigh the sum of visible creatures. The clear ocean, for example, is a vast diverse rich soup of 'unobservable' living forms and there is a huge mass of thermophilic bacteria in the earth's crust. Moreover, there are many other unobservables of a macroscopic kind like dinosaurs. In addition there are unobservables that have left no known trace, like the common ancestor of ourselves and the elephant.

other hand the current scientific world view, most notably in physics, threatens cherished notions of causality and contains serious internal contradictions.

Van Fraassen⁴ with his Constructive Empiricism claims that one should at best suspend belief in the unobservable entities; their existence cannot be known. He accepts the reality of macroscopic entities. On the other hand, the evident theory dependence of observations, and the continuum between the observable and the unobservable⁵, suggests that the ontological issue must be considered together with the wider issue of the known⁶ 'underdetermination of theory by experiment'. The question seems then to be as much whether the current theory, say, of the electrical conductivity of metals is true or approximately true, as whether one is entitled to believe that metals exist while electrons may not⁷. If the theory is approximately true then the belief in electrons seems well founded. This last claim depends upon finding an understanding of the difficult concept of the approximate truth of scientific theories which provides a justification for the claim. I make some further introductory remarks on the critical issue of the approximate truth of theories in section 1.2.

Towards the end of a book-length, scholarly account of the Scientific Realism versus Scientific Anti-realism debate, Kukla⁸ summarizes the position: 'It is not inconceivable to me that someone may yet devise a novel argument that gives an advantage to one side or the other. But I wouldn't hold my breath.'

⁴ van Fraassen, B. C. 1980 *The Scientific Image*, Oxford: Oxford University Press; van Fraassen does not deny that theoretical statements have truth values, he is an Anti-realist about theoretical entities and claims that the Scientific Realist claim of truth is extravagant: science he says properly aims only at empirical adequacy and should eschew 'inflationary metaphysics'.

⁵ Maxwell, G. 1962 'The Ontological Status of Theoretical Entities' in H. Feigl and G. Maxwell (Eds) *Scientific Explanation, Space, and Time* Minnesota Studies in the Philosophy of Science Volume 3 Minneapolis: University of Minnesota Press pp. 3-27.

⁶ Logically, a finite body of evidence allows of more than one theory. However, some philosophers argue that empirical equivalence does not entail underdetermination because non-empirical qualities (simplicity, elegance, explanatory power) can enable legitimate discrimination between empirically adequate theories.

⁷ Metals are often defined in terms of their electronic structure. The remark in the text implies that alternatively metals can be defined as exhibiting all or most of the following physical qualities: they are opaque, good thermal and electrical conductors; and lustrous when polished.

⁸ Kukla, A. 1998 *Studies in Scientific Realism* Oxford: Oxford University Press p. 154.

One proposed vindication of Scientific Realism arises from an argument based on the alleged empirical observation that successful scientific theories generally are fertile theories. It is argued that this conjunction of success and fertility is itself a decisive criterion indicating that the 'structures postulated in the theory correspond reasonably well to the structures of the real.'⁹ Thus Scientific Realism is justified. It is this notion that will be the major focus of the present inquiry. The chief proponent of this interesting position is Ernan McMullin¹⁰, and he has a particular definition of fertility that is central to the claim. I return to his definition in section 1.4.

Apart from empirical adequacy, theory criteria in science include fit with existing accepted theory (which is often incorporated within empirical adequacy), simplicity, elegance, and fertility (or fruitfulness)¹¹. Simplicity is complicated¹², and is extensively discussed in the philosophical literature. It is in any case not

⁹ McMullin, E. 1976 'The fertility of theory and the unit for appraisal in science' in R.S. Cohen *et al* (Eds), *Essays in Memory of Imre Lakatos*, Dordrecht: D. Reidel p. 423.

¹⁰ See for example, McMullin, E. 1968 'What Do Physical Models Tell Us?' in *Logic, Methodology and Philosophy of Science*, R. van Rootselaar and J.F. Staal (Eds) Amsterdam: North-Holland pp. 385-396; McMullin E. 1970 'The History and Philosophy of Science: A Taxonomy' *Minnesota Studies in the Philosophy of Science* R. Stuewer (Ed) Volume 5 Minneapolis: University of Minnesota Press pp. 12-67; McMullin, E. 1976 'The fertility of theory and the unit for appraisal in science' in R.S. Cohen *et al* (Eds), *Essays in Memory of Imre Lakatos*, Dordrecht: D. Reidel, pp.395-431; and McMullin, E. 1984 'A Case for Scientific Realism' in *Scientific Realism* J. Leplin (Ed) Berkeley: University of California Press, pp. 8-40.

¹¹ See for example Chalmers, A. 1999 *What is this thing called science?* Brisbane: University of Queensland Press. How, for example, empirical adequacy is understood and weighted will vary depending on the weight given to the other virtues, so that these criteria are to some extent interdependent.

¹² Is $y = px^5 + qx^2$ simpler than $y = xz$? In the latter we have linear dependence but more variables. Also, a relationship that looks simple in one coordinate system (say polar coordinates) may not do so in another (say Cartesian coordinates). Nevertheless, simplicity is widely invoked by scientists and perhaps the seeming arbitrariness of simplicity indicates only that it is context dependent (Bird, A. 1998 *Philosophy of Science* London: McGill-Queen's University Press p.158). In any case the hypothesis that the simpler theory has higher prior probability is false, if simplicity depends solely on parameter counting (Curd, M. and Cover, J.C. 1998 *Philosophy of Science* M. Curd and J.C. Cover (Eds) New York: W.W. Norton and Co p. 652). Hypotheses with fewer parameters logically entail hypotheses with more parameters; every planet travels in a circle entails every planet travels in an ellipse. Thus simpler theories are less probable than more complex ones. (For Popper this would of course be an advantage of simple theories, but this is an unusual stance.) Further, physicists are inclined to see quantum theory as simple, which seems to be stretching the concept somewhat, although they would argue for its large scope and few assumptions.

obvious that it is commonly used as a criterion of theory choice in science, although physicists in particular do sometimes invoke it. As McMullin remarks¹³: 'As theories develop, as factors at first laid aside are taken into account, the tendency has historically been to greater, not less, complexity.'

Elegance is admired by scientists, particularly theoretical physicists¹⁴, and even more by mathematicians, but is commonly ignored by philosophers in their critique of science (as distinct from their appraisal of philosophical theories). I suspect this is on the grounds that it is even more improbable that the world is required to be elegant than that it is required to be simple (whatever either might mean)¹⁵. Fertility is discussed very little by philosophers, but is regarded as important by scientists¹⁶.

The present inquiry tries to explore why fertility is regarded as important, and centrally, why it might indeed be important. In relation to this last, the major question, as already suggested, is whether fertility can be used to argue for Scientific Realism.

¹³ McMullin, E. 1996 'Epistemic Virtue and Theory Appraisal' in *Realism in the Sciences* I. Douven, and L. Horsten (Eds) Leuven: Leuven University Press, pp. 1-34 p. 24.

¹⁴ Although Einstein, against the general run of theoretical physicists, is reputed to have said, 'If you are out to describe the truth, leave elegance to the tailor'. The elegance may sometimes arise simply because mathematics is the best language physicists have, and they regard the corpus of mathematics as elegant. In biology, Crick's supremely elegant solution to the problem of the genetic code (why do we produce only 20 amino acids rather than 64 from a code of four 'letters' three at a time) – 'Codes Without Commas' – was highly regarded for some time. Nature with its degenerate code turned out not to be as elegant as Crick's proposal.

¹⁵ What theoretical physicists mean by referring to a theory as elegant is various and complicated and relates to a highly specialized aesthetic notion born of deep working involvement with abstruse theories, and the need to make tractable theories via sophisticated idealizations. It also often involves some notion of the necessity of aspects of the theory. For example, Newton's gravitational theory took the inverse square law because this accounted for Kepler's observations; Einstein's general relativity theory must use an inverse square law. If elegance as a criterion of truth were to be taken in its ordinary aesthetic sense it would imply the necessity of the observer – an idea repugnant to the Scientific Realist.

¹⁶ This last statement must be treated with caution since the term fertility is used in different ways. Some of the appeal to scientists of fertility (that is effectiveness in predicting new phenomena or leading to new observations or new generalizations via extension to existing theory) has little to do with fertility as McMullin uses the expression. This point is taken up in section 1.6 below.

1.2 Approximate Truth and Verisimilitude

I referred at the outset to the truth or approximate truth of theories, while McMullin uses phrases like correspondence to the structures of the real. The point is important because a central part of Scientific Realism for many philosophers of science¹⁷ is the notion of approximate truth, or an alternative, namely verisimilitude. McMullin does not accept the usual account that theories are 'approximately true' or that some have greater verisimilitude than others, because this immediately raises the question of how near to the truth. The question, he believes, is in principle unanswerable. He believes that Scientific Realism can be accepted without an account of the approximate truth of theories or their verisimilitude. His response¹⁸ to the Anti-realist who demands such an account is to enjoin her to resist '... the temptation to suppose that whatever cannot be said in a semantically definitive way is not worth saying.'

I do not believe the problem with the approximate truth of theories that concerns McMullin can be avoided. There seems to be no useful distinction between the claim that a theory is approximately true, or alternatively has great verisimilitude (i.e. in relation to the mind-independent world), and

The basic claim made by scientific realism, once again, is that the long term success of a scientific theory gives reason to believe that *something like* [italics added] the entities and structure postulated by the theory actually exists¹⁹.

Or

the explanatory success of a theory gives a limited warrant for the success of the theory or (what amounts to the same thing) for the existence of

¹⁷ See for example Popper, K.R. 1963 *Conjectures and Refutations*, New York: Harper and Rowe; Niiniluoto, I. 1999 *Critical Scientific Realism* Oxford: Oxford University Press; Psillos, S. 1999 *Scientific realism: how science tracks truth* New York: Routledge.

¹⁸ McMullin, E. 1984 'A Case for Scientific Realism' in *Scientific Realism* J. Leplin (Ed) Berkeley: University of California Press p. 36.

¹⁹ McMullin, E. 1984 *ibid* p. 26

explanatory structures *roughly like* [italic added] those postulated by the theory²⁰.

Or the passage²¹ in an argument for Scientific Realism against Instrumentalism:
 When we ask about a particular theory, how likely it is that it is true
 (correlatively, how likely it is that *something like* [italics added] the explanatory
 entities it postulates actually exist), it is to these virtues that we are inclined to
 turn.

The virtues referred in the last quote are the 'superempirical' values like fertility,
 and of course the crucial phrase in this and the first quote is 'something like', and
 in the second quote 'roughly like'. In fact, McMullin does on a number of
 occasions refer directly to the approximate truth of theories²².

McMullin argues against the pertinence of approximate truth by first pointing out,
 in response to van Fraassen's assertion that Scientific Realists must claim that
 science aims at truth, that scientists do not believe their theories to be true²³.
 This is accepted by van Fraassen, and indeed is rarely contested either by
 scientists or philosophers of science. McMullin goes on to say that the scientist
 claims her theories give some insight into the structures of the world but she
 could not say 'how good that insight is'²⁴. This seems to be the issue; scientists
 reject one theory in favour of another precisely on some judgment of the
 goodness of the insight. They make comparisons, as they must, between
 competing theories. McMullin might be right to question this, and to affirm that
 the scientists indeed do not know how good their insights are. However, the
 challenge then is to justify claims about the progressive nature of science and its

²⁰ McMullin, E. 1996 'Epistemic Virtue and Theory Appraisal' in *Realism in the Sciences I*.
 Douven and L. Horsten (Eds) Leuven: Leuven University Press pp. 1-34, p. 29.

²¹ McMullin, E. 1993 'Rationality and Paradigm Change in Science' *World Changes: Thomas
 Kuhn and the Nature of Science*, P.G. Horwich (Ed), reprinted in *Philosophy of Science* M. Curd
 and J.A. Cover (Eds) New York W.W. Norton and Company pp. 119-138 p. 135.

²² McMullin, E. 1985 'Galilean Idealization' *Studies in History and Philosophy of Science* 16, 247-
 273 p. 262; McMullin, E. 1987 'Explanatory Success and the Truth of Theory' in N. Rescher (Ed)
Scientific Inquiry in Philosophical Perspective Lanham: University Press of America p. 70;
 McMullin, E. 1996 'Epistemic Virtue and Theory Appraisal' in *Realism in the Sciences I*. Douven
 and L. Horsten (Eds) Leuven: Leuven University Press pp. 1-34, p. 29.

²³ McMullin, E. 1984 *op. cit.* p. 35.

²⁴ *Loc. Cit.*

realist pretensions, while implicitly denying that scientists can meaningfully make claims that one theory is nearer to the truth than another.

It does not get us out of the dilemma of necessary comparison to contend²⁵, 'The language of theoretical explanation is of a quite special sort. It is open-ended and capable of ever further development.' This statement is a useful pointer to McMullin's view of scientific theory as metaphor and his insightful thesis about the fertility of theories. Nevertheless McMullin needs to be able to claim that the successive versions of the progressively modified successful theory are in general improvements with regard to proximity to describing the mind-independent world.

In a later paper McMullin says²⁶,

first that a scientific realist is committed to attributing *some* sort of truth-value to successful theories, and second, that the measure of truthlikeness or likelihood to be attributed to the theory is precisely the degree of explanatory success the theory is taken to have shown.

He amplifies this by giving an example, saying that the striking success of the plate tectonic model gives good reason for the geologist to believe it is *true* that the continents and ocean floors are carried on massive rocky plates in slow relative motion. And he adds, reasonably, that the theory is approximate because it is not definitive, while not wanting to acknowledge that current plate tectonic theory is approximately true. Now the Scientific Anti-realist philosopher and the Scientific Realist philosopher both accept (in their legitimate shared ignorance) what the leading geologists are currently saying about the basic ideas in plate tectonics. The reasonableness of current science is not in dispute; the problem is that it seems impossible to get away from the facts that (i) scientists do not believe their theories are true; (ii) they necessarily make judgments about the relative merits of theories; and (iii) science is a rational activity. If the comparative

²⁵ *Ibid* p. 36

²⁶ McMullin, E. 1987 'Explanatory Success and the Truth of Theory' in N. Rescher (Ed) *Scientific Inquiry in Philosophical Perspective* Lanham: University Press of America p. 57.

judgments made by Scientific Realists are not based on the truth or approximate truth or verisimilitude of the competing theories, it is difficult to see what they can mean. It is possible to take the metaphysical stance that there is a mind-independent world, which we cannot know at all, but that is not the doctrine of Scientific Realism, as is discussed in the next section.

There are of course different types of theory, some avowedly instrumental, such as idealizing masses as point masses, or talking about rigid bodies, or taking a fluid to be a continuum. The Scientific Realist does not take such theories to be true or approximately true of a mind-independent world in the sense that they closely describe that hypothesized world. Thus, approximately true when said of such theories, refers primarily to their empirical adequacy and explanatory power. However, theories describing the electron or the hydrogen bond are believed to be near to describing the nature of these entities. For such theories for the Scientific Realist (in the full blooded sense that McMullin is attempting to establish) it is a necessary claim that successive versions of the theories in a given domain progressively increasingly resemble the true nature of the mind-independent world. This progress may not be monotonic²⁷, but the claimed approach to the structure of the real requires a usable concept of the approximate truth or the verisimilitude of theories.

As McMullin writes²⁸:

The guiding insight here is that if a theory displays just the sort of resources over time that one would expect if it were (approximately) true, this gives a *prima facie* case for supposing that it is in fact (approximately) true.

²⁷ With hindsight good reasons can sometimes be seen for the progress not to be monotonic. Drude's classical theory explained Ohm's Law, but subsequently Bloch's quantum mechanical account showed that electrons would experience no resistance in propagating through a periodic lattice. The role of impurities, lattice defects and thermal fluctuations restored resistance within the quantum story.

²⁸ McMullin, E. 1996 'Epistemic Virtue and Theory Appraisal' in *Realism in the Sciences I*. Douven and L. Horsten (Eds) Leuven: Leuven University Press pp. 1-34 p. 26.

These issues, around approximate truth and verisimilitude, in what one might call causal theories like these last, and those like the fluid example, are taken up in Chapters 3 and 4.

1.3 Scientific Realism

Ladyman²⁹, in prefacing his account of Scientific Realism, lays out a common form of Realism which he characterizes as follows.

If we incorporate both metaphysical and semantic realism about some subject matter S (which could be ethics, mathematics, aesthetics or theoretical science, among others) and add an *epistemic* requirement we get a strong form of realism about S:

(i) the entities or kinds of entities talked about and/or described by a discourse about S exist;

(ii) their existence is independent of our knowledge and minds.

These are the metaphysical requirements.

(iii) Statements about S are irreducible/ineliminable and are genuinely *assertoric* expressions;

(iv) truth conditions for statements of S are objective and determine the truth or falsity of those statements depending on how things stand in the world.

These semantic requirements are cashed out in terms of a correspondence theory of truth, as opposed to a pragmatic or a coherence theory of truth.

(v) Truths about S are knowable and we do in fact know some of them, and hence the terms of S successfully refer to things in the world.

This is the epistemic requirement.

Ladyman adds that If we now take S to be science we have a Scientific Realist position. There is an alternative Scientific Realist position which is to accept all of

²⁹ Ladyman, J. 2002 *Understanding Philosophy of Science* London: Routledge p. 158.

the Ladyman account except for the commitment to a Correspondence Theory of truth³⁰.

1.4 McMullin's P-fertility

We can return now to the claim that fertility of theories can provide support for Scientific Realism and begin with an account of the supposed virtue of fertility.

Scientists generally regard novel prediction as a highly significant indicator of the value of a proposed theory³¹. Whether this is justified is a matter of controversy among both scientists and philosophers of science, and I return to this question shortly. However, the first point is that it is common to equate novel prediction with fertility, that is, if a theory makes novel predictions³² it is a fertile theory. This is not the definition that McMullin uses in his argument that fertility is a ground for believing that the fertility of well established theories is evidence for Scientific Realism. Before moving on to McMullin's definition of fertility it is useful to say something further about novel prediction, which is a related concept.

From a logical point of view there is no reason to value more highly the explanation of facts, which emerged after the development of a theory, than those facts known at the time the theory was developed³³. Nevertheless, as

³⁰ The possibility of a Scientific Realist position not incorporating a correspondence theory of truth is rejected by some writers. For example, Sankey (Sankey, H. 2004 'Scientific Realism: An Elaboration and a Defence' *Knowledge and the World: Challenges beyond the Science Wars* M. Carrier, J. Roggenhofer, G. Küppers and P. Blanchard (Eds) Berlin: Springer Verlag p. 69) says, 'Correspondence theories which treat truth as a relation between language and reality are the only theories of truth compatible with realism.'

³¹ Not all historians of science agree with the more ambitious claim, which is commonly made by scientists themselves, that scientists value this most highly. Scerri and Worrall state that Mendeleeff's contemporaries were more impressed with his ordering of the known elements than his correct predictions of the finding of new elements (Scerri, E.R. and Worrall, J. 2001 'Prediction and the Periodic Table' *Studies in the History and Philosophy of Science* 32A, 407-452). Of course this example, even if fully established, tells us little about the claimed generalization.

³² A novel prediction in this context is an outcome of the theory under consideration that is subsequently confirmed, was not evident when the theory was formulated and clashes with current expectations.

³³ Matters are somewhat more complicated. If a scientist, unaware of some important fact that was known to others, developed a theory, and yet the theory explained that fact, this would be seen as impressive evidence for her theory. Moreover, it is not just the explanation of 'new' facts that is impressive, but also a new explanation of well-known facts that it was not the original

already mentioned, an explanation of 'new' facts is more highly valued by scientists. *Inter alia* it suggests that a theory is not *ad hoc*. I do not here mean *ad hoc* in the sense that the theory has been built to explain the known facts – all theories are rightly built like this. I mean that the development of the theory may have been fudged to fit all the known facts, whereas a theory built to explain some subset of these facts, which then predicts the remaining (predicted) facts, can not have been fudged to fit these unknown (predicted) facts. This brief account, sufficient for immediate purposes, derives from the extensive account given by Lipton³⁴. Lipton illuminates the point by an analogy with deducing a missing word in a crossword puzzle using the clue alone or using the clue plus the letters provided by intersecting words. The result of the former method, if it then fits the given letters as well as the other constraints, has greater credibility compared with the latter method. It should be stressed that fudged as used here is not meant to suggest any impropriety; it simply means that the weight appropriate to various pieces of evidence may have been wrongly estimated³⁵.

Fertility, as McMullin uses it, is not to be equated with novel prediction. The important element as McMullin sees it is the 'actual success the theory has had in opening up new areas, in meeting anomalies, and so forth'³⁶ via the modifications made in the historical course of the development of the theory. This

intention of the theory to explain. Again, from a logical point of view, the proposed theory gains no explanatory merit by virtue of the ignorance of the person propounding it.

³⁴ Lipton, P. 2004 *Inference to the Best Explanation* 2nd edition London: Routledge Chapter 10.

³⁵ On the other hand something questionable may take place. John Ziman, himself an able theoretical physicist, makes the following remark. 'Obviously general agreement between theory and experiment is very satisfying, but one must always be conscious of the possibility that the theory may have been tailored to fit the known facts. Professional theoretical physicists are trained to look for adjustable parameters in other people's theories that might have been used for this purpose.' (Ziman, J.M. 1981 *Puzzles, Problems and Enigmas* Cambridge: Cambridge University Press p. 40)

³⁶ McMullin, E. 1976 'The fertility of theory and the unit for appraisal in science' in R.S. Cohen *et al* (Eds), *Essays in Memory of Imre Lakatos*, Dordrecht: D. Reidel, p. 400. This notion of fertility was advocated earlier by Frank, but as a criterion of theory acceptance, not as an argument for Scientific Realism. Frank writes: 'The most important reason for the acceptance of a theory beyond the "scientific criterions" in the narrower sense (agreement with observation and simplicity of the mathematical pattern) is the fitness of a theory to be generalized, to be the basis of a new theory that does not logically follow from the original one, and to allow prediction of more observational facts.' (Frank, P.G. 1961 'The Variety of Reasons for the Acceptance of Scientific Theories' in *The Validation of Scientific Theories* P.G. Frank (Ed) New York: Collier Books p. 16.)

McMullin calls proven fertility (P-fertility), which he contrasts with untested promise (U-fertility)³⁷. P-fertility then is the success of the theory as traced historically since its inception.

There is a difficulty here in deciding what constitutes the continuous life of a theory as it is modified to explain anomalies or to accommodate new or more accurate data; when does a theory change sufficiently to be deemed a new theory? (This issue is taken up in section 2.2.) Nevertheless, there seems sometimes to be comparatively clear continuity. The kinetic theory of gases with its point molecules and its elastic collisions can perhaps properly be seen as the same theory when allowance is made for particles of finite size (intermolecular repulsive forces) and intermolecular attractive forces. Conversely, special relativity is arguably a clear break from Newtonian mechanics.

This difficult and essential question as to what constitutes the life of a theory on McMullin's diachronic view of theory appraisal is taken up in Chapter 2.

As I have just remarked, the notion of proven fertility is inextricably connected with the point that the theory must be considered as it develops over time. This is not inconsistent with the claims of Scientific Realism, since in general it is only for theories that have been explanatorily successful for a considerable period (albeit modified) that the claim is made that they are approximately true, and the unobservable entities they postulate are real.

P-fertility consists not in outcomes entailed by the theory in its original form, and for which the theory was designed. P-fertility consists in the explanation of new or additional phenomena via modifications in the original theory. These successive modifications are suggested by the theory in its original and then in its

³⁷ The interest in U-fertility would be the practical value it would have (if one could estimate it) in deciding that a theory was worth pursuing because of its high developmental promise, that is, high U-fertility. Nolan likens it to the value of a lottery ticket (Nolan, D. 1999 'Is Fertility Virtuous in Its Own Right?' *British Journal for the Philosophy of Science* 50, 265-282 p. 270).

successive forms, and require creative input. As I have said, P-fertility is not, on McMullin's stance, novel prediction. It is widely accepted that a theory that explains data not known at its inception, and especially, remote from the data used in developing the theory, is particularly convincing. Sometimes these new facts will have been entailed by the theory in its original form; these entailed facts will constitute novel prediction. However, these facts do not bear on the issue of fertility as McMullin uses the notion in his P-fertility; they are simply strong confirmation of the theory. A classic case is Fresnel's theory, which entailed the bright spot at the centre of the shadow, a phenomenon that was not known at the time the theory was proposed. This is not P-fertility. It is of course novel prediction³⁸.

A theory with a track record of successful successive modifications, arising naturally from the successive forms of the theory, is precisely a P-fertile theory. The theory viewed from the perspective of fertility is a changing entity, progressively requiring creative input from scientists. As a result, this concept of fertility lies outside the scope of a strict logical analysis of the merits of a theory since it is in part a historical question. Whether a theory has proved to be fertile will be, for example, partly a result of the intensity of interest in the theory manifested by the scientific community. This in turn will depend on factors like the perceived importance of the theory, and this, in its turn, may sometimes depend on factors that have nothing to do with scientific questions but may turn on the exigencies of societal needs. Thus a 'potentially' P-fertile theory in some obscure esoteric area of science may remain undeveloped and have little P-fertility. Further, the P-fertility of a theory will depend upon the talent of the scientists engaged with the theory during its lifetime and on other contingent factors.

³⁸ The phenomenon was calculated by Poisson to be entailed by Fresnel's theory; some historians claim that Poisson believed at the time that this improbable result (in fact swiftly confirmed) was a refutation of the theory.

McMullin likens a good theory to a metaphor. It is suggestive in the same way and also hints at the unattainability of a final definitive theory. He has in mind not so much a striking phrase such as 'her mother is a tiger', but rather an extended metaphor like Plato's metaphor of the shadows on the wall of the cave. I am not referring here to the brevity of the metaphor but rather to its depth. Short metaphors like 'society is a sea', 'marriage is a zero sum game', 'The Lord is my Shepherd' or 'poverty is a crime' have far reaching resonance and suggestiveness³⁹.

McMullin sees a model as always accompanying the theory.

A theory is intended not as a description of what one already has, but as an hypothesis, something that goes beyond the evidence by introducing a postulated physical structure that could provide a causal account of the data to be explained, although it is not (as yet, at least) itself directly observable. The structure here is called a 'model' every theory has a model associated with it, otherwise it could not serve to explain. It would be merely descriptive in intent.⁴⁰

McMullin is not exclusively using model here as it is used by the logician, as 'an entity that "satisfies" a formal system; i.e., if this entity be taken as an interpretation of the system, it makes all the statements of the system true.'⁴¹ The theory comes from the model and the model is not to be equated with the theory. For McMullin the model constructed by the scientist is built in the hope that it contains 'surplus content' and a good model does indeed do so. It is true that 'the statements of the theory apply correctly to the model.'⁴² A model is a system whose characteristics are specified by a stipulation as to how its terms are to be used, and McMullin's concept of a model does not exclude examples that meet

³⁹ These are cited in Black, M. 1993 'More about metaphor' in *Metaphor and Thought* 2nd edition A. Ortony (Ed), Cambridge: Cambridge University Press.

⁴⁰ McMullin, E. 1968 'What Do Physical Models Tell Us?' in *Logic, Methodology and Philosophy of Science*, R. van Rootselaar and J.F. Staal (Eds) Amsterdam: North-Holland p. 388.

⁴¹ McMullin, E. 1974 'Models and Analogies in Science' *New Catholic Encyclopedia* Washington DC: CUA Press (Supplementary Volume) 16, 295-299 p.298.

⁴² McMullin, E. 1968 *ibid* p. 389.

the logician's criteria for a model. For example, a Newtonian Particle System is a system that satisfies the three laws of motion and the law of universal gravitation. Thus a Newtonian model could be used to represent the Earth/Moon system as a two body system uninfluenced by any other matter in the universe (the Earth and the Moon might be taken as ideal spheres with a rotation-symmetric mass distribution). The resulting model then is an interpretation (or realization) of the general law. Obviously, other less idealized models could be made of this Newtonian system.

The metaphor parallel is a way of explaining the role of the initial complex of model and theory in suggesting modifications that might be made. The model and the theory, like a metaphor, are not exactly how things are, but are suggestive, often by analogical argument, in getting insight into the way things 'really' are (Greek *metaphero*, to carry over, translate, change, transfer). The theory like a good poetic metaphor suggests to the creative mind ways in which the theory/model might be augmented or modified⁴³. These changes are not guaranteed to be in the direction of improvement. As Black⁴⁴ says of metaphor, 'Ambiguity is a necessary by-product of the metaphor's suggestiveness.' McMullin uses the Bohr Theory of the hydrogen atom as an example. The simple planetary model of an electron, conceived of as a small particle of mass m revolving in fixed orbits around the stationary heavier nucleus of mass M , swiftly suggested a modified model where the two particles rotate around a common centre. This allowed an explanation for the slight difference between the spectral lines of ionized helium and the hydrogen atom because M/m is different in the two cases.

⁴³ Some writers see the use of metaphor in science as distinct from its use in literature and would object to this parallel. For a review see Montuschi, E. 2000 'Metaphor in Science' in *A Companion to the Philosophy of Science* W.H. Newton-Smith (Ed) Oxford: Blackwell pp. 277-282.

⁴⁴ Black, M. 1993 'More about metaphor' in *Metaphor and Thought* 2nd edition A. Ortony (Ed) Cambridge: Cambridge University Press p. 29.

McMullin does not give an extensive discussion of his notion of metaphor⁴⁵, but he is suggesting that current theory, like metaphor, is what generates the ideas that extend existing theory.

A fertile theory is one with the capacity to grow and change. These changes are not simply logical extensions of the original theory into new contexts; they are the product of creative imagination rather than strict logic. They are part of the resources of the original theory, not as predictions (novel or otherwise) would be, but as the sorts of hints one would expect a good metaphor to provide when it is set against new contexts.⁴⁶

A metaphor is not best seen as a form of simile – by saying her mother is a tiger we are not saying she is like a tiger. As Searle says⁴⁷,

Similarity is a vacuous predicate; and any two things are similar in some respect or other. ... 'Juliet is the sun' does not mean 'Juliet is for the most part gaseous' or 'Juliet is 90 million miles from the earth,' both of which properties are salient and well-known features of the sun.

A metaphor makes us apply concepts from a familiar source domain to a target domain. A common example is the treating of life as a journey, the latter being familiar – 'our life together is going nowhere', 'it will be a rocky road', and 'we have reached a dead end' – to take some gloomy examples. The question is whether metaphors can be creative. Black⁴⁸ illuminates this issue by considering the following question, 'Did the slow-motion appearance of a galloping horse exist before the invention of cinematography?' The metaphor is seen as a cognitive instrument, like cinematography here. Likewise I believe the model conjoined to the scientific theory is in McMullin's view a cognitive instrument in just this way.

⁴⁵ Bradie, in a generally sympathetic account of McMullin's position remarks, 'we can nowhere find a clear statement of McMullin's concept of metaphor' (Bradie, M. 1980 'Models, Metaphors, and Scientific Realism' *Nature and System* 2, 3-20 p. 5).

⁴⁶ McMullin, E. 1979 'The Ambiguity of 'Historicism'' *Current Research in Philosophy of Science* P.D. Asquith and H.E. Kyburg (Eds) Lansing: Philosophy of Science Association p. 60.

⁴⁷ Searle, J. 1979 'Metaphor' in *Metaphor and Thought* A. Ortony (Ed) 2nd edition Cambridge: Cambridge University Press p. 106.

⁴⁸ Black *op. cit.* p. 37.

For McMullin, P-fertility is of vital importance because in his view it provides the main, and perhaps the only, support for Scientific Realism. He argues that the progressive development of theory in dealing with new data, with anomalies, and in being extended to new domains, can only be explained on the basis of Scientific Realism. This claim is discussed in the following sections.

1.5 Scientific Realism and P-fertility

P-fertile theories and their associated models are successful. It is important to recognize that this is an empirical claim. We can take 'successful' here to mean that they have continued to be used and accepted as 'approximately true' by practicing scientists. This in turn means that they have proved empirically adequate over an extended period (this has involved modification) and have appeared to be explanatory. For the scientist a successful theory is an acceptable theory, that is, the criterion for acceptability is success. Newton's immensely successful theory was finally superseded because *inter alia* it could not give an adequate account of the Michelson-Morley experiment and there was an alternative that could.

To quote McMullin⁴⁹ on his concept of P-fertility, 'This kind of fertility is a persistent feature of structural explanations in the natural sciences over the last three centuries and especially during the last century'. He remarks in an earlier paper⁵⁰, 'What counts, perhaps, most of all in favour of a theory is not just its success in prediction but what might be called its resilience, its ability to meet anomaly in a creative and fruitful way.' Here he is claiming that the historical record shows that his P-fertility has been demonstrated to be a feature of long standing accepted theories. His point is that this is a contingent feature of the history of science. It is possible, for example, in his view, that it could have been

⁴⁹ McMullin, E. 1984 'A Case for Scientific Realism' in *Scientific Realism* J. Leplin (Ed) Berkeley: University of California Press p. 33.

⁵⁰ McMullin, E, 1975 'History and Philosophy of Science: A Marriage of Convenience?' *Boston studies in the philosophy of science* 32, 585-601 p. 597. It is clear from the context that McMullin means repeated ability to meet anomaly.

the case that long standing successful theories may not have shown P-fertility⁵¹. Modification to them might have resulted from the compelling nature of evidence and a requirement for a radical innovation not related to or stemming from the existing version of the theory. Alternatively, it might have been the case that long standing successful theories were unattainable; the history of science could have been an abrupt erratic kaleidoscope of theoretical positions. His argument for Scientific Realism, as I have tried to emphasize, is not an *a priori* argument⁵². In other words the quoted 'what counts most' claim is intended by McMullin to be the same claim as the quoted 'persistent feature' remark above. The wording of the 'what counts most' remark could alternatively be interpreted as suggesting that P-fertility is the criterion for success and acceptability, but I think this would be incorrect⁵³. McMullin is claiming that acceptable/successful theories have been P-fertile as a matter of historical fact. It is on this that his Scientific Realist claim relies.

The argument thus goes as follows. First, P-fertility has been found from the historical record to be a feature of successful/accepted theories. Second, the proper unit of theory appraisal is the historically evolving theory. That is, the claims for truth or approximate truth are made properly for long standing successful theories that of necessity have been modified over time⁵⁴. Third, the theory incorporates a model, and it is primarily the model that suggests the modifications to the theory (and to the model itself), that take place over time.

What makes it heuristically sensible to proceed in this way is the belief that the original model does give a relatively good fit to the real structure of the

⁵¹ McMullin, E. 1984 'A Case for Scientific Realism' in *Scientific Realism* J. Leplin (Ed) Berkeley: University of California Press, pp. 8-40, p. 33.

⁵² Some Scientific Anti-realists who point to numerous successful theories that have ultimately proved to be false contest McMullin's empirical claim. The argument then revolves around various issues such as whether the theories failed in essential, rather than peripheral, respects. In any case, McMullin is rightly insistent that Scientific Realism does not entail the claim that all successful theories approach the structure of the 'real'.

⁵³ He makes this point clear in his extended account of the development of the Bohr Theory of the atom given in several of his papers.

⁵⁴ There is an inescapable vagueness about the term 'long-standing' here. It is probably fair to say that it is impossible to quantify as it varies not only over historical periods but also between disciplines.

explanandum object. Without such a fit there would be no reason for the model to exhibit this sort of fertility. This gives perhaps the strongest grounds for the thesis of scientific realism.⁵⁵

McMullin says that as a matter of historical fact it can be shown that the theories which have been successful have over time shown P-fertility. For this not to be circular, McMullin has to persuade us that the criteria for success can be asserted independently of P-fertility. His criteria as I understand him are internal consistency, consistency with other well established theories, lack of *ad hoc* features, explanatory power, continued empirical adequacy and, where appropriate, continued novel prediction⁵⁶.

Although the history of modern science is immensely complicated, it is useful to grant McMullin his not implausible empirical claim, and on this basis, to examine the claim that this leads us to Scientific Realism. The acceptance of McMullin's position incorporates the reasonable further concession that each of the theory modifications suggested itself to the innovators as at least partially a consequence of the then existing theory/model.

Given this empirical fact McMullin then takes for granted that a hypothetical true theory, that is, one which described the world as it is, would be successful⁵⁷. Further, he implicitly assumes an approximately true theory, that is, one that in his terminology gives an 'approximate insight into the real structure of the object

⁵⁵ McMullin, E. 1985 'Galilean Idealization' *Studies in History and Philosophy of Science* 16, 247-273 p. 264.

⁵⁶ McMullin, E. 1987 'Explanatory Success and the Truth of Theory' in N. Rescher (Ed) *Scientific Inquiry in Philosophical Perspective* Lanham: University Press of America.

⁵⁷ Logically, truth is neither necessary nor sufficient for success. It is not necessary since a false premise can lead to true conclusions (numerous writers have noted that Ptolemaic astronomy was quite successful in predicting eclipses). It is not sufficient since there may currently be no evidence for a true theory or a true theory might be conjoined with false auxiliary assumptions from another domain (Lyons, T.D. 2003 'Explaining the Success of a Scientific Theory' *Philosophy of Science*, 70, 891-901).

studied⁵⁸, would be successful and hence acceptable. This is not obvious although plausible. However, its degree of plausibility is a subtle question involving the complex and elusive notion of approximate truth. I return to this important question in Chapter 3.

Now given the empirical claim we have just conceded, and given the claim about approximately true theories, the central question is whether the explanation of the fact that successful theories are fertile is that they are approximately true.

Obviously, all P-fertile theories are successful – this is inherent in P-fertility definition – but we must explain why most successful theories are P-fertile. This is explained if P-fertile theories are generally approximately true, since as has just been asserted, approximately true theories will be generally be successful. This then is an abductive argument; it does not say if a theory is P-fertile then it is successful (which as already mentioned is a given from the definition of P-fertile). Nor does it say if a theory is successful then it is P-fertile; it says simply that the common conjunction of P-fertility and success is explicable if both follow from approximate truth⁵⁹.

If P-fertile theories were not approximately true, how can one otherwise account for the commonality of P-fertility and long term success? If they were far from the truth, yet empirically adequate, what accounts for the fact that they have the inherent capacity for modification which leads to their successful modification?

⁵⁸ McMullin, E. 1970 'The History and Philosophy of Science: A Taxonomy' *Minnesota Studies in the Philosophy of Science* R. Steuwer (Ed) Volume 5 Minneapolis: University of Minnesota Press pp. 12-67 p. 14.

⁵⁹ It seems that each of the successor theories that constitute the P-fertile theory must be approximately true. Worrall draws attention to what he calls the non-transitivity of approximate truth (Worrall, J. 1989 'Structural Realism: The Best of Both Worlds' *Dialectica* 43, 99-124 (reprinted in D. Papineau (Ed) 1996 *The Philosophy of Science* Oxford: Oxford University Press)). He says if we took photographs at one second intervals of a developing tadpole, each would be approximately like the previous one, but the final photograph would be of a frog, which does not resemble a tadpole. McMullin could however respond that all the members of a limited sequence of theories, constituting the P-fertile theory, may be similar to one another and to the truth.

As Bradie⁶⁰ puts McMullin's argument, 'The grounds for construing theoretical models realistically is the 'striking success' enjoyed by the models associated with fertile theories.'

We need however to consider successful P-fertile theories that are now known to be false. The usual example is the phlogiston theory of combustion proposed at the beginning of the eighteenth century. Briefly, a metal heated in air gave up its phlogiston and became the oxide (calx), and a metal oxide heated in the presence of charcoal became the metal as a result of absorbing the phlogiston from the (phlogiston rich) charcoal. Not only did the theory hold sway for a very long time but it also made successful predictions⁶¹ despite the fact that phlogiston is non-existent.

All theories that claim to represent reality do get modified as the result of new data. This does not tell us very much – new facts emerge from research and they sometimes conflict to a degree with the current theory or theories. If they cannot be accommodated by modification we need a Kuhnian revolution⁶². Whether theories are substantially true or substantially false, the gathering of new evidence will lead to their modification. In the case of substantially false theories, McMullin's empirical claim is that modification does not in general prove viable. So what about the theories involving phlogiston, caloric, or the aether, which were false and successfully modified over a considerable period?

⁶⁰ Bradie, M. 1980 'Models, Metaphors, and Scientific Realism' *Nature and System* 2, 3-20 p. 9.

⁶¹ Joseph Priestly predicted 'correctly' that phlogiston, produced by the action of a metal on HCl, would convert an oxide to the corresponding metal. Curiously it was well known at the time, and indeed had been known well before the phlogiston theory was proposed, that a metal increased in mass when converted to its calx. This anomaly was ignored and remained unexplained because the theory 'successfully' explained what we would now call oxidation and reduction of metals, and it had no serious rival (Conant, J.B. 1963 'The Overthrow of the Phlogiston Theory' in *Science: Method and Meaning* S. Rapport and H. Wright (Eds) New York: New York University Press pp. 88-106.) Even more striking is the prediction, made by both Dalton and Gay-Lussac on the basis of the caloric theory, that the rate of increase of volume with temperature at constant pressure would be the same for all gases (Losee, J. 2004 *Theories of Scientific Progress* London: Routledge Chapter 15).

⁶² This is a rather crude account. It is not necessary to accept Kuhn's radical division between normal science and revolutionary science. The point here is merely that if modification is not possible we need an entirely new theory.

One can immediately say two things in response to the long term success of the phlogiston theory. First, McMullin's claim is that he is drawing conclusions from a valid generalization not an exceptionless law. He is making an abductive argument and claiming that in the absence of an alternative explanation his is a legitimate inference. Second, one cannot ignore the dramatic difference between contemporary science and its eighteenth century and earlier predecessors. McMullin makes his claim about the P-fertility of successful theories with regard to recent science. He could not make his claims about early Greek and subsequent speculations about, for example, cosmology or the fundamental nature of matter. False theories were commonly generated from what we now know to have been hopelessly false theories. However, modern science is hugely cumulative and finding examples of twentieth and twenty-first century science where we have successful theories which were subsequently abandoned, rather than substantially incorporated into later versions, is difficult⁶³.

Smart⁶⁴ makes this point forcefully:

One trouble about testing scientific realism in the light of history is that science has changed so much over the centuries. The Ptolemaic cosmology is so anthropocentric and foreign to us now. We know the distances of the sun, stars, and galaxies so that it does not seem at all surprising that the Ptolemaic system was overthrown. This should not suggest that all contemporary physical theories will one day be overthrown.

Kukla⁶⁵ makes a related point in resisting the Pessimistic Meta Induction:

⁶³ In 1963 D.J. deSolla Price estimated that 80 to 90 percent of all scientists who had ever lived were then alive (deSolla Price, D.J. *Little Science, Big Science* New York: Columbia University Press Reissue edition (August 1986)). This is a striking figure since it is conservatively estimated that fewer than one in ten of all people who have ever lived is currently alive. The proportion of all scientific knowledge that is post nineteenth century science, given the cumulative nature of the power of scientific investigation, is probably considerably greater than 90%.

⁶⁴ Smart, J.J.C. 2000 'What is This Thing Called Philosophy of Science?' *Metascience* 9, 172-202 p. 190.

⁶⁵ Kukla, A. 1998 *Studies in Scientific Realism* Oxford: Oxford University Press p. 16.

Suffice it to say that the falsehood of our past theories, all by itself, is a very weak basis for projecting that falsehood onto our present theories. If we were marching steadily toward an attainable goal of absolute theoretical truth, our interim theoretical accounts of the world might nevertheless all be false until we got very close to the end.

There is clearly some danger in arguing for the superiority of modern science. It is a feature of any contemporary view to see itself as substantially correct⁶⁶. There is every reason to believe that the Babylonians had great faith in their cosmology. On the other hand, comparing the experimentally determined human genome with that of other mammals and then with pre-mammalian creatures, and tracing the differences and similarities, seems a different practice from the predominately armchair science of previous eras. The human, financial and technical resources used in high energy particle physics or genomics to establish new data under controlled conditions are so different from the resources available to Johannes Kepler or William Harvey and their peers that it seems a different enterprise⁶⁷. The long standing success and seeming P-fertility of older theories can be attributed by McMullin to the less severe critical environment.

Another factor supporting the greater credibility of recent science, at least for the Scientific Realist, is the cumulative and interactive nature of the enterprise. It has often been possible to develop one branch of science as a result of seemingly unrelated advances elsewhere. For example, there was for a long time evidence from the fossil record that the Earth was very ancient. However, in the nineteenth century it seemed impossible to scientists that the Earth could be more than about 100 million years old, because there seemed to be no way in which the

⁶⁶ On the other hand, the basis for the claim that past scientific theories have been shown to be false is commonly that current theories show them to be false, that is, it is assumed that there is significant truth in current theories.

⁶⁷ Norris (Norris, C. 2004 *Philosophy of Language and the Challenge to Scientific Realism* London: Routledge p. 12) remarks in a different context, 'Locke might not have so forcefully denied the possibility of advancing from "nominal" to "real" essences or definitions had he been less struck by the limits of contemporary scientific knowledge with regard to just such matters.' It does seem a radical change to be able to say that gold is the element of atomic number 79, rather than that it is a metal of a particular colour, malleable, ductile, and resistant to dissolution in most acids.

sun could have continued to produce the energy it does and to have existed for more than this length of time. Once nuclear fusion was understood this difficulty disappeared.

Thus considerations about the difference between contemporary and earlier science might suggest that the persistence of false scientific theories prior to the Enlightenment is not a legitimate concern for McMullin. However, critics might not unreasonably argue that the maturity of different fields will differ chronologically. Some might say that the phlogiston theory, considered above, was sufficiently sophisticated to be seen as a mature theory in the field of combustion. This enjoyed wide support for nearly one hundred years from about 1700. Why was it successful and P-fertile, while both false and not continuous with what Lavoisier proposed? McMullin might deny that it was P-fertile, and argue with some justice that it persisted (in an earlier era) for want of an alternative. Perhaps also he might argue that the eighteenth century was still pre-modern as far as this branch of chemistry is concerned. Alternatively, he could suggest that it was simply an accident that a false theory showed P-fertility; something merely highly unlikely but certainly not impossible on McMullin's stance. The limited scope and substantial imprecision of the experimental evidence at the time meant that the theory was not easily falsified. Besides, the idea of a fluid used in combustion, although empty, can be seen with hindsight to be suggestive. This last would demand that historical analysis showed that such success of false P-fertile theories was rare in modern science (on the basis of some account of what constituted a mature science). Finally, there are those who claim, I believe implausibly, that Lavoisier's theory of combustion is a (substantial) modification of the phlogiston theory. (It is odd to claim that a cylinder of phlogiston contains the absence of oxygen.)

1.6 Another Form of Fertility (E-fertility)

Before considering a major criticism of McMullin's argument in the following section, I want to return briefly to a point made earlier (see footnote 16) in

relation to the widely held view among scientists of the great value of fertility. It is important to disentangle this common view from the evaluation of P-fertility as defined by McMullin. The merits or demerits of McMullin's case may be clouded by confusing it with a view that sees fertility either as novel prediction (already discussed) or as mere suggestiveness. I consider an example below of fruitfulness or fertility which is seen as valuable but has little to do with McMullin's argument. The purpose of the present digression is to make clear the complexity of the concept of fertility as it is broadly used as a term of esteem in science, and to contrast it with the quite specific P-fertility that provides McMullin's case for Scientific Realism.

Sometimes (rarely) an experiment is determined by theory in that a specific definitive experiment is suggested that will discriminate between two rival theories. Alternatively, an experiment may test a prediction entailed by a single theory, and provide either confirmation or disconfirmation. There are classic experiments of this sort, and these are the bread-and-butter of philosophers of science. The quintessential example is perhaps the Michelson-Morley experiment. However, as I shall argue, many experiments are not of these types; they are essentially exploratory.

Commonly, a theory, or the conjunction of a number of theories, suggests an experiment, and the unsuspected results of the experiment then suggest a modification of a distinct theory, or further experiments. I will provisionally call this experimental fertility E-fertility⁶⁸. It might be defined as the development of new theories or theoretical concepts as a result of novel experimental observations not entailed by or resulting directly from the existing body of theory.

⁶⁸ It is noteworthy that the following example of E-fertility depends on the combination of an existing theory and a new experimental technique. This will be a common but not a necessary feature of E-fertility. For example, Röntgen's discovery of X-rays in 1895 was made with existing apparatus – a cathode ray generator.

McMullin's P-fertility requires that the outcome of a fertile theory is a modified version of that theory. In contrast, McMullin's metaphor picture merely suggests that a fertile theory is more instrumental in generating new knowledge than a less fertile theory. On the metaphor picture one might perhaps argue that a good metaphor may result (indirectly) in more E-fertility than a poor one. However the situation is almost invariably complex as more than one theory is commonly involved⁶⁹.

Perhaps the argument of this section is best illustrated by an example. Take the well established theory that vacant lattice sites (vacancies) are present in a metal in an equilibrium concentration proportional to $e^{-U/kT}$, where T is the absolute temperature, U is the activation energy for vacancy formation, and k is Boltzman's constant. The concentration of vacancies will obviously be highest at high temperature. Now if the metal is sufficiently rapidly cooled from just below the melting point, so that equilibrium cannot be maintained, the now excess vacancies will tend to precipitate at the lower temperature. The cooling rate required to maintain a non-equilibrium concentration of vacancies will depend on the activation energy for vacancy migration U_m . U and U_m can be calculated approximately for a given metal and the values suggest that the required cooling rates are experimentally attainable. So much the two theories tell us. However, the theory, being about equilibrium, does not tell us in what form the precipitation will take place.

In 1956 the technique of transmission electron microscopy was developed⁷⁰. This allowed the direct observation of lattice defects in metals. So, to confirm the 'novel prediction' that vacancies will precipitate, and to investigate the form of the

⁶⁹ The outcome even of a theory specifically designed to test a theory may also be complex. The famous Stern-Gerlach experiment showed that classical theory was false, and was designed in addition to see if Sommerfeld's old quantum theory was correct. It was taken to do so, but in fact was later seen to be in conflict with it, and to be in accord with the new quantum mechanics (Weinert, F. 1995 'Wrong Theory – Right Experiment: The Significance of the Stern-Gerlach Experiments' *Studies in the History and Philosophy of Science* 26, 75-86).

⁷⁰ Hirsch, P.B., Horne, R.J. and Whelan, M.J. 1956 'Direct Observation of the Arrangements and Motions of Dislocations in Aluminium' *Philosophical Magazine* 1, 677-84.

precipitation⁷¹, a rapid cooling (quenching) experiment was done, and the predicted effect observed. However, the nature of the vacancy precipitation in different metals as faulted or unfaulted edge dislocation loops or stacking fault tetrahedra was unpredicted and proved very informative. Now, typically in these circumstances an experimenter will do further experiments arising from quite general principles. She will repeat the experiment on a similar metal, and also on the same metal with a lower level of impurities (the first experiment having been done perhaps with the purest material to hand, say 99.99%, but not the purest obtainable at the time, say 99.9999%).

The point is that these experiments are guided by theory not determined by theory; nothing about the form of the precipitation is entailed, or even suggested, by the established theory, which is about equilibrium. The new experiments are deliberately conducted away from equilibrium. The additional experiments are not guided by another theory in the area – there isn't one. They are guided by the experimenters' general knowledge. The variables tried seem reasonable – the metal, the purity, and the quenching speed. The experiments are intended to gather data on which to begin to form a theory of the non-equilibrium behaviour of vacancies in metals. Nature is being interrogated, and one would be foolish to await a proper theory in a difficult terrain, when one can swiftly conduct an informative experiment. The fertility of the original theory thus need not, and commonly does not, lie in the generation of another theory, but rather lies in the suggestion of new experiments which may result in modifications of existing theories. In addition the new results are, as here, commonly the outcome of the conjunction of theories, rather than simply arising from one theory. Moreover, the experiments will not, in general, be experiments like the observation of the precise orbit of the perihelion of Mercury or the Michelson-Morley experiment. That is, they will not be conducted to test a precise prediction of a well-developed theory of great generality. There is an understandable tendency in the philosophy

⁷¹ Experimenters had previously studied the changes in electrical resistivity of quenched specimens heated to intermediate temperatures after quenching. These experiments need not concern us here, as they make no difference to the point being made.

of science literature to concentrate on a small number of atypical landmark experiments. Moreover, although the post-Kuhnian revolution⁷² did result in philosophers looking more closely at the realities of scientific work, they (and Kuhn himself) nevertheless continued commonly to focus on the seminal experiments.

I have given the quenching example in some detail, but the point is general. For example, the vast subject of catalysis is guided by a broad theory, but the discovery of numerous effective catalysts has been a matter of intelligent trial and error (aided by the huge sums of money available for this potentially lucrative area). Then, subsequent to the discovery of a particularly effective catalyst, further research has contributed to a refinement of theory. Similarly, research into materials for solar power has been guided by theories of electromagnetic energy conversion in semiconductors, and the guided empirical development of new or improved materials has resulted in advances in the theory. The development of (non-equilibrium) optimal precipitation-hardened aluminium alloys, another area of commercial significance, has been guided by a general understanding of the thermodynamics of alloys, that is, a theory concerning equilibrium. Again, these non-equilibrium structures have been to a large extent developed by intensive systematic procedures of trial and error, and, as a result, there is an extensive theoretical understanding of non-equilibrium aluminium alloy structures.

Scientists regard fertility as a criterion of the value of a theory, and, although it seems improbable, in the case of E-fertility this could conceivably be fully accounted for by Kuhn's explanation; he somewhat cynically remarked⁷³:

This last criterion, fruitfulness, deserves more emphasis than it has yet received. A scientist choosing between two theories ordinarily knows that his decision will have a bearing on his subsequent research career. Of course he

⁷² Kuhn, T.S. 1970 *The Structure of Scientific Revolutions* Chicago: University of Chicago Press.

⁷³ Kuhn, T.S. 1977 *The Essential Tension* Chicago: University of Chicago Press p. 322. This remark need not be taken to be wholly cynical; the concrete successes will normally follow the choice of what proves to be the correct theory, and that the choice was motivated in part by ambition does not necessitate a lack of an overriding concern for truth.

is especially attracted by a theory that promises the concrete successes for which scientists are ordinarily rewarded.

One might ask, in the context of E-fertility, whether a substantially false theory is likely to result in fruitful new experiments, if it were to be suggested that E-fertility was a genuine epistemic merit in a theory. There seems no reason why a false theory should not suggest new experiments. Nor does there seem to be any reason why these experiments, undertaken on the basis of a false theory, should not produce interesting results. However, the relevant question is whether an approximately true theory is more likely to lead to interesting experiments leading to valuable new results. The classic case of an extensive experimental programme based on a false theory is the history of alchemy. A lot of interesting work was done by the alchemists. We now seem to be involved in the impossible evaluation of this work in comparison with an equal amount of work based on a hypothetical rival approximately true theory. There is an intuitive plausibility to the notion that an approximately true theory will be a better guide to progress in science than a load of nonsense. However, this last remark is not quite fair, because all of the theories in play will have been through the filters of empirical adequacy and so on, at least as seen by contemporary scientists. Newton, perhaps the greatest scientist in the history of the discipline, devoted huge amounts of time and energy to alchemy.

The fertility of scientific investigation in its widest sense – the fact that the practice of science has led to a remarkable proliferation of knowledge and that its progress does seem autocatalytic – may conceivably be an argument for Scientific Realism⁷⁴. On the other hand, E-fertility may illustrate no more than the banal truth that if one insists on looking in the middle of the night for something one has lost, it is sensible to look in places where there is some illumination.

⁷⁴ If one assumes the truth of Scientific Realism it is plausible that rational experiments like the quenching experiments described above would lead to new knowledge of the assumed mind-independent world. On the other hand, the Anti-realist might claim, against McMullin's thesis, that the success of the experiments not guided by a 'theory as metaphor' indicates that his explanation of the conjunction of success and P-fertility is superfluous.

However, it is important to distinguish this from McMullin's argument, based specifically on P-fertility.

1.7 Is P-fertility Explained Away as Novel Prediction?

Nolan⁷⁵ has challenged McMullin's position. He argues that the claimed value of fertility is explicable in terms of the other desirable features of theory. These he lists⁷⁶ as 'empirical adequacy, simplicity, strength (or comprehensiveness) and coherence (both internal consistency, and coherence with other good theories)'. Nolan deals first with U-fertility, and makes the valid point that it is simply the potential a theory has for developing the other accepted virtues. The principal interest in this, he says, is to examine whether there are means of assessing it. If one could do this it would be valuable for practicing scientists in deciding which of two competing theories to pursue.

In turning to P-fertility, Nolan has interesting things to say. In particular, he questions whether P-fertility exists in a meaningful sense. Nolan asks us to consider why a new theory U should be thought inferior to an exact contemporary V_3 , which is a successor to earlier successful theories V_1 and V_2 . This is to be considered in the case where U and V_3 have equal merit in terms of simplicity, strength, and coherence. He contends that this is to be explained by regarding the V series as successive forms of a more general theory (a less explicit theory) or a meta-theory (a theory about theories). Thus, for example, atomic theory is a general theory of matter; the earliest form of the kinetic theory of gases is an atomic theory, as are subsequent more sophisticated versions of the kinetic theory. Thus the earliest version has led to novel predictions in generating the later versions of the series, and the merit of the V series is no more than the value of novel prediction. Hence fertility *per se* is subsumed under the value of novel prediction, and it is this that lends P-fertility its intuitive plausibility.

⁷⁵ Nolan, D. 1999 'Is Fertility Virtuous In Its Own Right?' *British Journal for the Philosophy of Science* 50, 265-282.

⁷⁶ Nolan, D. *ibid* p. 265.

Nolan explains away McMullin's P-fertility in terms of an account of novel confirmation of either a general or a meta-theory. I want to argue that Nolan's account is inconclusive and that it is unsatisfactory in general.

Nolan's argument goes as follows. There is a class of V theories, $V_1, V_2 \dots V_n$ that meet McMullin's description in that they arise as improvements and modifications of one another following on new evidence or new theoretical insights. Improvement here means that empirical adequacy was restored or improved without serious new problems arising in complexity for example. Why should V_3 be preferred to U, a new theory which by definition has all the virtues of V_3 except that it is not a successor to earlier U-type theories⁷⁷?

Nolan readily acknowledges that P-fertility is not the same as novel confirmation. (As he points out a theory may have made numerous correct predictions without having been revised at all.) The improvements in the V series need not have involved novel confirmation at all, when one simply considers the reasons why the successive theories were preferred. However, Nolan says, when we look at the meta-theory, which is what unifies the V series, we do have novel confirmation of the meta-theory. This is the reason for preferring V_3 to U.

As an illustration, consider the meta-theory to be the theory that gases are composed of molecules; then the various successive gas laws $PV=RT$, $P(V-b)=RT$, and $(P+a/V^2)(V-b)=RT$ (where P is the pressure, V the volume, T the absolute temperature, and a, b, and R are constants) are each examples of novel confirmation when viewed as developments of the meta-theory. The meta-theory was advanced before it was known that a law of the second form would give a better fit to the data and a better explanation (because of the finite size of the molecules). Likewise the meta-theory was proposed before it was known that the third form would be still better (because of the intermolecular attractive forces).

⁷⁷ Nolan incorrectly here implies that P-fertility is used by McMullin as a criterion for theory selection, rather than simply as historical evidence for the approximate truth of the theories. However, this implication is not an essential part of Nolan's critique.

Thus the merit of P-fertility is the merit of novel confirmation, which was discussed above in terms of our intuitions about 'fudging'. This is so despite the fact that P-fertility as defined by McMullin is not novel confirmation.

The question at issue is not, as Nolan couches it, whether a theory that lends itself naturally to revision, which enables it to accommodate new data, is to be preferred to one without this characteristic. The question is whether the fact that successful theories generally have this characteristic can be justifiably assumed to be approximately true. Now McMullin claims, and we have for the purposes of argument conceded, and Nolan does not deny, that successful long standing theories have been established as having this characteristic. For Nolan, these successful long standing theories are commonly based on a meta-theory or a general theory that enjoys novel confirmation. In our particular example, the theory that gases consist of atoms or molecules can be seen with hindsight to have been sufficiently flexible to account for the many discoveries about gases made since.

The dispute is most easily judged in terms of specifics, and Nolan's account in avoiding specifics has obscured McMullin's point. Nolan eschews particular cases, saying⁷⁸, 'actual cases need to be described fairly carefully, and often possess features which cloud specific issues'.

If, for example, we now consider the Bohr Theory of the atom, an appropriate theory on the Nolan proposal would be that there is a positively charged nucleus attracting a negatively charged electron that is confined (arbitrarily) to discrete energy levels. In this case history suggests it is the detail in the successive theories that gives rise to the successive modifications. First, the finite nuclear mass, then the degenerate elliptical orbits, where the degeneracy is removed in an electric field (the Stark effect), then the relativistic effects, all seem to be understandable modifications of the immediately prior theory. It does not seem

⁷⁸ Nolan, D. *ibid* p. 273.

reasonable to see them simply as novel predictions of a general theory or a meta-theory. For example, there seems to be nothing in the hypothetical 'Nolan' theory above that suggests the explanation of the Stark effect, and hence the propriety of treating it as a novel prediction.

It is useful to consider in detail another example, which is perhaps more typical of science, in that it does not involve a revolution like the quantum theory of the atom. (I will return to the molecular theory of gases, and the fact that it lends itself to the Nolan interpretation.)

We can take a simple contemporary theory like that describing the plastic deformation of ductile metals at low temperature⁷⁹. The phenomenon of the increasing stress required to produce a given extension (or, more generally, strain) as plastic deformation proceeds is known as work-hardening. The phenomenon will be familiar to anyone who has observed a blacksmith at work. After a certain amount of hammering a horseshoe into shape, it must be strongly heated before it can be shaped further. The heating allows the dissipation of the work-hardening, because at high temperatures atoms can move about readily in the metal, restoring it to something like its state prior to the hammering.

The beginning of a satisfactory work-hardening theory was the postulation in 1934 of the dislocation by Taylor⁸⁰. In the same year both Orowan and Polanyi, separately, also postulated the dislocation to explain the anomalously low stress at which plastic deformation occurred in many pure metals, but only Taylor offered a theory of work-hardening. The dislocation is a line imperfection in the crystal lattice; one type can be thought of as the line bounding an extra half plane of atoms inserted into an otherwise perfect crystal. The 'Nolan' meta-theory might be that plasticity takes place by dislocation movement. The work-hardening

⁷⁹ Low temperature does not refer to being near to the absolute zero, but is with reference to the melting point, T_m , and is commonly taken approximately to be $< T_m/4$ where T_m is expressed in kelvin.

⁸⁰ Taylor, G.I. 1934 'The Mechanism of Plastic Deformation of Crystals 1.Theoretical' *Proc Roy Soc London A* 145, 362-387.

theory being discussed conforms well to McMullin's criteria for having high P-fertility. It has been modified extensively and successfully since the dislocation was first proposed to explain plasticity in metals.

As mentioned above, there was a prior problem to the work-hardening problem, namely the inexplicably low stresses at which pure metals deform plastically. Plastic deformation takes place by shearing one crystal plane over another. The preferred planes on which this glide (or slip) occurs are the most densely packed (or widely spaced) in the particular crystal structure. Calculation of the stress required to move one crystal plane over another gave enormously greater values than those observed experimentally. The dislocation, a line imperfection in the crystal lattice, allowed this process to occur sequentially across the plane, rather than everywhere at once. (The common analogy is the greater ease of moving a carpet by moving a ruck across it, rather than moving the whole carpet at once.) Thus the plastic properties of crystalline solids are not for the most part directly determined by the average behaviour of the atoms, but by that minority of atoms located at lattice imperfections. The accounts of the nature of the defect itself, and the nature of interactions between dislocations, and the development of the dislocation distribution during plastic deformation, have all been the subject of successive and successful refinements. The present theory is a recognizable and greatly improved version of Taylor's original proposal.

Taylor, very reasonably in the absence of any data, made the simplest possible assumption, namely that the dislocations were arranged in a regular three dimensional array of positive and negative dislocations. He arrived at a value for the flow stress, and explained the increase in flow stress with continuing deformation as a result of increasing dislocation density. That is, a decrease in the spacing of the dislocations in the regular array.

Taylor's theory gave no mechanism for this postulated increase in dislocation density. Frank and Read⁸¹ suggested an ingenious mechanism for dislocation multiplication, the so-called Frank-Read source. It is important to note that this had general ramifications for further development of work-hardening theories, and that its invention was driven by the need to explain the dislocation multiplication demanded by the earliest work-hardening theory. That is, it was not obvious that a dislocation multiplication mechanism was needed simply from the postulation of dislocations to explain the anomalously low initial resistance to plastic deformation.

Taylor had evidence mainly from polycrystals, the usual form in which metals are found. In polycrystals the atoms are everywhere arranged in a regular three dimensional lattice; small volumes (called grains) are differently oriented in space and are separated by grain boundaries. It is however possible to grow single crystals of metals by a variety of techniques, enabling experimental results that are more amenable to theoretical treatment. In the development of the theory of work-hardening, the most important piece of experimental evidence was that if a graph is plotted of stress against strain for a single crystal deformed in tension, it shows three distinct stages, named Stage I, Stage II, and Stage III. Stage I is approximately linear and shows a very low gradient (the work-hardening rate is this gradient), Stage II is steep and also linear, and finally in Stage III the work-hardening rate falls steadily, the curve being approximately parabolic. These are the central facts a work-hardening theory must account for – ideally, quantitatively.

In Stage I, dislocations move predominantly on a single slip system, which is that on which there is the highest resolved shear stress – the primary system. The slip systems (slip planes and slip directions) are determined by the crystal structure. Early writers attributed the work-hardening in Stage I simply to the

⁸¹ Frank, F.C. and Read, W.T. 1950 'Multiplication Processes for Slow Moving Dislocations' *Physical Review* 79, 722-723.

increasing primary dislocation density raising the stress necessary to force dislocations past one another. Subsequently, the importance of intersecting dislocations as impediments to movement was proposed. In fact detailed mechanisms for creating immobile dislocation barriers were postulated. The history of the subject (not just in relation to Stage I but quite generally) shows a constant iterative communication between theoretical studies of the detailed nature of the geometry of possible dislocation interactions on the one hand, and theories of the nature of the elastic interactions between postulated dislocation configurations on the other. While it is logically possible that each could have proceeded without the other on the basis of some meta-theory, the evidence is that a development like the recognition of the possibility of the Lomer-Cottrell lock⁸² fostered a variety of ideas about possible contributions to work-hardening of interaction between primary and secondary dislocations. The concept of novel prediction as Nolan outlines it is not appropriate. Certainly, the existence of Lomer-Cottrell locks was predicted and subsequently observed, but the work-hardening theories which incorporated this were modifications of earlier theories, which did not directly depend on this specific dislocation barrier, but rather in a general way on the now more plausible postulation of a significant role of the interaction between primary and secondary dislocations. Thus while one could imagine that the postulation of the Lomer-Cottrell lock could arise from a general theory such as I have hypothetically attributed to Nolan, it is simply not the case that the development of the Stage I theory of work-hardening can be seen as arising from the general theory, rather than from the then current most plausible theories.

If we turn to Stage II, the regime of steep, approximately linear hardening, there are similar difficulties for the Nolan stance. For example, the Cottrell-Stokes Law

⁸² Lomer, W.M. 1951 'A Dislocation Reaction in the Face-Centred Cubic Lattice' *Philosophical Magazine* 42, 1327-1331; Cottrell, A.H. 1952 'The Formation of Immobile Dislocations during Slip', *Philosophical Magazine* 43, 645-647.

was found as a result of experiments specifically conducted to attempt to resolve conflicts between different then current theories of Stage II work-hardening⁸³.

The law states that the ratio of the temperature dependent and temperature independent contributions to the flow stress is approximately constant in Stage II. Now this is important in ruling out certain possible work-hardening mechanisms, and in supporting the belief that the dislocation distribution remains similar, changing only in scale, during Stage II⁸⁴. There is nothing in the hypothetical general theory that could have led to the prediction of the Cottrell-Stokes Law. One might claim that the general theory had led to the genesis of the conflicting theories, which in turn led to the Cottrell-Stokes Law. However, this is not novel prediction in any plausible sense.

Of course it is not possible to prove non-existence but the burden of proof is surely on Nolan to justify the profoundly implausible claim that there will in general be a theory which lends itself to allowing the construal of the complex interaction between theory and experiment, which seems almost invariably to be the process of generating theory change, as being the outcome of novel prediction from such a hypothetical theory.

It is now generally agreed that the roughly parabolic Stage III work-hardening is explained by the exhaustion of work-hardening. The dislocation sources now operating are predominantly the edge dislocation dipoles generated in the earlier stages of deformation⁸⁵. Progressively narrower dipoles become available as the stress is raised, and because the number of dipoles per unit volume is greater the narrower the dipoles, the work-hardening rate falls. The important point for

⁸³ Basinski, Z.S. 1959 'Thermally Activated Glide in Face-Centred Cubic Metals and its application to the Theory of Strain Hardening' *Philosophical Magazine* 4, 393-432.

⁸⁴ See for example Kuhlmann-Wilsdorf, D. 1985 'Theory of Workhardening 1934-1984' 50th Anniversary of the Introduction of Dislocations, *Metallurgical Transactions* 16A, 2091-2108; Hirsch, P.B. 1975 'Work Hardening' *The Physics of Metals 2 Defects* Cambridge: Cambridge University Press pp. 189-246.

⁸⁵ Brown, L.M. 2005 'Unifying concepts in dislocation plasticity' *Philosophical Magazine* 85, 2989-3001.

our purposes is that again this account is not in any way even foreshadowed by the meta-theory. Its genesis lies primarily in the transmission electron microscope observations of the very high density of edge dislocation dipoles, which was not predicted by theory.

The point illustrated by these two examples is that any detailed examination of the history of a long standing successful theory is likely to show that it is the specifics of the current theoretical and experimental situation that leads to the next development. Nolan's view seems plausible if we consider successive versions of the kinetic theory of gases based on the theory that gases consist of molecules. Here it seems obvious that the finite size and attractive forces, at first ignored, will be incorporated in further advances. However, this is a rather odd case. In general it seems wholly implausible that a general theory or a meta-theory should contain within it, as novel predictions, the future discoveries in its domain. First, these are commonly the outcome of unexpected and unpredictable experimental discoveries. The contrary view may result from the tendency of philosophers of science to focus on seminal historical experiments, designed to test an important theory, and the parallel tendency to ignore the sheer adventitiousness of scientific developments. Certainly, 'In the fields of observation, chance favours only those minds that have been prepared', as Louis Pasteur famously remarked, but few scientists would deny the role of chance. Second, the iterative interaction between theory and experiment in a research programme, as described in the brief history of work-hardening theory, is the norm in science. It is difficult to see how the general theory or the meta-theory proposal can encompass this.

It is important to recognize that the P-fertile theory is the original Taylor theory and its successors, up to today's sophisticated theory. That is a theory, as McMullin would see it, involving the early simple proposal and now incorporating dislocation locking mechanisms, dislocation sources, the Cottrell Stokes Law, the existence of dislocation dipoles and a great many other innovations forced by

experimental results. These experimental results themselves were often the result of theory driven investigations (from successive forms of the theory) and sometimes adventitious observations by a variety of experimental techniques.

Now it can be said that the current theory does not in fact enjoy a superior status because of its P-fertility as opposed to, for example, its power at predicting and explaining the evidence. Indeed this is true, but McMullin is not setting these against one another. His claim (accepted by Nolan) is that P-fertility and success can be shown historically to be commonly conjoined. Nolan, eschewing detail, gives us no grounds for the central supposition that there will in general be a suitable general theory or meta-theory, and the claim I make is that this is implausible. My one detailed example obviously does not constitute proof since Nolan does not claim that there will be a meta-theory in every case. My contention rather is that the example is typical of science, particularly in the messy areas which constitute the bulk of contemporary science. The kinetic theory of gases is a fine exemplar of science at its most nearly logical, as opposed to merely rational, and is quite unusual.

There is another distinct problem for Nolan, namely that there is a difficulty in deciding what the general theory or meta-theory might be. Why in my earlier example is it the Bohr Theory rather than Rutherford's plum pudding model? The choice seems arbitrary. For example, I might have chosen to foist on Nolan the general theory that dislocations are responsible for work-hardening, rather than the somewhat vaguer general theory that they are responsible for plasticity. It happens that the 1934 Taylor paper (that corresponds to the first) was submitted before the 1934 Polanyi and Orowan papers (that correspond to the second) but it actually appeared later. If the general theory is chosen with historical hindsight (assuming that one can be found that meets the requirement of resulting in the appropriate novel predictions) can these be considered as novel predictions of the proposed general theory? In one way it might be argued that Nolan simply has to find a theory which can accommodate the successive versions of the P-

fertile theory under consideration, since this theory was itself viewed with hindsight as P-fertile. This I have argued is in general implausible. However, it is a further nice question as to whether one can properly view the consequences of such a hypothetical general theory, proposed with hindsight, as novel predictions. The novel prediction argument is not that it confers logical plausibility on a theory but that it downgrades the likelihood of ‘fudging’, and this hardly seems applicable to the case we are now considering since the meta theory is merely hypothetical.

In summary, the essence of McMullin’s argument is that the successive successful modifications of numerous major scientific theories would be an inexplicable historical phenomenon were it not for their increasing approximate truth. In addition, he is claiming that it is the metaphorical suggestiveness of the successive approximately true theories that generates the successive theories. Nolan’s account does not explain the claimed empirical historical fact that long standing successful theories are P-fertile. Consequently, McMullin’s claim still requires a rebuttal from Scientific Anti-realists, either via a historical analysis which successfully rejects the empirical claim he makes, or by some new counter argument.

1.8 The ‘No Miracles Argument’

Whether one accepts Nolan’s argument or not, it is reasonable to ask how McMullin’s fertility argument differs from the standard ‘no miracles argument’ (NMA) usually associated with Putnam⁸⁶. Why is the particular aspect McMullin emphasizes, namely his P-fertility, more persuasive than the broad general argument that the success of science as such should convince us of Scientific Realism?

⁸⁶ Putnam, H. 1975 *Philosophical Papers* Volume 1, Mathematics, Matter and Method Cambridge: Cambridge University Press p. 73. The priority for the ‘no miracles’ argument actually belongs to Smart (Smart, J.J.C. 1963 *Philosophy and Scientific Realism*, London: Routledge and Kegan Paul p. 39).

The well known Smart/Putnam argument says that since science is successful and since true theories would be successful, the success of science would be a miracle if scientific theories were not approximately true (the assumption here is that approximately true theories share to a substantial degree the success that true theories would have)⁸⁷. This argument is what Peirce would have called abduction⁸⁸, and one counter is that one individual's abductive argument is another individual's affirming the consequent, as Laudan has forcefully argued⁸⁹.

It can be argued that since McMullin's argument necessitates Inference to the Best Explanation, it is superfluous since, given this acceptance, the NMA covers the ground. However, this is not right. The NMA explains success while the McMullin argument explains the conjunction of success and P-fertility. It may well be that there are Anti-realist arguments which explain the former and not the latter, leaving intact McMullin's claim that his argument is the best defense of Scientific Realism. McMullin himself seems to want to avoid using the term Inference to the Best Explanation and to talk in terms of inferring to a good explanation⁹⁰. This is because we can never know that we have the best explanation. In this context this does not seem to be a significant caveat; the Putnam and the McMullin arguments are what is normally termed an Inference to the Best Explanation (or affirming of the consequent).

⁸⁷ This is not uniformly accepted. Some writers maintain not only that the success of science is not a miracle, but to be expected, since false theories are often successful, indeed nearly all, if not all, successful theories are false (Horwich, P.G. 1991 'On the Nature and Norms of Theoretical Commitment' *Philosophy of Science* 58, 1-14 p. 9). The defenders of the NMA usually ascribe an important role to successful prediction as part of success to further their argument for the meaningfulness of the NMA (see for example Wright, J. 2002 'The Explanatory Role of Realism' *Philosophia* 29, 35-56).

⁸⁸ Sometimes called retrodution or hypothetic inference (Peirce, C.S. 1935 *The Collected Papers of Charles Sanders Peirce* C. Hartshorne and P. Weiss (Eds) Cambridge MA: Harvard University Press, 5.189)

⁸⁹ Laudan, L. 1981 'A Confutation of Convergent Realism' *Philosophy of Science* 48, 19-48. Laudan draws attention to past successful theories now known to be false, thus denying that success implies truth or approximate truth. Others have drawn attention to the problem of unconceived alternatives, giving historical examples of current theoretical accounts, unconceived of at an earlier time, and for which evidence was available at that time, while an earlier theory was being championed as the best explanation (Stanford, P.K. 2006 'Darwin's Pangenesis and the Problem of Unconceived Alternatives' *British Journal for the Philosophy of Science* 57, 121-144).

⁹⁰ McMullin, E 1992 *The Inference that Makes Science* Milwaukee: Marquette University Press.

In this context it is worth considering a critique of the NMA which would at the same time undermine McMullin's argument if the critique was sound. Van Fraassen⁹¹ has proposed a skeptical explanation of the success of science. Thus he rejects the NMA and advances an 'evolutionary' argument – only the successful theories survive. However, this is specious; it explains why we have successful theories but not why they are successful, as Leplin⁹² has pointed out. Competition explains why Wimbledon finalists are great tennis players or British Open winners are great golfers, but not why Rod Laver was the former and Jack Nicklaus the latter⁹³. Sankey, who accepts the NMA as one strand in the argument for Scientific Realism, argues that the methods of scientific theory selection are truth conducive and thus that true or approximately true theories are selected. Hence van Fraassen is right, but in addition the explanation of why the selected theories are approximately true is that this particular 'evolution' is not 'blind', like natural selection, but is directed towards truth by the methodology of science⁹⁴. This last is dependent *inter alia* on the resolution of the difficult question of arriving at a plausible account of something general but distinctive that constitutes the method of science, and showing that it selects true, not merely successful, theories. Boyd⁹⁵ has earlier argued on empirical grounds for this truth-conducive view of scientific practice; however the argument must seem circular to the Anti-realist. This last does not of course mean that the argument is invalid.

⁹¹ van Fraassen, B.C. 1980 *The Scientific Image* Oxford: Clarendon Press p. 40.

⁹² Leplin, J. 1997 *A Novel Defense of Scientific Realism* Oxford: Oxford University Press pp. 8-9. In fairness to van Fraassen, he is in fact claiming only that his account explains why we have successful theories, because he believes this is sufficient to dissipate the force of the NMA (van Fraassen 1980 *ibid* p. 219 footnote 34).

⁹³ Weston (Weston, T. 1992 'Approximate Truth and Scientific Realism' *Philosophy of Science* 59, 53-74, p. 39) argues that the van Fraassen proposal is inadequate on a different ground, namely that it does not explain successful novel prediction. But van Fraassen could respond that this is a feature considered in retaining or discarding theories.

⁹⁴ Sankey, H. 2004 'Scientific Realism: An Elaboration and a Defense' *Knowledge and the World: Challenges beyond the Science Wars* M. Carrier, J. Roggenhofer, G. Küppers and P. Blanchard (Eds) Berlin: Springer Verlag pp. 69-72.

⁹⁵ Boyd, R. 1990 'Realism, Approximate Truth, and Philosophical Method' reprinted in D. Papineau (Ed) 1996 *The Philosophy of Science* Oxford: Oxford University Press pp. 215-255.

Without claiming that there is something unique that can be called the scientific method or that such a method is truth conducive, it seems that it may be legitimate to suggest that science is designed to increase our factual information about the world rather than to generate true theories; that is science generates instrumental success and information. Will⁹⁶, who is in fact a Realist, put it as follows.

The practices of science are practices developed for the exploration of nature; they are the gears and levers of a great engine that has been fashioned over the years for the purpose of expanding knowledge of a certain kind, of using achieved knowledge to generate further knowledge ...

Brown⁹⁷ has offered a different objection to van Fraassen's 'evolutionary' argument, which, Brown claims, assumes that rational choice and success go together. But Brown points out that the former often includes super-empirical criteria, so that the theory rationally chosen may not be the most successful. Hence, the 'evolutionary' argument gives no explanation of the fact that the historically chosen theories are successful.

Van Fraassen's argument in any case does less than justice to Darwin's theory, since it implies that successful theories survive because they are successful. Darwin is sometimes wrongly criticized on the grounds that his arguments, like this one, are tautological. Darwin himself gave accounts as to what were the specific competitive advantages that fostered the survival and reproductive success of a plant or animal species under the relevant environmental circumstances. These were not necessary and sufficient conditions but were plausible accounts as to why a particular set of environmental circumstances probably existed and would have favoured a particular heritable feature of a given species⁹⁸.

⁹⁶ Will, F.L. 1997 *Pragmatism and Realism* Lanham: Rowman and Littlefield p.98.

⁹⁷ Brown, J.R. 1985 'Explaining the Success of Science' *Ratio* 27, 49-66.

⁹⁸ Thus, for example, competition for scarce reproductive opportunities is a persuasive explanation for the development of the horns of stags via natural selection through direct

McMullin would also argue against van Fraassen that it is peculiarly convincing that one can in practice repeatedly find a modification to deal with new facts. If the revised theory were not a step in the direction of more nearly representing reality then it is highly unlikely that it would continue to encompass the original facts and the new ones. A part of this argument is the metaphor parallel and particularly the extended success over time of the progressively modified theory. It is each successor theory that suggests its own successor, and again this is an empirical observation, according to McMullin. A succession of theories, each no nearer to reality, will not lead to empirically adequate theories. This is what would be too much of a coincidence.

1.9 Conclusion

The picture put forward by McMullin, of scientific theory as metaphor, has much to recommend it. In particular, as I proposed in my analysis of the history of the development of the theory of work-hardening in section 1.7, his historical viewpoint and his metaphor concept attempt to deal with the messy diffuse areas typical of science in practice. This contrasts with the concentration on mathematical physics which, for example, characterizes the seminal work of the Logical Positivists. Mathematical physics, while perhaps rightly seen as the most developed aspect of science, is in fact atypical.

McMullin sees good metaphor as an intrinsic part of the creative scientific process. In developing new ideas on the basis of existing theory there will need to be some sort of extension, in part via analogical reasoning from existing ideas, thus enabling a move beyond preconceptions. As Norris puts it⁹⁹ ‘...some metaphors prove themselves capable of progressive ‘rectification’ through a

competition and/or sexual selection by the hind. Numerous well documented examples have been given since Darwin wrote. The visibility to bees of UV radiation (which is more readily polarized on reflection than light in the human visibility range) is one: bees make use of the polarization of scattered sunlight for orientation. The adaptation of moth’s colouring to the industrial environment in the UK as a survival advantage is another.

⁹⁹ Norris, C. 1997 *New idols of the cave* Manchester: Manchester University Press p. 96.

process of jointly conceptual and empirical research'. The importance of metaphor in the advance from pre-scientific thought has been discussed at length by Derrida, who emphasized that much of what we take to be literal, in philosophy as elsewhere, is also metaphorical. Many followers of Derrida have focused almost exclusively on his point that the metaphorical characteristics of a language limit the possibility of mastering the subject under discussion. Norris¹⁰⁰ argues that this is to neglect Derrida's significant and detailed argument concerning the direct relationship between the use of metaphor and scientific creativity and theory development.

McMullin, within the framework of his notion of scientific theory as metaphor, offers a specific argument that P-fertility is a virtue independent of the other virtues, and is found to be commonly present in successful theories. This he argues is inextricably connected to the metaphorical role of theory, and both are explained by the substantial truth of the incomplete theories. Hence Scientific Realism is vindicated. The argument, as has already been said, is an Inference to the Best Explanation.

The importance for epistemology of a belief in or a denial of Scientific Realism warrants a more detailed study of the validity of the conjunction of P-fertility and success as a criterion of an approximately true theory. The diachronic view of theories is an essential element of the P-fertility discourse and is examined in Chapter 2. The notion of approximate truth or some surrogate for it seems to be an essential element in McMullin's case for Scientific Realism, despite his own disavowal of the need for the concept, and is the subject of Chapters 3 and 4.

¹⁰⁰ Norris, C. 1998 'Deconstruction, Post Modernism and Philosophy of Science: Some Epistemological Bearings' *Cultural Values* 2, 18-50.

CHAPTER 2 THE DIACHRONIC VIEW OF THEORIES

2.1 Introduction

A necessity for McMullin's case for Scientific Realism is the conjunction of P-fertility and long term success. It will be recalled that this conjunction, which I will refer to as the McMullin Conjunction, has been assumed to hold. This in turn requires that we accept a sequence of theories for consideration as one long standing successful theory. McMullin argues that a detailed account of what this acceptance entails is not needed. However, this is not obviously the case and the issue of what is required for an account of diachronicity is pursued in this chapter.

A possible objection to the abductive argument in favour of Realism is that the very notion of theory identity across time is only supportable from a Realist position. Consequently, it might be claimed that the argument begs the question against Anti-realist views (who won't grant the terms in which the argument is framed). The objection is refuted by showing how to make sense of theory identity across time from the major Anti-realist positions. Moreover it is necessary to examine the implications of P-fertility and how it relates to various Anti-realist points of view. If an alternative Anti-realist position is as good as or better than that of the Scientific Realist, when the McMullin Conjunction and hence diachronicity are accepted, McMullin's abductive argument for Scientific Realism becomes problematic.

In a situation where a theory is modified, what criteria entitle one to classify the outcome as a new theory, and conversely what criteria enable it properly to be classified as a modification of the existing theory? What are the essential features of this second case where the current and the earlier theory can be treated as one theory? It is part of the notion of P-fertility that some temporal sequence of theories $T_1 T_2 T_3$, which might be regarded as distinct theories, can

legitimately be regarded as successive versions of a theory T. Is there a basis for this legitimation?

McMullin writes¹⁰¹ in relation to this question that the unit of appraisal for his judgment of P-fertility is, '... a historical explanatory system which may undergo numerous modifications over the years, and yet be presumed still the "same" theory, on the basis of some usually not-very-well-defined continuities of basic structure'. He gives some further clues as to what is intended when he writes¹⁰² of the usage of scientists, '... who will speak of the "wave-theory of light" for example, as something which developed over several centuries'. Elsewhere he gives an indication of the bounds of his proposed categorizations when he states that the career of the Bohr Theory of the H-atom from 1911 to 1926 is unequivocally one theory¹⁰³, whereas Thompson's electron and Dirac's have little in common¹⁰⁴.

The 'usage of scientists' which McMullin invokes is inappropriate as a guide. Scientists will quite properly speak very broadly of theories of the solar system or theories of electron diffraction, or, alternatively, quite specifically of theories of the action of a particular catalyst in a specific chemical reaction. There is no implication in the former broad usage that the theories which fall under the umbrella term 'wave theories of light' may not be radically different. McMullin perhaps makes his position clearest when, in response to a criticism of his alleged failure to enable a distinction between two stages of the same theory or two distinct theories, he writes¹⁰⁵:

Are continental drift and the plate tectonic model two stages of the same theory or two different theories? It all depends on how 'theory' is defined and

¹⁰¹ McMullin, E. 1976 'The fertility of theory and the unit for appraisal in science' in R.S. Cohen *et al* (Eds), *Essays in Memory of Imre Lakatos*, Dordrecht: D. Reidel, p 417.

¹⁰² McMullin, E. 1976 *ibid* p. 416.

¹⁰³ McMullin E. 1976 *ibid* p. 420.

¹⁰⁴ McMullin, E. 1984 'A Case for Scientific Realism' in *Scientific Realism* J. Leplin (Ed) Berkeley: University of California Press p. 29.

¹⁰⁵ McMullin, E. 1984 *ibid* pp. 32-33.

how sharply theories are individuated. I do not see that very much hangs on this decision, one way or another.

The important thing to note is that there *are* structural continuities from one case to the next, even though there are important structural modifications.

What 'hangs on this decision' in the general case (i.e. not confining ourselves to the motion of the continents) is whether it is possible to give a meaningful account of P-fertility. If the ideal gas theory and the van der Waals theory are distinct theories, then it is not possible to say that a unit for appraisal (and for the judgment of P-fertility) is the van der Waals theory viewed as an historical entity resulting from the modification of the earlier accounts of the behaviour of gases. This seems clearly to be a truism if one goes to an extreme case and tries to claim (as McMullin does not) that the special theory of relativity is P-fertile because Aristotle's and Newton's mechanics have undergone successive modifications arising from the metaphorical suggestiveness of Aristotle's and then Newton's accounts of the motion of projectiles.

The foregoing is not intended to suggest that an argument cannot be mounted to permit a distinction between modification and novelty. It may well be that a 'not-very-well-defined' account of what constitutes a single historical theory can sometimes make this distinction. Indeed, as well as clear discontinuity, which has just been instanced, there are innumerable cases where a theoretical account is subject only to a minute modification.

In section 2.2 I consider how one might give an account of diachronicity and of continuity versus novelty. In section 2.3 I consider an alternative account (to that given by McMullin) of the McMullin Conjunction which is available to the Scientific Realist. In section 2.4 I examine this stance from within Anti-realism. In section 2.5 I look at how these considerations determine the meaningfulness of the concept of P-fertility under some prominent Anti-realist stances. In section 2.6, in the light of these alternative views, I briefly examine how McMullin's account of the metaphorical suggestiveness of theories relates to scientific practice.

2.2 What Makes Theories T_1 and T_2 a Single Theory?

Consider a temporal sequence of theories in a given domain. Some of these have been discarded altogether, others still have some adherents, and the current most widely accepted theories in the relevant scientific community are generally regarded as better theories than all of their predecessors. Better does not mean simply containing fewer falsehoods; indeed the opposite may often be true. To say that gases consist of molecules is an inferior theory to one that is more specific, more explanatory, more informative, and less likely to be wholly true. In the temporal sequence T_1, T_2, T_3, T_4 , it may be that the theory T_3 , seen from the standpoint of T_4 , is unquestionably inferior to T_2 . It is perhaps now perceived to have been a fortunate (but not necessarily accidental) cancellation of errors that led to its empirical adequacy. Nevertheless, for the Scientific Realist there is a true theory T , and it may be that T_4 is further from T than T_3 despite this perception.

The claim made by McMullin is that the temporal sequence (within what Kuhn would call normal science) can be taken to be one long standing successful theory in so far as there has not been the introduction of some radical new concept. The long standing success is the ongoing ability to make successful predictions and account for new observations. This, together with P-fertility (the ongoing capacity for effective modification via the theory's metaphorical suggestiveness and the scientist's creative input), then grounds the hypothesis that the sequence of theories is such that the latest is nearest to the true theory T , and that each of the earlier theories is better than its predecessor and was also in some sense near to T .

There is no basis for an assumption that there exists a unique temporal sequence. To take just one example, the various successive theories of the structure of DNA of Watson and Crick prior to 1953 were different from those of

their contemporaries Pauling or Wilkins or Franklin¹⁰⁶. These in turn were different from one another. It is simply not the case that there was a unique series of theories or that the later theories were always better than earlier ones. In general the McMullin position cannot rest on a simple notion of temporal sequence. There is an implicit notion of historical distance and an ordering (with the wisdom of hindsight) of the successive consensus positions attained at various times by the relevant scientific community as a whole. Indeed the thrust of McMullin's arguments is that a marriage is needed between the history and the philosophy of science¹⁰⁷.

It is difficult to give a precise account of what constitutes a single theory to be viewed diachronically, and McMullin does not regard it as important to do so. My proposal as to what makes theories T_n and T_{n+1} historical versions of a single theory T apt for consideration with respect to P-fertility is the following:

- (i) T_{n+1} contains a significant set of significant claims made in T_n and
 - (ii) there is an appropriate causal chain linking T_n to T_{n+1}
- or
- (iii) $(\forall T_i)(\forall T_j) \{(\exists T)(T_i \approx T \ \& \ T \approx T_j) \rightarrow T_i \approx T_j\}$

Clause (i) is one requirement if two theories are regarded as being part of one ongoing theory; if no significant claim were retained the two theories would commonly be distinct. Clause (ii) incorporates McMullin's point that the earlier version of the theory is important in the genesis of its successor and in particular excludes the case of distinct rival theories with a common claim. Clause (iii) is to ensure transitivity: theories count as diachronically similar (symbolized by \approx) if they emerge via a process of evolution through diachronically similar theories.

¹⁰⁶ Watson, J.D. 1968 *The Double Helix* London: Weidenfeld and Nicolson; Wilkins, M.H.F. 2003 *The Third Man of the Double Helix* Oxford: Oxford University Press.

¹⁰⁷ McMullin, E, 1975 'History and Philosophy of Science: A Marriage of Convenience?' *Boston studies in the philosophy of science* 32, 585-601.

I will call these the 'Continuity Conditions'¹⁰⁸. There is of course here an implied notion of similitude in allowing for a modifiable long standing theory, in that the successive versions of the theory bear a resemblance to their predecessor. However, there is here no problem with the context dependence of this notion, nor need there be the notion of any of the theories being approximately true. (The significance of this point about approximate truth is taken up in Chapters 3 and 4.)

In accepting McMullin's historical claim, as I have done throughout, the diachronic view of theories has necessarily been accepted. In the foregoing I have simply given a more explicit account than is given by McMullin of what constitutes a single theory.

An immediate question is what is meant by 'significant' in (i). It is not difficult to give plausible examples from the history of science. For many cases it seems that a claim can be said to be significant if it plays an essential explanatory role. For example, species variation and natural selection are significant claims in the theory of evolution, the impossibility of two electrons with the same spin occupying a given state is a significant claim in the theory of the Periodic Table, and the existence of molecules is a significant claim in the kinetic theory of gases. It is however difficult to be more precise when considering the broad spectrum of science.

We can illustrate the desiderata in a little more detail by viewing them in the light of the theory of work-hardening described in section 1.7. The claim that there are dislocations is significant and distinguishes the Taylor theory from earlier theories. There is structural continuity in that the entity responsible for the plasticity is the dislocation. The dislocation has progressively been described in more detail; first the simple edge dislocation, then the screw dislocation, and later

¹⁰⁸ There is nothing in these criteria that depends on an assumption of Scientific Realism or Anti-realism.

the partial dislocations possible in various crystal structures. The stress field around a dislocation and the nature of dislocation interactions has been amplified and refined. In the successive refinements of the theory changes have been incremental rather than radical. For example, the account of the plasticity of precipitation hardened aluminium alloys required the introduction of the stress field around precipitate particles and the interaction of dislocations with these. The various developments described in section 1.7 have all been accepted in broad outline as refinements of the theory. Finally, no major feature of what G.I. Taylor described in his 1934 paper has been discarded.

Einstein's rejection of absolute space, Lavoisier's rejection of phlogiston, Planck's introduction of quanta in place of the continuous nature of energy transfer, are all examples of the arrival of a new theory. For example, in the case of the quantization of energy, the application of classical mechanics and electromagnetism ceased to be tenable in discussing atomic structure, and indeed in principle elsewhere. Of course, between these instances and the work-hardening theory I have taken as an example, there are borderline cases. This is inevitable; 'being a new theory' is a vague predicate. Given this vagueness of the criteria, the difficulty is to establish the general legitimacy of treating a so called 'long standing successful theory' as a single theory undergoing modification. The problem thus remains that, even if my Continuity Conditions are acceptable as far as they go, it is unclear whether different historians of science would draw the same conclusion about continuity in the different cases of importance to McMullin's argument.

2.3 An Alternative Realist Explanation of P-fertility

As already discussed in section 1.2, McMullin's claim is that the contingent historical fact, that I have labeled the McMullin Conjunction, 'finds its best explanation in a broadly realist account of science'.¹⁰⁹ In the paper just cited he

¹⁰⁹ McMullin, E. 1984 'A Case for Scientific Realism' in *Scientific Realism* J. Leplin (Ed) Berkeley: University of California Press, p. 34.

gives a historical account of a series of scientific explorations emphasizing the 'structural types of explanation and ... the role played by the criterion of fertility in such explanations.'¹¹⁰ In essence, the illustrations he gives are of the naturalness of the development of the theories on the basis of the assumption that each phase of the development of the theory successfully represents the structure of the real.

It is useful to quote him in relation to a particular example¹¹¹.

The complex molecules of both inorganic and organic chemistry have been more accurately charted over the last century. The atomic constituents and the spatial relations among them can be specified on the basis both of measurement, using X-ray diffraction patterns, for example, and on the basis of a theory that specifies where each kind of atom *ought* to fit. ...

These structures are coming to be more exactly charted, using a variety of techniques both experimental and theoretical. The coherence of the outcome of these widely different techniques, and the reliability of the chemist's intuitions as he decides which atom must fit in a particular spot in the lattice, are most easily understood in terms of the realist thesis.

This account, which McMullin rightly remarks is characteristic of much of contemporary science, emphasizes the 'chemist's intuitions' and it is these creative intuitions which are engendered by the approximate truth and hence the metaphorical suggestiveness of the current version of the theory. That is, it is the fertility, born of the approximate truth of the current interim version of the successful theory, which results in these reliable intuitions.

For the Scientific Realist, in the light of the Continuity Conditions set out in section 2.2, there is an alternative explanation of the McMullin Conjunction which

¹¹⁰ McMullin, E. *ibid* p.26.

¹¹¹ McMullin, E. *ibid* p. 28.

does not however provide support for McMullin's case for Scientific Realism. The origin of both the success and the P-fertility is simply the true claims in the successive versions of the theory¹¹². This account does not require that the successive versions of the theory are approximately true. Thus, to take the example discussed in detail in Chapter 1, the assertion that there are dislocations does a great deal to explain the P-fertility of work-hardening theory without making the assumption that the Taylor theory and its various successors are each approximately true.

2.4 Anti-realist Truth and P-fertility

A move for the Anti-realist in giving an account of the McMullin Conjunction is to make a claim like that just outlined, namely that the theories satisfying the Conjunction contain significant truths. The question that needs to be faced is whether this claim has sufficient explanatory power when truth is understood Anti-realistically.

This question could be tackled in two ways. One might fix on the notion of truth admitted by the Anti-realist and then ask whether this has adequate explanatory power. The disadvantage of this is that there is no generally accepted Anti-realist account of truth¹¹³. Alternatively, one could see what demands the explanatory project here places on the notion of truth and then examine whether any of them runs counter to Anti-realism. Because of the problem of finding a sufficiently widely Anti-realistically acceptable notion of truth I shall adopt the second procedure.

What we want to explain is that a long-standing successful theory is P-fertile. Assume that such a theory contains significant truths. Assume too that truth is a

¹¹² This point is taken up later in the chapter when considering the different accounts of the scientific enterprise.

¹¹³ For example, in discussing Internal Realism (which is considered in Chapter 6) Putnam in his later writings resists giving an explicit account of truth.

stable property of claims: if a claim is true then it continues to be so¹¹⁴. If our theory does contain significant truths then these truths continue to be so. If these truths survive into (causally) successor theories then clause (i) in the Continuity Conditions is satisfied and clause (ii) is satisfied by the definition of succession. So the theories count as diachronically similar and we have P-fertility.

The theories are explained as being successful because they contain significant truths, so the presence of significant truths explains the McMullin Conjunction if we have reason to think that a theory containing significant truths is likely to pass them on to its successor. Nothing so far assumes anything more than that truth is stable and that is usually granted by the Anti-realist (e.g., it is one of the platitudes on Wright's minimal conception of truth¹¹⁵).

Thus the question is whether there is any reason to suppose that a theory will transmit its significant truths. There is no guarantee to be had here; nothing ensures that the evolution of a successful theory may not proceed via a stage where truths are lost. However we can legitimately build the assumption that they are not into our explanation. The McMullin Conjunction is explained by the fact that in general long standing successful theories contain significant truths which are passed on to their successors. Nothing thus far draws on a realist notion of truth unless it is supposed that if scientific practice tends to preserve truths this must be explained in Realist terms. However, there is no reason to question the adequacy of an Anti-realist notion of truth here, for if truth is constitutively linked to epistemological factors it will be exceptional for an implementation of epistemological procedures to involve the loss of truth. Hence an epistemological notion of truth which exhibits the property of stability suffices to explain the

¹¹⁴ Wright, C. 1987 *Realism, Meaning and Truth* Oxford: Blackwell Chapter 5.

¹¹⁵ Wright, C. 1992 *Truth and Objectivity* Cambridge MA: Harvard University Press p. 34.

McMullin Conjunction. Both superassertibility¹¹⁶ and idealized rational acceptability¹¹⁷ would seem to meet the requirement.

Thus if one accepts this account and that given in section 2.3 above, the empirical observation of the McMullin Conjunction is neutral between Scientific Realism and Anti-realism.

2.5 Anti-realism and P-fertility

2.5.1 Introduction

Four major types of theories of science that can be characterized as Anti-realist have been identified and extensively discussed in the literature¹¹⁸. (Nothing hangs on this characterization in my discussion of the theories, and some of the authors and/or the advocates of some of them regard them as Realist theories.) These are Structural Realism (Worrall), Entity Realism (Cartwright, Hacking), Instrumentalism (van Fraassen)¹¹⁹, and Internal Realism (Putnam)¹²⁰. They can be so characterized at least in the sense that they do not meet the account of Scientific Realism given in section 1.3. I very briefly discuss why I characterize these as Anti-realist in the consideration of each of them. I consider the nature of the explanation of the McMullin Conjunction in the context of the first three of these in the following sections¹²¹. If they can give a satisfactory alternative

¹¹⁶ A statement is superassertible ...if and only if it is, or can be, warranted and some warrant for it would survive arbitrarily close scrutiny of its pedigree and arbitrarily extensive increments to or other forms of improvement of information (Wright, C. *ibid* p. 48)

¹¹⁷ Putnam, H. 1983 *Philosophical Papers* Volume 3 Realism and Reason Cambridge: Cambridge University Press p. 84. Unlike rational acceptability, idealized rational acceptability is seen by Putnam as stable; his conception of idealized rational acceptability is discussed in Chapter 6.

¹¹⁸ See for example Psillos, S. 1999 *Scientific realism: how science tracks truth* New York: Routledge.

¹¹⁹ There are a number of distinct Instrumentalist theories. I have chosen to focus on van Fraassen's theory as this is currently widely regarded as the most plausible. Thus when referring to Instrumentalism I will be referring to this.

¹²⁰ I have indicated in parentheses here the principal recognized proponents of these theories. Van Fraassen calls his Instrumentalist position 'Constructive Empiricism' rather than Instrumentalism because he thinks scientific theories have a truth value (this he thinks is irrelevant to scientific practice). Specific references to these and other writers are given in the further discussion.

¹²¹ Putnam's Internal Realism is considered in Chapter 6.

account of the McMullin Conjunction, this historical observation is neutral between Scientific Realism and Anti-realism.

2.5.2 Structural Realism

As has already been mentioned, the history of science is littered with failed theories. Moreover, many of these have been successful for long periods and have successfully made predictions, and indeed have had all the hallmarks of theories currently accepted as true or at least approximately true. This has led to the so called Pessimistic Meta Induction (PMI) – the view that current theories are likely also to be false. I have already discussed some responses to this in Chapter 1, but one response, not discussed there, is of interest in the present context of concern with possible Anti-realist accounts of P-fertility and the McMullin Conjunction. This is the response given by Worrall¹²² and known as Structural Realism, whereby Worrall attempts to reconcile the contradictory PMI and NMA. How, he asks, can we reconcile the evident success of science and the conclusion therefore that we have latched onto truth about the world, and the equally evident periodic overthrow of well established successful theories? The latter show that numerous successful theories were not even approximately true on any interpretation of this expression.

Worrall confines his attempted reconciliation to mature science. He defines a mature science as one that is predictive of general *types* of phenomena, without those phenomena having been ‘written into’ the theory¹²³. For him Fresnel's optics is mature science, chemistry before Lavoisier is not. Worrall would characterize as mature any long standing successful theory, in McMullin's terminology, that had not been discredited (as the phlogiston theory for example has been).

¹²² Worrall, J. 1989 ‘Structural Realism: The Best of Both Worlds’ *Dialectica* 43, 99-124 (reprinted in D. Papineau (Ed) 1996 *The Philosophy of Science* Oxford: Oxford University Press). Worrall is non-committal in this article as to whether he sees himself as a Scientific Realist or not. He does not say that current best theories are approximately true in the way that expression is ordinarily used.

¹²³ Worrall, J. 1989 *ibid* (D. Papineau (Ed) 1996 *The Philosophy of Science* Oxford: Oxford University Press p.154).

Worrall's argues that most of the mathematical structure of mature theories is retained even in what Kuhn would call scientific revolutions. Thus the structure of Fresnel's theory of light is retained in Maxwell's theory, but the proposed nature of light is not. Fresnel's theory is not claimed to be approximately true, because there is now known to be no elastic medium through which light is propagated, which is an essential part of his theory. His equations however are absorbed into the later theory. The theory is 'realist' in that this mathematical structure is representative of the real relations between unobservables of the actual structure of the world, and this distinguishes Worrall's stance from Instrumentalism. As I remarked above, some writers do characterize Structural Realism as a form of Scientific Realism¹²⁴. I treat it as Anti-realist in terms of the definition given by Ladyman, cited in section 1.3, because Worrall does not claim that 'the entities or kinds of entities talked about and/or described by a discourse about S exist', where S is the theory.

Worrall has made a rare proposal for dealing with a central problem, namely the reconciling of the PMI with arguments for having faith in current theories on the basis of the NMA, given that revolutionary changes do occur regularly within science. It is worth asking how P-fertility can be dealt with on the basis of Structural Realism.

Structural Realism does not require that discarded scientific theories are approximately true. Fresnel's theory is not seen as approximately true but there is structural continuity. The continuity of mathematical structure may transcend scientific revolutions, so that there is no general claim that this structural continuity reflects a continuing theory revision as distinct from the genesis of a new theory.

¹²⁴ See for example Chakravartty, A. 2004 'Structuralism as a form of scientific realism' *International Studies in the Philosophy of Science* 18, 151-171; Ritchie, J. 2008 'Structural realism and Davidson' *Synthese* 162, 85-100. In fact Chakravartty says 'Many sophisticated accounts of realism are closet versions of structuralism' (*ibid* p. 157).

In the transition from Newton to Einstein continuity of structure might be claimed by arguing that as the velocity of light becomes infinite the equations of special relativity become those of classical mechanics. This seems to be going too far, and indeed Worrall would not make this claim. In this case a scientific revolution in Kuhn's sense has resulted in a new theory. Newton's claims of absolute space and the independence of mass and velocity are discarded, and the equations of motion are quite different in the two theories.

There may however be a difference between the Structural Realist and the Scientific Realist on the question of what constitutes a successful long standing theory. There are numerous theories where the criterion of structural continuity in Worrall's sense is impossible to discern. For example, most theories in biology do not have the sort of structure that makes them amenable to a judgment on Worrall's criteria. Worrall is untroubled by this because his purpose is to put forward a Realist perspective on theory structure, and thus to make a claim about the truth of theories in this sense, while not defending Realism about the entities postulated. Thus it seems that it is primarily useful to consider his theory in the domain of mathematical physics. Presumably Worrall might argue that a successful demonstration of the reality of structural relations in this domain can be used as a foundation for a more general Structural Realism when theories in other areas become sufficiently mature to have a mathematical structure.

How, on the basis of Structural Realism, can one explain the fact that 'long standing successful theories' in mathematical physics are 'P-fertile'? Suppose we consider the successive versions of the p , V , T relations in the kinetic theory of gases. Are the theories distinct for the Structural Realist since the equations arising are different? Alternatively can one regard the theories as showing structural continuity since the successive equations are modest modifications of their predecessor? It seems here one could accept the latter since there is an argument which establishes that the continuity of structure is more than the

plausible utilization of the most appropriate and convenient mathematical formalism. The intermolecular attractive and repulsive forces provide just such an argument. Thus in this case Structural Realism explains the conjunction of P-fertility and long standing success.

Now in accepting the factual status of the McMullin Conjunction, we are claiming that these distinct theories are part of a general pattern in the history of science, namely that such successive distinct theories arise naturally from their predecessor and continue to account for the new experimental observations that arise over time.

For the Structural Realist metaphorical suggestiveness and P-fertility arise from the true claims in the false theories¹²⁵ not from any claim about approximate truth. This bears a distant resemblance to the 'general theory' proposal of Nolan discussed in Chapter 1 because it is indeed the particular instance that suits Nolan's argument. However, it is not now being suggested that the subsequent developments are novel predictions of some general theory. On the contrary, each revision commonly arises from its predecessor in the manner proposed by McMullin. The difference from McMullin is that it is not the overall approximate truth of the theory, but the true claims about the structural relations within the theories that provide the metaphorical suggestiveness.

It is not clear how Worrall's Structural Realism can be extended beyond mathematical physics, or at least how it can be extended beyond theories with a mathematical structure. It is true that there will be structural relations in non-mathematical theories but in this case separating out the structural component would seem to be an intractable problem. If one looks, to choose just one

¹²⁵ It is of course perfectly possible for fruitful suggestions to arise from the false elements in theories that contain significant truths or in theories that are substantially false. Lyons has drawn attention in a careful historical analysis to the central importance of numbers of false views held by Kepler in successful prediction and in subsequent theoretical development (Lyons, T.D. 2006 'Scientific Realism and the Strategema de Divide et Impera' *British Journal for the Philosophy of Science* 57, 537-560). Moreover, Lyons cites evidence that this is by no means an isolated example in the history of science.

example, at the development of Molecular Biology since Watson and Crick's 1953 paper (when it became a mature science by any reasonable definition), it has made huge strides, without any parallel retention of structure as distinct from nature. The nature/structure of DNA has been elaborated and theories of the precise role of RNA and DNA have been and are continuing to be developed in a series of major advances. This is no argument against the applicability of Structural Realism in the appropriate domain. Nevertheless, it is an argument against the theory as a full explanation of the McMullin Conjunction because the claimed scope of this is wider than the scope of the explanations offered by Structural Realism.

This last is perhaps something of an oversimplification since the Structural Realists can argue that where their theories do not apply they can adopt an alternative stance which is in no way contradictory. Thus Worrall could agree that successive theories which do not share a common structure, because no mathematical structure is postulated in any of them, can be viewed as successive versions of the same theory. The basis for this could simply be that the theories, despite their different structural equations, are closely similar and meet what I have called the Continuity Conditions (section 2.2).

2.5.3 Entity Realism

To consider the P-fertility issues in the context of Entity Realism I first give a brief account of the view. Rather than claiming that the relevant theories are approximately true and in general successively more closely approaching the truth, Entity Realism makes the more modest claim that the postulated entities exist. It is not claimed that the description of the entities in the relevant theory is approximately true. I treat it as Anti-realist in terms of the definition given by Ladyman, cited in section 1.3, because the Entity Realist does not in general claim that, 'truth conditions for statements of S are objective and determine the truth or falsity of those statements depending on how things stand in the world', where S is the theory. She also does not make the general claim 'Truths about S

are knowable and we do in fact know some of them, and hence the terms of S successfully refer to things in the world.' Although it is true she might claim one or other of these in relation to specific theoretical claims in a restricted domain.

Cartwright argues for an Instrumentalist view of general theories combined with a realist stance on entities and an acceptance of the low-level phenomenological laws of science. Her scepticism about fundamental theories arises because she says they are hedged with *ceteris paribus* clauses of such ubiquity and seriousness that the theories cannot be believed to be of universal validity. The fundamental laws of electrostatics assume *per impossibile* that no gravitational forces are acting. Falling autumn leaves are inescapably affected by forces other than those due to gravity. As a result the fundamental laws are false and corrections are always needed to bring the theoretical account closer to reality¹²⁶. From a Scientific Realist stance, the fact that allowing for air resistance gives a better account of the trajectory of a projectile in a medium such as air does not undermine Newton's theory as applied to motion in a vacuum¹²⁷. Cartwright can respond that a universal law confined to motion in a vacuum is not a credible universal law any more than one referring to point masses and inextensible strings. Her position is that the widely accepted universal laws are not laws of nature, but make a patchwork of discrete accounts which enable ordering and prediction in various aspects of a world which may well be a messy reality. As

¹²⁶ It is really the fundamentalism about the laws of physics and the belief in their universal applicability that Cartwright denies (Cartwright, N. 1994 'Fundamentalism vs The Patchwork of Laws' *Proceedings of the Aristotelian Society* 93, 279-92). Cartwright regards the 'laws of nature' as descriptive rather than prescriptive, that is, rather than as counterfactual conditionals that apply at all times with appropriate *ceteris paribus* clauses.

¹²⁷ Both Cartwright and the Scientific Realist would agree that better agreement with experiment is obtained by the avowedly instrumentalist step of introducing a resistance term proportional to velocity in calculating the trajectory of a projectile. Cartwright takes a wholly Instrumentalist stance towards this fact. The Scientific Realist view is that if we were able to solve the intractable problems of turbulent flow in a medium like air to calculate the forces between the medium and the projectile over the course of its flight, we would arrive at a result that showed that taking the resistance to be linearly proportional to velocity was a good approximation to the truth. That is, a good approximation to an extremely complex formula (involving local pressures, projectile interface temperatures and other variables) actually quite closely describing the reality of processes in the mind-independent world.

she puts it¹²⁸: 'We may use the fundamental equations of physics to calculate precise quantitative facts about real situations, but abstract fundamental laws are nothing like the complicated, messy laws which describe reality.'

The Scientific Realist counter argument, as exemplified by the air resistance example above, relies on a faith in the composition of causes. We can, it is claimed, make an idealized model and subsequently 'de-idealize' it to allow for its known oversimplification, made initially to attain a more tractable theory/model¹²⁹. Cartwright responds by saying that 'it is rarely the case that models of the phenomena are arrived at as de-idealizations of theoretical models.'¹³⁰ She might well also respond that it is far from obvious that it is proper or possible in this way to make a quantum mechanical calculation involving elementary particles ignoring relativistic effects, and then simply to add them in.

Cartwright would agree that it is not merely proper, but essential, for scientists to conduct their experiments under highly artificial laboratory conditions. This is the only way scientists can produce uniform, precise, lawlike results that can be replicated (or not) by other experimenters at other times in different laboratories. Her claim is that this necessity vitiates the claim that universal laws have been discovered. This is not merely a matter of terminology. Cartwright believes that there are no established universal laws.

¹²⁸ Cartwright, N. 1986 'Fitting Facts to Equations' *Philosophical Grounds of Rationality* R.E. Grundy and R. Warner (Eds) Oxford: Clarendon Press.

¹²⁹ Morrison denies that the plausible argument about de-idealization given above in the example of air resistance can be generalized. She points out that in most cases the more refined model introduces new concepts and hence the 'composition of causes' argument cannot be invoked. For example, the ideal gas law gives no possibility of a phase change, whereas the van der Waals equation makes (incorrect) predictions about the critical phenomena. The inclusion of attractive intermolecular forces allows for the possibility of liquefaction. The situation, she says, is even worse for contemporary theories in particle physics, where there is no way of knowing the relationship between the postulated model and a hypothetical true structure. Indeed, she claims, the models in elementary particle theory are largely heuristic. (Morrison, M. 2005 'Approximating the Real: The Role of Idealizations in Physical Theory' *Idealization XII: Correcting the Model. Idealization and Abstraction in the Sciences* M.R. Jones and N. Cartwright (Eds) (Poznan Studies in the Philosophy of the Sciences and the Humanities Volume 86) pp. 145-172.

¹³⁰ Cartwright, N., Shomar, T. and Saurez, M. 1995 'The Tool Box of Science' in *Theories and Models in Scientific Processes* W.E. Herfel, W. Krajewski, W.I. Niiniluoto and R. Wojicki (Eds) Amsterdam: Rodopi p. 142.

According to Cartwright to ask what the best theory of the electron is, for example, is to make a mistake. Theories are not in competition, but are chosen to make predictions, guide experiments, design equipment and the like. Newton's theory is good for sending rockets to the moon but not adequate for designing high energy particle accelerators. This does of course accord with scientific practice in many areas. In discussing the lamellar flow of liquids we assume that water is a continuum, while in explaining the freezing of water we assume it has discrete molecules in a (more or less) random array in the liquid form, and these molecules form a regular lattice in ice. Cartwright believes in the entity, the H₂O molecule, because its presence in water explains solidification, Brownian motion, and the results of the nuclear magnetic resonance, optical and X-ray experiments on water, ice and water vapour.

Hacking¹³¹ has a somewhat different Entity Realist perspective. He argues that the fact that we can manipulate the postulated unobservable entities of physics is convincing evidence that they are real. It may be that our description of the electron is faulty, but the fact that we can focus them to produce an image in the electron microscope shows that they exist¹³². Hacking does also endorse other arguments for Entity Realism, including the 'continuum argument' mentioned in section 1.1; that is, the argument that the naked eye, the magnifying glass, the optical microscope¹³³, and the electron microscope allow the observation of

¹³¹ Hacking, I. 1983 *Representing and Intervening* Cambridge: Cambridge University Press.

¹³² Hacking gives various examples; this one is mine and is not one of his. Kukla suggests that for this 'manipulability' argument to have any force it must be argued that you can spray electrons, to use Hacking's example, 'in a sense that doesn't presuppose their existence'. The only appeal the argument has, Kukla says, is to one's intuitions, and these are precisely what the Anti-realist denies (Kukla, A. 1998 *Studies in Scientific Realism* Oxford: Oxford University Press p. 90).

¹³³ Hacking has also given a detailed argument for accepting the veracity of optical microscope observation (despite the necessary intervention of some interpretative theory of imaging) by showing that optical microscopes operating on quite different theoretical principles give similar results. Quinon has given a further argument to the 'continuum argument' of Maxwell (cited in section 1.1) for accepting the results obtained from the instrumental extensions of human vision, by pointing out that at each stage of increased magnification, objects visible in the previous stage can still be seen to be the same object. Thus if one accepts naked eye observations one should also accept the deliverances of magnifying instruments (Quinon, A. 1973 *The Nature of Things* London: Routledge and Kegan Paul p. 303).

progressively smaller entities, and there are no grounds for privileging the naked eye within this continuous spectrum¹³⁴.

Entity Realists must accept that there is some element of truth in the account given of say the elementary particles of physics¹³⁵. However they do not need to claim that the theories overall are true or approximately true. They would agree that¹³⁶ 'a viable realism must endorse some theoretical properties as well as a theoretical ontology'. Thus, for example, they might agree that it is true that electrons obey Fermi-Dirac statistics, and that their charge and mass are with a few hundredths of a percent of the present accepted values. Hacking¹³⁷, for example, asserts, 'We know an enormous amount about the behaviour of electrons'. This leaves scope for not believing that Quantum Electrodynamics is true or approximately true.

On the Entity Realist stance there are some interesting questions about what is meant by the concept of diachronicity. The Entity Realist is in part, like the Structural Realist, responding to the PMI. Science is successful because it correctly identifies the (unobservable) entities which explain the phenomena, in particular the results of carefully controlled scientific experiments. McMullin's diachronic view of theories is not really applicable in the same way as the Scientific Realist would use it – what survives theory change is related to the

¹³⁴ Van Fraassen, who treats observable as observable by unaided humans, has a response to the continuum argument. He says, '*Contrary to Sextus Empiricus there is a real distinction between touching your mother's toe with your little finger and having incestuous intercourse, even if the difference is a matter of degree, and the line is drawn differently in different contexts.*' (van Fraassen, B.C. 2000 'Constructive Empiricism Now'

<http://webware.princeton.edu/vanfraas/mss/APA-Albuq2.htm> p. 8) The example is also used, differently put, in his *The Scientific Image* Oxford: Clarendon Press p.16. Kukla has a careful discussion which points out the difficulty with van Fraassen's argument that since it is human science, so an unaided human perspective is appropriate for observability. Just as a blind scientist can be part of the scientific community, so could a mutant scientist with extraordinary visual capacities (Kukla, A. 1998 *Studies in Scientific Realism* Oxford: Oxford University Press Chapter 10).

¹³⁵ Psillos, S. 1999 *Scientific realism: how science tracks truth* New York: Routledge p. 256.

¹³⁶ Leplin, J. 2006 'Realism' *The Philosophy of Science: An Encyclopedia* S. Sarkar and J Pfeifer (Eds) London: Routledge p. 687.

¹³⁷ Hacking, I. 1983 *Representing and Intervening* Cambridge: Cambridge University Press p. 232.

postulated entities as much as to the theories. However, this does not pose a problem for the Entity Realist in accepting the diachronic view of theories; there may be a need to word the Continuity Conditions somewhat differently.

The success of the theory modified over time can in this case be interpreted in a manner parallel to the McMullin approach. For the Entity Realist the argument would be that the correct identification of some important aspect of the causal unobservable entity is the origin of the success of the theory viewed instrumentally. In other words, it is again the significant true claims of the theory which give it its metaphoric suggestiveness and lead to the successful modifications and the P-fertility.

To consider an example, the Entity Realist might agree with the Scientific Realist that the water molecule, H_2O , exists. Currently, molecular weight, bond angles, energy levels of excited states, and numerous other features are described in great quantitative detail. The Entity Realist does not believe the current detailed interpretation of the whole of this because she does not believe the relevant theory describes the lawlike behaviour that the Scientific Realist assumes for natural kinds like H_2O . Let us suppose the current theory has a long history of successful modification to meet new observations. Can this be accounted for by the simple assumption that there are H_2O molecules with two hydrogen atoms, hydrogen bonded to an oxygen atom, and no more? More realistically, is more required and if so how much more, and how much of this does the Entity Realist accept?

These questions raise some complex issues. There is a theory of the water molecule that has resulted from a large body of experimental evidence. There will be results from spectroscopic studies of water in the liquid and gaseous forms over a range of wavelengths. That is, these studies will range from nuclear magnetic resonance to the optical and the ultra violet end of the electromagnetic spectrum. There will be X-ray and neutron diffraction studies of the various

crystallographic forms of ice. There will be studies of the physical, mechanical, thermal and electrical properties of H_2O over a range of temperatures, which for some of these properties, will be carried out in the solid, liquid, and gaseous states.

The importance of the details of the picture of the water molecule will be different in a theory of each of the following arbitrarily chosen phenomena involving the molecule.

- (i) the viscosity of water;
- (ii) the thermal conductivity of water;
- (iii) the thermal conductivity of water vapour (H_2O in the gaseous state);
- (iv) the absorption spectrum of water vapour for electromagnetic waves;
- (v) the temperature dependence of the conductivity of ice;
- (vi) the allotropic transformations of ice¹³⁸;
- (vii) the plastic deformation of ice.

Understanding (vi), the origin of the various crystalline structures of ice formed at different temperatures and pressures, and (iv) the absorption spectrum of water vapour, is importantly dependent on the picture of the H_2O molecule. The present (limited) understanding of (vii), the plasticity of the predominant form of terrestrial ice, is not greatly dependent on the detail of the molecular structure. Relating the properties of dislocations to atomic structure, even in the simple, near to close-packed hexagonal structures of metallic elements like zinc, constitutes a formidable many body problem. The complex hexagonal structure of the common form of ice makes relating dislocation structure to the details of the non-spherically symmetric intermolecular forces quite intractable.

What then is the significance for Entity Realism of a successful long standing P-fertile theory in one of these areas? It depends on the extent to which the (possibly false) theory (in its successive forms) is dependent on the detailed knowledge of the entity itself demanded within the particular theory.

¹³⁸ There are some twelve different known crystalline structures of ice.

First, consider Hacking's stance based on the view that we know water molecules exist because we can manipulate them. This, for example, would seem every bit as satisfactory as full blown Scientific Realism to account for the fact that water when heated becomes a gas with some 1700 times the volume per unit mass of the liquid, and further that latent heat is required for the process. However, we cannot 'manipulate' the hydrogen bond that is postulated to be an essential element in the stability of the H_2O molecule, so that many other theories about the behaviour of water which depend on our understanding of the hydrogen bond remain unexplained, and likewise the P-fertility of these theories. Hacking would respond that this is in order. We believe in the reality of the H_2O molecule (thus taking us beyond the Instrumentalist in our realism) but suspend judgment about the reality of the hydrogen bond.

Thus the situation is reminiscent of the Structural Realist position. Within its domain of applicability Hacking's Entity Realism is able to account for P-fertility and the McMullin Conjunction. It does so via the true claims about the entities considered and does not invoke the approximate truth or verisimilitude of the relevant theories. Again there is a lack of generality in the explanation as in the Structural Realism case (assuming always the fact of the McMullin Conjunction).

Cartwright's position is somewhat different. She does not require manipulability. If light is bent as it passes close to the sun, she is prepared to accept the general theory of relativity as an explanation of the phenomenon. What she does not accept is the general theory of relativity as an account of the behaviour of space-time. She is not an Instrumentalist because, like Hacking, she believes in electrons and their causal power and their real existence. However, she, unlike Hacking, 'believes' that space-time is curved because this is the best account we have of a number of important experiments. I have put believes here in inverted commas because Cartwright does not believe that the general theory of relativity is a true or approximately true account of laws that apply universally in a mind-

independent world. In her view we do not have such an account, and we have no reason to believe there is such an account. She states¹³⁹: 'To grant that a law is true – even a law of “basic” physics or a law about the so-called fundamental particles – is far from admitting that it is universal, that it holds everywhere and governs in all domains.'

Cartwright, with regard to any and all reliable scientific data and scientific theorizing, would draw exactly the same conclusions as an Instrumentalist. Her departure from Instrumentalism is her belief in the real existence of unobservable entities. In her case this would include entities like the hydrogen bond with its causal powers. Her instrumentalism is explicit in so far as she does not merely deny that we do or can know the laws of nature, but asserts that there are no such laws. If there are no laws of nature then the theories introduced to bring order to observations and to make predictions are most sensibly viewed from an Instrumentalist standpoint. Cartwright adds to this the plausible idea that there are entities like the electron or the atom or the hydrogen bond, and we do know something about them. We do not have a theory which we are justified in taking to be universally true.

I use the image of the tool-box of science to describe a kind of instrumentalism that I defend as part of this movement to undermine the domination of theory. ... Our scientific understanding and its corresponding image of the world is encoded as much in our instruments, our mathematical techniques, our methods of approximation, the shape of our laboratories and the pattern of industrial developments as in our scientific theories ... these bits of understanding so encoded should not be viewed as claims about the nature and structure of reality which ought to have a proper propositional

¹³⁹ Cartwright, N. 1994 'Fundamentalism vs The Patchwork of Laws' *Proceedings of the Aristotelian Society* 93, 279-92 p. 280.

expression that is a candidate for truth or falsehood. Rather they should be viewed as adaptable tools in a common scientific toolbox.¹⁴⁰

I will therefore consider the implications of Cartwright's stance as part of the following section on Instrumentalism.

2.5.4 Instrumentalism

Instrumentalism is Anti-realist because, unlike the Scientific Realist, the Instrumentalist believes that empirical adequacy rather than truth is what scientific theories deliver¹⁴¹. Thus in terms of the definition given in section 1.3 the Instrumentalist *inter alia* does not believe the important assertions:

- (i) The entities or kinds of entities talked about and/or described by a discourse about S exist.
- (v) Truths about S are knowable and we do in fact know some of them, and hence the terms of S successfully refer to things in the world.

In considering how the McMullin Conjunction can be accounted for on the basis of an Instrumentalist account of scientific theories it is useful to look at an example. I have chosen one of the most fertile (as distinct from P-fertile) theories in the history of science – Dalton's Atomic Theory (*circa* 1803).

The theory stated

- (i) elements consist of atoms
- (ii) the atoms of an element are qualitatively identical
- (iii) the atoms of an element are different from those of any other element
- (iv) atoms of one element can combine with atoms of other elements to form compounds and a given compound always has the same relative numbers of types of atoms

¹⁴⁰ Cartwright, N., Shomar, T. and Saurez, M. 1995 'The Tool Box of Science' in *Theories and Models in Scientific Processes* W.E. Herfel, W. Krajewski, W.I. Niiniluoto and R. Wojicki (Eds) Amsterdam: Rodopi p. 138.

¹⁴¹ As mentioned previously, in talking about Instrumentalism I am focusing on van Fraassen's Constructive Empiricism.

- (v) atoms cannot be created, divided into smaller particles, nor can they be destroyed in the chemical process¹⁴².

Based on this atomic theory, Dalton said that when compounds form between elements the ratio of the masses will be whole numbers.

However, Dalton added the doctrine that unless there was some compelling reason, compounds were binary, for example, water was HO. Thus he believed the observation that two volumes of hydrogen and one volume of oxygen combined to form two volumes of water vapour ($2 \text{H}_2 + \text{O}_2 \rightleftharpoons 2 \text{H}_2\text{O}$) was erroneous¹⁴³. Thus Dalton's theory often did not agree with experiment. The explanation was given by Avogadro in 1811; he made the distinction between elemental molecules and elemental atoms, a distinction that Dalton had not considered (since Dalton believed that like atoms would not attract one another to form molecules of an element in the gaseous state). Avogadro's work was ignored, in part because of Dalton's prestige, until resurrected by Cannizaro some 50 years later. In the intervening period, chemistry made great progress, as experimental techniques improved, and closely confirmed Gay-Lussac's early observation (1809) that when gases combined in chemical reactions they did so in ratios, by volume, that were always simple whole numbers.

Dalton's theory was a triumph, but the Instrumentalist is entitled in the nineteenth century to her sceptical stance about the existence of atoms. Indeed it is now accepted that Dalton's indestructible atoms were in a legitimate sense a useful fiction¹⁴⁴, and the Instrumentalist might well claim the same is likely to be true of current elementary particle theory (the so called Standard Model).

¹⁴² It is of course the specificity of clauses (ii), (iii) and (iv) that distinguishes Dalton's work from the various earlier 'atomic theories'.

¹⁴³ Precise measurements on gaseous reactions were not easily made at the time, and Dalton was in any case extremely sceptical of the results of other scientists. Hence he did not regard this observation as a compelling reason to abandon his belief that water was HO.

¹⁴⁴ It is now well known that atoms are not indestructible (Marie Curie observed radioactivity in 1897 although its significance was not then wholly clear), and that atoms of the same element commonly are not all qualitatively identical.

For the Instrumentalist a long standing successful theory is simply one that works. It makes successful predictions and over time it accounts for new observations by modest modifications. If either the postulated entities or the theoretical postulates change radically, the Instrumentalist would not regard the theory as being the same theory. On the other hand, the long standing success of a theory has no more implication than simple success as far as truth is concerned. Dalton's theory was a long standing successful theory and was successfully modified over a long period viewed from an Instrumentalist perspective. The Instrumentalist need not believe in Dalton's indestructible atoms.

Why it is that this long standing successful theory (that is false) can be readily modified to accommodate new data. In this case, the theory is wrong in proposing that atoms are indestructible and wrong in saying that all the atoms of a given element are qualitatively identical. The reason that it might be P-fertile (given that it is successful) is again because it contains significant claims that are true and it is these which give rise to P-fertility. Atoms are not destroyed in chemical reactions and atoms of a given element which differ (atoms of different isotopes) will still behave identically in chemical reactions. As a result the true features of a false (and not approximately true) story can lead to P-fertility in more or less precisely the manner that is claimed for (what we can say for the moment is) the meaningless notion of approximate truth. In the present case the features of the atomic theory that led to progress in fields such as chemistry and the kinetic theory of gases are present in Dalton's false story¹⁴⁵.

The immediate question is whether this type of account can be invoked more generally. All P-fertile theories are successful. It is plausible that all (or at least most) long standing successful theories contain true claims, and this accounts for

¹⁴⁵ This progress took considerable time and it is not suggested that the developments were obvious on the basis of Dalton's theory. For example, Dalton took the atmosphere to be a mixture like sand in water, and could not understand why the heavier oxygen did not sink to the bottom (Kuhn, T.S. 1970 *The Structure of Scientific Revolutions* Chicago: University of Chicago Press, p. 131).

their P-fertility. For van Fraassen, science is interested in empirical adequacy, not truth, but he has no need to resist the notion that a successful theory (i.e., an empirically adequate theory) contains true claims. He would for example say that the van der Waals theory of gases was empirically adequate in some respects, for some gases, under some conditions, but was certainly not true. But within the framework of Constructive Empiricism it seems to me that one could perfectly well accept that it is true that there are intermolecular attractive forces in gases. It is certainly no part of Constructive Empiricism to assert that all the postulates of empirically adequate scientific theories are false. A Constructive Empiricist might assert that there is no reason to take any of them to be true, but she would undertake unnecessary baggage by denying that the most obvious claims are true. Her concern is to deny that it is rational to take the theories to be true.

Does the possession of significant true claims within an empirically adequate theory give as adequate an account of the McMullin Conjunction as does approximate truth? The question can be considered in the first instance by asking whether my example of Dalton's atomic theory can be taken as typical. The two notions differ in that approximate truth incorporates the idea of nearness to truth. The Instrumentalist denies the relevance of such a concept to scientific theorizing. The committed Scientific Realist could in some circumstances take my account of what I have described as a theory containing significant true claims as a proper account of an approximately true theory. We are back in the dilemma posed by the concept of the approximate truth of theories. How is one to decide whether or not Dalton's theory is or is not approximately true?

However, the present question is whether the Instrumentalist has an explanation for the McMullin Conjunction. In my example of Dalton's theory, I have given an explanation in the terms a Scientific Realist would use. Namely, much but not all of what Dalton postulated is true of the mind-independent world. The Instrumentalist makes no such claim and simply asserts that the theory is empirically adequate. What then explains the McMullin Conjunction? For some

reason a useful theory is readily modified to accommodate new data. It is contrary to the spirit of van Fraassen's Constructive Empiricism to appeal to truths within the theory. Nevertheless the explanation of the McMullin Conjunction must indeed be that the theories considered contain true claims as suggested above in the example of the intermolecular attractive forces in real gases.

In light of the view that making sense of the very idea of fertility requires taking the fertile theory to be something over and above a recipe for prediction and control, or the claim that the theory is empirically adequate, it can be argued that Constructive Empiricism can offer no satisfactory account of the McMullin Conjunction. The point here is that if the aim of science is merely to organize the data and thus make predictions, McMullin's idea of scientific theories as fruitful metaphor seems difficult to sustain. The generative capacity of the successive versions of long standing successful theories can no longer helpfully be described as fertility. It seems that someone who merely accepts a theory as van Fraassen proposes, rather than believing in it, has no ground for regarding it as generative. Her explanation of the McMullin Conjunction, as I have said, may be that the assertions in the theory which may be true (although she does not necessarily believe them to be true) are possibly the origin of the seeming capacity of the theory to lead to further empirically adequate theories. However, it might be more consistent with van Fraassen's stance simply to argue that given new experimental evidence scientists modify their theories, and it is an interesting historical fact of no great significance that they are often successful in intelligently elaborating their theories. Further, the Constructive Empiricist might say the supposed McMullin Conjunction is more salient than is of any real significance because the very common unsuccessful elaborations are mostly lost to history.

However, a Constructive Empiricist might take a strong line and argue that there is no justification for taking any of the statements within a scientific theory to be

true, rather than merely arguing that the theory can not be taken to be true or approximately true. If it cannot be asserted that there are truths within the theory, she has no obvious explanation for the McMullin Conjunction, and the most reasonable response is to adopt towards it the sceptical stance outlined above, namely that it not significant.

The Entity Realist is perhaps in a slightly better position. If her claims about the entities are accepted, then it may be that the explanation of the metaphorical suggestiveness of the theories results from the correct identification of aspects of these entities. While this is not implausible in the Dalton example, it is not at all clear that arguments along these lines are plausible in elementary particle physics where the entities and the theory are inseparable. This is not to suggest that Cartwright may not be correct in her rejection of universal laws, but only to say in this last area, among others, she has no obvious account of the McMullin Conjunction. Of course, she has no obligation to accept the premise I have adopted for the purpose of argument, namely that the McMullin Conjunction holds.

The question remains as to whether Instrumentalism, in particular van Fraassen's Constructive Empiricism, is itself a plausible form of Anti-realism, and this has been extensively discussed in the literature¹⁴⁶. One immediate question is whether Instrumentalism can account for the Pessimistic Meta Induction. This does not seem to be a serious concern. Since no use is made of the No Miracles Argument, scientific theories are not taken to be true and there is no reason to suppose that new experimental evidence will not show them to be false. As

¹⁴⁶ See for example Psillos, S. 1999 *Scientific realism: how science tracks truth* New York: Routledge; Niiniluoto, I. 1999 *Critical Scientific Realism* Oxford: Oxford University Press; Horwich, P.G. 1991 'On the Nature and Norms of Theoretical Commitment' *Philosophy of Science* 58, 1-14; Kukla, A. 1992 'On the Coherence of Instrumentalism' *Philosophy of Science* 59, 492-497; Norris, C. 2004 *Philosophy of Language and the Challenge to Scientific Realism* London: Routledge; Dicken, P. and Lipton, P. 2006 'What can Bas believe? Musgrave and van Fraassen on observability' *Analysis* 66, 226-233; Muller, F.A. and van Fraassen, B.C. 2008 'How to talk about unobservables' *Analysis* 68, 197-205; Dicken, P. 2009 'On the syntax and semantics of observability: a reply to Muller and van Fraassen' *Analysis* 69, 38-42.

Popper forcefully argued¹⁴⁷, this last is straightforward compared with establishing veracity.

The arguments advanced by Scientific Realists against Instrumentalism are sometimes simply advocacy of the greater plausibility of Scientific Realism itself, and sometimes claims that the position collapses or is incoherent because of the failure of van Fraassen's distinction between acceptance and belief or between observables and unobservables. They will not be rehearsed here. However, a novel argument for scepticism about Instrumentalism is contained in Appendix A.

2.6 Conclusion

McMullin's account of P-fertility in terms of approximate truth seems straightforward. The price paid for this is the introduction of the concept of the approximate truth of scientific theories. By considering the criteria justifying the diachronic stance one can see that successive sub-theories may be sufficiently similar without drawing on the concept of approximate truth or approximate similarity. On McMullin's own account it would be desirable that each theory suggested its successor. However, perhaps one can justify dispensing with this criterion in the instances where we can establish the proximity of the successive theories. The important issues of (i) the difficulties of giving a meaning to the concept of the approximate truth of scientific theories and (ii) the legitimacy of using the concept to account for the McMullin Conjunction are taken up in Chapter 3.

The Anti-realist¹⁴⁸ is not committed to either the notion of P-fertility or to the acceptance of its conjunction with long standing success. It is the McMullin

¹⁴⁷ Popper, K.R. 1959 *The Logic of Scientific Discovery* London: Hutchinson; Popper, K.R. 1963 *Conjectures and Refutations*, New York: Harper and Rowe.

¹⁴⁸ I am here, of course, persisting with the characterization of Structural Realism, Entity Realism, Constructive Empiricism, and Internal Realism as Anti-realist. I have, as mentioned earlier, postponed the discussion of Internal Realism to Chapter 6. What is clear is that McMullin's full blooded Scientific Realism contrasts with all of these positions. McMullin writes, 'If the resources of the theory have shown that it was able to cope with the unexpected, the argument is that this gives us reason to believe that the structures postulated in the theory correspond reasonably well

argument for Scientific Realism that draws on this dual commitment. However, if the McMullin Conjunction is accepted on the basis of the historical evidence, Anti-realism does require that it be explained. Here the Anti-realist can fall back on the idea that successive theories may indeed be suggested by their predecessor as a consequence of the significant true claims contained within them. The Anti-realist is not required to invoke the approximate truth of successor forms of a P-fertile theory. Every member of such a sequence of theories can be false.

It could be argued by a Scientific Realist that *contra* McMullin, the McMullin Conjunction can be explained by the true claims in the successive theories, and not by their approximate truth. The acceptance of the approximate truth of theories has been imputed by me to McMullin as a necessary part of his position as discussed in section 1.2. If we allow the 'true claims' account on a Realist construal then we have parity between Scientific Realism and Anti-realism, and the abductive argument for the former is considerably weakened. However, in Chapter 5 I argue for a stronger Anti-realist case, namely that Scientific Realism requires the approximate truth of theories.

Why does containing significant true claim suffice to give a theory its metaphorical suggestiveness? A Scientific Realist could accept that an approximately true theory made significant true claims and that it is predominantly these that make for P-fertility. After all the Scientific Realist is not claiming that the intermediary theories are true. Equally a Scientific Realist, from what has been argued so far, could dispense with approximate truth, and use the

to the structures of the real.' (McMullin, E. 1976 'The fertility of theory and the unit for appraisal in science' in R.S. Cohen *et al* (Eds), *Essays in Memory of Imre Lakatos*, Dordrecht: D. Reidel p. 423.) Or as he writes elsewhere, 'realism has to do with the existence implications of the theoretical entities of successful theories.' (McMullin, E. 1984 'A Case for Scientific Realism' in *Scientific Realism* J. Leplin (Ed) Berkeley: University of California Press, p. 13.) or 'realism has to do with the reality of certain postulated entities' (McMullin, E. 2003 'Van Fraassen's Unappreciated Realism' *Philosophy of Science* 70, 455-478 p. 457). Thus the first of these claims would set Scientific Realism against all four Anti-realist positions, the second pair would allow Entity Realism, but the truth of the claim of 'existence implications' or 'reality' would not be acceptable to a Constructive Empiricist or an Internal Realist.

significant true claims argument of the Anti-realist. (As mentioned above, I return in Chapter 5 to a discussion of this claim that the approximate truth of theories can be dispensed with by the Scientific Realist.)

Dalton's atomic theory made the significant true claim that atoms are not destroyed in chemical reactions. This is entailed by the false claim that atoms are indestructible. It is the former claim that led to the success of Dalton's theory. Indeed the significant true claims account can be applied to theories that the Scientific Realist would take to be false, in addition to those taken to be approximately true. As Almeder¹⁴⁹ writes,

the occurrence of the sensory phenomena that we would expect if the designated theoretical claims were true might just as easily be the result of other theoretical or nontheoretical claims made in the theory, claims that serve as auxiliary hypotheses or simple observational claims, while the designated theoretical claims are literally false. This hypothesis, for example, would constitute one plausible way to explain the predictive success of Ptolemy's astronomy. While the designated theoretical claims of Ptolemy's astronomy can be viewed as literally false, the predictive success of such claims would need to be a function of other true claims made in the theory.

The argument can be summarized as follow

- (i) In accepting the empirical claim of the McMullin Conjunction we have accepted the diachronic view of theories and hence some notion of similitude of theories and a commitment to some notion of significant continuing claims within the successive versions of the theory.
- (ii) The diachronic view of theories is acceptable to the Scientific Realist and the Anti-realist.

¹⁴⁹ Almeder, R. 1987 'Blind Realism' *Erkenntnis* 26, 57-101, p. 73.

- (iii) P-fertility can be explained by both Scientific Realism and Antirealism on the basis of the significant true claims in the successive versions of the long standing successful theories.
- (iv) Hence the historical observation of the McMullin Conjunction is neutral between Scientific Realism and Anti-realism unless McMullin's explanation for the conjunction is accepted and is seen to be more plausible.

McMullin's novel argument for Scientific Realism rests on the claim that the successive versions of the long standing successful theories are approximately true. The status of the claim of the approximate truth of scientific theories, questioned in Chapter 1, is examined in detail in Chapter 3.

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Appendix A

Instrumentalism has been criticized on a variety of grounds by various writers¹⁵⁰. One difficulty that seems not to have been considered lies in the interesting cases where a macroscopic model faithfully represents some 'unobservable' that the Instrumentalist claims we are not justified in taking as a real existent. This is illustrated in the following two examples.

Bragg's soap bubble raft model of a crystal seems to be an excellent macroscopic simulation of a two dimensional crystal. A bubble raft is produced by forcing pressurized air through a micropipette submerged in a glycerine-soap solution, resulting in uniformly sized bubbles of about 1 mm diameter. The interaction of the bubbles is qualitatively similar to the inter-atomic pair potential of face-centred cubic metals. Now the Instrumentalist will readily concede that there are visible dislocations in the bubble raft. Nevertheless, she is compelled to say that this gives us no reason to believe in the unobservable¹⁵¹ dislocations in real crystals. The bubble raft simulation shows macroscopically (in two dimensions) numerous features of dislocation behaviour that theory predicts in three dimensional real crystals. Moreover, some of these observed features were successively encompassed in the progressive refinements of dislocation theory as applied to real crystals. The inter-atomic forces are by no means identical to those between the soap bubbles, but the similarity in relevant respects to the forces in face-centred cubic metals makes it plausible that the observed dislocation behaviour in the bubble raft is a simulation of dislocation behaviour in these crystals. It does seem mysterious that the 'useful fiction' of the unobservable entities can be paralleled by identical observable entities in the simulation. The explanation that there are dislocations in crystals seems in this

¹⁵⁰ See for example Psillos, S. 1999 *Scientific realism: how science tracks truth* New York: Routledge; Niiniluoto, I. 1999 *Critical Scientific Realism* Oxford: Oxford University Press.

¹⁵¹ Of course both the Scientific Realist and many Anti-realists deny that dislocations are unobservable and cite *inter alia* the electron microscope evidence.

case to account in a natural way for the behaviour of real crystals and the behaviour of the bubble raft.

The Instrumentalist might argue that in some way the dislocation hypothesis reflects an underlying structure, albeit one which may not actually include dislocations. This seems difficult to rule out completely, and the Instrumentalist will maintain that belief in the postulated entities adds nothing to our understanding. However, one is left with no explanation for the parallel between the bubble raft and real solids.

A similar but even more striking example is the play of light from opals, caused by the diffraction of light, which in every qualitative and quantitative respect replicates the diffraction of X-rays by crystals¹⁵². Opals consist of a close packed array of silica spheres of approximately constant size whose diameter is a few hundred nanometers (i.e., about the wavelength of visible light)¹⁵³. An Instrumentalist who does not believe the current account of X-ray diffraction from regular arrays of unobservable atoms¹⁵⁴ (again arranged as close packed spheres) but does accept the precisely parallel account of the diffraction of light by opals seems to be faced with a difficulty. She must presumably accept that X-rays, on the one hand, and light in the range of human visibility on the other, are identical forms of electromagnetic waves, differing only in wavelength¹⁵⁵. How then can she explain the identical behaviours while denying the existence of atoms?

¹⁵² Sanders, J.V. 1968 'Diffraction of light by opals' *Acta Crystallographica* A24, 427-434.

¹⁵³ The naturally occurring opals have been deposited over geological times (sometimes in sedimentary rocks and sometimes in rocks of volcanic origin) from an aqueous solution of silica in certain restricted circumstances of composition, cooling rate and pressure. The diffraction experiments have been repeated on artificial opals, created in the laboratory by manufacturing and depositing tiny silica spheres of uniform diameter.

¹⁵⁴ Here the inter-atomic spacing is about 0.3 nanometers, about the same as the wavelength of the X-rays, paralleling on a scale about 1000 times smaller, the opal case.

¹⁵⁵ Van Fraassen might be imagined to argue that the small manufactured silica spheres are also in the category of unobservables. However, diffraction experiments can be done using microwave radiation of centimetre wavelengths, and indeed are commonly done in undergraduate laboratories precisely to demonstrate the analogy with the X-ray experiments.

Of course Instrumentalism is an elusive doctrine and a contemporary Instrumentalist could believe in dislocations and atoms but not necessarily in quarks. However, this is at least partially to join the ranks of the Scientific Realists, who also are legitimately selective in the theories they believe.

CHAPTER 3 SCIENTIFIC REALISM, TRUTH, APPROXIMATE TRUTH

3.1 Introduction

McMullin's argument, outlined in Chapter 1, makes the claim that explanatory structures 'roughly like' those postulated by contemporary long standing successful scientific theories exist. In other words these theories are approximately true. So what is approximate truth¹⁵⁶? The purpose of the present chapter is to examine the difficulties of the concept of approximate truth. The crucial issue is the admissibility of the concept of the approximate truth of scientific theories, as distinct for example from the approximate truth of an estimate of a single numerical variable.

It is important to note here that we have thus far put aside a distinction as to what different philosophical stances intend by the claim that 'electrons exist'. For an Anti-realist the claim may be rationally acceptable or warrantably assertible, and she may nevertheless characterize herself as a Scientific Realist. She would be a realist about scientific ontology. A Scientific Realist for whom truth was non-epistemic would make the claim that electrons exist and add that the claim could be false, however compelling the evidence was for the existence of electrons. Truth conditions are separate from verification conditions. She would be an ontological and semantic realist. For the purposes of this chapter the nature of approximate truth can be considered in relation to a truth concept that is not firmly specified.

The basis for the contention that the postulated unobservable entities exist, outlined in Chapter 1, is that changes in scientific theories over time are such that

¹⁵⁶ I will use approximate truth as approximating to the truth, a concept that will require further clarification. I exclude the sense where one might say that something that is vague but not false is approximately true, like, for example, John is tall, when John is 180cm, where one might argue whether or not he is tall, that is, we may not be dealing with a determinate truth.

the successful theories become progressively closer to describing the mind-independent-world (progress need not be monotonic). That is, they become more nearly approximately true, as evidenced by the fact that only this can satisfactorily explain the observation that successful long standing theories show P-fertility.

One further point needs to be emphasized. What I have said so far does not apply in quite the same way to all theories. Not all legitimate and powerful scientific theories claim to be approximately true in the sense that I have just outlined. Scientific models may be abstractions, which incorporate only features deemed important, or idealizations, which make useful but knowingly false moves, like replacing extended masses with hypothetical point masses, or they may be a mixture of both. As Sklar¹⁵⁷ writes:

Science is replete both with schemes intended to truly characterize 'how things are' and with other schemes intended only as knowingly false but useful models of the real situation. But only the science itself can do the job of explicating the intended purpose of its own descriptive and explanatory schemes.

Thus, for example, the liquid drop model of the nucleus was a powerful device, but nobody thinks the nucleus resembles a drop of liquid (except for specific analogies useful for many important calculations). However, Sklar's statement somewhat oversimplifies the situation¹⁵⁸, since at a given time the same model/theory may be differently viewed by its supporters. For example,

¹⁵⁷ Sklar, L. 2003 'Dappled Theories in a Uniform World' *Philosophy of Science* 70, 424-441 p. 431.

¹⁵⁸ Cartwright believes that Sklar's position is false rather than an oversimplification; no scheme truly characterizes how things are. She writes: 'We construct different models for different purposes, with different equations to describe them. Which is the right model, which is the "true" set of equations? The question is a mistake.' (Cartwright, N. 1983 *How the Laws of Physics Lie* Oxford: Clarendon Press p. 11.) Cartwright's position is that we have no inductive justification for accepting the underlying Scientific Realist belief in the existence of universal fundamental laws. These laws, according to her, are either false as generalizations of such universal scope or so hedged with *ceteris paribus* clauses as to make them vacuous – the fundamental law is true except when it is not. She does not deny that these laws are both useful and wisely chosen to serve the scientist's (in her case generally physicist's) purposes. I return to this question in Chapter 5.

advocates of the computational theory of mind differ radically in the extent to which they believe that the brain resembles an electronic computer. The point is an important one with respect to the so-called Pessimistic Meta Induction – the view that the long human history of numerous successful theories, which ultimately proved to be false, should make us sceptical of Scientific Realism¹⁵⁹. Those who resist the Pessimistic Meta Induction sometimes argue that the falsely postulated entities in past successful theories were really used only as heuristic devices¹⁶⁰.

Of the avowedly false theories most are extremely useful. For example, sophisticated theories in fluid dynamics are used to make accurate predictions of the aerodynamics of aeroplane wings and ship's hulls and the like. These theories assume a liquid is a continuum, whereas we know liquids consist of atoms, molecules or ions. Such theories are not taken to be true, but usefully model some major aspects of the behaviour of fluids. In these theories a fully described model is proposed, and it is then proposed that the world is approximately like this model in relevant respects. Such a continuum theory of fluid mechanics is quite useless in trying to decide when a given liquid might

¹⁵⁹ The Pessimistic Meta Induction is a sort of negative mirror image of the famous 'no miracles argument' – science has an endless succession of failed refuted theories, so we should be sceptical of current theories. This is a related issue to that discussed in section 1.5, where the concern was that there have been successful theories whose central entities are now known not to exist – phlogiston being the best known example. However, the argument about the improved nature of modern science can again be invoked in this case to blunt the Pessimistic Meta Induction. Indeed, if this last is accepted it can be claimed that no scepticism is warranted, and that the Pessimistic Meta Induction is an example of the false positives fallacy (Lewis, P. 2001 'Why the Pessimistic Induction is a Fallacy' *Synthese* 129, 371-380).

¹⁶⁰ The Pessimistic Meta Induction is an important Scientific Anti-realist argument. Realists often argue that the relevant aspects of the successful false theories were in fact those that were true. However, Stanford (Stanford, P.K. 2003 'Pyrrhic Victories for Scientific Realism' *The Journal of Philosophy* 100, 553-572) points out that this is a poor defense, since there is good historical evidence that at the time the false theories were accepted, the presently agreed false features of these theories were viewed as essential elements in them. Hence, the Realist has a new difficulty, namely her inability to give good grounds for her current belief in the essential elements of current theory. Thus in the process of attempting to explain away the anomaly (for her) of past successful false theories, the Realist counter argument to the Pessimistic Meta Induction undermines the Realists' current belief claims. I have already referred in section 1.5, in discussing the phlogiston theory, to what I regard as a better defense for the Scientific Realist – the substantial change in the reliability of contemporary science compared with even the nineteenth century.

crystallize, since such a phase transition is wholly dependent on the fact that a liquid is not a continuum, but consists of molecules, atoms or ions. Obviously, the same is true of an attempt to explain on such a theory the random movement, known as Brownian motion, of tiny particles like pollen in a liquid. Brownian motion is caused by the bombardment of the particles by the molecules of the liquid; it was analyzed in 1905 in a fundamental paper by Einstein.

As already stressed, many scientific theories are assumed to be true or approximately true – our theories of the atom, or the nucleus, or our theory of the way metals deform plastically under stress. Scientists believe that there are molecules and electrons and protons and quarks and DNA, and these entities cause the experimental observations we make. These theories I will refer to as causal theories. A somewhat different type of theory, that can also be called causal, is something like the theory of evolution, where processes like natural selection and species diversity are believed to account for species change. Here again, scientists believe on the basis of the theory that about 300 million years ago there lived a common ancestor of ourselves and the ostriches.

Thus I have divided the theories considered into two categories¹⁶¹. First, there are those that might be called modeling theories, where an avowed idealization is claimed to approximate the world or an aspect of the world. Second, there are those that might be called causal, where a theoretical account is given of entities which are supposed to cause the phenomena of the world, and that account is approximately true, and the entities exist.

Before looking at the approximate truth of theories, I will in section 3.2 look at how approximate is used in ordinary language. This *inter alia* provides a viewpoint on two equivalent alternative definitions of approximate truth for sentences. I will then in section 3.3 and 3.4 look at approximate truth for simple and complex sentences; section 3.5 then discusses approximate truth for

¹⁶¹ It is not being suggested that these categories are exhaustive.

theories. Finally in section 3.6 I discuss the suggestion that while we cannot satisfactorily specify the approximate truth of theories, we can accept our intuitive notion of the concept.

3.2 Ordinary Language and Approximate Truth

3.2.1 Introduction

It is worth briefly examining what sorts of things can be spoken of as approximately true in ordinary usage. What sorts of things can be approximated and what sort of statements can be approximately true? Curves can be approximated – a function can be expanded in a power series and only a few terms kept. It is a separate claim to say that it is approximately true that the expansion represents the original curve. People cannot be approximately immoral, they cannot approximately move, one cannot be approximately killed. Figures can be approximately identical or square or circular. Articles cannot be approximately coloured, people can't be approximately tall or short or fat, but they can be approximately 180 cm tall. Dogs can approximately follow a circular route or precisely follow an approximately circular route. People can be approximately forty years old but they cannot be approximately old.

It is perhaps worth seeking some systematic categorization.

3.2.2 Vague Terms

One possible category seems to be terms that are not precise, such as old, young, tall, fat, beautiful, ugly, bald, hairy or pleasant or nasty or heavy or light. These terms are all significantly dependent on the standards applicable in the context in which they are applied – a hairy gorilla is somewhat differently so than is a hairy man. One might think that most things are not easily described as approximately approximate (although perhaps one can be approximately middle-aged) and indeed one does not talk of being approximately old, young, tall, fat, beautiful, ugly, bald, hairy or pleasant or nasty or heavy or light. Approximately bald seems less absurd than approximately pleasant, perhaps because it is

feasible to think of measuring baldness and less so to think of some measure of being pleasant.

Nevertheless concepts that are already approximate are tricky, as just mentioned with regard to being middle-aged. Colour terms are used in an approximate way, and snow can be approximately white. Here it is perhaps because colours, in a sense, are both precise and approximate. One could specify a range of wavelengths to be deemed red and a narrower range to be deemed magenta. It is clear to people with normal vision under good natural light that something is not magenta if it is well outside this wavelength range. On the other hand, there may well be disagreements if the sample is at the edges of the range.

3.2.3 Precise Terms

Another category consists of certain terms that are necessarily precise. A number that is not a prime is not approximately prime, even if it differs in magnitude from a number that is a prime by one millionth of one percent, or has only one divisor (other than one and itself) when other equally large non-prime numbers may have a million distinct divisors. This category is also tricky because the term rectangle is precise, but things can be approximately rectangular, and events can be approximately simultaneous, another precise term. It seems significant here that it is possible to resemble a rectangle and by nearness in time to resemble simultaneity but no number that is not a prime resembles a prime in any meaningful sense.

Another 'precise' category is that of many actions; one cannot approximately kill or move or drink or eat or laugh or cry. This is not because one cannot come very close to killing, but one either kills or one does not; whereas a rectangle with sides of length 1.000 and 1.001 is approximately a square, although it is not a square. One would say Fred very nearly killed Mary, and this rectangle is very nearly square, but one would not use approximately in the former case. The point seems to be that there is a continuous transition from rectangle to square

whereas this is not so from life to death. However, one can approximately determine, count, measure and calculate, so not all actions are ruled out.

3.2.4 Possessing Qualities

In some cases, statements or propositions can be approximately true of the possession of qualities which themselves cannot be approximate. It can be approximately true that Fred is idle or conscientious. Here there is a continuum stretching from the ideally industrious to the completely idle person, and a comparison is appropriate. It can be approximately true that I am idle, but I cannot be approximately idle. It seems in saying that it is approximately true that Fred is idle one is averaging over time (Fred might be idle most but not all of the time) or over respects (Fred might be conscientious at work but reluctant faced with domestic tasks).

3.2.5 'Averaging' Over Degrees and/or Respects of Approximation

There are questions of averaging over the degree of departure from truth. An approximately true account of the plot of Shakespeare's *Hamlet* might be one saying that eating poison had killed Hamlet's father and Ophelia killed herself by leaping off the parapet at Elsinore. The account would not be accepted as approximately true if either enough details were false in these minor ways, or if all else was correct except that it was claimed that Hamlet murdered Ophelia.

It is worth noting at this point that the foregoing assumes an acceptance of an implicit weighting scheme. For example, I have said, not unreasonably, that the statement that poison was poured in the ear of Hamlet's sleeping father is a minor detail. However, doubtless there are Shakespeare scholars who would disagree. One might claim that the sinister use of words is importantly represented by images of ears and hearing. Ears receive both truth and poison. In addition to Claudius's murder of the king there is Hamlet's claim to Horatio that 'I have words to speak in thine ear will make thee dumb'¹⁶². The ghost uses the

¹⁶² *Hamlet* Act IV Scene vi

poison poured in the king's ear to symbolize the corrosive effect of Claudius's dishonesty on Denmark's health.

3.3 Approximate Truth of Simple Sentences

Before considering the approximate truth of theories, it is worth examining the concept of the approximate truth of simple sentences. By simple sentences I mean something like 'that is green', namely logically simple sentences.

There seems to be a problem with two obvious accounts of approximate truth of sentences. The first, given by Horwich¹⁶³:

(1) 'Snow is white' is approximately true iff snow is approximately white.

The second given by Smith¹⁶⁴:

(2) 'Snow is white' is approximately true iff approximately snow is white.

Smith qualifies this by insisting that the modifier 'approximately' can sensibly be applied to the proposition¹⁶⁵, which it clearly can in my example.

In (1) the approximation operator is modifying the property and in (2) it is modifying the proposition. These are generally identical in intent and application – the only way they can be seen to differ in the example above would be on the eccentric interpretation that (2) was referring to something known as 'approximately snow'. However, the second formulation might be better in general, as can perhaps be vindicated by comparing (3) and (4).

(3) 'Fred is idle' is approximately true iff Fred is approximately idle.

The second given by Smith:

(4) 'Fred is idle' is approximately true iff approximately Fred is idle.

Problems arise when we explore what we mean by saying something is approximately true.

¹⁶³ Horwich, P.G. 1990 *Truth* Oxford: Blackwell p. 66.

¹⁶⁴ Smith, P. 1998 'Approximate Truth and Dynamical Theories' *British Journal for the Philosophy of Science* 49, 253-277.

¹⁶⁵ Smith, P. 1998 'Approximate Truth for Minimalists' *Philosophical Papers* 27, 119-128 p. 122.

(5) 'Figure A is square' is approximately true iff approximately Figure A is square.

Now does this mean that the figure has approximately (four sides, four straight sides, four sides of equal length, four equal internal angles of $\pi/2$, four-fold and twofold rotation symmetry)?

One's first intuitive response is perhaps that for (5) to be acceptable, Figure A has to be near to having all of these attributes. How near is not clear.

However, suppose it turns out that the four sides of Figure A are not straight; they are arcs of circles of large radius of curvature or lines whose gradient changes infinitely many times in a finite length. The equation for these does not resemble the equation for a straight line. Now it can truly be said that Figure A looks like a square and it is not a square, but is it approximately true that it is a square. Moreover, if it had none of the attributes of a square, it could still look extremely like a square and in some vague sense still be like a square. For example, it could have all its corners very slightly and differently truncated. It would not then be unreasonable to say that it was approximately square. This is despite the fact that it has none of the defining attributes of a square, and despite the fact that it now has eight sides, which could hardly be called near to having four sides.

These remarks seem to point to three issues. First, we do not always have an obvious criterion for saying something is approximately true, even in the simple case where there is only one variable, like the whiteness of snow. How near must snow be to white? Second, if there are respects as well as a degree in the definition of something, in which respects must there be nearness? It seems that in different circumstances it may be all, some, or none. Third, if we are to make comparisons as to whether a given sentence is more nearly approximately true than another sentence, there is an immediate question concerning which respects are most salient.

The first of these issues seems only to be the usual problem with vague predicates. Someone who is thirty nine years old is approximately forty, and thirty seven might be acceptable and thirty unacceptable.

The second and third issues require that generally we can only speak of approximate truth with respect to some criterion of salience. If symmetry is paramount, and retaining all the symmetry properties is the criterion, then a circle approximates a square more closely than a rectangle does. If right angles and straight sides matter, then a rectangle of edge lengths a , b with $b \neq a$, and say $a < 1.01b < 1.01a$ resembles a square more closely than does a circle.

3.4 Approximate Truth of Sentences Involving Logical Vocabulary

3.4.1 The Existential Quantifier

As I remarked earlier, it is not unreasonable to say:

'Snow is white' is approximately true iff snow is approximately white.

However, one's intuition is that there is something seriously wrong with:

(6) ' π is given by a rational number' is approximately true iff π is given approximately by a rational number or,

(7) ' π is given by a rational number' is approximately true iff approximately π is given by a rational number.

Statement (6) is false: the left hand side of the biconditional is false, $\pi \neq p/q$ where p and q are integers; the right hand side is true $\pi \approx 3.141592654$.

Statement (7) seems difficult to interpret. The left hand side of the biconditional is false, and on the most obvious, albeit dubious, interpretation the right hand side is also false – compare approximately 1.01 is given by an integer.

Now we can symbolize that π can be given approximately by a rational number as ' $(\exists x)(\pi \approx x \ \& \ x \in \text{rationals})$ '. All Horwich or Smith assert in (1) and (2) is that 'x

is F' is approximately true iff x is approximately F – nothing has been said about how approximate truth is defined for sentences of the form: $(\exists x)(x \text{ is } F)$. Now that ' $(\exists x)(x \text{ is } F)$ ' is true cannot require that a sentence of the form 'x is F' is true, since there may not be a relevant named object. Thus it may be true for example that there exist macroscopic objects between 11,000 and 11,001 light years from the Earth, but none is named. The same applies to approximate truth. However, if a sentence 'x is F' is true, then it follows that ' $(\exists x)(x \text{ is } F)$ ' is true. In contrast, in the case of approximate truth, 'x is approximately F' does not imply ' $(\exists x)(x \text{ is } F)$ ' is approximately true.

This can be put more formally.

For *reductio* assume

(1) 'x is F' is approximately true entails '(There is an x such that x is F)' is approximately true.

Where F is any predicate

(2) 'Fx' is approximately true iff $\text{Approx}Fx$.

Where 'Fx' is an open sentence.

Hence,

(3) $\text{Approx}Fx$ entails '(There is an x such that x is F)' is approximately true, hypothetical syllogism (1) and (2).

Hence,

(4) π is approximately equal to a rational number entails ' π is equal to a rational number' is approximately true.

(4) is false, hence (1) is false.

In other words, 'There is something that is F' is approximately true is not the same as: 'There is something that is approximately F'. For example, take $F \equiv$ equal to a transcendental number¹⁶⁶. We can consider the statement, 'There is an integer that is F is approximately true.' This is false. Whereas the statement:

¹⁶⁶ A transcendental number is a number which is not the root of a single variable algebraic equation with integral coefficients, like $5x^3 + 7x^2 + 4x + 7 = 0$.

'There is an integer that is approximately F' is true. The integer 377 is approximately equal to the transcendental number 120π (actually very close to 376.99). Thus a number is either equal to a transcendental number or it is not, but a number that is not equal to a transcendental number can be approximately equal to one. Indeed it always is to any desired degree of approximation. It is not possible to be both an integer and a transcendental number, but every integer differs from a transcendental number by as little as one cares to specify.

Of course, 'There is something that is F' is approximately true may be true at the same time as: 'There is something that is approximately F'. We might say this of a heap of coloured papers ranging from teal to violet. There is something that is approximately blue and it is approximately true that there is something that is blue.

Thus the existential quantifier does not in general approximate.

3.4.2 The Universal Quantifier

A statement like, 'everything is F is approximately true' needs to be accompanied by a consideration of what is meant. There are three obvious possibilities.

- (8) For all x, x is F or x is approximately F
- (9) x is F for nearly all x
- (10) x is F or approximately F for all or nearly all x

Consider the sentence S:

'Every planet in a certain planetary system is spherical' is approximately true.

Case1.

All but one of the planets is spherical and one is approximately spherical.

S is then true under all of (8) (9) and (10).

Case 2:

All but one of the planets is spherical and one is an oblate spheroid of great eccentricity.

Under (8) S is false, under (9) and (10) it is true, nearly everything is F.

Case 3:

All but one of the planets is approximately spherical but not precisely spherical and one is an oblate spheroid of great eccentricity.

Under (8) S is again false. Under (9) S is also false; under (10) S is true; nearly every planet is nearly spherical.

The version of (8) (9) and (10) that perhaps best accords with the usual intuitions of nearness to truth is (8). It seems plausible first to consider (8) as a possible basis for discussing approximate truth in the context of scientific theories. A serious difficulty with (9) and (10) is the vagueness of the phrase 'nearly all'. Moreover, a well established single serious exception is a cause for profound concern in science.

For (8), consider the following sentence,

(11) 'Everything Socrates says is true' is approximately true iff nearly everything Socrates says is true or approximately true.

Now *ipso facto* the predicate true can be qualified by approximately.

Nevertheless, (11) is false. Nothing that Socrates says may be true, while everything that he says may be approximately true. He may say only things like, the population of South Africa is 40 million, $\pi = 3.142$.

For (9) consider the sentence,

(12) 'Everything Socrates says is true' is approximately true iff nearly everything Socrates says is true.

This might seem to be false, as Socrates may utter a vast number of platitudes but also a significant number of statements like 'there is a greatest prime', 'the square root of every integer is a rational number', and the like which may imply an unlimited number of falsehoods. However, if we confine ourselves to the statements made by Socrates, i.e. not their implication, then (12) seems correct and (9) and (10) are acceptable.

However in the context of scientific theories this approach seems implausible – some generalizations are the norm and their implications are of central importance. That is, problems with the use of universal quantification are of concern when approximate truth is being invoked.

3.5 Approximate Truth of Scientific Theories

It is difficult to ignore the difficulties considered thus far, since scientific theories do make vital use of universal quantification and of existential claims. 'Every body continues in a state of rest or of uniform motion in a straight line ...' Causal theories as I initially described them seem to depend on the use of approximate truth of complex sentences.

There is a further difficulty with the approximate truth of theories.

If we consider a theory as a set of core statements P, Q, R ... and the theory is true if each of P, Q, R ... is true, this will usually involve also the truth of (P&Q&R ...) which one is entitled to assume. As Smith¹⁶⁷ points out in the case of approximate truth as distinct from truth,

... how do we move the approximation operator across the conjunctions?
Approximately (P & Q & R ...) is not equivalent to 'approximately P & approximately Q & approximately R ...', since the latter plainly does not imply the former. We will therefore need, in particular cases, some principled way of distributing the effect of the approximation operator across a theory conjunction.

For example, something that actually weighs 1001 kg weighs approximately 990 kg and also weighs approximately 1000 kg, but it would be incorrect to say that it was approximately true that it weighed 990 kg and 1000 kg.

¹⁶⁷ Smith, P. 1998 'Approximate Truth and Dynamical Theories' *British Journal for the Philosophy of Science* 49, 253-277 p. 258.

This example is somewhat misleading since the two approximations given are mutually exclusive. In dealing with scientific theories it should be the case (although it cannot always be known with certainty) that the postulates are not mutually exclusive. In this case approximately P & approximately Q & approximately R... is commonly equivalent to approximately (P&Q&R...) ¹⁶⁸. Hence the problem as set up by Smith does not arise. Nevertheless, it is true that the use in a theory asserting approximately true statements P, Q, R... can lead to outcomes that are not approximately true. Thus 40 terms in error each by 1% can lead to a large error in their product.

Smith believes that an overall account of approximate truth is not possible. He believes that the problem must be differently considered for particular classes of theories; in addition, that in each case approximate truth will be interest relative; and that the problem is commonly insoluble, essentially for the reasons I have given in relation to simple sentences and logically complex sentences.

However, Smith claims in one special case that there is a legitimate way to talk about the approximate truth of theories. These are what he calls geometrical modeling theories (GM theories) and, as Smith says, these constitute quite a large class of scientific theories, at least in physics. These belong to the class I have called modeling theories.

I think that these theories do not offer any comfort to Scientific Realists in supplying a notion of the approximate truth of theories that serves their needs.

A GM theory has two components – component M is the purely mathematical model and component A is the geometrical structure to be found in some real

¹⁶⁸ There is a complication in that it may be the case that the likelihood of the approximate truth of P is reduced by the likely approximate truth of Q, thus affecting the likely approximate truth of (P & Q). Thus care is necessary in asserting the approximate truth of (P & Q) even when there is not evident incompatibility. (Putnam, H. 1973 'Explanation and Reference' reprinted in *The Philosophy of Science* R. Boyd, P. Gasper and J.D. Trout (Eds) Cambridge MA: MIT Press p. 181.)

world phenomenon. This is most easily illustrated by an example. We can take as a model of a simple pendulum that it consists of a massless inextensible string and a point mass and there is no friction. The mathematics of this model system can be handled exactly. The theory then is that a real system can be described by this model¹⁶⁹. Thus, since there are no approximations in M, we can say

(10) If '(M & approximately A)' then approximately (M & A).

Which as Smith acknowledges says no more than if '(bachelors are unmarried men and approximately $T = 2\pi\sqrt{l/g}$)' then approximately (bachelors are unmarried men and $T = 2\pi\sqrt{l/g}$).

The heart of Smith's claim is that GM theories are such that from M we can define a phase space in which the trajectories of the variables are plotted. If the trajectories derived from M depart from the experimentally observed trajectories in this defined phase space everywhere by an amount less than some ϵ , we can justifiably say we have an approximately true theory. As Smith puts it¹⁷⁰

... a GM-theory is approximately true is to be taken as the claim that the geometrical structure in question approximately replicates the relevant structure to be found in the target real world phenomenon ... where the structures in question are both curves embedded in the same space ... it is natural to say that one structure approximates to the other if the first curve can be distorted into the second by a transformation which (a) moves points by no more than some ϵ , and (b) preserves smoothness ...

In other words, over the whole range of the variables the departure from that predicted by theory must be small. This is intended as a stipulation of what is meant by approximate truth, as I will argue. The merit of this approach is that we

¹⁶⁹ This is of course a Newtonian model in a non-Newtonian world. One cannot have an inextensible string in a relativistic world, since signals could be propagated along it instantaneously.

¹⁷⁰ Smith, P. 1998 'Approximate Truth and Dynamical Theories' *British Journal for the Philosophy of Science* 49, 253-277, p. 259.

have a precise definition of empirical adequacy for a GM theory. But it tells us little about approximate truth that is relevant to the claims of the Scientific Realist.

While a model GM theory is a given, it is not a self-evident truth that the parameters of the experimental situation will be the same as those of the model. To take the simple pendulum example, one cannot assume that the motion of a real pendulum will remain in the plane of the original motion. The mass is not a point mass, nor is it uniform, the string is extensible and we have air resistance. The geometrical model says nothing, for example, about motion out of the plane of the initial motion, that is, the conceptualization and the structure in the world do not necessarily occupy the same phase space (variable range) and it begs the question of its approximate truth to assume that it does so.

To continue with the simple pendulum case, if we assume that the model of a light inextensible string and a point mass is approximately true and we find that the parameters we measure on this assumption differ by an amount as small we please (or more realistically a small as we can measure) from those predicted on this assumption, then we supposedly have an approximately true theory. But this begs the question. This is so whether it is a GM theory or any other sort of theory. Smith has 'defined' approximate truth as empirical adequacy in circumstances where empirical adequacy is precisely defined in terms of a theoretical model that is exact. Now this is perhaps an improvement on the many cases where one cannot even give a precise definition of what one means by empirical adequacy, but it is no solution to the problem of approximate truth in the context of experimental science. None of this is to deny that the greater the explicitness of the system variables, and the higher the degree of agreement between theory and experiment, the more convinced is the scientist of the truth of her theory. This seems very reasonable because it is what one would expect if the theory were true; however it is not germane to the question of avoiding the problems of the concept of approximate truth for scientific theories, even in the case of GM theories. As already mentioned, the central problem with

approximate truth is that it is indeterminate in relation to relevant respects as well as degree.

As Chakravartty¹⁷¹ puts it: 'From a realistic perspective, talk of geometrical structures is at best a shorthand for finer grained theoretical claims which are either true or false, concerning those parameters whose values map out geometrical structures to begin with.'

3.6 Intuitive Approximate Truth

3.6.1 Approximately True Theories

Psillos comes to the conclusion that no formal definition of approximate truth is possible¹⁷² and offers what he refers to as an intuitive account which he believes is satisfactory. He rightly remarks that real science proceeds without a defined notion of approximate truth, despite the fact that its theories are not true. As he says, idealizations and simplification are necessary in theories explaining and describing empirical observations; experimental results are never free of error.

Psillos gives an account of his intuitive notion of approximate truth as follows¹⁷³:

a theory is approximately true if the entities of the general kind postulated to play a central causal role in the theories exist, and if the basic mechanisms and laws postulated by the theory approximate those holding in the world, under specific conditions of approximation.

He spells this out more formally as 'A description D is approximately true of a state S if there is another state S' such that S and S' are linked by specific conditions of approximation, and D is true of S'.'

¹⁷¹ Chakravartty, A. 2001 'The Semantic or Model-theoretic View of Theories and Scientific Realism' *Synthese* 127, 325-345 p. 341.

¹⁷² Psillos, S. 1999 *Scientific realism: how science tracks truth* New York: Routledge p. 276

¹⁷³ Psillos, S. 1999 *ibid* p. 277.

Intuitively one believes that one can be nearer to or further from the truth. As an instance of an approximately true theory Psillos says that the orbit of Mars around the Sun can be approximated by an ellipse and this is derived from the assumption that the Mars-Sun interaction can be treated as a two body problem in classical mechanics because the gravitational effects on Mars from the other planets can be taken to be negligible. The error resulting from this 'approximately true' theory can be estimated. There are innumerable similar examples of very good agreement between approximate theories and experiment (where the approximate theory is often also explanatory). Indeed this is similar to the Smith account of GM-theories discussed above.

The Mars-Sun example, Psillos says, is truthlike because the approximations made result in small errors. Moreover, the extent of error can be specified. We have, quite generally, an intuitive notion of this which can be spelled out in various ways. The fact of small errors alone is not sufficient for an approximately true theory, but here the fact that the errors can be accounted for within the assumptions of the theory combined with its generality and explanatory power, Psillos argues, is enough for the Scientific Realist. The argument has been questioned as a general defense of approximate truth, as Smith is at pains to make clear in his narrower defense of the validity of the concept for GM-theories¹⁷⁴

But what of claims like 'Bohr's theory of the atom was approximately true' or 'Mendelian genetics is approximately true' or 'the theory of mind deficit of autism is approximately true'? On our framework account to say that Bohr's theory of the atom was approximately true is to assert *approximately* B, where B is a long story encapsulating Bohr's theory. And how are we to construe that? Without some extended gloss (e.g. about which parts are supposed to approximate to what according to what metric) the assertion *approximately* B is simply opaque; likewise for other cases.

¹⁷⁴ Smith, P. 1998 'Approximate Truth for Minimalists' *Philosophical Papers* 27, 119-128 p. 127.

As Psillos is aware, the intuitive notion as exemplified in the Mars-Sun example does little more than confirm that empirical adequacy can be understood and justified. (There is, of course, a very real sense in which Newton's theory is not approximately true.)

However, I think Psillos and similar thinkers are not, in using 'intuitive', asking us to trust our intuitions, although some writers do invoke them in their support. The claim is rather that approximate truth or truthlikeness is both vague and a cluster concept, meaning different things in different circumstances. It is none the less a legitimate concept as is the famous concept of a game, with games having family resemblances without necessarily having any one common feature¹⁷⁵. We know what games are and we know what approximate truth is.

Bueno¹⁷⁶ objects that approximate truth has been defined by Psillos in terms of truth, and given that the Scientific Realist is interested in truth we need therefore to get to truth via truthlikeness. Either truth is a limiting case of truthlikeness¹⁷⁷, which seems unacceptable given that truthlikeness is defined in terms of truth, or we need some other route to truth. However, unlike truth, approximate truth is context dependent as discussed earlier (it is approximately true that the earth is spherical in the context of space exploration but not in the context of international aviation). This seems to create an unbridgeable gap between the two concepts, since truth is not so dependent. However, it can be argued against Bueno that it is proper to define approximate truth in terms of truth, and then to justify this definition by virtue of experimental results which demonstrate the convergence of approximate truth on truth in each of the circumstances where a theory is claimed to be approximately true. Thus Psillos might say $PV=RT$ is approximately

¹⁷⁵ Wittgenstein, L. 1953 *Philosophical Investigations* Oxford: Blackwell § 66. Worrall remarks of this sort of strategy, 'anti-realists might be tempted to regard this as an example of the well known injunction: "if you can't get what you'd like, like what you can get."' (Worrall, J. 1994 *The Ontology of Science* Vermont: Dartmouth p. xix.).

¹⁷⁶ Bueno, O. 2001 'Quests of a Realist' *Metascience* 10, 341-370 p. 361.

¹⁷⁷ This is the proposal of Aronson *et al* (Aronson, J.L., Harre, R. and Way, E.C. 1994 *Realism Rescued* London: Duckworth) which is plausibly rebutted by Psillos (1999 *op. cit.* pp. 270-273).

true for the inert gases at high temperatures and low pressures, and experiments will 'confirm' this.

The question that naturally arises is how effective is an intuitive notion of approximate truth for the purpose of vindicating McMullin's account of the McMullin Conjunction. This is taken up in the following section.

3.6.2 McMullin's Argument and Intuitive Approximate Truth

The important question for our purposes is whether the notion of intuitive approximate truth of theories can serve to underpin McMullin's argument for Scientific Realism. It remains necessary for McMullin to be able to compare the relative approximate truth of different versions of the successful theories since his claim is that successive versions are more accurately describing the hypothesized mind-independent world.

On the intuitive concept comparing theories will have to be done with regard to some criterion or criteria, as was discussed above in relation to a decision as to which imperfect figure most closely resembled a square. Now one might compare theories on the basis of their empirical adequacy, with some agreed weighting for the variables considered. For example, in comparing theories of the characteristic of real gases one could consider comparing the prediction of the pressures at various temperatures at constant volume. Alternatively or additionally one might compare the values of the critical constants. However, as soon as these pragmatic criteria are introduced, the improvement over time of successive versions of the theories, as judged by the chosen criteria, ceases to provide a good argument for a Scientific Realist account along McMullin's lines. All that has now been shown by the improved performance is that a better fit to some subset of the data has been found, i.e. the later version of the theory is better than the earlier for some purpose. For McMullin's argument to go through he needs to argue that the successive versions of the theory are better theories (and are closer to the hypothesized true theory). An improved fit with a

necessarily somewhat arbitrarily weighted subset of observations is inadequate for the Scientific Realist argument McMullin is proposing. In fact a Scientific Realist might well be disposed to acknowledge that a theory near to giving a true description of the mind-independent world might well give poor agreement with some interpretation of the data assumed to be important in a theory adjudged to be approximately true¹⁷⁸.

The Scientific Realist assumes that there is an unknown true theory. The necessarily vague notion of approximate truth of a scientific theory is how close the theory is to the hypothesized true theory. She judges the approximate truth of previous theories on the basis of nearness to the current best theory. In addition the salient interests used in making this judgment are also based on this current best theory. Consequently, as a basis for McMullin's explanation of the McMullin Conjunction this argument must make the assumption that the current best theory is approximately true. For example, if Descartes vortex theory were the current best theory, the observation that the planets all orbit the sun in the same direction would support the belief that the theory was approximately true. In fact, of course, this has nothing to do with the explanation of the stable motion of the planets in their orbits and the periodicity of this planetary motion, but is a consequence of the genesis of the solar system.

There is a further point in relation to the way approximate truth of a scientific theory is specified as being close to the hypothesized true theory. Psillos in his account of intuitive approximate truth refers to the 'specific conditions of approximation' but does not spell out how these are to be judged. A scientific theory is approximately true iff if it is judged to be close to the Scientific Realist's hypothesized true theory. The judgment of approximate truth is made on the basis of the current best scientific theory, and hence the salient respects used in

¹⁷⁸ It is not being suggested that correctly interpreted experimental results would conflict with a true theory. Rather an approximately true theory (by Scientific Realist lights) might wrongly suggest that a correctly measured variable should vary in a certain way with another and it might indeed do so. A better theory might (rightly) have no explanation for the observed variation.

this judgment are also based on the current best theory. If the current best theory was far from the truth¹⁷⁹, an erroneous judgment would probably be made on this basis. Irrelevant or minor variables would be treated as significant so that a proposed revision of the theory could well be judged an improvement when it was in fact further from the Scientific Realist's hypothesized true theory. Consequently, for the successive revisions of the theory to be based on a rational assessment, the current best theory must be taken to be approximately true, and so the approximate truth of a scientific theory is being accounted for in terms of the approximate truth of a scientific theory. This contrasts, for example, with our knowledge that it is approximately true that the former standard meter kept in Paris is one meter long. (The meter is now defined as the distance light travels, in a vacuum, in $1/299,792,458$ seconds, with time measured by a cesium-133 atomic clock.) In this last case we know with great precision what it is to be one meter in length, and we can set a precise value on the maximum departure of the former standard meter from one meter and express this as a fraction of the length of the standard meter.

An important general point here with regard to the approximate truth of theories is that scientific theories are not normally merely narratives¹⁸⁰, for example like those given in discussing a well defined ideal entity like a simple pendulum. Here we are given by fiat an inextensible string, a point mass, a frictionless environment, a constant gravitational field, planar motion and a Newtonian world. All of these idealizations are false and unattainable in any experimental set-up but they allow a complete (fictional) story. However, the notion of approximate truth of a scientific theory only makes sense against a circumscribed set of background questions (specified independently of the theory), and that is not in general possible for an interesting scientific theory, which must be interrogated in any number of ingenious and unanticipatable ways. That indeed is the basis of

¹⁷⁹ Psillos is aware of the difficulty discussed here but discounts it on the basis of the 'impressive predictive successes' of current theories.

¹⁸⁰ As McMullin remarks, 'The language of theoretical explanation is of a quite special sort. It is open-ended and capable of ever further development' (McMullin, E. 1984 'A Case for Scientific Realism' in *Scientific Realism* J. Leplin (Ed) Berkeley: University of California Press p. 36).

experimental science. As Ritchie¹⁸¹ comments in another context, ‘Science forces us to revise our conceptions of what is and is not possible, of where a concept can be meaningfully deployed and where it can’t. Who knows how things will look in the future?’

Thus the discussion, given in section 3.2.5 above, about the problems in deciding whether a brief account of the plot of Shakespeare’s Hamlet might be approximately true, deals with the difficulties of giving appropriate weight to known facts about the text. The problem of say the approximate truth of the current sophisticated theory of the development of the mammalian embryo is altogether more intractable. We have very little idea of how future developments in biochemistry, molecular biology, and genetics (or indeed in other relevant branches of science) will impact on our assessment of the present theory.

3.7 Conclusion

It seems that we cannot sustain a well defined view of the approximate truth of scientific theories. Our intuition that some statements can be further from the truth than others lends credibility to the notion of approximate truth. However, this intuition is a little undermined by Brink’s¹⁸² rhetorical question: ‘given that spiders have eight legs, are we all agreed that the theory that spiders have seven legs is better than the theory that they have six?’ More importantly the intuition stems primarily from our common use of the notion in circumstances where there is only one variable. To enable talk of the approximate truth of scientific theories requires a more closely specified account than is required in the case of our normal acceptance that someone 199 cm in height is approximately 2 metres tall or that a closed account of some series of past events is substantially accurate.

Even if one were prepared to accept a necessarily imprecise notion of the approximate truth of theories, McMullin’s Inference to the Best Explanation from

¹⁸¹ Ritchie, J. 2008 *Understanding Naturalism* Stocksfield: Acumen p. 147.

¹⁸² Brink, C. 1989 ‘Verisimilitude: Views and Reviews’ *History and Philosophy of Logic* 10, 181-201, p. 196.

the conjunction of long standing success and P-fertility is poorly motivated on such a basis. This is because the successive versions of the theories cannot convincingly be seen as progressively better theories rather than merely seeming more empirically adequate on the basis of the presumed approximate truth of the current best theory.

A major attempt to get out of the problems associated with the concept of the approximate truth of theories has been to use the concept of verisimilitude instead of approximate truth. This is taken up in Chapter 4.

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CHAPTER 4 VERISIMILITUDE

4.1 Introduction

In Chapter 1 it was suggested that the conjunction of P-fertility and long standing success might provide grounds for Scientific Realism. This was because the successful successive modifications of a scientific theory could perhaps not be explained so readily on the basis of an alternative Instrumentalist or other Scientific Anti-realist stance. That is, while the success of science (particularly the predictive success), is seen by some as grounds for Scientific Realism, it can perhaps be explained otherwise; in contrast McMullin argues P-fertility cannot. However, it is essential to the Scientific Realist position that science is progressive, that is, it is in general the case that successive scientific theories approximate more closely to some hypothesized true theory. It is also essential that not only is this so, but in addition the current well established theories are in general close to this hypothesized true theory. The problematic notion of the approximate truth of theories was explored in Chapter 3 and there seem to be substantial difficulties in making the notion clear. This chapter considers the alternative to approximate truth commonly advanced, namely the concept of verisimilitude¹⁸³.

Verisimilitude invokes nearness to the Truth, that is, to the 'one whole truth'. It incorporates notions like information (minimizing ignorance) as well as avoiding error. Rather than saying that a theory is approximately true, we can consider how near it is to the 'Truth' where the 'Truth' is the whole truth and nothing but the truth. In this sense, a false theory may be nearer the Truth than a true theory by virtue of saying more true things than a theory that, for example, says a small number of obviously true things. Or a false theory may only be true of a set of 'close' worlds whereas a true theory may be true of many including 'very distant'

¹⁸³ Numerous authors use the term truthlikeness as a synonym for verisimilitude; some however use it as a synonym for approximate truth (see, for example, Kukla, A. 1998 *Studies in Scientific Realism* Oxford: Oxford University Press p. 14).

worlds. The essential point here is that verisimilitude takes into account the importance of information content in a theory, and not merely the avoidance or the minimization of error.

Popper¹⁸⁴ was the first to give an account of verisimilitude. Because of his combination of a falsificationist stance and his enthusiasm for science as a source of knowledge, Popper needed to justify the view that, in general, later (false) theories, which were to displace falsified theories, were in fact superior.

Popper's proposal is that a theory Y is superior to a theory X in truthlikeness iff

either $X \cap T \subseteq Y \cap T$ and $Y \cap F \subset X \cap F$

or $X \cap T \subset Y \cap T$ and $Y \cap F \subseteq X \cap F$

Where T is the set of true sentences and F is the set of false sentences. This then relies on theory content and deductive logic alone.

This approach has the hugely attractive feature that it makes no use whatsoever of the notion of approximate truth. Statements are either true or false. At the same time, Popper captures the notion that theories are better if they correspond more closely to the way the world is.

As we shall see, Popper's definition of verisimilitude is untenable. Nevertheless, a number of authors defend the concept in a modified form. They argue that some sort of weighted sum over distances from the many truths that constitute the whole truth is a defensible development of Popper's concept. However, as I will argue, the problems identified as problems of approximate truth infect such concepts in their application to scientific theories.

In section 4.2 I discuss Popper's proposal. In section 4.3 I discuss verisimilitude as it has been defended post-Popper, and consider the difficulties posed by the

¹⁸⁴ Popper, K.R. 1963 *Conjectures and Refutations*, New York: Harper and Rowe p. 391. I discuss below the fact that the theory has a fatal flaw.

modern concept. In section 4.4 I discuss the problems of applying the concept to ranking scientific theories in a given domain even if the difficulties posed by the concept itself could be circumvented.

4.2 Popper's Verisimilitude

Popper's theory has attractive features within the class of true theories. First, the whole truth is best. Second, the set of logical truths is at the bottom of true theories, which makes sense, since it is the least informative set. Unfortunately, the account has a disastrous flaw – it can readily be proved¹⁸⁵ that it entails that no false theory is nearer to the truth than any other false theory. Since we are concerned entirely with false theories, hopefully of progressively greater merit (truthlikeness), this approach seems unhelpful.

It is worth giving the derivation of this important result. I use that given in a review by Oddie¹⁸⁶.

Let us suppose that A and B are both false, and that A's truth content exceeds B's. Let a be a true sentence entailed by A but not by B. Let f be any falsehood entailed by A. Since A entails both a and f the conjunction a&f is a falsehood entailed by A, and so part of A's falsity content. If a&f were also part of B's falsity content B would entail both a and f. But then it would contain a contrary to the assumption. Hence a&f is in A's falsity content and not in B's. So A's truth content cannot exceed B's without A's falsity content also exceeding B's. Suppose now that B's falsity content exceeds A's. Let g be some falsehood entailed by B but not by A, and let f, as before, be some falsehood entailed by A. The sentence f materially implies g is a truth, and since it is entailed by g, is in B's truth content. If it were also in A's then both f and f materially implies g would be consequences of A and hence so would g,

¹⁸⁵ The proof was given independently by Miller (Miller, D. 1974 'Popper's Qualitative Definition of Verisimilitude' *British Journal for the Philosophy of Science* 25, 168-177) and Tichy (Tichy, P. 1974 'On Popper's definition of Verisimilitude' *British Journal for the Philosophy of Science* 25, 155-160).

¹⁸⁶ Oddie, G. 2000 'Truthlikeness' *The Stanford Encyclopaedia of Philosophy*, (Fall 2000 Edition) Edward N. Zalta (Ed) <http://plato.stanford.edu/archives/fall2001/entries/truthlikeness/>.

contrary to the assumption. Thus A's truth content lacks a sentence, f materially implies g, which is in B's. So B's falsity content cannot exceed A's without B's truth content also exceeding A's.

[Oddie seems to have omitted quotation marks around some expressions but his meaning is perfectly clear.]

The argument is in essence saying if we try to get an improved theory B from a false theory A by adding truths to A, we add falsehoods to B that are not falsehoods from A. If we try to get a better B from A by subtracting falsehoods from A we subtract truths from A that are not truths of B (when errors of falsity are decreased errors of ignorance are increased¹⁸⁷).

It might be argued that one need consider only the subset of theories that is empirically adequate and that for these one needs to consider only truth content as a guide to relative merit. However, this leads to an unsatisfactory position. Consider the following two theories. 'Roses are red', 'grass is green', and 'elephants are fish.' 'Roses are red', 'grass is green', and 'all living creatures are fish'. The latter does not seem more truthlike. It is also true that if we consider truth alone, a false theory may be a better theory than a true theory. However, this is acceptable, and indeed will be common since a tautology is true but contains little truth.

An alternative approach to salvaging Popper's theory might be to consider only atomic sentences. Such a move would defeat the flaw in Popper's theory. However, this does not seem acceptable. A theory is answerable for logical deductions from its postulates¹⁸⁸, and, in any case, some postulates may not be

¹⁸⁷ Niiniluoto, I. 1999 *Critical Scientific Realism* Oxford: Oxford University Press p. 67.

¹⁸⁸ An attempt to rescue Popper's concept has been made via denying this proposition. It can be argued that scientists do not want all the logical consequences of a theory – some are irrelevant and/or superfluous. Thus if P is a consequence of theory T, it is of no interest that (P or Q), for arbitrary Q, is a consequence. It is such irrelevant or superfluous consequences that are used to undermine Popper (Brink, C. 1989 'Verisimilitude: Views and Reviews' *History and Philosophy of Logic* 10, 181-201 p. 196).

expressible as atomic sentences (e.g., a body continues in its state of rest or uniform motion ...). It may however be possible to assess content in terms of atomic sentences included or entailed by the postulates. However, this would be a formidable task for a natural language and probably also for a more restricted scientific language.

There are important additional concerns relating to Popper's argument. It is a very inadequate measure of a theory to simply count the number of bull's eyes and the number of misses a theory makes.

The Scientific Realist claims not only that the observational consequences of scientific theories are true or approximately true, but also that the postulates of the theory are true or approximately true and that the entities postulated exist. Popper's verisimilitude is silent about this. (He was entitled to his silence since he believed that theories are merely careful and powerful explanatory conjectures which scientists must attempt to falsify. Popper's falsificationist stance says nothing about nearness to the truth – a later theory is better than an earlier one because it has not yet been falsified.)

As Oddie says¹⁸⁹

The fundamental problem with the original content approach lies not in the way it has been articulated, but rather in the basic underlying assumption: that truthlikeness is a function of just two variables — content and truth value. This assumption has a number of rather problematic consequences.

Two things follow if truthlikeness is a function just of the logical content of a proposition and of its truth value. Firstly, any given proposition A can have only two degrees of verisimilitude: one in case it is false and the other in case it is true. This is obviously wrong. A theory can be false in very many different

¹⁸⁹ Oddie, G. 2000 *ibid.*

ways. The proposition that there are eight planets is false whether there are nine planets or a thousand planets, but its degree of truthlikeness is much higher in the first case than in the latter. ... Secondly, if we combine the value of content for truths and the value of content for falsehoods, then if we fix truth value, verisimilitude will vary only according to amount of content. So, for example, two equally strong false theories will have to have the same degree of verisimilitude. That's pretty far-fetched. That there are ten planets and that there are ten billion planets are (roughly) equally strong, and both are false in fact, but the latter seems much further from the truth than the former.

4.3 Verisimilitude Post Popper

Various subsequent writers have proposed that verisimilitude is nevertheless a viable criterion for judging the relative merits of different theories.

Niiniluoto¹⁹⁰ illustrates the problems of finding a motivated basis for theory comparison with the following anecdote. (Note that in the following account distance from the truth of particular sentences is considered, not merely their truth or falsity, reinforcing the point made by Oddie as cited above.)

Suppose that you drive on Friday through a village with your yellow car. Suppose further that three villagers give later their report about this occasion. Mr A says that he saw you driving on Wednesday in an orange car; Mr B claims that he saw you in a red car on Wednesday; and Mr C insists on having seen you driving on Thursday with a red car. Whose report is closest to the truth?

Most of us would agree that Mr A's report has more truthlikeness than Mr B's, since they make the same mistake about the day and A's mistake concerning the colour is smaller than B's mistake. For the same reason, Mr C's report is closer to the truth than Mr B's. However, no easy answer is available in the comparison of the reports of Mr A and Mr C. Two alternatives seem possible here: (1) one may follow Keynes and say that the reports of Mr A and Mr C

¹⁹⁰ Niiniluoto, I. 1978 'Truthlikeness' *Synthese* 38, 281-329 p. 296.

belong to "different orders of similarity", i.e., they are incomparable with respect to similarity (cf. Keynes, 1921, *A Treatise on Probability*, London: Macmillan (New Edition, New York: Harper and Rowe, 1962 pp. 36 – 38)); or (2) one may decide how much weight is given to the relative mistakes with respect to the date and to the colour, and then obtain, relative to this decision, a measure for the relevant degrees of similarity. The latter alternative makes all the given reports comparable, but only at the price of the introduction of pragmatic factors into a measure of truthlikeness.

Miller¹⁹¹ pointed out this problem with the various measures of distance from the truth in the verisimilitude accounts in the literature. It is often called the language dependence problem. Essentially, Miller claims that the similarity-distance relationships thus far proposed are not translation invariant. They depend on the language used in perfectly inter-translatable languages, and hence are inherently arbitrary. This is essentially a re-statement of the problem as dealt with by Keynes in the passage referenced above and reflects the interest dependence of similarity.

If we consider the problem after weights are given to things that 'belong to a different order of similarity' this problem can then be seen as the problem of the interest dependence of the weights.

Mott¹⁹² puts it clearly.

The interest-dependence of verisimilitude appears, in the literature, in a rather disguised form as '*language-dependence*'. To see how this occurs consider the theories $X = Cn\{P, Q, \sim R\}$ and $Y = Cn\{\sim P, \sim Q, \sim R\}$ where the truth is $P \& Q \& R$. Now it seems that we can tell at a glance X is better than Y. But note that in X we have $\sim(P \equiv R)$ and $\sim(Q \equiv R)$ while in Y are found $P \equiv R$ and $Q \equiv R$. In this regard Y appears better than X. From an interest point of view there need be no mystery here. Relative to the problems 'Whether $P \equiv R$ ', 'Whether $Q \equiv R$ ', we

¹⁹¹ Miller, D. 1978 'The Distance Between Constituents' *Synthese* 38, 197-212.

¹⁹² Mott, P.L. 1978 'Verisimilitude by Means of Short Theorems' *Synthese* 38, 247-273 p. 250.

see that Y is best. Relative to the problems 'Whether P', 'Whether Q' then X is the better. Perhaps we should leave it at that, and make verisimilitude relative to a deliberately chosen list of problems. ...

We have in no way produced an argument to the effect that Y's successes are unreal or to be deprecated in any way at all. We have noted that we feel that an answer to the question 'Whether P' is more important than an answer to the question say 'Whether $P \equiv Q \vee (R \& S)$ '. And this feeling is based, I suggest, not on any deliberately chosen list of problems but upon a more or less implicit and automatic choice: the sentence letters of a language. This choice is not to be *defended* – but it is to be noted.

This may seem to be a quibble, since the alternative query here seems improbable, but it is important in drawing our attention to the way in which the problem being investigated is a determinant of what is relevant.

Miller's language dependence argument is controversial because it appears to threaten a range of measures of similarity, and complex counter arguments, both epistemic and metaphysical, have been advanced by various writers¹⁹³.

Verisimilitude presents other problems in the context of scientific theories, one being that false propositions increase in verisimilitude when independent variables are added to the consideration of a system¹⁹⁴. (This will be discussed further and is illustrated in Table 3 and in Appendix B.)

Niiniluoto¹⁹⁵ maintains that despite these acknowledged difficulties, and, additionally, the fact that avoidance of error and maximizing truth (avoiding agnosticism) pull in opposite directions, we can nevertheless use truthlikeness to ground Scientific Realism. It is desirable to examine the claims of verisimilitude in more detail.

¹⁹³ Barnes, E. 1991 'Beyond Verisimilitude: A Linguistically Invariant Basis for Scientific Progress' *Synthese* 88, 309-339; Britton, T. 2004 'The Problem of Verisimilitude and Counting Partially Identical Properties' *Synthese* 141, 77-95.

¹⁹⁴ Psillos, S. 1999 *Scientific realism: how science tracks truth* New York: Routledge pp. 267-269.

¹⁹⁵ Niiniluoto, I. 1999 *Critical Scientific Realism*, Oxford: Oxford University Press Chapter 3.

Since Popper's proposal there has been something of a verisimilitude industry, with various new definitions having been proposed and it is claimed that these can rank competing false theories. Verisimilitude is often discussed in terms of simple model worlds which vary over a limited range of parameters, each commonly taken to having only two possible states.

I will take the simple world which can be hot or not and rainy or not. There are then various possible worlds as shown in the table, where we take the actual world to be hot and rainy.

Table 1

	hot (h)	rainy (r)
W1	T	T
W2	T	F
W3	F	T
W4	F	F

These four possible worlds, including the actual world, W_A , which we have arbitrarily said is W1, can be characterized by their distance from the actual world – zero in the case of W1 to a maximum in the case of W4. Intermediate worlds will be at different distances from the actual world and this distance will *inter alia* depend on the weighting of hot and rainy.

The analysis can be made quantitative by adopting the following natural seeming definition of verisimilitude¹⁹⁶. I will return to the naturalness of this shortly.

¹⁹⁶ I follow the account given by Psillos (Psillos, S. 1999 *Scientific realism: how science tracks truth* New York: Routledge Chapter 11).

The truthlikeness of a proposition is a function of the ratio of the sum of the distances between the actual world W_A and the world in which the proposition holds divided by the total number of worlds in which the proposition is true. We assign weights t_i to the basic states such that $\sum t_i = 1$, we then define the numerical distance between the actual world and a possible world as the sum of the weights of the basic states over which the two worlds disagree for those worlds in which the proposition holds. Then the distance of a proposition from the truth is given by this figure divided by the cardinality q of the set of propositions in which the proposition under consideration holds.

$$Dt(q) = \{\sum Dt (W/W_A)\}/q$$

Where W_A is the actual world; the verisimilitude is this distance subtracted from 1, i.e. $1 - Dt(q)$.

This distance will depend on the weighting of hot and rainy (how important they are in relation to the truth). Clearly this will be different if one is hoping to fill the farm dam or planning to hang out the washing. The distance will also depend on the way we define the measure of distance from the truth, which is an intuitive judgment¹⁹⁷.

We can consider the situation in more detail when, for example, we arbitrarily make the weightings equal and look at the combinations in the left hand column of Table 2.

¹⁹⁷ Brink, C. 1989 'Verisimilitude: Views and Reviews' *History and Philosophy of Logic* 10, 181-201). The measures are defended by arguments from intuitions in simple cases. As Brink points out, different intuitions have been used by various reputable scholars.

Table 2

	Holds in world	Cardinality	Dt(q)	Dt(q)/q	1-Dt(q)/q
		q			
h&r	W1	1	0	0	1
h	W1,W2	2	0+1/2	1/4	3/4
r	W1,W3	2	0+1/2	1/4	3/4
h&~r	W2	1	1/2	1/2	1/2
~h&r	W3	1	1/2	1/2	1/2
~h	W3W4	2	1/2+1	3/4	1/4
~r	W2W4	2	1/2+1	3/4	1/4
~h&~r	W4	1	1/2+1/2	1	0

It turns out that verisimilitude varies in interesting ways with the number of variables. If one moves from the simplest example given above, using two variables, to three variables (adding say windy) and then to four (adding say cloudy), we obtain the results shown in Table 3 for h, ~h and ~h&r¹⁹⁸.

Table 3

	2 variables	3 variables	4 variables
h	3/4	2/3	5/8
~h	1/4	1/3	3/8
~h&r	1/2	1/2	1/2

Table 3 indicates what is shown by analysis, namely that the verisimilitude of h tends to decrease towards one half and that of ~h to increase towards one half

¹⁹⁸ The 3 variable and 4 variable calculations are given in Appendix B.

as the number of variables increases¹⁹⁹. At the same time, the value of $\sim h$ remains constant at the value of one half. Given that it is avowedly hot, it is sometimes suggested that it is somewhat counter intuitive that the verisimilitude of its being hot should converge on that for being not hot, as the number of independent variables increases. However, the important point for our purposes is that in comparing the verisimilitude of different theories if irrelevant variables are considered (as they may be since we do not know the truth) the rankings may alter on an unacceptable basis.

Clearly, this result is an integral part of the proposed metric, and one question is whether it is physically reasonable that a model of a weather system should show these characteristics. In other words, if it is in fact hot and rainy, and further investigation shows that it is either windy or not, is it reasonable that the verisimilitude of it being hot or not become closer to one another. Some commentators argue that given the assumption that windiness is an independent variable, that this should not be the case. However intuitions differ and Niiniluoto has responded to this by pointing out that verisimilitude is context dependent²⁰⁰. If I ask only how many members of the current English cricket team have scored Test centuries and the answer given, correctly say, is five, then this has a certain verisimilitude. If in addition I ask how many are competent slips fielders and how many are over 1.8m in height, it is not unreasonable that the answer five to the first question now has lower verisimilitude, since in the new context it is less informative. Nevertheless, this dependence might still seem worrying in comparing truthlikeness of scientific theories because what may be a totally irrelevant variables (believed to be relevant in the different theories) will influence the ranking of theories vis-a-vis the supposed truth.

In the foregoing all the variables have been given equal weight. Weighting itself is also problematic, and this will be discussed in the context of real theories.

¹⁹⁹ In fact h is given by $(n+1)/2n$ and $\sim h$ by $(n-1)/2n$, where n is the number of variables.

²⁰⁰ Psillos cites a private communication (Psillos, S. 1999 *Scientific realism: how science tracks truth* New York: Routledge p. 269).

(This is not intended to imply that is not also problematic in the simple theories of the hot, rainy, windy type discussed in this section.)

Aronson *et al*²⁰¹ have attempted to overcome some of these problems by giving an account of verisimilitude via a theory involving the ordering of natural kinds into a hierarchy of types. Aronson states²⁰² 'it is the structure of natural kind hierarchies that provides us with similarity differentiation ...' Smith remarks²⁰³, 'it just seems a mistake to suppose as he [Aronson] apparently does, that the ordering of natural kinds into subtypes and supertypes could provide the basis for some general theory of approximate truth.' In fact Aronson and his colleagues do believe that a general theory of verisimilitude can be based on a type hierarchy of natural kinds²⁰⁴. (This has already been mentioned in 3.6 in the context of their belief that truth is to be defined as the limiting case of verisimilitude.) The type hierarchy notion is given some plausibility in their example of kingdom, phylum ... genus, species and sub-species in the natural world, where evolution has provided an historical ordering of descent²⁰⁵. However, it is odd to think there is some hierarchical ordering among, for example, the elements in the periodic table. Is Lead near to Tin because they are both in Group IV and have somewhat similar properties, or is Lead of mass number 210 near to Bismuth because in 22 years half of it will have become Bismuth, also of mass number 210. There is of course ordering among the elements, but it nicely illustrates the complexity and interest dependence of ordering. The Periodic Table shows two major types of ordering determined by the electronic structure. The structure of the nucleus allows yet another perspective. The point is that verisimilitude and approximate

²⁰¹ Aronson, J.L., Harre, R. and Way, E.C. 1994 *Realism Rescued* London: Duckworth Chapter 6; Aronson, J.L. 1997 'Truth Verisimilitude, and Natural Kinds' *Philosophical Papers* 26, 71-104.

²⁰² Aronson, J.L. 1997 *ibid* p. 80.

²⁰³ Smith, P. 1998 'Approximate Truth for Minimalists' *Philosophical Papers* 27, 119-128 p. 128.

²⁰⁴ Aronson, J.L., Harre, R. and Way, E.C. 1994 *op. cit.*

²⁰⁵ Even here the biologists face difficulties. For example, there are ring species where population A can mate with population B and so on, but finally population P cannot mate with population A. What population is most similar to another requires a decision as to what has primacy, genome studies or phenetic issues, and within either category further, somewhat arbitrary, decisions are required. Also, where there is sexual dimorphism, males and females of one population are regarded as more similar than two males of neighbouring populations whose genetic or phenotypic characters may be closer.

truth are context dependent as Aronson *et al* acknowledge. Even in their favoured example of biological kinds, there are formidable difficulties with a type hierarchy. Dogs belong to one biological species. The Australian dingo has probably adapted twice, first to become domesticated in prehistoric Asia, and then to become a wild species in Australia when it was introduced there about 3,500 years ago. Accordingly the dingo can be classified among dogs with the wolf as a wild animal or with the Pekinese as a domesticated animal, although it is otherwise unlike either animal. The type hierarchy here is legitimated by certain commonalities of DNA or ability to interbreed but seems not to bear on any ordinary notion of verisimilitude.

The ordering in the type hierarchy will depend on the purpose to which it is to be put, as is always the case, although this fact can be obscured by examples where there is an obvious ordering. On the biological classification, backed by DNA studies, we can say a dolphin is more like the other mammals than it is like a shark. But something must make this the contextually relevant type hierarchy, something not evident in all of the theories we might have about the ecology of the oceans. The notion of type hierarchy seems less useful in formulating a concept of verisimilitude than the possible worlds approach described earlier.

4.4 Verisimilitude and Ranking Scientific Theories

Even if one were to ignore the difficulties of the concept of verisimilitude arising from the arbitrariness of the chosen metric, there are formidable difficulties in assigning weighting.

For any actual scientific theories the variables generally will be continuous, not of the simple form hot or not hot. One could of course introduce a measure of say hotness giving it a value from 0 to 1. Complications result in that the variables in a scientific theory will commonly not be independent. (In reality this complication already exists but has been ignored, in that it is more likely in many countries to be rainy if it is not hot.) The interdependence of the variables may itself be a non

linear function of the values of the variables. Heavy rain may commonly drive the temperature down more than light rain²⁰⁶. A decision must also be made on how relatively important are departures from the data in each of the variables. This is the problem discussed earlier in considering whether one of two figures which is not square is more like a square – is it angles or edge length or symmetry, and so on.

An interesting historical example has been given in another context by Eco. In this instance something with maximum weight on one theory has zero weight on a competing theory.

The platypus²⁰⁷ was first observed by colonists in 1797 (it had been observed for some 40 000 years by the original Australians, but they were uninterested in its biological classification). There were two schools of thought. One school believed it was a mammal, namely it belonged to the category where the female suckles its young, and has mammary glands with nipples and does not lay eggs. No evidence for mammary glands was found in the early specimens, supporting those who believed that the platypus was oviparous (egg laying) and ‘therefore not a mammal’. In fact, as was discovered in 1824, the female platypus has mammae but no nipples, and the mammae are visible externally only when she is lactating. The infant suckles by licking the pore-like mammae, so the platypus is a mammal, and, as it later turned out, does lay eggs. Now prior to the discovery of the mammae the anatomical evidence suggested that the platypus was oviparous (the discovery of actual platypus eggs occurred much later). The point of interest for our purposes is the weight that would have been given, prior to 1824, by the two schools of thought, to the discovery of platypus eggs. For those who believed the platypus was a mammal despite the anatomical evidence of egg laying (because it seemed wrong to argue that it was a reptile, bird, or a

²⁰⁶ Brink’s account (*op. cit.*) discusses many of the numerous complications in deriving verisimilitude.

²⁰⁷ In giving the history, I have broadly followed the account given by Eco (Eco, U. 1997 *Kant and the Platypus* New York: Harcourt Brace and Company pp. 241-248).

fish), the discovery of eggs was unimportant. They could accept this and maintain their view that the platypus was a mammal. By contrast, those who believed the platypus was not a mammal, on the grounds of the anatomical evidence that it laid eggs, believed that the discovery of eggs would unequivocally confirm the anatomical evidence and prove that the platypus was not a mammal.

This platypus story illustrates a problem with the weighting of evidence. This problem will commonly manifest itself in the problem of the weighting of the possible states (like hot, rainy, and windy). Consider theories of the evolutionary origin of the platypus. In such theories factors like oviparous, possessing mammae, having aquatic habitat will be among a large set of variables. The weighting of these will be suggested on the basis of the theory. Consequently, comparisons of calculated verisimilitude may not be useful, since each is made in a different theoretical framework. In contrast, the various worlds in the simple picture of Table 1 are all being compared in a fixed framework.

Another problem in considering a real theory is that theories are approximate and their relative merits are the extent to which they combine being empirically adequate with being explanatory. Thus judging them bears little resemblance to the toy model of hot, rainy and windy, or not, which is commonly discussed²⁰⁸.

The questions one might ask and the variables involved in even a simple but real theory are vastly more complex and numerous, and additionally, as mentioned earlier, are often at least partially interdependent. Most importantly, the toy theories do not require consideration of significant background assumptions in contrast to the case of real theories.

²⁰⁸ These are not without interest, and have shown, for example, Popper's seemingly plausible general principle whereby 'a true theory is the nearer to the truth the stronger it is' is false. Consider the following theories: (a) 'red roses are red or grass is green', and (b) 'red roses are red or (grass is green and the moon is made of green cheese)'. It would be odd to claim that to go from (a) to (b) it is to make progress toward the truth. Yet this is what Popper's stance requires, for (a) and (b) are true, and (a) does not imply (b), and (b) implies (a).

It is useful in this context to consider a simple but more realistic theory. For this purpose, consider the theory of the mechanical properties of a simple model precipitation-hardened aluminium alloy with only one alloying element, copper. The postulates²⁰⁹ are as follows.

- (i) Plastic deformation takes place by the movement of dislocations (a postulated unobservable entity).
- (ii) The hardening (compared with pure aluminium) is due to the precipitate particles (introduced in a non-equilibrium distribution by the process of controlled heating, quenching, and subsequent annealing) that obstruct dislocation motion. (These particles are also unobservable in the usual Scientific Anti-realist sense.)

One can then predict that there will be an optimum particle-size distribution to maximize strengthening. If the particles are too large the dislocations can readily bypass them; if they are too small the dislocations can readily cut through them. The variables to produce the optimum particle size will be the temperatures T_1 (the temperature from which the specimen is quenched), T_2 (the temperature to which the specimen is quenched), the quenching speed, S , the annealing temperature T_3 ($T_2 < T_3 < T_1$) and the annealing time t . Providing T_1 is high enough and T_2 low enough and S high enough we can ignore these variables for a given copper content. One can experiment with T_3 and t to produce alloys of different strength.

This is a theory of precipitation hardening. The further role of theorizing is to enable quantitative predictions of the hardening under different experimental regimes. For this purpose, different theories will use different approximations to make the problem mathematically tractable. For example, one theory might assume the particles are all of the same size, and are randomly distributed;

²⁰⁹ There is a good deal of existing theory from other areas embedded in these postulates; for example, the theory of dislocations in metals, including the nature of the stress field around a dislocation.

another might assume an approximately Gaussian distribution of particle sizes and uniformly spaced particles. The force field between the particles and the dislocations would be approximated in various ways (perhaps on the assumption that the matrix is elastically isotropic, for example). These theories will make predictions of the strength of the alloy for various values of T_3 and t .

Now how are we to estimate the relative weighted verisimilitude of these theories? Are we to use the integral of the distance between the curves of the predicted and observed strength, versus pairs of values of T_3 and t ? Are we to use the prediction of the value(s) of T_3 and t that confers maximum strength? On the earlier argument, it is some weighted average of these and other possible measurements. How might we arrive at a weighting? The theories are avowedly false, in that they have deliberately made approximations. Hence, the idea of weighted verisimilitude relates in an immensely complex way to the theory and to its agreement with experiment.

Now this simple story in a simple case is typical. There are numerous loci for error and numerous facts known only approximately. This suggests that this weighted verisimilitude is not a legitimate basis for comparison unless the competing theories are closely alike in appropriate respects. It is not obvious that as a general rule there is a proper methodology for determining the superiority of one theory over another²¹⁰. Indeed, it is not self evident that, in general, of two false theories, the one with greater weighted verisimilitude is the better theory. After all, it is a given that we do not know the Truth, and hence we cannot know the verisimilitude, even if our weighting was in some sense correct. In other words, if our weighting can be seen with the wisdom of hindsight to have led over time to a choice of theories each of which was superior to its predecessors in all measurable respects, we could nevertheless be slowly spiraling towards the Truth rather than marching directly towards it. More seriously, in these same

²¹⁰ Zamora Bonilla has emphasized this methodological point (Bonilla, J.P.Z. 2000 'Truthlikeness, Rationality and the Scientific Method' *Synthese*, 122, 321-335). The theory with the greatest verisimilitude may indeed have been refuted.

circumstances, we could also be traveling away from the truth. As was discussed in Chapter 3 via the example of fluid dynamics, theories which give results that accord closely with experiment may be far from the truth²¹¹.

The foregoing simply emphasizes a point made earlier, namely that verisimilitude in general tells us at best only about empirical adequacy as determined in relation to the competing theories. The relative merits of those scientific theories which are claimed by Scientific Realists to accurately represent the structure of the world are commonly judged by other factors such as explanatory power. The measure of empirical adequacy used is itself dependent on the details of the competing theories and the conclusion to be drawn from an experiment showing improved empirical adequacy from theory T_1 to T_2 is not always straightforward. For example the Stern-Gerlach experiment (mentioned in section 1.6) was set up to distinguish classical theory and Sommerfeld's old quantum theory. The experiment did indeed, as the experimenters concluded, disconfirm the classical theory, but as later correctly interpreted it also discredited Sommerfeld's old quantum theory. In other words, the actual improved empirical adequacy (correctly interpreted) disproved the better (false) theory as well as the inferior (false) theory. A correct interpretation of the experiment would have said that the Stern-Gerlach experiment merely showed that both theories being tested were not empirically adequate (in different ways). Historical examples can also readily be found where better theories, showed less empirical adequacy, at least initially, than their predecessors.

At the heart of the problem in the approximate truth of real theories (as distinct from the toy hot rainy windy theories) is the obvious one I have already alluded to: you do not know what you do not know. As already mentioned (section 1.5), in

²¹¹ This is of course not a criticism of fluid dynamics as science – the continuum model is appropriate, and this is one of those cases where a useful model is known to represent one aspect of a physical system, and consciously chosen, while failing to represent another. The point here is simply the truism that empirical adequacy can, unlike this case, unknowingly rather than knowingly, coincide with a false picture.

the nineteenth century Lord Kelvin gave a proof that the Earth could not be more than 100 million years old, since on the most conservative estimates the sun would have used up all its possible energy in that length of time (the sun must be at least as old as the Earth)²¹². Of course, nothing was known about nuclear fusion. He erroneously believed in the approximate truth of the theories that led to his result. Lord Kelvin's calculation of the possible energy output from the sun could perhaps be compared with other calculations of the time, but there was no way they could usefully be compared with distance from the unknown truth, since the theories were incomplete in an essential way.

Barrett²¹³ writes:

Exactly how much closer to the truth is a theory that predicts neutrons decay with a half-life of about 12 minutes from one that predicts neutrons are stable? And is this second theory closer or further from the truth than a theory that correctly predicts neutron decay but falsely predicts that protons decay with a half-life of 1,000,000 years?²¹⁴

Thus it may be of great theoretical significance whether a particle is truly stable or has a half-life of 10^{10} years, so that judgments about what constitutes empirical adequacy may be intimately related to theory.

Verisimilitude has the merit of taking into account how much is informative in a theory. It does not now seem very useful to say that gases consist of atoms or molecules. A more informative theory which says gases consist of atoms or molecules that are randomly distributed and whose mean kinetic energy depends on the temperature is less likely to be true, while having greater verisimilitude.

²¹² Kelvin made an independent calculation using the time for the cooling of the Earth and the establishment of the present temperature gradient, and found reasonable agreement. Unfortunately, this was also incorrect, since nothing was known of radioactivity.

²¹³ Barrett, J.A. 2003 'Are Our Best Physical Theories Probably and/or Approximately True?' *Philosophy of Science* 70, 1206-1218 p. 1216 (see also Brink's remark cited earlier about the asymmetrical spider)

²¹⁴ At the time of writing, if the proton decays, experiments suggest that it must have has a half life greater than 10^{35} years; proton decay (into a positron and a neutral pion) has not been detected. The particle decay described here refers to an isolated particle, not one within an atom.

Nevertheless, verisimilitude is little less problematic than approximate truth, because the intractable weighting problem remains, as does the problem of defining the distance metric.

Verisimilitude does however ingeniously get around the problems I described for approximate truth with regard to existential statements and universal generalizations. In the case of quantified statements the difficulty lies in the problem of looking for instances of approximate truth. There is a difference between being an approximation and being approximately true. Small adjustments can sometimes turn something that is not a square into a square and vice versa. On the other hand, in the case of verisimilitude one is simply seeking a world where the statements of the theory are true, and then determining the distance of this world from the actual world.

Thus while it is not actually possible to determine the relative verisimilitude of competing theories, the Scientific Realist claims that successor theories do in fact have greater verisimilitude, and this explains their comparative success. She cannot establish this, because the truth is unknown according to the Scientific Realist. She must rely on an abductive inference which is not wholly negated but made doubtful by the difficulties around the metric and the around the theory dependent weighting problem posed by the open ended nature of scientific theories in contrast with the closed narratives describing idealized models.

4.5 Conclusion

It seems from the foregoing that there is not a convincing account of scientific progress in the sense demanded by the Scientific Realist. None of what has been said demands that Scientific Realism is false. Moreover, the majority of successive theories in a given area put forward by scientists may have greater values of verisimilitude, on some not unreasonable definition of the concept, than their predecessors. However, the considerations advanced in the last two

chapters suggest that the McMullin's Scientific Realist argument is not convincing because we need a more satisfactory notion of the approximate truth of scientific theories, or of their verisimilitude than is attainable.

It is clear that we cannot obtain something that might be called absolute verisimilitude. Imagine Scientific Realism is correct and imagine we know the truth in some domain. What is this supposed to mean? We can return to the example of the plastic deformation at low temperature of copper single crystals – a comparatively simple problem in plasticity. The dislocation density rises during deformation from about 10^4 to 10^{10} cm^{-3} and each dislocation interacts with all the other dislocations. The orientation of the Burgers vector of each dislocation in relation to the dislocation line is specifiable at each short segment along its length. There are about 10^{11} impurity atoms per cm^3 in the purest metals available and the position of each aggregate of each type of impurity is a 'knowable' fact. The actual distribution of the lattice defects at each stage of plastic deformation does determine the behaviour, but obviously a huge range of hypothesized dislocation and impurity distributions²¹⁵ would result in the same outcome within any possible level of experimental accuracy. (It should be noted that this 'huge range' is a minuscule subset of the possible arrangements.) It is not a defect of Scientific Realism that the project of finding an absolute (not interest based) comparison of the actual to the succeeding hypothesized situations in progressively better theories, is an utterly unattainable notion²¹⁶.

The whole truth is not merely unknown but generally it cannot be known. There will be many aspects of the truth which cannot be conceived of at any given time,

²¹⁵ The account could readily be complicated further by considering other lattice defects, but this is superfluous in making the present point.

²¹⁶ This point about the interest dependence of both verisimilitude and approximate truth has already been mentioned. A detailed account of the need to relate approximate truth to the specifics of a theory is given by Weston. He argues at the same time with regard to verisimilitude, that even when it is considered relative to a chosen target, it remains arbitrary and cannot be used to justify Scientific Realism (Weston, T. 1992 'Approximate Truth and Scientific Realism' *Philosophy of Science* 59, 53-74).

since new concepts will be required²¹⁷. The arguments of section 3.6.2 in relation to the difficulties in assessing approximate truth of open ended scientific theories, with their unknown future developments, apply equally to assessing verisimilitude. Indeed in both cases, if one were dealing with theories that were far from the Scientific Realist's truth, irrespective of possible future developments, there would be no justification for a relative ranking.

It is common sense to claim that the postulation of about ninety stable elements, explicable on the atomic theory, is an advance on the notion of the four elements earth, fire, air and water. Science does not claim to have the whole truth but rather to give an explanatory account in a particular domain. The Scientific Realist claim is that in general this account is true in the sense that the postulated entities, like atoms and electrons and bacteria, exist in the same sense as medium sized dry goods exist, and that most of the well established long standing theories describing such unobservable entities are approximately true. The inability to satisfactorily define approximate truth of theories or to measure verisimilitude does not necessarily undermine this claim, although it may be that the concept of an approximately true scientific theory is incoherent. (The inability to define satisfactorily simplicity, pornography, beauty or the colour blue does not mean that the concepts are without meaning.)

Finally, the issue to consider is whether the conjunction of P-fertility and long standing success gives the claims of Scientific Realism significant support, and a defensible account of a criterion of whether one theory is better than another. The context dependence of the concept of the verisimilitude of theories means that it suffers from the same disadvantage as the concept of approximate truth of theories. The conclusion drawn vis-a-vis approximate truth in Chapter 3, namely

²¹⁷ Aristotle could have asked but not answered why the sky is blue; he could not have asked whether all the isotopes of lead are stable, although of course radioactive substances existed billions of years before Becquerel. The fact that most of the scientific questions Aristotle could have asked are now answered, speaks to the limitations of the science of any particular time, including our own. Any question involves presuppositions which change, and commonly grow in number, over time.

that approximate truth cannot adequately sustain McMullin's defense of Scientific Realism, applies equally to verisimilitude.

Appendix B

Table B1

	hot (h)	rainy (r)	windy (w)
W1	T	T	T
W2	T	T	F
W3	T	F	T
W4	T	F	F
W5	F	T	T
W6	F	T	F
W7	F	F	T
W8	F	F	F

Table B2

	Holds in world	Cardinality q	Dt(q)	Dt(q)/q	1-Dt(q)/q
h&r&w	W1	1	0	0	1
h&r	W1,W2	2	0+1/3	1/6	5/6
h&~r&w	W3	1	1/3	1/3	2/3
h	W1,W2,W3,W4	4	0+1/3+1/3+2/3	1/3	2/3
~h&r	W5,W6	2	1/3+2/3	1/2	1/2
~h	W5,W6,W7,W8	4	1/3+2/3+2/3+3/3	2/3	1/3
~h&r&~w	W5	1	2/3	2/3	1/3
~h&~r	W7,W8	2	2/3+3/3	5/6	1/6
~h&~r&~w	W8	1	3/3	1	0

Table B3

	hot (h)	rainy (r)	windy (w)	cloudy (c)
W1	T	T	T	T
W2	T	T	T	F
W3	T	T	F	T
W4	T	T	F	F
W5	T	F	T	T
W6	T	F	T	F
W7	T	F	F	T
W8	T	F	F	F
W9	F	T	T	T
W10	F	T	T	F
W11	F	T	F	T
W12	F	T	F	F
W13	F	F	T	T
W14	F	F	T	F
W15	F	F	F	T
W16	F	F	F	F

Table B4

	Holds in world	Cardinality q	Dt(q)	Dt(q)/q	1-Dt(q)/q
h	W1 to W8	8	$0+1/4+1/4+1/2+1/4+1/2+1/2+3/4$	3/8	5/8
~h	W9 to W16,	8	$1/4+1/2+1/2+3/4+1/2+3/4+3/4+1$	5/8	3/8
~h& r	W9 to W12	4	$1/4+1/2+1/2+3/4$	1/2	1/2

CHAPTER 5 APPROXIMATE TRUTH, REALISM AND ANTI-REALISM

5.1 Introduction

A problem, which has thus far been implicit, is to what extent the concept of the approximate truth of theories is essential to Scientific Realism and to Anti-realism. As was discussed earlier, McMullin himself eschews approximate truth, but despite this stance, the concept seems to be an indispensable part of his argument for Scientific Realism. His explanation of the conjunction of P-fertility and long standing success is that there is a progressively closer approach to the 'structure of the real'. What has not been discussed so far is the more general question as to whether approximate truth (or verisimilitude) is necessary for every form of Scientific Realism. Thus the argument so far has been as follows: Given the acceptance of the empirical observation of the McMullin Conjunction then:

- (i) McMullin has an argument for Scientific Realism that allegedly does not rely on the concept of the approximate truth of theories;
- (ii) In fact it does do so;
- (iii) The concept of the approximate truth of scientific theories is not defensible;
- (iv) McMullin's argument for Scientific Realism fails.

If (iii) is true this raises the question as to whether all arguments for Scientific Realism necessarily invoke the approximate truth of scientific theories and hence all are suspect. This in turn raises the question as to whether Anti-realism requires the concept of the approximate truth of scientific theories, since if the concept is inadmissible, as I have argued, the answers to these two questions may give grounds for discriminating between these contending positions in the philosophy of science.

While I have just urged that McMullin's argument for Scientific Realism relies on some concept of approximate truth, it is not obvious that this is more generally true of arguments for Scientific Realism. As discussed in section 1.5, the strongest case for taking scientific theories to be true (and resisting the Pessimistic Meta Induction) lies in mature contemporary science. This is best exemplified in contemporary physics as the basic science. Therefore in section 5.2 I discuss an implication of the ongoing conflict between the two paradigmatic theories of twenty-first century physics – quantum mechanics and general relativity; namely the general necessity for approximate truth within Scientific Realism. In section 5.3 I suggest that this conflict does not have the same consequence for the Anti-realist.

5.2 Approximate Truth and Realism

It is possible to be a Scientific Realist without an acknowledged commitment to the concept of approximate truth of theories or that of verisimilitude. This is the usual commonsense position, where there may be an unexamined acceptance in this case of something like the approximate truth of theories. However, as soon as one seeks a philosophical basis for Scientific Realism the question of approximate truth arises. In Chapter 1 I cited an account of the commitments of Scientific Realism and the epistemic requirement was there given as:

- (v) Truths about S are knowable and we do in fact know some of them, and hence the terms of S successfully refer to things in the world.

In other words, the Scientific Realist believes that science tells us something true about the world.

As has been discussed earlier, the most commonly cited basis for this epistemic tenet is the well known No Miracles Argument. The success of science is here explained on the basis of the approximate truth of those of its currently accepted mature theories which attempt to describe the mind-independent world. (I am excluding here avowedly instrumentalist theories like fluid mechanics where real

fluids are treated as a continuum.) It is agreed by Scientific Realists that current scientific theories are generally not true.

This scepticism about the truth of current theories is commonly based on the historical evidence that even the best theories in the most advanced scientific disciplines have always been repeatedly subject to significant modification²¹⁸. These modifications have sometimes arisen from entirely new experimental observations, sometimes from increased accuracy in experimental observations, and sometimes from new theoretical insights. The latter have sometimes arisen in the theory itself, but also have often come from changes in cognate theories which have implications for the theory under consideration. The continuing change is not surprising given the scale of the contemporary scientific enterprise, and, equally importantly, given the autocatalytic nature of the enterprise. A development like the invention of the laser has led to a vast proliferation of sensitive new experimental techniques in optical spectroscopy for example. Another spectacular example has been the application of the extraordinary increase in computing power²¹⁹ to the enhancement of a huge range of experimental techniques, ranging from the radio astronomy studies of distant galaxies to the electron microscopy of viruses. Whatever the origin of the ongoing modification of even the best theories, it is clear that the force of the NMA lies in the acceptance of the notion that the successive versions of the theory are approximately true or have increasing verisimilitude.

There is however another common sense stance that does not rely on the No Miracles Argument for the truth or approximate truth of scientific theories. The starting point is Realism about everyday objects rather than Scientific Realism.

²¹⁸ This is in essence a much attenuated version of the Pessimistic Meta Induction (PMI), discussed in Chapters 2 and 3. Instead of claiming that current theories are false because of the failures of theories in the past, there is the more limited acknowledgement that even the best current theories are highly likely to be imperfect because of the observation that they normally need to be modified in the light of new evidence.

²¹⁹ The so called 'Moore's Law', the doubling of the number of transistors possible on a chip every 18 months has now persisted for over 50 years. This is a remarkable example of genuine exponential growth that still persists without any sign of the usual saturation phenomenon.

Observable entities, like stones and mountains, and the moons of Jupiter, obviously exist, and this is the basic notion. Sankey²²⁰ proposes: 'There is no need for the scientific realist to argue for the reality of ordinary, everyday material objects, since commitment to such entities has already been established at the level of common sense.'

Thus far there is no requirement for an acceptance of approximate truth because the commonsense observations are taken to be true *simpliciter*. The question now is whether one can move from Realism to Scientific Realism without any further assumptions. On this Realist picture, what allows us to see at sunrise is light, and science maintains that the origins of our seeing are electromagnetic waves within a certain wavelength range. The Scientific Realist accepts this view of the scientists and believes in the existence of photons. These are particles without mass that she cannot see. She accepts, because she is a Scientific Realist, that she can detect them with suitable apparatus (one at a time if the light source is dim enough). Further, being a Scientific Realist she makes no distinction between her ability to see with the naked eye, her spectacles, a telescope (for the moons of Jupiter) and a photon detector. In accepting the photons she accepts an extremely abstruse theory – Quantum Electrodynamics (QED). Indeed as Feynman explains²²¹, for her to accept the account she needs to 'lose her common sense', which has been relied upon in the Realist story above. So, if she is both to believe she is seeing stones or mountains or the moons of Jupiter, and believe in how she is seeing them, and what they consist of, common sense is of no use to her. As Russell put it²²² some 70 years ago,

²²⁰ Sankey, H. 2004 'Scientific Realism: An Elaboration and a Defense' *Knowledge and the World: Challenges beyond the Science Wars* M. Carrier, J. Roggenhofer, G. Küppers and P. Blanchard (Eds) Berlin: Springer Verlag p. 65. In resisting my criticism which follows, Sankey argues that the corrections to common sense provided by science are to the explanations offered for the appearances, and that the common sense account of the appearances themselves remains valid.

²²¹ Feynman, R.P. 1985 *QED: The Strange Theory of Light and Matter* Princeton: Princeton University Press p. 5. The need to abandon common sense is thoroughly documented in Feynman's account.

²²² Russell, B. 1940 *An Inquiry into Meaning and Truth* London: George Allen and Unwin p. 15.

'Naive realism leads to physics and physics, if true, shows that naive realism is false. Therefore naive realism if true is false; therefore it is false.'²²³

Quantum Electrodynamics is an extremely well established part of physics and it may well be true, but the reason scientists and laypersons have for accepting its explanations and predictions is that it is believed to be either true or approximately true. In any case, even if the naive Realist insisted that Quantum Electrodynamics was true, many other of the explanations delivered by science that she accepts are unlikely to be true. They are likely to be approximately true (as Scientific Realism understands this concept).

Nevertheless such a Realist might say, 'I believe what the scientists are currently saying is true; if it is false, as I acknowledge it may be, when I learn this I will cease to believe it'. Theories are either true or false, she might say, and neither come in degrees. Indeed, she might abandon the Realist's 'compelling doctrine' 'about the ordinary observable physical world' and as a Scientific Realist simply accept current best science as the truth²²⁴. Where reputable science is uncertain or is in disagreement she will suspend judgment.

Thus this, what we might call Common Sense Realism, rejects the Pessimistic Meta Induction and appears not to require the concept of the approximate truth of theories.

²²³ As Heisenberg said in responding to critics of the Copenhagen interpretation, They [some physicists] would prefer to come back to the idea of an objective real world whose smallest parts exist objectively in the same sense as stones or trees exist, independently of whether or not we observe them.

This however, is impossible or at least not entirely possible because of the nature of the atomic phenomena ...

(Heisenberg, W, 1958 *Physics and Philosophy* New York: Harper and Brothers p. 129) Quantum mechanics does not provide a secure argument against Scientific Realism but it does so against naive realism.

²²⁴ This is not necessary; she can perfectly well continue to accept the everyday objects on the common sense grounds cited earlier.

However, there is a problem even if the Pessimistic Meta Induction is rejected. There remains the problem of accepted mature successful theories that are in conflict.

Consider the highly successful theory, quantum mechanics, and the current view within theoretical physics that it is incompatible with another highly successful theory, namely general relativity. Indeed highly successful is something of an understatement. Experiments on the gravitational red shift and time delay have confirmed general relativity, and the whole of modern chemistry and much of current physics constitute a confirmation of quantum mechanics. To quote Penrose²²⁵:

It has been said that quantum field theory is the most accurate physical theory ever, being accurate to about one part in about 10^{11} . However, I would like to point out that general relativity has, in a certain clear sense, now been tested to be correct to one part in 10^{14} (and this accuracy has apparently been limited merely by the accuracy of clocks on earth).

This incompatibility between the two main planks of contemporary physics has been a puzzle since the formulation of quantum mechanics. There are claims that a workable quantum theory of gravity for low energies has been proposed but the claim that the theory is adequate is strongly contested. There is general agreement that the two powerfully explanatory and extremely accurate quantitative theories are incompatible at high energies²²⁶.

The Scientific Realist is unable to accept that both of these theories are true, and her explanation of their success is that both must be approximately true²²⁷ (There is the usual fallibilist proviso that one or both may be false and not approximately true – a problem for the Scientific Realist given two such well attested and pivotal theories for which no contrary experimental evidence has been found in nearly a

²²⁵ Hawking, S. and Penrose, R. 2000 *The Nature of Space and Time* Princeton: Princeton University Press p. 61.

²²⁶ For a recent account see for example http://en.wikipedia.org/wiki/General_relativity.

²²⁷ One might be true, a possibility included here within being approximately true.

century²²⁸.) She is compelled to take this stance because the Scientific Realist believes that the mind-independent world, which is being represented either accurately or approximately by science, cannot allow two true incompatible theories. Smolin, the distinguished theoretical physicist and committed Scientific Realist, writes as follows²²⁹.

There cannot be two different theories of principle, applying to different domains. Because the world is a unity, everything interacts ultimately with everything else, and there can only be one language used to describe these interactions. Quantum theory and general relativity are both theories of principle. As such, logic requires their unification.

It might be argued that the Scientific Realist can use my notion (in the context of explaining the McMullin Conjunction in Section 2.4) of the true parts of each of these false theories (and not approximately true theories) as explaining the success of each of the highly successful incompatible theories. However, these theories are successful in such detail and with such accuracy that the Scientific Realist undermines her primary case for Scientific Realism by merely claiming partial truth (and not approximate truth) for what are the two most successful theories in the history of physics. Given this, the incompatibility of the two theories makes the simultaneous approximate truth of both theories an untenable stance from a Scientific Realist perspective.

²²⁸ Einstein says of the general theory, 'The chief attraction of the theory lies in its completeness. If a single one of the conclusions drawn from it proves wrong it must be given up; to modify it without destroying the whole structure seems to be impossible.' (Einstein, A. 1956 *Out of My Later Years* New Jersey: Citadel Press p. 58a); Sklar says of relativistic quantum field theory (which is consistent with special relativity), in what I believe would be a typical view, 'our best available foundational scientific theory' (Sklar, L. 2003 'Dappled Theories in a Uniform World' *Philosophy of Science* 70, 424-441 p. 425). Note that being consistent with special relativity is a very minor problem by comparison with being compatible with general relativity – in special relativity we are not dealing with forces at all.

²²⁹ Smolin, L. 2007 *The Trouble with Physics* London: Penguin Books p. 10. Cartwright would deny this, not because of the claim that 'the world is a unity', which can be variously interpreted, but because there are no laws of universal applicability so that the demand for unification is unmotivated.

5.3 Approximate Truth and Anti-realism

Given the situation just described, where the two theories cannot be combined since this leads to a contradiction, an Anti-realist could believe that both theories are true because each is defining a different conceptual scheme. For example, she may believe that both of the incompatible theories are, or are what would be, the result of an Ideal Epistemic Inquiry, and all the evidence supports them both. It is important to recognize that Ideal Epistemic Inquiry does not imply²³⁰ ‘... conditions which are *simultaneously ideal* for the ascertaining of any truth whatsoever, or simultaneously ideal for answering any question whatsoever.’

An ideal epistemic situation need mean no more than a situation where I might declare ‘this is a chair’ in circumstances of good lighting and where my eyesight was good and I was not confused by drugs and so on²³¹. As will be discussed further, there is no reason for the Internal Realist²³² or Cartwright’s Entity Realist to demand that there is, or that we can find, a single conceptual scheme that will encompass all theories. (It is almost universal among theoretical physicists to assume that such a scheme can be found, and for more than 80 years they have been actively seeking a theory which will consistently combine quantum theory and general relativity²³³.) Van Fraassen’s Constructive Empiricist would accept

²³⁰ Putnam, H. 1990 *Realism with a Human Face* Cambridge MA: Harvard University Press p. viii. This is of course different from Peirce’s hypothetical ultimate limit of inquiry.

²³¹ Putnam, H. 1990 *ibid*; Putnam at various times in talking about truth uses the terms idealized epistemic justification, idealized rational acceptability, or idealized warranted assertability, always viewing truth as ‘not radically non-epistemic’, but never allowing for perfection or omniscience in any verification procedure. More recently Putnam has retreated somewhat and currently resists explicit definitions of truth; he still rejects both the deflationary and correspondence notions.

²³² An account of the Internal Realist position and the notion within it of distinct conceptual schemes is taken up in Chapter 6.

²³³ For some thirty years theorists have intensively explored String Theory as a possible means to this end. For theoretical physicists this assumption of a single universal conceptual scheme need be no more than a methodological maxim, warranted by the history of physics, rather than a belief; a stance advocated by Giere (Giere, R.N. 2006 ‘Perspectival Pluralism’ in *Scientific Pluralism* S. H. Kellert, H. E. Longino, and C. K. Waters (Eds) Minnesota Studies in the Philosophy of Science Volume 19 Minneapolis: University of Minnesota Press). This form of Scientific Pluralism, wherein the assumption of a single universal conceptual scheme is treated merely as a methodological maxim, while maintaining agnosticism about a more profound Scientific Pluralism is, Giere maintains, less obviously plausible in the biological sciences. Conceptual relativity is discussed further in Chapter 6.

that there is no need to claim any more than that each of the theories is empirically adequate in its domain, although he would be troubled by their inferential isolation. An Anti-realist is on good grounds if she can assert that both theories are true, since they underpin an enormous part of modern science. It is hard to find any commonplace fact that is more secure than the claim that it is curved space-time that accounts for gravity or any postulates more secure than those of quantum theory.

It might be argued that it is contrary to the notion of truth that two incompatible theories are both declared to be true. Since coherence is evidently an epistemic virtue, the conflict certainly does mean for the Scientific Realist that physics overall does not constitute an epistemically ideal theory. However, the Internal Realist does not claim human omniscience; and it may be that any single conceptual scheme fails and will continue to fail to construct a seemingly consistent universe. For the Internal Realist quantum mechanics and general relativity constitute two distinct conceptual schemes. As Putnam writes²³⁴: ‘... two statements which are incompatible at face value can sometimes both be true (and the incompatibility cannot be explained away by saying that the statements have ‘a different meaning’ in the schemes to which they respectively belong).’

There is no reason to suppose that as a result of an Ideal Epistemic Inquiry we can know all the truths. Putnam’s position is that there is no fact of the matter as to which of the two incompatible positions arising from different conceptual schemes is correct. Putnam does not believe that there is any reason to suppose that science will converge to a single definite picture with a unique ontology. He asserts, ‘to say... that convergence to one big picture is required by the very concept of knowledge is sheer dogmatism.’²³⁵

²³⁴ Putnam, H. 1990 *ibid* p. x. He defends this statement, made in the introduction, in Chapter 6 of the same book.

²³⁵ Putnam, H. 1990 *ibid* p. 171.

Putnam commonly illustrates what he means by saying that 'two statements which are incompatible can sometimes both be true' by using the contrast between the consideration of three geometric points allowing mereological sums as objects, and using the ordinary Carnapian scheme²³⁶. In the former case it is true to say that there are seven objects and in the latter case it is true to say that there are three objects. Each statement is true within a conceptual scheme²³⁷ and each describes the same state of affairs. So for Putnam, truth is relative to a conceptual scheme and nothing in nature privileges a particular scheme in the case of scientific theories. He says²³⁸: 'it is an illusion that there could be just one sort of language game which could be sufficient for the description of all reality'. He is here concerned with rebutting the notion that a conceptual scheme appropriate for science can tell the whole story of human knowledge. There is nothing to compel the Internal Realist to believe that even the whole of science can be incorporated within a single conceptual scheme²³⁹. Such a view does follow from Scientific Realism but not from Internal Realism.

Scientific Realists complain that an epistemic notion of truth leaves us without an explanation of the success of science. In their view it is the correspondence of the truths with an independent reality which provides the explanation of the general success of science. Putnam regards this position as incoherent²⁴⁰. He

²³⁶ Putnam, H. 1990 *ibid* p. 97. (This is discussed further briefly in section 6.2.3 footnote 300.)

²³⁷ This claim is not universally accepted. Arguments have been put forward to say that it is false to say that if x and y are distinct material objects, neither of which is a proper part of the other, that there exists a third material object z, not identical to either x or y, whose matter is exhausted by that of x and y (see Gross, S. 2004 'Putnam, Context, and Ontology' *Canadian Journal of Philosophy* 34, 507-554). Scientific Realists like Boghossian commonly have a different objection. Boghossian says that Putnam is mistaken in drawing his conclusion from the options of saying truly there are three or seven objects. All that can be concluded (which obviously every Realist would concede) is that different descriptions of one set of circumstances are possible. Putnam's point however is that no specifiable description is determined other than via a conceptual scheme. This, Putnam claims, does not lead to the relativism that disturbs Scientific Realists (Boghossian, P. 2006 *Fear of Knowledge* Oxford: Clarendon Press) because, while more than one true description is possible, false descriptions are not (with the usual fallibilist caveat). I return to this issue in section 6.2.

²³⁸ Putnam, H. 2004 *Ethics without Ontology* Cambridge MA: Harvard University Press p. 22.

²³⁹ Putnam's picture of truth being embedded within a conceptual scheme is taken up in section 6.2.2.

²⁴⁰ More specifically, Putnam gives an account of the only version of the Scientific Realist's views that he treats as though it is plausible and which he calls Metaphysical Realism. This postulates a

takes each of the theories of physics to be explanatory insofar as it meets the usual scientific criteria like empirical adequacy, unifying power and predictive power. Despite being an early champion of the NMA he would certainly not now explain the 'success of science' on the basis of correspondence with a mind-independent reality.

In any case, as Blackburn points out, Internal Realism can use the argument for the success of science here being used by the Scientific Realist but without her mind-independence clause. Blackburn puts the Realist argument as follows²⁴¹: 'It is because opinion is caused, perhaps indirectly, by the fact that p, that it converges upon p.' This argument is available to the Internal Realist since it is the way the world is according to the chosen conceptual scheme that determines that we accept the conceptual scheme. The experimental and theoretical

correspondence theory of truth and a world consisting of a unique fixed totality of mind-independent objects. He argues that this is incoherent. Numerous philosophers regard Putnam's account as an inadequate or improper description of the Scientific Realist position; see for example Khlentzos, D. 2004 *Naturalistic Realism and the Antirealist Challenge* Cambridge MA: MIT Press; Sankey, H. 2004 'Scientific Realism: An Elaboration and a Defense' *Knowledge and the World: Challenges beyond the Science Wars* M. Carrier, J. Roggenhofer, G. Küppers and P. Blanchard (Eds) Berlin: Springer Verlag; Sankey, H. 2004 'Scientific Realism and the God's Eye Point of View' *Epistemologia* 27, 211-226; Haack, S. 1998 *Manifesto of a Passionate Moderate* Chicago: University of Chicago Press; Grayling, A.C. 1997 *An Introduction to Philosophical Logic* Oxford: Blackwell. A common objection to Putnam's characterization is to the 'fixed totality' clause. For example, Khlentzos (*op. cit.* p. 195) says:

Why couldn't a metaphysical realist allow the possibility that the world is simply too rich in structure and complexity for any one language or theory to countenance? Perhaps the world is infinitely complex, even at the microphysical level? If so it may just be our lot to aspire in our theories to a number of true partial descriptions of 'the way the world is' with no genuine prospect of ever integrating them into a single 'final theory'.

Other critics object to Putnam's alleged assertion of the incompatibility of mind-independence and different descriptive ontologies, 'we must see that there is no difficulty in admitting a mind-independent world while at the same time admitting that our *cognitive* powers extend only to a "textualized" world' (Margolis, J. 1986 *Pragmatism without Foundations* Oxford: Blackwell p. 283). Putnam's position rests on his contention that any plausible form of Scientific Realism involves the necessary conjunction of all the claims he ascribes to the Metaphysical Realist (Clark, P. and Hale, R (Eds) 1994 *Reading Putnam* Oxford: Blackwell p. 253; Putnam, H. 1983 *Philosophical Papers* Volume 3 Realism and Reason Cambridge: Cambridge University Press). One or more of Putnam's claims that the Metaphysical Realist is committed to correspondence, uniqueness, and bivalence are contested by all of the authors cited above. Khlentzos believes that Putnam's argument (which Khlentzos does not accept) does not require the Scientific Realist to be committed to a correspondence theory of truth but only to evidence-transcendent truth (Khlentzos, D. 2004 *Naturalistic Realism and the Antirealist Challenge* Cambridge MA: MIT Press p. 33).

²⁴¹ Blackburn, S. 1993 *Essays in Quasi-Realism* Oxford: Oxford University Press p.30.

constraints upon any 21st century theory of the electron, for example, are such that the conceptual scheme (Quantum Electrodynamics) physicists place such confidence in is currently forced upon them.

The Anti-realist can dispense with approximate truth, unlike the Scientific Realist; she does not need the concept in the context of defending two inferentially isolated well established scientific theories. That is, two well established theories which if combined would lead to a contradiction.

Of course the foregoing does not entail that the Internal Realist makes the unscientific claim that say quantum mechanics and general relativity theories are exactly true in the sense of completely and precisely accounting for all the phenomena that may ultimately come to light. The claim is only that everything said in the theories may be true, that approximate truth is not a concept that is required and that 'truth does not come in degrees' with regard to scientific theories. Thus it seems reasonable to say, for example, that the theory that gases consist of molecules which repel at short range is true *simpliciter*. This is not to claim that this is the whole truth about the behaviour of gases or indeed that we will ever know the whole truth in this or any other area of science.

Thus all that is needed by the Internal Realist is the notion that theories contain significant truths, and theories are to be judged on some or all of the usual criteria of comprehensiveness, simplicity, explanatory force and empirical adequacy. The concept of approximate truth need not be invoked. However, it might be supposed that there exist examples of theories that contain no truths and where the McMullin Conjunction is found and the theories give an account that plausibly describes the Scientific Realist's mind-independent world. Such a hypothetical long standing successful P-fertile theory would be best accounted for by a Scientific Realist invoking approximate truth. The existence of a theory of this sort which unequivocally contains no significant truths is implausible. However, it is an unexplored empirical historical question whether there has been

such a theory. Of course, even if no historical examples exist, it cannot be completely ruled out that such a counter example to the case against postulating the approximate truth of scientific theories will be found in the future. But even so, the most that would be generated is a realism restricted to such a theory – no general case for realism is in the offing.

5.4 Conclusion

In this chapter I have suggested that a central problem of Scientific Realism is that the use of the concept of the approximate truth of scientific theories is inescapable. This is so quite apart from any account of P-fertility. Anti-realism does not essentially require the concept of the approximate truth of theories, can accommodate inferentially isolated well established scientific theories, and can otherwise rival the account given by Scientific Realism.

Putnam's Internal Realism is considered in the following chapter to investigate whether it can give a better account of the scientific enterprise than that offered by McMullin. In particular, the issue is whether it can give an account of co-existing inferentially isolated well established theories without invoking their approximate truth, while accounting for the P-fertility of most long standing successful theories.

CHAPTER 6 PUTNAM'S INTERNAL REALISM

I believe that the first step in the setting up of a 'real external world' is the formation of the concept of bodily objects... Out of the multitude of our sense experiences we take, mentally and arbitrarily, certain repeatedly recurring complexes of sense impression...and we attribute to them a meaning – the meaning of bodily object. Considered logically this concept is not identical with the totality of sense impressions referred to; but is an arbitrary creation of the human (or animal) mind. On the other hand, the concept owes its meaning and its justification exclusively to the totality of the sense impressions we associate with it.

The second step is to be found in the fact that, in our thinking (which determines our expectation) we attribute to this concept of bodily object a significance, which is to a high degree independent of the sense impression which originally gave rise to it. This is what we mean when we attribute to the bodily object 'a real existence'. The justification of such a setting rests exclusively on the fact that, by means of such concepts and mental relations between them, we are able to orient ourselves in the labyrinth of sense impressions.

Albert Einstein²⁴²

6.1 Introduction

The argument in Chapter 5 relies heavily on Putnam's statement already quoted ... two statements which are incompatible at face value can sometimes both be true (and the incompatibility cannot be explained away by saying that the statements have 'a different meaning' in the schemes to which they respectively belong).²⁴³

²⁴² Einstein, A. 1956 *Out of My Later Years* New Jersey: Citadel Press p. 60.

²⁴³ Putnam 1990 *op. cit.* p. x.

This is a consequence of Putnam's Internal Realism²⁴⁴ whose central element is as follows.

The suggestion which constitutes the essence of "internal realism" is that truth does not transcend use. Different statements – in some cases, even statements that are "incompatible" from the standpoint of classical logic and classical semantics – can be true in the same situation because the words – in some cases, the logical words themselves – are used differently. But this is not to say that talking of "use" instead of "meaning" is going to provide another sort of reductive account of or substitute for "intentionality".

Describing the use of words involves describing many things – when sentences containing those words are acceptable, what typically causes an expert/ordinary speaker to use those words in particular ways, what interests the ways of speaking in question subserve, what the phenomenology of the particular way of speaking is, and so on. These things are no more reducible to physical-cum-computational language than is meaning talk or reference talk²⁴⁵.

Internal Realism's conceptual relativism²⁴⁶ is trying to steer a course between Scientific Realism and various other forms of Anti-realism, without falling prey to

²⁴⁴ Putnam's exposition of Internal Realism is to be found in various sources. Some major references are Putnam, H. 1983 *Philosophical Papers* Volume 3 Realism and Reason Cambridge: Cambridge University Press (the Introduction and chapters 11 and 12) and the last chapter of Putnam, H. 1988 *Representation and Reality* Cambridge MA: MIT Press. A more recent defense is in his responses to the articles in *Reading Putnam* 1994 P. Clark and R. Hale (Eds) Oxford: Blackwell. It seems from Putnam's writings that he regards the labels Realist and Anti-realist as too imprecise to characterize his Internal Realism as falling into one or other of these categories. He sometimes refers to his views as Scientific Realism, and says, 'scientific realism in this sense does not presuppose either metaphysical realism or metaphysical antirealism, although it is incompatible with all forms of operationalism and phenomenalism' (Putnam, H. 2005 'A Philosopher Looks at Quantum Mechanics (Again)' *British Journal for the Philosophy of Science* 56, 615-634 p. 617). However, it seems proper to characterize Internal Realism as Anti-realist, *vide* his aphorism 'the mind and the world jointly make up the mind and the world' (Putnam, H. 1981 *Reason, Truth and History* Cambridge: Cambridge University Press p. xi). He gives a later explanation of the metaphor, which he still likes, where he says, 'The rich and ever-growing collection of truths about the world is the joint product of the world and language users.' (Clark, P. and Hale, R (Eds) 1994 *Reading Putnam* Oxford: Blackwell p. 265); this last statement is somewhat ambiguous and a literal reading makes it wholly acceptable to a Scientific Realist.

²⁴⁵ Putnam, H. 1988 *Representation and Reality* Cambridge MA: MIT Press p. 115.

²⁴⁶ I have introduced conceptual relativism where inferential isolation is demanded because two satisfactory theories if combined would lead to a contradiction. Putnam's picture may be wider

relativism or scepticism. Putnam writes that he wants to put an alternative to, '... metaphysical realist views ... on the one hand, and to cultural relativist ones, on the other'²⁴⁷.

Section 6.2 gives an account of Internal Realism and Putnam's defense of the 'incompatible statements' quotation above against various objections. Section 6.3 examines Internal Realism's credentials for dealing with the McMullin Conjunction, which has been accepted throughout, and hence must be accounted for by an acceptable theory. In section 6.4 I briefly consider the import of the many historical examples of successful false theories for Scientific Realism and for Internal Realism.

6.2 Internal Realism

6.2.1 Putnam's Thesis

Internal Realism is characterized by three postulates, as follows²⁴⁸.

- IR1 What objects does the world consist of is a question that it only makes sense to ask within a theory or a description.
- IR2 There is more than one 'true' description of the world.

and include cases where the two schemes simply use different modes of explanation. It seems that he might, for example, regard the scheme involving a neural account of mental events and the account given by folk psychology as constituting different conceptual schemes, although no contradiction may result from combining them. It is of course also possible that one of these accounts is sufficiently underdeveloped so that inevitable contradictions have not become apparent, or indeed that both are.

²⁴⁷ Putnam, H. 1987 *The Many Faces of Realism* La Salle IL: Open Court p. 1.

²⁴⁸ Putnam, H. 1987 *ibid*; the essentials of Putnam's Internal Realism in what follows are not negated by his shift to what he calls natural realism about perception, in opposition to representational theories (Putnam, H. 1994 'Sense, Nonsense, and the Senses: An Inquiry into the Powers of the Human Mind' *Journal of Philosophy* 91, 445-517). As Wright comments, there is encouragement for the view that, whatever his own impression of the matter, many features of Putnam's recent reviews represent not a rejection of the middle-period internalism, but a further working out of the implications and possibilities of the (somewhat relaxed) form of evidential constraint on truth which that outlook is most usefully seen as involving.

(Wright, C. 2003 *Saving the Differences* Cambridge MA: Harvard University Press p. 208) For a detailed discussion of the continued viability of what Wright calls moderate internal realism see Wright, C. *ibid* Chapter 11. Putnam himself remarks, 'So whether I am still, to some extent, an 'internal realist' is, I guess, as unclear as how much I was including under that unhappy label.' (Putnam 1994 *op. cit.* p. 463 footnote 41)

IR3 Truth is some sort of idealized rational acceptability²⁴⁹.

For the Internal Realist the world still plays its part in determining the results of our experiments and will exclude many interpretations. Internal Realism rejects the forms of relativism which hold that the truth is constituted by what is believed, for example, about elementary particles by the international community of theoretical physicists. That is, the Internal Realist, like the Scientific Realist, allows that the whole community might be mistaken.

The fact that one cannot have a 'God's eye view' of the world but must have a system of concepts does not mean that our observations are illusions. The Internal Realist can give the same unequivocal ontological status to electrons and neutrinos as to moderate sized dry goods or rainbows or shadows. She does not share with the Scientific Realist the thesis that the entities to which ontological commitment is made can coherently be said to exist independently of any conceptual scheme. She asserts that any talk of quarks is necessarily embedded in a conceptual scheme, and that there may be a legitimate inferentially isolated conceptual scheme that makes no use of quarks. Moreover, neither of these conceptual schemes can inevitably be given a privileged status as describing the Scientific Realist's mind-independent world.

A useful metaphor is that of a grid. We need a grid (a conceptual framework) to describe the world, and many grids may serve, but that does not mean we can describe the world in any way we like. For example, it is no longer possible to use the *weltanschauung* of nineteenth century physics to discuss the trajectory of elementary particles.

²⁴⁹ Putnam no longer believes this is an adequate account of truth but does still hold that truth is a substantial property. Truth and ideal warrant are interdependent notions but truth is not defined as ideal warrant. Internal Realism requires an epistemic notion of truth but it is not essential that it be given by IR3, although Putnam continues to believe IR3 is adequate in relation to most ordinary empirical assertions.

A passenger in a uniformly moving train is quite right to assert that she is at rest, and a fellow passenger is quite right to assert that she is moving at 150 km per hour. They are using different frames of reference and these are simply different descriptions of the same event. Alternatively they could be described as each using a different grid. However, nobody can correctly use a conceptual scheme which allows movement faster than the velocity of light – a conceptual scheme that was implicitly used for millennia prior to 1905. For Putnam this is not because there is a definite Scientific Realist's world that the 'Newtonian grid' does not fit, rather the proposed grid fails satisfactorily to confront the data.

These last remarks say no more than would be acceptable to many Scientific Realists or within various other philosophical stances. As Ayer writes:

the very notion of there being a world of such and such a character only makes sense within some system of concepts which language embodies. This is not to say that the world does not exist independently of our talking about it, or that any one system of concepts is as good as any other. We can and do bring different systems to the judgment of experience, and our observations lead us to believe that the world has existed, and probably will continue to exist, without containing any human beings to be conscious of it. Even so, our experience is articulated in language, and the world we envisage as existing at times when we do not is still a world which is structured by our method of describing it. As I said before, we cannot detach ourselves from every point of view. If we abandon one then we have to occupy another. The idea that we could prise the world off our concepts is incoherent; for with what conception of the world would we then be left?²⁵⁰

Putnam's point is that there is no way the world can dictate our conceptual scheme to us although it does constrain adoption of a scheme. Hence Internal Realism is a form of conceptual relativism – truth is relative to a conceptual

²⁵⁰ Ayer, A.J. 1976 *The Central Questions of Philosophy* Harmondsworth: Penguin Books p. 49

scheme²⁵¹. 'To grant that there is more than one true version of reality is not to deny that some versions are false'²⁵².

Putnam's view is in stark contrast to the views of the Scientific Realist who commonly claims that 'the world has a definite and mind-independent natural-kind structure'²⁵³. Lewis goes so far as to say that Putnam's thesis is incredible²⁵⁴. He goes on to write²⁵⁵:

Among all the countless things and classes that there are, most are miscellaneous, gerrymandered, ill-demarcated. Only an elite minority are carved at the joints, so that their boundaries are established by objective sameness and difference in nature. Only these elite things and classes are eligible to serve as referents.

Lewis is asserting that there is an objective inegalitarianism of classifications, and that contemporary physics comes close to providing a correct list. For Lewis, sticks and stones are elite but less so than electrons and quarks. Sticks and stones would in turn be more elite than an object like a complex of a grue gemstone and Putnam's arteries; this last being not elite at all. Putnam has responded by calling Lewis's talk of elite classes 'spooky' and 'medieval sounding'²⁵⁶.

²⁵¹ Note this is different from the extreme position seemingly sometimes taken by Kuhn (and taken by some of his successors) that reality is relativized to paradigms. These philosophers have argued that theory influences perception to such an extent that holders of pre and post revolutionary theories in science literally see the world differently ('Pendulums were brought into existence by something very like a paradigm-induced gestalt switch.' Kuhn, T.S. 1970 *The Structure of Scientific Revolutions* Chicago: University of Chicago Press p. 120). Putnam would deny that the Newtonian scheme and general relativity are incommensurable. Putnam is not saying that we can go from the fact of introducing a concept for a kind of thing permits individuation of things of the kind, to a conclusion that the introduction of such concepts has a role in creating the things in its extension. Some writers have attributed to Wittgenstein the conceptual relativism embodied in Putnam's Internal Realism citing, for example, 'Here we see that the idea of "agreement with reality" does not have any clear application.' (Wittgenstein, L 1969 *On Certainty* Oxford: Basil Blackwell § 215)

²⁵² Putnam, H. 1980 'Models and Reality' *Journal of Symbolic Logic* 46, 464-482, p. 477.

²⁵³ Psillos, S. 1999 *Scientific realism: how science tracks truth* New York: Routledge p. xix.

²⁵⁴ Lewis, D. 1984 'Putnam's Paradox' *Australasian Journal of Philosophy* 62, 221-236 p. 221.

²⁵⁵ Lewis, D. *ibid* p. 227.

²⁵⁶ Reported by Lewis *ibid* p. 229.

Putnam's Internal Realism says there is 'more than one true version of reality'. It is not merely that there are different perspectives on the world and that some perspective is inescapable. We must reject the appealing visual metaphor that we can have different perceptions of a mind-independent world. The metaphor is appealing because we readily and correctly think of different views of the world. We can make an image on a photographic plate of the night sky with an optical telescope. The photographic plate is manufactured so as to be sensitive in the human visual range, so we will get a picture which is like that of the unaided eye, but more detailed. Likewise, we can use a ground or satellite based telescope with sensors in the infrared region of the electromagnetic spectrum and we will get a different picture, also informative, but very different and inaccessible to unaided human vision. It is natural to think of these as different perspectives on a 'unique world out there'.

This visual metaphor, says Putnam, is a fundamental error²⁵⁷. There is no such mind-independent world and it is meaningless to talk as if there were. It must be remembered that the perspectival stance leads readily to the metaphorical parallel with different visual perspectives on a given scene, and this slide leads to the erroneous concept of a unique correct description of reality.

It is worth noting here the contrast with McMullin's position. Mc Mullin writes²⁵⁸:

The Dirac electron is completely specified by Dirac's theory. But the *real* electron, the entity whose nature has gradually become clearer, thanks to a confluence of theories and a strong intervention-type argument is not

²⁵⁷ Sankey argues that Putnam's rejection of a God's eye point of view, which *inter alia* makes Putnam reject the perspectival stance, is mistakenly applied to Scientific Realism. We can, Sankey argues, view the world from a perspective and 'propose an epistemological model of the relation between human thought and our surrounding environment'. Sankey describes how we can consider, from our perspective, the supposed mental life of animals without being able to comprehend their thought processes. Likewise he says we can do so for our own species (Sankey, H. 2004 'Scientific Realism and the God's Eye Point of View' *Epistemologia* 27, 211-226). (While he defends the propriety of the God's eye point of view, Sankey also maintains that the view is in any case not necessary for Scientific Realism.)

²⁵⁸ McMullin, E. 1987 'Explanatory Success and the Truth of Theory' in N. Rescher (Ed) *Scientific Inquiry in Philosophical Perspective* Lanham: University Press of America p. 65.

exhausted by any theory we now have or for that matter ever may have.

(Author's italics)

For the Internal Realist the *real* electron of McMullin's Scientific Realist is not a tenable postulated entity. (Putnam would not deny that we may never have a completely satisfactory theory in any terms, on any conceptual scheme, that accounts for all the phenomena that the electron of current Quantum Electrodynamics claims to account for.)

The following passage²⁵⁹, already quoted in section 5.3, is important for Putnam's argument, because otherwise IR3 would be in conflict with IR2. Idealized rational acceptability does not imply, '... conditions which are *simultaneously ideal* for the ascertaining of any truth whatsoever, or simultaneously ideal for answering any question whatsoever.'²⁶⁰ Putnam²⁶¹ makes an analogy between idealized rational acceptability and the physicist's frictionless plane, an idealization we can approach as nearly as we please but not attain. Idealized is short of perfect²⁶². Early in the nineteenth century an Internal Realist would have asserted that space was Euclidean. This would have been a strongly held framework truth. On Putnam's continuum²⁶³ from the analytic to the synthetic, this 'truth' would have been near the analytic end.

6.2.2 Conceptual Schemes

Given Putnam's stance that there is a continuum between the analytic and the synthetic, it might be thought that he must follow Davidson in the conclusions he draws from rejecting the scheme-content dichotomy. Thus, there might appear to be concerns about the postulation of conceptual relativism, that is, the proposed

²⁵⁹ Putnam, H. 1990 *Realism with a Human Face* Cambridge MA: Harvard University Press p. viii.

²⁶⁰ Wright remarks more generally, 'There seems no good reason ... why all questions which are empirical in content should become decidable under ideal conditions.' (Wright, C. 1992 *Truth and Objectivity* Cambridge MA: Harvard University Press p. 39)

²⁶¹ Putnam, H. 1981 *Reason, Truth and History* Cambridge: Cambridge University Press p 55.

²⁶² Putnam acknowledges that there is no universal method for specifying these satisfactory conditions for making true empirical assertions since they are relative to the type of discourse (Putnam, H. 1983 *Philosophical Papers* Volume 3 Realism and Reason Cambridge: Cambridge University Press p. xvii, p. 231).

²⁶³ Putnam, H. 1994 *Words and Life* Cambridge MA: Harvard University Press p. 249.

existence of distinct conceptual schemes. Davidson famously rejects conceptual relativism²⁶⁴ – according to him the notion of alternate schemes is not comprehensible. But if one cannot say that entities exist either mind-independently or mind-dependently because mind-independence is an unacceptable notion, then this is an argument for Anti-realism. As Khlentzos says²⁶⁵: ‘Once we reject the illusion of a distinction between a conceptual scheme or a system of practices and a reality to which the conceptual scheme or system of practices is answerable, the realist’s credo is revealed as simply incoherent.’ However, Davidson thinks that the Realism, Anti-realism debate is misconceived²⁶⁶.

The important question for present purposes is whether one has to accept Davidson’s rejection of the possibility of different conceptual schemes. I argue that the answer to this question is in fact no.

I have earlier accepted Grover Maxwell’s (and other) arguments against a sharp observable-unobservable distinction (sections 1.1 and 2.5.3) and will argue that this is compatible with conceptual relativism.

The conclusions that Davidson draws from his holism are not universally accepted, and there are significant arguments against them in the literature²⁶⁷. For example, Williamson states²⁶⁸,

²⁶⁴ Davidson, D. 1984 *Inquiries into Truth and Interpretation* Oxford: Oxford University Press.

²⁶⁵ Khlentzos, D. 2004 *Naturalistic Realism and the Antirealist Challenge* Cambridge MA: MIT Press p. 43.

²⁶⁶ Davidson’s position in the Realism Anti-realism debate is subject to controversy, ranging from his own view that on his terms the debate is misconceived, to the view that the Principle of Charity is an Anti-realist stance to, at the other extreme, a Realist construal (see for example Calvert-Minor, C. 2009 ‘Commonsense Realism and Triangulation’ *Philosophia* 37, 67-86).

²⁶⁷ See for example Soames, S. 2003 *Philosophical Analysis in the Twentieth Century* Princeton: Princeton University Press Volume 2 Chapter 13; Williamson, T. 2007 *The Philosophy of Philosophy* Oxford: Blackwell Chapter 8; Goldberg, N. and Rellihan, M. 2008 ‘Incommensurability, Relativism, Scepticism: Reflections on Acquiring a Concept’ *Ratio* 21, 147-167.

²⁶⁸ Williamson, T. *ibid* p. 260 (the reference to Davidson (1974) is to ‘On the very idea of a conceptual scheme’ which is reprinted in Davidson, D, 1984 *Inquiries into Truth and Interpretation* Oxford: Clarendon Press).

Davidson's principle of charity evokes massive disagreement. However, it is not wholly to blame for the contentious conclusions that Davidson wishes to draw. It figures in his notorious argument against the very idea of mutually incommensurable conceptual schemes, alien ways of thought or untranslatable languages (1974). Besides that argument also makes the verificationist assumption that other creatures have beliefs only if we have good evidence that they have beliefs and the constructivist assumption that we can have good evidence that they have beliefs only if we can have good evidence as to which beliefs they have. Neither assumption follows from the principle that beliefs tend to be true. Neither assumption is warranted as we are far from omniscient interpreters ...

Kuhn²⁶⁹, in defending his incommensurability position against Davidson, developed a local version of incommensurability, restricting translation failure to clusters of inter-defined terms from the different theories. He proposed that in a scientific revolution that there was a shift from one taxonomy of natural kinds to another, untranslatable into the first. Restricting inability to translate to clusters of terms, or even to the entire vocabulary of the theories, may eliminate the necessity of making sense of either the scheme-content dualism or an untranslatable language.

Davidson explicitly denies this²⁷⁰:

We must conclude, I think, that the attempt to give a solid meaning to the idea of conceptual relativism, and hence to the idea of a conceptual scheme, fares no better when based on partial failure of translation than when based on total failure. Given the underlying methodology of interpretation, we could not be in a position to judge that others had concepts or beliefs radically different from our own.

²⁶⁹ Kuhn, T.S. 2000 *The Road Since Structure* J. Conant and J. Haugeland (Eds) Chicago: University of Chicago Press pp. 93-94.

²⁷⁰ Davidson, D, 1984 *Inquiries into Truth and Interpretation* Oxford: Clarendon Press p. 197.

The argument he gives for his stance is essentially the same as that which he gives for the case of total failure. In the partial failure case critics have argued that it is problematic *a priori* to rule out the possibility that Putnam postulates.

Swyer²⁷¹ remarks,

the possibility that certain aspects of human cognition result from the contingent, often highly accidental, evolutionary history of our species, and if the similarities among human beings stem from contingent features of our brain and our sensory apparatus, creatures with quite different biological makeups might perceive and think in quite different ways.

Aliens might understand one another while we cannot understand them.

Moreover, we might even be able to learn their language and still be unable to translate it into ours. 'If a lion could talk, we could not understand him'²⁷² may not be right. Perhaps we could understand, having learned the language, but still be unable to translate into English. Of course, the fact that we can coherently imagine beliefs that differ in modest ways from ours does not provide a firm assurance that we can coherently imagine ones that differ massively. We cannot envisage alternatives that involve jettisoning the means that enable us to envisage them. What is clear is that for the physicist there are two conceptual schemes – that of general relativity and that of quantum mechanics – and these schemes are inferentially isolated. That is, any combining of the two schemes would result in a contradiction.

If the denials of the most radical aspects of Davidson's stance are accepted, we can allow that the Internal Realist can propose conceptual relativity – the existence of distinct inferentially isolated conceptual schemes. That is conceptual schemes which if combined would lead to a contradiction. Hence the Internal Realist can believe both the general theory of relativity and quantum mechanics, despite the fact that they are inferentially isolated, and, as an Internal Realist,

²⁷¹ Swyer, C. 'Relativism' *The Stanford Encyclopedia of Philosophy*
<http://plato.stanford.edu/entries/relativism>.

²⁷² Wittgenstein, L. 1953 *Philosophical Investigations* Oxford: Blackwell p. 223.

one can accept this and the possibility that they may continue indefinitely with precision to account for every observation in each of their domains. Putnam does not accept the assumption that physics is ever going to give us a Grand Unified Theory; indeed, as I cited him earlier (section 5.3) he rejects this claim as 'sheer dogmatism'.

This acceptance of these two scientific theories, on the grounds of the distinct conceptual schemes they embody, raises the question of precisely what is meant by Putnam by the term 'conceptual scheme'. Clearly scientists do not accept that 'p' and 'not p' can arise from one acceptable theory. Nor do scientists normally accept that two theories they regard as incompatible are both true. In the example mentioned earlier, the (erroneous) caloric theory that heat is a material substance was favoured for a long time over the contemporaneous kinetic theory of molecular motion. Scientists at the time will have used each for particular purposes, but would either suspend judgment as to which was correct, or believe one but not the other, taking it to have merely instrumental value. In the quantum mechanics general relativity case the situation is the same. However, the Internal Realist can believe both, which perhaps few practicing scientists would do, though they do certainly use both. Now the Internal Realist is not claiming that whenever there are conflicting meritorious scientific theories, there are legitimate distinct conceptual schemes, and hence both theories should be believed. What then is meant in the quantum mechanics general relativity case in claiming that we do indeed have two conceptual schemes?

Soames, in the different context of discussing Davidson's work, asserts that 'conceptual scheme' is a vague notion, but offers the following example²⁷³.

Consider the beliefs of speakers whose natural and unreflective way of looking at the world does not involve the recognition of objects that persist through time, but does involve the recognition of temporal stages of objects

²⁷³ Soames, S. 2003 *Philosophical Analysis in the Twentieth Century* Princeton: Princeton University Press Volume 2 p. 320.

that bear proximity relations to one another. Do such speakers have a conceptual scheme different from ours? Is it conceivable that we could interpret them? So long as the speakers in the alien culture are willing to grant that there are certain kinds of sums of temporal stages over time that could be regarded as enduring objects – as strange and unnatural as these objects seem to them (*“Of course such objects exist but why would anyone be interested in them?”*) – and so long as, given our enduring objects as starting points, together with moments of time, we are willing to grant the existence of temporal stages – as strange and unnatural as they seem to us – there may be no fundamental disagreement between the two cultures as to what objects exist. Neither culture need view the claims made by the other as false. Rather each may view the entities that the other takes as most worthy of attention as strange, unusual and derivative; and because of this, each might find it difficult to explain or understand how the other could take such unnatural entities as basic. But speakers of the two cultures need not disagree over the kinds of objects that exist, or even over their properties. Thus, the need to ensure agreement in order to construct a Davidsonian theory of interpretation does not have any significant bearing on the possibility that such an alien culture might exist, or even that we could interpret them.

These schemes are incompatible in that one does not recognize objects that persist through time and the other does so. The situation can be compared with the incompatibility of the Newtonian scheme with that of special relativity. If the velocity of light were infinite these two schemes would be compatible. Long before Einstein's realization that the velocity of light was constant it was known that the velocity of light was finite²⁷⁴. If some earlier genius had proposed the

²⁷⁴ A reasonably accurate measurement of the speed of light was made in 1676, by Romer who studied Io, a moon of Jupiter, which was eclipsed by Jupiter at regular intervals. Romer found that during the course of a year the eclipses lagged more and more behind the expected time, but then began to pick up again. As the Earth and Jupiter move in their orbits, the distance between them changes. The light from Io takes a finite time to reach the Earth, and takes longest when the Earth is furthest from Jupiter. Indeed Newton wrote in the *Principia* (Book I, section XIV), published in 1687: 'For it is now certain from the phenomena of Jupiter's satellites, confirmed by

special theory in the eighteenth century, a Newtonian would, in principle²⁷⁵, have been forced to acknowledge that her scheme was untenable.

It must be acknowledged that to accept the argument that we do have two viable conceptual schemes in operation, we have to accept the scheme independence of at least part of our evaluative principles. In other words, judgments are made that in some respects transcend the internal judgments within the two schemes. The Newtonian has to accept that within her conceptual scheme the experimental evidence of the constant finite velocity of light cannot be accommodated. This is merely to reiterate Putnam's point that the existence of more than one conceptual scheme does not lead to an empty relativism. However, prior to Einstein's thought experiment, the Newtonian scheme had every appearance of internal consistency²⁷⁶.

The grounds for making a claim for differing legitimate conceptual schemes in the quantum mechanics general relativity case are twofold. As outlined earlier they are, first, the predictive power, coherence and profound insight provided by each scientific theory, and the spectacular agreement of each theory with all the data relevant to it; second, the long term failure, despite a huge concentration of effort, to find a theory that reconciles the two schemes.

In accepting both the quantum mechanical and the general relativistic schemes the Internal Realist is relying on the belief that in part her evaluative principles are applicable to both schemes; otherwise the judgment could not be made. This is not inconsistent with the two schemes being inferentially isolated; it is only to say that the two schemes are correct judged by criteria internal to each of them,

the observations of different astronomers, that light is propagated in succession and requires about seven or eight minutes to travel from the sun to the earth' (cited in 'Relativity resources' www.relativitybook.com).

²⁷⁵ Many physicists persisted in scepticism about the special theory long after 1905.

²⁷⁶ As was noted in footnote 274 above, the finite velocity of light was known in Newton's time, so it seems legitimate (with hindsight) to speak only of the 'appearance' of internal consistency.

and neither is successfully invalidated by any criterion that can be applied to each.

In discussing this, Caruana²⁷⁷ suggests that having the same doxastic attitude towards some basic propositions leads to the requirement that Putnam acknowledge the mind-independence of something – namely this common ground. This Caruana argues does not undermine Putnam's Internal Realism because all that is required is a shared human nature and thus a common cognitive faculty.

Returning to the incompatibility of quantum theory and general relativity, the schemes are incompatible in the same sense as the Soames example. A quantum theory of gravity requires gravitons²⁷⁸ for which quantum mechanics cannot give a credible account. Einstein's general theory of relativity makes no place for the existence of gravitons, that is, it is not a quantum theory. Consequently the conjunction of the two theories is not an acceptable theory²⁷⁹.

The Scientific Realist on the quantum theoretical view could envisage the gravitational attraction between two bodies as being due to the exchange of virtual 'gravitons' – the quanta of the gravitational field. However, gravity differs from electromagnetism in that the gravitational field is nonlinear. This nonlinearity arises because the gravitational field possesses energy; this implies that gravitons will interact with other gravitons, unlike photons, which interact only with electric charges and currents and not with other photons. In quantum

²⁷⁷ Caruana, L. 1996 'Beyond the Internal Realist's Conceptual Scheme' *Metaphilosophy* 27, 296-301.

²⁷⁸ The particle that mediates the gravitational force; there is currently no direct experimental evidence for its existence.

²⁷⁹ For many applications general relativity and quantum mechanics have disjoint experimental domains. General relativity is observable with massive objects. Quantum effects are primarily observable with minute particles, although macroscopic phenomena like superconductivity require quantum mechanics for their explanation. Nevertheless, for many applications these incompatible theories can coexist in a temporary truce. Unfortunately any calculation that simultaneously uses both of these theories gives nonsense. Theorists argue that the origin of this lies in equations which become badly behaved when particles interact with each other across distances of the order of 10^{-33} cm – the Planck length.

electrodynamics the resulting infinities are removed by renormalization and sensible answers are obtained. Thus quantum electrodynamics is a renormalizable theory because all such infinities can be removed by a systematic procedure; in effect, a single set of mathematical operations is sufficient to remove the infinities. This cannot be done for quantum theories of gravity.

There remains the vexing problem of giving a clearer and more detailed account of what constitutes a conceptual scheme. The Newtonian scheme was accepted as accurately encompassing many physical phenomena and it coloured the views of scholars well beyond the arena of its direct applicability²⁸⁰. Nevertheless, it left uninfluenced large areas of conceptual thought even among those contemporaries and successors who were well versed in Newton's work. Indeed even among these scholars it had no impact within many fields of science. It seems one can say little more than that a conceptual scheme provides an intellectual framework (a set of background assumptions) applicable within a certain domain. It does not appear possible to define precisely that domain even within a given conceptual scheme. Where the Internal Realist differs from the Metaphysical Realist is however clear. Putnam's Metaphysical Realist believes that a mind-independent world specifies a unique true way of describing the world.

The physicists' current stance that they can use quantum mechanics and they can use general relativity in circumstances where one or other is appropriate, but cannot allow inferences from one of these schemes to the other is in stark contrast to the stance of the Metaphysical Realist. We have an instance of two conceptual schemes as the Internal Realist might characterize them. As I have mentioned previously, many theoretical physicists are dissatisfied with this situation, which they see as indicating that something is seriously wrong with one or both theories.

²⁸⁰ It is sometimes argued that it was Newton's conception of the universe based upon natural and rationally understandable laws that became the seed for Enlightenment ideology.

6.2.3 Putnam's Pragmatism

Putnam explains his use of 'use' in the important second quotation (in section 6.1 above) beginning 'The suggestion which constitutes the essence of "internal realism" is that truth does not transcend use', as follows²⁸¹. Putnam draws on an interpretation of Wittgenstein asserting that those inside a language game have learned not only rules or a technique, but 'correct judgments' Putnam gives an account of how a competent electrician might describe measuring current, including an account of the workings of a voltmeter and adds²⁸²,

... knowing the 'use' of 'current is flowing through a wire' is knowing things like *this*. Of course, much else is presupposed; in fact, acculturation in a technical society, with all that this entails. *Understanding a language game is understanding a form of life*. And forms of life cannot be described in a fixed positivistic meta-language, whether they be scientific, religious, or of a kind we do not have in Western societies today.

This key insistence on the primacy of practice seems at first sight to make far too much of a concession to relativism and the authority of the communal norms than Putnam intends; but Putnam emphatically does not believe that whether a statement is true or not is a matter of community acceptance, '... it is a property of truth that whether a sentence is true is logically independent of whether the majority of members of a culture believe it to be true.'²⁸³

His argument in the remark about current flow in the wire is that the competent electrician or electrical engineer in this example will confront reality, because various things are part of a day to day system that works, resulting in a world somehow incorporated in the 'form of life' (the intertwined social and linguistic practices) of the competent electrician or electrical engineer. Putnam is claiming that truth and assertibility are inextricably related, and the form of life of the

²⁸¹ Putnam, H. 1995 *Pragmatism: An Open Question* Oxford: Blackwell pp. 45-52.

²⁸² Putnam 1995 *op. cit.* p. 48.

²⁸³ Putnam, H. 1988 *Representation and Reality* Cambridge MA: MIT Press p. 109. Various philosophers argue that Putnam's own Internal Realism undermines this robust rejection of this form of relativism (see for example Norris, C. 1997 *New idols of the cave* Manchester: Manchester University Press Chapter 6).

competent electrician enables her to warrantably assert there is 'current flowing in the wire', assuming she is at home in the form of life and in an ideal epistemic situation (where ideal is humanly possible, and is not some hypothetical perfect limit of inquiry)²⁸⁴. Inquiry is inductive and open-ended. Thus practice can be assumed to provide the cues for truth. Moreover, our epistemic standards evolve in the trial-and-error process of inquiry itself.

A difficulty with this pragmatist stance is that highly competent experienced practitioners in various areas have throughout history agreed on, and regarded as obviously true, important falsehoods. Thus there seems to be a danger of an unacceptable relativism. However the Internal Realist accepts that some conceptual schemes are inadequate. Internal Realism is fallibilist, so false beliefs (common in science) would draw the response that no one doubts that mistakes have been made in the past, and past and present knowledge are limited. Further, Putnam would say, despite the obvious truth that errors have been made in the past and surely are being made currently, we should not doubt any propositions we have no specific reason to doubt.

... we don't need a 'rule' to take care of a 'doubt' that is wholly without justification!²⁸⁵

Pragmatism ... is characterized by being *fallibilist* and *anti-skeptical*²⁸⁶

The heart of pragmatism is the idea that notions that are indispensable to our best practice are justified by that very fact ...²⁸⁷

Putnam is anxious to maintain simultaneously the truth of our common sense view of a table and the physicist's view of the same table in terms of elementary

²⁸⁴ Putnam does not take the true to coincide with the warrantably assertible – they must come apart because something that was warrantably assertible at some time in the past may now be known not to be true.

²⁸⁵ Putnam, H. 2001 *Enlightenment and Pragmatism* Amsterdam: University of Amsterdam Press p. 40.

²⁸⁶ Putnam, H. 2004 *Ethics without Ontology* Cambridge MA: Harvard University Press p. 99.

²⁸⁷ Putnam, H. in *Reading Putnam* 1994 P. Clark and R. Hale (Eds) Oxford: Blackwell p. 260.

particles. This he calls conceptual pluralism.²⁸⁸ He cites the rainbow, which we see as showing bands of red, orange, yellow, green, blue, indigo, and violet, while in fact there is a continuous spectrum (the physicist's rainbow)²⁸⁹. Vision does not show us a 'ready-made world'; our perceptions are already structured²⁹⁰ by our conceptual scheme. As Putnam points out, this 'human' rainbow is in part determined by our visual system including the way our brain processes its input. This last may include cultural factors. The 'human' vision of the rainbow is not defective because of the fact that it includes bands of colour not present in the physicist's rainbow – it 'fits the objects *for us*, not metaphysical things in themselves'.²⁹¹ The two pictures are not incompatible and as I said earlier this is an example of Putnam's conceptual pluralism.

Putnam argues that Scientific Realism²⁹² entails that our concepts and categories refer because they match up in some mysterious manner with the pre-structured

²⁸⁸ In *Representation and Reality* he called this 'conceptual relativity'. Later he acknowledges this as a mistake and refers to the chairs versus assemblage-of-molecules dichotomy as 'conceptual pluralism'. By conceptual pluralism Putnam means that we can use both the common sense view and the physicist's view 'without being required to reduce one or both of them to some single fundamental and universal ontology' (Putnam, H. 2004 *Ethics without Ontology* Cambridge MA: Harvard University Press p. 49) Thus, for example, it is simply a matter of convention whether a chair is identical with the region it occupies in space-time or with the sum of the time slices of the molecules it contains. Conceptual relativity implies conceptual pluralism but the reverse is not the case (Putnam, H. 2004 *ibid* pp. 48-49). It seems reasonable that someone might refer to metal window frames and someone else to aluminium window frames and a third person to precipitation hardened aluminium alloy window frames. The first two are correct ways of speaking; the last is more precise and could be called the solid state physicist's window frame. A theoretical physicist's window frame might consist of quarks and leptons. It is unclear what significance Putnam attaches to conceptual pluralism; it seems perfectly acceptable to the Scientific Realist and unlike conceptual relativity it does nothing to undermine the notion of a unique ontology. Of course, the Scientific Realist denies the meaningful existence of Putnam's conceptual relativity and argues that there is only the acceptable conceptual pluralism. In her view there is always a unique truth of the matter, even if we do not or cannot know it. (Putnam's common example of conceptual relativity is the three objects, seven objects dichotomy in the Carnapian versus mereological account described in section 5.3 above and discussed further in footnote 300.)

²⁸⁹ Putnam, H. 1981 *Reason, Truth and History* Cambridge: Cambridge University Press p. 146.

²⁹⁰ Putnam himself points out that that this can lead to falsehood. We may see someone as a witch, as was done with complete conviction in Europe for hundreds of years (Putnam, H. 1995 *Pragmatism: An Open Question* Oxford: Blackwell p. 67).

²⁹¹ Putnam 1981 *op cit.* p. 146.

²⁹² Putnam refers to the position he defines and criticizes as Metaphysical Realism. Insofar as they are valid they would apply to Scientific Realism as I have defined it (after Ladyman) in Chapter 1.

categories, kinds and individuals that are inherent in the external world²⁹³. He rejects this, saying²⁹⁴: 'It is we who divide up "the world" – that is the events, states of affairs, and physical, social, etc., systems that we talk about – into "object", "properties", and "relations", and we do this in a variety of ways.' The world does not come pre-structured but structure is imposed on it by the human mind and its conceptual schemes. The conceptual pluralism is acceptable to the Scientific Realist, who would not deny the rainbow story just given²⁹⁵. However, for Putnam the imposition of conceptual schemes by the human mind also leads to conceptual relativism, an essential element of Internal Realism, where different conceptual schemes may be incompatible.

This position is of course not uncontroversial. Brown writes²⁹⁶

One can grant that it is false that the world has one true classification or division into objects and kinds of objects without granting that it has *no* organization of its own. A *modest* realist might well say that the world has infinitely many classifications, that it contains infinitely many different kinds of objects. Such a moderate realist could say that when we develop a language we are not imposing an organization on the world, but selecting one of the world's organizations for our own use. On this view the world "in itself" has *more* objects than we usually talk about, not fewer.

The Internal Realist would regard this statement as incoherent; 'the world' does not have any organizations from which to choose. There are no self identifying objects in the world. There can be many human descriptions but no one of them is of some 'real' mind-independent world. Putnam asserts²⁹⁷:

²⁹³ 'The metaphysical realist insists that a mysterious relation of correspondence is what makes reference and truth possible.' (Putnam, H. 1990 *Realism with a Human Face* Cambridge MA: Harvard University Press p. 114)

²⁹⁴ Putnam, H. 1994 'Replies' in Clarke and Hale (Eds), *Reading Putnam* Oxford: Blackwell p 243.

²⁹⁵ For the Scientific Realist these are simply different observations leading to different description of (or perspectives on) a mind-independent world.

²⁹⁶ Brown, C. 1988 'Internal Realism: Transcendental Idealism?' *Midwest Studies in Philosophy* 12, 145-55.

²⁹⁷ Putnam, H. 1994 *Reading Putnam* P. Clark and R. Hale (Eds) Oxford: Blackwell p.291.

There are ways of describing a physical object other than describing its appearance from a visual perspective; referring to alternative schemes as 'perspectives', of course, makes it sound as if there must be ways of describing the world apart from the descriptions. This is just the disastrous Kantian error we need to avoid.

Brown in turn would presumably respond to this response by saying that to deny that the world has a determinate unique structure is not to deny that it has structure. Putnam however maintains that the only plausible form of Scientific Realism is the form that he has characterized as Metaphysical Realism and this incorporates physicalism, a Correspondence Theory of truth and hence a unique structure.

McMullin believes that the rejection of Metaphysical Realism does not immediately undermine Scientific Realism, and maintains *contra* Putnam that there is an acceptable form of metaphysical realism, which he does not specify, although he rejects aspects of Putnam's account of Metaphysical Realism. It is worth quoting his slightly ambiguous remarks at length²⁹⁸.

My own inclination is to defend a form of metaphysical realism, though not necessarily under all the diverse specifications Putnam offers of it. But that is not the point here. What is to the point is that scientific realism is not immediately undermined by the rejection of metaphysical realism, though the character of the claim scientific realism makes obviously depends on whether or not it is joined to a concept of truth in which the embattled notion of "correspondence" plays a part. Further, the type of argument most often alleged in its support *does* use the language of correspondence: it is the approximate correspondence between the physical structure of the world and postulated theoretical entities that is held to explain why a theory succeeds as well as it does.

²⁹⁸ McMullin, E. 1984 'A Case for Scientific Realism' in *Scientific Realism* J. Leplin (Ed) Berkeley: University of California Press, p. 25.

McMullin here uses a form of words that seems to commit the unnamed philosophers to approximate truth and to a correspondence theory of truth.

The essence of Putnam's defense against the charge that his claim, 'truth does not transcend use', leads to an unacceptable form of relativism is that science is a shared practice. The different equally correct descriptions (including quite possibly a different ontology) in a given domain are each the result of a common shared practice of inquiry involving cooperation and communication²⁹⁹. Each is fallible and they can be incompatible, and each can be true. There is no one true description in the sense that any different description is therefore false. This does not allow two true descriptions to be contradictory but it does allow them to be incompatible³⁰⁰. Thus it is not possible to express the general theory of relativity in a manner consistent with quantum mechanics. It is of course also possible that revision is required in any or all of the descriptions.

Thus the Internal Realist takes the present successful mature scientific theories to be ideally rationally acceptable or ideally warrantably assertible, since there is

²⁹⁹ This might appear to be inconsistent in that the shared environment must be identified with the rejected mind-independent reality. However, this is not so; the sharing comes through 'common inductive practices involving communication and cooperation' (Mueller, A. and Fine, A. 2005 'Realism, Beyond Miracles' in *Hilary Putnam* Y. Ben-Menahem (Ed) Cambridge: Cambridge University Press p. 114). This has already been discussed in section 6.2.2 where Caruana's earlier paper is cited.

³⁰⁰ To say 'There is a fire in the room and I do not believe there is a fire in the room' is to make incompatible assertions. The assertions are not however contradictory. Wittgenstein referred to this as 'Moore's Paradox'. Putnam's usual example is somewhat different; it is the three objects seven objects dichotomy in the Carnapian versus mereological account described in section 5.3. In this case both the Carnapian and the mereological counter will recognize that the other gives the same answer as she gave previously for all the cases where they themselves gave the same answer. That is, counting is taking place and there are no grounds for believing that either is in error or, as Putnam would add, there is any truth of the matter. The schemes are inferentially isolated in my analysis because if they were not we would have a contradiction. Provided they are isolated there is no instance of asserting p and not p (such as there are 3 objects and there are not 3 objects because there are 7). Scientific Realists resist in various ways this conceptual relativity thesis that leads to the concept of incompatible but not contradictory theories. Scientific Realists who reject Putnam's view that their stance commits them to a direct correspondence theory of truth can argue that the three objects seven objects dichotomy does not deny a unique ontology. See for example Horgan, T. and Timmons, M. 2002 'Conceptual Relativity and Metaphysical Realism' *Nous* 36, 74-96.

no reason to suppose that they do not coincide with the outcome of an Epistemically Ideal Inquiry.

From the point of view of the concerns of this thesis, the most important point, as already mentioned, is that the Internal Realist can avoid approximate truth³⁰¹, and can take the less solidly accepted current scientific theories to be false until such time as it is reasonable to suppose they can be justifiably presumed to coincide with the outcome of an Epistemically Ideal Inquiry. Whereas the Scientific Realist must draw on the approximate truth of theories, because her claim for success rests on nearness to a supposed mind-independent reality, the Internal Realist explains success either on the basis of truth or on the basis of the significant claims that are true in the theories. Her stance does not allow of nearness to a mind-independent reality because there is no such thing.

6.3 Internal Realism and the McMullin Conjunction

My argument has been that the Internal Realist need not say that the current theory of the electron is approximately true. As truth is properly understood the theory, which is explanatory and in excellent agreement with experiment, is believed to be true. In saying that the theory is believed to be true, one is not, as an Internal Realist, saying that it will necessarily be believed in its entirety twenty years from now. If it is believed to be true now by the Internal Realist's lights it may be thought or known to be false then by the same standards, by virtue of new data. The dilemma presented by inferentially isolated, long standing, highly successful, mature theories like quantum mechanics and general relativity should persuade an Internal Realist to abandon the concept of approximate truth because she, unlike the Scientific Realist, does not need it (as discussed in

³⁰¹ Putnam himself is untroubled by the notion of approximate truth, which he refers to at various times and treats as unproblematic (see for example Putnam, H. 1984 'What is Realism' in *Scientific Realism* J. Leplin (Ed) Berkeley: University of California Press; Putnam, H. 1975 in 'Philosophy and Our Mental Life' *Philosophical Papers* Volume 2 Mind, Language and Reality Cambridge: Cambridge University Press p. 301). He does acknowledge its ill defined nature but regards it as usable nevertheless (Putnam, H. 1994 *Words and Life* Cambridge MA: Harvard University Press p. 311).

section 5.3). Thus the Internal Realist can avoid an argument that undermines McMullin's case for Scientific Realism.

Turning to nearness to some putative final answer, an Internal Realist would respond, if asked of various figures which one is closest to being a square that it depends on one's explanatory interest. Without that clarification, it is a pseudo question. In other words, an Internal Realist would affirm that a rectangle of edge lengths a , b with $b \neq a$, and $a < 1.01b < 1.01a$, resembles a square, provided one is not interested in four-fold symmetry. A similar response would be given to a question involving an equiangular figure whose four edges of equal length were arcs of circles of large and equal radius. The proviso in this case would be that it was unimportant that the curvature of the edges should always be zero.

The Internal Realist is not suggesting that all conceptual schemes are equally acceptable. The conceptual scheme Mendeleev proposed for the ordering of the elements was hugely successful in predicting the existence and properties of new elements. Obviously, alternative schemes were possible as would be acknowledged by Realists and Anti-realists alike. The point is that Internal Realism fully accepts that the world determines the outcome of experiments. Once an experiment has been set up, the opinions of the scientists, while they unquestionably may play a major role in the interpretation of the results, obviously play absolutely no role in such things as the meter readings or particle tracks³⁰². These results can lead anyone with a theory which explains past results to a new theory which explains the apparently conflicting results of new experiments. The simplest explanation for the Internal Realist (just as for other Anti-realists, as suggested in Chapter 2) of the successive generation of theories from their predecessors is that the theories contain significant claims which are true. Of course, as stated there, this explanation is available to the Scientific Realist. Like McMullin, most Scientific Realists would however prefer to attribute

³⁰² This is not intended to preclude the possibility that the results will be faultily observed as well as misinterpreted.

the P-fertility to the approximate truth of the successive theories because this is consonant with the Scientific Realist picture.

Dalton's atomic theory was wrong about the behaviour of nuclei (of which nothing was known at the time), but it contained significant true claims about the extra-nuclear behaviour of atoms. Similarly, the caloric theory of heat in assuming that the material substance, caloric, was conserved did incorporate (in a restricted form) the true claims about the conservation of energy. It thus allowed developments in the understanding of the distinction between heat capacity and temperature and in the understanding of specific heat.

For the Internal Realist a long standing successful theory based on a given conceptual scheme can be viewed as a succession of theories, each modified just as McMullin suggests. As discussed earlier in relation to the other Anti-realist positions, it is not approximate truth that is invoked to account for this, but the significant claims which are true – truths in this case within the chosen conceptual scheme. These true claims persist as the theory demonstrates its P-fertility over time. Thus to take the two examples previously discussed at some length, given the conceptual scheme, it is true *simpliciter* that dislocations account for the plastic deformation of metals and that the energy levels in the Bohr atom and its successors are quantized.

The Scientific Realist might respond to this by saying that her account is explanatory in a way that the Internal Realist's is not. This argument has little force for someone who believes the Scientific Realist view is incoherent. Nevertheless, because of the underdetermination of theory by experiment, it may seem too easy to propose a theory and maintain that it is true by the Internal Realist's lights. However, the constraints in the mature sciences provided by the extremely rich and precise data and extensive theorizing in related areas make it difficult to find even one satisfactory theory. Hence the underdetermination argument is somewhat academic in the context of the problems arising from the

inferential isolation of the conceptual schemes of quantum theory and general relativity.

6.4 False Successful Theories

Scientific Realists account for the success of false theories by claiming that they are approximately true. This clearly does not work for theories like the phlogiston theory which is not approximately true, so that this remains a problem.

Nevertheless, the success of long standing successful current theories, which the Scientific Realist would say were unlikely to be true, is dealt with by claiming they are approximately true.

The Internal Realist explains that the success of theories that are false is primarily a result of the true significant claims in the false theories³⁰³. (These of course include the theories that the Scientific Realist is claiming are approximately true). The choice between competing theories is made by the Internal Realist on the same basis as the Scientific Realist. The chosen theory simply has more explanatory power, or gives results more closely in accord with the most well established experimental observations, or it is least in conflict with related theories, or some combination of these and other relevant factors³⁰⁴. That is, for the Internal Realist it is more rationally acceptable. The notion of approximate truth need not be invoked.

In the case of successful theories that are entirely devoid of truth, the Scientific Realist and the Internal Realist are equally at a loss. The theory that projectiles return to the Earth because that is their natural resting place certainly made innumerable correct predictions. A Scientific Realist contemporary of Aristotle

³⁰³ There may be a contribution from false elements also (see footnote 124).

³⁰⁴ Such considerations of course do not ensure a correct conclusion. The caloric theory that heat is a material substance was favoured for a long time over the contemporaneous kinetic theory of molecular motion. One reason for this was the very low specific heat of mercury compared with water. In comparing the two liquids it seemed inexplicable on the kinetic theory that the very much heavier mercury atoms could require less energy to raise their temperature than the water molecules. Twentieth century theories of specific heat would not now allow this simple comparison of a liquid like water with a liquid metal.

would have taken the theory to be approximately true and an Internal Realist would have taken it to be true³⁰⁵. The present argument is that the latter claim is clear and false, and that it is not possible to attach a meaning to the former claim. What might it mean to say that this teleological theory was approximately true?

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³⁰⁵ As was remarked in section 1.5, McMullin's claim of the conjunction of P-fertility and long standing success is in fact made for modern or near modern theories.

CHAPTER 7 CONCLUSION

It has been argued that the interesting and novel argument for Scientific Realism that McMullin advances (on the basis of the conjunction of long standing success and P-fertility) requires a satisfactory notion of the approximate truth of scientific theories or of the relative verisimilitude of different theories to the truth. It has been claimed here that *inter alia* the context dependence of these concepts undermines the McMullin's argument. Given this, even based on the acceptance of the correctness of the supposed observed McMullin Conjunction, Internal Realism, without any concession to the notion of approximate truth, and taking truth to be epistemically constrained, is an attractive alternative 'best explanation', based on the truths in the successive versions of the theory.

I have accepted throughout, for the purpose of considering McMullin's thesis, the reality of the McMullin Conjunction. However, rather than drawing on the progressive nearness to the truth of the successor modified theories, the McMullin Conjunction can be explained (by the Internal Realist and the Scientific Realist) by the fact that it results from the true claims in the theories, and the resulting metaphorical suggestiveness of the theories or the productive experiments they suggest. Each of the sequence of theories may lead to a further theory also containing true claims. Sometimes true theories are arrived at, and the Internal Realist would say that, for example, Quantum Electrodynamics is true.

The Scientific Realist picture has an intuitive appeal if the notion of approximate truth of scientific theories is accepted. If the idea of ideal elastic 'billiard ball' point molecules is accepted as approximately true of a mind-independent world, then the modifications of it that lead to the van der Waals theory arise naturally and seem more closely to describe that world. On the other hand, quantum mechanics makes this world, analogous to our macroscopic world, a complete

fiction, albeit a most useful and fruitful guide to a better understanding of the behaviour of gases.

The Internal Realist does not lack resources for explaining P-fertility. She has a stable account of truth to legitimate the claim of persisting significant true theoretical claims within successor versions of long standing successful theories. She has a conceptual scheme which draws on all the scientific evidence and her changing picture of recalcitrant reality changes precisely because the evidence suggests the changes. As already remarked the constraints of the data and related theories severely limit possible conceptual schemes. The notion of our epistemic situation given by Internal Realism offers an account of the success of science without drawing on approximate truth or verisimilitude.

A founding pragmatist, Peirce, wrote³⁰⁶:

There are Real things, whose characters are entirely independent of our opinions about them; those Reals affect our senses according to regular laws, and, though our sensations are as different as our relations to the objects, yet, by taking advantage of the laws of perception, we can ascertain how things really and truly are; and any man, if he have sufficient experience and he reason enough about it, will be led to the one True conclusion.

The Scientific Realist and the Internal Realist would both broadly agree with the above proposition. The former would say that the 'Real things' have a knowable character independently of any account we might give of them, and that science gradually enables us more precisely to establish that unique character. To the extent that it is successful, the theory that expresses this is true or approximately true.

³⁰⁶ Peirce, C.S. 1935 *The Collected Papers of Charles Sanders Peirce* C. Hartshorne and P. Weiss (Eds) Cambridge MA: Harvard University Press 5.384.

Putnam's Internal Realism would have the Internal Realist conclude that there is nothing to be said about the 'Real things' until we propose a conceptual scheme; having done so we would claim that our account (within one or other of our conceptual schemes) of the 'Real things' is either true or false, or we must suspend judgment. (Putnam would however not accept the 'one True conclusion' Peirce sees as the ultimate outcome of scientific inquiry since this is akin to the Metaphysical Realism Putnam rejects.)

In asserting earlier that the McMullin Conjunction as a valid historical account can be explained on the basis of Internal Realism; it is not being claimed that it provides unequivocal support for Internal Realism. If we accept that the McMullin Conjunction is a valid generalization over a broad spectrum of scientific theories, and we also assume that the approximate truth and the verisimilitude of theories are concepts that we cannot justify, then Instrumentalism must also be considered.

In this context one can consider Cartwright's Entity Realism together with van Fraassen's Constructive Empiricism. Neither writer believes in the truth of universal theories; Cartwright because the world is a messy place, and van Fraassen because acceptance is all one should have as an attitude towards scientific theories. Neither philosopher denies there may be a mind-independent world in a sense that Putnam does deny. Cartwright believes in the existence of electrons and quarks, while van Fraassen does not, and asserts that the proper way to pursue knowledge is to accept rather than to believe the scientific theories that postulate such entities. Cartwright would take exception to the assumption Peirce makes in the statement quoted above that there are universal laws. Van Fraassen would take exception to the implication that science seeks and can find the truth. He would be troubled by incompatible theories but not be concerned with problems in giving an account of the approximate truth of theories. Cartwright on the other hand would be sceptical of claims that either quantum mechanics or general relativity is a true account of the world.

We know that innumerable false theories are successful both currently and historically. In different ways fluid dynamics and Newtonian mechanics are hugely successful false theories³⁰⁷. Physicists commonly believe that one or both of the theories of quantum mechanics and general relativity are false, while believing that they contain truths, and are seeking a quantum theory at all energies which accounts for gravitation. To believe that these theories are false is not to deny that they contain important contributions to our knowledge.

The No Miracles Argument based on the success of science is readily accommodated by internal Realism, Entity Realism, and Constructive Empiricism. It can simply be argued that science as a self-correcting enterprise unearths facts about the world (in relation to some conceptual scheme in the case of the first of these three). As a result, later scientific theories generally contain more significant truths and/or fewer significant falsehoods than earlier theories. The fact that a theory contains significant true claims will commonly lead to experiments that will provide information on the merits of the conceptual scheme and/or the theory, and suggest modifications. This does not make the theory more nearly approximately true, since such a notion is unclear.

If the McMullin Conjunction is accepted, Internal Realism, Entity Realism, and Constructive Empiricism remain plausible accounts of the scientific enterprise. (Of course aspects, other than the rejection of approximate truth and verisimilitude and the acceptance of the McMullin Conjunction, which might support or cast doubt on these theories have generally not been explored in this thesis.) The latter two views suggest that there is a given mind-independent

³⁰⁷ I have claimed earlier that the Scientific Realist must argue that one or both of these theories is approximately true. This was in the context of the common premise (shared by McMullin) that success is an indicator of truth. While a common assumption, it is by no means obviously reasonable. As Lyons remarks, 'The truth of one of the many competitors that would share our theory's success would also explain our theory's success.' (Lyons, T.D. 2006 Review of Peter Lipton 'Inference to the Best Explanation' *British Journal for the Philosophy of Science* 57, 255-258). These competitors would of course include theories that have not yet been propounded (see also footnote 89).

world on which we have (perhaps significantly incorrect) perspectival views. The Internal Realist rejects this 'visual perspective' metaphor, and believes there is nothing other than our various conceptual schemes which enables us to partially know the world that circumscribes our conceptual schemes.

While the McMullin Conjunction might be explained on the basis of some clear context independent notion of the approximate truth or the verisimilitude of scientific theories, in the absence of such a notion the novel case made by McMullin does not succeed in providing a convincing abductive justification for Scientific Realism. First, because both the Internal Realist and the Scientific Realist can give a satisfactory account of the conjunction on the basis of the significant true claims in the successful theories; second, because of the reliance of the latter position on the questionable notion of the approximate truth of scientific theories. Thus the claim that Scientific Realism is the best explanation of the observed McMullin Conjunction is not sustained.

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