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WHICH METHODOLOGIES OF SCIENCE ARE CONSISTENT WITH SCIENTIFIC REALISM?

DARRELL PATRICK ROWBOTTOM

THESIS SUBMITTED FOR THE DEGREE OF MA IN PHILOSOPHY,
OF THE UNIVERSITY OF DURHAM

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ABSTRACT: WHICH METHODOLOGIES OF SCIENCE ARE CONSISTENT WITH SCIENTIFIC REALISM?

DARRELL PATRICK ROWBOTTOM

MA IN PHILOSOPHY OF THE UNIVERSITY OF DURHAM, 2002

This thesis sets out to examine which methods it would be most consistent for a scientific realist to adopt when practising mature science, given his philosophical predilections. Moreover, it aims to establish the means by which the closely related question, "Is there any support for the philosophical stance known as scientific realism, given the methods that the modern scientist does, in fact, employ?", might be answered.

In order to do this, it is first necessary to examine scientific realism in detail, and to compare it with competing philosophical positions on science; this is the role of chapter one. Scientific realism is seen to involve four distinct theses: metaphysical, semantic, epistemic, and teleological. The vital role which induction plays in justifying each of these theses is then illustrated, in part by elucidating the limitations of Popper's anti-inductivism, and of subsequent critical rationalist positions.

Second, it is necessary to examine how the methods which one adopts when practising science might be affected by one's philosophical viewpoint. This is the task undertaken in chapter two, where the distinction between normative methods, in which all scientists must take part (in order to practice anything which might be justifiably thought of as a mature science), and auxiliary methods, which are dependent upon the psychological state of the individual practitioner, is established, based on plausible demarcation guidelines between science and pseudo-science. It is clearly shown that different auxiliary methods are adopted by realist and anti-realist scientists: the construction of the quantum formalism in the 1920s, which is mentioned at several points throughout the thesis, is used as the primary example.

The final chapter consists of the conclusions, which are as follows: (i) Scientific realists who are also practising scientists should be metaphysical realists of a Lowean variety, and employ metaphysical analysis in order to delimit ontological possibilities, before using experience (e.g. experiment) to choose between those possibilities; (ii) The question of whether there is support for scientific realism from the practice of modern scientists rests on whether there is really any legitimate distinction to be made between belief and acceptance; (iii) The semantic thesis of scientific realism is just as plausible as the metaphysical thesis of scientific realism, given that a practising scientist must behave *as if* both are true (viz. instrumentalism appears to be a highly dubious position).

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1

FORMULATING SCIENTIFIC REALISM

> What exactly is scientific realism? Naïvely stated, it is the view that the picture science gives us of the world is true, and that the entities postulated really exist... But that statement is too naïve; it attributes to the scientific realist the belief that today's scientific theories are (essentially) right.
> (van Fraassen [1976], p.250)

> This book is an attempt to defend scientific realism: the view that mature and genuinely successful scientific theories [many of today's] should be accepted as nearly true.
> (Psillos [1999], p.xiii)

INTRODUCTION

From a historical perspective, the doctrine of scientific realism is related to, although it should not be conflated *with*, the metaphysical realism to which many medieval philosophers subscribed. In common, both forms of realism make claims about the mind-independent existence of entities which are essentially unobservable; about '*suprasensible* entities that lie beyond the reach of human perception.'[1] However, whereas the entities of interest to the medieval realist were universals, viz. abstract entities such as 'redness', the contemporary (scientific) realist is, rather, concerned with the theoretical entities, such as the electron, that are employed in the theories of mature sciences.[2] Although it may seem tempting to claim that there is a stronger analogy between metaphysical realism and scientific realism (or, indeed, between nominalism and instrumentalism), as Rescher does, I think that this is a misleading move.[3] For it is neither logically inconsistent, nor ostensibly irrational, to be an anti-realist about abstracta, on the one hand, and a realist about theoretical entities as employed by science, on the other; the converse may also be true.[4]

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[1] Rescher [1987], p.xi

[2] The mature sciences to which I refer are physics, chemistry, and biology. I will avoid making any claims about social sciences, psychology included, because I doubt that scientific realism is applicable to them. Certainly, to contend that it is would be to put forward an unnecessarily strong thesis, logically speaking, for the empirical successes of social sciences have not, in my opinion, been unequivocally demonstrated.

[3] For example, he claims that conceptualism - 'universals are...mind-made all right, yet not arbitrarily, but under the guidance of certain natural predispositions of the mind' - is the analogue of approximationism - 'While the theoretical entities envisioned by natural science do not actually exist in the way current science claims them to be, science does (increasingly) have "the right general idea".' Rescher [1987], pp.xi-xii

[4] In the former case, one might argue that theoretical entities are *not* eliminable from predictively successful theoretical discourse, whereas many abstracta, such as universals, are. In the latter case, which is admittedly more problematic, one might suggest that the success of

In order to avoid unnecessary confusion, then, I shall present scientific realism as a stand-alone philosophical position. I will pay minimal heed to its precursors, insofar as they were precursors, and instead elucidate it *as is*. Since the purpose of this thesis is not to *defend* scientific realism, *per se*, but rather to examine which methodological views of science are compatible with it, this strategy seems the most direct. Before embarking upon it, however, I would draw the attention of the reader to one final *caveat*.

This is, as suggested by the conflicting quotations which head this chapter, that scientific realism is ecumenical, and has therefore often been misunderstood, through no fault of their own, by its critics.[5] As Leplin puts it: 'Like the Equal Rights Movement, scientific realism is a majority position whose advocates are so divided as to appear a minority.'[6] In light of this, I shall attempt to make my initial presentation of the position as logically weak as possible, so as to capture the common ground. In section 1.1, the 'hard-core' of scientific realism will be expressed in terms of three distinct theses or components, the metaphysical, the semantic, and the epistemic, which form the basis of consent. Thereafter, in section 1.2, I will discuss an additional component, the aspirational thesis proposed by some, and explain why I believe its inclusion, at least as an axiom, to be ill-advised. Finally, in section 1.3, I will provide an outline of the issues on which there is room for legitimate disagreement among scientific realists. Unavoidably, this discussion about the possible consequences of the 'hard-core' will be less of an exposition, and more of an exploration; hence the conclusions drawn therein should not be seen as representative of scientific realists in general.

1.1 THE 'HARD CORE' OF SCIENTIFIC REALISM

Simply put, the scientific realist views science as a search for truth: as a process of discovery and explanation. The instrumentalist, on the other hand, views theories as simple rules for calculation, which should be accepted only on the basis that they allow us to make phenomenological predictions. Whereas the instrumentalist would think it foolhardy to speculate on the nature of unobservable entities or mechanisms that underlie the act of prediction, the realist believes that science can and should, at least to a certain extent, identify and investigate these suprasensibles. Even at this early stage

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theoretical discourse in science is based directly upon the reality of *numbers* and their relationships (a form of structural realism), rather than that of theoretical entities. Perhaps it might be claimed that talk of numbers that relate phenomena must be couched in talk of theoretical entities?

[5] What Feyerabend calls 'scientific realism', for example, bears little similarity to the contemporary (mainstream) position that is elucidated by those such as Devitt [1984] and Psillos [1999]. See Feyerabend [1981], chapters 1 and 2.

[6] Leplin [1984], p.1

of the presentation, this draws our attention to two important questions which are key to the overall theme of this thesis: (i) Can either belief-system, at least ostensibly, be adopted by real, successful, scientists?; (ii) If yes to (i), will scientists who adopt one system proceed differently than those who adopt the other? I will, therefore, make a brief stab at answering both these questions in the affirmative, before discussing scientific realism in any further depth.[7]

Scientific Realism vs. Instrumentalism – Two Illustrative Episodes

Although it is prudent to avoid drawing general lessons from specific historical episodes, there is one particular area of physics which would seem to suggest that scientists can be either realists, or instrumentalists, and still succeed in some measure of predictive success. Specifically, I refer to quantum mechanics; one only needs to look to the declarations of the central figures in its development, during the late 1920s, in order to appreciate that they had very different ideas about the proper role of physical theories.[8] Bohr, for example, wrote:

'There is no quantum world. There is only abstract quantum physical description. It is wrong to think that the task of physics is to find out how nature *is*. Physics concerns what we can say about nature.'[9]

'What we can say about nature', here, is presumably just what we can say *predictively* about macroscopic events, involving observables (or, to be more precise, sensibles). This reading is supported by Bohr's statement that:

'The entire quantum formalism is to be considered as a tool for deriving predictions.'[10]

Squarely opposed to what he saw as this 'shut up and calculate' approach to physics, however, was Einstein:

'1930. Physics is the attempt at the conceptual construction of a model of the *real world*, as well as its lawful structure'[11]

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[7] Primarily, this is in order to explain why the issue treated by this thesis is interesting; how, that is, it relates to science as a real activity.

[8] The instrumentalists were the younger group of physicists: Bohr, Heisenberg, Born, and Pauli. The realists were the older group: Einstein, Schrödinger, and de Broglie. See Cushing [1994], chapter 7.

[9] Bohr, quoted in Squires [1994], pp.117-118

[10] Bohr, N. *Dialectica* **2**, 312 (1948)

[11] Einstein, quoted in Fine [1996], p.97. With concession to Fine's argument, I do not want to contend that Einstein would have been *fully* committed to the position of scientific realism. I make only the weaker claim, that Einstein advocated a realist *programme* of research, as a valuable, or perhaps even indispensable, heuristic *for* physics.

So, unnecessary technicalities aside, it seems eminently clear that these two physicists, as representative of their opposing schools of thought (see footnote 8), approached the task of theory-construction from radically different viewpoints.[12] Indeed, this is borne out by the fact that each school produced its own formalism. On behalf of the instrumentalist school, Heisenberg proffered his matrix-mechanics to the physics community. On behalf of the realist school, Schrödinger tendered his wave-mechanics. However, what is somewhat fascinating is that both these formalisms were subsequently shown to be empirically equivalent! To this very day, both formalisms are used interchangeably for certain calculations in non-relativistic quantum mechanics.[13] So, in short, it would seem that the answer to both (i) - can either belief-system, at least ostensibly, be adopted by real, successful, scientists? - and (ii) - will scientists who adopt one system proceed differently than those who adopt the other? - is a resounding "yes". This said, though, one might claim that the answer to (ii) is not an important methodological concern, since the *end results* of both approaches, empirically speaking, were indistinguishable. To this empirical congruence, I will concede for the moment. Nevertheless, I shall offer one further example, building upon the previous discussion, in order to show that the results of the two approaches may differ in other important respects.

At the Solvay Conference of 1927, after the empirical equivalence of the differing formalisms had been established, the two competing schools came together in order to discuss how best to interpret the mathematical backbone of quantum mechanics. The most prominent realist idea was the 'pilot-wave' interpretation put forward by de Broglie, but this was thought (erroneously) to be untenable, after a technical objection from Pauli.[14] The instrumentalist school carried the day, and the 'Copenhagen Interpretation' was born. Indeed, it was not until twenty-five years later, when David Bohm realised that Pauli's objection was spurious, that the pilot (or guiding) wave idea was re-examined. His two papers, published in 1952, showed how de Broglie's basic idea *could* be employed, provided that non-locality was explicitly accepted in the process.[15]

Now what we might ask, technicalities again avoided, is *why* Bohm went back to de Broglie's old idea, when quantum mechanics had been such a great empirical success? The answer, in his own words, is simple:

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[12] Zahar, though, is of the opinion that one would do better to examine what scientists actually *do*, rather than just what they say. I do so in this sub-section, but only to a very limited extent; deeper analysis will not be appropriate until later. See Zahar [1989], pp.3-4

[13] The Schrödinger wave equation is typically preferred in most cases, except when calculating spin-states.

[14] See Cushing [1994], pp.118-121

[15] Bohm, D. *Physical Review* **85**, 166 (1952) & Ibid. 180 (1952)

'What I felt to be especially unsatisfactory was the fact that the quantum theory had no place in it for an adequate notion of an independent actuality...the theory could not go beyond the phenomena or appearances.'[16]

Bohm was a realist, and this led him to adopt a different method to that which is prevalent in modern physics. Rather than purely engaging in experimental or theoretical work at the 'cutting-edge' of the discipline, he questioned a widely-accepted theory which had been (and still is) empirically adequate. As Kuhn put it:

'given a theory which permits normal science...scientists need not engage the puzzles it supplies...they could instead seek potential weak spots...Most of my present critics [Popper, Watkins, and other such 'rationalists'] believe they should do so. I disagree but *exclusively on strategic grounds.*'[17] [emphasis mine]

Now what this seems to suggest is that there may be genuine links between realist thought and motivation, and the *critical* approach to science which is prescribed by Popper, among others. Nevertheless, to make the claim that there is anything more than a *suggestion*, here, is too quick. Why? Because Bohm's theory makes no empirical predictions which differ from its (Copenhagen) predecessor. Whereas Popper's methodology is supposed to drive science forward, in terms of empirical success (*inter alia*), Bohm's theory was, *prima facie*, predictively sterile.

All that Bohm's theory *does* do, it seems, is to express quantum mechanics in a fashion that is more ontologically consistent with, and explanatorily amenable to, theories in other domains of physics. In the words of Chang:

'If we are concerned with the goal of understanding or explanation that teaches beyond the making and testing of predictions, it becomes crucial to eliminate conceptual contradictions. In the case of quantum physics the conceptual contradictions largely remained unresolved, and in this sense its success was not so great. The common opinion that quantum physics was a spectacular success results from the value system prevalent in contemporary science, which gives more weight to prediction than to understanding.'[18]

To the instrumentalist, then, the Bohmian theory might be seen as preferable on purely *pragmatic* grounds; it might be said to 'tell a more plausible story'.[19] But she might also claim that scientists should not be concerned with mere storytelling, because new empirical successes, which Bohm's theory does *not* provide, are an important goal for science by any standards! Does the realist have a good counter? Can it be cogently argued that realist methods *do* lead

[16] Bohm [1987], p.33

[17] Kuhn [1970], p.246.

[18] Chang [1995], p.135

[19] There are other options. Instead, some anti-realists reject the importance of explanation completely (conventionalists such as Duhem); others say that two predictively equivalent theories are always explanatorily equivalent (i.e. in the 'covering-law' sense due to Dray, and subsequently adopted by Hempel in his deductive-nomological and inductive-statistical models of explanation.) These positions are discussed later in this section.

to more empirical success than instrumentalist ones? In order to answer such questions, we will first need to explore scientific realism in detail.

The Metaphysical Thesis

Thesis M - There exists a world that is external to humans, which is populated by entities that have objective and mind-independent existence.

Thesis M has two important dimensions. First, it says that there are entities which exist quite independently of what we humans believe, conceive of, or can even recognise; they are objective in the sense that they are not '*constituted by* our knowledge.'[20] This is *not* to say that such entities are epistemically inaccessible; this thesis, taken alone, allows for objective entities which are either sensible or insensible, either identifiable or unidentifiable.[21] In short, ontological and epistemological concerns, as they relate to the external entities posited by thesis M, are to be kept strictly separate.[22] This dimension, henceforth referred to as dimension O (Objectivity), is thus explicitly opposed to modern verificationist accounts, specifically those of Dummett and Putnam, 'which reduce the content of the world to whatever gets licensed by a set of epistemic practices and conditions.'[23]

Second, thesis M says that both the world which it posits, and the entities which inhabit it, are not constituted by the mental (as distinct from our *knowledge*). Against Leibniz, the world does not just consist of many minds. Against Berkeley, an entity (even a sensible one) does not consist of a bundle of 'ideas', viz. sense-data; *esse* is most certainly not *percipi*. One vulgar way of expressing this dimension, henceforth dimension I (Independence), would be to say that, were all minds to be suddenly extinguished, there would still be many entities left in existence.

Of course, thesis M is seen by most, philosophers and laymen alike, as vapid, if not completely 'obvious'. Rescher makes the case that such a world-view is a *necessary* presupposition for any form of experiential inquiry; it is a 'postulation made on *functional* rather than *evidential* grounds.'[24] It does seem plausible, as he claims, that one must accept thesis M in order to practice science in the first place; for science cannot, and does not, provide any unequivocal evidence *for* thesis M. Moreover, it seems equally plausible that, without accepting thesis M, there would be no reason why one should want to communicate with another. If I were crossing a road, and was almost

[20] Devitt [1984], p.15

[21] Indeed, it is permissible to accept thesis M and believe that both types of objective entities (accessible and inaccessible, epistemically speaking) inhabit the world.

[22] The link between the two is established, for the realist, in the subsequent theses.

[23] Psillos [1999], p.xix

[24] Rescher [1987], p.126

struck by a car (a near-collision, rather than a near-miss!), I might well ask bystanders to describe the car. Now if I had thought it blue, but I spoke to a large number of people who said that it was, instead, green, I would usually do well to believe them. But why would I want to, if I did not accept that there really was an external object (the car), about which some of my beliefs might be incorrect? As Rescher points out, to reject thesis M is to preclude inquiry-related discourse:

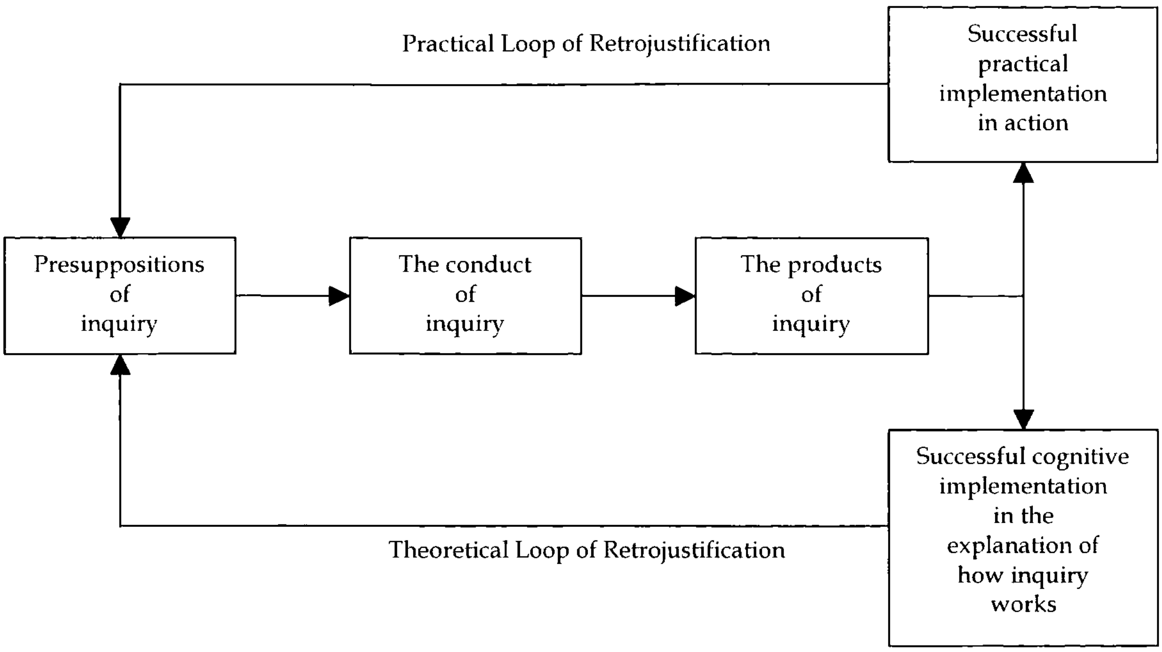
'It is crucial to the communicative enterprise to take the egocentrism-avoiding stance that rejects all claims to a privileged status for *our own* conception of things. In the interests of this stance we are prepared to "discount any misconceptions" (our own included) about things over a very wide range indeed...[instead] we are committed to the stance that factual disagreements as to the character of things are communicatively irrelevant within very broad limits. The incorrectness of conceptions is venial.'[25]

All this, unfortunately, is still not enough to convince the sceptic. For, even if one accepts that we must all behave *as if* a thesis is true, this is not to say that one should commit to the belief that it *is* true. It is quite ironic that Rescher makes this point most clearly when he is arguing *for* thesis M as a natural presupposition, by stating: 'The utility of the conception of reality is such that even if reality were not there, we would have to invent it.'[26] This just begs the question, and the eager sceptic will simply ask, "Well, haven't we?" Unfortunately, as we shall see, providing a cogent answer to the negative is as problematic today as it was in the time of the Ancient Greeks.

Rescher's strategy is to claim that the successes of our inquiry-related discourse provide a type of retro-justification for thesis M. He claims there are two 'loops' of retro-justification, the 'practical' and the 'theoretical', as depicted below: [27]

[25] Ibid., p.135
[26] Ibid., p.140
[27] Figure from Rescher [1987], p.144

FIG. 1



So the benefit of hindsight about our presuppositions supposedly 'confirms' them. On the one hand, they are 'confirmed' by the fact that their adoption has led to practical success, whereas on the other hand they are 'confirmed' by the role that they play in the explanation of inquiry. Rescher acknowledges the *obvious* circularity, and wants to have it that is not vicious:

'The pragmatic turn does crucially important work here in putting at our disposal a style of justificatory argumentation that manages to be cyclical without vitiating circularity.'[28]

However, I believe that his argument fails when inferential concerns are taken into account. First, let us note that Rescher fails to grasp the nettle of scepticism firmly, because a sceptic about thesis M is most certainly going to be a sceptic about induction. Let us imagine an individual struggling with a Cartesian demon. To him, it will seem that to believe in retro-justification is just to succumb to the illusion; the ephemeral 'successes' resulting from his intuitive method of empirical inquiry are just the product of an evil monster, attempting to lure him into a false sense of security. Frankly, to suggest that this individual would adopt Rescher's complex philosophy, rather than limit himself to the Cogito, is just absurd. Once the Cogito is found, then *acceptance* of the way that things appear is pragmatically efficacious, but in no way necessitates attribution of *beliefs* about any putative 'veiled reality'. Since the pragmatic justification of induction still allows for scepticism, as its proponents candidly admit, Rescher could not adopt it and still claim his

[28] Ibid., p.142

argument was sceptic-proof.[29] Moreover, to claim that nothing like induction is ever necessary is to renounce any claims about confirmation whatsoever (even about singular statements). In the words of Popper:

'The best we can say of a hypothesis is that up to now it has been able to show its worth, and that it has been more successful than other hypotheses although, in principle, it can never be justified, verified, or even shown to be probable.'[30]

The sceptic would, then, need some way of determining that induction is justifiable *a priori*, in order to accept any further claims about a mind-independent reality that might, for all he knows, not even *exist*.

Unfortunately, even if we do try to attach an *a priori* justification of induction to explain the validity of retro-justification (following the principle of charity, we might assume that Rescher would have it so), we then run into another brick wall. Specifically, we encounter a *hidden* circularity, because it is quite clear that induction *cannot* be so justified without recourse to the validity of thesis M (in particular, dimension O thereof). Bonjour's conclusion, when considering how we can justify knowledge of the future, based on experience of the past, is that:

'[an a priori] solution to the problem of induction depends on the tenability of a non-Humean, metaphysically robust conception of objective regularity (or objective necessary connection).'[31]

Without a strong metaphysical underpinning for the argument, then, any attempt to avoid entertaining scepticism about thesis M (or indeed, about induction) is going to prove fruitless. We need to show that, metaphysically speaking, it is just *impossible* for thesis M not to obtain. It is important to recognise, as is suggested by the title of Bonjour's book, that such a project is not feasible if we think of metaphysics in Kantian (or Neo-Kantian) terms, because its given role, in such a system, is too narrow:

'space and time, as the necessary conditions of all (outer and inner) experience, are merely subjective conditions of all our intuition, in relation to which therefore all objects are mere appearances...much may be said *a priori* that concerns their form, but nothing whatsoever about the things in themselves that may ground them.'[32]

In fact, recent work on transcendental arguments has suggested that they can only provide theses with *invulnerability* from scepticism, viz. show that one should not be concerned about the challenge posed by the sceptic. According to Hookway:

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[29] The proponents that I refer to are Reichenbach and Salmon. For an excellent outline of their position, see Bonjour [1998], pp.192-196

[30] As will be discussed in section 1.2, this is a serious problem for anti-inductivist 'realism' about science. Quotation from Popper [1980], p.315

[31] Bonjour [1998], pp.214-215

[32] Kant [1997], A49, p.188

'The 'logical possibility' which the modest argument fails to eliminate is dismissed as irrelevant once we take note of the uses to which the transcendental argument is put. Thus the argument is not irrelevant to the belief's being justified: *but it does not originate that justification;* it merely undermines a challenge to it.'[33] [emphasis mine]

Transcendental arguments are not sufficient to *prove* a thesis to be true (in the sense of correspondence), and we might, therefore, be left wondering, "What other options are left?" Gladly, I think that there is one possibility that remains, and it is 'The Possibility of Metaphysics' in Lowe's sense:

'it is possible to achieve reasonable answers to questions concerning the fundamental structure of reality - questions more fundamental than any that can be competently addressed by empirical science. But I do not claim that metaphysics *on its own* can, in general, tell us what *there is*. Rather - to a first approximation - I hold that metaphysics by itself only tells us what there could be. But given that metaphysics has told us this, experience can then tell us which of various alternative metaphysical possibilities is plausibly true in actuality.'[34]

If such a view of metaphysics can be defended, then the correct method of proving thesis M would be to demonstrate that any metaphysical system that attempts to exclude it is simply not sustainable.[35] I shall not attempt to do so here, however, and for two distinct reasons. First, because this would involve setting out a list of criteria for distinguishing between metaphysical systems that allow for different possibilities - this is quite outside the scope of this section. Second, because such a quest would result in a lengthy thesis in its own right.

All I want to *suggest* is that Lowe's take on metaphysics promises a chance of cogently defending thesis M, whereas all other attempts to do so have failed. The importance of this, in context of this thesis as a whole, will become apparent in Section 3.2, where I will discuss the role that metaphysics could play in a realist methodology of science. For the moment, though, I would ask the reader to turn his attention back to the exposition of scientific realism.

The Semantic Thesis

Thesis S - The terms in scientific theories, be they observational or theoretical, have putative reference.

Thesis S says that the terms in a scientific theory refer to entities that are theory-independent. In particular, the emphasis is upon theoretical terms, such as gravitational force, rather than observational terms, such as the

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[33] Hookway [1999], p.186

[34] Lowe [1998], p.9

[35] Illustrating that dimension O is ineliminable, even in the near-nihilist limit where one could posit that only the empty-set existed (in addition to human minds), is not too problematic. However, sustaining that dimension I is ineliminable would be far more difficult.

Young's modulus of an iron bar. When coupled with thesis M, and a correspondence-theory of truth, the conclusion drawn from thesis S is that a true theory will pick out mind-independent entities that *exist*. An extended example will help to clarify this point, by showing how putative properties and putative particulars are interrelated. Let us imagine that Maxwell's equations, here simplified to apply to the case of electric and magnetic fields in a vacuum, are all *true*:

(1) $\nabla \cdot \mathbf{E} = 0$ (from Coulomb's Law)

(2) $\nabla \cdot \mathbf{B} = 0$ (from Gauss's Law)

(3) $\nabla \times \mathbf{B} - \mu_0 \varepsilon_0 \partial \mathbf{E}/\partial t = 0$ (from Ampere's modified Law[36])

(4) $\nabla \times \mathbf{E} + \partial \mathbf{B}/\partial t = 0$ (from Faraday's Law of Induction)

So (and I emphasise, this is on the assumption that the laws are *true*) we have two real particulars, viz. electric field, **E**, and magnetic field, **B**. We also have two real properties of free space, viz. its permittivity, ε_0, and its permeability, μ_0. Now, only elementary mathematics is necessary to elucidate their relationships:

We take the curl of (4), using the vector relationship,

$\nabla \times (\nabla \times \mathbf{A}) = \nabla(\nabla \cdot \mathbf{A}) - \nabla^2\mathbf{A}$

and obtain,

(5) $\nabla(\nabla \cdot \mathbf{E}) + \nabla \times \partial \mathbf{B}/\partial t = \nabla^2\mathbf{E}$

Then, substituting (1) into (5), and reversing the operators on **B** (which are mathematically interchangeable), we arrive at,

$\partial/\partial t\, (\nabla \times \mathbf{B}) = \nabla^2\mathbf{E}$

and may substitute from (3) to find that,

(6) $\nabla^2\mathbf{E} = \mu_0 \varepsilon_0 \partial^2\mathbf{E}/\partial t^2$

Indeed, analogously, by taking the curl of (3), using the same vector relationship as above, and substituting from (2) and (4), we find the symmetrical equation,

[36] The initial version of Ampere's law would have read $\nabla \times \mathbf{B} = 0$ in this case, but Maxwell modified it to include a displacement current. Notice the symmetry between 3 & 4.

(7) $\nabla^2\mathbf{B} = \mu_0 \varepsilon_0 \partial^2\mathbf{B}/\partial t^2$

So we note that both the electric field and the magnetic field obey the classical wave equation, which is just:

$\nabla^2\mathbf{A} = (1/c^2)\, \partial^2\mathbf{A}/\partial t^2$ where c is the speed of the wave

Thus it becomes clear that for **E** and **B** to exist (alone) in a vacuum they must be bound together in the form of a self-propagating wave.[37] This 'electromagnetic wave' will be transverse, with **E** and **B** both perpendicular to the direction of propagation (as suggested by Fleming's left-hand-rule), and will travel with a definite speed of,

$c = 1/\sqrt{(\mu_0 \varepsilon_0)}$

Now what is interesting is that we have 'picked out' a new 'thing', a bundle of **E** and **B**, which deserves the status of a particular in its own right. Its **E** cannot exist without **B**, and vice versa:

> 'Suppose the magnetic field were to disappear. There would be a changing magnetic field which would produce an electric field. If this electric field tries to go away, the changing electric field would create a magnetic field back again. So by a perpetual interplay - by the swishing back and forth from one field to the other - they must go on forever... They maintain themselves in a kind of dance - one making the other, the second making the first - propagating onward through space.'[38]

In less elegant terms, the electromagnetic wave is *indivisible*, so should be considered as a fundamental building block of the world, even though it is a composite of **E** and **B**. It is *primitive* in a sense that a water wave, or a wave on a string, is not. But what of its speed, c? Should this be considered as a *primitive*, viz. metaphysically basic, property of the e-m wave itself? Surely not, for it is the initial properties of free space itself (or in the more general case, the medium through which the e-m wave is moving) that define it.[39] Let us remember that c is not a variable (as velocity for an object with mass is), it is rather a *constant* that relates the energy and momentum of a photon, as follows:

Energy = p c (from the equation for total energy of an entity[40])

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[37] For derivation of plane-wave, see Feynman [1964], pp.18-4 to 18-8.

[38] Ibid., p.18-8

[39] A charge of circularity may be levelled at my presentation, since I assumed that the permittivity and permeability of free space were real initially. With the benefit of hindsight, of course, we might want to say that c is *primitive*, whereas one of μ_0 or ε_0 are not; either way, we still end up with only *two* primitive properties. (Note that Maxwell first worked with just c and ε_0, interestingly enough.)

[40] Specifically, $E^2 = (p\,c)^2 + m_0^2 c^4$. $E = p\,c$ is the special case for a body with no rest mass. The other derivative equation is for a body with no momentum - the infamous mass-energy equivalence equation, $E = m_0 c^2$.

The important lesson, here, is that *some theoretical talk of properties may be reducible to talk of a smaller number of real primitive properties*, but still adopted for convenience; thesis S allows for this. (Notice footnote 39, and see my later discussion in the next sub-section, 'The Epistemic Thesis'.)

Adopting thesis S sets the scientific realist apart, first, from reductive empiricists. Talk of theoretical terms is not just taken to be *disguised* talk of observational terms, as one who adopted a verification criterion of meaning would want to have it.[41] The work of the early Carnap is particularly important in this area, for his attempt to show that theoretical discourse could be rephrased in terms of purely observational discourse was a failure. I shall not cover his argument in detail, here, for it is already treated extensively elsewhere.[42] It should suffice, instead, to note that Carnap finally conceded that theoretical terms were necessary for successful scientific theories:

'1. Without using theoretical terms 'it is not possible to arrive...at a powerful and efficacious system of laws'...'this is an empirical fact, not a logical necessity'...

2. Scientific theories do formulate comprehensive laws with the help of theoretical terms.

Therefore, theoretical terms are indispensable.'[43]

However, this said, there is still another position that can be adopted to avoid endorsing thesis S; that of the instrumentalist. While it might well be the case that scientific theories, if viewed as genuinely assertive about the world, must be seen to make putative reference to theoretical (as well as observational) entities, the instrumentalist would just deny that theories do make such assertions. Instead, he would claim that the theories of science are just means by which we give structure to phenomena which might, otherwise, appear to be unrelated. They are merely constructs, designed for 'organising experience...and for guiding further experimental investigation.'[44] The instrumentalist essentially believes that observational terms are still epistemically *privileged*, even though he rejects the verification criterion of meaning. There are, broadly speaking, two 'churches' of instrumentalism; on the one hand, the conventionalist, and on the other, the eliminativist.

The conventionalist, such as Duhem, would argue that science can proceed *successfully* irrespective of whether there is a reality underlying the phenomena, or whether scientists believe that there is. That is, albeit that theoretical discourse may be ineliminable from scientific theories. Simply

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[41] If statements containing theoretical terms were unverifiable, then they would not be meaningful from a logical positivist perspective.
[42] Psillos [1999], pp.3-10
[43] Ibid., pp.10-11, with some quotation from Carnap himself.
[44] Ibid., referring to Nagel's work in the 1950s, p.17

put, these are matters with which the scientist, and therefore science, *need not be concerned*:

'The aim of theory is to classify experimental laws...In classifying a group of experimental laws, physical theory teaches us absolutely nothing about the foundation for those laws and the nature of the phenomena they govern...physical theories and metaphysical truths are independent of one another.'[45]

The conventionalist may even, therefore, wholeheartedly endorse thesis M, but remain quite agnostic about whether theoretical terms in theories are really referential to entities that are inaccessible to the senses. Niels Bohr's position would be best characterised as being within the conventionalist 'camp', since he believed in a deeper-structure to the world, but simply maintained that our intuitive (classical) conceptualisation thereof was too limited to make sense of it:

'The quantum theory is characterized by the acknowledgement of a fundamental limitation of the classical ideas when applied to atomic phenomena. The situation thus created is of a peculiar nature, since our interpretation of the experimental material rests essentially on the classical concepts.'[46]

In particular, Bohr's doctrine of complementarity reflects this concern; in his eyes, one could speak of a quantum entity as 'wave' or as 'particle', but these two concepts were *mutually exclusive*.[47] In a similar way, for any non-commuting variables, viz. properties such as spin on the x and y axes of a silver atom, there may well be something *real* underneath our picture, but we cannot elucidate it because we are bound to use models that are derived from our *macroscopic* experience.

At the root of the conventionalist position, that 'A physical theory is not an explanation'[48] (Duhem), and that 'It is wrong to think that the task of physics is to find out how nature *is*'[49] (Bohr), is a historical-epistemic argument. Since the history of science clearly shows that many explanations provided by previous theories were wrong, the conventionalist wants to maintain that the empirical progress of science is based only upon the fact that a successor theory carries over some of the *structural* aspects of its forerunners. In short, the conventionalist claims that it is developments in *formalism* ('the representative part' of theories), rather than *interpretation* ('the explanatory part' of theories), that really drive science ahead.[50] This idea is at the core of the *pessimistic meta-induction*, a powerful argument against scientific realism, which attempts to sever the connection between empirical success and the

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[45] Duhem [1996], pp.36-37

[46] Bohr, N. Address to the International Congress of Physics at Como, Italy, on 16th September 1927.

[47] See Fine [1996], on Bohr's belief in *semantic* disturbance due to measurement, pp.34-45

[48] Duhem [1954], p.19

[49] Bohr, quoted in Squires [1994], pp.117-118

[50] For the distinction between explanatory and representative parts, see Duhem [1954], pp.31-39

truth-like representation of the world's deep-structure. How the realist attempts to establish and defend this connection is also vital for the epistemic thesis of scientific realism (thesis E), so discussion of this concern will therefore be postponed until the next sub-section. For the moment, let us turn our attention to the other 'church' of instrumentalism, namely eliminativism, and see how semantic realists argue against it.

The early eliminativists, such as Mach, claimed that there is simply nothing other than experience to represent; thesis M, as it relates to suprasensibles, viz. theoretical entities, was considered to be incorrect. According to Mach, posited entities such as atoms are afforded properties which have no bearing upon the way that we would expect bodies to be, from our macroscopic experience: 'with properties that absolutely contradict the attributes hitherto observed in bodies'.[51] Notice the similarity, here, with Bohr's aforementioned point. The difference is that, whereas Bohr might believe in things like atoms, which unfortunately have properties that we can never hope to identify (due to our inability to mentally 'step outside' our macroscopic prejudices), Mach thought that such entities could not exist, because real things are simply not of that sort. In short, for Mach, our macroscopic experiences are *not* just prejudices; they constitute privileged knowledge of the way that things must be. We should endeavour to speak of unobservable properties if, and only if, they are extrapolated from observable properties:

'The properties of atoms are formed in a way discontinuous with the properties observed in the phenomena. In positing atoms, the principle of continuity is violated. For Mach this means that 'the mental artifice atom' is suspect: something, perhaps, to be used provisionally, but disposed of ultimately.'[52]

Mach's view on atoms is generally thought to have been proven unsustainable, after Perrin, in his *Les Atomes*, showed how Avogadro's number (the number of atoms in one mole of a gas) could be determined, to a high degree of agreement, by no less than thirteen different experiments. As Psillos points out, the simple argument that *'we can count them, therefore they exist'* was enough to convert Poincaré, who was previously one of Mach's more notable sympathisers, to atomism.[53]

However, although Mach's attempt to dismiss thesis M (with respect to unobservables) was unconvincing, it did pave the way for later developments in eliminativism. For, in recognition that it was problematic to advocate theoretical discourse as an economic necessity, while simultaneously maintaining that it did not imply the existence of unobservables, Craig

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[51] Mach, quoted in Psillos [1999], p.20

[52] His 'principle of continuity' does important work in avoiding idealism of the Berkelian sort; Mach can endorse the idea of an unobservable vibration in a stiff rod, for example, since we can imagine such vibrations becoming incrementally smaller and reaching a stage at which they *become* insensible. Quotation from Psillos [1999], p.20

[53] Psillos [1999], p.22

adopted a different strategy. Specifically, he put forward a theorem which demonstrated that theoretical discourse can be completely eliminated from a scientific theory, T, not by reducing it to talk to observables (the failed Carnapian scheme), but rather by forming a new theory, Craig(T). Formal logic aside, Craig's theorem demonstrates that, for any theory T, employing both theoretical and observational terms, there exists a theory Craig(T) which uses only the observational terms in T, yet is *functionally isomorphic* with T.[54] Functionally isomorphic, that is, in the sense that all the deductive consequences of T that can be expressed in observational language are *also* deductive consequences of Craig(T). This poses what Hempel called the '*paradox of theorizing*':

> 'if the terms and principles of a theory serve their purpose, i.e., if they establish definite connections among observable phenomena, then they can be dispensed with since any chain of laws and interpretative statements establishing such a connection should then be replacable by a law which directly links observational antecedents to observational consequents.'[55]

Then, by adding two uncontroversial statements - (i) If the terms and principles of a theory do not serve their purpose, then they can surely be dispensed with; (ii) For any theory, its terms and principles either serve their purpose or do not – Hempel concludes that the terms and principles of *any* theory must, therefore, be dispensable. *Prima facie*, this 'Theoretician's Dilemma', as Hempel christened it, seems fatal for thesis S: 'Craig's theorem guarantees that if we deem a class of terms 'unwanted', then it can be dispensed with.'[56]

This said, though, the semantic realist would want to make the case that theories not only establish deductive connections, but also *inductive* ones. And theoretical terms *are* 'wanted' precisely because they are responsible for making the *inductive* connections between observable terms. Consider the following:

i) A theoretical hypothesis, H, deductively entails a group of observational consequences, $\{O_1 \ldots O_n\}$.

ii) Another theoretical hypothesis, J, deductively entails a different group of observational consequences, $\{P_1 \ldots P_n\}$.

iii) When H and J are conjoined, a novel (testable) group of observational consequences, $\{Q_1 \ldots Q_n\}$ is predicted. These predictions, after appropriate experimental investigation, are found to be correct.

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[54] See Craig [1956]: There are problems for Craigian theories - for example, that the number of axioms in a Craigian system is infinite – that are not discussed here, because it is the *logical validity* (rather than practical applicability) of Craig's theorem that is relevant in this context.
[55] Hempel [1965], p.186
[56] Psillos [1999], p.25

Now what is important to note is that $\{O_1 \ldots O_n\}$ and $\{P_1 \ldots P_n\}$ alone *cannot* lead to a prediction of the novel observational consequences, $\{Q_1 \ldots Q_n\}$. Rather, it is necessary to inductively infer that both H and J hold, because $\{O_1 \ldots O_n\}$ and $\{P_1 \ldots P_n\}$ both obtain (in their respective domains), before any attempt to conjoin could be considered worthwhile. H and J are, thus, quite indispensable in making a connection between $\{O_1 \ldots O_n\}$ and $\{P_1 \ldots P_n\}$, on the one hand, and $\{Q_1 \ldots Q_n\}$, on the other. In this context, Craig(H) and Craig(J) are *predictively sterile*. It seems fitting to leave the final word on the Theoretician's Dilemma to Hempel:

'if it is recognized that a satisfactory theory should provide possibilities also for inductive explanatory and predictive use and that it should achieve systematic economy and heuristic fertility [see footnote 54], then it is clear that theoretical formulations can not be replaced by expressions in terms of observables only; the theoretician's dilemma, with its conclusion to the contrary, is seen to rest on a false premise [that theories should only provide deductive connections between observables]'[57]

The Epistemic Thesis

Thesis E - Scientific theories, through their predictive empirical successes, can achieve degrees of confirmation. A well-confirmed theory, in a well-established domain of mature science, is *approximately true.*

Thesis E says that most of our current scientific theories, those that have shown themselves to be empirically successful in terms of accuracy and scope, are essentially right. Given theses M and S, thesis E thus means that, in the eyes of the scientific realist, the theoretical entities employed by the aforementioned theories are *real*, and actually do inhabit the (mind-independent, objective) world. As an example, let us take the electron, which plays a central role in the predictive successes of nuclear physics (beta decay), statistical thermodynamics (Fermi-Dirac statistics), solid state physics (conductivity, heat capacity, optical behaviour), quantum chemistry (valency and periodicity), and even stellar astrophysics (degeneracy pressure). For the scientific realist, this means that the electron must be just as real as planets, stars, or even the keyboard on which I type. Moreover, it means that the properties of the electron which play a role in the formalisms of science, such as charge, mass, spin, and wavelength, must also be *approximately* correct. For, if 'spin' does not refer to something *about* the electron, or at least the electron-world system, then why would the use of such a property, say in accounting for the inert chemical nature of helium, lead to such great predictive power?

Of course, the caveat mentioned towards the beginning of the previous sub-section, that some talk of 'properties' may ultimately be found to be talk about

[57] Hempel [1965], p.222

other more fundamental (or *primitive*) properties, is one that the realist can happily accept. Indeed, although from a modern metaphysical perspective one might be inclined to think that the objects of everyday experience do not really possess a fundamental property of 'colour' - one might think, instead, that colour is a relationship between light, the media through which it moves, and the faculty of human perception - there is an important sense in which colour is still a 'property' of an object. If I speak of two books in front of me, one red, and one blue, then with the necessary *ceteris paribus* clauses (e.g. both books have white light shining on them), I am still picking out an important difference between those two books. In a more practical context, were I to be offered a salad containing brown lettuce leaves, then I would not partake of it, because I would inductively infer something important *about* those lettuce leaves, specifically that they were rotten. And just as to talk of a lettuce leaf being 'brown' is to say something about that leaf, to talk of an electron having '+1/2 spin on the x-axis' is to say something (although we might not be sure precisely what that something *is*) about that electron.

Central to the realist defence of thesis E is the notion that *abduction* (or inference to the best explanation) empowers one to choose which of a competing set of empirically equivalent theories is more likely to be true. For example, were I to wake up on Christmas Day and find that a package addressed to me had mysteriously appeared by my bedside, I might, in principle, posit a large number of theories to explain its appearance there. One such theory might be that Santa Claus came down the chimney during the night and deposited the package, whereas another might be that my loving parents put it there shortly after I went to sleep. And I would choose the latter, the realist would argue, just because it is the better of the two explanations for the phenomenon (better, in this case, on grounds of the theoretical virtues of simplicity and consistency).[58] In short, when Watson sycophantically congratulated Holmes for making a "brilliant deduction", he should, instead, have said "brilliant *abduction*". Following the principle of charity, though, it only seems fair to *abduce* that Conan Doyle was aware of this fact, but did not wish to make it appear that Watson was congratulating Holmes on a successful criminal action!

The main objections to thesis E have been made by those who accept theses M & S, but reject the notion that we can ever *know* of a theory that it is any more *true* than another. *True*, that is, in a correspondence sense; for those who reject thesis M, or indeed thesis S, are free to endorse thesis E in a rather trivial sense. As Hendry points out:

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[58] It is simpler, in an Ockham's razor sense, because I am not positing an extra entity, in this case Santa Claus, without necessity. It is more consistent because it fits in with what I know about the world, experientially; it might also be a good inductive inference, if my parents told me that they left the present that 'mysteriously' appeared by my bedside on Christmas Day the year before.

'There is...a sense in which the issue of whether theories can be confirmed as true arises in an interesting way *only* if a non-epistemic theory of truth is agreed upon...Putnam's acceptance of the epistemic component [Thesis E] is rendered *Pickwickian* on this view by its identification of truth with rational acceptability in the ideal limit of enquiry.'[59]

There are three broad types of objection to thesis E (made by those who endorse a 'non-epistemic theory of truth'): the critical rationalist, the historical-epistemic, and the constructive empiricist. The most distinct and striking position is that of the critical rationalist. He, following Popper, would argue that thesis E is incorrect because induction is unjustifiable (or, perhaps, because there is not even such a thing as 'inductive inference'). Yet curiously, he would still want to claim that the aim of science *is* truth. Now this is an interesting type of 'realism', at least *prima facie*, and is certainly worthy of further investigation. Unfortunately, it is one that is, as I hope to demonstrate in the next section (1.2), internally inconsistent and rationally unsustainable.[60] Here, I will only treat the other two types of objection, which both, in common, focus their attack on the realist's use of abduction.

From the historicist (or historical-epistemic) perspective, which has already been touched upon during the previous discussion of conventionalism, any methodology of science should be judged on the basis of a specific meta-methodological rule. Specifically, that "The better of two methodologies of science is that which shows the better fit with history."[61] And the historicist, such as Laudan, would claim (along related lines) that 'epistemic realism [the result of thesis E, given M & S], at least in some of its extant forms, is neither supported by, nor has it made sense of, much of the available historical evidence.'[62] By citing several previous theories in science, Laudan points out that a theory can enjoy a degree of both predictive and explanatory success, even when one of its central theoretical terms can be seen, with the benefit of hindsight, to have referred to literally *nothing*. Indeed, a scientific realist (committed to the belief that the current theories of chemistry and physics are approximately true) can hardly claim that there really *are* such things as caloric, phlogiston, and the electromagnetic aether![63] To avoid becoming embroiled in a detailed account of Laudan's argument, however, it is worth simply pointing to his central assumption that '*a realist would never want to say that a theory was approximately true if its central terms failed to refer.*'[64]

[59] Hendry [1995], p.58
[60] This said, an important lesson about the central role of induction, in *any* realist account of science, will be drawn from that discussion.
[61] For example, if a given methodology made a novel prediction about a specific historical episode in science (i.e. we searched through previously unexamined historical documents, such as the letters of scientists, and established its success), then that methodology, *itself*, would be seen to attain a degree of confirmation.
[62] Laudan [1981], p.1114
[63] Laudan's other examples of non-referential terms include 'the crystalline spheres of ancient and medieval astronomy' and 'the vital force...of physiology'. Laudan [1981], p.1126
[64] Ibid., p.1126

However, as Psillos points out in his case study of the caloric theory of heat, sometimes the laws which form a theory can be totally independent of the assumptions made on the grounds of a *supposedly* 'central' theoretical term.[65] Indeed, many scientists at the turn of the nineteenth century actually recognised that there just wasn't enough evidence available to choose between the caloric and dynamic theories of heat. Laplace and Lavoisier, for example, wrote:

'The conservation of the free heat, in simple mixtures of bodies, is...*independent of those hypotheses* [caloric or dynamic theories of heat]: this is generally admitted by the physicists, and we shall adopt it in the following researches.'[66] [emphasis mine]

So, the realist would argue, the caloric theory of heat was approximately true, in the sense that it captured the conservation of energy principle, even though one of its central terms was non-referential. Moreover, in terms of my formulation of thesis E, the realist might deny that the caloric theory was ever 'well-confirmed'; on the contrary, what *was* well-confirmed (as the scientists of the day recognised) was the *shared ground* between the dynamic and caloric theories. It should also be emphasised that it was never appropriate to use *abduction* to choose between the dynamic and caloric theories of heat, precisely because they were *not* empirically congruent (although it was clear that they dealt with different phenomena in the same domain).[67] That is to say, further experimentation was considered to be the most appropriate means by which to proceed.

On a higher level, I think that it is also fair to criticise the historicist approach by drawing attention to the questionable nature of its meta-methodology. In particular, I do not believe that there is *just* one 'methodology of science', one science-philosopher's stone, which can lay bare the inner workings of such an essentially human activity. Rather, I believe that science can be approached in many different ways, and that empirical success can be achieved, in varying degrees of efficacy, by adopting many *different* methods.[68] Popper, for one, agreed with this point:

''Normal' Science, in Kuhn's sense, exists...it must be taken into account by historians of science. That it is a phenomenon which I dislike (because I regard it as a danger to science) while he apparently does not dislike it (because he regards it as 'normal') is *another question...a very important one.*'[69] [emphasis mine]

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[65] Psillos [1999], ch.6

[66] Laplace and Lavoisier, quoted in Psillos [1999], p.118

[67] E.g. The caloric theory predicted an increase in weight (due to more caloric) as a body was heated, whereas the dynamic theory did not.

[68] As suggested towards the beginning of this chapter, under the heading '*Realism vs. Instrumentalism – Two Historical Episodes*', if this idea can not be defended then this Thesis is simply based upon a false premise.

[69] Popper [1970], p.52. Kuhn *also* appreciated that his disagreement with Popper was only made on '*strategic grounds*' (see footnote 17), so it seems that I am in good company (both logicist and historicist) in my views about methodology.

Of course, it *is* quite possible that there is some sort of 'core methodology', some bare set of methodological requirements that are necessary to do anything that would be recognisable as 'science', but this would be pretty thin (and, indeed, unlikely to be practically applicable, without *auxiliary methods*). And it is only in establishing such a 'core methodology', I would argue, that the historicist's meta-methodology would prove useful. However, my views on methodology will be presented in the next chapter (2), so I shall not dwell upon this point here.

As mentioned long ago, in a paragraph far, far away, the final objection to thesis E, critical rationalism aside (until 1.2), is made by the constructive empiricist. 'Constructive', as van Fraassen explains when he mints the term, because such an empiricist believes that the proper aim of science should be to *construct* theories that are empirically adequate, rather than to *discover* truths about mechanisms that underlie the phenomena.[70] Although it may seem curious, *prima facie*, van Fraassen's position is very close to that of the scientific realist, for he not only accepts theses M and S, but also most of thesis E; he would just substitute *'empirically adequate'* for *'approximately true'*. In his words:

> 'The distinction I have drawn between realism and anti-realism, in so far as it pertains to acceptance [of a theory], concerns only how much belief is involved therein.'[71]

His suggestion is that it is only *acceptance* of a theory that is important, in the context of science as a *practice*, because it is only necessary for a scientist to *accept* a theory (as empirically adequate) in order to work with it. Now this is clearly true to a certain extent, certainly if a scientist is using a theory in one of the 'puzzle-solving' senses that constitute Kuhnian 'normal science', but the realist would argue that this can not be the whole story.[72] In particular, if we take two theories in different domains that we only accept as empirically adequate, then why would we want to conjoin them?

If 'A is true' and 'B is true', then '(A & B) is true' is deductively entailed, but if 'A is *just* empirically adequate', and 'B is *just* empirically adequate', then there are no obvious grounds for asserting that '(A & B) is empirically adequate'. In recognition of this point, van Fraassen makes the claim that nothing like conjunction occurs (or at least, occurs often); instead, he suggests that 'Putting two theories together...[involves] correction.'[73] But this is surely insufficient, the realist rejoins, because correction occurs *prior* to the process of conjunction. If (A & B) fails, one does not just correct it to form (A & B)*. Instead, one corrects A {or B} and forms A* {or B*}, before *re-conjoining* it with

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[70] van Fraassen [1980], p.5
[71] Ibid., p.12
[72] See Kuhn [1996], chapter 3, and my later discussion in 2.2.
[73] van Fraassen [1980], p.84

B {or A}, and thus arriving at (A* & B) {or (A & B*)}.[74] Besides, the realist adds, simple conjunction (without *any* theoretical correction) happens regularly in science. For example, in making a typical prediction about the elemental composition of a star, via spectroscopic analysis, we just slam together laws from quantum mechanics, optics, electromagnetism, and even *electronics* (which is employed in the construction of modern instruments). Note that such a prediction will *always* be of the form 'it is 99.9% likely that star X contains element Y', rather than 'star X definitely contains element Y', since experimental (*not* theoretical) errors must always be taken into account. Thus a failure to make a successful prediction in any one isolated case is *never* sufficient to suggest that any *theoretical* corrections are necessary, and does not pose a serious threat to the realist line on this issue.

However, aside from offering a positive alternative to scientific realism, by the substitution of 'empirical adequacy' for 'approximate truth' in the statement of thesis E, van Fraassen also offers a criticism of abduction, with respect to theoretical entities. And it is this negative thesis, which turns on the old empiricist distinction between what is 'observable' and what is 'unobservable', which proves to be more cogent than his positive one. In presenting this criticism, I shall not follow van Fraassen directly - primarily because he *does* mix his positive and negative theses, and I believe that there is no logical reason to do so[75] - but shall, instead, suggest that there might be an internal inconsistency in the realist position.[76] My argument, for what I shall subsequently refer to as the *Abduction-Observation Tension* (or AOT), is as follows:

The scientific realist wants to sustain that:

A) Suprasensibles may be quite unlike sensibles, in many respects.

Else, the realist would have to concede that Mach's 'principle of continuity', mentioned in the discussion of thesis S, is essentially right.

But also wants to sustain that:

B) Abductive inferences allow one to select between empirically equivalent theories, on the basis of their relative verisimilitude (or nearness-to-the-truth).

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[74] See Psillos [1999], p.206 (Referring to work by Hooker.)
[75] See van Fraassen [1980], p.21: 'the premiss that we all follow the rule of inference to the best explanation when it comes to mice and other mundane matters - that premiss is shown wanting. It is not warranted by the evidence, because that evidence is not telling *for* the premiss *against* the **alternative hypothesis I proposed**, which is relevant in this context.' [emphasis mine]
[76] The *spirit* of this objection is very similar to van Fraassen's, although I believe that my presentation is novel.

Else, theory-choice is either radically underdetermined, or merely a *pragmatic* matter.

But now we must ask whether the type of explanation that is normally expected of an abductive inference is not just one that is based upon *experience;* for example, in theorising that a Christmas present is from one's parents, rather than Santa Claus. In terms of the earlier discussion under the heading of *'Scientific Realism vs. Instrumentalism'*, would a realist not just prefer Bohm's interpretation over the Copenhagen one because it relates more to their preconceptions based on macroscopic experience? This points to an internal tension in the scientific realist's position:

AOT - How can we judge how *explanatorily successful* a theory is (B), without succumbing to *experiential predilections* (forbidden by A)?

The answer is by no means clear, and this clearly speaks in favour of van Fraassen's agnosticism about (*not* denial of) the existence of suprasensibles, at least as they are characterised by scientific theories. In his words:

> 'if it matters more to us to have one sort of question answered than another, there is no reason to think that a theory which answers more of the first sort of questions is more likely to be true (even with the proviso 'everything else being equal'). It is merely a reason to prefer that theory in another respect.'[77]

The only strategy available to the realist, in order to diffuse the AOT, seems to be to perform a historical analysis of science past, in an attempt to demonstrate that those theories which *we* (humans) consider to be more explanatorily successful have turned out to be the better ones in the *long-term.* At least, if such a link between explanatory power and long-term empirical success (in terms of successor theories) were to be established, then philosophers could agree that "If scientists behave *as if* the suprasensibles employed by their theories exist, then they will perform better science." That would be a good start.[78]

Scientific Realism - A Brief Recapitulation

Unfortunately, the arguments surrounding scientific realism are complex at each stage (or each thesis), and it is easy to lose sight of how it stands in relation to its competitors, instead becoming transfixed by one technicality or another. In light of this, I believe that a diagrammatic presentation might prove useful to the reader, and serve to add clarity to my previous prose:

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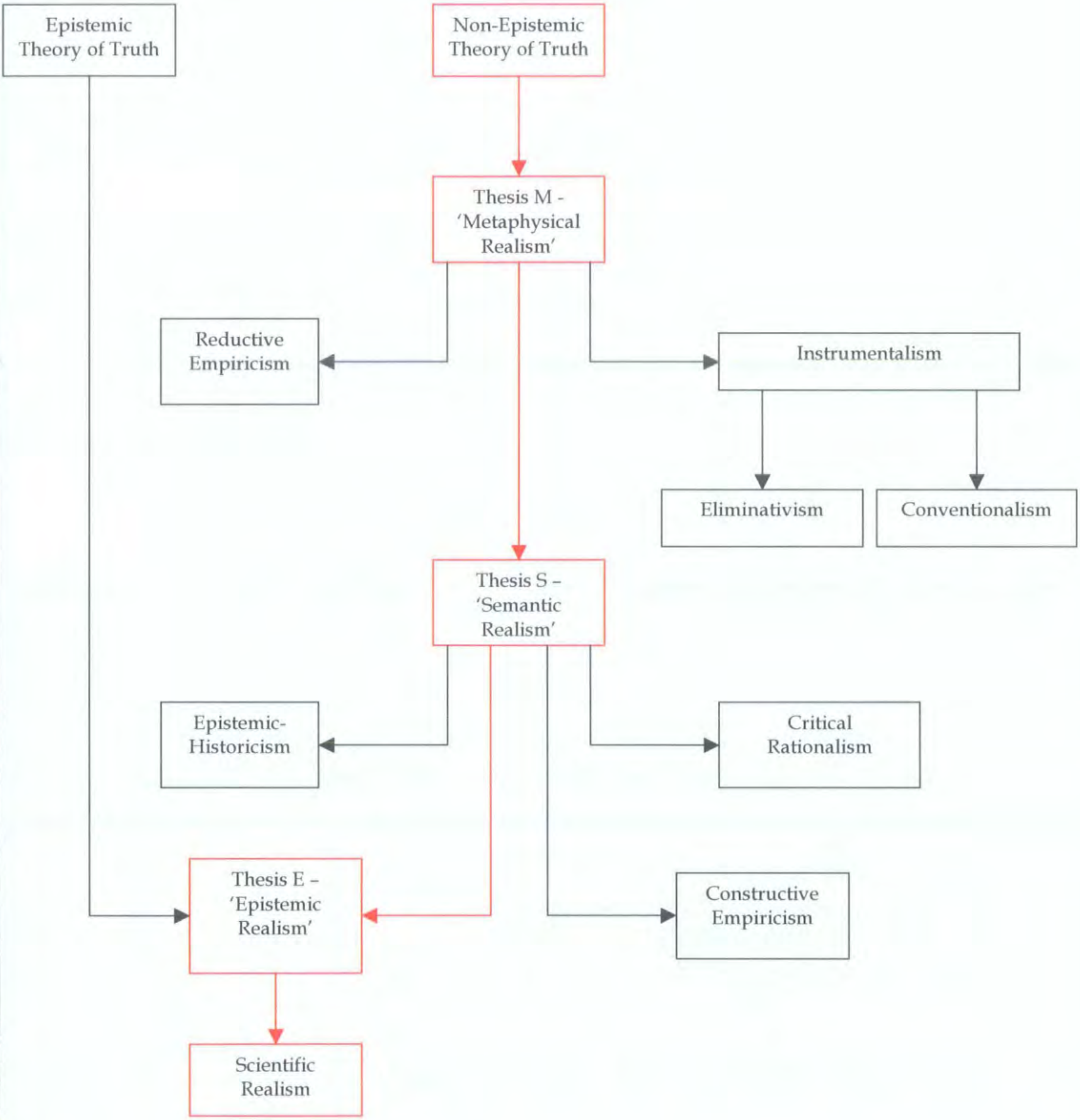
[77] van Fraassen [1980], p.87

[78] Of course, local scepticism would still be *possible,* although it would be less plausible. Note also, in this context, that van Fraassen isn't a sceptic about thesis M - so why should he be one about thesis E, if the realists were successful in establishing an 'explanatory power-empirical success' link?

FIG. 2 – SCIENTIFIC REALISM AND ITS COMPETITORS



Only by following the line in red does one arrive at scientific realism.

Although fig. 2 is not designed to be comprehensive - for example, I might have included Poincaré's fictionalism as a competitor to thesis S - I believe that it encapsulates the spirit of the debate between realists and anti-realists, with respect to science. Indeed, I think that looking at the structure of the debate in such a manner allows one to draw three interesting conclusions:

1) Central to the scientific realist position is the notion that there is no valid epistemic distinction to be made between observables and unobservables. (Which is not to say that unobservables should be necessarily thought to have properties which are similar, in any respect, to those of observables.)

2) Over time, the anti-realist positions have 'slipped', becoming ever closer to scientific realism. This is reflected by the fact that modern anti-realists are generally in the 'epistemic-historicist' (Laudan), 'critical rationalist' (Miller), or 'constructive empiricist' (van Fraassen) camps; there are hardly any reductive empiricists or instrumentalists left to speak of.[79]

3) The move of the later Putnam, towards an epistemic theory of truth, is in some senses a brilliant one. It allows him to evade a plethora of problems, and jump straight to the 'Holy Grail' of scientific realism, namely thesis E. However, although fig. 2 makes it clear why he should want to take such a line, the fact that it is so 'easy' should, I believe, make one a little suspicious. Such a 'quick fix' seems very implausible, especially in view of his earlier work in mainstream scientific realism (the red line on fig. 2). I contend that in philosophy, as in life, it is often the most difficult trails that lead to the most interesting places. [80]

   Besides, in getting to thesis E via the 'easy route', Putnam loses sight of his initial reason for wanting to get there - to defend the intuitive belief that we can say much about the world that is independent of our place in it, and our sense-data. The pot at the end of the rainbow, the same one that he began to follow in his youth, just turns out to be filled with iron pyrite. (This is only because he has deviated from following the rainbow, and jumped to the place where he has guessed that it will end - if one follows it diligently, then one takes the risk that it might never end, although one always has the promise of gold if it does.)

This concludes my discussion of the 'hard-core' of scientific realism, and I shall now proceed to defend my decision to avoid including an 'Aspirational

[79] I might also mention other modern positions, such as structural realism (Worrall), and entity realism (Hacking), which are also based upon acceptance of thesis S.

[80] I do *not* intend to imply that Putnam has just taken the easy route for the sake of it. I want to suggest, rather, that it has not yet been established whether the mainstream is tenable, and that his choice to avoid it seems a little too quick.

Thesis' therein, by examining the Neo-Popperian position known as 'critical rationalism'.

1.2 THE POVERTY OF THE 'ASPIRATIONAL THESIS' – AGAINST POPPER(S) AND CRITICIAL RATIONALISM

'I am not going to read your diatribe!' Popper had shouted when he saw my comments on his diatribe against Bohr. (He calmed down when I told him that many people had complained about my aggressive style and had ascribed it to his influence. 'Is that so?' he said, smiling, and walked away.)
(Feyerabend [1995], pp.145-146)

In his PhD thesis, Hendry claims that scientific realism should also admit an 'Aspirational Component', in order to state what counts as success in science.[81] This thesis would look something like:

Thesis A - The aim of science is truth. Science aims to discover the truth about objective and mind-independent reality, and explain (truthfully) how this bears upon the phenomena.

Specifically, this is not to pass judgement about the motivation of individual scientists, but rather to state what the aim of the game itself (science) is - 'discovering the truth', in science, would thus be analogous to 'forcing one's opponent into checkmate', in chess.

Now, while I do not disagree with Hendry that this 'Aspirational Component' is an important part of scientific realism, I want to claim that it just *follows from* the three theses (M, S, and E) presented in the previous sub-section. That is to say, if one accepts M, S and E, then one can hardly think that the aim of science is just empirical adequacy, *irrespective of what scientists might think that its aim is.* Certainly, if science has just *happened* to get closer to the truth over time, even though that wasn't the aim of the game (and part of the rules of the game), then thesis E might be true today, and false tomorrow! This is clearly not the sort of thing that a scientific realist would want to admit.

However, in order to understand *why* Hendry wants to admit thesis A, we must look at 'realism about science' in a wider context. For he believes that it is necessary to distinguish between critical rationalists (putatively 'realists'), on the one hand, and constructive empiricists, on the other:

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[81] Hendry [1995], p.58

'Popper adopts aspirational realism as a kind of categorical epistemological imperative that motivates the methods of science. For van Fraassen, the methods presumably determine the aims: we should trim the latter to suit what can be achieved with the former.'[82]

On this issue, I am firmly on the side of van Fraassen, as Hendry characterises him; indeed, this is why I think that his position is internally consistent, whereas Popper's (and that of the subsequent critical rationalists) is not. The question that I will ask in this sub-section is, then, "Does it make any sense for a critical rationalist to claim to be a realist?" Or, perhaps more forcefully, "Is any sort of realism about science consistent with critical rationalism?" Before it becomes clear why I want to answer in the negative, it will first be necessary to examine both Popper's philosophy, and subsequent critical rationalist work, in further depth.

The Logic of Scientific Discovery – The 'Popular Popper'

Popper's philosophy rests, first and foremost, on his thoughts about the *problem of induction* (specifically, the problem of *justifying* induction, rather than *classifying* which inferences are inductively valid):

'the principle of induction must be a synthetic statement; that is, a statement whose negation is not self-contradictory but logically possible. So the question arises why such a principle should be accepted at all, and how we can justify its acceptance on rational grounds.'[83]

Indeed, Popper thinks that the problem of induction is so serious that any account of science which mentions it must be quite wrong; that one cannot even learn from induction that one theory is more *probable* than another. So his 'logic of scientific discovery' is designed specifically to avoid induction, at all costs - it consists, instead, of 'deductive testing of theories'.[84] In a nutshell, this deductive method consists of the following:

i) We put forward theories, and it just *does not matter* how we arrive at them: 'The question how it happens that a new idea occurs to a man...is irrelevant to the logical analysis of scientific knowledge.'[85] What *does* matter is that any theory should be internally consistent, and empirically testable-in-principle (even if it is not currently testable). That is to say, it should make predictions (singular statements) which can be verified or falsified.

   If a theory is *not* testable, then it is *not a scientific theory*; this is Popper's demarcation criterion:

[82] Ibid., p.58
[83] Popper [1980], pp.28-29
[84] Ibid., p.32
[85] Ibid., p.31

> 'I shall...admit a system as empirical or scientific only if it is capable of being *tested* by experience...*it must be possible for an empirical theory to be refuted by experience.*'[86]

ii) Necessarily, if a theory makes a false singular statement, then it is false (since the singular statements it makes are deductively entailed by it), and must be rejected. A new theory is clearly necessary to take its place.

iii) In judging between two theories, we should *never* ask which is more likely to be true: 'I never assume that by force of 'verified' conclusions, theories can be established as 'true', or even as merely 'probable'.'[87] We say of one theory that has survived more tests (or more severe tests) than another that it is just better *corroborated* by experience.

When choosing which of a set of *new* theories to adopt, we should prefer the one with the most empirical content, viz. which predicts the most, because it will be the most testable:

> 'our methodological rule that those theories should be given preference which can be most severely tested...[is] equivalent to a rule favouring theories with the highest possible empirical content.'[88]

Essentially, then, what drives science ahead (for Popper) is a continual process of 'Conjecture and Refutation' - the faster we set up theories, knock them down, and set up new theories, the faster that science will progress. But now we might ask "progress *in what way*?" It is pretty obvious that the process Popper suggests might drive science ahead in terms of empirical adequacy - but how about in terms of truth-like representation of a mind-independent, objective, world?

If we can all agree that the set of possible theories is infinite, then identifying any finite number of theories as being 'false' rather than 'not proven false', over time, will bring us no closer to appreciating any truths about the world, no matter how basic. And Popper does *not* suggest in the original version of his 'Logic of Scientific Discovery', even once, that the theories of today are any more truth-like, or closer to the truth, or *verisimilar* (a term which he coined later, as will become clear), than their predecessors.

The original 'Logic of Scientific Discovery' is thus, taken alone, a consistent philosophical thesis about science, that makes no mention of realism as an aim of science whatsoever.[89] It is later, when Popper tries to smuggle 'realism' in through the back door, that the inconsistencies begin. Consider

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[86] Popper [1980], p.40
[87] Ibid., p.33
[88] Ibid., p.121
[89] Popper *does* claim, therein, that the 'quest for truth' is what *motivates* the scientist, and this is not incompatible with his position. See next footnote.

the following, fascinatingly awkward (if not downright contradictory), addition to the 1972 edition of the LScD:

'The logical and methodological problem of induction is not insoluble, but my book offered a negative solution: (a) *We can never rationally justify a theory*, that is to say, our belief in the truth of a theory, or in its being probably true. This negative solution is compatible with the following positive solution, contained in the *rule of preferring* theories which are better corroborated than others: (b) *We can sometimes rationally justify the preference* for a theory in the light of its corroboration, that is, of the present state of the critical discussion of the competing theories, which are critically discussed and compared from the point of view of assessing their nearness to the truth (verisimilitude). The current state of this discussion may, in principle, be reported in the form of their degrees of corroboration. The degree of corroboration is not, however, a measure of verisimilitude...but only a report of what we have been able to ascertain up to a certain moment of time, about the comparative claims of the competing theories by judging the available reasons which have been proposed for and against their verisimilitude.'[90]

It is my hope that the reader is as thoroughly confused by this as I am, and as Lakatos, Newton-Smith and Salmon (*inter alios*) have been.[91] How can we (b) even *sometimes* rationally justify our preferences for a theory, when (a) our preferences are no guide, whatsoever, to the truth-likeness of that theory? Surely, if I know and accept (a), then I would have to admit that I have absolutely no good reasons *whatsoever* for preferring any one theory, with respect to its verisimilitude, to any other.[92] Judging the 'comparative claims of...competing theories' would just be a futile exercise - a foolish game - that would only be entered into by those ignorant of (a).[93] Certainly, such an exercise should not have any place in the deductive methodology which Popper initially proposed; the time of the scientist would be better spent in trying to refute existing theories, or in proposing new ones with high testability.

Moreover, as Salmon has pointed out, it is extremely important for any model of science to be able to explain how and why prediction occurs; yet more, to explain why (more often than not) science has had such great predictive successes.[94] But how, Salmon asks, can Popper's philosophy of science achieve this? Certainly, it seems reasonable to say that the corroboration of a theory might *motivate* an individual to adopt it for practical (predictive) purposes - but that is quite beside the point. Salmon's question is along the lines of "How does the corroboration of a theory *justify* our choice to employ

[90] Popper [1980], pp.281-282

[91] Lakatos [1969]. Newton-Smith [1981], ch.3. Salmon [1988].

[92] As mentioned above, since there are an infinite set of theories available, there is no reason to believe even that a theory which has *not yet been proven false* is any more likely to be true than one that has.

[93] We wouldn't know what true theories, or even partially true theories, looked like - so we could only judge a theory against those theories that have been subsequently classified as false. And all are *equally* false, in our eyes...

[94] Salmon [1981], p.434. A few notable examples (mine) are: The times of lunar and solar eclipses, the Poisson bright spot, and the bending of light around Sol.

it for practical (predictive) purposes, rather than merely *theoretical* ones?" And Salmon thinks that the only possible answer is an inductivist one:

'When, for example, scientists assembled the first man-made atomic pile under the West Stands at the University of Chicago, they had to make a prediction as to whether the nuclear chain reaction they initiated could be controlled, or whether it would spread to surrounding materials and engulf the entire city - and perhaps the whole earth - in a nuclear holocaust...It *may* be possible to excise all inductive ingredients from science, but if the operation were successful, the patient (science), deprived of all predictive import, would die.'[95]

This point is very cogent, for one may know that a theory has predicted some things successfully in the past, viz. that it is well-corroborated, but to justify claims about the future, we must use *induction.*

As we are beginning to see, the choice to renounce induction has some very serious consequences for any philosophy of science; indeed, there have been so many attacks on Popper's anti-inductivism that it is quite beyond my means to treat them all herein.[96] Instead, let me just reiterate the important tension between any 'realism about science' and anti-inductivism, for this is my central concern in context of this Thesis. I believe that Newton-Smith said it best:

'1 For Popper the goal of science is increasing verisimilitude. The principles of comparison involved are based on corroboration - the more corroborated theory is to be preferred.

2 There is no way within the confines of the Popperian system to ground rationally the claim that corroboration is linked to verisimilitude. [or even prediction, which is Salmon's separate point]

3 Popper's [only] way out involves abandoning what is unique about the system. [anti-inductivism]'[97]

In other words, without a stronger epistemic component to his realism (something nearer to thesis E), Popper's talk of 'truth' is far too cheap. If we can not even see that one theory is more verisimilar than any other (even those which have been subsequently classified as false), then it seems there are no rational grounds for claiming that finding theories of increasing verisimilitude constitutes 'success in science' (thesis A).

Unfortunately, this is far from being the end of this story, since the critical rationalist will claim that I have made a terrible mistake in all the foregoing arguments. More, that I have fundamentally *misunderstood,* if not deliberately misrepresented, Popper.

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[95] Salmon [1981], p.443
[96] See Miller [1994], ch. 2.
[97] Newton-Smith [1981], p.70

Behind Closed Doors - The 'Socratic Popper'

Although the emphasis in most of the literature on Popper is generally on falsification, those who knew the man in person often take a strikingly different view of his philosophy. For example, Bartley writes that 'the importance lent to the falsifiability criterion and the demarcation problem by Popper and others distorts his thought.'[98] And Boland, who studied under Agassi (Popper's assistant at LSE, for a time), also favours the view that 'falsifiability plays a very minor role.'[99] Instead, Popper's 'disciples' place emphasis on the anti-justificationist nature of his philosophy; that is to say, on the putative fact that Popper wants to link rationality to *criticism* rather than *justification*.[100] Indeed, Miller claims that, for Popper: 'what we call scientific knowledge simply is not knowledge in the philosophers' sense of being justified, or supported by good reasons.'[101]

This is a genuinely revolutionary view, which rejects a core assumption of typical Anglo-American philosophy - namely that part of what makes a belief count as knowledge is that it is *justified*. As Boland frankly admits, it serves to place knowledge claims from areas that have traditionally been considered 'pseudo-scientific', such as mysticism, on a firm par with those made by mature sciences.[102] And while the cynic might want to suggest that Boland, as an economist, would support this view just because he is a 'pseudo-scientist', this hardly constitutes a reasoned riposte. Instead, I will use Popper's own words: 'In science (and only in science) can we say that we have made genuine progress: that we know more than we did before.'[103] Presumably, Popper meant that science is the only way to learn more about the *actual* (and I will add a caveat, that he might have meant knowing *just* some things which are definitely false), rather than the *possible*.

But is it really plausible that Popper adopted one stance in his seminars, 'behind closed doors', and another in his literature? For this is precisely what some would have us believe:

'The all-consuming situation in the early 1960s was that while there was a rapidly growing interest in the philosophy and history of science, the name most often mentioned was not Popper's but that of Thomas Kuhn. Some of the disciples claim that Lakatos took advantage of the situation and, in effect, hijacked Popper's seminar. Supposedly, Lakatos convinced

[98] Bartley, quoted in Boland [1994], p.154
[99] Boland [1994], p.157
[100] I will avoid addressing merely verbal issues, since Miller admits that he is happy to be called an 'irrationalist', if the word 'rationalist' is just taken to mean 'one who acts on good reasons'. See next footnote.
[101] Miller [1994], p.53
[102] Boland [1994], pp.166
[103] Popper [1970], p.57

Popper that the desired recognition could be obtained by recasting Popper's views in a form closer to Kuhn's. Thus Lakatos and Popper made much more of the growth of knowledge implications of Popper's view and much, much less of the Socratic dialectical aspects which the disciples advocated.'[104]

So the 'disciples' mentioned in the above passage seem to paint a picture of Popper as a rather weak and vain man, easily seduced by the promise of fame, and of Lakatos as the metaphorical serpent who corrupted his otherwise virtuous mentor. More, they seem to imply that Lakatos and Popper worked in close collusion, to present a united front against the dominancy of the Kuhnian, if one will excuse the pun, paradigm. But to both these suggestions, I say "Poppycock". Certainly, Popper would have been the first to deny such characterisations, as is abundantly evident from the following passage:

'For many years he [Professor Lakatos] attended my lectures and seminars, at which criticism was invited as a matter of course; and I made myself regularly available for him to discuss - if he so wished - any problems or criticisms he might have...*The only occasion on which he took up my invitation to discuss with me his criticism of my position led to a conversation about corroboration and verisimilitude*'[105] [emphasis mine]

This said, however, I am not entirely unsympathetic to the idea that there is a side to Popper's philosophy, hidden deeper in his works, which is not often appreciated. For example, in stark contrast to the statement mentioned earlier (footnote 90), that '*we can sometimes rationally justify the preference* for a theory', Popper wrote elsewhere, in the same year, that:

'in spite of the 'rationality' of choosing the best-tested theory as a basis of action, this choice is *not* 'rational' in the sense that it is based upon *good reasons* for expecting that it will in practice be a successful choice: *there can be no good reasons in this sense, and this is precisely Hume's result.*'[106]

That these two statements are in tension is an understatement, since the former would suggest that rational justification *is* 'worth something', whereas the latter would suggest that it is simply impossible, and that to seek it would be a fool's errand. Of course, it would be very easy to accuse Popper of yet more inconsistency, but that is really beside the point. Instead, I want to take the latter quotation above as evidence, albeit minimal, that there is some legitimacy in the 'Socratic' view of Popper. Indeed, perhaps it might even be fair to suggest that any apparent inconsistencies are just based on his use of established words, viz. 'rational' and 'justify', in non-standard senses.

In the final analysis, I suppose it does not matter which of the two views, 'Popular' or 'Socratic', is the right view, or indeed whether either is; it is more important, indeed, just to recognise that there *are* two views, and that both

[104] Boland [1994], p.162-163
[105] Popper [1974], p.999
[106] Popper [1972], p.21

should be taken into account when discussing Popper's philosophy. As Feyerabend put it, 'long live Popper$_{-\infty}$, and happy fornication (NOT with him, God beware!)'[107] Critical rationalism has moved on from its roots, and is deserving of consideration in its own right.

Critical Rationalism: Restated and Assaulted

For the critical rationalist, all human knowledge consists of hypotheses, or unsupported and *unsupportable* guesses.[108] The process of inquiry, then, is to be thought of as twofold. First, it consists of subjecting *accepted* hypotheses to harsh criticism, in the hope of refuting them: 'it relies on expulsion procedures, rather than entrance examinations.'[109] (Note, of course, that to *accept* a hypothesis we do not have to have good reasons for believing it.) Second, it consists of making educated guesses to replace those rejected. 'Educated', that is, in the sense that not just any old hypothesis will do; those that are admissible are only those that can be refuted *in principle* (hopefully in practice) and have not been antecedently refuted. In the case of science, a hypothesis (or guess) can only be admitted if it is experimentally refutable; to this extent, at least, the proponents of the 'Socratic Popper' and the 'Popular Popper' agree.

Now the obvious question, here, seems to be "How can we legitimately classify any given hypothesis as false, if we can have no *good reasons* at our disposal for believing that it is, in fact, false?" Indeed, in the case of science in particular, we might ask "How can we know that a given theory is false, without having *good reasons* for believing that certain observation statements are *true*?" If the critical rationalist confesses that we *can* have good reasons for believing in the truth of observation statements, while simultaneously contending that observation is theory-laden (which Popper certainly did), then he has dug himself a hole.[110] For, as Crispin Wright points out:

> 'Since such good reason [regarding observation statements] requires good reason to believe the background theories which, by hypothesis, condition our assent to those observation-statements, and since the relevant notion of truth is - presumably - absolute, the Popperian realist is in a poor position to explain, what is crucial to his position, the possibility of reasonable confidence that a theory has been shown to be false.'[111]

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[107] Feyerabend, in a private letter to Lakatos dated 28 February 1970. See Motterlini [1999], p.193

[108] As Miller puts it: 'there are no such things as good reasons; that is, sufficient or even partly sufficient favourable (or positive) reasons for accepting a hypothesis rather than rejecting it, or for rejeceting it rathing than accepting it, or for implementing a policy, or for not doing so.' See Miller [1994], p.52

[109] Miller [1994], p.6

[110] For Popper on the theory-laden nature of observation, see Popper [1982], p.3

[111] Wright [1993], p.295

However, it is my understanding that a critical rationalist would reject the notion that we can even have 'reasonable confidence that a theory has been shown to be false'; certainly, he would reject the notion that we can ever be *certain* that a theory is false.[112] Miller's argument, for example, seems to be that it is perfectly reasonable to *classify* theories as false, without any reference to our beliefs whatsoever:

'science is a collection of statements...the business of science is the discovery, as far as is practicable, of the truth values (and perhaps of the relative degrees of approximation to the truth) of these statements. The whole business can be explained, quite satisfactorily, without any reference to certainty, probability, confirmation, support, reliability, confidence, justification, good reasons, or *knowledge*. Truth and falsehood suffice.'[113] [emphasis mine]

In fact, it seems that any talk of knowledge, in the context of the critical rationalist view of science, will just be rebutted by a statement like "All knowledge is just hypothetical." Thus, critical rationalists are what I would call *hypothetical realists*; that is to say they *accept* realism, without committing to any belief in it. In answer to the question "Why does your philosophy contain the hypothesis that Thesis M is correct?", the critical rationalist will 'answer ', "Why not?"[114] More, the critical rationalist will confess that 'falsificationism is unable to justify (in whole or in part) its role in the search for truth.'[115] And when one asks "Well, why should I believe that it is fruitful?", one will (predictably) be answered by another "Why not?"[116]

Of course, the critical rationalist is just inviting the questioner to play his game, and by offering criticism one does, presumably, concede that there is certainly some link between criticism and rationality. Nonetheless, I shall not let this deter me from offering several criticisms of critical rationalism.

First, let me reiterate that Popper's philosophy was founded on his 'solution' to the problem of induction; that is, the problem of *justifying* induction. Yet it has arrived at the conclusion, at least as developed by some of his successors, that there simply *isn't* (and never has been) a real problem of induction, just because justification isn't important. Given this, one might legitimately ask why the critical rationalist does not just accept induction, in the same way that he accepts thesis M (and thesis S), as a working hypothesis.[117] Surely he would not want to contend that induction has been subjected to enough

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[112] Except, perhaps, in the trivial case where a theory has internal logical inconsistencies.
[113] Miller [1994] , pp.11-12
[114] Or, as Miller puts it, 'Science proposes order for the world, it does not presuppose it.' See discussions in Miller [1994], sections 2.1a and 2.2a.
[115] Ibid., p.48
[116] See the above reference, where Miller makes it clear that he believes the onus is on the critics to explain why falsificationism is 'inadequate to its task'.
[117] One answer might be that no-one has managed to properly *classify* what does, and does not, count as 'inductive logic'. However, this is insufficient, for I might ask "Why not accept that there are inductive inferences, as a working hypothesis, and join in the quest to classify them?"

criticism (and remember, the criticism that it is unjustifiable is not important) to render it falsified? If so, then I shall invite the next critical rationalist that I meet to hold his hand in a candle flame. And when he replies that he will not, just because he accepts a theory that doing so will cause him pain, my rejoinder will be "Should you not subject that theory to another test? Should you not be actively trying to refute that theory, at every given opportunity? After all, to assume that a theory is confirmed, just because it is corroborated, is the sort of sin committed by your arch-enemy, the vile inductivist!"[118]

Second, I question why the critical rationalist should want to say that the 'aim of science is truth' when this has no visible link to the deductive methodology which he proposes. Consider:

i) We can never know that one theory is closer-to-the-truth than another, or even more 'likely to be true', in light of any evidence whatsoever.[119] Thus, we have absolutely no evidential grounds for judging whether the scientific theories of today are any more truth-like than those of Neolithic men (who were capable of building remarkably accurate solar calendars, *inter alia*, as Stonehenge demonstrates). Indeed, it is a distinct possibility that many past theories were *more verisimilar* than those of contemporary science.

ii) We adopt a methodology (Conjecture and Refutation), and *hypothesise* that it is suited to the task of finding theories of increasing verisimilitude.

iii) We could never empirically falsify that hypothesis, from i.

So the critical rationalist claim that 'the aim of science is truth' (under their normative methodology) is not only hypothetical, but also highly dubious. As a *reductio ad absurdum* of this position, we need only note that the critical rationalist might as well say that 'the aim of all human activity is truth', that is, if there is no discernible link, no matter how partial, between method and aim.[120] To put it another way, *why claim that science is any more likely to yield theories of increasing verisimilitude than nose-picking*?

Of course, the critical rationalist will want to say that his methodology is akin to shooting an arrow at a target (the truth) which is concealed by a heavy fog, but such an analogy is disingenuous because the nature of the target is unknown and unknowable. Thus, it would be no less rational to cast a fishing

[118] This goes back to Salmon's criticism - that we cannot explain the predictive successes of theories without appealing to induction.

[119] Of course, the critical rationalist does allow that we can *prefer* one theory over another, if it is more corroborated.

[120] And I reiterate that the aim of science, here, is *what counts as success in science*, not what motivates scientists, individually or en masse.

line into the fog than it would be to throw out a cigarette butt, or even to shout loudly while hopping on one foot! In other words, the natural methodological complement to the critical rationalist's epistemic stance is just Feyerabendian pluralism; that is, if he still wants to claim that truth (as correspondence to the actual metaphysical world) is the aim of science.

Alternatively, if his aims are trimmed to what can be *demonstrably* achieved by his methods, then 'success in science' for the critical rationalist should lie in achieving either empirical adequacy (truth as correspondence to the *phenomenal* world), or human consensus (in the ideal limit of enquiry).[121] Therefore, the only rational paths for one who sticks to the critical rationalist methodology are anti-realism or empiricism. The notion of correspondence truth as the *aim of science* (but not as an aspiration) must be completely rejected. Watkins forcefully drives home this point :

> 'I...accept that the idea of truth is a regulative ideal for science: truth is what science aspires after. But to aspire after X is not equal to aiming at X. A schoolboy who dreams of being a military hero does not yet have military aims. If one is to aim at X, and pursue one's aim rationally, one needs to be able to monitor the success or failure of one's attempts to achieve X. Are Popperians entitled to claim that one could do so if X were simply *truth*? Here is a simplified version of what, for us, would be a paradigm of scientific progress. Within some problem-situation a powerful new theory T_1 is advanced. It is tested and for a time it only wins corroborations. But then a more corroborable theory T_2 is advanced. Crucial experiments between it and T_1 are performed, and they go in favour of T_2. The splendid T_1 has fallen in battle. Later, the pattern is repeated, with T_2 being refuted and superseded by the more corroborable T_3. Was science fulfilling the aim of truth in this admirable progression? Not with T_1, which turned out to be false, nor with T_2 which suffered the same fate. Perhaps this aim was fulfilled with T_3? Well, we may learn that it was not but we'll never learn that it was.'[122]

Third, and finally, I would point out that just because one rejects justificationism, one need *not* hypothesise that conjecture and refutation is the best way to proceed in inquiry-related discourse.[123] And I would ask the critical rationalist how one can refute his epistemology, when criticism of it is parasitic on that very epistemology. Is 'to be a critical rationalist' not, ironically, 'to be a *dogmatist* about epistemological concerns'? And does critical rationalism not meet its doom in the fact that it is deductively irrefutable, and thus inadmissible as a valid hypothetical epistemology according to its very own rules?[124]

To sum up, here is a philosophy that says there is such a thing as truth, although that truth is evidence-transcendent (insofar as we could never

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[121] Another option is for the Critical Rationalist to say that the aim of science is *possible* truth; this is Watkins' approach. I would characterise this approach as being: "Science aims at the recognition of falsehoods." Watkins [1997].

[122] Watkins [1997], paragraph 13.

[123] Arguably, foundherentism (Haack) and contextualism (Annis) both avoid the pitfalls of 'justificationism' as Popper would have understood it.

[124] Analogously, of course, verificationism pronounced itself to be 'meaningless'.

recognise it, were we to have it), and that truth-hunting is a worthy exercise, even though we can never know when a hunt has been successful. Moreover, it fails to explain why science is a worthwhile activity, say in comparison to chasing shadows, simply because we can not even know if our method of truth-hunting is really suited to the task of finding the experientially inaccessible truths! At least shadows can be seen, if never caught.

1.3 CONSEQUENCES OF THE 'HARD-CORE'

By taking a holistic view of the hard-core of scientific realism, in contrast to its competitors - especially critical rationalism - several points emerge:

i) Scientific realism is a *justificationist* programme; scientific realists seek to justify their beliefs, in each of their theses (M, S & E), in the face of many different forms of local scepticism. Overall, though, scientific realists also employ a positive argument, put forward by Hilary Putnam, according to which scientific realism 'is the only philosophy that does not make the success of science a miracle.'[125]

This 'No Miracle Argument', as it is typically known, is intended to be a peculiar form of *abductive* argument; the explanandum is the contingent fact that science works, and the explanans is the three theses in the core of scientific realism. Unfortunately, however, the anti-realist will level the criticism that this form of argument just presupposes something which is already at question in thesis E, namely the validity (or even existence) of *abductive* inferences, period.[126] So the 'No Miracle Argument' is really a *meta-abduction*, and the accusation is that it is viciously circular.

The realist rejoinder is based, first, upon a distinction between premise-circularity and rule-circularity. An argument which is circular in the former sense is one that is designed to prove the truth of a proposition, P, but also presupposes the truth of P in its premises; this is obviously vicious. But an argument which is circular in the latter sense is quite different, at least *prima facie*, since it takes a set of propositions $P_1,\ldots,P_n$, employs a rule of inference, I, and then reaches a conclusion Q. However, Q has a special property, specifically that it serves to support, or confirm, the *rule of inference*, I.[127]

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[125] Putnam [1975], p.73

[126] See my *Abduction-Observation Tension* (AOT), mentioned towards the end of section 1.1, in the discussion of Thesis E.

[127] The distinction between premise-circularity and rule-circularity was first made by Braithwaite, in his defence of induction. See Psillos [1999], pp.82-83

Since the 'No Miracle Argument' *is* an example of rule-circularity, the realist can shift the focus of this debate to epistemology, and argue for an *externalist* account of justification.[128] As Psillos puts it:

> '*externalist* accounts sever the alleged link between being justified in using a reliable rule of inference and knowing, or having reasons to believe, that this rule is reliable...all we should require of a rule-circular argument is that the rule of inference employed *be* reliable; no more and no less than in any ordinary (first-order) argument.'[129]

So the purpose of the 'No Miracle Argument' is not necessarily to give a reason for believing in the validity of abduction; it is just to show that, if such inferences are reliable (overall), *which we have no prior reason to doubt* (or to believe), then scientific realism has support which its competitors do not. More, even if this meta-abduction *is* taken as a defence of abduction, the realist might pose a dilemma for the internalist:

> 'When it comes to the defence of our basic modes of reasoning, both ampliative and deductive, it seems that we either have no reasonable defence to offer or else the attempted defence will be rule-circular.'[130]

For, as Carnap has pointed out, deduction is just as circular as induction; one can not convince a sceptic about deduction that it is a valid form of inference without using deduction (or induction, which the sceptic about deduction might equally be sceptical about).[131]

ii) *Induction* is the foundation-stone of scientific realism (because it is a *justificationist* programme). In the first instance, one notices how induction is related to thesis M; that an endorsement of thesis M serves to partially justify the use of induction, whereas the evidence that we can successfully employ inductive arguments serves to partially justify thesis M.[132] In the second instance, one notices how the proposition that theories make *inductive* links as well as deductive ones is vital in justifying thesis S; that is, in avoiding Hempel's 'Theoretician's Dilemma'. And in the final instance, one sees that some form of inductive logic is necessary if the scientific realist is to support the claim, in thesis E, that theories can achieve degrees of confirmation.

Moreover, there are other arguments employed by scientific realists, not treated in depth in the foregoing discussion, which rely on

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[128] Note, of course, that several of my previous criticisms - e.g. on Rescher's attempted justification of thesis M - have been made from an *internalist* perspective. Here, I am just outlining a typical scientific realist argument, without necessarily committing to it.

[129] Psillos [1999], p.84

[130] Ibid., p.86

[131] Carnap [1968],p.265-267

[132] This was what led to circularity in Rescher's retro-justification argument.

induction. For example, *abduction* is seen by some as just a peculiar form of induction; that is, even though this might make my *Abduction-Observation Tension* a much more serious problem.[133]

iii) Scientific realists need not believe that science will eventually converge upon the 'final' truth. Rather, they can accept that the aim of science is just to generate theories of increasing verisimilitude, or with a higher degree of approximation to the truth. Thesis A should be understood in those terms.

iv) A scientific realist who was also a practising scientist would have more reason to place stock in successful theories of the past. And it is certainly reasonable to *suppose*, although I shall not attempt to fully justify this until later, that such individuals would adopt different theory-construction strategies to those of anti-realist scientists. If empirical success and truth-likeness are linked in some way, then any practically successful theory must be seen to have something 'truth-like' somewhere within it.

Negatively, of course, this might suggest that realists could be more dogmatic than anti-realists, in certain circumstances. Certainly, that is one way (though not necessarily the right one) to think of Einstein's objections to the Copenhagen Interpretation.

This concludes my presentation of scientific realism, and I shall now move on, as suggested by point (iv), to discuss methodology in greater detail.

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[133] If induction is based on experience (this seems undeniable), and abductive inferences are just a sub-set of inductive inferences, then it follows that abduction is based on experience. Hence the idea that we succumb to experiential predilections, in judging the relative explanatory value of competing theories, seems to be undeniable.

2

ON METHODOLOGY

INTRODUCTION

As was mentioned towards the beginning of the previous chapter - under the heading '*Scientific Realism vs. Instrumentalism: Two Illustrative Episodes*', in section 1.1 - I want to claim that scientists can be either (scientific) realists, or anti-realists, and still enjoy a measure of *empirical success* in science. And *empirical success*, as I characterise it, just consists of the successful application of theory in practice: of using theories in either predictive roles, viz. as a guide to practical action, or in *non-ontic* explanatory roles, viz. to explain relationships between phenomena in terms of Hempel's 'covering law' model.[134] Arguably, it is these 'benefits' that most nearly mark mature sciences as somehow 'different' from other academic disciplines, certainly in the view of the wider (non-academic) community. The predictive benefits of science, as a driving force for technological innovations such as the computer on which I type, and the internal combustion engine that drives my car, are certainly the most obvious.[135] But the weak explanatory benefit, that allows one to answer the question "Why do objects fall to the ground?" simply by saying "Because of gravity" also seems to offer comfort to many. And rightly so, for to say that an individual died because he was a smoker is to make a synthetic statement, and to offer some form of explanation, albeit not comprehensive, for his death.[136]

In the context of this chapter, what demarcates science from non-science (or pseudoscience) is of vital importance, since any plausible methodology of science might rely precisely on that difference (or those differences). Hence this will be my first area of discussion, in section 2.1, and I shall advocate demarcation *guidelines*, rather than *rules* (or laws), based on the theoretical virtues that Kuhn puts forward in his 'Essential Tension'.

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[134] I want to leave the question of whether the realist and anti-realist enjoy the same degree of *heuristic* success, in generating new theories or experiments, open.

[135] 'Beneficial' might be understood, more properly, to mean 'useful'. For example, the creation of the atom bomb was clearly useful, with respect to the goals of the US government during WWII, although one might legitimately question whether its creation was of lasting *benefit* to mankind.

[136] As this example should make clear, the predictive and the non-ontic explanatory roles are interrelated. If I believe that individuals have died (early) *just* because they were smokers, then I would believe the prediction that "Smokers are at more risk of dying young, *ceteris paribus*, than non-smokers." Perhaps the relationship is best illustrated by the counterfactual "If he had not smoked, then he would have lived longer." which is obviously predictive, though is clearly just another way of saying "He died (earlier than he might have) because he was a smoker."

This completed, I shall move on - in section 2.2 - to present my view of scientific methodology as a two-tiered enterprise, involving *normative methods*, in which all scientists take part, and *auxiliary methods*, which are flexible additions to the aforementioned. And my claim will be that it is the *normative methods* which are primarily responsible for *empirical success* (as characterised above), although it is possible, in principle, that certain *auxiliary methods* could contribute to greater efficacy, in a heuristic sense.[137] This will be seen to be significant because it will be argued that realists often adopt a different set of *auxiliary methods* to their anti-realist counterparts.

Finally, in section 2.3, I will offer a brief recapitulation. Particular emphasis will be placed on the relationship between the two foregoing sections; that is, how my views on methodology (and especially *auxiliary methods*) follow from my views on demarcation.

2.1 THE DEMARCATION OF SCIENCE FROM NON-SCIENCE

While the notion of *empirical success* is clearly far too crude, taken alone, to constitute a valid demarcation criterion for (mature) science and non-science, I would argue that it captures the spirit of the difference. As Ruse puts it, 'In looking for defining features, the most obvious place to start is with science's most striking aspect - it is an empirical science about the real world of sensation.'[138] Of course, this needs to be bolstered by considerations such as *reliability*. That is, to make it clear that it is more reasonable for one to have confidence in Newtonian mechanics, as they relate to bridge building, than it is for one to have confidence in the 'laws' of Western Astrology, as they relate to our personal behaviour. Typically, this has been addressed by different philosophers of science in terms of two interrelated features of properly scientific theories (or domains): *testability/tentativity* and *predictive novelty*. The first feature, *testability* or *tentativity*, results from the fact that a scientific theory is designed to account for certain experimental observations of relationships between phenomena. Thus, a scientist opens himself up to the possibility of encountering evidence which demands alteration to existing theory (or at least the auxiliary hypotheses used in its testing):

> 'Ultimately, a scientist must be prepared to reject his theory... Nothing in the real world would make the Kantian change his mind, and the Catholic is equally dogmatic, despite any empirical evidence about the stability of bread and wine. Such evidence is simply considered irrelevant.'[139]

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[137] Heuristic, that is, both in terms of designing new theories, *and* designing new experiments.
[138] Ruse [1982], p.39
[139] Ibid., pp.40-41 Of course, saying 'Nothing in the real world would make the Kantian change his mind' is a little imprecise, since he might change his views based on conversations with other human beings (who, even for Kant, really exist).

The second feature, *predictive novelty*, is based on the historical evidence that scientific theories *successfully* predict phenomena that would have been considered highly implausible beforehand. The history of physics, alone, is replete with examples: the prediction of barrier-penetration by quantum theory; the prediction of light bending around the sun, by general relativity; the prediction of time dilation by special relativity; the prediction of the Poisson bright-spot by the wave theory of light; the accurate prediction of the return of Halley's comet, by Newtonian celestial mechanics; and even the prediction of the sonic boom, by statistical mechanics. In comparison, Lakatos points to the manifest failures of Marxism, as illustrative of pseudosciences:

'It predicted the absolute impoverishment of the working class. It predicted that the first socialist revolution would take place in the industrially most developed society. It predicted that socialist societies would be free of revolutions. It predicted that there will be no conflict of interests between socialist countries. Thus the early predictions of Marxism were *bold and stunning* but they failed.'[140] [emphasis mine]

Of course, the requirement of *predictive novelty* is complementary to the requirement of *testability/tentativity*. For Lakatos would want, correctly, to point out that sustaining one's 'pet theory' in light of *any evidence whatsoever*, simply by tacking on ad hoc auxiliary hypotheses, is unscientific. But bearing in mind that a scientist could, in principle, hold that only his auxiliary hypotheses were tentative - and that is the thrust of the Duhem thesis - the requirement that a properly scientific discipline should predict new phenomena, or previously unnoticed relationships between those already known, does not seem unreasonable. In Kuhnian terms, we might say that science must consist of *fruitful* theories.[141]

Nonetheless, there are still obvious exceptions which we would want to call scientific. Boyle's law, for example, deals explicitly with an ideal case which never, in reality, happens, and does not seem to predict anything other than a simple relationship between a pre-determined set of macroscopically measurable (and observable) variables. It is, thus, neither particularly *tentative*, nor *predictively novel*. And there are other examples: the law of reflection - that the angle of incidence of a ray that strikes a reflective surface (taken from a line orthogonal to that surface) is equal to its angle of reflection; the Zeroth law of thermodynamics - that if a body A is in thermal equilibrium with a body B, and body B in thermal equilibrium with a body C, then body A is in thermal equilibrium with body C; and Newton's first law of motion - that any body which is not at rest, or at constant velocity (*not* speed), has a resultant force acting upon it.

This seems to suggest that, rather than adopt a piecemeal strategy in order to state whether any given law is scientific or non-scientific, one would do better

[140] Lakatos [1977], p.25
[141] This virtue is discussed further, toward the end of the next sub-section.

to consider only *theoretical frameworks*. That is to say, in terms of the last example, one should attempt to explain only why *Newtonian mechanics*, viz. the sum of Newton's three laws of motion, his law of gravitation, and *all the metaphysical assumptions used in their application*, is scientific. For a whole, in virtue of the relations which its parts enter into, is in a sense greater than *just* the sum of its parts.

If one bears this in mind, it also becomes clear that any 'hard and fast' demarcation criteria are going to be doomed to dismal failure, precisely because many theoretical frameworks that are good guides to practical action contain metaphysical assumptions that are neither trivially dispensable, nor always readily identifiable. In the context of Newtonian mechanics (the framework), action-at-a-distance (metaphysical assumption, without which, the laws couldn't justifiably be thought of as spatio-temporally invariant) is an excellent case in point.

What we should look for, then, are demarcation *guidelines* (or rules-of-thumb) - a set of factors which we can apply in order to *roughly* weigh the relative status of a given theoretical framework, domain within a discipline, or even discipline itself, in comparison to competing systems.

Theoretical Virtues as Demarcation Guidelines

In his 'Essential Tension', Kuhn suggests five qualities, or virtues, that might be characteristic of *good* scientific theories: *accuracy, consistency, scope, simplicity* and *fruitfulness*.[142] But even though his emphasis is on the fact that these theoretical virtues are shared epistemic values by which scientists choose between theories put forward within *pre-existent domains of science* – for example, Bohm's interpretation of quantum mechanics vs. the Many Worlds one – I think that they might also be useful in the context of demarcation. That is to say, I want to claim that these virtues might just be characteristic of scientific theoretical frameworks, *period*. Kuhn also suggests this, although it has sometimes been overlooked:

> 'If the list of relevant values is kept short (I have mentioned five, not all independent) and if their specification is left vague, then such values as accuracy, scope and fruitfulness are *permanent attributes of science*.'[143] [emphasis mine]

It is extremely important, however, to understand that I will not make any claims about realism vs. anti-realism in the subsequent discussion of these virtues.[144] For they can be accepted by realists, on the grounds that they constitute abductive principles which allow us to 'sniff out' the more truth-

[142] Kuhn [1977], p.321
[143] Ibid., p.335
[144] To do so would, after all, be unpleasantly circular in the context of this thesis as a whole.

like of two competing theories, and also by anti-realists, on the grounds that they merely represent our *pragmatic* preferences. My discussion will centre on the empirical and historical facts about what we would want, *intuitively*, to call "science". Indeed, since it is obvious that the issue of demarcation has significance in moral and legal respects, e.g. in the case of *McLean v. Arkansas*, this is imperative.[145]

Accuracy - A *scientific* theoretical framework will usually account for the available evidence in its domain, and do so with reasonable *precision*, both qualitatively and quantitatively.

First, in the properly Kuhnian sense, a theoretical framework is accurate if its deducible consequences are in agreement with existing observations and/or experiments. And in this sense, it is easy to get a quick grasp of what might demarcate science from non-science. On the one hand, we have the Newtonian framework, which accounts with reasonable success for the period of a simple pendulum, as a function of its length. On the other hand, we have the obvious failure of astrological frameworks, or some religious doctrines, to account for the available evidence. For example, take the column in yesterday's newspaper which told me that I would meet a new love interest; this turned out to be false, just because I didn't leave my house, and in fact did not see another human all day![146] (This, without even pointing to the obvious fact that other Pisceans, say, were in a coma.)

Indeed, in the case of creation 'science', the claim is often that no empirical evidence whatsoever is relevant to the doctrine:

> '...it is...quite impossible to determine anything about Creation through a study of present processes, because present processes are not created in character [of past processes]...we are completely limited to what God has seen fit to tell us, and this information is in His written Word. This is our textbook on the science of Creation!'[147]

So creation 'science', or rather *creation non-science*, involves the construction of theories that are not empirical, in virtue of the fact that they can not be confirmed, or refuted, experientially. Rather than investigate the phenomena, one is encouraged to read a book, and place unswerving faith in its contents, irrespective of all else! The existence of fossils is simply irrelevant, presumably because "God works in mysterious ways." Of course, this is not to say that the theories of creationists are necessarily wrong, though a scientist would be entitled to claim that they have an extremely low probability of

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145 See Ruse[1982a], Laudan [1982], and Ruse [1982b].

146 Of course, this might be seen as only 'one church' of astrology, and there are many astrologers who would frown upon the writers of such newspaper columns. But all I am really saying is that the writers of those columns are not practising science!

147 Henry Morris, quoted by Ruse [1982b], p.57

being true, on the assumption that there are spatio-temporally invariant laws of nature.[148]

However, there is also a second sense in which accuracy might be thought of, and this is perhaps better characterised in terms of *precision*. Consider the following successful prediction, offered by the Newtonian framework,

A) {Made in 1705, by Edmund Halley} - The comets seen in A.D. 684, 1066...1531, 1607, 1682 were all one and the same object, which is in an elliptical orbit about the sun, with a period of approximately 75.5 years. It will return in 1759, 1835, 1910, (late) 1985, etc.

in contrast to the following prediction, offered by the astrological framework,

B) {Sidney Omarr, Horoscope for 12th April 2001, in the Chicago Sun-Times} -

> '**PISCES (Feb. 19-March 20):** An important person takes note of your talent, product. Push hard to maintain your creative control. Rewrite material, major opportunity exists. Scorpio plays top role.'[149]

Now as is eminently clear, B is very vague in comparison to A, and with good reason - namely, to increase its likelihood of applicability to any given Piscean reader. In my case, the 'product' might be this thesis, the 'important person' might be my supervisor, and Omarr's advice would be to rewrite this section, because there might be a 'major opportunity' (for a paper, perhaps?) in here somewhere. Of course, horoscope entries never read as follows:

"**PISCES (Feb. 19-March 20):** At 9:33:23 AM, May 5, 2001, you will experience a sharp pain in your chest. Do not bother to take an aspirin; the pain will subside 22 seconds later."

So my claim is that the difference between A & B might be characterised in terms of *precision* (or, inversely, *vagueness*). For, whereas A predicts a set of specific and time-indexed events, B might be seen to have predicted any number of events, over an indeterminate period of time, on the basis of recalcitrant experience. If A had failed, then it would have been obvious, and it would certainly have caused some adjustments to the overall Newtonian *framework* (not necessarily the laws therein). However, it isn't even clear that B *can* be seen to fail; for example, its author might even make the practically untestable assertion that his prediction was only right for the *majority* of Pisceans.[150]

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[148] It is this assumption that the creationists challenge.
[149] See Internet URL: http://www.suntimes.com/index/omarr.html
[150] Note that I am avoiding the Duhem problem, here, by discussing theoretical *frameworks* rather than individual laws.

Consistency - A *scientific* theoretical framework will be internally consistent, in logical terms. In addition it will usually be externally consistent, *with other scientific theoretical frameworks.*

Clearly, as a stand-alone demarcation principle, consistency would only be useful in its internal respect, and *prima facie* its external aspect may seem to be somewhat parasitic on the other demarcation principles that I shall cover here. This should not be problematic, however, because the principles are only *guidelines*, and because they are not supposed to be useful for demarcation purposes when applied in isolation from each other.

Nonetheless, it is very important to understand that two theoretical frameworks which are externally consistent, each with the other, do *not* have to be in strong agreement on a metaphysical level (although their fundamental metaphysical assumptions are almost always similar, if not identical). Consistency is more important, rather, in terms of either *conjunction* or *correspondence*.

In terms of *conjunction*, which has already been mentioned in the earlier discussions of the theoretician's dilemma and constructive empiricism (section 1.1), scientific frameworks can often be combined (or conjoined) in order to predict/explain (in Hempel's sense) phenomena that lie in the overlap between their two respective domains. For example, in order to calculate the average velocity of molecules in a gas at known temperature, volume, and pressure, one might calculate their translational energy from statistical mechanics (working out how much of their total kinetic energy was in fact stored in other degrees of freedom, viz. vibrational or rotational), and then apply classical mechanics. Mature science is 'special' in this respect since areas such as biophysics, biochemistry, and physical chemistry, are feasible. In non-science, religions such as Christianity and Buddhism are manifestly incompatible, as are the predictions issued by Numerology, as compared to those from Western Astrology. For example, I was born on 5 March 1975, hence my life path number is 3 {5+3+1+9+7+5=30, 3+0=3}, and my Sun sign is Pisces. According to the same source, namely Michael McLain, in terms of numerology,

> 'Here we are apt to find...bright, effervescent, sparkling people with very optimistic attitudes...The approach to life tends to be exceedingly positive...and your disposition is almost surely sunny and open-hearted.'[151]

Yet in terms of astrology (and it is worth nothing that 'my' sun is slap-bang in the middle of the Pisces house),

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[151] Michael McLain, internet URL: http://www.astrology-numerology.com/num-lifepath.html

'You're a moody and introspective person, and it's hard for others to understand you. It may be hard for you to understand yourself sometimes. Your temperament varies from being optimistic to being acutely pessimistic.'[152]

In terms of *correspondence*, a scientific theoretical framework will generally explain how its predecessor(s) are empirically adequate, under the correct circumstances. Hence, the special relativistic framework reduces to the Newtonian one in the correct parameter limit, viz. when velocities much lower than the speed of light are at play, and $\gamma \approx 1$. So 'consistency' in this respect is not so much about demonstrating overall agreement, so much as *explaining* points of disagreement; for absolute inertial mass in Newton's second law, one might substitute 'relativistic mass'.[153] In the case of astrology, however, the discovery of the outer planets (Uranus, Neptune and Pluto) was brushed off as unimportant to personal qualities, in virtue of the fact that the period of these planets would mean that a lot of very similar people should be walking around. (The period of Pluto, though erratic, is around 250 years.) Instead, when an outer planet changes sign it is supposed to represent a *societal* change; a very convenient story, viz. *ad hoc* addition to the theory, indeed.[154]

Scope - A *scientific* theoretical framework will often have implications that were not used in its construction. Sometimes, these will not even have been *envisaged* before it began to be employed.[155]

Although Newton's system of mechanics was created in order to account for the motion of celestial bodies, it coincidentally served to explain other phenomena such as the tides, and projectile motion, on Earth. That is to say, it synthesised Galileo's terrestrial mechanics and Kepler's celestial kinematics. Likewise, when Maxwell devised his system of electromagnetism, by positing symmetry between curl of electric field and curl of magnetic field, the notion

[152] Michael McLain, internet URL: http://www.astrology-numerology.com/insigns-sun.html As an aside, I might also add that astrology is not even internally consistent, since predictions based on sun sign are often at odds with those made by the position of the moon, or the planets, in one's astrological chart. The professional astrologer thus spends much of his time attempting to resolve contradictions (a radically subjective exercise), in order to give a holistic view of an astrological chart.

[153] Whether or not, I hasten to add, one wants to see those two terms as taking on new reference or meaning (as Kuhn or Feyerabend might claim).

[154] In addition, note that astronomy has proven capable of *predicting* the existence of new planets, whereas astrology has not. Presumably, the astrologist of the past did not notice anything 'wrong' with his theory - just because no serious attempt was made to test it.

[155] In my opinion, Kuhnian 'scope' is very similar to what philosophers of science now call 'use-novelty'. This reading is supported by the following quotation: '[a theory] should have broad scope: in particular, a theory's consequences should extend beyond the particular observations, laws, or subtheories it was initially designed to explain.' Kuhn [1977], p.322

of electromagnetic waves moving without a medium would have been thought absurd.

Nonetheless, when Einstein dropped the bomb of special relativity on the physics community (and when the results of the Michelson-Morley experiment became widely known), Maxwellian e-m proved to fit neatly into the new mechanics. (See the earlier derivation in section 1.1, under 'The Semantic Thesis'.) Here, then, it is worthwhile to note the relationship between scope and *conjunction* (as an aspect of external consistency). For when special relativity arrived on the scene, Maxwellian electrodynamics proved - miraculously, as some realists would accuse their opponents of saying - to be quite compatible. Yet, as alluded to earlier, the discovery of Pluto, and its inclusion in astrology, did not serve to make astrology any more compatible with numerology, or religious doctrine, or any of the other non-sciences. That is, even though it might be argued that astrology demonstrated *explanatory* scope, since it was found that it could supposedly account for more societal changes than its creators had originally envisaged.

This seems to suggest that empirical scope, in science, is quite different from a looser notion of 'explanatory scope', and that a theory in non-science may have a great deal of the latter, while having none of the former. Henceforth, then, I shall write only of *empirical* scope in the demarcation context.

Simplicity - A *scientific* theoretical framework will not usually posit any entities which are not strictly necessary in order to account (in predictive terms) for the phenomena in its domain.

In its most commonly accepted philosophical sense, the simpler of two theories would postulate the existence of fewer entities, while maintaining the same descriptive power as its competitor. This idea, first put forward by William of Ockham, is a key argument in favour of nominalism, when compared to metaphysical realism. In the arena of science, however, the situation is less clear; as Laudan argues: 'Notoriously, one man's simplicity is another's complexity'.[156]

Nonetheless, there is a very clear sense in which a scientist would not want to state that quarks have another thirteen flavours, which have no predictive consequences, in addition to isotopic spin, strangeness, charm, beauty, and truth.[157] And by contrast, the introduction of 'demons in hell' in a theological context seems to have little predictive interest - in *life*, at least. Certainly, it is possible that there are 'demons in hell', but one might legitimately point out that such a claim can not be presently judged in comparison to the one that 'there are pink elephants, and fluffy bunnies, in hell.' Lucifer might even be

[156] Laudan [1990], p.339

[157] Isotopic spin can be viewed as a flavour for up and down quarks.

an animal lover, especially in the case of serpent-kind![158] (After all, Adolf Hitler was a vegetarian.)

This said, though, considerations of simplicity often *seem* to give way to those of explanatory value, even in science. For example, some physicists might be seen as preferring Bohm's interpretation of quantum mechanics for its superior explanatory value, even though it introduces a 'quantum potential' which is not found in the Copenhagen interpretation. However, I think that such a view is fundamentally misguided, just because introducing a 'quantum potential' does away with the need for wave-function collapse (a process posited by the Copenhagen interpretation); hence the proper way to view these competitors is as *equally simple*, at the very least.[159] In either case, anyway, the underlying mathematical structure of quantum mechanics remains the same; Bohm just chose to express the wavefunction in polar, rather than rectangular, form.[160]

Naturally, individual scientists can and *do* have religious and spiritual beliefs, and undoubtedly some of these might be adapted to play a role in explaining scientific results. However, I think it highly implausible that a science journal would publish a paper which suggested that particles are more good (in a moral sense) than waves, or that pulsars are angels, unless either of these statements were inextricably bound-up with a theory that had new predictive consequences (visible-in-principle to the community, which consists of *living* scientists, that is).[161]

Fruitfulness - A *scientific* theoretical framework will often draw attention to new phenomena, and reveal links, previously hidden, between phenomena that are already known.

In the first sense - drawing attention to new phenomena, or what I shall call *sensible disclosure* - the virtue of fruitfulness is tightly bound up with the idea of *predictive novelty* (due to Lakatos), which was discussed toward the beginning of this section. In addition to the examples given there (of barrier penetration, etc.), one might add red-shift, as predicted by general relativity. When we compare astronomy to astrology, we notice that it was the former

[158] I refer to the hypothetical serpent in the hypothetical Garden of Eden, of course.
[159] It might be argued, further, that Bohm's interpretation is actually *simpler* than the Copenhagen one, because it avoids complementarity, but I shall not make that case here.
[160] From Euler's formula, $e^{i\theta} = \cos\theta + i\sin\theta$, it is clear that any complex number (and hence function) can be expressed in either rectangular or polar forms, $z = x + iy$ [rectangular form] = $r(\cos\theta + i\sin\theta) = re^{i\theta}$ [polar form].
[161] More properly, such metaphysical claims would only need to *appear* to be inextricably bound-up with the predictive power of theory. That is, in light of the current status of the debate in the community.

which has proven more fruitful, because it has disclosed Neptune (a sensible thing).[162]

In the second sense - revealing previously unknown links between known phenomena, or what I shall call *relationship disclosure* - the Newtonian framework is an excellent case in point. This accounted for Kepler's laws governing celestial motion, the swing of the simple pendulum, the path followed by terrestrial projectiles, and even the movement of the tides, all in terms of one common type of force. Of course, it is noticeable that there is a link between this aspect of fruitfulness and *scope*, for a theory with maximal scope (e.g. a theory-of-everything) would necessarily also disclose a new relationship (if only in a bare mathematical sense, which would require subsequent interpretation) between all known phenomena.

Theoretical Virtues in Application

Although it will not be possible for me to apply these virtues to various disciplines in depth, I envisage that doing so would pick out, broadly speaking, four different groups of enterprises:

i) Mature Sciences

ii) Partial Sciences

iii) Potential Sciences

iv) Non-Sciences

First, the mature sciences, including chemistry and physics, have a long history of predictive success, with impressive precision - they have demonstrated good *accuracy*. They have productive research areas that are interdisciplinary, such as physical chemistry, and their theories can often be conjoined successfully (e.g. The Periodic Table and Quantum Mechanics) - they are externally *consistent*, each with the other. They have compact yet powerful theoretical frameworks, which demonstrate a wide range of applications - viz. have good *scope* and *simplicity*. And finally, they draw our attention to that which we might never have noticed, without their influence - they are *fruitful*.

Second, the partial sciences, such as psychology and economics, have certainly had some predictive successes, but only in certain domains - they

[162] Sometimes, sensible and reproducible *constant conjunctions of events* are disclosed; this is a good way to think of the quantum tunnelling example. (Remember, here I want to avoid belief-talk about theoretical entities, to keep my demarcation discussion neutral on the question of realism.)

have limited accuracy. And conjoining theories across disciplines might often lead to the compounding of errors, rather than any useful insight (e.g. in combining psychology with macro-economics, to construct a micro-economic theory) - they have limited external consistency. They do, in some cases, have excellent scope (in theory of perception, for example), but this is often parasitic on mature sciences (e.g. biology, chemistry and physics, which provide background theories on photoreceptors, light, the ear, sound, etc.) Their theoretical frameworks *are* usually simple, but only *because* their accuracy is limited. And they can be fruitful in terms of both sensible disclosure and relationship disclosure, but again only in a few domains (e.g. experimental psychology).[163]

Third, the potential sciences, such as astrology, are notoriously inaccurate; enough so, indeed, to rule them out as 'partial sciences' at this very first hurdle. Yet at their core, there are hypotheses which are, in principle, open to empirical investigation; in astrology, one such hypothesis might be that there is a relationship between the career of an individual and his astrological house. Of course, given modern technology, it becomes quite feasible to investigate such a claim, and although it might turn out that there is some 'truth' in it, we will certainly find that the relationship is not the same as that which the astrologers had guessed at beforehand.[164] Astrology is not scientific, then, just because of the way that it is currently practised; yet its practitioners might aspire to more, by adopting a different methodology.

Fourth, and finally, the non-sciences, most notably 'creation-science' and Kantianism, are dogmatic doctrine, just because they have no empirical content whatever. No matter what happens, the creation-scientist can always turn around and say "Yes, I know it *appears* that I'm wrong, but I'm not. God is just testing us." Likewise, the Kantian can point out that the core of his transcendental theory cannot be refuted by any experience, because it already presupposes a certain relationship between the perceiver and the perceived!

Now all this points to the important break between science and non-science happening somewhere within those areas which I have classified as 'partial sciences', and I think there is every reason to believe that it is a purely methodological one. For, as I have argued, even astrology might aspire to a scientific status, were the correct method of inquiry to be adopted.

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[163] Compare Jungian dream analysis, which is highly speculative, with the work of von Helmholtz, Necker, and Ames (of Ames room fame). See Gillies [1993], pp.140-146

[164] When Gauquelin compiled the careers and birth times of twenty-five thousand Frenchmen, he found that there was no relationship between house and career - but he did find some relationships between the position of certain planets and career. See Thagard [1978], p.30

2.2 SCIENCE AS A TWO-TIERED ENTERPRISE: NORMATIVE METHODS AND AUXILIARY METHODS

"Only when they must choose between competing theories do scientists behave like philosophers."
(Kuhn [1965], p.7)

In considering scientific methodology, the philosopher is faced with two important tasks. On the one hand, she must account for the fact that science is a unique activity, which produces results that are quite unlike those in any other area of human endeavour. The nature of these results, which bring us so much predictive and technological power, suggests that there is a logic of scientific discovery. Yet on the other hand, she must recognise that science is practised by a wide range of individuals, with differing goals and personal approaches to their work. This suggests that the psychology of scientific research might also be a relevant consideration.

Now when we turn our thoughts, cursorily, to science, we tend to think of the 'greats': Galileo, Newton, Darwin, Dalton, Maxwell and Einstein. That is, we are drawn to the glamour, the putative 'genius', of those that have not only overthrown a certain way of seeing the world, but also replaced it with a new, and certainly more productive, world-view.[165] When we think of the 'discovery' of the nucleus in the atom we think of Rutherford, the great theoretician. But how often do we pause to consider the work of his students, Geiger and Marsden, who actually *performed* the experiments with alpha-particles and gold foil? Are our untamed thoughts not just as distorted as those who think of the battles in North Africa, during W.W.II, as a contest between just two men, Rommel and Montgomery? And is it not only at our peril that we should forget the tens of thousands of men on the ground, without which, no battles would ever have been fought? In essence, this is the criticism that Kuhn levels at Popper:

'Sir Karl has characterized the entire scientific enterprise in terms that apply only to its occasional revolutionary parts. His emphasis is natural and common: the exploits of a Copernicus or Einstein make better reading that those of a Brahe or Lorentz... [but] it is for the normal, not the extraordinary practice of science that professionals are trained... If a demarcation criterion exists (we must not, I think, seek a sharp or decisive one), it may lie in just that part of science which Sir Karl ignores.'[166]

Indeed, I do not remember ever sitting a physics course entitled "209: How to Question the Status Quo", or "310: Breaking the Boundaries of Modern Physics: How to Bring about a Scientific Revolution", and I think that this is rather unsurprising. Can we imagine a modern scientist who, having failed to build a rocket which succeeds in achieving escape velocity, would begin to

[165] Ironically, the same is true in philosophy, and the revolutionary nature of Kuhn's work, in itself, serves to partially explain its widespread popularity.
[166] Kuhn [1965], p.6

question the validity of the principle of conservation of energy? I would say not. For, on the contrary, the vast majority of scientists - those responsible for constructing ever faster computer processors, designing ever more efficient data-storage media, and testing ever faster aircraft - need to assume that the theories which they are *given*, those of modern science, are sufficient to solve the puzzles that they are faced with. And only if those theories begin to fail in a wide variety of applications, only if the scientific community is faced with a critical-mass of anomalous results, does it become necessary to re-examine those theories which are generally accepted, and have proven to be extremely useful in the past. That, Kuhn claims, is when normal science ends, and *extraordinary* science begins: 'A failure that had previously been personal may then come to seem the failure of a theory under test.'[167]

However, in interpreting the previous quotation, I believe it is of vital importance to understand that 'come to seem' should read as 'come to seem, *in the eyes of the wider community*'. And I join those, such as Watkins, who recognise that although something like disciplinary matrices do exist, they simply do not have the sort of monopoly that Kuhn insists they do. If they did have, then Bohm's work in the early fifties would have to be considered *unscientific* (as would the work of Everett on relative-states, Wigner on subjectivism, etc.) since it could hardly be said that the formalism of non-relativistic quantum mechanics had then encountered serious problems in application. So I wish to claim that scientists can be a little more *individual* than Kuhn gives them credit for, insofar as the community of scientists will always contain those who try to attack the status quo (the ruling disciplinary matrices), when their personal philosophical predilections are at odds with doctrine. As Watkins writes:

> 'It seems that a dominant theory may come to be replaced, not because of growing empirical pressure (of which there may be little), but because a new and incompatible theory (inspired perhaps by a different metaphysical outlook) has been freely elaborated: a scientific crisis may have theoretical rather than empirical causes... there is more free thinking in science than Kuhn supposes.' [168]

This said, I do *not* want to concede that normal science, as Kuhn explains it in *methodological* terms, does not happen, and is not of vital importance for science. It is certainly curious of Popper to admit that ''Normal' Science, in Kuhn's sense, exists'[169], but claim that it is 'a danger to science and, indeed, to our civilisation.'[170], while simultaneously maintaining that 'scientific method does not exist'.[171] Unless, that is, Popper's objection is purely on moral grounds, viz. that it is wrong for large groups who are in agreement to stifle those who disagree, which do not concern me here. Kuhn was not, I think,

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[167] Ibid., p.7
[168] Watkins [1965], p.31
[169] Popper [1970], p.52
[170] Ibid., p.53
[171] Popper [1983], p.5

merely trying to prescribe behaviour for scientists; his intent was to look, rather, at what methods the scientific community prescribes to its students, and its members.[172] These are the *normative methods* of which I wish to write, and they are *normative* not in the sense that I think they are the best way to practice science, but in the sense that the community actively encourages its members to use them. This is because *they yield great practical benefits*, and we have the technological evidence, the evidence of our senses, to back up such a claim.[173]

Besides, in the final analysis, it seems quite obvious that any scientist who wishes to criticise a theory which is widely accepted by his community must also understand what it is to do 'normal science' very well, and for two distinct reasons:

a) Else, he never would have made it into a position where his peers would put any trust in his words. (He would just be thought of as a crank.) Indeed, he would not even understand the special theory-infected *language* that the scientific community employs.

b) In criticising any given theory, it will still be necessary to accept doctrine in other domains of the science. For example, when Bohr constructed his model of the atom, he did not suddenly 'throw away' the idea of electrons, or claim that the charge on such theoretical entities (or constructs) was radically different from the approximations made beforehand. And when Einstein put forward his special relativity, he did not throw out Maxwellian electromagnetism along with Newton's laws of motion. Indeed, if anything, Maxwell's framework was later viewed as lending *support* to the theory of special relativity.

Normative Methods and Normal Science – Logic of Discovery

In answering the question "What does normal science consist of, methodologically?", Kuhn draws a rather sharp distinction between two areas of endeavour, 'theoretical activity...and fact gathering.'[174] However, since I think it best to make his claims consistent with the idea that observation is theory-laden, I shall deviate from his presentation, instead accepting that 'facts and theories...are never as neatly separated as everyone makes them

[172] According to Kuhn, 'the descriptive and the normative are inextricably mixed'. See Kuhn [1970], p.233 + 237.
[173] Note that it is enough, for this argument to go through, that science constructs theories of increasing nearness to empirical adequacy.
[174] Kuhn [1996], p.31

out to be.'[175] I shall treat three areas of activity, not necessarily unrelated, that constitute the work of the normal scientist.[176]

The first, which I would call *classification and prediction,* involves determining numerical information that will be useful for prediction, according to the ruling paradigm, and then slotting this information back into its theoretical framework. For example, discovering the Young's modulus, coefficient of thermal expansion, and yield strain of a new metal (*classification*) would be beneficial to an engineer; armed with facts such as these, he could assess which applications the material was suitable for (*prediction*). This said, however, there are certain facts gathered within this domain which do not appear, *prima facie,* to have any practical value. For example, Kuhn cites data within astronomy, namely stellar position and magnitude, which appear to have been gathered for purely academic interest.[177] Still, I would argue that such knowledge has been used for predictive purposes on many occasions; consider navigation at sea by the use of a sextant and an astronomical chart, or the use of Hubble's notoriously imprecise 'law' in order to predict the velocities of receding galaxies.

Second, there is what I call *experiment-theory comparison and application extension.* On the factual side of this domain of normal-scientific investigation, there is the search for new evidence which can further bolster the paradigm; new apparatus, or a novel experimental set-up, is devised in order to demonstrate the agreement of theory with nature itself. In reciprocation to the theory, these experimental comparisons then draw attention to the approximations that had to be made in order to 'demonstrate' the aforementioned agreement. An excellent example is Newton's attempt to derive the equations governing the motion of a pendulum from his law of gravitation. As Kuhn correctly points out, Newton needed to assume a point mass for the pendulum-bob, as well as disregarding the effect of air resistance. [178] Moreover, from a modern perspective, it is noticeable that he assumed a small angle of swing in order to simplify his proof.[179] If one were to be truly pedantic, then one could also draw attention to several other factors neglected: the properties of the rod bearing the bob, the non-uniformity of the gravitational field, and the friction at the bearing. Such glaring omissions drive theoreticians to devise more elegant and powerful proofs, viz. theories which incorporate more factors, to extend the

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[175] Since I think there is no harm in making this concession to the Popperian camp (and other anti-realist positions, such as van Fraassen's, which involve theory-ladenness), it seems best to make it. Quotation from Feyerabend [1993], p.51

[176] Note that throughout the following discussion, I will only use the word 'paradigm' in the 'disciplinary matrix' sense, viz. as referent to 'a Constellation of Group Commitments'. See Kuhn [1996], pp.181-187

[177] Kuhn [1996], p.25

[178] Ibid., p.31.

[179] Sin $\theta \approx \theta$, when θ is small. This approximation allows one to treat the motion of the pendulum as being 'simple harmonic'.

applicability of the paradigm. As Duhem put it, sixty years before Kuhn, 'The degree of approximation of a law, though sufficient today, will become insufficient in the future'.[180] In the case of pendulum motion, we now have theories to describe both damped and forced oscillations; we can also consider the properties of the rod, and even explain oscillations involving large angles of swing. It is important to note that these sophisticated theories, in line with their predecessors, do not question the validity of the Newtonian paradigm.[181]

The third and final domain, to use Kuhn's terminology directly, is that of *articulation*. On the quantitative side, this involves the use of experiment to determine physical constants such as Planck's constant or the permeability of free space; other experiments may also be performed with the aim of discovering quantitative relationships such as the Ideal Gas law, or Faraday's law of induction.[182] On the qualitative side, there are experiments designed to examine aspects of the paradigm which are unexplored, or somewhat ambiguous. It is eminently reasonable to believe that, in both cases, such activity would not come about without theories, based on a widely accepted paradigm, to guide the experimenter; in fact, Kuhn boldly states: 'More than any other sort of normal research, the problems of paradigm articulation are simultaneously theoretical and experimental'. [183] To add cogency to this argument about *articulation*, let us consider a 'normal science' account of the discovery of non-locality, which builds upon my earlier discussion of quantum theory towards the beginning of the first chapter:

Given formalisms with clear predictive successes, theoreticians such as Bohr, Heisenberg, and Born, set about interpreting them.[184] And in response to some of the counter-intuitive results of the paradigm, which in this case I would describe as 'quantisation', they advocated a standpoint that was agnostic about (or perhaps even dismissive of) claims of the value of certain non-commuting variables, such as spin, before they were measured. Yet nonetheless, other theoreticians continued to question implicit problems (or puzzles), and a case in point is non-locality. Bell's theorem, following in the footsteps of the famous EPR paper[185], and Von Neumann's flawed locality theorem, predicted the data which would be obtained if quantum entities were local. Subsequently, Aspect, and more recently Kwiat et al., performed

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180 Duhem [1954], p.178.

181 I disregard relativistic considerations, here, since they are irrelevant in principle. Many of the developments to which I refer happened *before* 1905, and there is no reason to believe they would not have continued as they have, even if special (and general) relativity had not yet been proposed.

182 As Williams points out: 'there could be no better demonstration of 'normal science' than the experimental researches in electricity of Michael Faraday'. Williams [1965], p.50.

183 Kuhn [1996], p.33.

184 Specifically, I refer to the Schrödinger equation, Heisenberg's matrix mechanics, and Dirac's q-number/c-number theory.

185 Einstein, Podolski, and Rosen. *'Can Quantum Mechanical Description of Physical Reality be considered complete?'*, in Physical Review **47** 777.

experiments which demonstrated that Bell's inequality was violated; 'spooky action-at-a-distance' is, by most, now regarded as a genuine consequence of the quantum formalism, even though the Copenhagen Interpretation did not explicitly deal with it.[186] Thus, Kuhn could say that the paradigm of 'quantisation' (or the 'disciplinary matrix of quantum mechanics') was articulated. When he says that *articulation* 'can [only] resemble exploration', however, his anti-realist colours begin to show, and I want to leave the question of whether it is *really* exploration up-in-the-air.[187] What *is* clear is that it is progressive in a predictive sense, which is - as I have already argued earlier in this chapter - not only what makes it scientific, but also what makes it worthwhile in the eyes of the scientific community.

Overall, then, what strikes one about these *normative methods* is that they are, on the whole, pretty boring. The progress is painfully slow, the work is often repetitive, and the discovery of anomalous results that are generally agreed upon (as being the flaws of theories, rather than inept scientists) might take many years. (The Mpemba effect is an excellent case in point.[188]) But ultimately, the phenomena win out even though experiment and theory are symbiotic, and any given theory might eventually need to be jettisoned; this is when the critical comparison between new competing theories, *extraordinary science*, comes into play. Of course, none of this will stop certain individuals (and these will not be the majority) from bucking the trend, and actively attacking certain disciplinary matrices (in certain domains of science), while committing to belief in (or at least full acceptance of) others.[189] And I would suggest that this is *as it should be*; in the words of Kuhn, 'Revolutions through criticism demand normal science no less than revolutions through crisis.'[190]

I will now move on to discuss *auxiliary methods*, which play a major role in *extraordinary science*, viz. in theory-choice and theory-construction (heuristically). It will also be argued that they have a limited role in *normal science* - insofar as they might sometimes affect the specific techniques that a scientist might apply *in order* to solve any given puzzle. It is pleasing to find that this follows on from where Kuhn leads, when he writes that:

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[186] For the Kwiat experiment, see Internet URL - http://p23.lanl.gov/agw/2crystal.pdf
[187] Kuhn [1996], p.29.
[188] Mpemba was a Tanzanian student who, in 1969, was responsible for the physics community's 're-discovery' of the fact that hot water can sometimes freeze faster than cold water (at the same pressure). Initially, he was ridiculed by his teacher, as no doubt he would have been by most physicists of the time, even though Aristotle, Bacon, and Descartes had all previously mentioned this curious phenomenon. Moreover, as Jeng points out: 'the effect was known by laypersons around the world long before 1969.' See 'Can Hot Water Freeze Faster than Cold Water?', by M.Jeng, at Internet URL - http://www.weburbia.com/physics/hot_water.html
[189] This is necessary in order to make an admissible attack, as explained under point b) at the end of the preceding sub-section.
[190] Kuhn [1970], p.233

'There can be no set of rules adequate to dictate desired *individual* behaviour in the concrete cases that scientists will meet in the course of their careers... Shared ideals affect behaviour without making those that hold them ideal...it may be vitally important that different individuals decide in different ways. How else could the group as a whole hedge its bets?' [emphasis in the original][191]

Auxiliary Methods and the Individual Scientist - Psychology of Research

In Normal Science

When the young normal scientist has finished his schooling, he will be equipped with a good knowledge both of contemporary theories, and of the mathematical techniques necessary in order to solve puzzles that involve the use of those theories.[192] And if he wishes to become a professional scientist, then his first move will be to decide which specific domain of his science it is that most appeals to him. Of course, his philosophical predilections will influence this choice, practical considerations notwithstanding - the choice of *which type of puzzles* he wants to tackle.

Prima facie, it might not seem that this is an important *methodological* issue. For while the realist might want to work in quantum gravity because what it currently suggests to him about 'the real world' is just too incredible to swallow, the anti-realist might want to work in the very same domain just because its current formalism seems to be unnecessarily complex.[193] But it might still be argued that there are certain circumstances, albeit rare ones, in which an individual (or small group) might see a certain area of their discipline as posing worthwhile puzzles, while the majority might not. For example, the anti-realist might consider that a predictively successful formalism, with observational consequences that are sufficient for all present practical purposes, needs no further *qualitative articulation*- instead he might begin to work on its *quantitative articulation*. On the other hand, the realist might want to see how the formalism could be better understood in terms of correspondence to the world. And this, one might claim, is how it was made explicit that the non-relativistic formalism of quantum mechanics is, strictly speaking, compatible with both indeterministic and deterministic views thereof.[194]

However, I think that this line of argument is misleading, for it confuses the *puzzles set by normal science*, those generated by acceptance of (or belief in)

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[191] Ibid., pp.238-241
[192] This is where *shared examples* (or exemplars) play an important role in the disciplinary matrix. See Kuhn [1993], p.187-191
[193] Or that the explanations it offers are not of sufficient *practical* value.
[194] Popper's view is an example of the former, whereas Bohm's is an example of the latter. As Cushing puts it: 'In the end, we may be left with an essential underdetermination in our most fundamental physical theory and this issues in an observational equivalence between indeterminism and determinism in our world.' See Cushing [1994], p.214

established theories, with what the individual may perceive as *problems* with those theories. That is to say, I do not think that what Bohm did in the early 1950s can be properly understood as *articulation* of quantum mechanics within the realms of normal science. Rather, it points to a belief on his part that the period of *extraordinary science* which generated the Copenhagen Interpretation had ended too soon. Thus, he put himself into the mind-set of the *extraordinary scientist*, with the goal of generating a theory that might have been accepted by the community in place of the Copenhagen one, had it been placed on the table in the late 1920s. And any *auxiliary methods* he employed, in constructing his theory of non-relativistic quantum mechanics, were only due to his deliberate choice to act as an *extraordinary*, rather than *normal*, scientist. Historically, this argument is supported by the fact that Bohm started out by writing a book, *Quantum Theory*, which was designed to elucidate Bohr's views on quantum mechanics, and only began to question his approach *after* meeting with Einstein. As Bohm put it: 'This encounter with Einstein had a strong effect on the direction of my research, because I then became seriously interested in whether a deterministic extension of the quantum theory could be found.'[195] Further, it is clear that Bohm spent considerable time investigating the arguments that took place during the period of crisis in the late twenties, for he went on to write:

'it occurred to me that if de Broglie's ideas had won the day at the Solvay Congress of 1927, they might have become the accepted interpretation; then, if someone had come along to propose the current interpretation, one could equally well have said that since, after all, it gave no new experimental results, there would be no point in considering it seriously. In other words, *I felt that the adoption of the current interpretation was a somewhat fortuitous affair, since it was affected...by the generally positivist empiricist attitude that pervaded physics at the time.*'[196] [emphasis mine]

Thus, I want to claim that *auxiliary methods* are not relevant to the choice of which puzzles an individual will tackle, but they are relevant if an individual begins to perceive that there are *problems* with an established theoretical framework (not just in the way that it has been applied). In the latter case, such an individual would effectively be deciding to act *as if* he were in a period of *extraordinary science*, which will be treated in the next sub-section.

This said, however, there may still be another fashion in which *auxiliary methods* might creep into *normal science*; in the *means by which* a given individual chooses to tackle a recognised puzzle.

In chess, for example, strategic considerations dictate that control of the centre of the board is an important goal - and the puzzle "How do I gain control of the centre?" is one that a good chess player will try to solve in the first phase of every game (the opening). Some prefer making a quick grab for it, and if

[195] Bohm [1987], p.35

[196] In addition, he also mentions that he spent time considering Pauli's original objection to de Broglie's guiding-wave idea; Pauli sent this to him. Ibid., p.39

both players opt for such tactics then an opening such as the Queen's gambit, Ruy Lopez, or Centre Counter is likely to occur.[197] But others prefer to allow their opponent to occupy the centre, in the hope that he will over-extend and allow a swift counter-attack; this is the Black player's intent in more passive systems such as the Modern and Pirc defences.[198] Of course, at our present level of knowledge about chess, it is not immediately clear which approach (say for Black) is better, although there is almost certainly a truth of the matter which will be discovered when we have a computer of sufficient power to analyse all possible games. And might this not suggest that there could be certain puzzles in normal science to which the realist is better suited than his anti-realist counterpart (and vice versa)?

Given the puzzle "What is the charge on an electron?", it is hard to see why an anti-realist would have performed the Millikan oil-drop experiment, since it is conceived in terms of capturing a real discrete 'thing' (rather than a theoretical construct), and examining it. Conversely, given the puzzle "What are the relative rates of electromagnetic and weak interaction particle processes?", it is difficult to see why an ardent realist would posit theoretical entities such as virtual particles, when 'it is meaningless to argue whether or not they are there...As any attempt to observe them changes the outcome of the process.'[199] In short, there are many ways to break an egg, and this is the very essence of *auxiliary methods*.[200]

In Extraordinary Science

When a critical mass of anomalies confronts a theory, or when an individual embarks upon a personal critical crusade against a generally accepted theory (e.g. Bohm, against orthodox quantum mechanics), *normal science* (for the community or individual, variously) ends. Theory-construction, and theory-choice (between competing theories that are constructed), viz. those activities which constitute *extraordinary science*, begin in earnest. And *auxiliary methods* will, inevitably, begin to be employed by the scientist.

In both theory-choice and theory-construction, and it is clear that these two are intertwined, I would follow Zahar in saying that '*ontology may be taken to have prescriptive import.*'[201] But in the context of realism vs. anti-realism, it is

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[197] Queen's Gambit: 1. d4, d5; 2. c4 Ruy Lopez: 1. e4, e5; 2. Nf3, Nc6; 3. Bb5
Centre Counter: 1. e4, d5

[198] Pirc Defence: 1. e4, d6; 2. d4, Nf6 Modern Defence: 1. e4, g6; 2. d4, Bg7

[199] Internet URL - http://www2.slac.stanford.edu/vvc/theory/virtual.html

[200] I do not want to preclude the possibility that different methods might, ultimately, lead to highly similar (or even identical) results. However, it might take a long time for such 'levelling out' to occur, even assuming that it can and does.

[201] They are intertwined because an individual who prefers theories with certain qualities will attempt to ensure that any new theory which he constructs also possesses those same qualities. Quotation from Zahar [1989], p.22

more important to recognise that it is *whether* one works to an ontology, or not, which is the issue at hand. For a hard-headed anti-realist, who rejected the need for ontic explanation in science, would have no reason to care what his theory might suggest to a realist. Instead, his only concern would be to save the phenomena in the 'neatest' fashion possible. For one such as Duhem, for example:

> 'A physical theory…is a system of *mathematical propositions deduced from a small number of principles, which aim to represent as simply and completely as possible a set of experimental laws.*'[202]

It is important to remember that Duhem was not just adopting a peculiar form of empiricist metaphysics; he was not claiming that there is nothing beyond our perception to speak of. Instead, he just believed that science could progress without appeal to ontology - that the scientist would do best to adopt an *agnostic* stance about the suprasensible, and to avoid entertaining any speculative metaphysical thoughts.[203] Of course, this is perfectly compatible with the choice to construct (and prefer) theories which satisfy the demarcation guidelines led out in section 2.1 of this chapter; for, as I argued there, they might be preferred on purely *pragmatic* grounds.

The realist, on the other hand, would likely think that what a theory says or implies on the metaphysical level is a vital consideration.[204] And just as Einstein was driven to reject the Copenhagen Interpretation of Quantum Mechanics because it was observer-dependent, indeterministic, and apparently non-local (theory-choice), de Broglie and Bohm were motivated to construct competing theories which obeyed as many of those constraints as possible (theory-construction). Thus, while the demarcation guidelines (2.1) might be thought of as relevant considerations for both the realist and the anti-realist *extraordinary scientist*, the realist considers something extra - specifically, the *metaphysical content* of a theory. In the eyes of the realist, this need *not* be what demarcates science from non-science, but it might well be that which demarcates good science from bad science.

In short, when the realist constructs a new theory, he wants it to suggest that the theories which it supersedes were *approximately true* (in correspondence to the actual metaphysical world) - that his new theory has added resolution to a basic, but essentially correct, ontological picture. When the anti-realist constructs a new theory, he only wants it to demonstrate that the theories which it replaces were *approximately empirically adequate* - ontological continuity be damned, for it is only saving the phenomena which counts.

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202 Duhem [1954], p.19

203 See the discussion of conventionalism in Chapter 1, section 1.1.

204 This is because he links the explanatory parts of theories with their empirical success - the conventionalist, structural realist, or constructive empiricist, would sever that link

Throughout the foregoing discussion, I have left the specific details about *auxiliary methods* essentially vague, and this was quite intentional. For they are legion, and rooting out all their intricacies would require in-depth analyses of many historical episodes.[205] Yet I still wish to offer a few examples of the heuristic tools that a theoretician might employ in theory-construction, and how these might differ, in both application and importance, for the realist and anti-realist:

i) *The Correspondence Principle.* This principle, which has already been alluded to in my earlier discussion of the theoretical virtue of external consistency (2.1), says that a new theory should reduce to its predecessor (in the same domain of science) under the correct conditions. To reiterate my earlier example, the deducible consequences of special relativity reduce to those of classical (Newtonian) mechanics, as $v/c \rightarrow 0$. As Zahar correctly points out: 'this is also how Newtonian gravitation, though strictly incompatible with Kepler's laws, is nonetheless based on them.'[206]

However, another of Zahar's examples, that of $h \rightarrow 0$ in quantum mechanics leading to its correspondence with classical mechanics (this is how Bohr followed the correspondence principle) is much more interesting, because this is something that realists have disputed. And I believe that the further examination of this dispute, which follows, will reveal how realists might apply the *Correspondence Principle* differently to their anti-realist counterparts.[207]

Now in comparing v/c with h, the first difference is that the former quantity is dimensionless, whereas the latter *does* have dimensions, specifically Js or kgm^2s^{-1}. So when we consider Planck's law, $\varepsilon = h\nu$, if h were literally to hit zero, then there would be no relationship between the energy of a photon and its frequency (or wavelength, from $\varepsilon = hc/\lambda$) whatsoever. Analogously, if we imagine G (the gravitational constant) reaching zero, then there would be no relationship, period, between the gravitational field emitted by a body and its mass.

The second difference is that h is a constant, rather than a variable (such as v), so it is difficult to see, from a realist perspective, how it could tend towards anything other than its actual value, in this actual world. Analogously, it would seem very strange to speak of π (a dimensionless example) as tending to anything other than that which it *is*. Certainly, one does not spend one's time attempting to draw circles

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[205] Analyses of the sort performed by Zahar, in his 'Einstein's Revolution'.
[206] Zahar [1989], p. 19
[207] I will remain neutral on the question of whether there is a *right* way to employ it, viz. a way that is best for science. I do, however, contend that both anti-realists and realists can use something like it, in order to construct more virtuous (consistent) theories.

with a circumference that is not equal to πd, since it is impossible by definition. And while much experimental effort has been devoted to finding ever more accurate values for G, none has ever been directed toward trying to somehow 'change it'.[208]

In light of these comparisons, the obvious move for one with realist sympathies is to accuse Bohr of making a rather foolish error, in both mathematical and indeed empirical terms. But while I think that the foregoing mathematical argument is quite legitimate, insofar as v/c tending to zero in special relativity is *not* analogous to h tending to zero in quantum mechanics, I think it fair to suppose that Bohr was aiming for a different, empirical, notion of correspondence.[209] That is to say, I think that Bohr wanted to offer an answer to the question, "Why didn't we realise earlier that classical mechanics is, and was, empirically inadequate?", rather than questions such as, "How do we show that classical mechanics, properly construed, is a special case of quantum mechanics? That classical mechanics is *approximately true* in the correct parameter limits?"

So with his loose talk of Planck's constant tending to zero, I contend that Bohr was attempting to express a proposition such as "If we don't factor h into our equations, viz. if we assume that the values of dynamic variables are continuous rather than discrete, then we arrive at classical mechanics. And if h had been many orders of magnitude higher in the actual world, then we could have easily recognised that the velocity of a tennis ball, say, is quantised." Of course, the scientific realist will find such an account lacking in one important respect, specifically that it does not justify the claim that classical mechanics was *approximately true* (Thesis E), but the constructive empiricist or conventionalist will find it quite pragmatically satisfying. Certainly, it will suffice as a means of explaining to physics undergraduates that relationships between phenomena are often more subtle than one would appreciate in everyday life, and will justify the claim that classical mechanics was *approximately empirically adequate*.

Yet Bohm, who *was* searching for some means of showing that classical mechanics is just an approximation to a more verisimilar theory, namely quantum mechanics (in a certain experimental domain), needed to adopt a different heuristic strategy. He sought, instead, to find a *variable* which could be thought of as literally reaching zero, and

[208] This is *not* to say that it will not spontaneously change, say at 9:00 a.m. tomorrow. But if it did, at least by any significant fractional margin, it would be immediately noticeable – the Earth's orbit about the Sun would change, as would the weight of a kilogram mass on Earth, etc.

[209] After all, it would be curious to accuse Bohr of being a poor mathematician, given his success in theory-construction.

genuinely yielding a classical result, from a realist perspective. He achieved this by forging the equation $d\mathbf{p}/dt = -\nabla(\mathbf{V} + \mathbf{Q})$ in his mechanics, which reduces to the expected classical result of $d\mathbf{p}/dt = -\nabla\mathbf{V}$, as $\mathbf{Q} \rightarrow 0$.[210] Moreover, it is worth noting that the explicit non-locality in Bohm's theory also 'disappears' as the quantum potential tends to zero, which is a result that Bohr never managed to effectively explain in an equivalent (but anti-realist) fashion.[211]

ii) *Interpretation of Mathematical Entities.* As was suggested by the first part of my discussion of *The Semantic Thesis* (section 1.1), choosing which mathematical features of a theory should be taken to have real physical significance - and which parts of a theoretical framework should be taken *literally* - is a task which the semantic realist will take seriously. For example, consider the following two theories, which we would both recognise (from a modern perspective) as representing the same law, namely Coulomb's law of electrostatics:

$$F = k\,(q_1 q_2/r^2) \qquad \text{(Eq. 1)}$$

$$F/q_1 = q_2/4\pi r^2\varepsilon_0 \qquad \text{(Eq. 2)}$$

(where F is the magnitude of the force between two charged entities, q_n is the charge on entity n, and r is the distance between the two entities.)

Now, the first thing to note is that the two equations above are completely empirically equivalent; one could determine an extremely good value for k (Coulomb's constant) by experiment, irrespective of any belief in equation two. [212] Second, it is important to note that equation one is more structurally simple; I would have less to put into my calculator in order to use it.[213]

However, I want to contend that the realist would find equation two to be far preferable to equation one, since it is more amenable to the ontological interpretation that he would attach to other domains of

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[210] Where **V** is the classical potential, and **Q** is the quantum potential.

[211] However, he might well have said, following on from my interpretation of his statement about Planck's constant, "If we don't factor h into our equations, then there could be no superposition of states in the dynamic properties of an entity. Hence probability would not enter the theory, and non-locality would not be a feature thereof."

[212] I want to claim there is a possible world in which we could just have discovered the relationship depicted by equation one, and not made the jump to equation two; this shouldn't seem too unreasonable, as an indication that the two can be viewed as different laws. It is too quick, with hindsight, for us to say "Oh, it's just the same law written differently".

[213] In addition, in the Ockham's razor sense of simplicity, why should we want to introduce π? Where's the circle? Surely it's not strictly necessary, from a wholeheartedly instrumentalist perspective?

physics.[214] On the one hand, the mention of $4\pi r^2$, viz. the area of a sphere, is significant because it promotes the picture of an isotropic field that is 'emitted' from each entity (or of a prior non-divergent substance, disturbed by the presence of each entity). As the area of the sphere increases, so the field strength will decrease; thus, there is an analogue in the relationship between the intensity of light or sound from a fixed-power point source, and the distance that one is from that source. On the other hand, the use of ε_0 binds equation two together with Gauss' law (or Maxwell's first law), thus strengthening the idea that free space has a real property, called its permittivity. This, rather than us having two isolated constants (k & ε_0) which just help us to predict the relationships between phenomena in different situations.[215]

As against this approach, an instrumentalist's primary aim will be to tease out the mathematical relationships between sensible things in *whatever fashion possible*; he need not consider himself bound, in theory-construction, by any non-empirical considerations of what has gone before. And I contend that it is only *after* such a working mathematical relationship has been found - say between elements and the spectra which they emit - that he would be interested in couching it in terms amenable to those used in other domains of physics. He may even resent having to do this, although it is likely that such 'translation' is indispensable, in communicating a new idea to a community which is steeped in considerable tradition (viz. partial to an old disciplinary matrix, which will need to be replaced or altered). Certainly Bohr appeared to show some irritation at this process, as was suggested in Chapter One.[216] It seems likely that other non-semantic-realists, such as Duhem, Poincaré, and Mach, would have too.

iii) *Visual vs. Abstract Modelling*. In constructing a new theory, the realist will often try to think of the way that things are in the hidden or unobservable realm, in spatio-temporal terms. Or he might even ask what the geometry of space and the nature of time would really have to be like, in order for the phenomena to be as they are.[217] Of course, this latter type of thought was responsible, in part, for both of the greatest revolutions in mechanics. As Zahar writes:

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[214] Here, the realist is examining the relative external consistency of the two theories in an *ontological*, rather than pragmatic or empirical, sense.

[215] Admittedly, some anti-realists could also prefer equation two in a pragmatic respect, just because it is more structurally consistent with other laws in the same domain of physics. But they *need not*.

[216] See quotation referenced in footnote 10, in 1.1, *Instrumentalism vs. Scientific Realism - Two Illustrative Episodes*.

[217] Of course, this is what the Greek from which the word is derived, meaning 'land-measuring' or 'Earth-measuring', also suggests.

'In Einstein's words, geometry constituted one of the oldest physical theories... In the preface to his *Principia*, Newton treats geometry as a branch of mechanics, i.e. as a branch of physics.'[218]

Anti-realists, however, have traditionally thought of our geometry as more of a construct, necessary only to make sense of the world in anthropocentric terms; there are echoes of such an approach in Kant's views on space-time, and Hume's views on causality.[219] As was mentioned in section 1.1, Bohr thought that our 'classical concepts' are limited in applicability, but that we are nevertheless stuck with them.[220] And Poincaré, in a similar vein, thought that there were no empirical grounds whatsoever for expressing a physical theory (such as special relativity) in terms of non-Euclidean, rather than Euclidean, geometry:

'experience plays an indispensable role in the genesis of geometry; but it would be an error thence to conclude that geometry is, even in part, an experimental science...It is meant by natural selection that our mind has *adapted* itself to the conditions of the external world, that it has adopted the geometry *most advantageous* to the species: or in other words the most *convenient*... geometry is not true, it is advantageous.'[221]

Given this, and there are some links here to the interpretation of mathematical entities as mentioned above, I want to contend that a realist in the process of theory-construction is more liable to adopt a *visual* modelling strategy, while the anti-realist may be more amenable to an *abstract* modelling strategy. For, whereas Schrödinger attempted to explain quantum phenomena in terms of wave-like motion (something with which we have visual familiarity, from watching the ripples on a pond or the vibrations of the string on an instrument), Heisenberg preferred a more abstract, and purely algebraic, approach. According to van Fraassen:

'To be good, it is not necessary for a model to have all its elements correspond to elements of reality. What is needed instead is that the model should fit the phenomena it was introduced to model.'[222]

And following his own advice, he proceeds to introduce a modal interpretation of quantum mechanics which, in rigorous exposition, demands page after page of abstract quantum logic.[223] In van Fraassen's model, we find nothing quite as visually striking as the

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[218] Zahar [1989], p.34
[219] See Kant [1987], A49, p.188 and Hume [1748], section VII, parts 1 and 2.
[220] See also the discussion of Bohr's views about *semantic disturbance* in Fine [1996], pp.34-35
[221] Poincaré [1913], pp.79-80 & p.91
[222] van Fraassen [1991], p.16 Admittedly he adds the caveat that we also want our theories to be informative, in a pragmatic vein, but other empiricists need not accept the force of that claim.
[223] See van Fraassen [1991], pp.306-327

Bohmian depiction of the paths followed by particles in the two-slit diffraction experiment, due to his 'quantum potential'.[224]

I confess that it does seem rather curious for empiricists, given their emphasis on sense-data, not to want to fashion models that are more amenable to the inputs which we have on an everyday basis (and are couched in those spatial terms). But nonetheless they seem to prefer - perhaps ironically - to trust to obscure logical arguments.[225] Conversely, the realist wants to try to describe a model which we can envisage, and posit entities or processes that we could imagine touching, seeing, and feeling, were our sensory range to be extended.

Of course, there may also be other differences in the *auxiliary methods* employed by the individual which are not divisible along purely realist vs. anti-realist lines; for example, gender, culture or race may be other relevant considerations. Nonetheless, the notion that personal philosophical predilections *do* have an important (if not always decisive) impact on theory-construction and theory-choice does seem very sound. I have just tried to argue that these predilections actually lie along a realist-antirealist axis, *if only in one dimension*.

All that remains is to show how theory-choice and theory-construction are deeply bound, and to bring out how my views on demarcation closely relate to my views on methodology. I will also take the opportunity to provide a brief summary of this chapter, in order to cement my position in the mind of the reader.

2.3 A SUMMARY: THE RELATIONSHIP BETWEEN THEORETICAL VIRTUES AS DEMARCATION GUIDELINES, AND SCIENCE AS A 'TWO-TIERED' METHODOLOGICAL ENTERPRISE

"If I have a theory of how and why science works, it must necessarily have implications for the way in which scientists should behave if their enterprise is to flourish."
(Thomas Kuhn [1970], p.237)

With regard to demarcating between science and non-science, my antecedent argument was founded on the intuitive notion that it is *empirical success*, viz. the successful application of theory in practice (in predictive and non-ontic

[224] Vigier [1987], pp.176-177

[225] Which is to say, they are considerably keener on the application of *pure reason*, rather than extrapolation from the observable, than their empiricist colours might suggest. (This is at some, non-fatal, tension with their claim that we should consider our beliefs-in-sensible-things to be more epistemically secure than our metaphysical beliefs.)

explanatory roles), which is the salient feature of science. Add to this that science has a degree of *testability/tentativity*, and that it makes *novel predictions* which often prove to be *reliable*, and the spirit of the difference between science and non-science is captured.

Moving on to flesh out this difference, I then argued from a *descriptive* standpoint that theoretical frameworks (or domains consisting of multiple theoretical frameworks) in science are more virtuous, in terms of *accuracy, scope, consistency, simplicity* and *fruitfulness*, than those in non-science. And that, moreover, we might judge the status of a discipline by considering the overall virtuosity of the domains of which it consists. In other words, it is a *visible feature* of scientific disciplines that they contain a lot of virtuous domains, because those domains are composed of virtuous theoretical frameworks.[226]

Now, and I have not made this point explicitly before, theoretical frameworks in mature sciences become highly virtuous - in part - because the practitioners of those disciplines actively *work* to make them so. At least, they do in this day and age - although it is possible that the first successful theoretical frameworks (in terms of empirical success) just *happened* to partake of these virtues, and thus groups set out to create more theoretical frameworks with the same properties.[227] This is what the *normative methods* (handed down from generation to generation) which comprise *normal science* are all about. A theoretical framework is placed on the table (most do not care how it arrived there), and normal scientists work to make it *more virtuous*, for example by *experiment-theory comparison* and *articulation*. In order to do this, they need to 'buy into' the theoretical framework in some way, whether that is characterised in terms of belief (as Kuhn would have had it), or acceptance (as van Fraassen might, instead, suggest). The community agrees on a set of legitimate puzzles posed by the theoretical framework, and sets out to solve them. But there is a feedback loop, and in solving puzzles, new ways to bolster the virtues of the theoretical framework (or to disclose the full extent of its virtues, in the case of *scope* and *fruitfulness*) are found.

Of course, the *auxiliary methods* employed in theory-choice and theory-construction (by the theoretician) are also designed to fashion new theoretical frameworks with both maximal virtue (in terms of *accuracy* and *consistency*) and maximal *potential* virtue (in terms of *scope* and *fruitfulness*). But the community does not prescribe these methods, they are not 'handed-down' in the same way that the normative ones are, precisely because few scientists

[226] *The virtues lie in the theoretical frameworks*, but we can talk of a domain as being 'virtuous' if it contains virtuous theoretical frameworks. And we can talk of a discipline being 'virtuous' if it contains many 'virtuous' domains.
[227] That is to say, they may have posited a link between these properties and the practical value of the theoretical frameworks which possess them. For example, Brahe's obsession with accuracy does not seem to have been mirrored in any earlier work in astronomy.

ever engage in revolutionary thought, viz. put themselves into the frame of mind of the *extraordinary scientist.* There are no textbooks on how to forge new theories, though I would join Zahar in suggesting that there probably *should be,* and thus the bold theoretician is not only left to his own devices, but also afforded a great deal of creative freedom. This, then, leads to the important difference between the heuristic devices employed by the realist and the anti-realist. For while both will want to maximise the virtuosity (and potential virtuosity) of their creation, the former will want to ensure it has *ontological continuity* with the frameworks which it succeeds. Additionally, it is also possible that the realist might accord the individual virtues different weights than his anti-realist counterpart; for example, he might think that consistency is more important than simplicity, when forced into a situation where he must favour one over the other.

In addition, as I have also mentioned, *auxiliary methods* can creep into normal science in the *means by which a puzzle is solved.* And my suggestion would be that it is actually *good for science* that there are realist scientists as well as anti-realist scientists, at least on this level. It is solving a puzzle that is important for pragmatic purposes, after all, and there is no harm in having multiple lines of attack (adopted by different individuals and groups) until it is solved.

Overall, then, I have tried to present science as an enterprise with a great deal of tradition, which relies on a prescribed methodology (and concordant logic) of discovery. However, I have also made the case that the free-thinking of the individual is vital for the enterprise to continue. There must be those with critical minds, driven by personal philosophical predilections, who attempt to forge new and better theoretical frameworks which are cast in the image (in terms of virtues) of those that they replace. If a catchphrase is wanted, then "Science isn't an art, but it needs some scientists who are also artists."

3

METHODOLOGY FOR THE SCIENTIFIC REALIST

INTRODUCTION

In chapter one, it was concluded that scientific realism, qua philosophical position, is a *justificationist* programme, which is heavily reliant upon the premise that valid inductive arguments not only exist, but are also sometimes recognisable.[228] Induction plays a vital role in: justifying (at least dimension O of) thesis M; rejecting the 'Theoretician's Dilemma' as a decisive argument against thesis S; explaining how theories might achieve degrees of objective confirmation, in the argument for thesis E; and in making the case that abductive arguments can also be valid (at least, for those who see them as a sub-set of inductive arguments).

Then, in chapter two, it was concluded that the scientific realist, if also a practising scientist, will behave differently to his anti-realist counterparts when engaged in theory-choice and theory-construction (these being intertwined).[229] Specifically, he will take the view that the empirical successes of current theories are based, in part, on their truth-like elements; elements which he would hope to tease out, and retain, were he to set about generating new (successor) theories. So the realist will reject the Popperian suggestion that 'how it happens that a new idea occurs to a man... is irrelevant to the logical analysis of scientific knowledge', instead believing that science progresses *because* the successful elements of old theories are incorporated into their successors.[230] That is to say, because good theory-construction relies upon making sound *inductive inferences* about which ontological elements of theory-past were approximately true.[231] As I will argue in section 3.1, then,

[228] The means by which we purportedly distinguish between valid and invalid inductive arguments could be genetic, from a naturalist perspective. One might argue that our ability to recognise the superiority of "The sun will rise tomorrow" over "I will be alive tomorrow" (even if only in *probabilistic* terms - validity of an inductive argument need not be dependent upon its truth or falsehood) is necessary to guide practical action. Then, given that our practical action succeeds in allowing us to thrive, that this ability leads to empirically adequate results. Finally, that the results are empirically adequate just because the ability allows us to establish probabilities that are *objectively*, rather than just *subjectively*, good approximations to the truth. (Here, I allude to some sort of logical interpretation of probability, following Keynes or Carnap.)

[229] It was also argued that scientific realists might adopt different puzzle-solving strategies, on occasion; this may also be relevant, because their puzzle-solving experimental design strategy might be linked to *ontological* considerations.

[230] Popper [1980], p.31

[231] Naturally, some anti-realists might also think that induction is important in the choice of theory-construction strategy, though *not* with respect to examining the ontologies of previous

the scientific realist is committed to the idea that induction is an indispensable part of the logic of scientific discovery, in not only a justificatory, but also a *practical methodological*, respect.

It follows, of course, that the scientific realist who also happens to be a practising scientist should take a keen interest in both metaphysics and the history of science, at least if he wants to construct new theories with greater verisimilitude than their predecessors. For how else could he hope to rationally ascertain the ontological threads which have run through successive theoretical frameworks in the past? And by what other means could he hope to determine which ontological alleys have proven to be blind, and which might yet lead to new ground? It is obviously insufficient merely to take scientific doctrine now, and believe in it wholesale, because it is clear that ontological discontinuities *have* occurred (e.g. action-at-a-distance in Newtonian mechanics, to action-by-contact in modern field theories), and are likely to occur again unless a special (and new) interpretative strategy is adopted. Thus, as I shall argue in section 3.2, the realist should think like a Lowean metaphysician, with an eye to what could, and what could not, be compatible with the phenomena.[232] That is to say, he must take underdetermination seriously, if he is to account for the ontological mistakes of the past, to minimise those of the future, and to identify what *can* be justifiably said about the actual metaphysical world, given the current state of inquiry. Besides, it is only if one appreciates the limit of one's current scientific knowledge - and avoids getting caught up in the speculative excesses which often become, erroneously, bound up with it - that one can devise new experiments designed to extend it.

Together, sections 3.1 and 3.2 should therefore provide a clear answer to the question "Which methodologies of science are compatible with scientific realism?" in the first of two distinct emphases thereof. Specifically, with regard to how a scientific realist should practice science, *irrespective* of whether his philosophical position is ultimately correct or not. Or, to rephrase the question, "How would it be most rational for an individual who committed to belief in scientific realism to practice science?"[233]

But there is a second, more suggestive, way of emphasising the question: "How would scientists have to practice science, descriptively speaking, in order for the philosophical position of scientific realism to be most justifiable view of science?" And it seems natural, after having found an answer to this question, to want to ask, "Are there any good arguments *for or against*

(empirically progressive) theoretical frameworks. For example, the empiricist might suggest that one should inductively infer which mathematical techniques are best suited to saving the phenomena in a particular domain.

[232] This takes up the suggestion made towards the end of my discussion of thesis M, in section 1.1.

[233] This is the question that I primarily set out to answer.

scientific realism, from the methodological status quo?" In the final section of this thesis (3.3), I will therefore point out what my prior conclusions might suggest, with respect to this closely related issue.

3.1 THE ROLE OF INDUCTION IN THE LOGIC OF DISCOVERY

As has already been mentioned, Popper proposed his logic of scientific discovery on the central assumption that 'the logic of knowledge... consists solely in investigating the methods employed in those systematic tests to which every new idea must be submitted if it is to be seriously entertained'.[234] His prescription, as Miller explains it, is that the theoretician should:

> 'propose conjectures, which might be true, and incorporate them into science, without regard for whether there is anything that could be called evidence in their favour, without regard for whether there is any reason to think that they are true.'[235]

However, as I have previously argued, it seems very clear that many great scientific revolutions have been brought about by men who employed, in contradistinction to Popper's advice, (various forms of) the correspondence principle.[236] In formulating his celestial mechanics, Newton looked to Kepler's laws. Later, in constructing his transformations, Lorentz looked to Newton's laws. Of course, the Popperian might argue, nonetheless, that theoreticians would do better to avoid such inductive moves, but it remains for him to show they could ever be ironed out *in practice*. For, as Zahar argues:

> 'there exist general inductive reasons why continuity must govern the transition from one theory to the next. When scientists speak of past empirical successes, they have in mind *virtual* as well as *actually observed* facts... They set out to account, not only for known isolated observations, but also for allegedly observed regularities. Such regularities are regarded as being approximately captured by existing laws... It therefore seems as if the correspondence principle is with us to stay, if only as a desideratum.'[237]

While Zahar might be sceptical about the heuristic value of such moves, though, the scientific realist is liable to want to cultivate them, as the seed of an argument for his position. This argument would go something like this:

α Scientific 'knowledge' has been, and continues to be, our most reliable, viz. most epistemically secure, 'knowledge'.

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[234] Popper [1980], p.31
[235] Miller [1994], p.9
[236] See the sub-section *'In Normal Science'* in section 2.2.
[237] Zahar [1989], p.21

β Inductive inferences are indispensable in theory-construction. Theory-construction is necessary for progressive science. Therefore, inductive inferences are necessary for progressive science.

Thus, it is a distinct possibility, though by no means a demonstrable certainty, that the inductive inferences employed in theory-construction are causally responsible, if only in part, for the veracity of α. And Popper, for one, would likely admit that α *is* true.[238]

This said, it is not my foremost intention to further criticise Popper's position here, even though elements of the foregoing argument might have been applicable to the discussion of critical rationalism in chapter one. Rather, I just want to suggest that - from the scientific realist's perspective - an important part of the process which Popper describes, specifically the *deductive testing of theories*, might plausibly be thought to work *alongside* the inductive means by which theories are formulated. Against Popper, the scientific realist would not necessarily think it worthwhile to test a theory which had been 'made up' without reliance on prior science, *even if it had carefully laid-out observational consequences.* For Popper, attempting to determine whether any of the singular statements derivable from a new theory prove to be false (or whether there is any evidence which corroborates it) might well be seen as the best way to proceed, subsequent to the *induction* of said theory.[239] But I would eschew any suggestion that this is a radically new way of thinking, because Francis Bacon recognised, over three hundred and fifty years ago, that:

> 'it was a good answer that was made by the one who, when they showed him hanging in a temple a picture of those who had paid their vows as having escaped shipwreck, and would have him say whether he did not now acknowledge the power of the gods, - 'Yes', he asked again, 'but where are the pictures of those who were drowned after their vows?'... *in the establishment of any true axiom, it is the negative instance which has the greater force...*'[240] [emphasis mine]

Interestingly, Bacon also sheds further light on this issue, because he distinguishes between induction, viz. procession from true observation statements to the formulation of a theory, and super-induction, which involves, in his words, '[the] engrafting of new things upon old.'[241] And to formulate a new theory in science, based on science past, might be to 'super-induce' it, under his terminology. Admittedly, he was not eager to endorse

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[238] In his own words: 'In science (and only in science) can we say that we have made genuine progress: that we know more than we did before.' Popper [1970], p.57

[239] Of course what Popper calls 'corroboration' would be taken by the scientific realist as *confirmation* not only of the new theory, but also of the body of science which preceded it, and made its construction possible. Also, it is worth noting that theories alone do not issue singular statements; at least, not without the addition of auxiliary hypotheses. (Hence, my earlier distinction between *theoretical frameworks* and theories.)

[240] Bacon [1620], p.308

[241] Ibid., p.306

such a process, but I would claim that this was likely because super-induction *in his time* too often involved theories which had *not* been properly induced, initially: in particular, those in Aristotle's 'Physics'. As Lloyd puts it:

'...if there is a lesson to be learned from his [Aristotle's] work in dynamics for his method and approach to scientific problems as a whole, it is not, as has sometimes been maintained, that he blandly ignored facts in constructing his theories on *a priori* principles, but rather that his theories are hasty generalisations based on admittedly rather superficial observations.'[242]

However, the scientific realist might claim that if one starts with a set of properly induced theories, such as those founded during the Revolution which Bacon brought about, then to super-induce when they are seen to be empirically inadequate (or of insufficient virtuosity) is more efficacious than the alternatives. If the initial theories were properly induced, then there are many observation statements (specific measurements, now lost, perhaps) 'accounted for' by them. To throw away this previous work (which might now remain only in the theory), and to start again from first principles (i.e. first-level induction), might be thought foolhardy. Naturally, there is a caveat: that a new super-induced theory must be subjected to rigorous empirical examination, and not just taken for granted. This is where the core sentiment in Popper's philosophy can be seen, clearly, to be a wise one.[243]

Moreover, as Popper himself confesses, 'scientific method presupposes *the immutability of natural processes*, or the 'principle of the uniformity of nature'.[244] And the scientific realist might argue that such a presupposition actually *licenses* use of inductive inferences in certain circumstances.[245] For example, carbon-dating can only be thought to be a justifiable process if inductive inferences about *the past* are reliable: one measures the half-life of an element (or compound), and induces that it was the same before radiation was even known about, or before homo-sapiens even walked the Earth![246] In short, the asymmetry between past and future which is suggested by Goodman's paradox is not as significant as it may appear to be, *prima facie*. After all, contemporary science tells us that to say "The sun *will* always rise"

[242] Lloyd [1968], p.180

[243] Although I would deny that it is necessary for the examination to be performed with the specific aim of falsifying the theory (which is not *logically* possible, as Duhem showed, anyway). Instead, it is sufficient to *compare* it with experience, in the sense of *experiment-theory comparison* (as a part of normal science). This way, *theoretical frameworks* can be falsified.

[244] Popper [1980], p.252

[245] In recognition of this point, which many perceive as a weakness in Popper's position, Miller instead claims that: 'Scientific hypotheses propose order for the world; they do not presuppose it.' See Miller [1994], p.27

[246] This should not be a permissible conjecture for Popper, because it is obviously untestable without a time-machine; there is simply no available evidence to corroborate it. Besides, if it just 'irrefutable conjecture', with no good reasons to believe, how can the scientist legitimately disagree with the historian who conjectures, instead, that the half-life of carbon radically changed at an arbitrary point t, before humans walked the Earth? Answer: he can't.

is just as foolish as to say "The sun has always *risen*", irrespective of the fact that no human being has ever lived - as far as we are aware, given the limited observation reports which we possess - through a day in which the sun did not rise.

If a non-pragmatic argument is wanted, then the scientific realist might also say that we would neither exist, *nor continue to exist as we are*, if there were no regularities in nature. (That is, given that Thesis M is accepted, and that we *are* a part of nature.) From a Darwinian perspective, we would not have developed into intelligent beings were there no patterns to be found in nature; instead, random action would be the most 'rational', or suitable, mechanism for survival. From a Creationist perspective, intelligent design of a system implies that certain laws will obtain in it (else, why *design*?).[247] Thus, given our existence, induction is both justifiable and vital, if we are to understand our lot, or even to practically function. If induction were to suddenly become useless, then the regularities which it tapped into would also cease to exist; thus we would quickly recognise the manifest chaos around us, even if our minds were to continue to function in any fashion which we could presently comprehend.[248] (Arguably, it is more likely that they would cease to function properly were there no laws to constrain them, such as those of rationality, even were we monads.) Given that we don't recognise this, and that our minds do function, here and now, we should keep on using induction.

In science, the scientific realist will say, we should always try to put forward theories which are the most intersubjectively probable, given our current state of knowledge. And because nature *is* uniform in many respects, that which is intersubjectively probable will correlate with (though not precisely correspond to) that which is *objectively* probable. Ultimately, the evidence can and will decide, and mistakes need to be made if we are to progress; but it is better to make measured and educated guesses, in constructing new theories, than it is just to guess. Why? Because measured and educated guesses, viz. super-induced theories, will effectively take into account previous experimental evidence (that found by other humans, before our ephemeral stint on the stage) which might otherwise be lost or forgotten.[249]

Yet although this seems like a fair line of argument, part of which might be subscribed to by those who reject the 'approximately true' component of thesis E (i.e. just endorsing the notion that induction *is* vital in theory-construction, if only for non-ontological reasons), two tasks still remain for

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[247] Some of these laws might be spatio-temporally variant, but then there might be higher order laws that describe the spatio-temporal variance of those beneath them. (These would be atemporal; thereby the pitfall of an infinite regress of variant laws can be avoided.)
[248] Note, here, the vital role that presupposition of natural regularities plays in avoiding vitiating circularity.
[249] Needless to say, one also needs to bear in mind the means by which such evidence was likely gathered; for example, there are significant differences between the naked eye Astronomical observations of the early Greeks, and those of Tycho Brahe at Uranisborg.

the scientific realist. First, he has to explain to what extent we can (and usually do) determine what a theoretical framework says, in ontological terms; that is, he must flesh out the notion of *'approximately true'*. Second, he needs to propose specific tools in order to make good ontological judgements, with respect to theory-choice and theory-construction. I will now examine how he might achieve these tasks.

3.2 THE ROLE OF METAPHYSICS IN THEORY-CHOICE AND THEORY-CONSTRUCTION

'Scientists inevitably make metaphysical assumptions, whether explicitly or implicitly, in proposing and testing their theories - assumptions which go beyond anything that science alone can legitimate. These assumptions need to be examined critically, whether by scientists themselves or philosophers... Empirical science at most tells us what *is* the case, not what *must* or *may be* (but happens not to be) the case. Metaphysics deals in *possibilities*. And only if we can delimit the scope of the *possible* can we hope to determine empirically what is *actual*.'
(Jonathan Lowe [1998], p.5)

The scientific realist believes that, in order for a given theoretical framework to be *highly* empirically successful (in predictive and descriptive terms, we could take several examples from modern physics), it must have *some* truth-like ontological content.[250] Indeed, that the more successful a theoretical framework is on a phenomenal level, and the more *virtuous* it is, the more truth-like ontological elements it will have.[251] And given that science continues to produce theoretical frameworks which treat established domains of phenomena with increasing degrees of accuracy - a claim that few philosophers, if any, would want to dispute - there must be some ontological continuity between scientific revolutions.

However, the scientific realist need not claim that the truth-like ontological content of any given theoretical framework, even in modern science, is just ostensible. Instead, it seems more prudent for him to recognise that there will always be many and varied ontologies which are compatible with the *empirical consequences* of any given theoretical framework. Underdetermination is not just a wild anti-realist fantasy; this is a lesson, surely, which the ongoing debate about the interpretation of quantum mechanics should have taught us. On the most banal of levels, I would argue that the *testable* empirical consequences of any contemporary physical or chemical theoretical framework are perfectly compatible with either atheism

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[250] As mentioned towards the end of section 2.2, it is reasonable to deny that bare mathematical relationships can be applied to any phenomenal situation without some sort of correspondence rules, and without interpretation of mathematical entities. Thus, metaphysical considerations will *always* enter into theoretical frameworks.

[251] As argued in section 2.1, the scientific realist views the five theoretical virtues as indicators of *explanatory power*. And the realist believes that there is a link between the explanatory power of a theory and its truth-likeness, viz. that abduction is a valid form of inference.

or theism. And to the extent that our current theoretical frameworks in science go, questions about the possible existence of a God (or even a pantheon of deities) should remain a properly *metaphysical* concern. As Cushing cogently argues, the same is the case, *at least for now*, as regards determinism versus indeterminism.[252] (It should be added that such a recognition need not, and does not, imply that underdetermination is nearly as serious a problem as some, such as Quine, have made it out to be.[253])

To say that the theoretical frameworks of mature science are *approximately true* allows for a wide-range of epistemic positions, ranging from the naïve to the cautious. Of the term 'electron', the naïve realist might say, "It *successfully* refers to a wave-particle which is ontologically primitive; it is a building block of material objects in the actual suprasensible world." However, the cautious realist would say, "It *successfully* refers to something about the actual suprasensible world. But the effects which we use 'electron' to account for might be explained in a more truth-like fashion by reference to relationships between us and other, as yet unimagined or unidentified - perhaps even *unidentifiable* (though not unimaginable) - entities. That is to say, there are many legitimate *metaphysical possibilities*, with respect to 'electrons', which are compatible with the predictions issued by the use of the concept.'[254] Of course, some realists might say that to adopt the latter (cautious) position is to take the proverbial punch, viz. intuitive appeal, out of the position of scientific realism. But I shall argue that such a concession not only *preserves* the notion that the aim of science is truth (Thesis A), but also provides an excellent *defence* against the 'anti-Thesis E' arguments of sophisticated anti-realists, such as van Fraassen and Laudan.

On 'Approximate Truth'

First, let us note that scientific realism appeals, pre-philosophically, because we want to hold that the science of today tells us more about the actual suprasensible world than the science of yesterday; that it involves *discovery*, rather than pure invention. And to hold this, *we need only justify the claim that as science progresses, its theoretical frameworks become more verisimilar.* We need *not* show that modern theory is at any 'threshold distance' from the unvarnished truth about the actual metaphysical world; it is enough that successive theoretical frameworks in science do constitute *better approximations* to the truth. For if they do, of a fashion, then we might expect

[252] See the conclusion in Cushing [1994], sections 11.2.2. through 11.4.
[253] He writes: 'Total science, mathematical and natural and human... is underdetermined by experience. The edge of the system must be kept squared by experience, but *the rest, with all its elaborate myths and fictions, has as its objective the simplicity of laws.*' [emphasis mine] See the infamous Quine [1951], p.298. The tenability of such a claim is brought into serious doubt in Laudan [1990].
[254] See my discussion about how talk about some entities may in fact be talk about more fundamental entities, under '*The Epistemic Thesis*', section 1.1.

them to preclude adoption of ever-more false models of the actual world. Moreover, this is sufficient to justify thesis A - that 'The aim of science is truth...' - because it is rational to aim at something which we might never in practice achieve, in the recognition that we can make incremental steps towards it.[255] (It should not go unnoticed, and might even strike the reader as ironic, how close this position is to that of Popper's, but with the inclusion of induction.)

Second, we might note that just because a given theoretical framework is more empirically successful than another in the same domain, it does not necessarily follow that its truth-like ontological content, even if greater than that of its competitor, will be readily apparent. On the contrary, it may be the case that the proper ontological content of a theoretical framework, although *determinable*, is not usually correctly *determined* by the scientific community. Why should this be so? Because scientists do not generally have any training in metaphysics, and because many would eschew such training, even were they to be offered it. In short, there is no obvious reason why the aim of the majority of current scientists is to pursue the legitimate aim of science; indeed, even if many of them are oblivious to that aim, they may still unwittingly further it.

Simply put, then, the scientific realist might suggest that to call something '*approximately true*' is just to say 'it is the best approximation to the truth which we currently have'. And that it is justifiable to say that special relativity is more truth-like than Newtonian mechanics, but not on any numerical scale which is epistemically accessible.[256] For whereas no scientific realist has been capable of producing a plausible formula by which one might make claims such as 'Newtonian mechanics is 76% verisimilar, whereas special relativity is 80% verisimilar', or even 'Special relativity is 4% more verisimilar than Newonian mechanics', it does not seem entirely ridiculous to claim that special relativity is simply *closer* to the truth than its predecessor, based on its relative *virtuosity* (which constitutes its explanatory power).

What else could '*approximately true*' really mean? 'More than 50% verisimilar', perhaps? If that is the realist thought, then his position seems fatally flawed for an obvious reason: in order to know that a given theoretical framework is any given 'distance' from the truth, one must presumably know what the truth is!

In addition, the scientific realist might also suggest that there is 'hidden ontology' inherent in successful theoretical frameworks which needs to be teased out, and separated from the 'ways of thinking about things' which get

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[255] The same is true for those who strive to be morally 'good'.

[256] The notion that there really is some sort of numerical scale, underlying what we can judge from the observable, is not precluded.

bound up with it for pragmatic purposes.[257] Typically, there is also a hearty dose of pretentious pseudo-metaphysical speculation - for example, that modern science proves that God does not exist - which will also need to be isolated, quashed, and rejected.

To make this line plausible, it only remains for the scientific realist to justify, first, the central claim, *'that as science progresses, its theoretical frameworks become more verisimilar'*. Second, he must show how the 'proper ontological content of a theoretical framework' can and should be determined. In order to achieve both these tasks, I will show how Lowe's model of metaphysics, which has already been touched upon earlier in this thesis, might be employed.[258] Only then will it become clear what sort of defence the realist position outlined above can provide against the negative thesis of constructive empiricism (van Fraassen) and the pessimistic meta-induction (Laudan).

Lowean Metaphysics - Elucidating the Possible

As suggested by the quotation which heads this section, Lowe claims that the proper role of metaphysics is to examine the possible, rather than the actual; he also claims that this is the only sense in which metaphysics is possible, if the pun will be excused.[259] But the Lowean metaphysician should not be interested so much in that which is possible *de dicto*, but rather that which is possible *de re*. Specifically:

'Metaphysical possibility is... the possibility of a *state of affairs* (one which is *representable*, no doubt by a proposition)... The notion of a state of affairs, of course, is itself a metaphysical notion, just one of a large family of such notions... These notions are not purely 'logical' notions: they are *ontological*. They concern *being and its modes*...'[260]

In order to clarify this point, we might distinguish between three types of possibility (and therefore, necessity and impossibility): strict logical, narrow logical, and *ontological*.[261] That which is necessary in a strict logical sense is true on the basis of the laws of logic alone; 'It is not the case that I am married

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[257] Sometimes, it may be more pragmatically efficacious for a community to communicate (and think) in terms of ontological models which are obviously false. However, this will make it difficult for many members of said community to disentangle interpretation from formalism.

[258] See '*The Metaphysical Thesis*', in section 1.1.

[259] See Lowe [1998], p.8. He later retracts this point (which, after all, seems to have been made precisely to deliver the pun), noting - correctly - that establishing that which is metaphysically *impossible* and that which is metaphysically *necessary* is also possible.

[260] Ibid., pp.9-10

[261] Lowe uses the term 'broad logical', but I prefer 'ontological' because, as he points out, '[there is] a danger that this tradition [that of using the term 'broad logical'] may lead incautious philosophers to overlook the...division between logic and metaphysics'. Ibid., p.14

and that I am not married' is an example, because it is an instance of the law of non-contradiction. That which is necessary in a narrow logical sense is true on the basis of the laws of logic *in addition to* the definitions of non-logical terms; 'It is not the case that I am married and that I am a bachelor' is an example. That which is necessary in an *ontological* sense, however, is that which is true in every possible world; a fine example is 'Dolphins are mammals'.[262] Note that while it is necessary for a statement to be both strictly logically possible, and narrowly logically possible, in order for it to be ontologically possible (or necessary), it is by no means *sufficient.*

So the role of the metaphysician, according to Lowe, should be to determine:

i) That which is ontologically possible, viz. those states of affairs that may or may not obtain in the actual world.

ii) That which is ontologically necessary, viz. those states of affairs that necessarily obtain in the actual world.

iii) That which is ontologically impossible, viz. those states of affairs that necessarily do not obtain in the actual world.

With this *a priori* work done, it then makes little sense to mount any sort of empirical investigation into the necessary or the impossible; the role of experience, for Lowe, is just to select the actual from the possible, *when the possible has been delimited.*[263] In science, there is no point in investigating whether 'All triangles have three angles' (a narrow logical necessity) *or* 'Some events are substances' (an ontological impossibility) are true in the actual world, whereas it *is* worthwhile to determine whether 'All swans are white'. This is because, even were 'All swans are white' to be true in the actual world, it would only be so *contingently.*

To this, the objection might be raised that there are some metaphysical necessities which *cannot* be determined simply by *a priori* considerations, for example: 'Dolphins are mammals'; 'Hydrogen is an element'; and 'Earth is a planet'. But Lowe does not want to deny this. He wants to have it, rather, that the *a priori* delimitation of that which is possible (which he still holds is necessary before experience can be applied to determine that which is actual) should be performed in terms of *categories*, rather than *natural kinds.* And whereas the latter are obviously *not* knowable *a priori*, he claims that the

[262] As Lowe points out, this claim might be more properly rewritten as 'For any x, if x is a dolphin, then x is a mammal'; this avoids the objection that 'Dolphins are mammals' is plainly false in those worlds in which there are no dolphins. (His example is 'Water is H_20'.) Ibid., p.15

[263] In many cases, of course, experience will only be sufficient to *reduce* the number of 'possibly actual' theoretical frameworks, explanations, or models. 'Possibly actual', that is, in an *epistemic* sense.

former *are*.[264] Thus, one might say it is *a priori* ontologically necessary that 'Events are not substances', 'Concreta are not abstracta', 'Organisms are not artefacts', and 'Nothing is both black and white all over'.[265] (To clarify, we can know that there are such things as natural kinds, but not what sorts of natural kinds there are in any given world; for example, some ontologically possible worlds do contain mammals, and others do not. When we empirically establish that there is a natural kind such as 'mammal', and that dolphins instantiate this universal, it follows that 'Dolphins are mammals' is true in every possible world in which dolphins exist.[266])

By the actualist account, championed by Plantinga, states of affairs can exist even though they might not obtain.[267] They also enter into relationships with one another; that is to say, one state of affairs can be said to include, or preclude, others.[268] Under this framework, a possible world is said to be '*a maximally comprehensive possible state of affairs*'.[269] 'Maximally comprehensive' because it either includes or precludes each individual state of affairs, 'possible' because it cannot include contradictions such as 'Triangles have only one side'.[270] The term 'actual world' need not, then, be taken to be indexical, that is as picking out one particular spatio-temporally bounded entity (this universe).[271] Instead, it may be taken to refer to the maximally comprehensive possible state of affairs which *actually obtains*; it enjoys ontological priority precisely because it *does* obtain. That is to say, possible worlds can be seen as abstract entities which necessarily exist (though only one of them obtains), not to be confused with the physical universe or any of the objects in it, which are contingent, viz. only exist because a particular possible world obtains.[272]

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[264] Ibid., p.16

[265] See ibid., Fig.1, p.181. (Note, however, that there is some overlap as regards some of the categories on the diagram - for example, Lowe thinks that *all* universals are abstract. This seems right, because *greenness*, for example, is not a spatio-temporal thing.)

[266] This is an Aristotelian idea. In his *Posterior Analytics*, he writes: '...demonstration depends on universals and induction on particulars, and it is impossible to consider universals except through induction...and it is impossible to get an induction without perception - for of particulars there is a perception; for it is not possible to get understanding of them; for it can be got neither from universals without induction nor through induction without perception.' Aristotle 81b. See Barnes [1984], p.132

[267] This is similar to Plato's abstract entity account, where properties may exist but not be exemplified.

[268] For example, my writing this paper on my computer, as a state of affairs, includes the state of affairs that I possess a computer and precludes the state of affairs that I write this paper by hand.

[269] Loux [1998], p. 192.

[270] Logical possibility is a minimum requirement for ontological possibility, as previously mentioned.

[271] I refer, here, to Lewis' reductionist account of real possible worlds.

[272] Though it is important to note that this claim does not rely on states of affairs (or possible worlds) being ontologically prior to concrete substances. If concrete substances are prior, then they will determine the set of possible worlds that necessarily exist.

Should this picture of metaphysics be appealing to the scientific realist? I would argue so, because he is wont to believe that there *are* natural kinds, and that these are not a mere linguistic 'convention' (or invention), viz. a purely anthropocentric means of classification. That is to say, science for the scientific realist is a process of *discovery*; the distinction between plants and animals, for example, is one found in nature, rather than one which we have *imposed upon* nature. And Lowe's point, eloquently made below, is just that such a position is unjustifiable if he who holds it does not also hold that metaphysics is a legitimate, nay *vital*, enterprise:

'Only in the light of metaphysics can we vindicate a judgement that a caterpillar survives its transformation into a butterfly whereas a pig does not survive its transformation into the flesh of the python which devours it, or the judgement that water survives its transformation into ice whereas paper does not survive its transformation into ash [when combusted]. Neither macroscopically observable phenomena, nor scientific information concerning the 'internal constitutions' of things, can resolve such issues for us in the absence of metaphysical guidance.'[273]

But what role can Lowean metaphysics play in the scientific realist's claim that science can and does produce theoretical frameworks of increasing verisimilitude? The argument goes as follows:

i) It is a feature of theoretical frameworks in mature science that they are *fruitful*. That is to say, they are responsible for *sensible disclosure* and *relationship disclosure*.[274] With each new generation of theoretical frameworks, new phenomena and new relationships between phenomena are disclosed.

ii) There are entities and mechanisms (viz. laws of nature) in the actual metaphysical world which are causally responsible for that which we perceive. (It should be remembered that *we*, qua organisms, are *ourselves* a part of said world.) For example, to call one book 'red' and another book 'blue' is to point to a feature of the actual metaphysical world, *even if 'colour' is not a primitive universal*.[275] In short, there is a true ontic explanation for all that we perceive.

iii) Any true ontic account of the actual metaphysical world must account for all the phenomena, and all the relationships between the phenomena.[276]

iv) Thus, as that which is disclosed increases - and it is a feature of science (from i) that it increases this body of knowledge - the set of worlds that

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[273] Lowe distinguishes between *phase change* and *substantial change*. Ibid.,.p.179
[274] This was argued in section 2.1 of this thesis; for the discussion of fruitfulness specifically, see the sub-section entitled *'Theoretical Virtues as Demarcation Guidelines'*.
[275] Refer back to the discussion of this point at the beginnings of both the *'Thesis E'* and *'Thesis S'* sub-sections, in section 1.1. Naturally, this point is founded on the acceptance of Thesis M.
[276] It may be that some relationships are merely coincidental; still, it must *allow* for them.

constitute the possibly actual (that is, in an epistemic sense) will shrink. In short, science serves to narrow down the admissible set of models of the actual world, even when underdetermination is taken seriously; *science advances by ruling out possible worlds as candidates for the actual world.*

However, it must be made clear that the set of possible worlds which constitute the 'epistemically possibly actual' (EPA) will always be infinite, even though infinities of possible worlds will sometimes be ruled out of this very set. This may seem confusing, so in order to elucidate this idea, let us imagine that the actual world can be represented by a positive integer, n, and that the set of ontologically possible (OP) worlds is represented by the set of all positive integers.

Now we can imagine starting off, pre-science, with a set of EPA worlds which is just the same as the set of OP worlds, determined by category analysis, namely:

1, 2, 3, 4, 5, 6, 7, ... *ad infinitum*

And from this, experience will serve to remove certain sets of 'numbers' (possible worlds), both finite and infinite. In the former case, we might learn that $n>6$, removing only five possibilities (namely $1 \leq n \leq 6$), or simply that $n \neq 10^{34}$. In the latter case, we might learn that n is not even (ruling out the series n=2, 4, 6, ...), or that n is not a prime (ruling out n=2, 3, 5, 7, 11...). Thus, the set of EPA worlds *will* become an ever smaller sub-set of the set of OP worlds as science progresses, even though the number of EPA worlds will remain infinite.

Now it might be claimed, from point iv), that if the set of EPA worlds remains infinite, then it can hardly be maintained that 'the aim of science is truth'; that it would be better to say 'the aim of science is to eliminate error'. I do not believe this follows, however, because it is possible *in principle* to reach a stage at which all EPA worlds involve certain states-of-affairs; for example, that mass exists.[277] And science clearly makes incremental steps towards that stage: toward the limit of our possible knowledge about the actual, or 'as much of the truth as we could ever determine'. Thesis A survives.

The defence against van Fraassen's local scepticism, then, is seen to rest on a central *metaphysical*, rather than epistemic, premise. If van Fraassen wants to have it that a scientific theory can be true or false, literally construed (viz. endorse thesis S), then there is clearly a sense in which he must admit that there are true *ontic* explanations, or *accounts*, of the suprasensible; explanations such as "It is a necessary but not sufficient condition for

[277] In other words, we cannot find the *whole truth* about the world, but we can establish some facts, even about unobservables.

phenomenon x to occur that theoretical entity y exists." That is, irrespective of whether we can identify which explanation just happens to be true in any given case.[278]

Of course, this need not prevent van Fraassen holding that there is a pragmatic *dimension* to explanation; that the type of the explanations which we happen to prefer are not necessarily truth-like ones, and that a false explanation may sometimes be functionally equivalent to a true one. Indeed, sometimes a false explanation might even prove to be more efficacious, in the short term. But then, the Lowean metaphysician might counter, *it is simply not the role of the metaphysician to prefer one metaphysical system over another*; his role is merely to outline the possibilities. It may be possible for van Fraassen to rejoin by saying something like "There may be unthinkable possibilities", but any attempted justification of such a claim would, ironically, require recourse to metaphysics. He would need to claim that it is *metaphysically possible* for there to be *unthinkable metaphysical possibilities* - and reflection on the nature of such a claim makes it clear that it is just nonsensical.[279]

And what of the scientist involved in theory-construction? He would do well to heed Feynman's words, and be mindful of the different possibilities:

> '...we must keep all the theories in our heads, and every theoretical physicist who is any good knows six or seven different theoretical representations [read: ontology-laden theoretical frameworks] for exactly the same physics [read: means to derive the same observational results].'[280]

Against Laudan's pessimistic meta-induction, the defence accorded to the scientific realist by adoption of the foregoing metaphysical position is considerably different. It relies, first, on the frank admission that scientists have made, do make, and will make, mistakes. However, it can then be pointed out that such errors *are* clearly capable of being ironed-out - presumably, Laudan himself does not believe that caloric or the crystalline spheres exist - and that, moreover, their likelihood of occurrence may be reduced by the application of careful metaphysical analysis, and the adoption of an open-minded attitude.

It is certainly the case that realist scientists sometimes choose to favour one theoretical framework over another when both have the same empirical consequences. But it might be claimed that to do so is an error of judgment (if and only if the realist is in the process of seeking the truth - there is nothing wrong with a realist utilising any theory for pragmatic purposes, without making comment on its truth-status), *provided that both theoretical frameworks*

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278 My claim is that we can identify which infinite set of EPA worlds must contain the actual world, in the limit of enquiry.

279 Specifically, the unthinkable metaphysical possibilities might exclude the possibility of unthinkable metaphysical possibilities. Unthinkable, isn't it?

280 Feynman [1967], p.168

are metaphysically possible.[281] Of course, it might be added that there have been several theories, in the past, which had an extremely dubious metaphysical character - that were, and should have been seen to be, *metaphysically impossible.* For example, to adopt a bastardised version of Aristotelian cosmology according to which the planets lie on substantial concentric spheres about the Sun, while simultaneously employing an eccentric point displaced from the Sun about which the Earth moves in a circle, is clearly inconsistent. Yet this is precisely what Copernicus did in his *De Revolutionibus.*[282] (Of course, it was recognised more readily that Aristotelian spheres were not consistent with the subsequent Tychonic system, for the circle describing the motion of the Sun intersects that describing the motion of Mars.[283])

Admittedly, I have not stressed the *good reasons* to commit to belief in Thesis E here, although I did cover them earlier; the most obvious is Putnam's 'No Miracles' argument, which was defended in section 1.3.[284] My approach has been, rather, to present a special understanding of Thesis E which avoids the standard criticisms in the literature; this is the area in which the contemporary debate lies.

First Conclusion: How would it be most rational for an individual who committed to belief in scientific realism to practice science?

Stated plainly, my conclusion is, perhaps, somewhat remarkable. For I want to suggest that consistent scientific realists who practice science should be *metaphysical realists*, employing analysis of categories known *a priori* (they should claim) - such as property, relation, universal, substance (and, the most obvious, natural kind) - in order to delimit that which is possible of the world. And this will, of course, be an ongoing process - for sometimes, the only conclusions that will be able to be drawn will be of the form "If metaphysical possibility *a* is actual (viz. if the state-of-affairs *a* obtains), then theoretical framework *x* is false." For example, there may be occasions on which the realist might want to say "If it is the case that *space is absolute*, then theoretical framework *x* cannot be true in the actual world."[285]

[281] Perhaps abduction could be thought of as a rough-and-ready means by which we can sometimes distinguish between the metaphysically possible from the metaphysically impossible.

[282] See Kuhn [1957], p.170

[283] Ibid., p.206. (See also Fig. 37, on p.202)

[284] To this, we can now add my conclusion in 3.1 that induction is vital for the scientific enterprise in any event.

[285] This is not to say that there will not be other occasions where one might say "If it the case that *space is absolute or relational*, then theoretical framework *x* cannot be true." But since space could be no other way, this would make theoretical framework *x* metaphysically impossible.

Does it not seem obvious, if we cast aside any empiricist bias against metaphysics, that it would be ridiculous to believe that science is a process of *discovery*, rather than *invention*, while simultaneously maintaining that entities such as properties and non-objects do not exist? If the former do not, then what could the realist scientist say that mass *is*? If the latter do not, then what could he understand deep-water waves to *be*? Note that if *properties* and *non-objects* are inventions, then it follows that many of the 'things' which scientific realists place in those categories are also inventions; ways in which *we see the world*, rather than *ways the world could be* (or *is*). This is certainly the case when dealing with theoretical entities such as mass, or light. And note that these *are* theoretical entities, for although we can directly perceive 'gravitational' forces and colours, we cannot directly perceive that which we posit to be causally responsible (at least in part) for such experiences.

That is not all, however, for we might also question how a realist scientist could believe in *partial degrees of entailment* without adopting something like a logical interpretation of probability - that is, without believing that probabilities should be understood, at least in certain circumstances, as *rational degrees of belief* rather than mere *degrees of belief*.[286] Under such an interpretation, there is a real probability-relation, p, which exists between any two sets of propositions, evidence and hypothesis. And such a relation, were it to exist, would clearly be an abstract entity - one which the realist might want to claim, following Keynes, that we are capable of intuiting.

To claim otherwise, for example to accept a subjectivist account of Bayesian learning, would be to admit that 'degree of confirmation' is a purely *psychological*, rather than *objective*, quantity (or quality, if we allow for non-numerical probabilities).[287] But this is unsatisfactory for a scientific realist, since it raises serious doubts about the tenability of any link between the degree of confirmation of a theory, and its truth-likeness (relative to past theories, which may be accounted for by the correspondence principle). If the former is purely subjective, then how can it possibly relate to the latter, which is surely objective? There seems to be no plausible answer to this

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[286] The realist is also entitled to believe in *aleatory* probabilities, of course. Indeed, one might legitimately believe in *subjective, logical* and *frequency* (or *propensity*) interpretations, relative to context. Carnap writes of: 'probability$_1$, a degree of confirmation' and 'probability$_2$, relative frequency'. But he also writes: 'It cannot, of course, be denied that there is also a subjective, psychological concept for which the term 'probability' may be used and sometimes is used. This is the concept of the degree of actual, as distinguished from rational, belief... This concept is of importance for the theory of human behaviour, hence for psychology, sociology, economics, etc.' Carnap [1962], pp.29-37 + p.51

[287] Here, my use of '*objective*' should not be understood as suggesting a link to the frequency or propensity theories of probability; I refer to these as *aleatory*. For more on this, see Rowbottom [2001].

question, as I am sure that Popper would have been most eager to point out.[288]

I should add, however, that my presentation has been skewed towards the *theoretical*, rather than *experimental*, aspects of science; this is perhaps natural, though somewhat regrettable, given that such a bias is prevalent in the literature on the philosophy of science over the past century. Given this, it should perhaps be added that some realist scientists might never need to think metaphysically, if they are only going about the business of data collection. This would have been a rare occurrence in the past, for the two aspects of science were more closely intertwined - take Brahe's work in Uranisborg as an example - but in the present day, it is certainly possible. Some might call such scientists 'hacks', but this seems ill-advised given that there would be no science without data.

3.3 Arguments For or Against Scientific realism, from Methodology

As discussed in chapter 2, I want to have it that realists and anti-realists adopt different *auxiliary methods*, but rely on the same *normative methods*, when practising science; the latter are dominant in *normal science*, whereas the former are dominant in *extraordinary science*.

Thus, any arguments for scientific realism *from* methodology present (or past) will rely on claims about the relative efficacy of different sets of *auxiliary methods*, with particular attention being paid to periods of 'crisis', or *extraordinary science*.[289] Specifically, if it could be shown that there were certain *auxiliary methods* which were employed by realists, but not anti-realists, and were vital in theory-construction, then this might demonstrate that science *needs* realist practitioners. It might then be held to follow that realism about science was a superior philosophical position, at least on pragmatic grounds.[290]

Are there any such arguments for mainstream scientific realism, taken as a whole? I think that the answer to this question is difficult to see, insofar as some sophisticated anti-realists - such as constructive empiricists - would

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[288] This is precisely why Carnap wanted an objective, but not aleatory, account of degree of confirmation; else, Popper's criticisms of induction in the Logic of Scientific Discovery do seem to hit home.

[289] As I argued earlier, insofar as *auxiliary methods* are applied in *normal science*, in the means by which a puzzle is solved, it is good for science that it has both anti-realist *and* realist practitioners.

[290] Of course, it would not necessarily follow that scientific realism was more truth-like than anti-realism; as an analogue, even if he agreed that we need to behave *as if* thesis M is correct, the sceptic might still say that it is not.

claim to be able to *accept* literal interpretations of theories as approximately true, and work with them as if they were, without needing to *believe* that they were. And it seems a small step for those such as van Fraassen to claim that such an anti-realist could, therefore, adopt any set of *auxiliary methods* that involved viewing theories as being approximately true. (If this sort of claim is thought to be faulty, then the argument against it must take place on epistemological territory - philosophical analysis of science, present or past, cannot contribute much, if anything, to such a debate.[291])

However, this said, it is far from clear that there is not such an argument *for* Thesis S. After all, it seems that it is necessary to assign some sort of literal interpretation to many mathematical laws, in order to be able to apply them. In some cases, when dealing with macroscopic laws such as the Ideal Gas Law, only a few 'correspondence rules', clearly related to observables (e.g. 'p' corresponds to the observable 'pressure') are necessary. This presents little problem for the instrumentalist. However, in other cases, it seems *vital* to understand some mathematical symbols as being referent to *unobservable* entities (i.e. natural kinds, properties of those natural kinds, etc.). Consider 'refractive index of water', 'half-life of uranium', or 'period of B-DNA helices'. Of course, we could always employ Craigian versions of such laws/theories, but unfortunately we do not have an infinite period of time on our hands.[292]

As was suggested towards the end of section 2.2, this literal/realist interpretation of mathematical entities is a move that a genuine instrumentalist would really have little, if any, reason to want to do. That is, aside from his desire to curry the favour and support of his realist contemporaries. The realist, on the other hand, will welcome such a challenge, and take it very seriously. This is just as well, one might say, since it seems that it needs to be done *in any event*.

In short, my second conclusion is that, methodologically speaking, it is necessary for the scientist (theoretician) to behave *as if* thesis S is correct. But if it tells against scepticism that the sceptic must behave as if thesis M is right, then it is also tells against instrumentalism that the instrumentalist scientist must behave as if thesis S is right. Therefore, thesis S is, at the very least, just as plausible as thesis M; those who commit to belief in the latter, while believing that science is a worthwhile activity and denying the former, seem to be acting on a whim.

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[291] The line between 'acceptance' and 'belief' is certainly a blurred one, and it remains to be seen whether any principled distinction can be made between the two. This is a very difficult issue, which I do not feel that I am equipped to treat.

[292] Craig's theorem was discussed in section 1.1, under '*The Semantic Thesis*'.

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