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Destructive realism: Metaphysics as the foundation of natural science

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DESTRUCTIVE REALISM: METAPHYSICS AS THE FOUNDATION OF NATURAL SCIENCE

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ABSTRACT

This thesis has two philosophical positions as its targets. The first is 'scientific realism' of the form defended by Boyd, (the early) Putnam, and most recently Psillos. The second is empiricism in the vein of Mill, Mach, Ayer, Carnap, and Van Fraassen.

My objections to both have a rather Popperian flavour. For I argue that 'confirmation' is a misnomer, that so-called 'ampliative inferences' are heuristics at best, and that naturalism and subjectivism are regressive doctrines. At the heart of genuine realism, I argue, is a stance on the issues of perception and conception. In particular, I hold that to be a realist is to reject the notion that there are representations which have some sort of epistemic priority. And along related lines, I maintain that the closely aligned doctrine of physicalism cannot simply be presupposed. What this amounts to is that the search for some sort of 'solid foundation' for 'knowledge' is a futile enterprise. Such a foundation would be unimportant, even if there were to be one, and we ought to be free to critically examine any claim we like. So rather than *sapere aude*, I would have 'dare to err', and place an intersubjective emphasis on inquiry. And this goes for metaphysics, logic, and mathematics, as well as for natural science.

Yet I also advocate the view that we ought to be optimistic about our ability to find the truth, *ceteris paribus*. And to this end, I argue that we should accept that our faculty of conception is sufficient to allow us to connect with the possibilities of being, whereas our faculty of sense is sufficient to allow us to connect with that which is actual; this, given considerable critical struggle on our parts, both individually and collectively. I urge that it is methodologically advisable to behave as if this is so, if we are not to achieve only the self-paralysis of the Pyrrhonist.

In a nutshell, destructive realism says that natural science progresses by *ruling out* possibilities, in particular by ruling out possible worlds as candidates for the actual world, but that this is a two-stage process, involving both an *a priori* (metaphysical) and an *a posteriori* (observational) component. The aim of natural science is to eliminate false theories. Its aspiration is truth.

We are all metaphysicians; and science derives historically from metaphysics.

POPPER

[W]hen the love of ideas and fancies becomes a substitute for the love of living people, the lover, the committed man, often subordinates the requirements of human beings to the claims of his idea – the "demand of the object". Thus, that most inhuman of transformations occurs: human beings become objects, to be bent, broken, molded, even "educated," for the love of an idea.

BARTLEY

PROLOGUE

Ostensibly, this is a thesis about the epistemic status of natural science, and the judgement to be offered is based upon a particular understanding of its necessary, as opposed to contingent, features. I tackle 'scientific realism', 'constructive empiricism', and other trendy positions, in order to dismantle some of the prevalent contemporary perspectives, and then attempt to put something in their place. Curiously, however, the replacement is almost trite in its simplicity, in so far as I arrive primarily at the suggestion that a Socratic approach to inquiry is best. Indeed, it is actually saying anything more than this – trying to formulate all-embracing epistemic principles, general methods, and the like – that I argue against. As such I might come across as a 'spoiler', in the vein of Feyerabend, but I take this sort of spoiling activity to show *exactly* what I am arguing for. In other words, I take the means by which I conduct my investigation to be entirely consistent with my final conclusion, and while neither might be to the reader's taste, to do things differently would be dishonest. After all, this is an inquiry into inquiry.

So I ask the reader not to expect a sequentially constructed position, where each single section is slotted neatly into its preordained place in a preconceived jigsaw puzzle. I am not interested in *puzzles* in the Kuhnian sense, but rather problems, and hence present a smorgasbord of dialectical discussions on issues which are generally taken to be important in what has unfortunately become known as 'philosophy of science'. If there is mystery, or if I do not understand something, I say so. Further, I mean it: I should not like the reader to take such declarations as substitutes for argument, but rather as frank admissions that I, and we, have limits. I will confess to suspicions that many of the positions I argue against are contrivances designed to preserve dogmas, precisely because of their dismissal (or avoidance) of deep mysteries, but do not make it my business to throw around such accusations. The reader can judge for herself.

One would be unwise to think that the core issue is unimportant; an abstract matter only worthy of consideration by a 'philosopher's philosopher'. Given the high level of trust placed in even the most radical claims of natural scientists in the West[1], most pertinently within its schools, courts, and governments, such investigations become vital.[2] The faith in the authority of the natural scientist, or at least natural science, which seems to be burgeoning – and which is, alarmingly, oft accompanied by a considerable lack of detailed knowledge, even on trivial matters such as whether astronomers think that the Sun revolves around the Earth, or vice versa – may have disastrous consequences.[3] And when one finds philosophers making claims such as 'In science (and only in science) can we say that we have made genuine progress: that

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[1] In recent interviews performed for a report to the British government, OST & Wellcome Trust [2000], it was found that only 21 percent of the public sample agreed with the statement: 'The achievements of science are overrated'.

[2] The *McLean vs. Arkansas* case is an excellent illustration. See Ruse [1982a], Laudan [1982], and Ruse [1982b].

[3] Evidence of public misunderstanding of science is considerable. See, for example, National Science Foundation [2002]. 55% of respondents thought it was true that "Lasers work by focusing sound waves", which seems to suggest they didn't even realise that 'LASER' is an acronym. More worryingly, 35% of respondents thought it was true that 'radioactive milk [can] be made safe by boiling it', and 49% that antibiotics can kill viruses.

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we know more than we did before.'[4], one wonders if it is seriously being suggested that we should disregard the work of those such as Plato, Aristotle, Euclid, Archimedes, and Apollonius of Perga. Can it really be right that they were discussing mere trivialities, with which we can readily dispense, since we are blessed to find ourselves in a 'special period' where the truth is almost manifest? That philosophy is dead, or lives on only in so far as it is fed by the scraps that natural science throws it?

One might be forgiven for missing the subtext to my discussions, which is humanistic (yet not anti-religious), and ethical. But to attempt to spell this out in a sentence or two would be a mistake; perhaps Wittgenstein was right that some things can only be shown, and I have a particular *mode of engagement* – one that I strive for – in mind. With this said, let us begin.

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[4] Popper [1970], p.57. As I shall argue later, though, this statement is out of character, given Popper's radically anti-authoritarian perspective.

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I

REALISM ABOUT NATURAL SCIENCE: DISPELLING THE MYTHS

INTRODUCTION

In this chapter, I endeavour to provide both a systematic overview of 'scientific realism', and a subsequent critique. But in doing so, I should make it clear from the start that I take 'scientific realism' to be a philosophical position with a definitive history, which is entirely different from *realism about science*; indeed, I argue this. To be more specific, I take 'scientific realism' to be a particular *kind* of realism about science, the root of which lies in the 1960s. Broadly, I take it to have begun with Maxwell [1962], and Smart [1963], to have been further developed in pieces such as Boyd [1973] and Putnam [1975], and to have been recently developed and defended in Leplin [1997], and Psillos [1999]. Nonetheless, I hold that 'scientific realism' can be fairly characterised in terms of a number of distinct theses, several of which, but not *all of which*, I wish to challenge.

This strategy has its risks, however. For one who would call herself a 'scientific realist' might well pick and choose from several of the theses I outline, and have it that I present a caricature of her views. However, to this sort of potential objection, I should like to say first that I take Psillos [1999] to be a trustworthy source on what this position now involves, since it is its most recent defence. Second, and far more important, that in so far as I argue against specific theses (or conjunctions of theses) which I take to be *parts of* 'scientific realism', I also take myself to be arguing against *anyone* who holds those same theses (or conjunctions of theses). So even if *no-one* is a 'scientific realist' in exactly the manner I suggest, I hold that there are highly similar positions which my arguments strike against. For instance, in so far as I offer an argument against the claim that theories can achieve degrees of confirmation (rather than corroboration), I also take this to preclude the views of science which are advocated by *all* the aforementioned authors, if it is successful.

In outline, I shall proceed as follows. In the first section, I present the theses which I take to constitute 'scientific realism': metaphysical (MT), semantic (ST), epistemic (ET), teleological (TT), alethic (AT), and naturalist (NT). And with these explained, I then proceed to attack three of them – MT, ET, and NT – in part by considering their respective roles in the system, and in part by taking them in isolation. In 2.1, I argue that MT is too weak and vague, but that NT 'tops it up'. In 2.2 through 2.4, the bulk of this chapter, I argue against ET. And in the final section, 2.5, I explain my objections to NT. As such, I try to dismantle 'scientific realism', and rule out particular theses thereof, in order that an alternative position might be developed later. And I take it that the reasons for which I rule out those theses serve to shape my responses to the problems which remain; that is, not only to delimit the possibility space for philosophical positions with respect to science, but also to be suggestive of a particular class of alternatives.

1. 'SCIENTIFIC REALISM' APRICATED

A prudent means by which to begin to grapple with 'scientific realism' is to dissect it into several distinct theses, which mutually constitute it, yet are each in need of a detailed defence. As I have argued in greater detail elsewhere, one popular divisional scheme is as follows:[5]

Metaphysical Thesis (MT): The world is populated by entities that are both objective and mind-independent. Objective, since they are not dependent for their existence upon our thinking or conceiving of them, or our ability to think or conceive of them (if we can). Mind-independent, since they are not constituted by the *mental*, e.g. bundles of 'ideas' in Berkeley's sense.[6]

Semantic Thesis (ST): Theories in natural science should be understood literally, as involving assertions about the world and its contents. They are capable of being true or false, so construed. If any given theory is true, then the terms employed therein, be they 'observational' or not, refer to entities that exist.[7]

Epistemic Thesis (ET): Scientific theories can achieve degrees of confirmation, through their predictive successes. A well-confirmed theory in a mature (viz. well-established) natural science is approximately true.

However, I will now argue that these theses, taken alone, are insufficient to unveil the full commitments of the 'scientific realist'. For there are related theses that are essential to the position as typically defended, yet are not entailed by MT, ST, or ET, taken either in isolation, or in combination.

The first of these, which is suggested by Hendry, is teleological: it specifies the aim of natural science as an activity, rather than the goals of all, or even any, scientists.[8] And it is important for exactly the reason which Hendry states, specifically that it allows for a distinction to be made between anti-realist and realist positions which both deny ET; for example, constructive empiricism, on the one hand, and critical rationalism, on the other. (Here, Hendry is thinking of *realism about science* in general, rather than 'scientific realism' as an essential position, or class of highly similar positions. This is commendable, if discussion of these issues is not to be confined to the boundaries defined in fashionable schools of thought; as Popper puts it: 'To resist a new fashion needs perhaps as much courage as was needed to bring it about.'[9]) I do

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[5] Rowbottom [2002], sections 1.1 and 1.2.

[6] 'Mind-independent' is sometimes used to mean what I have defined as 'objective'; but here, I heed a scientific realist. See Devitt [1984], p.15.

[7] Some scientific realists would add the rider 'central', to the talk of terms. Given this, I try to invoke only terms that are widely agreed to be 'central' to a given theoretical framework, or to several such frameworks in a discipline, in my subsequent discussion.

[8] Hendry [1995], p.58. Hendry dubs this thesis 'aspirational', but I prefer 'teleological' just because I do not believe that 'aspiration' is synonymous with 'aim'. As Watkins has pointed out, it may be rational to aspire to that at which one cannot rationally aim. For example, science may aspire after truth, but not aim at it, even if its methods are not necessarily sufficient for establishing the truth, or bringing its successive theories closer to the truth. See Watkins [1997], section 13.

[9] Here, there is also a link to another theme, to which I shall return. It is what Popper called the 'great danger in the increase of specialisation'. And this may be true of the divisions within philosophy itself, both in formal, and social, respects. Popper [1970], pp.52-53.

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think it is correct that this teleological thesis, as follows, is strongly suggested by MT, ST, and ET, taken together, but simply because one (or all) of those may fall, it need not:[10]

Teleological Thesis (TT): The aim of natural science, *qua* activity, is truth. This is not to say that natural science will converge upon the 'whole truth'; to defend the notion that its aim is truth, it is sufficient to defend the claim that natural science makes progressive steps towards the truth.[11] (For example, that each successive generation of theories is more verisimilar than that before it.)

Second, and clearly more fundamental, is a thesis that specifically tackles truth. For although it is implicit, from MT, that some form of absolute truth, along correspondence lines, is compatible with 'scientific realism', the adoption of such is not necessitated even by its combination with ST and ET. For example, even were MT and ST accepted, the understanding of 'truth' operating in ET could be one of redundancy, following Ramsey and Prior, or a straightforward deflationary account, uncoupled from a substantive component. Trivially, just because a theory in natural science should be understood as referring to entities which would exist, were it to be true, it need not follow that those entities could fulfil a role as truth-makers. There are, therefore, some rather curious, and intuitively artificial, positions which could be invented in order to sever the intended link between ST and ET. And while I do the 'scientific realist' no injustice by the inclusion of the following thesis, since Psillos himself writes that: 'truth is a non-epistemic concept... assertions have truth-makers... these truth-makers hinge ultimately upon what the world is like', I think that it ought to be stated *clearly*:[12]

Alethic Thesis (AT): Truth is absolute, rather than relative. Assertions are, or disclose, truth-bearers.[13] (This renders the intended understanding of ST.) Truth-makers are objective and mind-independent entities. (That is, in the senses outlined in MT.)

Third, and finally, is the most curiously understated aspect of 'scientific realism'. In his formulation of the theses, or 'stances' as he calls them, Psillos seems to smuggle it in by inserting the casual phrase 'natural-kind' into MT; specifically, he states that 'The world... has a natural-kind structure'. (And that is the *only* mention of 'natural', in his initial formulation.) But what, precisely, does this apparently innocent addition really amount to? The answer comes just beforehand, where he baldly asserts that

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[10] Indeed, it is plausible that: MT & ST & ET & AT $\models$ TT. AT is discussed just below.

[11] Laudan [1981] offers a powerful argument against convergent realism, although it must be added that this partially relies on an inductive move.

[12] Psillos [1999], p.xxi. Psillos later states that the following is an *anti-realist* view of truth: 'if an assertion cannot be known to be true, or if it cannot be recognised as true, then it cannot possibly be true' (p.232). This seems misguided to me, since such a position is perfectly consistent with the view that for any possible world in which humans exist, humans are capable *in principle* of knowing all that which is true, when that which is true is only dependent on mind-independent and objective entities. Indeed, I believe he completely misses Dummett's real point, even though he quotes him just a line later: '[a] statement cannot be true...unless there exists that which, were we aware of it, would yield such knowledge'. One can, of course, call this 'anti-realism' if one likes; but one may very well call it 'optimistic realism', since it does not rule out a correspondence view.

[13] What I mean, here, is that sentences might be secondary truth-bearers, and propositions might be primary truth-bearers. But this is not the place for a metaphysical analysis of assertion, sentence tokens vs. sentence types, and propositions *qua* abstract entities.

'Going for realism is going for a *philosophical package* which includes a naturalised approach to human knowledge and a belief that the world has an objective natural-kind structure.'[14] Now I daresay that I am not the only self-professed realist – that is *realist about science*, not 'scientific realist' – who would want to strenuously object, *pace* Papineau and Boyd, from whom Psillos says he learned this 'lesson'. But I shall save my criticism of this thesis, an outline of which follows, until later:

Naturalist Thesis (NT):

α) Natural kinds are part of the structure of the *physical world*: to say that two tokens are of the same kind is to say that they have the same '*internal structure*'.[15] (Although 'a kind-term refers to a natural kind [only] by virtue of the fact that the body of information which is typically associated with a kind-term has its *causal origin* in the kind-constitutive properties of the kind.'[16] This is a descriptivist moderation of a full-blooded causal theory of reference.) Essentially, there are only natural kinds if there are 'kind-constitutive properties'.[17] These are 'natural properties' and there are also 'natural relations'; in general, the world is populated by entities which fall into 'natural classes'.[18]

ω) Human knowledge is to be explained in terms of α.

Now while there are other realist theses about science which might be suggested, such as a methodological thesis, involving the claim that natural scientists who are also realists are better equipped to perform successful natural science, the foregoing classification scheme should prove sufficient for my purposes.[19] That is, in so far as it imposes a useful structure upon the debate, which should not be taken to be final, or binding. For all too often, it is strict adherence to such conventional schemes that leads debates astray, or distracts attention from promising alternatives.

Armed with these theses, it is possible to elucidate the claims of specific philosophers with greater ease. For example, van Fraassen, in his *Scientific Image*, might be seen to be making the following claims, among others:

$$MT \,\&\, ST \,\&\, AT \,\nvDash\, \sim\Box\, ET$$

$$MT \,\&\, ST \,\&\, AT \,\nvDash\, \sim\Box\, TT$$

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[14] Psillos[1999], p.xix.

[15] Ibid., p.285.

[16] Ibid, p.287.

[17] Ibid, p.288.

[18] Ibid., p.66. And note that what is meant here is metaphysically presumptuous; Psillos means *classes*, not *categories*.

[19] Such a methodological thesis is the concern of Hendry [1995] and Rowbottom [2002]; however, while such a thesis might raise the plausibility of realism about science, were it to stand, it remains unclear that it would have any decisive *philosophical* impact. Primarily, this is because even if it is right that scientists should (or do) behave *as if* their theories are true, or approximately true, this does not entail that they are. Moreover, mere acceptance of theory, rather than belief in its truth, might suffice for all practical purposes; this is the view defended in van Fraassen [1980].

Similarly, one might take Popper's core claim – at least, the one that has been subjected to criticism by Lakatos and Putnam[20] – to be something like:

◊(MT & ST & AT & ~ET & TT)

In common, and as alluded to above, both van Fraassen (constructive empiricist) and Popper (critical rationalist) have objections to ET on inferential grounds; Popper because of his general anti-inductivist and anti-justificationist stance, van Fraassen because of his doubts about the validity of abduction as a means to judge between empirically equivalent theories which posit different unobservable entities (*inter alia*). But Popper claims that he is a realist, whereas van Fraassen claims that he is not; crucially, they differ on their beliefs about the truth of TT. Here, I mention this merely as an illustration of the fruitfulness of the theses I have proposed, as aids to discussion, but I shall return to this issue in greater depth later.

2.   SCIENTIFIC REALISM DESSICATED

As explained in the introduction to this chapter, I intend to attack 'scientific realism' on three distinct grounds. I can now add that in doing so, I will accept throughout that ST and AT are true. This is *not* just a matter of convenience, however: since these theses are generally agreed on by proponents of the positions I inveigh against herein (viz. scientific realism, and later empiricism), and I am also predisposed towards them, they constitute common ground on which the debate may take place. Besides, since each of my objections is explicitly expressed in conditional form, it is my hope that the reader will remain aware of these assumptions, which might be challenged in their own right.

Before I continue, however, I should add a word about the formal logical notation that I employ in some of this chapter. It is there just to make it clear what I think the relations between the theses are, understood broadly – as a heuristic, to aid in understanding. Strictly speaking, then, some of my claims might be a little loose: for example, were I to write that ST ⊨ MT & AT, I might better be understood to be saying ST & x ⊨ AT & MT, where x is implicit, rather than explicit, *in my initial formulation* of ST. (Remember, all the theses, such as ST, involve a large number of propositions.) Such problems are unavoidable when dealing with a subject matter of this complexity, and my arguments in no sense depend upon the formalism I use in an attempt to present them in a pellucid fashion. In short, if there are faults that I have missed in any of my reasoning, I should like to make them easy for others to spot; this is the aim of the notation.

Now if MR is taken to denote a weak form of Medieval realism, involving the claim that abstracta *might* exist, then my first line of attack, in I.2.1, might be expressed as follows: ST & AT ⊨ MT & MR. That is to say, if the semantic and alethic theses are true, then the metaphysical thesis is true, and abstracta might exist. Indeed, along related lines, I shall also defend the claim that: MT & ST & AT & ET ⊨ MR. This is a serious immanent critique of 'scientific realism' when it is taken to be devoid of NT, and its aim is to show that MT is not sufficient, taken alone, to account for the

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[20] Lakatos [1969] and Putnam [1969].

metaphysical commitments inherent in the position. Further, it links in to my third objection, as will become clear.

My second objection is simply, and directly, AT $\vdash \sim$ET; in particular, if truth is absolute, and truth-makers are objective and mind-independent entities, then theories cannot achieve degrees of confirmation. This will prove to be the most difficult and time consuming of my objections to defend, running through three distinct sections (I.2.2-I.2.4), but requires careful treatment due to its highly controversial nature. The ground, as already mentioned, will be inferential; specifically, I will follow Popper in denying both that there is such a thing as justification, in so far as that might involve 'good reasons' of a Humean sort, and that induction is a rational, or failing that genuine, form of inference. In short, as Popper explains towards the beginning of his *Logik der Forschung*, this move involves a separation of the context of justification from the context of discovery.[21]

Third, I shall cast serious doubt on NT, in I.2.5, by showing $\Diamond$(MT & ST & ET & TT & AT & $\sim$NT). The significance of this result is not to be underestimated, and will be highlighted, in particular, by consideration of the anti-scientistic consequence to the effect that (MT & ST & ET & AT) $\not\vdash \Diamond \sim$NT. In plain English, the point is that even if our theories in contemporary science are approximately true in the sense that the 'scientific realist' intends (which may itself legitimately be doubted), it does not follow that the naturalist thesis – or more generally, naturalism – is entailed.[22] Nor indeed is it rendered any 'more probable', as my second objection to scientific realism, outlined above, should serve to make clear. What is needed for a defensible realism about science is, instead, a metaphysical core which is more robust than MT (from the first objection), yet simultaneously yielding to the possibility that the naturalist thesis is utterly incorrect, both on its account of kinds, and indeed on its hasty epistemological proclamations.

With scientific realism fully dessicated, it will then be my goal to reconstitute the remnants in the subsequent chapters, and arrive at a suitable replacement.

2.1 THE INSUFFICIENCY OF THE METAPHYSICAL THESIS

The metaphysical thesis of scientific realism is plausibly its foundation stone, yet many authors on the position tend to rush over it, or even to advocate it as some sort of common sense basis for the position, which is not itself in need of analysis, let alone defence (or, for many of these authors, the justification which they profess to think so vital). Psillos runs it together with AT, and holds that it is part of 'a basic philosophical presupposition' made to avoid idealism, phenomenalism, and verificationism.[23] Newton-Smith does the same – both he and Psillos seem to dismiss

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[21] Popper [1959], ch.1, sections 2 & 3.

[22] Naturally, it is important to remember that they may be approximately true, *whether or not we can establish that they are*. It is only our alleged *ability to establish that they are approximately true* that comes under attack in my second objection. (ET has two central components, which should not be conflated, although I have grouped them for convenience.)

[23] Psillos [1999], p.xix. Against him, it can obviously stand without the AT, even when characterised in his own words, as being simply the claim 'that the world has a definite and mind-independent

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positions such as Kant's without a mention[24] – and adds a bizarre comment about the inadequacy of a view expressed in the *Tractatus Logico-Philosophicus*, by characterising *minimal realism* as:

> [T]he assumption that scientific propositions are true or false where truth is understood in terms of a cleaned-up version of the correspondence theory of truth. By this latter qualification I mean that we are assuming that to be true (false) is to be true (false) in virtue of how the world is independently of ourselves. The notion of correspondence is not to be understood as, say, propositions picturing or mirroring the world *à la* early Wittgenstein.[25]

For Rescher, who at least recognises its independence, MT is posited on a pragmatic basis, and its acceptance is necessary before the very practice of natural science could be thought worthwhile: '[It is a] postulation made on *functional* rather than *evidential* grounds.'[26] And even Popper seems to despair of treating its defence rigorously, instead admitting that idealism is irrefutable, while maintaining that '[Metaphysical] realism is so obviously true that even a straightforward argument... is just a little distasteful.'[27] (Note well: Popper's use of 'metaphysical realism' should not be confused with that of metaphysicians such as Loux, who use the same tag to refer to what I call 'Medieval realism', that is belief in the existence of universals.[28])

Now I have started this section in such a vein in order to draw attention to a genuine scandal in post-Kantian philosophy. It consists of the tame acceptance that *the* fundamental question – about the very nature of the world, and our interaction with it – is quite beyond our ability to answer decisively. If we *asseverate* that MT is *just* a presupposition, then we should want to take work in the Kantian tradition, *inter alia*, as seriously as we do the claims of modern natural science. Worse, if MT can only be defended on the basis of a pragmatic turn, then so much the better for pragmatism; so much the better, that is, for positions such as constructive empiricism. For is it not an inauspicious start for scientific realism to be founded on a pragmatic move, when its proponents want to vie against the pragmatic moves of others, not to mention wholesale pragmatism? I answer in the affirmative, and would urge that it is one thing to admit one's fallibility, but quite another to invite the sceptic in.

However, let me return to the work of Popper, which has been characterised somewhat unfairly above. First, because Popper holds that MT plays no part as a presupposition in his epistemology of science, as such. Second, because he does not

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natural-kind structure'. To reiterate my earlier point, in I.1: it says nothing *explicit* about what truth-makers are. One might even claim that the MT is true on an anti-realist account of truth!

[24] See, for example, Kant [1787], A49. He writes: '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.'

[25] Newton-Smith [1981], pp.28-29. His ruling out of the notion of propositions 'picturing or mirroring' the world is mistaken; he still wants to have them as truth-bearers himself, so all that is in dispute, here, is *how they bear the truth*. Needless to say, this has nothing, whatsoever, to do with what *truth-makers* are.

[26] Rescher [1987], p.126. However, he does seem to shoot himself in the foot a little later, on p. 140, when he writes that 'The utility of the conception of reality is such that even if reality were not there, we would have to invent it.' Such a comment only plays into the hands of the Neo-Kantian.

[27] Popper [1983], p.85.

[28] See Loux [1998], ch.1.

confuse it with the issues surrounding truth – indeed, he thinks, perhaps mistakenly, that Tarski's account is sufficient for his purposes. Third, because he thinks that there are strong critical reasons for believing in MT, and that these are not merely pragmatic. In his words:

> Metaphysical realism is nowhere used to support any of the solutions proposed in *L.Sc.D* [The Logic of Scientific Discovery]... It forms a background that gives point to our search for truth. Rational discussion, that is, critical argument in the interest of getting nearer to the truth, would be pointless without an objective reality, a world which we make it our task to discover... [T]here is no factual knowledge which is supported by more or by stronger (even though inconclusive) arguments.[29]

Now with all this I agree, except the claim that the arguments against idealism are 'inconclusive', beyond the sense in which the conclusion of any argument is never absolutely certain. Rather, I hold that the arguments are decisive, *when it is taken as a premiss that truth is absolute*, because all idealist talk must be understood to be veiled realist talk, if it is not to be strictly meaningless, or allegorical.[30] Here, I raise the issue only in order to give an early suggestion of just how shaky MT really is, even by the admission of the scientific realist camp. And I shall argue below that this is because it is far too weak – in so far as vague and diaphanous – a thesis.

Beforehand, I should state clearly that I do believe there is an intimate link between MT and AT, and that it is not surprising, as such, to find some authors running them together. My objection to such an approach is just that it neglects the analysis of this link, between two *logically distinct* sets of claims, which proves quite revealing. Indeed, it is plausible that the MT and the AT should be combined with the benefit of an appropriate gel, in order to provide a decent foundation for a realist take on natural science. The gel is precisely that which is missing from too many realist accounts.

ST & AT $\models$ *MT & MR*

According to ST, theories in natural science should be understood literally and are capable of being true or false, so construed. According to AT, assertions are truth-bearers, and truth-makers are mind-independent, objective, entities. Thus, were the assertions issued by (or 'that compose') theories in natural science to be true, the entities thereby referred to would exist. But this draws our attention to a serious problem, which tends to hamper much work in the philosophy of science: what, precisely, are theories? Unfortunately, I cannot hope to give a satisfactory answer to this important question here, so I will remain agnostic on the issue. What I will point out, instead, is that it should be evident that modern physics involves talk of 'electrons', chemistry of 'bonds', and biology of 'cells'. And for present purposes, it is sufficient for us to recognise that if one accepts AT and ST, one should accept that such talk is about posited entities. Entities that really would exist, were particular theories in contemporary natural science to be true.

Now given this, MT follows as a direct consequence, on the assumption that at least one possible scientific theory – not necessarily a contemporary one – must be true.

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[29] Popper [1983], pp.81-83.
[30] See I.2.2, under the sub-section 'From Knowledge as 'Justified True Belief' to Fallibilism'.

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This should hardly be surprising, since ST and AT provide accounts of what it is for something to be true (viz. to have mind-independent, objective, truth-makers), and what sort of things (viz. assertions, and theories) have the potential to be true. For example, ask a scientist whether electrons, bonds, or cells were around before humans, *according to the accepted view in science*, and she will state that they were. Thus, according to said view, they do not depend upon our existence, or upon us undertaking any activity (such as imagining, conceiving, believing, etc.), for their existence. They are mind-independent, and objective: natural scientists don't bother generating theories about things that aren't, like money. (If everyone on earth were to cease to believe in money, or to be eradicated, there would be no money. This, although many notes and coins might, of course, remain.) Another way of looking at this is that both MT and AT are necessary prerequisites for ST – if they fall, it falls, and ST & AT $\models$ MT is a sound argument just because ~MT υ ~AT $\models$ ~ST, as is evinced by the *actual claims in actual natural science*. (I am at pains to emphasise, however, that ◊(MT & AT & ~ST).)

It should be noticed, however, that there is absolutely nothing about ST that precludes the existence of objects that serve as truth-makers, but do not exist 'in' space and time. Still further, ST in no way denies that such entities could play a part in scientific theories, at least *in principle*. Thus, ST is consistent with the following claim:

Weak Medieval Realism (MR): Abstracta might exist.[31]

This result is a direct consequence of the fact that none of the theses treated here, indeed none of those outlined in the previous section save NT, serve to restrict the domain of natural science beyond that which is mind-independent and objective. In other words, it could not be thought *verboten*, on the basis of MT, AT, and ST, for a scientist to put forward a theory (or world-view) that gave active roles to entities such as universals, or sets. What is more, and this may seem curious, it could not even be thought wrong for a scientist to put forward a theory that had elements which were not supposed to be taken to be actual. (A good example would be one that depended upon Plantinga's theory of possible worlds.[32] As Armstrong puts it: '[I]t is often *true* that some state of affairs is possible, although the state of affairs does not obtain. Again, it is often true that it is possible that some object exists, although the object does not exist. What truthmakers are we going to supply for these truths?'[33]) There is no mention of either *actual world* or *physical 'world'* (which I take to mean physical things in the actual world, subject to the *caveat* in footnote 31) in MT, AT, or ST, although perhaps there should be.

Already, then, it becomes clear that MT is insufficient to provide a proper basis for scientific inquiry, let alone a serious philosophical account of science, even when conjoined with AT. The assumptions that must be made in order to enable these

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[31] Here I use 'abstract' in the sense of 'non-concrete', and am broadly partial to the account of abstract entities given in section II of Lowe [1995]: 'To exist *in* space and time is not to have a special kind of existence – for the notion of existence, like that of identity, is univocal. Rather, it is just to have certain sorts of properties and relations – spatiotemporal ones.'

[32] See Plantinga [1974], IV.3.

[33] Armstrong [1997], p.149.

projects are meatier, and I shall return to this point after I have shown why NT, which attempts to attend to the omissions revealed above, is far too presumptuous.

There is, however, a possible line of objection to my main argument here, which runs as follows: on the assumption that the contemporary (mature, predictively successful) theories in natural science are approximately true, abstracta do not exist. The thought would be that although science might have allowed abstracta to be posited, at one time or another, it has already ruled them out as potential 'inhabitants' of the world.[34] I now turn to the rejection of this claim.

MT & ST & AT & ET ⊧ MR

To many scientific realists, talk of abstracta may seem to be quite out of place in contemporary science. It may seem that entities in such a category – those 'outside' space and time – have no place in scientific discourse, and are certainly not employed in current theories. For example, I have already mentioned posited entities such as the electron, bond, and cell – entities that many scientific realists would take to exist. But each of these is 'located' in space and time, according to the *status quo* in science, is it not?

True. But now one might ask the scientific realist how to characterise such an entity: take the electron as an example. Is it not the case that he would want to say it is a natural kind, rather than a classification made on the basis of convention? That it is an entity-type that has been discovered, rather than invented? Further, would he not want to characterise the electron as possessing a definite rest mass of (approx.) 9.109390×10^{-31} kg, a definite charge of (approx.) $-1.602177 \times 10^{-19}$ C, and a spin of 1/2? If so, then he needs to think quite carefully about what he is saying. First, because it is far from obvious that the natural kind 'electron', rather than any individual electron, exists in space and time, or 'exists' only in so far as it is a useful way of talking about similar things. Second, because mass, charge, and spin all presumably exist, at least according to the scientific realist – there are plenty of m-terms, q-terms, and indeed S-terms, in the mathematical laws of physics. A few examples:[35]

Relativistic Momentum Equation: $\mathbf{p} = \gamma m_0 \mathbf{v}$

Coulomb's Law of Electrostatics: $\mathbf{F} = q_1 q_2 / 4\pi\varepsilon_0 r^2$

Magnetic Moment of Electron (about axis i): $\mu_i = q_e S_i / m_e$

The final equation provides the most striking example, since it involves the use of each of the aforementioned terms. And it might be understood, by anyone with good physical intuition (something akin to Duhem's *bon sens*, perhaps[36]), to show how each of the properties that these correspond to are responsible for the electron having

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[34] An analogous claim could be made about non-actual entities having been ruled out, but I take this to be so obviously false that it does not merit discussion.

[35] I would agree with Lowe that all these laws have 'from a logical point of view...the same status as such grandiose principles as Newton's laws of motion'. See Lowe [1987], p.326.

[36] Duhem [1954], pp.216-218.

a magnetic moment (about a given axis): in other words, that all μ-property talk, with respect to electrons, is translatable into talk of more fundamental properties. The equation might be understood to (strongly) imply that electrons only have magnetic moments in so far as they have spin, charge, and mass. Further, such a claim is defensible from a historical perspective, for an electron possessing both definite charge and rest mass was posited long before the possibility of its possessing a 'spin' had been considered; this was a suggestion first made in 1925, by Goudsmidt and Uhlenbeck.[37] (That electrons had a magnetic moment was, of course, suggested before 1925, e.g. in the 1922 experiments of Stern and Gerlach.[38] But such moments were never just assumed to be primitive properties of the electron; this was due, in no small part, to Maxwell's equations. In particular, $\nabla \cdot \mathbf{B} = 0$, from Gauss' law, which precludes the existence of magnetic monopoles, and $\nabla \times \mathbf{B} - \mu_0 \varepsilon_0 \partial\mathbf{E}/\partial t = \mu_0 \mathbf{J}$, the modified version of Ampère's law[39], which suggests that charge movement, or charge change, is responsible for magnetic effects. I have run a similar argument, elsewhere, which defends the view that the speed of light may be seen only to be a consequence of the permittivity and permeability of free space, plus Maxwell's laws/equations.[40] The key equation, which may be derived from these laws (acting in free space), is: $c = 1/\sqrt{(\mu_0 \varepsilon_0)}$.)

A step back from the detailed physics, though. What are these things that electrons – *inter alia* – are said to possess? For example, what are *mass* and *charge*? According to the austere scientific realist, the theories containing these terms are approximately true, and thus it would seem to follow that mass and charge are properties, and are not be confused with those concreta capable of possessing them. But an account of concrete objects as bundles of compresent tropes is by no means suggested by anything *in* physics; if anything, such a view would be discouraged by the manner in which many laws involve relations only between properties, e.g. $K = mv^2/2$ in classical mechanics. In plain language, the kinetic energy of any body is proportional to both its mass, and the square of its speed[41]: nothing else about the body – whether it is green or red, small or big, round or square, plastic or elastic, metallic or wooden – matters. So it would not appear to be just *a law about bodies*, at least at first sight; rather, it would seem to stipulate a relationship between properties either necessarily (mass) or contingently (velocity, under a classical understanding[42]) possessed by bodies.

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[37] G.E. Uhlenbeck and S. Goudsmit, *Naturwissenschaften* **47** (1925), 953. There is also a rather interesting transcript of a talk by Goudsmit, on the discovery of spin, which is available at the following URL: http://www.lorentz.leidenuniv.nl/history/spin/goudsmit.html. He says, for instance: 'When the day came I had to tell Uhlenbeck about the Pauli principle - of course using my own quantum numbers - then he said to me: "But don't you see what this implies? It means that there is a fourth degree of freedom for the electron. It means that the electron has a spin, that it rotates"... And when he said: "That means a fourth degree of freedom", then I asked him: "What is a degree of freedom?"'

[38] Stern, O, and Gerlach, W., 'Der experimentelle Nachweis der Richtungsquantelung im Magnetfeld', *Z.Phys.* **9**, 349-352.

[39] The initial version of Ampere's law would have read $\nabla \times \mathbf{B} = \mu_0 \mathbf{J}$, but Maxwell modified it to include a displacement current, and provide greater symmetry with Faraday' law of induction, $\nabla \times \mathbf{E} + \partial\mathbf{B}/\partial t = 0$.

[40] Rowbottom [2002], 'The Semantic Thesis', in section 1.1.

[41] This, for $v^2 = |v|^2$. Kinetic energy is a scalar, and only dependent upon the product of two scalars, in the non-relativistic case.

[42] Take the equipartition theorem, which states that a gas molecule has an average kinetic energy of kT/2 per degree of freedom; at absolute zero, then, the average kinetic energy would be zero, thus the

Furthermore, to return to electrons, we might also note that what is supposed to be essential to them is not merely *that they possess* a mass and a charge, but rather that they possess a specific mass and specific charge. Now this may seem to suggest that it is essential to the electron that it possesses particular modes, or tropes, of the corresponding universals. Two roses may be of the same species while displaying different shades of red, but two electrons may never have different 'quantities of matter' composing them.[43]

In summary, then, it is not the case that MR is precluded, and unclear how it could ever be conclusively precluded, by scientific theories, or more generally in the process of empirical inquiry, even if MT, ST, AT, *and* ET hold.[44] But this should not really be a surprise, since empirical inquiry is restricted to the domain of the *physical*, or what might be better understood to be the *concrete*.[45] Indeed, it is this very fact that makes it radically dependent upon metaphysical assumptions – one is precisely *that* there is a distinction between the concrete and the abstract, *whether or not any abstracta exist* – be they implicit or explicit. But I do not want to suggest, here, that all natural scientists, such as physicists, *need* to study the work of contemporary metaphysicians in order to press ahead. Indeed, it would be undesirable to restrict them in such a way; they need not worry overly about the underdetermination of theories by evidence, except in periods of crisis. Let them press ahead, and leave it to the metaphysicians (or *theoretical* physicists) to unveil what is compatible, if anything, with the 'predictive machinery', or 'experimental laws', which they produce.[46] As Duhem memorably puts it:

> To explain (explicate, *explicare*) is to strip reality of the appearances covering it like a veil, in order to see the bare reality itself [to this, the scientific realist does, I hold, commit]... Therefore, *if the aim of physical theories is to explain experimental laws, theoretical physics is not an autonomous science; it is subordinate to metaphysics.*[47]

Now Duhem was convinced about the autonomy of physics, but what he failed to recognise is that it may be effectively 'autonomous', descriptively speaking, if the bulk of non-theoretical physicists are just uncritical about the ontologies that they employ, and adopt dominant research programmes in order to 'puzzle solve' in the sense suggested by Kuhn. And I say this without suggesting that they would be *right*

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kinetic energy of each molecule would be zero, since the kinetic energy of any molecule could not take a value lower than zero, unless there were negative masses. (According to the quantum theory of heat capacities, on the other hand, rotational and vibrational energy of molecules is quantised. For example, a diatomic molecule could never have less vibrational energy than $h\omega/4\pi$, where $\omega=\sqrt{(k/m)}$, and m is reduced mass, m_1m_2/m_1+m_2.)

[43] It is worth noting that this construal of mass is not the only one, but I need not treat the incommensurability issue here. See van Fraassen [2002], pp.113-114.

[44] One important *caveat*: if a fictionalist or nominalist account of the role of mathematics in science is correct, then my argument might very well fail. But I do not take such an account to be part of 'scientific realism', and it would seem to be an *ad hoc* addition if it were cobbled on merely to motivate the rejection of MR.

[45] I come to demarcation later, in II.4.

[46] I might add that there are some physicists who are very 'metaphysically minded', and strangely enough they tend to be associated with revolutionary theories. Bohm is a case in point: see II.1.3.

[47] Duhem [1954], pp.7-10. It should be noted, though, that some empiricists, e.g. van Fraassen, adopt different views of explanation.

to do so – that they would be doing the best possible science, or anything better than *applied* science – although I postpone detailed discussion of this issue until II.4.

For the moment, let me just point to an illuminating discussion by Popper, in which he points out that many scientific papers are structured in an entirely misleading fashion, 'an inductive style', which involves a listing of observations in a manner that appears to be completely theory-neutral, and only then the so-called 'generation' of a theory. He writes:

> No doubt the idea which inspires the inductive style – the idea of adhering strictly to the observed facts and of excluding bias and prejudice – is laudable.... [But] objectivity, and also unbiased observation, are the results of criticism, including the criticism of observational reports. For we cannot avoid or suppress our theories, or prevent them from influencing our observations; yet we can try to recognize them as hypothetical and to formulate them explicitly, so that they may be criticized.[48]

Indeed, this 'inductive style' is demanded of pupils from the time they start to study natural sciences – I can corroborate that this expectation continues at undergraduate level – although it serves to encourage avoidance of foundational issues. And there is a simple lesson here, on which many a philosopher of science can agree: that we should look to what natural scientists actually do, rather just than what they *say* that they do, or how they present their work, if we are to understand why so many might think that science does *not* involve (or rest upon implicit) metaphysics. As Zahar puts it:

> In certain cases, scientists have written about their methodology and about their heuristics. However, the scientist's reports cannot always be taken at face value: he may either knowingly disguise the truth, or else he may be sincere but have a false consciousness of his own activity... a scientists' explicitly professed methodology, as distinct from his singular and often tacit value-judgments, has by itself no methodological significance.[49]

This said, I shall revisit the issue of the ontological status of properties in I.2.5, where I argue that NT is plausibly incompatible with the other components of 'scientific realism'. And it should be remembered that my core claim is as follows: NT is needed just because MT is too weak and vague, but NT is far too strong, viz. unmitigated.

2.2   AGAINST JUSTIFICATIONISM AND INDUCTION: *QUID JURIS* (AT $\models$ ~ET)

The goal of science has come to be envisaged as the accumulation of highly confirmed, or highly probable, or well-supported hypotheses; and only derivatively as an accumulation of truths. – Miller[50]

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[48] Popper [1983], p.48.
[49] Zahar [1989], pp.3-4
[50] Miller [1994], p.2.

A common dogma, widely adhered to in contemporary philosophy, is that knowledge is justified true belief.[51] To know that one knew that a proposition was true, under this understanding, would be to have a *completely justified* true belief, on pain of allowing for an infinite regress of 'knowing': for justified true beliefs, about justified true beliefs, *ad infinitum*.[52] But is such *complete justification* really attainable?

Before I turn to answer this question, though, a word about terminology. For 'justificationism' is a rather old notion in philosophy of science, but could be taken, by contemporary epistemologists, to mean something other than that which I intend. Thus, I offer a straightforward definition from Lakatos:

> Justificationism, that is, the identification of knowledge with proven knowledge, was the dominant tradition in rational thought throughout the ages... It turned out that *all theories are equally unprovable*... classical justificationists feared that once they conceded that theoretical science is unprovable, they would have to conclude that it is sophistry and illusion, a dishonest fraud. The philosophical importance of *probabilism* (or '*neojustificationism*') lies in the denial that such a conclusion is necessary.[53]

For convenience, I do not use the tokens 'justificationism' and 'neojustificationism' in what follows, but bundle both under the former term; by remaining cognisant of this, the reader should avoid confusion.

From Knowledge as 'Justified True Belief' to Fallibilism

> The basic statements at which we stop, which we decide to accept as satisfactory, and as sufficiently tested, have admittedly the character of *dogmas*, but only in so far as we may desist from justifying them by further arguments (or further tests). – Popper[54]

Nowadays, the majority of philosophers hold that most, if not all, synthetic knowledge is fallible. (Indeed, following Quine, some would hold that there is not even a legitimate analytic-synthetic distinction to be made.[55]) In the case of much *a posteriori* knowledge, because it is a general feature of ampliative (e.g. inductive) arguments that their consequences are only partially entailed by their premises; in other words, that they are not always truth-preserving. As Psillos, a scientific realist, puts it: 'certainty in decision procedures is a utopian aim, and hence... it must be abandoned.'[56] In the case of much supposedly *a priori* knowledge, because history has taught us that many axioms which once seemed self-evident – for example, that two parallel lines never diverge or converge – were plausibly just conventions; that they were not just concepts, or categorial axioms, to which we were in some sense

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[51] Here, I refer to knowledge in an informational sense, rather than as competence (e.g. 'I know how to play the violin'), or acquaintance (e.g. 'I know Jonathan Lowe'). See Lehrer [1990], pp.3-4, and also my discussion in II.3.

[52] If a pure coherence model of justification is adopted, viz. coherentism rather than foundationalism, this becomes, instead, a vicious circle of 'knowing'. I put this to one side, for the moment, in the interests of clarity.

[53] Lakatos [1970], pp.94-95.

[54] Popper [1959], section 29.

[55] In his words: 'Any statement can be held true come what may, if we make drastic enough adjustments elsewhere in the system'. Quine [1951], p.43.

[56] Psillos [1999], p.181.

'bound'.[57] And even Lowe, a champion of metaphysical realism, writes that: 'as far as actuality is concerned, metaphysics cannot provide us with certainties.'[58]

Indubitably, Kant deserves a mention here. For he holds to a three-way distinction between opinion, belief, and knowledge, which is made in terms of two conditions, *subjective sufficiency* and *objective sufficiency*, that correspond, respectively, to *conviction* and *certainty*. To opine is to take 'something to be true with the consciousness that it is subjectively as well as objectively insufficient'. To believe is to take 'something to be true [which] is only subjectively sufficient and is at the same time held to be objectively insufficient'. Finally, to know is to take something to be true which is both objectively sufficient and subjectively sufficient. These relationships are depicted below:

FIG. I.1 – KANT'S ACCOUNT OF OPINION, BELIEF, AND KNOWLEDGE

	SUBJECTIVE SUFFICIENCY (CONVICTION)	OBJECTIVE SUFFICIENCY (CERTAINTY)
OPINION	✗	✗
BELIEF	✓	✗
KNOWLEDGE	✓	✓

Yet Kant also wants to have it that:

> I must never undertake to have an opinion without at least knowing something by means of which the in itself merely problematic judgement acquires a connection with truth which, although it is not complete, is nevertheless more than an arbitrary invention. Furthermore, the law of such connection must be certain. For if in regard to this too I have nothing but opinion, then it is all only a game of imagination without the least connection to truth. In judging from pure reason, to have an opinion is not allowed at all. For since it will not be supported on grounds of experience, but everything that is necessary should be cognized *a priori*, the principle of connection requires universality and necessity, thus complete certainty, otherwise no guidance to the truth is forthcoming at all. Hence it is absurd to have an opinion in pure mathematics: one must know, or else refrain from all judgement.[59]

Here, however, the lynchpin of a problem for Kant's account of knowledge becomes apparent. It is as follows: if truth is taken to be mere consensus (not *even* consensus in the ideal limit of inquiry), the same scheme, depicted in the table above, seems to remain perfectly intact. Indeed, a link between conviction and certainty (understood as consensus) becomes apparent, in so far as anything on which all of a community's members agree (viz. are all convinced of) will be, by force of such a definition of truth, certain. And surely this would be to describe a community in a 'dogmatic slumber' – perhaps even a rather *unimaginative* slumber – if ever there were to be one. Admittedly, this criticism does not appear, *prima facie*, to be sufficiently

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[57] To admit this is as problematic for the transcendental realist as it is for the transcendental idealist, I think. So here is a genuine philosophical challenge, which is perhaps suggestive of Fries' trilemma. I discuss this in II.4.

[58] Lowe [1998], p.27.

[59] Kant [1787], A822-A823.

devastating to suggest that Kant's account should be dismissed out of hand. But it does cut, I will contend, to the very core of the problem with his approach.

Essentially, this is because Kant would want to appeal to phenomena as being that which we could know (be convinced of and certain of), and say, further, that such knowing is grounded in the operation of a certain form of *a priori* knowledge – our innate ways of 'seeing', or the necessary preconditions for our experiencing, the world – on the things-in-themselves that 'underlie' the phenomena; hence, his accounts of space and time.[60] (In that sense, Kant has a thoroughgoing empirical realism, which relates only to the *appearances*, emerge from his transcendental idealism. This realism is, I contend, his goal.) But what is to say that such a claim is, itself, not merely an imagined one? What is the relation between the phenomena and the things-in-themselves, or the thinkable and those things which are responsible, in part, for their thinkability? How is Kant even capable of *thinking* that there might be things-in-themselves? Could he be *certain* that there are, without holding that this was merely a way in which he was forced to think of phenomena, which was *itself* a mere precondition of experience? These questions do not seem unfair, for although the things-in-themselves are not outside us in a spatio-temporal sense, on Kant's account, they are nonetheless not held to be a *part of us*, and are thus 'external' in the same sense that abstracta are, on the account of a Platonist. And the point is just that Kant cannot know (be convinced and certain), *according to his own position on what constitutes knowledge* (conviction and certainty), that his account in the Critique is true.[61] A similar line of attack is proffered by BonJour, who writes:

> Suppose that we are concerned with some specific proposition *P* that is apparently synthetic *a priori*. Kant's suggestion is then that we can know *P a priori* in spite of its synthetic character because the mind so operates in structuring or "synthesising" experience as to make *P* invariably true within the experiential realm... But is easy to see that Kant's position offers no reason at all for thinking that the original proposition *P* is even true, let alone justifiable or knowable *a priori*. What would have to be true if Kant's account were correct is, not the original proposition *P*, but rather the apparently quite different proposition: *within the bounds of experience, P*; call this proposition *P**. And thus, insofar as the original intuitive datum to be accounted for is the apparent *a priori* justification of *P* itself, Kant's explanation does not really even speak to the issue.[62]

Now if a correspondence theory of truth is taken to be based on more than the mere appearances – more than the 'phenomenal world' – then there becomes a sense in which it is precisely the 'external' in which one should be interested; and fallibilism is, unavoidably, part of such a package. Yet this need not be of any great concern, just because the recommendation to behave as if one is fallible seems to be, as far can be gathered from the history of ideas, methodologically advisable. That is to say, Kant's requirement for 'judging from pure reason', in the passage quoted above,

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[60] Ibid., A23-A26, and A31-A33.

[61] In so far as Kant shows how the world would have to be, in order for us to have certainty and still be able to learn from experience (while simultaneously holding to a weak version of the correspondence theory of truth, at least), his achievement is most impressive. Alas, he could not show that things *could be such a way*, without appealing to traditional – that is, realist – metaphysics: in other words, his conclusions about 'any future metaphysics' cannot be right on the basis of his argument, since he employs more than that 'future metaphysics' in his Critique.

[62] BonJour [1998], pp.23-24.

seems to be too stringent. Is it is really wrong for one to have the opinion that Fermat's last theorem is true? On what grounds can this claim be criticised, in light of current evidence?[63] Moreover, is it not plausible that extremely useful mathematical operators have sometimes been invented, or discovered, without strict appeal to any prior mathematical 'proofs'? Consider the Dirac delta-function, the status of which was still being debated by mathematicians long after its use was accepted in the physics community.[64]

Thus, I do not recognise the force of the claim that there need be any non-psychological – that is epistemologically significant, in so far as truth-conducive – difference between opinion and belief, albeit that there should be an important difference between belief and knowledge, if a distinction between true belief and false belief is to be upheld. That is to say, there is an important sense in which all beliefs are just opinions; all are subject to revision in the light of criticism, in principle.[65] (Here, the use of 'belief' versus 'opinion' in pre-philosophical discourse is unenlightening. But see the discussion of 'degree of belief', when I cover the subjective interpretation of probability, below.) And if the goal of inquiry is just truth *simpliciter*, in the sense of correspondence to reality, then surely one should only be interested in settling upon beliefs which are true; one does not need certainty, or certainties. Or to put it differently, if there are no such things as certainties, it does not follow that there are no such things as true beliefs. Nor does it follow that true beliefs are unattainable by humans. Lowe puts it so:

> The reason why Kant sought to redefine the nature of metaphysical claims as being claims about the structure of our thought about reality rather than the structure of reality itself is that he believed that only in this way could the absolutely certain and non-empirical character of metaphysical knowledge be explained... however, even granting the truth of this metaphysical assertion, why shouldn't we respond to it by saying *not* that metaphysical knowledge as traditionally conceived is impossible...but rather that metaphysical knowledge is almost never *certain* knowledge – that is, that metaphysical knowledge-claims can almost never be absolutely invulnerable to falsification or disproof?[66]

To go even further, one might point to Kant's central claim, that we reside in some sort of prison of representations; whatever that is supposed to mean, without allowing for traditional metaphysics (and in particular, distinctions of *being* between things in themselves and things in themselves as thought). In his own words:

> If we let outer objects count as things in themselves, then it is absolutely impossible to comprehend how we are to acquire cognition of their reality outside us, since we base this merely on the representation, which is in us.[67]

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[63] No concrete instances have been found to allow this claim to be strongly criticised, yet surely it is *falsifiable*, even if the means by which it is derivable cannot be found.

[64] See Lützen [1982].

[65] Here I gesture again towards the difference between context of justification and context of discovery, which I will deal with in greater depth later. See Popper [1959], pp.31-32, 'Elimination of Psychologism'. Another way of explaining this is via the Quinean 'web of beliefs'. See Quine [1951].

[66] Lowe [2002], p.9. I would want to delete the mentions of 'almost', since they seem to take the sting out of this point. I discuss this further in III.5.3.

[67] Kant [1787]. A378.

Stroud characterises this passage as a clear indication that 'The doctrine of the epistemic priority of representations... seems simply to have been removed from the empirical to the transcendental level.'[68] But this doctrine, which arguably has a Cartesian root, is not itself defended by Kant, and is precisely the one that any self-respecting transcendental realist would want to criticise in the strongest terms.[69] First, because even were it the case that we could not 'comprehend' such a link, it would obviously not follow that there was *not* such a link; rather, there might simply be a limit to our explanatory ability. (Taken alone, this is sufficient to show that Kant's position is fallible.) Second, and more destructively, just because other alternatives are, I would claim, perfectly comprehensible. In the interests of brevity, I will only offer two examples, and leave the rest as an exercise to the reader:

i)   I, if I exist, am either part of a world containing more than one entity, or am the sole entity that constitutes the world. In the first case, I may either enter into relations with other entities in the world, or not. (Here, for simplicity's sake, I do not consider the fact that I may have parts, and other entities may have parts, which might enter into internal and external relations as well. In any event, it is by no means necessary that a relation between two entities, taken holistically, need be affected by the internal or external relations of the parts of those entities which enter into it.) If the former, then the relations between myself and other entities will depend for their existence upon both myself, and the other entities in the world. Moreover, it is not necessary that I should stand in only one relation to each other entity; I could stand in many such relations. There could be conceivability relations. There could also be perceivability relations. There could be relations responsible for my *a priori* knowledge of categories of being; said knowledge might also be pre-rational and innate, although dormant and unrecognisable without the application of reason, in a similar fashion to that suggested in Plato's *Meno*.[70] (Under such an understanding, it is clear that the conversion of such insight into explicit and criticisable conjectures would involve considerable work.)

ii)   Just as it may be a necessary precondition of my experience that there is something to experience, it may also be a necessary precondition of my ability to conceive that there are things to conceive of. Just as it does not follow that I need experience only myself, or parts of myself, it does not follow that I need conceive only of myself, or parts of myself. Furthermore, it simply does not follow that things in themselves are not responsible for my conceiving of things in themselves, and so forth. (And Kant does conceive of things in themselves, as well as things in themselves as thought.)

None of this is *verboten* in the realm of metaphysical possibility; I do not assert that these outlines successfully describe the actual world, only that they *might*. And I find the foregoing to be pellucid, and quite comprehensible. Indeed, I also understand why transcendental idealism might appeal to some – I confess it is common to see some statements as certain – but also see why there is simply no certainty that there are

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[68] Stroud [1984], pp.163-164.

[69] This links in to my discussion in II.2, and is a core contention of the thesis. And indeed the next sub-section is suggestive of the discussion in II.3: as such, there are glimpses of the position I wish to develop that already begin to emerge here, but which require weaving together.

[70] Plato, *Meno*, 79e-86c.

certainties. And overall my point is that the links I speculate about may very well be comprehensible without being *analysable*; they may be fundamental, in the same way that some take personhood to be.

From Fallibilism to the Rejection of Justificationism[71]

...induction being weaker than deduction, we now get merely an *Ersatz* certainty: probability comes in as the substitute, or surrogate, of certainty – not quite the thing, but at least the next best thing, and at any rate approaching it. All this is unacceptable. – Popper[72]

With one's fallibility accepted, one might nonetheless still want to hold that a belief must be *sufficiently justified*, as well as true, in order for it to constitute knowledge; but *sufficient justification* would, then, consist of something other than certainty (e.g. something other than a conclusion derived from a valid argument based on 'a self-evidently' truth-preserving form of inference, and a set of axioms which were 'self-evident truths'). However, I shall argue, following Popper, Bartley, and Miller, that there is no such thing as justification, because there are no such things as good reasons, albeit that there may be 'the subjective feeling of being in possession of good reasons'.[73] Moreover, that being in possession of 'good reasons' for holding any given true belief would be of no more practical benefit than merely holding said true belief, and that it is not methodologically necessary for us to seek out 'good reasons' in order to make rational decisions. (As Miller puts it, the goal should be 'rational decision-making', rather than 'rational-decision making'.[74])

Take a simple example: imagine that both I, and my supervisor, predict that the internal examiner of this PhD thesis will be a philosopher of science (viz. will have 'philosophy of science' as his, or her, area of specialisation).[75] I predict this simply because I believe that the only suitable internal examiner, in Durham's philosophy department, is Robin Hendry. My supervisor, on the other hand, predicts the same only because he has heard that Robin Hendry is about to be sacked, and replaced by another philosopher of science, Dr. G, who will be even better equipped to examine this thesis, because she has great expertise in metaphysics. Now imagine that Robin Hendry does, indeed, get the sack, and is replaced by Dr. G; further, that she is subsequently selected as the internal examiner of this thesis.

Was my supervisor's prediction *sufficiently justified*? Presumably, the justificationist would want to answer in the positive, or at least hold that it was *highly justified*. Was

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[71] It must be added, however, that one could opt for anti-realism, or reject AT, instead. For example, a 'justification' condition may be based on consensus, and coupled to a theory of truth based on consensus. In any thesis on such a broad topic, some avenues will remain open (in so far as not critically examined), but it must be remembered that my primary targets are scientific realism and empiricism, particularly constructive empiricism, both of which generally involve AT.

[72] Popper [1983], p.222.

[73] Miller [1994], p.66.

[74] Ibid., p.43.

[75] The example used here might seem to be closely related to that presented in Gettier [1963], pp.121-123. But notice that I am not attempting to formulate a situation such that there needs to be a condition in addition to justification in order for a true belief to constitute 'knowledge'. Rather, I am suggesting that one might attempt to resolve the apparent problem in this case – albeit, as I will urge below, mistakenly – by appeal to *degrees* of justification. See also Russell [1912], ch.13.

my prediction, by comparison? If I were a justificationist, I should want to say that it had *some justification*, at the very least; merely because I had no 'good reason(s)' to believe that Robin Hendry was about to be sacked, it does not follow that my prediction was not 'partially entailed', or 'rendered more probable', by the premises that I accepted.[76] Intuitively, in common parlance, one might want to say that I did not know the internal examiner of this thesis would be a philosopher of science. But then, it might be held that my supervisor *did know*, even though his prediction may also have been wrong, just because Dr. G may have been run over by a bus before being able to examine this thesis! The difference might seem to be only that my supervisor's prediction was *more justified* than mine, whether or not it was *sufficiently justified*.

One notices, then, that both the predictions made above might be understood, by the justificationist, to be inductive in nature: 'a matter of weighing evidence, and judging likelihood, not of proof.'[77] And one begins to wonder, therefore, what use justification could actually be in the quest for truth, and how such a notion, along with that of a 'good reason', could be formally represented. What is needed is an account of how evidence (one set of propositions, premises, which are accepted as true, or held themselves for 'good reasons') bears on conclusions (another set of propositions), when the conclusions in question do not deductively follow from the evidence. In other words, of how one gets from fallible axioms which are accepted as true to conclusions which there are 'good reasons' to believe in although they are not logically entailed, or even from one set of propositions believed for 'good reasons' to another. And any such account, no matter how diaphanous, should involve discussion of probabilities, because it is clear that it will involve *degrees of justification*, and that these will often depend – most obviously in the field of natural science – on the *degree of confirmation* of a theory. (Remember Lipton's explicit mention of 'likelihood'.) As Popper writes, in his discussion of the infinite regress which inductive logic falls foul of: 'Kant tried to force his way out of this difficulty by taking the principle of induction (which he formulated as the 'principle of universal causation') to be '*a priori* valid'.'[78] This points to the link between induction and justification that emerges when the quest for certainty is sacrificed; and as argued in the previous section, this sacrifice is needed.

Even if the justificationist wants now to chicane, in anticipation of that which is to come, and elect to hold that any given belief is either *sufficiently justified* (or if preferred, just 'justified') or not justified at all (relative to the evidence for/against it), it is still incumbent on her to show how degrees of belief can, do, and should change, and state at which point it is, precisely, that a degree of belief (or rational degree of belief) becomes sufficiently high to constitute a *sufficient justification*. For whether there really *is* such an inductive logic is precisely the point at issue, and those who would answer in the positive should be expected to put up a model for critical examination, rather than rely on vague appeals to intuition and 'common practice'. And I shall argue that, all too often, the mere falling of a leaf (or an apple) is taken to have great inductive significance, when it is simply a fallible observation (which may be expressed by a particular statement).

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[76] Here, it is plausible that many of my implicit premises were very similar to, or even the same as, those held by my supervisor.

[77] Lipton [1991], p.6.

[78] Popper [1959], p.29.

When dealing with probabilities, the golden rule is to recognise that they may be interpreted in several different fashions. Consider that I hold a coin in my hand, and ask, "When I flip this coin, what is the probability that it will land heads-up?" Two answers, equally valid, are common: (i) "One half, because even if it is biased, I don't know which *way* it is biased."; (ii) "I have no idea whatsoever." One who offers the first answer is taking the use of probability to be related to one's ignorance: a view which is still, surprisingly, uncritically asserted in many not-so-ancient undergraduate textbooks. Witness Boas, who writes:

> The word "probably" is frequently used in everyday life. We say "The test will probably be hard," "It will probably snow today," "We will probably win this game," and so on. *Such statements always imply a state of partial ignorance about the outcome of some event; we do not say "probably" about something whose outcome we know.* The theory of probability tries to express more precisely just what our state of ignorance is. *We say that the probability of getting a head in one toss of a coin is ½, and similarly for a tail.*[79] [Emphasis mine]

One who offers the second answer, on the other hand, is operating under a different, non-epistemic, understanding of probability. The thought is, instead, that there really is a probability inherent in each flip, or that would emerge (be determinable) after an infinite number of repeated flips (i.e. repeated experiments in approximately the same conditions, or same relevant conditions), which might take any value, between zero and one, whatsoever. Hacking sums up the differences between the two approaches in a most memorable fashion:

> probability...is Janus-faced. On the one side it is statistical, concerning itself with stochastic laws of chance processes. On the other side it is epistemological, dedicated to assessing reasonable degrees of belief in propositions quite devoid of statistical background.[80]

The terminology which he employs to express this distinction is also that which I will adopt. Any interpretation is either: (a) *epistemic* (or epistemological), meaning that it takes probability to be a measure of degree of belief, degree of rational belief, or perhaps even degree of knowledge; or (b) *aleatory*, meaning that it takes probabilities to be mind-independent features of the world, e.g. that possessed by a uranium atom with respect to its decay over any arbitrary period of time. This leads to a classification scheme which is represented in the table below:

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[79] Boas [1983], p.685. This is a textbook which was, and perhaps still is, recommended to undergraduates reading physics at the University of Bristol.
[80] Hacking [1975], p.12.

Fig. I.2 – Classification Scheme for Interpretations of Probability

EPISTEMIC	ALEATORY
CLASSICAL (Bernoulli, Laplace)	FREQUENCY (Ellis, Venn, von Mises, Reichenbach)
LOGICAL (Johnson, Keynes, Jeffreys, Carnap, Jaynes)	PROPENSITY (Popper, Miller)
SUBJECTIVE (Ramsey, De Finetti)	
INTERSUBJECTIVE (Gillies)	

Now I should hope it is reasonably obvious that aleatory interpretations of probability are unsuited to the task of providing an account of justification. Primarily, just because were they to be so suited, then there would have to be a continuum between truth and falsehood, whereby a proposition *really could be probably true*, in an utterly non-epistemic sense. And somehow, evidence would be responsible for altering the truth-value (here, take truth to be 1, falsity to be 0, and the interim values to be probable truths) of any given proposition – a notion that should seem ridiculous indeed, from the point of view of one who holds out for correspondence truth. For on any other account, for example where it is assumed that frequencies exist, and one can justify one's beliefs merely by spotting them – something akin to Reichenbach's view – there is a fatal objection.[81] As Lehrer puts it, in the specific case of sense-data samples taken as basic beliefs:

> To know the frequency of the presence of external objects...in the sense-data sample...one would have to know precisely what the frequency probability statement was supposed to enable us to know, namely, that beliefs about the external objects...are true. The attempt to justify statements about external objects by appeal to frequency statements is, therefore, futile.[82]

Even if this is recognised, it is still possible to argue that the beliefs in frequencies are themselves basic, but then they would have to be self-justified! And I agree with Lehrer that such a 'proposed solution...has the twin disadvantages of being unenlightening and incorrect'[83], simply because it reduces to an exercise in asserting, and repeating with a foot-stamp, "I am justified".

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[81] For a fuller discussion along related lines, see the outline of Reichenbach's position, and the criticism thereof, in Gower [1997], ch.10. Cohen expresses a similar view, open to the same criticism as above, and writes: 'It was a reasonable inference if it was the kind of inference that in an overwhelming number of cases leads to the truth if the premises are true. The probability of an inference, then, is the relative frequency with which its kind or type leads to true conclusions from true premises.' Cohen [1931], p.130. The point is that this may be accepted, and it may then be asked how on earth we can tell what is a 'reasonable inference' on such a criterion, and what could license the move to the acceptance of *any particular instance* of that kind as being truth-preserving.

[82] Lehrer [1990], p.77.

[83] Ibid., p.79.

The justificationist is left, then, only with the option of using an epistemic interpretation of probability: classical, logical, subjective, or intersubjective. Therefore I shall proceed by examining each of these in turn – save the last, since the architect himself suggests that it is incompatible with confirmation (rather than corroboration)[84] – and comparing their relative weaknesses and strengths. Two will be ruled out purely on the basis of their internal features:

Classical

This early interpretation of probability, which has its roots in the work of Bernoulli, but was first popularised by Laplace in his *Philosophical Essay on Probabilities*, is a product of the Enlightenment era.[85] Underlying it is the mechanistic philosophy which was so prevalent in that period, and the belief in *universal determinism* – the thesis that the present state of the universe is caused by the prior state, etc.[86] (Here, the notion of a 'clockwork universe' will suffice.) And due precisely to this acceptance of *universal determinism* is the corollary that probabilities are used only because of human ignorance. Specifically, given a situation in which one *knows* that a finite number of (mutually exclusive) events could occur, it follows that one event simply *must* occur, and if one does not know enough about the system which will lead to the outcome – the initial conditions of the system, plus the laws that relate those conditions to subsequent conditions – to expect one outcome over another, then one has recourse to probabilities. Central to this interpretation is the idea that probabilities are only applicable when faced with a finite number of *equipossible* potential outcomes; and that equipossibility can only be legitimately posited if there is no reason, whatsoever, to prefer one potential outcome, or one set of potential outcomes, over another. Hence, the central role of the *Principle of Non Sufficient Reason* as initially formulated by Bernoulli, which was only later dubbed the *Principle of Indifference* by Keynes.[87] In Laplace's words:

> Probability is relative, in part to this ignorance, in part to our knowledge…The theory of chance consists in reducing all the events of the same kind to a certain number of cases equally possible, that is to say, to such as we may be equally undecided about in

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[84] Gillies holds that his interpretation is not really suited to the Bayesian approach, particularly because P(h,b) is highly variable between research programmes, and more vitally that: '[D]ifferent individuals may come to quite different conclusions even though they have the same background knowledge and expertise in the relevant area, and even though they are all quite rational. A single rational degree of belief on which all rational human beings should agree seems to be a myth.' Gillies [1991], p.523.

[85] For a brief history, see Gillies, [2000], p.3-13. For greater detail, see Hacking [1975].

[86] Laplace puts it so: 'We ought…to regard the present state of the universe as the effect of its anterior state and as the cause of the one which is to follow. Given for one instant an intelligence which could comprehend all the forces by which nature is animated and the respective situation of the beings who compose it – an intelligence sufficiently vast to submit these data to analysis – it would embrace in the same formula the movements of the greatest bodies of the universe and those of the lightest atom; for it, nothing would be uncertain and the future, as the past would be present to its eyes'. Laplace [1814], p.4.

[87] For the moment, I will assume that the reader is reasonably familiar with the idea of this *a priori* synthetic principle, although it will be treated in greater detail during the subsequent discussion of the logical interpretation of probability. It is related, of course, to the Principle of Sufficient Reason. On this see Cohen [1931], pp.150-156.

regard to their existence, and in determining the number of cases favorable to the event whose probability is sought.[88]

This given, and in general, the probability of an event, E, is defined as the number of possible outcomes *favourable* to the event, n_f, divided by the total number of outcomes possible, n_p, when those outcomes are equally possible, mutually exclusive, and collectively exhaustive:

$P(E) = n_f/n_p$

To clarify, consider the following example. An experiment is to be performed which will involve two flips of a coin, and one is asked to predict how likely it is that a 'heads' result will occur (viz. subsequent to at least one flip, the coin will land heads-up). Given that one knows that each flip may result either in heads or tails, there are four outcomes, overall, to consider:

FIG. I.3

Flip No.	Outcome w	Outcome x	Outcome y	Outcome z
1	Heads	Heads	Tails	Tails
2	Heads	Tails	Heads	Tails

Now let 'at least one flip has a 'heads' result' be the event whose probability one wishes to estimate, E. Take each possible outcome to be *equipossible*, for there is no reason to expect one over other; this is an application of the *Principle of Non Sufficient Reason*. Since w, x, and y are the mutually exclusive outcomes favourable to E, n_f is equal to three. Since w, x, y, and z are the mutually exclusive and collectively exhaustive possible outcomes, n_p is equal to four. Thus, P(E) = ¾. Simple enough.

However, there are several criticisms of this interpretation of probability – based on problems that it should tackle, but cannot – which are sufficient, when taken together, to falsify it. First, it explicitly forbids the application of probability to non-uniform sample spaces, just because probability is defined in terms of equipossible outcomes. So when faced with an experiment involving repeated throws of a biased coin, say where the outcome 'heads' is far more likely than the outcome 'tails', one can apparently say nothing, just because probability talk cannot apply. Yet curiously, Laplace himself mentions the case of a biased coin, and suggests that probabilities can be applied in such instances. And thus, I am in agreement with Gillies, who concludes that: 'It looks as if Laplace forgot his philosophical foundations when developing the mathematical theory.'[89]

Second, since Laplace's only motivation for advancing it is a rather extreme metaphysical claim, specifically that *universal determinism* holds in the actual world, one who endorses this interpretation would seem to be committed to a dogma which is irrelevant for the purposes of providing an account of justification, or indeed of probability *qua* mathematical notion. (Cushing has recently argued that the question

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[88] Laplace [1814], p.6.
[89] Gillies [2000], p.18.

of determinism seems to be underdetermined by the evidence at hand.[90]) Since one is plausibly ignorant about whether such determinism *does* hold, it just seems to beg the question. How could the claim 'Universal determinism holds in the actual world' ever become *sufficiently justified*, under such an account of probability? Surely not on the grounds that probability as defined by Laplace is used herein!

The justificationist must look elsewhere.

Logical

The logical understanding of probability, at least in its Keynesian formulation, involves the idea that probabilities are objective in a Platonic sense, yet are not aleatory. Specifically, they are relations that subsist between propositions, and groups of propositions, which one is capable of intuitively grasping, or knowing by acquaintance. On the one hand, 'The Theory of Probability... is concerned with the degree of belief which it is *rational* to entertain in given conditions'.[91] On the other, Keynes also writes that these relations are equivalent to degrees of partial entailment, and is quite explicit that his project is to provide an account of justification:

> Let our premisses consist of any set of propositions h, and our conclusion consist of any set of propositions a, then, if a knowledge of h justifies a rational belief in a of degree α, we say that there is a *probability-relation* of degree α between a and h.[92]

There does not seem to be any obvious tension between these two claims, which I have presented in an order that Keynes would, perhaps, have found inverted. His position is best understood as follows: (i) There are partial degrees of entailment between propositions, and sets of propositions, which subsist; (ii) Given that there are partial degrees of entailment which subsist between propositions, or sets of propositions, one is only rational when making a probability judgement if one arrives at the degree of entailment in question. So according to Keynes, 'degrees of partial entailment' map onto 'degrees of rational belief', but this need not be seen as an identity claim, or a conflation.[93]

Yet while Keynes' work is admirable due to its clarity, and his forthright delineation of potential objections to his view – to which I shall come in due course – is refreshing, it must be emphasised that it is founded, at its core, on what are supposed to be certainties; his philosophy is a product of the pre-war (and pre-Wittgenstein) Cambridge philosophy of Russell and Moore.[94] For first, his view of knowledge is that it *requires* certainty: 'The highest degree of rational belief, which is termed *certain* rational belief, corresponds to *knowledge*...knowledge of a proposition always corresponds to certainty of rational belief in it and at the same time to actual truth in

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[90] See Cushing [1994], sections 11.2.2-11.4.

[91] Keynes [1921], p.4

[92] Ibid., p.4

[93] For Popper, 'degrees of partial entailment' (or logical proximity between statements) *do not* map onto 'degrees of rational belief'; his understanding of the logical interpretation of probability is therefore quite distinct from that of Keynes, or indeed Carnap.

[94] See, for example, Russell [1912]

the proposition itself.'[95] And second, he holds that we can *know* (viz. be certain) that a given probability-relation obtains just because there are particular propositions, which he calls 'secondary propositions', that we can grasp by direct acquaintance. Let me now explain this idea:

A primary proposition, **p**, is one in which we have a rational degree of belief, **r**. And the rational degree of belief is dependent upon our evidence, **e**, which is comprised of propositions – there are no unconditional probabilities, for Keynes.[96] In standard notation, then, $P(\mathbf{p}, \mathbf{e})=\mathbf{r}$; that is to say 'The probability of **p**, given **e**, is **r**', or in terms more amenable to Keynes, 'The probability-relation between **p** and **e** is **r**.' And the secondary proposition is precisely this. It is knowledge *of this secondary proposition* that serves to warrant a rational belief of degree **r** in the primary proposition **p**, given **e**. So Keynes' idea is that we can be certain there is a given probability-relation between two sets of propositions, say hypotheses and evidence, quite irrespective of whether any of the propositions comprising the evidence happen to be true or not. Assume the evidence to be true – better still, *know* that it is true – and a rational degree of belief in the hypotheses follows.

All this is wrong, however, primarily for the reasons already stated in my prior discussion of fallibilism; indeed, even the laws of *deductive* logic are not self-evident, beyond revision, or immune from criticism.[97] And as if this were not enough, Keynes encounters another problem, which is that he needs to account for the fact that many individuals differ on their probability assessments. Thus, he writes:

> Some men – indeed it is obviously the case – may have a greater power of logical intuition than others. Further, the difference between some kinds of propositions over which human intuition seems to have power, and some over which it has none, may depend wholly upon the constitution of our minds and have no significance for a perfectly objective logic. We can no more assume that all true secondary propositions are or ought to be universally known than that all true primary propositions are known. The perceptions of some relations of probability may be outside the powers of some or all of us.[98]

Arguing for direct acquaintance with concrete objects, sense-impressions, or moods – big tables, yellow hues, anxiety, etc. – is one thing. However, as Ramsey points out, it really does not seem to be the case that *anyone* can grasp probability-relations in the manner suggested by Keynes. Of the latter's logical relations, the former writes:

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[95] Keynes [1921], pp.10-11.

[96] He writes: 'It is as useless... to say "*b* is probable" as it would be to say "*b* is equal," or "*b* is greater than," and as unwarranted to conclude that, because *a* makes *b* probable, therefore *a* and *c* together make *b* probable, as to argue that because *a* is less than *b*, therefore *a* and *c* together are less than *b*.' Ibid., pp.6-7.

[97] More carefully, I might contend that even if there are some respects in which logic is not revisable, this does not mean that it is therefore infallible. In saying this, I agree with Haack that properly speaking, it is *agents* that are fallible: so all I am really claiming is that we ought not to be certain that our logic is correct, given that it might not be, since we are fallible with respect to identifying 'the correct' logic(s), on the assumption that such an (anti-instrumentalist) notion makes sense. See Haack [1978], pp.232-238. I might add that even under an instrumentalist view of logic, we would still, I hold, be fallible with respect to identifying the most useful logic(s).

[98] Ibid., p.18.

> All we appear to know about them are certain general propositions, the laws of addition and multiplication; it is as if everyone knew the laws of geometry but no one could tell whether any given object were round or square; and I find it hard to imagine how so large a body of general knowledge can be combined with so slender a stock of particular facts... If... we take the simplest possible pairs of propositions, such as "This is red" and "That is blue" or "This is red" and "That is red", whose logical relations should surely be easiest to see, no one, I think, pretends to be sure what is the probability relation which connects them.[99]

Indeed, paradoxically, it seems that we only find ourselves capable of making probability judgements in rather complicated situations; those in which we cannot even state all our premises, viz. what we take to be our evidence. But could Keynes account for this? From a modern perspective, one novel idea would be to employ the notion of possible worlds, in the recognition that strictly speaking, 'all propositions are true or false'.[100] And we might then think that what is held as evidence serves to narrow down the set of possible worlds under discussion in a probability assessment; that if the evidence is too limited, then the set of possible worlds is not sufficiently narrowed to allow us to reach a proper assessment. Under such an understanding, given a set of evidence sufficient to specify a set of possible worlds, **e**, we could examine in what *percentage* of those possible worlds a particular hypothesis, **h**, holds. For example, it could be the case that in 73% of the worlds in which **e** is true, **h** is true. It would then follow that the probability of **h**, given **e**, is 0.73.

This is a rather clever idea. However, I believe it is flawed on three distinct grounds. First, and foremost, it is not clear that there are many cases, if any, in which our evidence specifies a *finite* set of possible worlds – in many cases, then, the probability relation between the evidence and *any hypothesis* would be incalculable. Second, even if the evidence did specify a finite set of possible worlds, it would not necessarily follow that it was *rational* to commit to belief in a given probability for **h**, since each of us is bound to one particular world, namely the actual one; we would require something like the principle of indifference, or a principle of randomness, operating over worlds, in order to merit belief in the statement 'It is just as likely for me to find myself in any one of the set of worlds under discussion, as any other'. (And as I will argue below, such a principle is indefensible as aught other than a heuristic.) Third, and finally, in order to specify a set of possible worlds it is necessary to specify the laws that obtain in those worlds, and there are some instances in which we might want to posit probabilistic laws, as suggested by Popper's propensity interpretation of probability; in such cases the *rational* degree of belief, say in an atom of lead-214 (with a half-life 26.8 minutes) decaying over a one hour period, would seem to be parasitic on an *aleatory* posit.

However, even putting this line of objection aside – perhaps on the basis that the distinction between primary and secondary propositions may be abandoned, as Carnap suggests[101] – there are further problems with the logical interpretation of probability, from a mathematical perspective. In particular, because 'In order that numerical measurement may be possible, we must be given a number of *equally*

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[99] Ramsey [1931], p.162.
[100] Keynes [1921], p.8. As I have already mentioned, in my rejection of the notion that aleatory probabilities can be employed to account for confirmation, I agree; no proposition can be 'probably true' in an objective sense.
[101] See Carnap [1962]

probable alternatives'.[102] (Keynes allows for non-numerical probabilities, but I will not discuss that notion here. This, since it is surely a requirement of any interpretation of probability that it be mathematically satisfactory, and any interpretation of probability which does not allow for numerical probabilities is clearly *not* mathematically satisfactory.) But how are we to determine when we are faced with a number of equipossible alternatives? Enter the principle of indifference, the alleged *a priori* synthetic principle that I briefly mentioned previously:

> The Principle of Indifference asserts that if there is no *known* reason for predicating of our subject one rather than another of several alternatives, then relatively to such knowledge the assertions of each of these alternatives have an *equal* probability. Thus *equal* probabilities must be assigned to each of several arguments, if there is an absence of positive ground for assigning *unequal* ones.[103]

Now as Keynes admits, this principle 'may lead to paradoxical and even contradictory conclusions', at least in its current form. There are several examples of such paradoxes presented by Gillies, but I will concentrate on one of the most troubling, and most fascinating – a geometrical paradox that was first formulated by Bertrand, in 1889. La question? 'On trace *au hasard* une corde dans un cercle. Quelle est la probabilité pour qu'elle soit plus petite que le côté du triangle equilateral inscrit?'[104]

So let us take an equilateral triangle with centre O, inscribed in a circle with radius R:

FIG. I.4 – SITUATIONAL DEPICTION OF BERTRAND'S PARADOX[105]
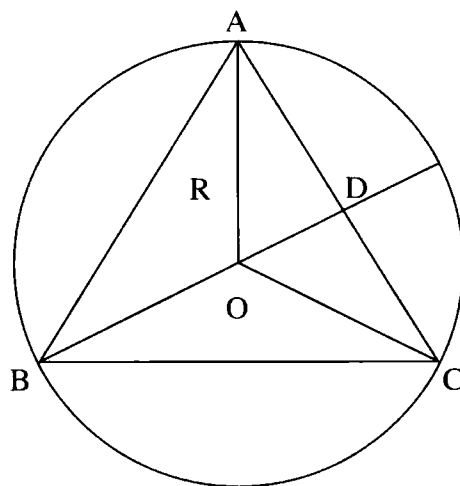
The chord passing through B and O – a diameter – bisects AC (viz. AD = DC). Thus, <ODC> is a right angle, and OD = Rsin30 = R/2. Now our problem: if we select a chord of the circle at random, what is the probability that it will have a length greater than the side of the triangle ABC, p(Ψ)? Well first, we might let XY be a random

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[102] Keynes [1921], p.41

[103] Ibid., p.42.

[104] Bertrand [1889], p.4

[105] There are no diagrams in Bertrand [1889]; those I employ here are based upon Gillies [2000], but altered with a view to improving clarity.

chord, and OZ be the line bisecting XY at point W, spanning from the centre of the circle to its edge:

FIG. I.5
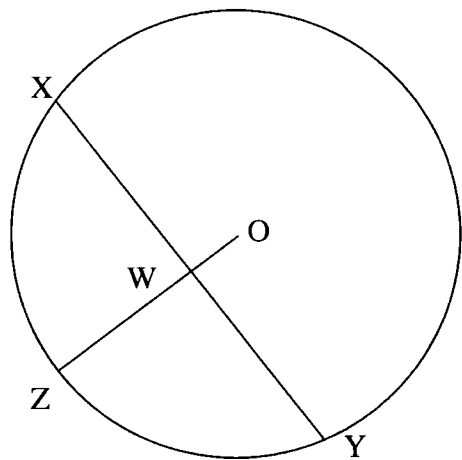
Now we have no known reason to presume that W is at any particular point on OZ rather than any other, thus all points along OZ are equipossible locations thereof, by the principle of indifference. In other words, OW has a uniform probability density in the interval [0,R]. And XY will be longer than the side of the triangle ABC if and only if OW is less than R/2. (Remember, OD, depicted in fig. I.4, had length R/2.) Thus,

$$P(\Psi) = p(OW < R/2) = \frac{1}{2} \qquad \text{(Result 1)}$$

Second, let AA' be a chord of the circle, with an angle θ to the tangent to the circle at point A (which is, remember, one of the vertices of the equilateral triangle ABC), as depicted below:

FIG. I.6
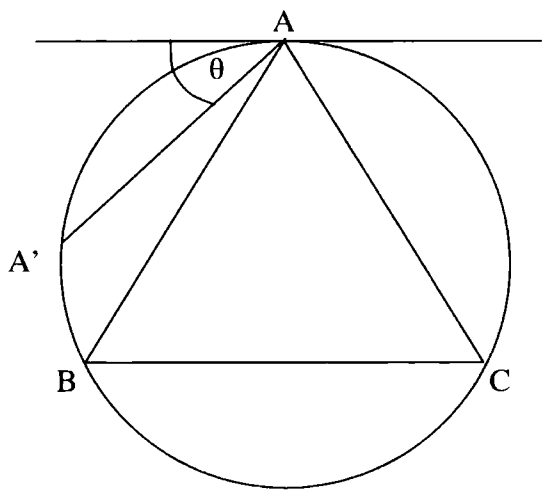
Now if AA' is to be longer than the side of the triangle (e.g. AB), θ must be between $\pi/3$ radians and $2\pi/3$ radians. And we have no known reason to suppose that θ has

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any particular value between 0 and π radians, rather than any other, hence by the principle of indifference, θ has a uniform probability density in the interval [0, π]. Thus,

$$P(\Psi) = p(\pi/3 < \theta < 2\pi/3) = \tfrac{1}{3} \qquad \text{(Result 2)}$$

Third, and finally, let us inscribe a circle in the triangle ABC – the 'secondary circle' – that will have a radius of R/2 (which is half that of the circle in which ABC is inscribed, the 'primary circle'). And let the chord be XY, drawn between any two distinct points on the circle's circumference. The situation is then as depicted below (with the triangle ABC omitted, for clarity):

FIG. I.7
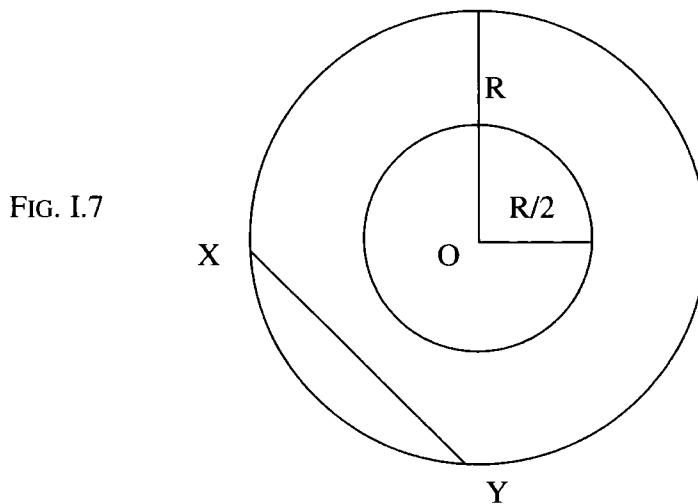
Now if it is to be the case that XY is longer than the side of ABC, it must be the case that its central point – call this W – lies inside the secondary circle. And we have no known reason to presume that W lies at any point inside the primary circle rather than any other, thus W has a uniform probability density in the primary circle, according to the principle of indifference. It follows, then, that:

$$P(\Psi) = \text{Area of secondary circle/Area of primary circle} = \pi R^2/4\pi R^2 = \tfrac{1}{4} \qquad \text{(Result 3)}$$

It seems, then, that the principle of indifference has led us astray, for we have three candidate answers to our problem, as initially defined. Moreover, I have noticed that Bertrand's third case may be extended, and rendered even more problematic. The idea is as follows: there are an infinite number of chords that have their centre as O – that is, the centre of the primary circle – but for any other point inside the primary circle, there is only one chord with that point as its central point. Upon my pointing this out to Gillies, he added:

> I think the consideration you introduce is rather ingenious and I haven't seen it in the literature. You could even use it to argue that P(CLSE) [my P(Ψ)] = 1/2 + (1/4)(1/2) = 5/8. The first 1/2 would refer to the chords through the centre which are LSE, [longer than the side of the equilateral triangle] while the second 1/2 would refer to

the remaining chords dealt with as previously to give 1/4. This would give yet another value for P(CLSE) making the paradox worse.[106] [Personal correspondence]

In the words of Bertrand: 'Entre ces trois réponses, quelle est la veritable? Aucune des trois n'est fausse, aucune n'est exacte, la question est mal posée.'[107] Yet Jaynes has argued that one of these candidate answers is, in fact, correct; it is result 1.[108] His idea is that the correct solution must obey invariance principles, with respect to rotation, scale, and translation. And what is more, he has tested result 1 empirically, after selecting it on the basis of said principles, and found it to be corroborated.

But let us accept that Jaynes is correct that invariance principles can solve certain apparent paradoxes; it remains the case that there might have been 'no known reason' for employing them in the example above, on the part of a hypothetical theorist, and the results obtained would, in fact, have been contradictory. The point: it is not acceptable to just apply the principle, see that it fails in a particular case, and then say 'there is something we should have known, or realised, or that we knew without knowing that we knew it'. For that would be a means of immunising the principle, *qua* logical (rather than heuristic), from criticism. (We should also be wary of immunising alleged deductive laws, such as that of the excluded middle, in a similar fashion.)

Still further, as Gillies points out, even if the principle of indifference is to be altered to involve appeal to invariance principles, all possible paradoxes cannot be evaded:

> It is easy to see how we can generalise...to produce a paradox in any case which concerns a continuous parameter (θ say) which takes values in an interval $[a, b]$. All we have to do is consider $\varphi = f(\theta)$, where f is a continuous and suitably regular function in the interval $[a, b]$ so that $a \leq \theta \leq b$ is logically equivalent to $f(a) \leq \varphi \leq f(b)$. If we have no reason to suppose that θ is at one point of the interval $[a, b]$ rather than another, we can then use the Principle of Indifference to give θ a uniform probability density in $[a, b]$. However, we have correspondingly no reason to suppose that φ is at one point of the interval $[f(a), f(b)]$ rather than another. So it seems we can equally well use the Principle of Indifference to give φ a uniform probability density in $[f(a), f(b)]$. However, the probabilities based on θ having a uniform probability density will in general be different from those based on φ having a uniform probability density; and thus the Principle of Indifference leads to contradictions.[109]

But what does Keynes say about all this? Remarkably, he was prescient of such problems, and introduces an indivisibility criterion, which is as follows:

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[106] It should be noted, however, that Bertrand may well have wanted to rule out this idea. For he writes: '[L]'infini n'est pas un nombre; on ne doit pas, sans explication, l'introduire dans les raisonnements. La précision illusoire des mots pourrait faire naître des contradictions. Choisir *au hasard*, entre un nombre infini de cas possibles, n'est pas une indication suffisante.' He continues by pointing out that choosing at random any number – 'entier ou fractionnaire, commensurable ou incommensurable' – between 0 and 100 which is greater than 50 is equivalent to choosing at random any number between 0 and 10000 which is greater than 2500, since 10000 is just the square of 100, and 2500 is just the square of 50. Yet in the first case, the probability would seem to be ½, whereas in the second it would seem to be ¾. He adds: 'D'où vient la différence des réponses? Le énoncés manquent de precision. Les contradictions de ce genre peuvent être multiplées à l'infini.' Bertrand [1889], p.4.

[107] Ibid., p.5

[108] Jaynes [1973].

[109] Gillies [2000], pp.41-42.

Let the alternatives, the equipossibility of which we seek to establish by means of the Principle of Indifference, be $\emptyset(a_1)$, $\emptyset(a_2)$, ..., $\emptyset(a_r)$, and let the evidence be h. Then it is a necessary condition for the application of the principle that these should be, relatively to the evidence, *indivisible* alternatives of the form $\emptyset$ (x).[110]

However, this is, I think, a very important concession when it is taken literally, for it would mean that the principle of indifference is inapplicable in continuous cases. (As it happens, Keynes himself seems to back away from this idea, quite mistakenly, by introducing the idea of using arbitrary finite intervals in continuous cases.[111] The problem is that these are sub-divisible, and that any sub-division is itself sub-divisible, *ad infinitum*.) But what of non-continuous cases, viz. those involving a finite number of discrete alternatives? I have argued elsewhere that the logical view may be construed as a 'topping-up' to the subjective interpretation (discussion of which follows) in such cases, and that the principle of indifference *is* an *a priori* synthetic principle, when the criterion of indivisibility is introduced (or more properly, included in the formulation of the principle itself); a position from which I would now wish to dissociate myself somewhat.[112] (One obvious problem: we can never *know* in Keynes's sense, i.e. be certain, that a given set of finite alternatives are, in fact, indivisible.) Yet it is interesting to note that that even when I did believe in 'partial degrees of entailment', I did not – indeed, *could not*, on pain of contradiction – find the Bayesian model of confirmation, discussed in greater detail shortly, to be acceptable. Let me now explain why, in a manner that I think is particularly revealing:

i)   Let us imagine that we know of an event, E, only that it may or may not occur on a given number of trials, n. Let n=2.

ii)   Let 1 denote that E occurs on any given trial, and 0 denote that it fails to occur on any given trial. Thus, there are four possible sequences that we might obtain, specifically 00, 01, 10, and 11.

iii)   We might choose to apply the Principle of Indifference in two different fashions. On the one hand, we might posit that each sequence is equipossible, and arrive at $P_1(01$ or $10)=1/2$. On the other hand, we might take each total number of occurrences of E (0, 1 or 2) to be equipossible, and arrive at a different probability distribution, specifically $P_2(01$ or $10)=1/3$.

iv)   If anything, Keynes' indivisibility criterion suggests that P_1 is valid whereas P_2 is not (E occurring once is divisible into the two sequences 01 and 10); if this is not taken to be the case, then i) - iii) is an exemplar of a paradox in a non-continuous case which his criterion does not resolve.

v)   Using P_1 in Bayes' theorem would mean that learning from experience was just impossible. For, if e is the result of the two trials, and h the hypothesis that E

[110] Keynes [1921], p.60.

[111] Ibid., p.62

[112] Rowbottom [2001]. On 'topping-up', see Gillies [1988]: the basic idea is that the Kolmogorov axioms serve as rationality constraints in the subjective interpretation, as I shall explain below in presenting the Ramsey-De Finetti theorem, but that further constraints may be suggested as 'top ups'. The POI is one possibility.

occurs on the third trial, then P(e)=1/4 (since there are four sequences) and
P(h)=1/2. Thus, P(e&h)=1/8.

If we now apply the following simple form of Bayes' theorem:

$$P(h|e) = P(e \& h)/P(e) \qquad \text{provided } P(e) \neq 0$$

It follows that P(h|e)=4/8=1/2=P(h); viz. that the posterior probability is equal to the prior probability.[113]

Now as I add, in a footnote, 'Carnap was the first to identify this problem; he considered two confirmation functions, $c^\dagger$ (state-description) and c^* (structure-description), which are equivalent to P_1 and P_2, respectively.'[114] But Carnap's preference for the second, c^* (or P_2) is simply not permissible – Gillies is kinder in merely calling it *ad hoc* – if the principle of indifference is to be defended by appeal to indivisibility. Indeed, there is a dilemma: sacrifice Bayesianism, or sacrifice the logical interpretation (in its Keynesian formulation, at least).[115] At this juncture, then, it would therefore seem advisable to move on to discuss the subjective interpretation of probability, and then Bayesianism.

Beforehand, I ought to re-emphasise that while I take myself to have shown that the logical interpretation is unacceptable in its Keynesian formulation, there are other approaches, most notably Popper's, which are quite distinctive. Popper's idea is that degree of probability – the probability relation between two propositions, or sets of propositions (although Popper uses sentence-types) – is *not* equivalent to rational degree of belief. Indeed, for Popper, C(a,b)≠P(a,b). I touch upon this later, but here it is not relevant, in so far as Popper argues precisely that 'confirmation' has nothing whatsoever to do with probability, although it may be defined *in terms of* (logical) probabilities. Popper abandoned the word 'confirmation' precisely because it became associated with the Bayesian programme of those such as Carnap:

> Until recently... I did not use the term 'degree of corroboration', but, in its place, the term 'degree of confirmation'. And I made use of this term... because of the need to avoid the term 'probability'... Until recently I used the label 'degree of confirmation' because this was Carnap's translation, in his 'Testability and Meaning', of my term *'Grad der Bewährung'*... However, the term was soon used with a new and different meaning; for Carnap assumed without further ado in the first sentence of his book *Logical Foundations of Probability* that the 'degree of confirmation' of a hypothesis satisfied the rules of the calculus of probability... I found this situation a little embarrassing, and when I published a paper (entitled 'Degree of Confirmation') in which I gave a definition of what I *now* propose to call *'degree of* corroboration', I referred to this development in a footnote... My paper received a reply from J.Kemeny... 'It should be pointed out that Popper used the term 'degree of confirmation' first – twenty years ago – and hence it is unfortunate that in recent years it has been widely used in a sense not intended by Popper...' As far as 'degree of confirmation' is concerned – in its more recent sense which makes it a probability

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[113] Rowbottom [2001], pp.10-11.

[114] See Carnap [1962], pp.562-565.

[115] But notice that Popper does sacrifice Bayesianism – indeed, puts it to the sword – even though his view of the logical interpretation is not Keynesian. The real dilemma, then, might be said to lie in choosing between probability as a measure of degree of rational belief and the principle of indifference as an *a priori* synthetic principle, given that the latter is only defensible by appeal to indivisibility.

– I cannot help feeling that the term is redundant. Why not stick to 'probability'?... Incidentally, I do prefer the label 'degree of corroboration'... For the term 'confirmation' may easily suggest a wrong idea. It contains the root 'firm', and it suggests either a process of making a hypothesis by degrees more certain, or even a process of making it finally secure. In other words, *the term 'confirmation' has strong verificationist associations.*[116]

Now I follow Popper's usage, since I agree that the connotations of 'firm' are unfortunate. (While it is true that both *'roborare'* and *'firmare'* can be understood as 'to strengthen', the latter is also employed, in Latin, in order to denote 'to prove', as indeed is *'probare'*.[117] More decisively, the adjective *'firmus'* is used figuratively in order to convey both 'true' and 'sure'; on the other hand, *'robustus'* conveys much the same as its modern equivalent in English, for instance 'mature', and 'rugged'.[118]) Thus I shall say that I am not necessarily opposed to the notion of *corroboration* – although this need not be limited to Popper's formal theory thereof – yet I argue against that of *confirmation* (and Bayesianism, *qua* theory of confirmation). I expect this to become clearer as I progress.

Subjective

According to the subjective approach, probabilities are degrees of belief, but not *rational* degrees of belief. In the words of De Finetti: '...only subjective probabilities exist – i.e. the *degree of belief* in the occurrence of an event attributed by a given person at a given instant with a given set of information.'[119] And to reiterate the comments of Ramsey on probabilistic relations, mentioned previously: 'All we appear to know about them are certain general propositions... the laws of addition and multiplication'.

The greatest success of the subjective interpretation of probability is taken to be that this core assumption – that probabilities reflect degrees of personal belief – allows one to derive the axioms of probability, via what I shall henceforth call the Ramsey-De Finetti theorem, with the invocation of only two additional assumptions. First, that gambling is a reasonable means by which to ascertain an individual's degree of belief. Second, that a bettor ought to obey the rationality constraint of *coherence*; that he must not allow a Dutch Book to be made against him. Let me now explain in further detail, while noting that this sort of positive result simply does not seem to be achievable by classical or logical approaches:

(a)   Gambling as a Measure of Degree of Belief

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[116] Popper [1983], pp.228-230.

[117] So here we see that Popper does have a genuine point: if by *'firmare'* one means *'probare'*, then why not just say *'probare'*? Why confuse matters, by the use of allegory? Is probability really *provability*, in the sense of the root of *'probabilis'*, or not?

[118] A hypothesis which has withstood many challenges, and the most difficult challenges we have yet conceived of, would fairly be described as 'mature' and 'rugged'.

[119] De Finetti [1970], pp. 3-4. This quotation also gives me the perfect opportunity to point out that I am a pluralist about probability (*inter alia*!); at the very least, I hold that aleatory and subjective interpretations are applicable in different contexts, and that all probability talk cannot neatly be reduced to either.

We start with the idea that a tester, T, wants to measure the degree of belief of a bettor, B, in either the truth of a proposition, R, or the occurrence of an event, E.[120] And for simplicity's sake, let us assume here that for each possible R on which a bet can legitimately be made, R_1, ..., R_n, there corresponds an event, E_i, such that if R_i is true, then E_i will occur.[121] (Henceforth, then, I shall only write of the probability of the occurrence of a given event, expressed as E.)

T asks B to choose a number q, his *betting quotient* on E, under the understanding that T will then choose a stake S, and that B will then pay T the sum of qS, and will win S in return, if E does, in fact, occur; otherwise, T keeps the sum of qS. S *may be positive or negative*, but must possess a magnitude such that it is neither large, nor trivially small, in relation to the total wealth of B.[122] (Note, of course, that if S is negative, then T will pay B the sum of $q|S|$, in return for $|S|$ if E does occur; else, B will keep $q|S|$.)

Now, one is asked to make an important assumption which I will refer to as the *ignorance assumption*, IA. It is as follows:

Ignorance Assumption (IA): B does not know, when choosing q, whether T will choose S to be positive or negative.

Plausibly, if B *knew* that T would choose S negative, he would choose q extremely high; conversely, if B *knew* that T would choose S positive, he would choose q extremely low. (This, given appropriate auxiliary assumptions, based on our understanding of 'real world' betting behaviour.) And then, q would not correspond to his actual degree of belief about the likelihood of E occurring; B would just be ensuring that he made as much money as possible from the bet!

Gillies offers the useful example of the jobber in the stock market when he discusses this.[123] And it seems right that if one approaches a car salesman saying that one wants to buy a given make and model of car, one will be 'quoted' a higher price than if one says that one wants to sell the very same make and model. Typically, the correct strategy for one to ascertain the true belief of an individual in the value of a commodity seems to be to ask "How much do you think the commodity is worth?", and withhold information about whether one wishes to buy or sell it.

It is also worth adding that the notion of precise numerical values for degrees of belief is admitted to be somewhat of a fiction. As Gillies points out, De Finetti tackles this point quite explicitly:

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[120] Strangely, adherents to the subjective interpretation seem to leap to talk of the probability of events (occurring), rather than that of propositions (being true), but I shall overlook this here, even though the move is somewhat suspect. (It is unclear how anything other than a proposition could be the *object* of an attitude such as belief, but I do the subjectivist the favour of assuming that this issue can be dealt with satisfactorily, perhaps by the process I mention above.)

[121] Again for purposes of simplicity, I assume the bets are only about the future.

[122] Note that there is some difference of opinion between Ramsey and De Finetti on the question of whether utilities, rather than monetary sums, are better employed in order to measure degree of belief via betting. I am willing to take it that such difficulties can be satisfactorily resolved, however, and broadly agree with Gillies that the monetary approach of (the early) De Finetti seems most unproblematic. See the discussion in Gillies [2000], pp.56-58.

[123] Gillies [2000], p.55

...if you want to apply mathematics, you must act as though the measured magnitudes have precise values. This fiction is very fruitful, as everybody knows; the fact that it is only a fiction does not diminish its value as long as we bear in mind that the precision of the result will be what it will be... To go, with the valid help of mathematics, from approximate premises to approximate conclusions, I must go by way of an exact algorithm, even though I consider it an artifice.[124]

(b)   *Coherence*[125]

The idea of coherence is introduced as the means by which the subjectivist can show that bare degrees of belief – understood, here, as those things measured as betting quotients – ought to obey the axioms of probability. It is as follows:

Coherence (C): If B bets on the occurrence of a set of events, $E_1, ..., E_n$, his betting quotients which correspond to those events, $q_1, ..., q_n$, are *coherent* only if T cannot choose a set of stakes, $S_1, ..., S_n$, such that B loses (money) no matter what occurs.

As Gillies says: 'It is taken as obvious that Mr B will want his bets to be coherent, that is to say he will want to avoid the possibility of his losing whatever happens. Surprisingly, this condition is both necessary and sufficient for betting quotients to satisfy the axioms of probability.'[126] Yet I do not agree that it is *sufficient*, although it is necessary, since further posits are required about the potential behaviour of the tester, and B's beliefs about that potential behaviour, in order to render the desired result. For one thing, IA is also being 'taken as obvious'. But yet further, the fact that the tester will attempt to win – to draw up a Dutch Book, given the chance – is also being 'taken as obvious'. (Or is it just that the tester *believes this* that is being 'taken as obvious'? What seems like a pellucid idea on the surface is less so, when it is critically examined.) Indeed, it may very well be the case that T is mathematically incompetent, and does not know how to draw up a Dutch Book. And I raise these considerations just to gesture at the underlying problems of specification, here. To point out that we would do well not to become carried away by what is indubitably a nice model, but is *only* a model.

(c)   *The Ramsey-De Finetti Theorem*

Given coherence, IA, and the equivalence between a bettor's degree(s) of belief and his chosen betting quotient(s) on the occurrence of an event (or events), all the axioms of probability are supposed to follow. (As I have just mentioned, I think there are further assumptions, but I gloss over those for the moment. The basic idea seems acceptable, and it does not do to nitpick aimlessly, although I have a salient point to add below.)

For example, the first axiom has two parts:

1a. $P(\Omega)=1$, where Ω is the certain event.

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[124] Ibid., p.57. (Quoting De Finetti.)

[125] Ramsey called this 'consistency', but De Finetti's 'coherence' is preferable, since it avoids confusion with the logical use of the former term. See Ramsey [1931], p.182.

[126] Gillies [2000], p.58.

1b. For any E, $1 \geq P(E) \geq 0$.

And the argument takes the form that for any E, $q(E) \equiv P(E)$, and given the condition of coherence, etc., $q(\Omega)=1$, and $1 \geq q(E) \geq 0$. Let us just consider 1a, and show how it is supposed to follow.

Well, taking into account IA, the bettor B realises that if he selects a $q(\Omega)$ such that $q(\Omega) \neq 1$, then T can choose an S such that T wins. If B chooses $q(\Omega)<1$, then T need only choose a negative value for S. Similarly, if B chooses $q(\Omega)>1$, then T need only choose a positive value for S.

But let us now express this 'proof' of 1a in a more detailed fashion:

There are two relevant possibilities which B considers, *given his assumption that the event on which he is betting will definitely occur*:

α)   T chooses S negative.

β)   T chooses S positive.

Now if he chooses $q(\Omega)=1$, then he will break even in either event, α or β; he renounces the chance for gain, as well as the chance for loss. But notice, if you would again, that if B takes $P(\alpha)<<P(\beta)$, even has sound evidence on which to base such a hypothesis, then the situation is very different. There might be some suspicion, then, that something like the principle of indifference is creeping in, here – that it is being assumed that the probability of alpha and beta is equal, *ceteris paribus*, or that it is reasonable for B to assume this. More nearly, it might be said that if the value selected for q is dependent on the values assigned to $P(\alpha)$ and $P(\beta)$, in addition to the value assigned to $P(\Omega)$, then $q(\Omega,\alpha, \beta)=P(\Omega)=1$ only if $P(\alpha)=P(\beta)$, or perhaps $P(\alpha)>P(\beta)$. But the genuine idea behind coherence seems to be, on the other hand, that $P(\alpha)$ & $P(\beta)$ are dependent on q; *that T is not going to respond in a set fashion, independently of what value B selects for q*. In other words, imagine the Martian who understands little of human affairs, but is assigned the role of T, and always selects the sign of S by rolling a fair die, and choosing S positive only if the result of the roll is 6: in this event, $q(\Omega) \neq P(\Omega)$. (If this is still unclear, let me try to put the point more simply: if I think an event is highly likely, even certain, but also that someone else thinks it to be highly unlikely, then it is reasonable for me to expect that they will bet against the occurrence of the event, and select a quotient accordingly, upon proposing a bet to said person. In genuine betting situations such considerations are vital, and this highlights the thinly-veiled artificiality of IA.[127] For example, if you want good odds for a bet on England emerging victorious in an international football match, then it is better to bet on them *outside* England. Why? The bookmakers are aware that

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[127] As I mention in II.3, I have general problems with such thought-experimental situations when they are used to draw sweeping epistemological lessons, or lessons about what is 'rational', because they tend to run roughshod over fine distinctions which are of considerable importance. But at least what the subjectivists are claiming here is rather weak and unobjectionable – unlike the Bayesian programme that is often tacked on.

more people will bet on England winning than on them losing, *in England*; this means they have an idea of something like P(β), construed as a *relative frequency.*[128])

Now this ought to be enough to give a flavour of the general approach of the theorem, and I shall not go through the rigmarole of deriving each axiom, since I take the foregoing discussion to indicate roughly how loose these 'derivations' are, which is not to say that they are unsatisfactory by intuitive standards, rather than those of the unusually pedantic philosopher. (I do, after all, have a cross to bear.) Overall, it has to be said that the idea is fruitful, and pretty much works. Indeed, it might be said, further, that the axioms arrived at are *better* than Kolmogorov's in some respects; for instance, conditional probabilities are not merely introduced by *definition*, but instead are introduced axiomatically, via a lengthy argument.[129] So while I think that Gillies gets a little carried away in writing the following – the italics are mine, and I take the opportunity to show where I would moderate somewhat, without wanting to impugn the spirit – I assent to the claim that the theorem is impressive:

> The Ramsey-De Finetti theorem... *clearly demonstrates* [strongly suggests?] the superiority of the subjective to the logical theory [that is, in its Keynesian and Carnapian variants?]. Whereas in the logical theory the axioms of probability could only be justified by a vague and unsatisfactory appeal to intuition, in the subjective theory *they can be proved rigorously* [argued for, or strongly defended?] *from the eminently plausible condition of coherence* [and several other auxiliary assumptions]... In addition, *the subjective theory solves* [avoids?] *the paradoxes of the principle of indifference* by, in effect making this principle unnecessary, or at most a heuristic device.[130]

On, then, to Bayesianism: the static rational-decision theory which epitomises the justificationist approach that I argue against herein For like any good Popperian, I am not in favour of any *all-encompassing* 'rational-decision' theory, or decision theory period, if that is to extend beyond a statement of Socratic platitudes about *decision-making*. Instead, I seek to motivate the view that the search for such is a waste of time, when we might instead get on with the process of inquiry. As Miller puts it:

> [I]f our goal is simply to sort out what is true, the detour through probability or confirmation, or support, in the company of the imaginary principle *P* [that is, such as one of a given 'rational decision theory', or of the nature of 'justification' as a component of 'knowledge'], is plainly gratuitous. For rather than spend our time trying to classify some absurdly general principle like *P* as true we would do better to investigate, and to classify as true (or as false) those much more manageable, yet much more interesting, factual statements that are at the centre of our concerns, namely genuine scientific hypotheses. *Such classification cannot always be done by appeal to empirical support... some classification must precede all certification.* [Emphasis mine][131]

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[128] In other words, they will estimate the ratio of the number bets on England winning to the total number of bets on the match.

[129] The derivations, and a brief comparison between the Ramsey-De Finetti axioms and those of Kolmogorov, are available in Gillies [2000], pp.59-69. The issue of countable additivity versus finite additivity, which I cannot spare the time to cover here, is particularly interesting with respect to the logical interpretation. For more on this, see Williamson [1999].

[130] Gillies [2000], p.64.

[131] Miller [1994], p.4. I elaborate this line in the next chapter; see II.3. I might also add that I would disagree with Miller's implication, here, that scientific hypotheses either are, or ought to be, at the

The Banality of Bayesianism

> Bayesianism voids scientific activity of its true purpose, and reduces the study of human knowledge to the study of how beliefs change, rather than to the study of how they are to be changed. – Miller[132]

> [I]t is trivially possible to reconstruct everyone as a conditionalizer. But not fruitful. – van Fraassen[133]

Prima facie, and for once the appearances do not mislead, Bayes' theorem is trivial. In its simple form, as mentioned briefly in my discussion of the logical interpretation, it is as follows:

$$P(h|e) = P(e\&h)/P(e) \qquad \text{provided } P(e){\neq}0$$

However, I think it is important to be crystal clear about the nature of epistemic probabilities, in particular with respect to the fact that they are always conditional: to reiterate, I agree with Keynes that 'It is as useless... to say "*b* is probable" as it would be to say "*b* is equal," or "*b* is greater than,"...' And no doubt Popper shared this insight, in so far as his corroboration function is defined in terms of h, e, *and* b, where b is taken to denote 'background knowledge'.[134] (I will repeat that I prefer 'Assumptions not taken to be under investigation in the test', and add that such assumptions are, of course, up for grabs in principle. What isn't?) The point is blindingly simple: it makes no sense to ask what the probability of any proposition is (or propositions are) in an epistemic sense, if given nothing, e.g. no other proposition(s), to measure it by. Hence, Bayes' theorem is to be properly written in the fashion suggested by Salmon, *inter alios*, in so far as it is relevant to epistemic probabilities:[135]

$$P(h|e\&b) = P(e|h\&b)P(h|b)/P(e|b) \qquad \text{provided } P(e|b){\neq}0$$

The fundamental notion underlying Bayesianism is, then, that we must – or do, or ought to, for Bayesianism is ecumenical – *conditionalise* the prior probabilities we assign to hypotheses according to the antecedent theorem. That is, that we must, ought to, or do, update our probability assignments when evidence is encountered, such that the posterior probability of a hypothesis on a given piece of evidence is equal to the conditional probability of the hypothesis on the evidence which was assigned beforehand. Update our *degrees of belief*, that is, if my prior arguments against the classical and logical interpretations – and indeed for the radical incompatibility of the Keynesian/Carnapian logical view and Bayesianism, given the requirement of indivisibility – go through. As Glymour puts it:

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centre of our concerns. Of course, this is not to say that scientific hypotheses ought not to be *among* our most important concerns.

[132] Ibid., p.132.

[133] Van Fraassen [1989], p.348

[134] In his words, P(*a,b*) measures: 'the degree to which a statement *a* contains information which is contained by *b*'. Popper [1983], p.293

[135] See Salmon [1990]

According to personalists [advocates of subjective Bayesianism]... an ideally rational agent always has his degrees of belief distributed so as to satisfy the axioms of probability, and when he comes to accept a new belief he also forms new *degrees* of belief by conditionalizing on the newly accepted belief. There are any number of refinements, of course, but that is the basic view.[136]

But what is more, it is important to notice that what is being suggested here is not that such an agent makes (or ought to make) objective (viz. intersubjective *or* aleatory) probability estimates that are then (or ought to be) *corrected* by application of Bayes' theorem, in order to move them closer to 'a proper objective answer', as such. For instance, De Finetti holds, and is right to say that he can hold, *according to the subjective Bayesianism of which he is arguably the father*:

Whatever be the influence of observation on predictions of the future, it never implies and never signifies that we *correct* the primitive evaluation of the probability $P(E_{n+1})$ after it has been *disproved* by experience and substitute for it another $P^*(E_{n+1})$ which *conforms* to that experience and is therefore probably *closer to the real probability*; on the contrary, it manifests itself solely in the sense that when experience teaches us the result A on the first *n* trials, our judgment will be expressed by the probability $P(E_{n+1})$ no longer, but by the probability $P(E_{n+1}|A)$, i.e. that which our initial opinion would already attribute to the event E_{n+1} considered as conditioned on the outcome A. Nothing of this initial opinion is repudiated or corrected; it is not the function P which has been modified (replaced by another P^*), but rather the argument E_{n+1} which has been replaced by $E_{n+1}|A$, and this is just to remain faithful to our original opinion (as manifested in the choice of the function P) and coherent in our judgment that our predictions vary when a change takes place in the known circumstances.[137]

Perhaps this gives an early indication of why I called this theory 'static' in the previous sub-section; there is little room, if any, for creativity, flair, or inspiration. And as I shall endeavour to show, the objections to subjective Bayesianism are so many and varied that it seems fair to conclude that it is devoid of philosophical significance in either its descriptive or normative variants. Indeed, there are so many lines of attack available that I cannot spare the space to cover them all.[138]

For starters, it might be pointed out that learning would be radically impossible under this scheme if it were the case that P(h|b), the prior probability for a given hypothesis, were assigned a value of zero. Indeed, since the subjectivist only relies on obedience to the axioms of probability as a 'rationality constraint' – those very same axioms that emerge via the Ramsey-De Finetti theorem, and would lead to doubts over its plausibility if embellished upon, thereby vitiating the best argument for the subjective interpretation – this is not explicitly forbidden. But as I have argued elsewhere:

[T]here was certainly a time when I would have assigned a prior probability of zero to the hypothesis 'All the air in my study will rush into its top left-hand corner, and remain there for thirty seconds', although I would now assign it a non-zero value. Admittedly, the Bayesian could claim that I *have not* learned this, but it would follow

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[136] Glymour [1980], p.588.

[137] De Finetti [1937], pp.146-147.

[138] For example, see Miller [1994], 6.5-8, and Glymour [1980]. Also note that the defence of Bayesianism presented in Howson and Urbach [1989] comes under a particularly strong challenge from the example of the 'chaotic clock' in Albert [1999], which is also discussed in Gillies [2000], pp.83-84. Chapter 8 of Miller [1994] mounts an attack along similar lines.

that 'studying' statistical mechanics in a physics course at university does not constitute a learning process! Intuitively, I should say that such a conclusion would be absurd.[139]

From this straightforward move, I thereby conclude that all learning, let alone adjustment of degree of belief, cannot be by Bayesian means; rather, that our background assumptions shift in a fashion that she is ill-equipped, nay powerless, to explain. What is worse is that they may very well shift in the course of an experiment (or series of experiments); for, to do her the favour of putting the point in terms that she would find amenable, $P(b|e)$ may also be under consideration. I take it that this is of no serious surprise, even to many Bayesians, though. The idea that such a petty mathematical formula could facilitate a comprehensive analysis of how we always learn, or always ought to update our beliefs, is not only silly. It is positively crass.

Still, it might be said, more modestly, that we *often* do, or *often* should, update our degrees of belief according to the Bayesian scheme. (When or when not is an issue on which Bayesians do not have a unified account. And I have never seen a convincing one, but am again happy to let this slide, for there is a much more incisive criticism, which follows.) However, the burning questions, if I may cut to the chase rather than take a detour through needless technicalities, are as follows: what link, if any, does the mere process of updating degrees of belief by Bayes' theorem have to do with acquiring the *truth*?; what does it even have to do with empirical adequacy?; what does it have to do, *period*, with any worthwhile aim (or aspiration) of science? And I hold that the answers are: none; nothing; nothing. That is, *unless an epistemic account of truth is to be adopted*.[140]

For whereas under the logical interpretation there is taken to be a *genuine objective link* between propositions or sets of propositions with respect to entailment – indeed, this is precisely why Keynes and Carnap adopt it – the subjective account is utterly silent on this matter. In other words, subjective Bayesianism may well give an account of how *probabilities* of propositions may be (or ought sometimes to be) adjusted, but not how the *probabilities of their truth* may be (or ought sometimes to be) adjusted. And in so far as there could be said to be an aim of science, for such a Bayesian, it could only be, to a first approximation, 'to acquire propositions with high probability relative to other propositions accepted'. It could not be 'to acquire propositions with high probability *of truth* relative to other propositions accepted as true'.[141] In other words, the aim of science would really seem to be just *high probability in a subjective sense*. And this is not a realist thesis; the most suitable

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[139] Rowbottom [2001]. I should add that I meant, *inter alia*, that I would have assigned a betting quotient of 0 to said hypothesis, when it was taken to range over my entire life. (Indeed I still would, for all practical purposes, which is also to criticise the idea that betting behaviour could be an accurate measure of probability assignment.)

[140] And remember, the core premiss I accept in this section is AT.

[141] I am not denying the Tarski equivalence, here. Remember, I am talking of 'truth' in an absolute, correspondence, sense. And I am pointing out that the actual *objective* probability of the truth of a proposition, given the actual truth of other propositions, does not necessarily correspond to the *subjectively assigned* probability of the truth of the same. No bridge between the two is proposed by the subjectivist, as I point out with the following quotation from De Finetti. Moreover, as I go on to suggest thereafter, it seems that what is measured by betting quotients is *willingness to act on* a given proposition, given others (as true). But *willingness to act as if true* need not neatly map on to *degree of belief in truth*.

adjective is, if I may borrow from Miller, 'spooky'. De Finetti is commendably honest about this, and writes:

> Our point of view remains in all cases the same: *to show that there are rather profound psychological reasons which make the exact or approximate agreement that is observed between the opinions of different individuals very natural, but that there are no reasons, rational, positive, or metaphysical, that can give this fact any meaning beyond that of a simple agreement of subjective opinions.*[142]

However, to build upon this, it might be added that some subjective theorists – as is made explicit above, De Finetti is an exemplar, in having it that all probability is subjective[143] – hold that no matter the value of one's starting priors (provided, of course, that these are not zero), adjustment by Bayesian conditionalisation with respect to specific chains of experimental (or experiential) results can, and generally does, cause the *degrees of belief* of different individuals to coincide, *in the long run*. Now although this is dubious, let it be granted.[144] Then it might be claimed that the aim of science is truth according to the Bayesian account, if truth is construed as *consensus in the ideal limit of inquiry*. Fair enough, say I. First, I am quite happy to grant this in context, since I only argue for AT $\not\models$ ~ET. But second, I am more than happy to grant it, since it is richly suggestive of the idea that there is a tension between subjective Bayesianism and the notion that the aim of science is *absolute* truth. There is no solace here for the scientific realist, who seeks an inductive logic that is applicable to a *non-epistemic* theory of truth.

Moreover – and I prefer this criticism because it does not seem to be strongly emphasised in the literature I have cited – it might be the case that the whole notion of 'degrees of belief' is incorrect. Instead, it might be said that one either adopts the attitude of belief towards a proposition or not, and that the betting procedure suggested by Ramsey and De Finetti approximately measures only *willingness to act on a belief, degree of confidence in a belief with respect to action*, or even *willingness to commit to belief in a proposition (or propositions) that have been considered*. (It would, after all, be counterintuitive to say "I believe 'God exists' to degree 0.001", rather than "I do not believe that 'God exists'." And given that I am an agnostic, what degree of belief would that suggest in "God exists", under the subjectivist model? 0.5 would seem to be the best fit, and Glymour agrees, but I do not think this can be right, since I really *do not have* either the belief that "God exists" or the belief that "God does not exist".[145] A weakness in the Ramsey-De Finetti theorem is that it presupposes a willingness to bet provided the risk of loss is not too great, whereas there may be principled reasons for refusing to bet which are not related merely to such potential risks.) The problem of context then rears it ugly head, for it may well be the case that what we are willing to act on in a theoretical respect is rather different to what we are willing to act on in a practical respect. Most obviously, I might employ a theory which is falsified, viz. has been classified as false, in order to aid in

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[142] De Finetti [1937], p.152.

[143] For a more complete treatment, see the discussion of exchangeability in Gillies [2000], pp.69-75.

[144] As already mentioned, see Albert [1999], Gillies [2000], pp.83-84, and Miller [1994], ch.8.

[145] Glymour writes: 'one…is agnostic just if his degree of belief is somewhere near a half'. Glymour [1980], p.588. Notice that "I do not believe that God exists" does not entail "I believe that God does not exist". Absence of belief that *p* need not result in belief that *not-p*, and this is interesting in the example presented because it just so happens that I have considered *p*, with respect to evaluating its truth, at some length.

the construction of a successor theory which, according to the statements accepted in order to falsify its predecessor, might be true. It does not follow that I would prefer the predecessor as a basis for action in a given practical context, particularly if the anticipated risks of failure were great. In the words of Watkins:

> [O]ur methods of hypothesis-selection in practical life should be well suited to our practical aims, just as our methods of hypothesis-selection in theoretical science should be well suited to our theoretical aims; and the two kinds of method may very well yield different answers in a particular case.[146]

But to return to the quotation that opens this sub-section, and attempt to bring together my lengthy objections to the justificationist approach and inductivism generally, the time has come to give a flavour of the alternative that is advocated by one who would link 'rationality' to criticism, rather than justification.[147] This may be summed up, broadly, as the advocacy of a dynamic approach to inquiry, based on critical examination, and with argument itself doing the work, rather than any hallowed 'epistemic principle' (or principles). As a basis for comparison, and to drive a wedge between the quest for truth and the quest for (high) probability, let me take the following issue:

> *Which ticket would you prefer to draw in a sweepstake: the one bearing the favourite's name, or the one bearing the winner's?* The answer I receive is almost always that the winning ticket is, of course, the best one to draw; and an agent's preference in the abstract would be for this ticket. But I am forcefully reminded after a semicolon's pause that, until the race is over, no one can know which ticket is the winning ticket, and as a matter of tactics the rational agent therefore prefers the ticket for which there is the best reason to expect victory. It is the 'therefore' here that takes my breath away. If the tactical preference for the most favoured ticket is not to be simply an underhand repudiation of the abstract preference for the winning ticket, then the agent must have conjectured that the ticket most likely to win actually will win; and he prefers the favourite not because it is the favourite but because he conjectures that it will be the winner, and evaluates the truth of that conjecture in the light of the evidence... what matters is the conjectured classification of the hypothesis as true... not its assessment as probable or reasonable.[148]

Now it just so happens that I have encountered similar resistance to that of which Miller writes, in my many discussions with justificationists (and indeed inductivists, as a subclass thereof).[149] So allow me to expand, by offering a specific example. Imagine I am entering into a lottery, and my aim is to win. The problem is clear, given my background assumptions (which I shall not, because I cannot, state in full):

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[146] Watkins [1968], p.65.

[147] This is a suggestion that is made by Bartley [1962] (see particularly chapters IV and V). I develop this line further in II.3 and II.4.

[148] Miller [1994], p.66.

[149] This passage is also singled out for criticism in Watkins [1995], pp.613-614. He writes: '[P]redictions that are true are reliable as well. A true prediction that we do not *know* to be true is still reliable... If, as he says, the demand for reliability cannot be met, then nor can the demand for truth. This pseudo-opposition between truth and reliability is systematic of a false antithesis that crops up all too frequently in this book.' However, Miller's entire point in section 3.4 is that truth is sufficient for reliability; indeed, that were we to have *the whole truth*, then we would never go astray. Yet on the other hand, a reliable belief (or proposition), and particularly what might seem to be a reliable belief, may very well be false. And this is Miller's real point, I think: from 'reliable-so-far', 'reliable' does not follow without truth.

which ticket will in fact, or actually, win? And what I should like is a true answer to this question.

To keep the situation simple, let us imagine, further, that I can buy only one ticket, and that there are just six tickets available, numbered from one to six. The winning number will be determined by the roll of a die, which will be performed by a mechanical contraption; the number on the top face of the die, when it comes to rest, will correspond to the number printed on, and thus determine, the winning ticket. All this I am aware of, and accept as true.

So what to do? Well initially, I would come up with an idea – a tentative hypothesis – of how the contraption, henceforth 'the rolling machine', operated. And I would prefer in the first instance, and therefore select if available, a hypothesis that could be tested somewhat (although not without the possibility of error) before the roll to determine the winning ticket was made. For instance, I might think like this:

There are only six ways the die can land, such that there is a result relevant to the lottery. Six results are *conceivable*, given the accepted situation and constraints (represented by propositions). But from this, it does not follow that all six results are physically (or nomically) possible, let alone *objectively* equipossible – that is, equipossible in an *aleatory* sense – given the way that the machine works. From the axioms of probability, which I am not going to question in this context, I say $p(1)+p(2)+p(3)+p(4)+p(5)+p(6)=1$, since these events are mutually exclusive and collectively exhaustive.[150] This is some sort of start, but I am still left with an infinite number of potential assignments to each of these probabilities; for example, it might be the case that $p(1)=1$, and hence that ticket one is sure to win (objectively speaking), as would become clear to me could I view the rolling machine operate an infinite number of times. So I am now interested in some experimentation with the rolling machine, or in some information about how it works, if either of these options seems to be available.[151] (If they are not, I will emphatically *not* just assume that 'any ticket is as likely to win as any other', or apply the principle of indifference as more than a heuristic. I will try to think of a different line of attack, and if I cannot then I may very well resort to picking a ticket on what *I* would consider to be a whim – the one bearing my 'lucky number', the number that I find most aesthetically pleasing, or what have you.)

To cut a long story short, then, I will proceed to do my best to determine which ticket will win, *via* determining how the die will land, or how the die is most likely, *objectively speaking*, to land. (Here I have in mind questions such as 'What is the *propensity* for the die to land on one?', 'Is the probability of each result independent?', and so forth.) If I am allowed to see the rolling machine run before buying a ticket, I shall try to see as many 'repeat experiments' as I can. I might even be mindful of the possibility that the machine has a hidden switch by which the operator can 'fix' the result of a given roll. If I cannot see such a switch, I may conjecture that there is not one. And so on. The process is dynamic, interactive, and

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[150] Imagine that should the machine fail, or another contingency prevent a roll being made, my ticket will be refunded; hence I can 'effectively exclude' such possibilities.

[151] Here, then, the demand for new evidence. Whence does this spring, for the Bayesian? 'Maximisation of expected utility' does not quite do the job; see Miller [1994], ch.7.

investigative, rather than a static one of updating probability assignments, in the fashion suggested by the approach of the Bayesian.

All this is, of course, perfectly consistent with what Miller has to say. Notice, in particular, how the use of probability entered my (supposed) deliberation: it was precisely such that there was a link between 'most likely to win' and 'will win'. *The only thing I was ever really interested in, contextually, was which ticket would win – that was my problem – and my invocation of 'probability' was only derivative.* At no point did I mention that I assigned a value of (epistemic) probability to the hypothesis that the rolling machine had a particular propensity to issue each of the results 1, 2, 3, 4, 5, and 6. That simply did not enter my deliberations at all.

To this, the Bayesian (or indeed the inductivist) might say that I have left out an account of some 'sub-personal process'; that somehow I hit upon an idea based on probability theory, rather than on astrology (or readings of tea-leaves). My rejoinder is that a 'sub-personal process' can hardly be an inference, and that the answer to how I arrive at various ideas is, at least in part, a matter for experimental psychology to decide. (As I have already mentioned, what it genuinely means to talk of 'degree of belief' is at stake, *inter alia*, and a specific take on this is not even part of a well-corroborated scientific theory, as far as I am aware.) Furthermore, indeed, it is hardly the case that because I came up with my ideas in a particular way that might be labelled 'inductive' – this is highly dubious, as I argue below, in I.2.4 – then there is, of necessity, something intrinsically 'rational' about the process. To claim otherwise would merely be to conflate the *quid facti* and the *quid juris*: the descriptive and the normative.[152] And what kind of message is "How we are predisposed to behave, or just so happen to behave, is how we ought to behave"?

Another line of attack on the anti-inductivist's position – and *honestly*, the genuine anti-inductivist prefers attacks, rather than requests for justification – is, however, available. It is put forward by Salmon, and built upon by those such as Newton-Smith and O'Hear:

> When, for example, scientists first 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. Their predictions had both theoretical and practical interest. Contemporary cosmologists, for another example, would like to *explain* certain features of our universe in terms of its origin in a "big bang"; many of them are trying to *predict* whether it will end in a "big crunch." In this case, the predictive question seems motivated by pure intellectual curiosity, quite unattached to concerns regarding practical decision making.[153]

The idea – here, with reference to my imaginary situation, involving the rolling machine – is as follows. Why ought I to have relied, or why was it rational for me to rely, on what I held true, in order to tackle the problem? For example, what licensed

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[152] It is interesting that this distinction should be central to my rejection of induction, while also being key to my rejection of naturalism. I suspect, indeed, that the focus on this distinction is characteristic of the rationalist's approach – rather than that of the empiricist, for instance – and am rather confident that it is a motivating force for Popper's critical rationalism.

[153] Salmon [1981], p.443.

my employment of probability theory, rather than the generation of an entirely new theory, such as "My friend Tony Booth always correctly predicts the winning ticket in a lottery"? That is, *with a view to action*?[154]

The short answer is: nothing. Nothing, that is, in so far as I had no guarantee, or partial guarantee, that tackling the problem via probability theory (and working to a particular interpretation of probability) would prove any more successful than asking Tony Booth. How there could be any such thing, I confess and repeatedly stress, is beyond my ken. (And if I may be impertinent, I hazard a guess that it is beyond anyone who does not simply *assert* that there is some sort of 'foundation of knowledge' – sense-impressions, physical objects, intuitions, or whatever other poison is preferred. But such bare assertion may only give us false hope.)

The inductivist may press further, though, and ask why I do not (or do not usually) swap hypotheses at random; why I do not ride the tempestuous waves of the sea of potential conjectures in a carefree fashion. The answer is that when I come up with an idea, I want to test it, and I will not relinquish it unless I come to accept the truth of a proposition that is inconsistent with it. (By which I mean incompatible with it, from the point of view of deductive logic.) I add that this is precisely what makes me 'rational', in so far as I am happy to use this word; because I *test* my hypotheses, and stick with them until they seem to fail. When do they 'seem to fail'? There is no ultimate algorithm, probabilistic or otherwise, that provides a decisive answer to this question. And I wonder if this is not the inductivist's real problem with understanding critical rationalism; that a refusal to say altogether too much – to splash around general 'epistemic principles' in a vain attempt to convert vague hopes into putative realities – is construed as some sort of surrender to scepticism. The critical approach is quite different, and Bartley quotes Shaw in this regard:

> The reasonable man adapts himself to the conditions which surround him. The unreasonable man persists in trying to adapt surrounding conditions to himself... All progress depends on the unreasonable man.[155]

What I am suggesting, then, is not that one ought to stick with a well-corroborated hypothesis, say, because its degree of corroboration is any indicator of its future success. (Popper never said so either, despite all the rather unnecessary fuss that has been generated on this score.[156]) Not at all. Rather, one should stick with a well-corroborated hypothesis – *or even one selected in the first instance due to its mere consistency with one's other beliefs, its apparent scope, its expected ease of testability, and the like* – because it may very well be true, and one *ought* to be interested in subjecting it to searching criticism. In putting it to the test. So what I am suggesting is that the answer to Salmon's insightful challenge amounts to this: it is not rational to classify a proposition or theory as true, and then declassify it *at a whim*, or

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[154] As Curd and Cover put it: 'If corroboration statements are solely about the past, how, without tacitly making an inductive inference, can they justify the choice of a theory on which to base predictions about the future?' Curd and Cover (eds.) [1998], p.510. As I shall shortly point out, however, the main problem here is their very request for 'a justification'; they have missed the point.

[155] Bartley [1984], p.xiii. He adds: 'I mean by "reasonable" what Shaw means by "unreasonable"; *and I agree with what Shaw says.*' Ibid., p.xvii.

[156] In his words: 'I regarded (and I still regard) the degree of corroboration of a theory merely as a critical report on the quality of past performance: *it could not be used to predict future performance.*' See Schilpp [1974], p.82.

vice versa. Fundamentally, acts of reclassification (but not necessarily initial classification) *ought* to be based on critical activity, be that reflective or discursive. And this is, after all, precisely the Socratic root of Popper's thought, at least according to those who are sympathetic to his work.[157] (How could this have been so badly misunderstood? I try to given an answer in the next chapter.)

But one final thing. And here, I quote myself, for I was once a defender of induction (or at least the idea that TT was indefensible without ET, and that ET was indefensible without appeal to induction, in so far as it is reliant upon the notion of confirmation):

> [O]ne might legitimately ask why the critical rationalist does not just accept induction, in the same way that he accepts thesis M [MT] (and thesis S) [ST], as a working hypothesis.[158] [Footnote from original] Surely he would not want to contend that induction has been subjected to enough criticism (and remember, the criticism that it is unjustifiable is not important) to render it [conclusively] 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!"[159]

At the time this seemed to me to be a sharp criticism. But it is really extremely poor. It is question begging. For to demand justification in the face of one that holds it is unnecessary is simply to miss their point. The critical rationalist referred to above has classified a particular statement, about the effects of holding one's hand in a candle flame (under a suitably specified set of potential circumstances, which are implicit in the context of the relevant utterance) as true.[160] And thus his reply might simply be "I say it is true, and further that the test you suggest is not of sufficient severity to make it worthwhile; indeed, there is a similarity of circumstance, here, to the last time I tried holding my hand in a candle flame, which leads me to conjecture – given the laws of nature I believe to be true, and appropriate auxiliary assumptions – that it will burn me. Besides which, you generate an artificial problem-situation. What criticism is it that you have of my position that putting my hand in the candle flame will burn me, if any? If it is good enough, *then* I will be more than happy to perform the experiment." The mere existence of a (sceptical) possibility does not necessarily make it worth investigating; needs must we pick what we investigate or test with care, and with a view to solving authentic problems.

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[157] For a useful overview of two different views of Popper, of which I prefer the second, see Boland [1994]. This aspect blossoms in Bartley [1962], and is emphasised most clearly by Popper himself in Popper [1983].

[158] 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?"

[159] Rowbottom [2002], p.38

[160] It might, of course, be the case that the critical rationalist in question has only classified a less general hypothesis as true, say that 'The next time I hold my hand in a candle flame, it will burn me'; but here I appeal to common sense.

All this said, however, there is one final option available for those who would champion inductivism; to claim that it is, in fact, the case that we learn by induction in something akin to a Humean format, and further that the very notion of 'rationality' is parasitic upon such learning processes. (The stratagem is similar to an 'analytic justification' of induction.) This dogma is still reasonably popular in contemporary psychology, but the purpose of the next section is to quash it, and thereby achieve emancipation from the highly addictive neurotoxin that is justificationism.

2.3   AGAINST INDUCTION: *QUID FACTI*

Experience is the name everyone gives to their mistakes. – Wilde

That all knowledge begins with the perception of the individual and then goes on by abstraction to the universal is a widespread dogma... We are impressed with a stranger's beauty, agreeableness, or reliability before we can specify his features or traits. It is therefore quite in harmony with fact to urge that the perception of universals is as primary as the perception of particulars. The process of reflection is necessary to make the universal clear and distinct, but as the discriminating element in observation it aids us to recognize the individual... A student will make little progress in geometry if his attention is solicited by the special features of his particular diagram rather than by the universal relations which the diagram imperfectly embodies... without some perception of the abstract or universal traits which the new shares with the old, we cannot recognize or discover new truths. – Cohen[161]

In his *Enquiry Concerning Human Understanding*, Hume clearly distinguishes between two epistemic questions that may legitimately be asked with regard to 'Matters of Fact'. (For Hume, disciplines such as geometry involve the acquisition of 'Relations of Ideas'; the truths, therein, can be determined by 'the mere operation of thought', and with 'certainty'.[162]) The first, *'What is the nature of all our reasonings concerning matter of fact?'*, he answers with 'they are founded on the relation of cause and effect', and further claims that our conclusions based on this putative relation are derived from experience, rather than aught else.[163] The second, *'What is the foundation of all conclusions from experience?'*, he takes to be the genuinely tricky problem, at least for the 'philosopher', rather than the 'agent'.[164] And this is the one with which we are all familiar: the infamous 'Problem of Induction'. That is, the problem of justifying induction, rather than describing it, on the assumption that it occurs.

In Kantian terminology, one might think of the first question as *quid facti*, and the second as *quid juris* (or *was Rechtens ist*).[165] But when the distinction between them is put in such a fashion, it becomes readily apparent that any attempt to deal with the *quid juris* question need not fundamentally depend on the answer which one accepts to the *quid facti* question. In short, the first question has a metaphysical (or factual) flavour, whereas the second has, rather, an epistemic flavour. And one need not accept Hume's claim that all our reasoning concerning matters of fact is genuinely founded on causal considerations.

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[161] Cohen [1931], pp.124-125.

[162] Hume [1748], 4.1.

[163] Ibid., 4.14.

[164] Ibid., 4.21.

[165] Kant [1787], A84.

Yet some seem to have denied that the *quid juris* issue genuinely arises, due to their confidence in particular *quid facti* accounts of how we learn, coupled with their other philosophical commitments, such as naturalism (to which I turn in the next section). That is to say, there are those who would deny that we are entitled to ask the *quid juris* question, for it is so clear how we do learn that it makes no sense to ask it. For example, Strawson might be accused of adopting just such a strategy (and following in Wittgenstein's footsteps), in arguing that:

> Every successful method or recipe for finding out about the unobserved must be one which has inductive support; for to say that a recipe is successful is to say that it has been repeatedly applied with success.[166]

However, as Popper points out, this is a trick with considerable vintage, and consists of:

> pointing to the psychological *fact* that we do, *in fact*, think in this way – or that we are, *in fact*, compelled to think in this way, or compelled to admit that certain… inferences are inescapable or 'necessary'.[167]

Such prestidigitation is not impressive. First, because it is unnecessarily pessimistic to assert that we are so surely bounded: fettered to adopt practices which are not revisable, or subject to critical scrutiny. Second, because it seems factually dubious that we are so fettered; to draw a deductive analogy, it does seem quite possible to question the validity of the law of the excluded middle, and it was possible for Enlightenment philosophers to forswear dependence upon syllogistic reasoning. (There is also a persuasive mathematical analogy in the parallel line axiom of Euclid, which once seemed undeniable.) Third, and against Strawson's argument in particular, because it is not acceptable to merely *declare* that validity is constituted by success. No one seriously thought that quantum mechanics had been unsuccessful in 1952, yet this did not stop Bohm from successfully criticising the Copenhagen Interpretation which was so entrenched at that time, and showing how it was not entailed by the evidence which it accounted for, even though it may have been compatible with the same. (Bohm's drive was his dissatisfaction with the theory as compared with his philosophical predilections. His goal was truth, not 'success'.[168]) More trivially, in a moral context, I could adopt many underhanded methods to achieve success in academia, such as bribing the internal examiner of this thesis, or sleeping with the editor of a journal, to persuade him (or her) to publish my work. But would those methods seriously be valid? In the words of Popper:

> …even if all of us who deny the existence of 'inductive procedures' are wrong, it would be the height of dogmatism to assert that these disputed 'facts' create standards of reasoning whose validity is not open to further discussion.[169]

I daresay that Kant would have agreed. And from a chronological perspective, perhaps he deserves the final word on this matter:

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[166] Strawson [1952], p.505.

[167] Popper [1983], p.38.

[168] See my further discussion on Bohm in II.1.3, Bohm [1987], Cushing [1994], and Rowbottom [2002].

[169] Popper [1983], pp.38-39.

> We are... not justified in repudiating these problems under the exercise of our incapacity, as if their solution really lay in the nature of things, and in rejecting further investigation, since reason has given birth to these ideas from its womb alone, and is therefore liable to give account of either their validity or their dialectical illusion... mere censure can... never bring to an end the controversy about what is lawful in human reason.[170]

Of course, what is being objected to, here, is not the approach taken by Keynes, Carnap, and other confirmation theorists, who would link the calculus of probability to the canons of (alleged) inductive reasoning. To those, the anti-inductivist will pay due respect, as did Popper. And although I would contend that all attempts to generate a satisfactory confirmation theory have failed, so far, I welcome the work of those who would seek to forge inductive logics; that is, those who treat the *quid juris* question seriously. Indeed, were I to be presented with a sufficiently robust account of inductive inference, I would genuinely believe that that our contemporary theories in natural philosophy – that is, physics, chemistry, and biology – could achieve degrees of confirmation, rather than mere corroboration.

But what to do with the stalwart individual who is utterly unconvinced by the foregoing arguments? What to do, when confronted by the woman who baldly, and repeatedly, asserts that we all *have to learn* by induction, because we all *do learn* by induction, and that is simply the end of the discussion? It might be tempting to show disdain, and shake one's head in utter disbelief, although this would not be liable to convince. Instead, begrudgingly, one might attempt to question her premises: that is, to push upon her assertion that we do, *in fact*, learn in the method suggested by Hume. This is not to deny, or dissociate oneself, from the points already made – rather, it is to mount an attack on different lines, and to attempt to poison the roots of her position. *Prima facie*, this might seem brave indeed, if not foolhardy, since the belief that we proceed by so-called 'inductive procedures' is so widespread, not only in philosophy, but also in psychology, biology, and folklore.[171] Nonetheless, this is what I shall attempt.

Elucidation of the Position under Attack

Before I undertake this, however, it is important that what is under attack be elucidated, lest I merely cast an ignited match onto a straw woman. First, it should be

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[170] Kant [1787], B791-B792.

[171] Take, for instance, the discussion in ch.7 of Garnham and Oakhill [1994], a textbook used in psychology. Popper is given fair mention, but not treated seriously, and it is baldly asserted that: 'Inductive reasoning is important both in everyday life and in academic, in particular scientific, investigation.' Yet just afterwards, it is admitted that: '...there is no generally agreed definition of inductive reasoning and, indeed, the term *induction* is used in several ways in the psychological literature on thinking and reasoning.' And this seems contradictory. The authors prefer Johnson-Laird's view that 'an induction is "any process of thought yielding a conclusion that increases the semantic information in its initial observations or premises".', but then the question of whether such processes are rational, or not, or are even processes of *reasoning*, is simply ignored. Still further, the question of what counts as a 'process of thought' is not tackled. All this is unsatisfactorily presumptive, and in any event it might be said that hypothesis generation does not yield 'a conclusion' in any sense, but only a *conjecture*. *Reasoning* seems to me to be an activity, and not one that we are constantly engaged in: if we were, then I daresay that philosophy would be much easier!

emphasised that I am willing to accept the claim that all learning is 'learning from experience'. Second, that I accept the claim that *some* of our hypotheses, at least, involve causal considerations.[172] Third, and finally, that I do not wish to argue that no human (or animal) *ever* proceeds, or has ever proceeded, by using what might appear to be, on the surface, 'inductive procedures'. And this, of course, raises the question which I have not yet tackled in any serious depth: what, precisely, is an 'inductive procedure'?

For Hume, it clearly involves learning from *repetition of observations*: his view is that this can provide some surety with respect to drawing universal statements. He writes:

> In reality, all arguments from experience are founded on the similarity, which we discover among natural objects, and by which we are induced to expect effects similar to those, which we have found to follow from such objects... It is only after a long course of uniform experiments in any kind, that we attain a firm reliance and security with regard to a particular event.[173]

So that which Hume has in mind, here, is not induction of an Aristotelian nature. For when Aristotle wrote in his *Posterior Analytics* that

> 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 a perception – for of particulars there is a perception...[174]

he was simply developing a chain of thought which he earlier expressed in his *Prior Analytics*, by stating:

> The method is the same in all cases, in philosophy and in *any* art or study... it is the business of experience to give the principles which belong to each subject.[175]

That is to say, to do astronomy, we must see shining lights in the sky, and see that they differ in brightness, and that they flicker, and generally move as if they are following a circular path apart from a few 'wanderers' (planets), etc. This is hardly objectionable. But now consider geometry. For Aristotle, our acquisition of knowledge of the essential properties of the triangle is based on the observation of (approximate) triangles which we have drawn; yet for Hume, as I have already mentioned, geometry only involves 'Relations between Ideas'. Thus, curiously, there is a sense in which Aristotle is more of an empiricist than Hume; he also avoids conflating the issues surrounding 'cause and effect' with the question of how we learn.[176]

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[172] It might be added, however, that causal reasoning may be distinct from inductive reasoning and deductive reasoning. This is the claim defended in Lowe [1975], and if he is correct then one might be an anti-inductivist without being a (pure) deductivist.

[173] Hume [1748], 4.20.

[174] Op Cit., 81^b1-10.

[175] Op Cit., 46^a1-35.

[176] The celebrated 'four causes' of Aristotle are simply not 'causes' in the contemporary sense. They are not 'secret powers' of entities, in a Humean sense. They are properly understood as the 'four becauses'; that is, as an outline of the forms of explanation which we employ. (In other words, '*aitia*' is best translated as 'explanation', or perhaps 'ground'.) I argue this in III.2, and in Aristotle's words: 'Knowledge is the object of our inquiry, and men do not think they know a thing till they have grasped the 'why' of it.' Aristotle, *Physics*, 194^b15-25.

So what does Hume have in mind? Unsurprisingly, taking his work in historical context, his position is more akin to a Baconian account of induction.[177] In Bacon's words:

> It cannot be that axioms established by argumentation should avail for the discovery of new works; for the subtlety of nature is greater many times over than the subtlety of argument. But axioms duly and orderly formed from particulars easily discover the way to new particulars, and thus render sciences active... Though all the wits of all the ages should meet together and combine and transmit their labours, yet will not great progress ever be made in science by means of anticipations; for radical errors in the first concoction of the mind are not to be cured by the excellence of subsequent functions and remedies... One method of discovery alone remains to us, which is simply this. We must lead men to the particulars themselves, and their series and order; while men on their side must force themselves for a while to lay their notions by and begin to familiarise themselves with facts...[178]

And while it would be foolish to draw too strong an analogy between Bacon's thought and that of Hume – for the former prescribes, whereas the latter describes (or rather, attempts to describe) – there are salient similarities: emphasis upon efficient and material causes, rather than formal and final; the idea that particulars are given to us, and that general classificatory notions such as 'Man, Dog, Dove... Hot, Cold, White... do not materially deceive us'.[179] Moreover, both make claims about how we *should*, or how we *do*, learn.

Yet while I am willing, unsurprisingly, to agree with both Bacon and Hume that observation is of vital importance in learning, their understanding of what it is 'to observe', or more precisely 'to perceive', might be criticised, and found wanting. If this were possible, the 'inductive procedures' *they* had in mind would simply not exist. Furthermore, even if perception were to be as they thought it, then the significance of repetition in learning might itself be questioned. And here, I shall concentrate only on this second line-of-attack, and execute my offensive primarily on evidential grounds. (I shall put off the former line in the interest of clarity, since it arises more naturally in my discussion of 'The Observability Criterion' of empiricism, in III.4. Thus, only later will it fully emerge how my account of the significance of repetition is compatible with my rationalistic view of observation, or perception.)

Beforehand, it is worth re-emphasising that although some people might get their best ideas by making some sort of 'repeated observations', on occasion, this doesn't mean that they learn in such a fashion. I might get my best ideas by drinking seven pints of ale, or snorting a line of cocaine; this doesn't mean that it is either methodologically advisable, or – God forbid – necessary, that schoolchildren be encouraged to indulge in the same activities.

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[177] Of course, I am not arguing against 'eliminative induction' if that is taken to be a purely deductive procedure. But I am not convinced that this was really Bacon's view, despite his recognition that the negative instance has greater force, and take it that the next quotation can do the talking on this score. In any event, I do not think that particulars simply 'give themselves to us' in the fashion that Bacon suggests; observation is theory, and category, laden. Moreover, I argue below that 'anticipations' are at the heart of the scientific enterprise.

[178] Bacon [1620], Book 1, Aphorisms 24-36.

[179] Ibid., Book 1, Aphorism 16.

The Significance of Repetition in Learning

In order to discuss the significance of repetition in the process of learning, it is necessary, first, to distinguish between different forms of learning; that is, between different types of activities which may be commonly thought to constitute learning processes. Popper suggests three: trial and error, habit formation, and imitation.[180] They might be characterised as follows:

(i)   Trial and Error

As may be thought from a common sense understanding of the phrase, 'trial and error' consists of repeated, and varied, attempts to solve a problem. That is to say, when we encounter a problem which, if not resolved, would serve to 'continue to irritate us', we generally attempt to solve it by several different lines of attack. If one line of attack fails, then we adopt another, and so on, until we discover an approach which appears to have been successful.

Here, there are many examples that one may draw upon. For instance, I was recently watching a group of rats, in my mother's garden, which were attempting to reach a container of nuts: it was hanging from a tree, by a thin wire, and was so located in the hope that only birds, among the wildlife that frequented the garden, could reach it. The problem situation, from an anthropomorphic perspective, was easily accounted for – the rats were hungry, and irritated by that hunger; they could smell where there was some food, and wanted to get to it, to satiate their hunger, and remove the associated irritation. Now near the tree was a hedge, and I watched as the rats attempted to jump from various places thereon, in order to reach the container of nuts. It seemed as if each rat took a turn from the point on the hedge which was closest to the container, and failed. At this point, my mother, who was also watching, laughingly voiced the opinion that the nuts were safe from the rats. But I was not convinced, for I could see a different approach. And sure enough, just a few minutes later, I noticed a rat on the tree, running along the branch to which the container's wire was attached. It nimbly slid down the wire, and began to enjoy its breakfast. Unremarkably, the other rats soon joined it, and all three dined in peace, although at rather odd orientations; impressed by their ingenuity, we left them to their feast.

But the story does not end there, for after they had eaten their fill, they began to realise that they could not return the way they had come. That is, after each had tried to ascend via the wire. Their next step was to attempt to gnaw through it, presumably in the expectation that the container would drop, and them with it. A new problem, and a new process of trial and error began. One jumped towards the hedge, and didn't make it. So did another, and although it jumped from the very top of the container, it still failed to make it. The third paused, then began to swing the container, and only jumped when it was closer to the hedge; it was successful. But the story has a happy ending, since none of the rats appeared to be seriously hurt, and they all scampered off together, to the field adjoining the garden. The rats had learned much, as had I.

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[180] Popper [1983], part 1, section 3.

At a human level, of course, things are slightly different. And Popper suggests that it is important to distinguish between *systematic* and *chance* observations, when considering learning of a 'trial and error' format. On the one hand, systematic observation involves intentional problem solving or conjecture testing; and naturally, such a process will always require hypotheses and expectations in the mind of the agent. Take any investigation in natural philosophy as an example, or even a seemingly trivial act such as measuring the dimensions of a room. (Here, the problem situation may arise because we are irritated by the décor of a room, and wish to replace the wallpaper.) One would not think it important to use a ruler, rather than a tape measure, or vice versa. Moreover, we all share the conjecture – hopefully – that placing such measuring devices against a wall does not significantly change the height of the wall itself. Of course, the inductivist may want to point out, here, that often one would check the measurements, perhaps in order to 'confirm them'. But if enumerative induction were a genuine practice, then would we not want to measure the room a few hundred times? We do not, I would say, just because we hold a hypothesis that such measurements are insensitive to slight variations in background conditions. (Among others.) The situation is different in some laboratory settings, say in performing an experiment to measure the gravitational constant, where the slightest of disturbances, such as a tremor caused by a passing lorry outside, can affect the outcome.

On the other hand, chance observation involves stumbling into a situation, or noticing an occurrence, which goes against one's expectations, and would lead one to reject a hypothesis. This, then, is the sort of observation suggested by the quotation from Wilde at the beginning of this section. My life, for one, is replete with examples of such learning. A trivial example involves my biting into an olive from a jar of supposedly 'pitted olives' which still contained its stone. More seriously, I remember expecting, when I was younger, that if the driver of a car signalled to turn left when she reached a junction, then she would turn her car left; and my misplaced confidence was soon to result in an accident at a crossroads. My experiences aside, however, there are also some rather spectacular examples of important discoveries that have been made in such a fashion. Popper mentions several, but I will concentrate on just two.

First is a rather infamous episode, namely Roentgen's discovery of X-rays in 1895. For those unfamiliar with the story, Roentgen was investigating the emissions generated by discharging electric currents in partially evacuated glass tubes, known as Hittorf-Crooks tubes. And one day, there 'just so happened' to be a fluorescent (barium platinocyanide coated) screen on a table adjacent to the tube, which he noticed was glowing. He posited that it couldn't be the cathode rays themselves which were causing the glow, due to the large distance between the screen and the tube. Over the following weeks, he tried covering the tube with various materials, none of which prevented the screen glowing when it was nearby, and he eventually realised that objects between the tube and the screen could create an image thereon. But what Popper points out, incisively, is that the fluorescent screen would not have been there if Roentgen had not been investigating invisible rays, that is cathode rays, to start with. Indeed, further, that Roentgen himself said "I was *searching* for invisible rays"![181] Now it is important to note that it had been noticed by others,

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[181] Popper [1983], p.41.

beforehand, that photographic plates stored near Hittorf-Crooks tubes had become fogged. So why they did not make the same discovery that Roentgen did? Curiously, Popper seems to think that this is a point against his account. However, I think the answer is just that such occurrences didn't counter their expectations in so obvious a fashion as a glowing screen would have. It is also possible that they may not have been so alert to conjectures involving invisible rays as Roentgen clearly was. Similarly, many of us 'explain away' unusual occurrences, such as prophetic dreams, by snap judgements. In that sense, "The exposed photographic plates must have been accidentally exposed to light" is analogous to "The correlation between events in the dream and reality was sheer coincidence".

The second example involves a much more amusing episode: a serendipitous blunder by a German chemist named Sapper. While heating a batch of organic chemicals, he accidentally broke a thermometer into the mixture. But then he noticed a change in the reaction, and discovered, after further testing, that phthalic anhydride had been produced; the mercury from the thermometer had acted as a catalyst that helped the oxidation of naphthalene. Why was this important? Just because phthalic anhydride could be easily converted into indigo, which was a highly desirable dye at the time. (In India, there were two million acres of land growing indigo plants.[182]) Again, the point is that Sapper was only alert to the observation because of a problem situation (in this case, of industrial chemistry). In the words of a great scientist, Louis Pasteur: 'In the field of observations, chance favours only the prepared mind.'

Now if this account of chance observations seems plausible, then it suggests that the inductivist story is thoroughly misguided. For the claim is that we don't just spot connections, and then account for them. Rather, in Popper's words:

> A 'chance observation' is like an unexpected stone in our path: we stumble over it just because we did not expect it – or more precisely because we *did* expect, though unconsciously, that the path would be smooth.[183]

(ii)   Habit Formation

So far, I have not discussed skill acquisition. Yet surely most would agree that one must practice regularly to become a genuinely good swimmer, high jumper, or pianist. Similarly, many will remember being required to recite their 'times tables' as a young child – perhaps to their chagrin! And it would certainly seem to be fair to contend that some form of learning, by 'mechanical repetition', or 'repetition proper', occurs in such a fashion.[184]

However, Popper would ask us to consider what the purpose of such 'learning' is. Consider his example of the pianist; and let us imagine that she is practising a particularly difficult piece, such as Rachmaninov's third piano concerto. Now initially, she may make fingering errors, and proceed to correct them as she replays the piece; but then, it does not seem unreasonable to contend that she will settle upon certain techniques which succeed, on the basis of such trial and error. At this stage, she arguably *knows* how to play the piece, but perhaps she can only do so slowly, or

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[182] http://www1.umn.edu/ships/modules/polymer1.htm
[183] Popper [1983], p.40.
[184] Ibid., p.42.

with a great deal of concentration and effort. And this, Popper plausibly claims, is precisely why she will continue to practice:

> ...the function of mechanical repetition – of 'practising', or 'learning by rote' – is not to discover something new, but to establish familiarity with something previously discovered. Its function is not to make us conscious of a new problem (as in the function of testing repeatedly some tentative solutions) but to eliminate as far as possible the element of consciousness from our performance.[185]

In the case of the pianist, to remove the need for concentration on the mere mechanics of a piece – the obsession with simply pressing the correct keys at the correct time – is arguably a prerequisite for achieving mastery in playing it. For only when such mechanics become unconscious can she concentrate on the content of the piece itself: 'the musical idea, the phrasing of the passage'.

Similarly, in the case of 'learning' one's 'times tables', one is not truly acquiring knowledge of how the mathematical operation of multiplication works; one is merely gaining familiarity with the results of its use in particular cases. And arguably, what separates someone who is good at arithmetic from one who is not is their ability to deal with more complex formulae. Personally, from my time studying physics, I can attest that there are certain 'tricks' which I use when multiplying; for example, if I am asked to multiply a large number by nine, then I often multiply it by ten – it is easy to add a zero onto the end, or shift the decimal point – and then subtract the initial number from the result. I acquired this technique through trial and error, not mere practice. I was never taught it at school, or university. And I practiced using the technique, by repeatedly solving mathematical problems, simply to improve my speed in applying it. (Of course, there are many other such 'tricks' that one may learn in applied mathematics, for example, in solving problems in mechanics which involve use of the constant acceleration equations.) To leave the final word on this to Popper:

> Repetition as such cannot attract our attention; rather, it tends to make our expectations unconscious. (We may not hear the clock ticking, but we may 'hear' it stop.)[186]

(iii)   Imitation

Finally, one might briefly discuss learning by imitation. For example, one watches a martial arts instructor perform a move, and then attempts to emulate that movement. But naturally, the instructor will correct one's failed attempts to emulate it, and thus trial and error is still at the heart of the enterprise. Indeed, in certain circumstances, one may even correct oneself, say in repeated attempts to learn how to walk, or how to pronounce a certain word. Naturally, when such imitation becomes successful, one may then make the newly acquired skill into a habit by practice, or repetition proper, as mentioned above. Thus, I type on this keyboard without needing to consciously think about which keys to press – instead, I focus upon my thoughts, and the best manner in which to express them.

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[185] Ibid., pp.42-43.
[186] Id.

Summary

Now the charge levelled at the inductivist is that she mixes up the aforementioned forms of learning, and fails to distinguish properly, in particular, between the first – that is, trial and error – and the second – that is, habit formation through repetition proper. It is agreed that we learn by experience, and that our knowledge either consists of, or is partly constituted by, expectations. But whereas the inductivist thinks that expectations are based on memories of observations that were somehow linked by associations discovered through repetition, Popper's charge is that new expectations are truly formed by the process of trial and error. Our initial expectations in a new field, or when confronted with a new problem situation, are tentative. If they mislead us, or fail us, then we revise them. But if they lead to success after success then we come to trust them; they become unconscious, as is my belief, at the moment, that I will not fall through the floor. (Of course, that doesn't mean that such trust is *justified* in the standard philosophical sense.) As Popper puts it, such expectations:

> ...may become (by repetition) automatic, unconscious, and petrified, and we gradually cease to be able to learn in that particular field.[187]

Most of us will have encountered individuals who are rightly described as being 'set in their ways'; often these are old people, some of whom will even admit this of themselves. But is such a condition enviable? Surely not; and that, I would argue, is the true beauty of philosophy *qua* activity. It fosters and sustains our critical instincts. In the words of Bertrand Russell:

> Philosophy is to be studied...because these questions enlarge our conception of what is possible, enrich our intellectual imagination and diminish the dogmatic assurance which closes the mind against speculation...[188]

And since I started this section with a quotation from a modern playwright, perhaps I should finish with another from an ancient one:

> Knowledge must come through action; you can have no test which is not fanciful, save by trial. – Sophocles, Trachiniae

This concludes my *assault* on inductivism, although I shall offer a further critique of the very notion of 'justification' in a qualitative (and non-formal) sense, in particular with respect to the fiction of 'knowledge', in the next chapter (II.3). My purpose there will be to dissect this (long) dead horse – mercifully put out of its suffering by Popper, in the mid thirties – rather than merely flog it. I should add that I do not believe in either resurrection or reincarnation, although the next section is designed to show why this would be undesirable in any event. Its character is apologetic.

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[187] Ibid., p.44.
[188] Russell [1912], p.161.

2.4   ANTI-INDUCTIVISM SETTLED: A MODEST PROPOSAL

Anti-inductivism might appear to be perversely extreme: to be a position that only a philosopher's philosopher, and an awkward one at that, could ever entertain.[189] So as my Parthian shot on the 'induction issue', I want to emphasise the core insight of the position, and explain how, and why, it is really much more moderate – not to mention modest, and cautious – than inductivism. In doing so, I shall draw primarily upon a discussion article by Settle, written in reply to a critique of Popper by Swann.

Now according to Settle, the Humean view of rationality is decidedly *narrow*. *Narrow*, that is, in so far as Hume assumes that in order for a belief to be tenable, it must be rationally justifiable. In other words, he holds to an epistemic law such that: one ought not to commit to belief in p – that is, in the truth of p – unless one is in possession of good reasons for belief in p. Cue the problem of induction.

But it is precisely this assumption that Popper rejects, and hence his solution to – or more properly, *dissolution of* – Hume's problem. In the words of Settle:

> Popperians commonly think that NR [the narrow view of rationality] creates an impasse. Irrationalism becomes a rationally excused alternative, if almost nothing worth believing can be rationally justified. Popper's alternative to the demand that to be rationally tenable our beliefs must be rationally justifiable, is then something of a relief. On his view, a person is rationally permitted to hold views that cannot be rationally justified, *provided that they are not rationally prohibited, and provided they are not held dogmatically (uncritically).*[190] [Emphasis mine]

To expand upon this, we ought to note that for Popper 'an inductive inference' – although such a phrase has the character of an oxymoron for him, as I shall explain – is simply not, and ought not to be, rationally compelling. And what is 'rationally prohibited', as Settle puts it, is the adoption of contradictions, or inconsistencies (either internal or external, viz. with respect to a particular view itself, or that view in relation to others held). In the general spirit of critical rationalism, it might therefore be said that there are ways of ruling views out, but not ruling them *in*; if 'in', then 'in' only provisionally, and conjecturally. But this goes for everything, and what is out today may indeed be 'in' tomorrow. Not even a particular system of deductive logic is presumed, although one system or another will clearly be required in order to do any ruling.[191] *Modus tollens*, construed quite literally as the 'the mood that denies' rather than as a form of argument, takes precedence over *modus ponens*. That is, although classical logic does not seem to be deficient in the relevant area, given the current state of critical debate.

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[189] I think that this is just because of the popularity of justificationism, in natural science and philosophy, which is in turn based upon the common use of 'probable' in everyday discourse. As an ex-physicist, I can say that there was a time when I was so sure of the pronouncements of contemporary physics that I *wanted* there to be such a thing as inductive support. Wanted it to suggest that my faith – what I *now* recognise as my faith, what I was subjectively sure about – should be everyone else's. I was a dogmatist, hiding behind authority claims.

[190] Settle [1990], pp. 401-403

[191] As to the means by which a logical system can be criticised: this is most obviously by transcendent means, namely by comparison with another such system. How the second system would serve to alter the classification of statements provided by the first may be examined.

So when faced with an argument for p which is not truth-preserving, Popper holds that one is not compelled to accept the truth of p – or more importantly, and significantly, *ought not* to be compelled to accept the truth of p – even if one believes that the premises of the argument are all true (or, in fact, they are all true). In the critical rationalist's view, what is held up as 'an ampliative inference to p' is akin to 'an attempted defence of p', 'a statement of the perceived advantages of believing in p', or more properly 'a statement of what p is consistent with', since there are not any genuine rules of ampliative inference. But put this way, Popper's point seems thoroughly unobjectionable. By contrast, inductivism seems to be very bold, at least in particular incarnations. For example, can it be right that 'the probability of the sun rising tomorrow is 0.999999', established by enumerative induction over a set of propositions that I accept as true (or even *are true*), constituting reports only of the sun having risen on previous days, thereby necessitates my committing – or *ought to compel me to commit, if I am to remain rational* – to belief in 'The sun will rise tomorrow', provided I accept no other premises that would contradict such a proposition?

So Popper's central point might be understood as follows: no matter how convincing an argument that is not truth-preserving may seem, it simply ought not to be compelling. Thus, we ought to be rather wary of such arguments, and see them in such a light: approach them, as it were, with our eyes open to the (obvious) risk of error. And as such, Popper's point that inductive arguments are not truth-preserving has real sting; because *not* truth-preserving, *not* rationally compelling, *even if we are predisposed to find them appealing*. As Settle puts it:

> [Popperians] think the point is that inductive inferences are not compelling, so that no one should be thought irrational who refrains from accepting conclusions from them. Other people think it irrational not to believe what induction supports, even though deduction is non-demonstrative [although I would say, for emphasis, *not truth-preserving*]. I find it hard to sympathize with this latter view, hard to locate what it is about inductive arguments that warrants such a demand upon my allegiance. And I agree with Popper that the view seems mischievous. Why should I feel *rationally* compelled, as opposed to psychologically or physically constrained, to believe what may turn out to be false? And why rationally *compelled*, as opposed to invited or attracted? If the conclusions of non-demonstrative arguments with true premises could be false, would not that be a reason for refraining from belief, for exercising caution, especially if the price of wrong belief were high?... [T]he dispute... is not...empty... The issues connected with scientific realism, to instance a matter of philosophical interest, turn upon it.[192]

Of course it needs to be frankly admitted that what we think are truth-preserving inferences may sometimes not be. We are fallible. But even the inductivist will admit, surely, that so-called 'inductive inferences' are *not* truth-preserving! Does she seriously want us, nevertheless, to find them compelling? This is the essence of the Popperian line-of-attack. And if the inductivist claim is just that one should assign a particular value of probability to a particular proposition, given a set of premises which allegedly 'partially entail' it, then this may be agreed. The link between probability and rational degree of belief – the Keynesian (or Carnapian) one – is utterly denied.

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[192] Ibid., pp.404-405

It must be added – I said I would get to this – that Popper does not like the phrase 'inductive inference' precisely because his view is that an inference must be rationally compelling, and thus truth-preserving.[193] Else, an inference could merely be a tool of persuasion; a rhetorical device. (Popper is deadly serious about the quest for truth, and about eliminating error in this quest, whence the focus on criticism rather than justification springs.)

But notice that what inductivists call 'inductive inferences' are simply *not* forbidden in the sense that one ought never to employ them. Far from it, for Popper himself offers arguments which he does not take to be rationally compelling, as he candidly admits.[194] Still further, he is not beyond making appeals to 'best explanations' in advancing his positions, although he nowhere claims that there is anything like abduction, or 'inference to the best explanation'. And I take this to be because he believes that there is no such thing.[195]

Settle's insightful recognition is, then, that 'inductive arguments' can be arguments for the *rational permissibility* of particular claims (given particular premises, 'background knowledge', and so forth). And none of this is to suggest that there are *degrees* of rational permissibility, which can be determined by use of the probability calculus. Rather, he introduces an extremely interesting posit which he does not, alas, go on to develop: the idea that 'inductive arguments' are only persuasive in so far as they are enthymematic. This is to say, in so far as a listener (or reader) may import the premises necessary to make them truth-preserving (or deductively valid), and then evaluate the truth-values of those imported premises, *inter alia*, in assessing what use the purported argument is.

This is an ingenious suggestion. But is it plausible? Well to return to the previous example of enumerative induction, it might be said that it seems wrong – I take it that many an inductivist would agree, in this particular instance – because we take it that not only some laws of physics would need to be introduced in order to have the conclusion follow, but there would also need to be a specification of the condition of the universe at a given stage, perhaps a comment on its being indeterministic (or deterministic), and so forth. And what we might very well find is that the premises of the initial argument would prove barely relevant to the conclusion! Yet on the other hand, 'Socrates is a man, 99% of men are mortal, therefore the probability that Socrates is mortal is 0.99' seems much better because all that is required to be added, in order to render it valid, is 'Socrates is picked at random from the set of men' (meaning something like 'Socrates is as likely to be any one man as any other, in the class of men'). So *prima facie*, at least, here is an explanans for the persuasive force of particular inductive-looking arguments. And its significance should not be underestimated, since this force is precisely what many inductivists seek to persuade of.

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[193] It is best put this way: If not truth-preserving, then not rationally compelling. If not rationally compelling, then not an inference. Therefore, if not truth-preserving, then not an inference.

[194] See, for instance, Popper [1983], pp.80-88

[195] Miller agrees. (Personal correspondence)

2.5 AGAINST NATURALISM

"After all," Searle rhetorically asks, "do we not know *from the discoveries of science* that
there is really nothing in the universe but physical particles and fields of forces acting on
physical particles?" The answer, contrary to his assumption, is "No, we do not."... could he
possibly just *point out* when, where, how and by whom this "discovery of science" was
made? Was it made? – Willard[196]

Naturalism is so plausibly false, if we are to take it that we have any evidence that we
can evaluate – here I point to the preconditions of our even *having* evidence, our
having the ability to *recognise it as such*, and indeed our having the ability for making
judgements on the basis of it – that I find it difficult to take seriously. (Yes, I am
suggesting that if we have evidence, and do inquire with a modicum of success, then
naturalism is false. As Garver and Hare put it: '[S]timuli do not come to us marked as
"evidence."... the only ones that count as evidence are those that *ought* to determine
our beliefs and actions in a certain way.'[197]) And if the argument here suffers because
of this, then I must apologise in advance, yet steadfastly refuse to embellish it.

But what do I take naturalism to involve, beyond the approximate specification in
NT? In effect, this is a catchall term for the positions of a fugitive band of
philosophers – I say 'fugitive' because it is unclear that they allow for there to be such
an enterprise as philosophy – who share, in varying degrees, commitment to: monism,
scientism, physicalism, materialism, and reductionism. But this does not get us very
far, of course, since we are now faced with further -isms, and perhaps the best way
ahead is to perform a brief review of what anti-naturalists see themselves opposing,
and ferret out how these strands are interrelated. Let us start with Moser and Yandell,
who highlight the monism of the naturalist as follows:

> Many philosophers have held, in the tradition of Plato and Aristotle, that there are
> uniquely philosophical, non-empirical methods of inquiry and that there are things
> whose investigation is reliably conducted via such methods. Rejection of any such
> methods or any such objects other than those available to sense experience or
> scientific methods traces back to the monism of the presocratic philosophers...
> Philosophical monism in general, including idealism as well as materialism, demands
> a *single* standard in metaphysics or epistemology, contrary to pluralism...
> Philosophical dispute concerning naturalism ranges over many issues, but at bottom it
> concerns the nature of philosophy. A basic question is whether there is a legitimate
> form of philosophical procedure, often called "first philosophy," that has ontological
> authority but employs methods "prior to" or at least not based on sense experience or
> the empirical sciences. In particular, can a philosopher operating without reliance on
> sensation or the empirical sciences legitimately engage in inquiry that posits real
> objects or at least yields genuine truths? Naturalists say no; antinaturalists, yes.[198]

Now what this means, happily, is that the naturalist must be opposed to the very
project of this thesis, in so far as she could not agree that there were a non-empirical
discipline named 'metaphysics' which involved delineation of any possibilities – let
alone possibilities of being – in order to enable sensory faculties, or more generally
experience, to select the actual. Rather, her view must be that not only the actual, but

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[196] Willard [2000], p.29.
[197] Garver and Hare [1986], p.12.
[198] Moser and Yandell [2000], pp.3-4.

also evidence, is somehow (and at the very least to a significant extent) 'handed to us'; a view which I argue against throughout this thesis, for example in sections II.2, II.3, and III.4. (Of course, both Popper and Feyerabend shared a similar dislike of such a view.[199] But looking even earlier, one sees a similar vein in the work of the lesser-known Morris Raphael Cohen.[200]) So allow me to put this aside, and offer an initial, and unavoidably *passionate* rebuttal – I believe in passion, as the naturalist cannot, you see – of this *anti-philosophical* stance:

We live in a world where norms pervade, and which is replete with content. We are capable of love, independence, vanity, and anger. We are capable of thought, imagination, and reasoned reflection. We have responsibilities to self and other. And paedophilia is wrong. But what can the austere naturalist tell us about this? What, moreover, can natural science tell us about this? Or about the interpersonal relationships of those very individuals that *perform* natural science? About their hopes, fears, and dreams? About what it is that they strive for? About the very spirit of wonder that drives natural science (and natural scientists) ahead, or about the necessary *social* conditions for natural science to even take place? "The complete natural science(s) will tell us" is not an acceptable answer, because it presupposes that there *is* an 'us' capable of 'completing natural science', rather than a simple collection of electrons, protons, neutrons, strings, and/or what have you. "The physical interactions will continue" would be a darn sight more honest – but notice, nothing in natural science can even tell us *that much* conclusively – and my fundamental challenge to the naturalist is along these lines: do you like to relax in the bath, to get drunk with your friends, or to make love to your partner? Whence the truth-makers for *these* facts? Or are they not, after all, facts? (Is there even a 'natural science' to speak of? What are its physical identity conditions? Worse, are identity conditions *themselves* physical?) And if any given naturalist is just a collection of physical entities, then why on earth *ought* I not to slay naturalists on sight? Why would this be any different, ethically speaking, than my pulling up a weed in the garden? In short, why *ought* one to seek the truth, or anything else, at all? In the words of Putnam, who agrees with me that reason cannot be naturalised:

> Why should we expend our mental energy in convincing ourselves that we aren't thinkers, that our thoughts aren't really *about* anything, noumenal *or* phenomenal, that there is *no* sense in which any thought is *right* or *wrong* (including the thought that no thought is right or wrong) beyond being the verdict of the moment, and so on? This is a self-refuting enterprise is there ever was one![201]

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[199] See, for instance, Feyerabend [1981a], pp.17-36.

[200] Cohen writes: 'It is easy for those who have not reflected on actual scientific procedure to say: Begin with the facts. But an even more fundamental difficulty faces us. What *are* the facts? To determine them is the very object of the scientists' investigations, and if that were but the beginning or first stage of science, the other stages might be dispensed with... Our expectations and prepossessions make us see things which do not in fact happen, and without the proper previous reflection we fail to notice many obvious things which do happen... Popular empiricism speaks as if we can readily eliminate all error and attain absolute and indubitable truth by purifying the facts given in sense perception from all taint of inference or interpretation. Error, it is said, comes in judgement, not in perception. But if we did eliminate all inference and interpretation would sense perception give us any facts? Certainly not enough to constitute any science, social or physical. For assertions of fact involve all sorts of assumptions... [I]f the term *scientific method* is used in any significant sense it cannot be said to begin with a *tabula rasa* and pure sense-impressions on it, such as the new born babe is supposed to have.' Cohen [1931], p.78

[201] Putnam [1983], p.246.

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Indeed, Geach offers an even more memorable line, which is supported in Flew [1986]:

> When we hear of some new attempt to explain reasoning or language or choice naturalistically, we ought to react as if we were told someone had squared the circle or proved $\sqrt{2}$ to be rational: only the mildest curiosity is in order – how well has the fallacy been concealed?

Now it may, of course, be said that a purely descriptive psychology can answer such questions *in principle*, and that norms can, as a matter of fact, be dispensed with. (I do not grant this, but will play along for a moment.) But then, I think, an important question is raised; for what, pray tell, is supposed to be so naturalistic about *description*? As Willard puts it:

> A description will be naturalistic only if what it is *about* – its *content*, what it mentions – fits into a naturalistic ontology. The issue for naturalism certainly is *not* just about normativity. What, for example, would make one think that an adequate description of the formation of belief by means of a reliable process would mention only things that would fit into a naturalistic ontology? Only, I suggest, the prior assumption that the "science" of psychology will, when fully perfected, be appropriately natural – that it will "emerge" from biology as biology from chemistry and chemistry from physics, or something like that... And this does not even raise the issue of whether or not description as such can be understood in naturalistic terms, or whether describing itself is an entirely natural event or fact. A fully naturalized semantics is, once again, just presupposed in the move to "description" without norms. But would not the description (of reliable processes of belief formation, etc.) itself have to be "correct," "right," "adequate," "justified"? And have we then gotten rid of norms if we simply do not mention them in our descriptions of epistemic processes?[202]

Admittedly, as Willard also points out, there are some self-styled naturalists who would simply accept, in the face of this burgeoning storm, that norms are perfectly natural: he instances Dewey as a case in point. But what would they be claiming then, precisely? On the one hand, they might state that we must merely look to natural science *in the ideal limit* in order to show us precisely what norms should be understood to be. (Does the notion of such a limit make sense, from a *realist* perspective, viz. if truth is absolute and the theories of science are to be taken literally? Laudan [1981] provides an extremely powerful argument that it does not, which most 'scientific realists' have incidentally come to accept.) On the other, they might urge that if norms have any effect on the physical world, they must themselves be a part of that world, since it is causally closed. Besides, does science not tell us that the only things which exist are 'in space and time'?

The distinction between these two lines is generally cashed out in terms of *ontological* and *methodological* naturalism, but unfortunately authors seem to disagree on what it is, exactly, that these broad groupings involve. For example, of the former, Koons writes: 'Ontological naturalism is the thesis that nothing can have any influence on events and conditions in space and time. According to the ontological naturalist, there are no causal influences from things "outside" space: either there are no such things,

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[202] Willard [2000], p.27.

or they have nothing to do with us and our world.'[203] Yet Moser and Yandell write: *'Ontological* naturalism takes various forms. We will understand such naturalism in terms of this core view: *every real entity either consists of or is somehow ontically grounded in the objects countenanced by the hypothetically completed empirical sciences (that is, in the objects of a natural ontology)'.*[204] However, I say the confusion is perfectly understandable, for the naturalist tends to vacillate between the specific claims (or theoretical content) of contemporary science – which she takes to be highly verisimilar and reducible, whence a tendency to physicalism – and the authority of 'science' (usually in virtue of its alleged method). (The latter stratagem comes into play when she is pressed hard upon what she takes to be statements of 'obvious scientific fact'. But there is much ammunition available, in history, for the challenger: failed theories about crystalline spheres, corpuscular light, caloric, phlogiston, luminiferous aether, and so forth.[205]) So when all is said and done, naturalism amounts to *methodological* monism: the naturalist is fundamentally of a scientistic mindset, and appeals to the authority of science *qua* discipline, in virtue of its putative method, and thus its content, both presently and in the final analysis.[206] Challenge a claim allegedly resulting from contemporary science, and if the naturalist finds the going hard – your arguments too plausible – then she will retreat to talk of the *impossible fiction* of 'complete science'. As Willard puts it:

> Naturalism staggers back and forth between physicalism (materialism) as a general ontology and first philosophy, and outright physics-ism or scientism (which need not take the form of physics-ism) – often, though not always, trying to derive physics-ism from scientism and then physicalism from physics-ism. This continues up to the present.[207]

If no space is left for inquiry outside natural science, however, then this *must* amount to empty rhetoric. Ontological naturalism would seem to stand or fall on the issue of whether methodological naturalism is correct – why else say not only that there are only physical things, but also that one is irrational to believe otherwise?[208] – yet methodological naturalism must be defended, if it is to be defended, on the basis of serious history of science, rather than textbook entries.[209] (And as I argue later in II.1,

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[203] Koons [2000], p.50.

[204] Moser and Yandell [2000], p.4.

[205] I should add that I am not sympathetic to the pessimistic meta-induction, in so far as it *is* an induction. So while I take it that this line of argument cannot establish that scientific inquiry will systematically continue to involve spectacular failures, it does, I think, establish the fallibility of inquiry.

[206] Is it this monism which brings about particular metaphysical and epistemological commitments, or the case that an unexamined set of such commitments brings about the monism? I think this varies from naturalist to naturalist: in any event, it is precisely the *commitment* that is the problem, as I try to explain in II.4.

[207] Willard [2000], pp.29-30.

[208] Notice that from Moser and Yandell's version of ontological naturalism, the leap to Koon's version, from the current theories in science, would be highly dubious. For how can we know what will, in fact, be the ontology in the 'final science', even assuming that such a terminus is possible? Further, what non-methodological reason is there for accepting Koon's version, other than appeal to 'scientific realism'? As we shall see, it is actually naturalism that is supposed to be required in order to support 'scientific realism', according to Boyd. So 'scientific realism' cannot simultaneously support naturalism! Furthermore, remember that I have already argued at length against ET, given AT.

[209] I take it, here, that ontological naturalism is not just nominalism, say of the form advocated by Sellars, who writes: '[A]lthough there are attributes, there *really* are no attributes... the qualification 'really' indicates that a philosophical point is being made, for in the *ordinary* sense of 'really,' of

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there is much that would seem to suggest the indispensability of a non-empirical dimension to inquiry, and there is an impressive array of historians, who have performed far more extensive historical research than I have, that agree.[210]) The point: how could we establish *within natural science* that its methods were sufficient to the task of providing a true theory-of-world, and have already made progress toward this goal? If we cannot, then methodological monism about natural science is indefensible, and arguably dogmatic: the project to defend it rests upon cognitive history, and thereby vitiates an argument for methodological monism about anything other than cognitive history. (I take 'cognitive history' to refer to a broad class of historical approaches which involve rational reconstruction of human behaviour, and an attempt to understand the thought-processes of 'the scientific greats', among other things: Zahar [1989] and Netz [1997] are exemplars of this.) Furthermore, on this historical line one might urge that the invocation of universals is absolutely *vital* to history, and thereby vitiates ontological naturalism too. Cohen puts this elegantly, while also suggesting – and foreshadowing one of the core claims I defend – that the proper method of science is not really distinct from the proper method of history, just because there is really only one overarching method of inquiry:

> If what is abstract is unreal, then the detached and characterless individual is the worst of all abstractions, and the most unreal!... [N]o historical description or explanation can possibly dispense with abstractions. Consider the description of any historical event whatsoever, e.g. the life or death of Caesar. Can we say that Caesar was rich, profligate, brave, ambitious, or what not, without using abstractions or aspects of life capable of indefinite repetitions?... *[W]hen we see a critical historian engaged in determining whether an alleged fact did or did not take place, his weighing of evidence does not differ in method from that employed in natural science.*[211] [Emphasis mine]

Indeed, we may press further by asking about the status of mathematics and logic, the very tools which scientists regularly employ. Ought we to rewrite history such that these were products of natural science?[212] Or can we throw such ladders away, in a Wittgensteinian manoeuvre? (I refer to the penultimate paragraph of his *Tractatus*.) Is the claim, now, that the final natural science(s) will be non-mathematical, or that mathematics will be 'retro-naturalised'? On this note, Willard is astute to point out that those who appeal to 'science' in support of naturalism tend only to appeal, in truth, to bizarre abstractions from the genuine article. For although individual scientists may say all sorts of strange things – take Bohr's obscure comments about 'complementarity' as a case in point[213] – it is hardly the case that it is a result of

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course, there really are attributes.' Indeed, his project is just, in his own words, to 'lay the groundwork for a theory of reference... which can claim to be the very foundation of a naturalistic ontology.' Sellars [1979], p.47.

[210] Their mere agreement is not an argument, of course, and I do not seek to use it as one; I merely wish to highlight that my view is not controversial.

[211] Cohen [1931], pp.14-15.

[212] For instance, Netz [1999] argues that the development of mathematics was quite separate from philosophy, let alone anything resembling contemporary science. This, although I should want to dispute whether there were some ontological assumptions that underpinned the exercise.

[213] On which, see Beller [1998]. In a subsequent letter to the periodical (August 1999, *Physics Today*), Sokal and Bricmont express considerable sympathy: 'Beller observes, correctly, that famous physicists such as Bohr, Born, and Pauli engaged at times in dubious (to say the least) extrapolations of ideas from quantum physics to politics, psychology, philosophy, and religion. She also notes that these writings were sometimes treated by physicists with excessive reverence, rather than being subjected to

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natural science that the only things that exist do so in space and time. Rather, those things are the *objects of inquiry with which natural science is concerned*:

> Could one possibly find the place in some comprehensive and duly accredited scientific text or treatment, or some technical paper, where it is demonstrated or necessarily assumed by the science concerned that *all that exists* consists of particles or fields or strings – or whatever the proper subject matter of the science is? Would Searle or anyone else be able to mention the name of the physicist who established this as an "obvious fact of physics"? Exactly where in the "atomic theory of matter" is the claim about what "the universe consists entirely of" to be found?[214]

As such, some analogues may be useful: where is the Neo-Pythagorean philosopher who will claim that only mathematical entities exist, and that this is a result of mathematics? Or the political theorist who will claim that only societies exist, and that this is a result of sociology?[215] (To the extent that there are such *soi-disant* philosophers in the latter case, the so-called 'Postmodernists', comparing them with naturalists is entirely appropriate. Crazies abound.) Indeed, one of the central points of this thesis is precisely that there is no overarching scientific method, as such. *It is a fiction, as anyone who has really done science must surely understand; courses on 'scientific method' are not found outside philosophy departments.* Rather, 'natural science' is an epiphenomenon of inquisitive *people* – I hope the hypothesis that scientists are people is not too implausible – engaging in critical discourse about particular problems which they attempt to state clearly, and tackle in earnest in order that they might discover the truth (or truths). And like all us humans, not only do they tend to concentrate only on a specific set of the available problems, but they also make mistakes. Trial and error is the game, and criticism the *organon*. To which I might add that it is a delightful irony that naturalists eschew *precisely* the kind of free-thinking, and critical exploration, that has been, and continues to be, vital to the practitioners of science. No doubt they would have been appalled by the notion of 'action at a distance' in Newtonian mechanics, Kepler's posit of an *anima motrix* emitted from Sol, Descartes' postulations about light and vision (despite his remarkable account of the rainbow), the explicit non-locality of Bohm's interpretation of quantum mechanics, Mpemba's claim that hot milk can freeze faster than cold, and so forth, were they to have found themselves in the relevant historical settings.[216] Appalled, that is, because instilled with a misplaced confidence in the then *status quo*; intoxicated by a heady cocktail of faith in authority and Kuhnian 'normal science', and badly in need of a sober historical perspective, in order to motivate future

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the critical analysis they deserve. Finally, she observes, again correctly, that the popular writings of these and other physicists – in which the foundational issues associated with quantum mechanics are often grossly oversimplified – served as one source of inspiration (among many others) for postmodernist musings about science.'

[214] Willard [2000], p.29.

[215] I do not assume that there are only natural-scientific questions, according to the methodological naturalist. Rather, she might hold that the only questions which can be satisfactorily answered, and are hence either (a) meaningful, or (b) worth discussing, are answerable by natural science(s). However, the problem is that endorsement of 'the one methodology' would then seem to be an act of faith, and open to the *tu quoque* objection proposed by Bartley, as I will explain in II.4.

[216] For example, Kepler's belief that an 'emotion' (from *animi motus*) was only emitted along the plane of the ecliptic led to what would now be seen as a mistake, namely the inverse-distance speed law. However, the underlying notion of force, understood by Kepler as 'the will of the sun', perhaps, proved fruitful indeed. (That is to say, neo-Platonic thought, along animist lines, contributed to a progressive research programme.) See Kuhn [1957], pp.214-216 & 245-249.

moderation. (I do not want to suggest that there is anything wrong with a bold hypothesis, as such: what is wrong is making such a hypothesis into a pet.)

Moreover, I ought to add a reminder that I have already argued in I.2.1 – I hope effectively – that the metaphysical foundation of 'scientific realism', MT, simply does not rule out an anti-naturalist stance with respect to the nature of properties (or indeed kinds, natural numbers, and so forth). It does not rule out MR, and does not suggest that ontological naturalism is a sustainable view. (Indeed, there is no mention of nominalism, or the need for any particular view on language, meaning, or reference, in the formulation of any of the theses that Psillos *explicitly* puts forward as constituting 'scientific realism'. But if additional theses are to be vital to the basic position he defends, then he owes us a straightforward statement to this effect. And the result would be that the system would become even more implausible: yet more *ad hoc* adjustments would be made in the attempt to render it 'effectively sustainable' in the light of criticism.) Now on this point, Moreland [2000] deserves a mention – although I cannot devote the space to outline his lengthy argument in full – since one of his fundamental claims is that there are many so-called naturalists who 'Accept a traditional realist construal of properties', but that 'often do so apparently without being aware of the fundamental issues involved... because these issues are matters of so-called first philosophy.'[217] Enough said.

I ought also to mention that in so far as I object to the epistemic thesis of 'scientific realism' (ET), and have argued against it in I.2.2-I.2.3, my attitude towards those who become obsessed with the content of contemporary science *tout court*, who almost *deify* it, is understandably one of disdain. (Theories have to be critically examined piecemeal. The mere fact that a theory is *accepted* into the corpus of a special science such as physics, even that it makes it into the textbooks employed to train young practitioners, does not thereby give it some sort of privileged epistemic status. *Inter alia*, it may not be well corroborated, and most expert physicists may correctly think it false, or to be of low verisimilitude. There may simply be *nothing better to put in its place* at a given point in time. And as a matter of fact, does anyone seriously think that one scientist will necessarily believe what another says simply because she has used a particular method to get her result, or that he really *ought to*? It is never that simple.) And here, interestingly, is ground on which I – and more generally critical rationalists, I think – can agree with some of the views advanced by van Fraassen, which I cover in greater detail later herein.[218] For the moment, let me just say that van Fraassen's brand of empiricism is, I would hold, a lesser evil than naturalism; for it is undogmatic to the extent that he would have it be a 'stance', and that he does not claim we are rationally compelled to be empiricists, or indeed to commit to belief in constructive empiricism. He argues, rather, why he is perfectly rational to *be* a constructive empiricist. And this I grant with admiration, although I strenuously disagree with his position.

Now to sum up, and tie this discussion to that of 'scientific realism', I ought to point out that I agree with Boyd that:

> Scientific realism is, by the lights of most of its defenders, the sciences' own philosophy of science. Considerations of the significant philosophical challenges

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[217] Moreland [2000], p.69.
[218] See II.1.3, III.4-III.5.4.

which it faces indicate that it can be effectively defended only by the adoption of a *meta*philosophical approach which is also closely tied to the science, *viz.*, some version or other of philosophical naturalism.[219]

Yet if methodological monism fails, *which is all that I really take myself to have argued for convincingly*, then it seems plausible that metaphilosophical disputes cannot be answered by appeal to science. (And to appeal to 'scientific realism', for Boyd, would be viciously circular.) Moreover, a significant sub-set of the views that go under the moniker 'naturalism' – e.g. materialism – suffer from a lack of serious motivation, except in so far as they might serve as research programmes for specific special sciences. This does not show them to be *false*, but rather entirely unmotivated in so far as they do not solve any problem(s), but rather create problems (such as the question about the status of norms), when applied in anything beyond such narrow domains.

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[219] Boyd [2002], section 6.6.

II

ON METAPHYSICS, REALISM, AND INQUIRY: PREPARING THE GROUND

INTRODUCTION

Thus far, my approach has been decidedly critical. I have tried to illustrate where I find 'scientific realism' to be lacking, even unacceptably dogmatic, with particular emphasis upon its metaphysical and epistemic foundations. Yet I should emphasise that I see epistemology, properly construed, as lying within the domain of the philosophy of mind. And I see the domain of philosophy of mind as lying within that of metaphysics; as such, epistemology is just applied metaphysics, where the objects of inquiry are persons – not minds, which are at best only parts of persons – and their interactions with other entities.[1]

But what motivates this view, and why do I think it not only to be worth defending, but also to be progressive? In this chapter, I will answer this question in some detail, while also trying to advance several theses about realism – in so far as such an overused word refers, and is useful – and metaphysics. Further, I shall try to draw some general lessons about inquiry, while elucidating the critical rationalism that I prefer, on the foregoing basis. As such, this chapter sets the stage for the remainder of this thesis, and the view of the relationship between metaphysics and natural science that will be developed hereafter.

First, then, by way of a historical perspective to motivate my view, I want to examine the significance of Aristotle's thought in the Latin West between the Fall of the Roman Empire and the oft-hailed 'Enlightenment', with particular emphasis on the question of ontological continuity. For it is claimed by some, mistakenly I think, that the rejection of Aristotle's metaphysics – in particular, his view of αἰτία – both actually occurred, and was vital for the 'birth' of natural science. That is, for its 'emergence' from previous 'natural philosophy', and its growth into a distinctive discipline with a privileged epistemic status. But against this, I shall argue that Aristotle was precisely the father of science – as far as we can tell, it must be added – and that his (admittedly imperfect) mode of inquiry set the standard for that which followed. As such, I defend the view that natural science began with Aristotle, or perhaps even earlier, rather than two thousand years later, in Western Europe. Indeed, that it was practised in various enclaves, with varying degrees of success, not only in the Latin West, but also in Ancient Greece, the Byzantine Empire, the Greek East, and the Islamic world.

Second, I want to explore the nature of 'realism', while trying to avoid mere quibbles

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[1] I ought to clarify that I am not discussing pure metaphysics, as *a priori* discipline, here. The view is that if we do our metaphysics well, and do our natural philosophy well, then the correct epistemological perspective will drop out. Epistemological inquiry would, I hold, be radically impossible without a backbone of categorial access and a body of sense faculties.

over the 'proper' meaning of the word. For 'realism' really stems, in its modern incarnation, from a rejection of the doctrine that *representations*, rather than things-in-themselves, have some sort of epistemic priority, be that with respect to perceiving the actual, or indeed conceiving of the possible.[2] The distinction between transcendental and empirical 'levels' is significant here, and I shall want to argue that although genuine realism must start from the transcendental level – indeed, that transcendental idealism is ultimately self-refuting – it must nevertheless avoid the pitfall of empirical idealism (such as that of Berkeley, or indeed Mill). As such, the theory of perception lies at the heart of the realist-idealist debate (see the previous footnote); for true realism is founded upon the hope – and perhaps it is only a hope – that there are not mystical 'barriers' between us, *qua* sentient organisms, and the other entities that inhabit the world. As I have already argued in the previous chapter, if fallibilism is forced upon us, as it seems to be, then realism seems our best bet. The reason is that it would be curious to merely assume the worst – that we are trapped in a prison of representations, even assuming 'we' genuinely refers – and thereby achieve only a dogmatic self-paralysis. (Even under Kant's ingenious view, the things we can be 'certain' about are hardly interesting. There is hardly a world we can make it our task to discover; it is all *there*, in so far as we could grasp it, *for the sensing*.[3]) To put it bluntly, then, my view is that realism is methodologically advisable, and perhaps psychologically and ethically so, to boot. Still further, that the distinction between 'transcendental' and 'empirical' is just fallacious, although it provides a useful heuristic for discussion, unless it amounts to a distinction between conception and perception, or more properly different modes of investigation of *being*. Of that which *might be*, and of that which *is*, in a robustly existential sense.

Third, I want to explain precisely how and why it is a mistake to think that one can do epistemology without ontology, or that epistemology is First Philosophy, and my vehicle for this discussion will be the very idea of 'knowledge'. For I shall argue that this is a

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[2] We might note that the Greeks of Aristotle's period never encountered the mind-body problem, but that this was not due to an oversight. Matson [1966] provides a plausible explanation: 'It is true that in translations of *De Anima* one finds "sensation" and "perception" used freely where Aristotle has *aisthêsis*. But this is seldom right. *Aisthêsis* means "sense" ("the five senses") or "sensing" (a generic term for cases of seeing, hearing, etc., individually or collectively taken). Perceptions and sensations are *had*, while the word-for-word equivalent in Greek of "to have a sensation," *aisthêsin echein*, would mean (if it meant anything) "to *be* sensed," i.e., to have an *aisthêsis* directed toward one, to be the object of an *aisthêsis*... an *aisthêsis* must have a cause, though it may turn out not to be what it was thought to be at first...the comparatively rare formation *aisthêma*, "that which is the consequence of the activity in *aisthêsis*," occurs in Aristotle's writings some ten times, and in three of these cases it is natural and perhaps inevitable to translate it by "sensation," "sense impression," or even "sense datum." All of them, though, occur in the treatise *On Dreams* (460b2, 461a19, 461b22), and the spooky context, the need for a word to designate a floating image not ascribable to sense perception, explains the usage... On the whole, it seems that the Greeks found it no more necessary, or even possible, to talk "phenomenal language" or "raw-feel-talk" than we do, and that their philosophers lacked motives... for exhorting them to do so. And no sensations, no mind-body problem.'

[3] In other words, I would suggest that Kant is the most devout of *empirical realists*, but that this 'realism' comes at a terrible price. As I have suggested beforehand, in section I.2.2, his very mention of things-in-themselves would seem impermissible if we can only come into contact – intellectually or perceptually – with representations. But then the very notion of 'representation' becomes dubious. For that which represents must have something *to* represent; and ultimately, what this amounts to is that 'presentation' would be a better notion, as I later suggest.

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hollow notion when it is understood in the sense advocated in much of contemporary epistemology, viz. 'justified true belief'. Further, that what we should be interested in is just that which is true, and that a wedge may be driven between the quest for truth (or true propositions as *objects* of belief) and the (mere) quest for 'knowledge', the former being preferable from an axiological perspective. That is, provided truth is understood in a non-epistemic fashion – that AT holds – such that a given proposition maintains its truth value irrespective of our inquiry, characterised in terms of means or otherwise.[4] In short, I hold that the *absolute* truth is that which we ought to seek, for reasons which may seem rather Platonic, and that our proper means of going about this task are fundamentally Socratic. Hence, that a woman who seeks only 'knowledge' may very well be a sophist or a rhetorician, with no interest – or at least a minimal and superficial interest – in *that which is*, and *that which is the case*.

Fourth, I should like to say something about the issue of demarcation. For the reader may have noticed that I have been careful to employ the phrase 'natural science' in my antecedent discussions, and have used this interchangeably with 'natural philosophy', and it now behoves me to explain why. In essence, this is since I take the view that a discipline is properly defined by the objects of its inquiry, rather than its methods, and as such that the only important demarcation criterion for science and non-science is whether the approach taken to the subject matter is *critical* or not. (This does link into the idea of whether the claims made therein are, or are not, severely tested; that is, tested to the best of our ability, in an ongoing process of investigation. My sympathy with Popper's critical rationalism should be obvious by now, and I make no apology for this.) To put this differently, a putative discipline of inquiry is simply not a discipline – a science, in the sense of *scientia* – unless it involves a critical approach to problems about some aspect of the world (understood, I emphasise again, as the sum total of that which exists).[5] Hence, I say mathematics is a science, and metaphysics is a science – the First, as it happens – as much as physics, chemistry, and biology are.[6] None of this is to deny that there are local methods, suitable to the performance of particular tasks such as solving a quadratic equation in algebra, or performing a statistical analysis of the results of an experiment to measure a posited constant (e.g. the gravitational constant, or the permeability of free space) in nature. It is to say that *those* methods do not characterise

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[4] If that which is 'true' is to be based on consensus, even in Peirce's 'ideal limit of inquiry', then I will agree that justification is of great import; further, as suggested earlier, that so-called 'inductive inferences' are important, since they clearly *persuade*. Yet I reject this theory of truth, which is fundamentally anti-realist, since it denies inquiry a satisfactory (or desirable) aspiration. This said, it should be sufficient in context to point out that I agree with Hendry [1995], p.58: '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 [what I call ET] is rendered *Pickwickian* on this view by its identification of truth with rational acceptability in the ideal limit of enquiry.'

[5] It might be added that this approximate explanation will not do in the final analysis, for metaphysics is, under my view, an examination of *that which might be*, and hence only derivatively involves a critical approach to problems about some aspect of *this* world. (In other words, it involves, to a first approximation, a critical approach to problems about some aspect of broadly logically possible worlds.) I ought also to add that my talk here is supposed to be atemporal; i.e. 'that which is in the actual world' should not be confused with 'that which is the case at present in the actual world'.

[6] This is not to say that there are not 'soi-disant metaphysicians', and 'soi-disant natural scientists', of course.

disciplines, as such – they are more like problem-solving strategies for specific problems, which involve objects of inquiry (or manipulation) that fall into the domain of a particular discipline. In a nutshell, this is again rather Popperian – at least as I understand Popper – in so far as there is no 'scientific method' above and beyond 'the one [proper] method of philosophy'.[7]

Finally, I ought to mention here that my foremost target in the remainder of this thesis is empiricism, and more generally *subjectivism* (which I see as having been at the heart of the empiricist project, historically speaking), which I see as a Scylla to the Charybdis of 'scientific realism'. (Indeed, although I wish to navigate us safely between these two, I would point out that the Scylla is a fearsome monster, whereas the Charybdis is natural enough, and would prove a less horrific way to bite the dust.) My arguments against such doctrines will come to a head in the subsequent chapter, but I begin to chip away at them here, in preparation for the battle proper. Section 4, in particular, draws together an important thread – on the role of authority – which runs, quietly, through this entire chapter.

1. THE RISE OF ARISTOTELIAN THOUGHT

> Aristotelian philosophy did not fill an intellectual vacuum, but invaded occupied territory –
> Lindberg[8]

After the fall of Rome in the fifth century, the tenuous link between the Latin West and the Greek East, which had long been divided along administrative lines, was finally severed. As Barker puts it:

> [T]he empire in the West after 476 A.D. was in abeyance for some hundreds of years, so far as a visible emperor, or a capital, or a system of administration was concerned.[9]

The vast majority of Greek manuscripts became inaccessible in the West, due both to linguistic and geographical barriers, and scholarship entered into decline, in both quality and quantity.[10] And whereas great centres of learning existed in the East, such as Constantinople and Alexandria, academic pursuits in the West were generally confined to monasteries.

However, due primarily to the efforts of Boethius (480-524), a small number of Aristotle's logical works were translated into Latin, and made available to ecclesiasts.[11] Through the use of *scriptoria*, these were preserved and disseminated somewhat, and

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[7] Popper [1980], p.16.

[8] Lindberg [1992], p.216.

[9] Barker [1923], p.84.

[10] The Roman society did not do a good job of preserving and translating the abstract work of the Greeks. As Singer argues: 'The matter seems to have lain deep in the Roman character...is not improbably related to the Roman obsession for Rhetoric...[and ultimately] by the Roman desire for 'useful studies'.' Singer [1923], pp.268-269.

[11] On Boethius, see Clagett [1957], pp.150-153. Calcidus had earlier translated some of Plato's work, most notably his *Timaeus*.

became accessible to successive generations. Such *philosophia* – when it was appealed to – was initially expected to play a handmaiden role to the Christian faith. But it proved to be an impressive one, nevertheless:

> For [apologetics]... the logical tools developed within the Greek philosophy proved indispensable.[12]

Indeed, Aristotelian logic had considerable influence on the works of: Isidore of Seville (ca. 560-626); Gregory of Tours (d. 595); Gregory the Great (ca. 550-604); and the Venerable Bede (d. 735).[13]

Now this would seem to suggest that the value of critical thought – viz. the careful application of reasoned argument, rather than rhetoric and appeal to authority – became inexorably greater in the eyes of religious scholars. And although this development was unavoidably slow, given the unpleasant situation in the West, it is not inconceivable that it might have occurred more swiftly, in a different socio-political setting. For a vital factor in the promotion of scholarship is *security*, in both economic and military respects; only when leisure time is available, travel is not periculous, large communities are established, and ideas can be readily exchanged, does it become possible for *literati* to become maximally productive.[14]

The plausibility of this claim is bolstered, first, by the fact that the greatest scholars of the ninth and tenth centuries – namely John Scotus Eriugena (fl. 850-75) and Gerbert (ca. 935-1003) – were both beneficiaries of the educational reforms made in the Carolingian Empire, under the guidance of Alcuin (ca. 730-804).[15] And the first chapter of the former's *Treatise on Divine Predestination*, for example, is entitled precisely 'That Every Question Is Solved by the Fourfold System of the Four Rules of the Whole of Philosophy', and the influence of Greek thought is manifest as he continues by writing:

> ΔΙΑΙΡΕΤΙΚΗ, ΟΡΙCΤΙΚΗ, ΑΠΟΔΙΚΤΙΚΗ, ΑΝΑΛΙΤΙΚΗ... No man instructed in the art of disputation has any doubt that it is indeed by means of those four parts, as by some useful and honourable fourfold method of human reasoning, that the very art of disputation, which is truth, is arrived at.[16]

Second, it is supported by the approaches of Anselm (1033-1109) and Peter Abelard (ca. 1079-ca. 1142), who were both born into a considerably more *secure* West.[17] The former's infamous 'ontological proof', in his *Proslogion*, is a fine exemplar of an attempt

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[12] Lindberg [1992], p.150.

[13] Ibid., pp.158-159, and pp.184-185. I should also add, at this stage, that there are also signs of Plato's influence in such works. See, for instance, Bede's striking reference to 'a secular writer [who] very truly said, the world would be in the happiest possible state if kings were philosophers or philosophers were kings.' Bede [731], p.309.

[14] I am not claiming security is a sufficient factor, just a necessary one.

[15] Charlemagne brought Alcuin in from York. See Lindberg [1992], pp.185-190

[16] Eriugena [850], p.8

[17] In the words of Lindberg: 'When Gerbert died...Western Europe was on the eve of political, social, and economic renewal.' Ibid., p.190

at syllogistic reasoning, devoid of authority-based claims. And arguably his earlier *Monologion* is intended to be entirely devoid of appeal to authority, since as he explains:

> Some of my brethren have often and earnestly asked me to write down, as a kind of model mediation, some of the things I have said, in everyday language... They specified... the following form for this written meditation: nothing whatsoever to be argued on the basis of the authority of Scripture, but the constraints of reason concisely to prove, and the clarity of truth clearly to show, in the plain style, with the everyday arguments, and down-to-earth dialectic, the conclusions of distinct investigations.[18]

The latter's '*Sic et non*' is clearly designed to encourage the reader to employ critical faculties, and engage in philosophical reflection, as indeed are several of his other works. Indeed, in his *Dialogue Between a Philosopher, a Jew, and a Christian* we find a particularly strong focus on dialectic: '[I]t's the philosophers' job to investigate the truth by means of reasons, and in all things to follow not people's opinion but reason's lead.'[19] In both Anselm and Abelard, then, we find the beginnings of a conflict between reason and faith – a rationalist turn, whereby the learned began to place greater trust in the capacities of their own minds and senses, was underway. Indeed, it is interesting to note that Anselm writes of 'the truth of the senses', talks of their potential deception, and even mentions the example of the stick that appears to be bent when part of it is underwater.[20]

This said, it might still be denied that such a turn could truly be considered to have been due, even in part, to the influence of Aristotle's logical works. Moreover, it might be argued that the development of such a conflict did not constitute any sort of progress, at least with respect to encouraging either the practice of, or the development of a suitable methodology for, science. Both these objections will need to be countered, if my central claim in this sub-section, specifically that Aristotle's logical works encouraged the adoption of an attitude *beneficial* for scientific work, is to be maintained.

To the first objection, which I consider to be the easier to counter (given the foregoing historical examples), I think it best to offer a couple of quotations directly from Aristotle:[21]

> The method is the same in all cases, in philosophy and in *any* art or study... it is the business of experience to give the principles which belong to each subject.[22] [Emphasis mine]

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[18] Anselm [1059-1107], p.5. See also passage 66 of the *Monologion*, p.72. Further, note that his work *On Truth* also suggests something like a correspondence account of the truth of statements, and a rather sophisticated distinction between 'two truths'. He writes: 'A statement is then right and true either because it is correctly formed or because it fulfils its function of signifying correctly. The former belongs immutably to it, the latter is mutable. The former it always has, the latter not always. The former it naturally has, the latter accidentally and according to use.' See pp.154-155

[19] Abelard [1136-1139], p.59. Of course, this chain of thought comes to Aristotle, presumably, via Plato, and thus Socrates (from what we can tell, on the basis of Plato's, and to a lesser extent Xenophon's, characterisations of him).

[20] Anselm [1059-1107], *On Truth*, pp.158-160.

[21] Admittedly, there may still be some doubts about whether these aspects of Aristotle's thought were noticed, and interpreted in a literal fashion, but it seems plausible to assume that they were, without available evidence to the contrary.

[C]onvention represents the opinion of the majority, whereas the wise speak according to the standard of nature and truth.[23]

To answer the second, and more difficult, objection, there can be no finer place to look than to the core debate in *Criticism and the Growth of Knowledge*. For on the one hand, Kuhn argues that a revolution can only occur when a 'critical-mass of anomalies' (viz. predictive errors, or explanatory gaps) are seen to confront a dominant disciplinary matrix, or 'constellation of group commitments'.[24] Whereas on the other, those such as Popper and Watkins hold that revolutions can also be brought about by direct criticism of the status quo.[25] Unsurprisingly, this is a reasonably complex discussion, which would require considerable time to fully unveil and examine. Gladly, however, this will not prove necessary, since I hold that my argument goes through under either viewpoint.[26]

Prima facie, it should seem obvious that this second stage of my claim would be supported by both Popper, and his 'critical rationalist' followers.[27] Indeed, they would likely want to characterise the aforementioned 'rationalist turn' as representing a shift from an uncritical acceptance of Christian (esp. Biblical) doctrine, to a recognition that criticism was indispensable, both to resolve literal contradictions, and to persuade the unbeliever.[28] Initially, such criticism may only have aimed at comprehension of the (often obscure) doctrine – but this was just a slippery slope, which eventually led to claims such as "Moses was a (white) liar"[29], and "God cannot bring about the logically (or even *ontologically*) impossible".[30]

But how is my claim also compatible with the Kuhnian view? (There was no obvious 'critical-mass of anomalies'.) Well it might be argued that Aristotelian logic managed to take its place as an *exemplar* (or shared example).[31] Specifically, that Aristotle's demonstrative arguments provided a 'pattern' which was subsequently emulated by theologians. Why should this be significant? Just because, as already mentioned, it then

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[22] Aristotle, *Prior Analytics*, 46^{a}1-30. Barnes (ed.) [1984], p.73.

[23] Aristotle, *Sophistical Refutations*, 173^{a}25-30. Ibid., p.294.

[24] This is bound up with the Kuhnian notion of 'normal science', which involves attempts to solve the *puzzles* that are generated by the belief in (or acceptance of) a given disciplinary matrix. When a community is engaged in 'normal science', the failure to solve a puzzle is seen to be the fault of the puzzle-solver – any schoolboy can botch the simplest of experiments – but when belief in a disciplinary matrix is shaken, by its repeated failure to predict/explain certain phenomena, the community may then come to see this as *problematic* for said matrix. ('Extraordinary science' will then ensue.) See Kuhn [1996], ch.4, 6 & 7, and Kuhn [1977], pp.266-292.

[25] Popper [1970] and Watkins [1970].

[26] I discuss these conflicting views further when I come on to the issue of demarcation, in II.4.

[27] For a modern overview of this position, see Miller [1994]. For particular emphasis on this aspect of Popper's thought, see Boland [1994].

[28] That is, criticism based both on reason, and experience.

[29] Here, I refer to Aquinas's explanation of Genesis 1:6. See Kuhn [1957], pp.110-111.

[30] In the thirteenth century, some theologians claimed that God could not move outside the universe, or create a universal without something to exemplify it. Lindberg [1992], pp.237-238.

[31] I try to avoid talk of 'paradigms', because the term is notoriously inaccurate. (See Masterman [1970].) Instead, I refer either to 'disciplinary matrices', or 'exemplars'. For an explanation of these two distinct, yet intertwined, ideas, see the postscript in Kuhn [1996], pp.176-191.

becomes 'the business of experience to give the principles which belong to each subject', and movement towards a 'critical-mass' becomes possible. In simpler terms, it is surely true that, for a scientific approach to have a chance of emerging in a community, it is better for it to believe that experience plays some role in the search for truth, rather than none whatsoever![32]

Furthermore, it should be noted that even if Miller is right, ultimately, that 'Scientific hypotheses propose order for the world; they do not presuppose it'[33], it remains plausible that the presupposition of an order to the world, when widespread in the members of a community, plays an important role in *motivating* natural philosophy.

1.1   THE DOMINANCE OF ARISTOTELIAN THOUGHT

Revolutions through criticism demand normal science no less than revolutions through crisis. – Kuhn[34]

In the twelfth century, when the Latin West had achieved greater stability, the chance came to gain access to more of Aristotle's work. This was quickly leapt upon by men such as Gerard of Cremona (ca.1114-87), who translated several of Aristotle's works from Arabic manuscripts, and culminated in the translation of the entire Aristotelian corpus directly from Greek, by William of Moerbeke (fl.1260-86).[35] During the period which separated these two men, it became clear to scholars just how much scope the Aristotelian philosophy truly had, and its study became mandatory in the newly formed universities. In the words of Lindberg: 'by the second half of the thirteenth century...no student emerged from a university education without a thorough grounding in Aristotelian natural philosophy.' And as the natural culmination of my argument above: 'Methodologically, the universities were committed to the critical examination of knowledge claims through the use of Aristotelian logic.'[36]

Understandably, given the situation beforehand, Aristotle began to be seen as an authority himself – hence, worthy of the title *Philosophus* – and this led to an uncritical acceptance of his overall cosmological framework. So while the scholastics did enter into serious criticism of the minutiae therein, much as Kuhn's aforementioned model of 'disciplinary matrices' might suggest, they were loath to reject any core elements of his system, in particular the central Earth.[37] To the men of the Middle Ages, the suggestion that the Earth revolved around the Sun would probably have seemed just as ridiculous as the

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[32] 'None whatsoever', that is, beyond the trivial sense in which one must use one's senses in order to read 'the word of God'.

[33] Miller [1994], p.27

[34] Kuhn [1970], p.233

[35] Lindberg [1992], pp.204-205

[36] Ibid., p.212. The first claim is too bold, and 'very few students' should replace 'no student'.

[37] This is firmly entrenched in an Aristotelian worldview, linking to the notion of generation and corruption on Earth, the natural positions of the elements in the sublunary region, the physical determinism of the universe (and hence astrology), etc. Of course, it might be argued, fairly enough, that this held back astronomy's progress.

inverse suggestion, in the modern day; and understandably, given the nature of their research programme.[38]

So I am perfectly willing to concede that Aristotle's *cosmology*, taken holistically, did not constitute, contribute to, or even partially engender, a progressive (rather than degenerative) metaphysical research programme. But might it not be the case that the devil was, to a considerable extent, in the *detail* thereof? And might it not be held, furthermore, that there were several aspects of his ontology, and moreover his methodology, which not only served to promote a *more scientific* approach in the Latin West, but were also retained in the so-called 'scientific revolution'? I shall argue that the answers to both these questions are affirmative.

Superficially, it certainly appears that McMullin would want to disagree, holding instead that: 'the new science [of the Renaissance] involved a revolution in the domain of *method*'.[39] This 'new science' was, he claims, different from the 'Aristotelian science' in four distinct respects:

i)   It did not rely on *a priori* and demonstrative argumentation, of the sort advocated in Aristotle's *Posterior Analytics*.

ii)   It had a quantitative, rather than qualitative, character. It involved the application of mathematics to the description of nature; a process which Aristotle did not recommend.

iii)   It examined phenomena in contexts which Aristotle would have called 'non-natural', and therefore advised the scientist to ignore.[40]

iv)   It unified the previously separate Greek concepts of θεωρία and τέχνη: '[a] possibility that...could scarcely even have crossed Aristotle's mind.'[41]

I would, however, beg to partially differ. And in reply to the antecedent assertions, sequentially:

i)   In his biological work, Aristotle does not stringently follow (or even appear to be particularly concerned with) the method that he advocates in his *Posterior Analytics*. On the contrary, he makes striking statements, such as that which follows, which seem to suggest an almost Baconian view:

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[38] Here, I allude to a Lakatosian idea. The central Earth might be seen as part of the negative heuristic, or 'hard core', of the medieval natural philosophers' research programme. See Lakatos [1970], pp.133-138.

[39] McMullin [1965], p.108

[40] To clarify, Aristotle held that certain types of motion were 'artificial', insofar as they were brought about by (*inter alia*, personal) agents. He would therefore consider a projectile, such as a thrown javelin, to be behaving 'artificially'. Likewise, he would consider a stone held in the hand to be 'constrained', viz. prevented from undergoing its natural motion toward the centre of the universe.

[41] McMullin [1965], p.123

Such appears to be the truth about the generation of bees, judging from theory and from what are believed to be the facts about them; the facts, however, have not yet been sufficiently grasped; if ever they are, then credit must be given rather to observation than to theories, and to theories only if what they affirm agrees with the observed facts.[42]

Unfortunately, this may have gone somewhat unnoticed just because medieval scholars tended to concentrate on the Aristotelian *Organon* (logical works) first, and the *Libri Naturales* second: '[even] less frequently would a student encounter the large group of works on zoological subjects.'[43]

More importantly, Aristotle's methodology in the *Posterior Analytics* is not really *a priori* in the sense that McMullin intends. For as I have already mentioned, the first principles used in demonstrative arguments must be arrived at via *experience*, and involve the careful observation of particulars. A plausible reading of the Aristotelian claim that 'all intellectual learning come[s] about from already existing knowledge' is, I venture, that the human intuition, and ability to conceive, is (only) capable of delimiting the ontological *possibilities*.[44] In Aristotle's words:

...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.[45]

Thus νους has its place, but is inextricably bound to επιστήμη. Lloyd would agree:

...if there is a lesson to be learned from his 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.[46]

ii)   If anything, Aristotle took a neutral position on 'the question of the applicability of mathematics to nature'.[47] Against McMullin – who fails to cite where Aristotle putatively disavows such an idea – he thought that mathematics could play an important explanatory role with respect to natural philosophy:

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[42] Aristotle, *Generation of Animals*, 760^{b}29-33. Barnes [1984], p.1178

[43] Copenhaver & Schmitt [1991], p.10

[44] This is Lowe's central thesis in his [1998], although he does not directly attribute it to Aristotle, and is a view I develop in III.1.

[45] Aristotle, *Posterior Analytics*, 81^{a}40-81^{b}9. Barnes [1984], p.132. It might also be added that Aristotle admits 'it often happens that we make mistakes...if there were no triangles other than the isosceles, having two right angles would seem to belong to it as isosceles.' Ibid., 74^{a}3-74^{a}19, p.119

[46] Lloyd [1968], p.180

[47] Lindberg [1992], p.86

it is for the doctor to know the fact that circular wounds heal more slowly, and for the geometer to know the reason why.[48]

I think it is correct to say that Aristotle viewed areas such as medicine and geometry as radically separate, in principle. Still, this would not preclude a medic from studying geometry, and using it in both explanatory and *heuristic* medical roles (viz. to generate medical hypotheses). (As a matter of fact, does the modern doctor not attain a measure of competence in pure mathematics, during his schooling?) Besides, it must be remembered how much Aristotle valued explanation; αιτία play a core role in his *Metaphysics*.

iii) There is evidence from Aristotle's zoological works, as mentioned in (i), which suggests that his natural/non-natural distinction is more subtle than is oft appreciated. For example, Aristotle writes that 'what is done by violence is contrary to nature', yet he clearly examined the internal organs of animals which were killed by his own hand.[49] (Here, perhaps the death of the animal may be considered unnatural, but its formation, prior to that point, assumed to be natural.[50]) Moreover, his intellectual grandson, namely Strato, 'more than any earlier Greek scientist...tried to use experimentation to investigate physical problems.'[51] Is it plausible to suggest that this bears no causal relation whatever to Aristotle's earlier work? I should say not, and suggest that Aristotle's approach may have served to encourage Strato to undertake such investigations.

iv) Guessing what may or may not have crossed Aristotle's mind is surely inadvisable. He was clearly an extremely competent philosopher, capable of much original thought – this, surely, is why his works were prized enough to have survived the Hellenic era. This aside, as far as the written evidence goes, Aristotle seems to have held the view that these 'two-tiers' of human endeavour interacted; much as the foreman (theorist) on a building site does with the workers (technicians) under his guidance. For example, he writes:

> With a view to action experience seems in no respect inferior to art, and we even see men of experience succeeding more than those who have theory without experience...But yet we think that *knowledge* and *understanding* belong to art rather than experience, and we suppose artists to be wiser than men of experience...and this is because the former know the cause, and the latter do not. Hence we think that the master-workers in each craft are more honourable and know in a truer sense and are wiser than the manual workers...[52]

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[48] Aristotle, *Posterior Analytics*, 79^{a}14-17. Barnes [1984], p.129

[49] Aristotle, *Generation of Animals*, 788^{b}28-29. Ibid., p.1218

[50] Ironically, killing an animal does, of course, have an effect on the whereabouts of the blood in its body. (This is why dissection of animals might be seen to support the notion of vital heat.)

[51] Lloyd [1973], p.19. I say more on Strato later.

[52] Aristotle, *Metaphysics*, 981^{a}13-31. Barnes [1984], pp.1552-1553

Now to my reading, this suggests a feedback loop between τέχνη and θεωρία. Specifically, that the former could serve to elucidate new phenomena (whether understood as 'constant conjunctions of events', or 'facts', etc.) which demanded subsequent causal explanation by the latter.

Thus, while it is clear that the scientific revolution was a great *cosmological* revolution, it is less clear that it *needed* to involve any significant, rather than merely superficial, methodological or ontological revolution. Curiously, McMullin himself admits:

> ...a measure of continuity is being taken for granted [in this paper], a continuity of belief in the generalizing power of the human mind...[53]

Moreover, he does want to agree that:

> [Aristotle] was the first to vindicate in detail the claim of the human mind to discover intelligible permanent patterns intrinsic to nature. He stressed the importance of observation more than anyone else had done up to his time.[54]

And finally, that:

> ...there is scarcely a single element in the new methodological complex that did not have *some* precedent...[55]

So it emerges, first, that McMullin and I differ mainly in emphasis; he makes his comments about continuity only in passing. Second, that he points to a revolution which I would claim was only *contingently* beneficial, just because medieval philosophers did not read Aristotle in the correct fashion, and placed too much faith in his cosmological conclusions, rather than the method (and associated ontology) by which he reached them. This picture does not seem unreasonable, given that the curriculum in medieval universities rarely included Aristotle's zoological works, and that the scope of the Aristotelian corpus was far in excess of the available competition.

To put it bluntly, I would therefore hold that Aristotle's philosophy was not methodologically 'regressive' with respect to its true content, but rather its *perceived* content.[56]

1.2   Two Historical Theses

Bacon's attempts to construct a discipline of observation were little better in their direct benefits for the practice of natural philosophy, but at least they proclaimed an empiricist ideal that proved more inspiring than Zabarella's efforts to reform Peripatetic methodology. –
Copenhaver and Schmitt[57]

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[53] McMullin [1965], p.108

[54] Ibid., p.127

[55] Ibid., p.129

[56] Analogously, in the modern day, many scientists claim to support Popper's falsificationist view, but mistakenly understand the Popperian notion of 'corroboration' to be *confirmation*. That is to say, they support a vulgar version of Popper's methodology, which is not anti-inductivist.

If my conclusions in the foregoing section are correct, then it would follow that Greek natural philosophy after Aristotle had every chance of making considerable progress, and perhaps even *did*. Moreover, it would also follow that (most of) Aristotle's methodology and ontology might have been left intact during the Renaissance, to no detrimental effect, *provided that his cosmology was rejected, and that his empirical claims (in particular, observation statements) were re-examined.* I shall now argue that there is some evidence, albeit not conclusive, in favour of both these claims.

On Greek Science

In favour of the first claim, there is the work of several historians, who would all agree with Farrington that:

> Taking into account its content and methods only, Greek science from the 2nd century B.C. onward was ready for the scientific revolution.[58]

Which non-methodological factors may have prevented the revolution? According to Farrington, slavery. According to Clagett, the influence of the Romans, and the growth of Christianity. According to Lloyd, the lack of printing, and the absence of social support for natural philosophers (i.e. they were neither seen, nor treated, as a *professionals*).[59] A sensible view seems to be that each of the aforementioned were causally contributory, to a lesser or greater extent.

But I need not, and do not want to, defend such a bold thesis as Farrington's.[60] Instead, it is sufficient for me to point out that progress was made, in the work of Aristotle's intellectual grandson, Strato of Lampsacus.[61] From what can be gathered, he argued that:

a)   All bodies hold a *degree of weight* (so 'heavy' and 'light' are not contraries); lighter bodies are displaced by heavier bodies. Obviously, this interpretation is more amenable to a mathematical treatment than Aristotle's. For *degrees of weight* may be expressed numerically, in terms of an arbitrary unit.

b)   When bodies are moving naturally, they move ever more quickly as they approach their natural place (e.g. falling bodies accelerate). The impact of a falling body depends not only on its weight, but also the height from which it is dropped. This demonstrates that he grasped the notion of *instantaneous*, rather than average, velocity. It also suggests a proportionality relationship

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[57] Copenhaver and Schmitt [1991], p.121.

[58] Cohen [1994], pp. 249

[59] For a brief overview of all these positions, see Ibid., pp.249-255

[60] For I would hold that there was also one significant methodological factor: the geometrical, rather than algebraic, approach adopted by Greek mathematicians.

[61] See Lloyd [1973], pp.16-19

between potential energy and height, and potential energy and weight, for bodies near to Earth.[62]

This achievement is all the more remarkable because Strato was born a generation before Archimedes, and was probably not prompted by the great mathematician's 'lever law'. Indeed, on a slight tangent, this draws it our attention that the greatest mathematicians of ancient Greece (of which we know) with the exception of Euclid (fl.380 B.C.), namely the aforementioned Archimedes (ca.287-212 B.C.), and Apollonius of Perga (fl.210 B.C.), both came *after* Aristotle. So we are left to wonder if Aristotle would not have recognised that mathematics could play a greater role in natural philosophy, and achieved even greater things, had he been exposed to their work.

It is also worth adding that medieval natural philosophers were not, in general, versed in the later mathematical work of the ancient Greeks. According to Boas:

> The works [sic] of the best period of Greek Science, of the Hellenistic scientists of c.300-150 B.C., was little known in the Middle Ages, partly because it was often highly mathematical and always complex and difficult.[63]

Given this, it does not seem unreasonable to suggest that a general lack of mathematical sophistication was responsible, to some extent, for the inability of Medieval natural philosophers to bring about a 'scientific revolution'. Although it is conceivable to blame this lack on the dominance of Aristotle's philosophy, it is hardly plausible; for Aristotle certainly promoted mathematics as a worthwhile area of study, albeit not necessarily for natural philosophical goals.[64]

On Methodology: Zabarella vs. Bacon

In support of the second claim, we might look to the work of Pietro Pomponazzi (b.1463) and Jacobo Zabarella (b.1533), who were both devoted Aristotelians. The former was certainly a man of empirical colours, whose 'whole strategy was to find purely natural causes for effects that seemed to be supernatural...leaving no room in his philosophy for faith or supernatural agency.'[65] The latter, along similar lines, thought that the Peripatetic philosophy should undergo an empirical 'reformation', and stated (in support of some of my earlier suggestions):

> I will never be satisfied with Aristotle's authority alone, I will always rely upon reason... and...imitate Aristotle in using reason.[66]

Thus, *ipso facto*, it was possible to commit to belief in Aristotelian method, and still be of a scientific bent. Furthermore, it was only contingent that Pomponazzi and Zabarella did

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[62] I allude to the Newtonian approximation, that gravitational potential energy is mgh, or wh.

[63] Boas [1962], p.25

[64] For example, Aristotle took the mathematical proof as prime exemplar of valid demonstrative argument.

[65] Copenhaver and Schmitt [1991], p.105

[66] Ibid., p.121

not happen to be highly skilled mathematicians, and it does not seem ridiculous to believe that they may have made some impressive discoveries, had they been exposed to the treatises of the later Greek mathematicians. William Harvey, another Aristotelian, certainly made some useful progress in medicine, where mathematical considerations were somewhat less important![67]

How, then, might the attack on Aristotle's methodology – which is clearly what men such as Francis Bacon intended – be thought to have been a good thing? Well, in so far as attacking a methodology might lead to serious doubts about *all* the conclusions reached by its application, it is clearly an efficacious means to encourage criticism thereof. (More efficacious, surely, than attempting a piecemeal analysis of all Aristotle's conclusions in natural philosophy, using his own methods; that was Zabarella's project.) Attack a method cogently, and the authority of its user, along with the validity of his conclusions and observation statements, becomes dubious. And a rejection of Aristotle's cosmology *was* vital for progress, especially in astronomy.

1.3 SYNOPSIS: ON AUTHORITY AND METHOD

Before moving on, let me elucidate some themes in this 'brief history' further, and try to draw some additional points from them:

Authority, and the Distinction between Metaphysics and Cosmology

Aristotle's philosophy became dangerous, or at least a barrier to progress, only when Aristotle came to be seen as an authority, whose *cosmological conclusions* were 'approximately true'; the Scholastics' fixation with the role of the central Earth, because of its apparently fruitful explanatory role in his overall cosmology, is a case in point.

Yet ironically, the dominant religions in the Latin West were sometimes a barrier to progress for similar, if not isomorphic, reasons; because their hierarchy relied on authority-based claims about what was true of the world – e.g. how it was created – and tried to stifle those who disagreed. And Aristotle's philosophical method provided a means by which not only their authority would be challenged, but also his own.

One lesson, here, is this: philosophy should not – and perhaps cannot, for any extended period of time – be made to play a handmaiden role to any other discipline. Not theology, and not contemporary science. Obsession with the *cosmological conclusions* of contemporary science might be a barrier to progress too: something with which van Fraassen, if I understand him correctly, rightly agrees.

The re-emergence of metaphysics, in our age, might therefore be seen as a just reaction to the passing post-Enlightenment illusion that philosophy had become a handmaiden to

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[67] Ibid., p.61 I might also mention the importance of the 'final cause' in biology; the relevance of this type of causation in other contexts was challenged by Francis Bacon.

science. Philosophers have done much to question the value of contemporary science, and its boundaries (in several senses, limited not just to the epistemic). And there is absolutely no reason why the contemporary metaphysician cannot agree with those such as van Fraassen that 'analytic ontology' of a Quinean form is abhorrent, and that it may be inadvisable, even dangerous, to promote the idea that all theories in contemporary science are 'approximately true'. (Here, I take the usage to mean 'highly verisimilar', not 'possess a degree of verisimilitude'.) *Au contraire*, I would claim the true 'metaphysical stance' is anti-scientistic (but not anti-science); take Lowe's view, *qua* metaphysician in the Aristotelian tradition, as a case in point:

> [T]he role of philosophy is quite as much normative as descriptive – with everything, including science, coming within its critical purview... the critical thinking that must be done cannot look to the methods and objects of empirical science for its model.[68]

So it would be right to suggest that the metaphysician is interested in the content of contemporary science. Yet likewise, she is interested in all claims about the nature of being – from theology, in philosophy of mind, and even in our daily lives. ('Metaphysics' is no easier to demarcate than 'empirical science', or 'natural philosophy', is.) But I shall say more on this subsequently, when I explain my conception of pure metaphysics, or 'First Philosophy', in the next chapter.

No Revolution in Method

The Enlightenment period was 'special' only in so far as there was an increase in mathematical sophistication, the adoption of a more critical attitude, the availability of means by which to accrue and effectively disseminate *objective* knowledge (viz. printing, secure trade routes, universities, etc.), a widespread belief in the intrinsic order of nature, and an unswerving faith in the capacities of man to understand his lot (and thus mankind to understand its). Of course, there was also an increased interest in experience as a means by which to disclose the actual: but that was a corollary. Still further, for these men to devise an experiment would have required much prior metaphysical thought.

Take the confusion between average and instantaneous velocities that was tackled by Galileo: this required him to consider how motion could be, thus how space and time could be. (*Not* how they obviously were, or are.) And it is all very well for those such as van Fraassen to try to explain how Newton was 'really' successful in spite of his metaphysical commitments – let's assume he is right, although this is dubious. Another question still remains: could Newton – not you and I, here and now – have arrived at his laws of gravitation, and motion, without having any such commitments (or just ideas), and engaging in thought about the possibilities of being?

I say not. Moreover, that many such commitments still ground contemporary science, although we have now had more time – metaphysicians and natural scientists alike – to examine them, and make *some of them* explicit. And Bohm's work in the early 1950s is a

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[68] Lowe [1998], p.5.

case *par excellence* of theory-construction inspired by metaphysical thought. In Bohm's own words:

> 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 – i.e. of an *actual movement or activity by which one physical state could pass into another*. My main difficulty was…that the wave function…could only be discussed in terms of the results of an experiment or an observation, which has to be treated as a set of *phenomena* that are ultimately not further analysable or explainable in any terms at all. So, the theory could not go beyond the phenomena or appearances… On thinking about what all this meant, it began to occur to me that the *quantum theory might actually be giving a fragmentary view of reality*. [emphasis mine][69]

Now, *inter alia*, van Fraassen professes to be a pragmatist. But can he provide a sound defence of the claim that metaphysical thought has no practical value whatsoever? Did Bohm teach us something, or absolutely nothing?[70] Is it not feasible that his interpretation of quantum mechanics could have suggested avenues of inquiry that might have proven fruitful, and which the Copenhagen interpretation did not? Is it not possible his interpretation might be a better way to *teach* quantum mechanics, in comparison to the Copenhagen hegemony? Does Bohm's model not make the notion of non-local connections explicit, and easy to grasp, in a way that of Bohr, Heisenberg, and Born, does not?[71]

I suspect van Fraassen's line of reply, or more generally the 'anti-metaphysics empiricist's' might be something like this: Bohm taught us something, but didn't really explain his motivation and thought-process very well in the passage above, and neither engaged in, nor was inspired by, thought about the nature of being. However, this would be a tenuous line to walk. For it verges on the precipice of the claim that anything which looks like metaphysical thought and is practically useful is not, in fact, metaphysical thought.

This aside, there are many philosophers who have tried to show the value of metaphysical thought in empirical science (and with whom van Fraassen, who will later emerge as one of my targets, does not seem to engage in *The Empirical Stance*). A brief list which is by no means exhaustive: Duhem, Popper, Lakatos, and Zahar. As Gillies puts it:

> The conclusion seems to be inescapable that metaphysical ideas are not only meaningful, but necessary for science. They provide an indispensable framework within which scientific theories can be constructed and compared with experience. Metaphysics acts as a guide, or heuristic, for science.[72]

Indeed, even if one is more partial to Kuhn's philosophy, as van Fraassen seems to be, one might note his later thoughts:

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[69] Bohm [1987], pp.33-34.
[70] Cushing [1994] argues that he did.
[71] Chang [1995] argues that it does.
[72] Gillies [1993], p.201.

Obviously the degree of a community's commitment varies as one goes from heuristic to metaphysical models, but *the nature of the models' cognitive function seems to be the same.*[73]

For with this concession, half the battle is won for the advocate of metaphysics. If the cognitive function of metaphysical models is isomorphic with that of putative 'heuristics' – what these are supposed to be, if they aren't metaphysical, Kuhn doesn't tell us – the only reason not to adopt a metaphysical model would be if it *really* involved commitment to belief. Point is, it doesn't. Rather, a given model may be preferred on pragmatic grounds, considerations involving internal and external consistency aside. It is still a *candidate* for being true, though, as long as it saves the phenomena; and this gets to the heart of what applied metaphysics is! A theory in natural philosophy (and for the matter, metaphysics) is put forward as *true*, literally construed, and critically examined on precisely that basis. It is not put forward as a mere *story*, to be liked or disliked, and embellished upon, or altered, according to whim.

2.   *MULTUM IN PARVO:* NAÏVE EMPIRICISM, REPRESENTATIONS, EPISTEMIC 'FOUNDATIONS', AND SUBJECTIVISM

It was clear from Descartes's reflections that the epistemic priority of ideas or appearances or perceptions over external physical objects has fatal consequences... The challenge is to reveal the incoherence of the traditional conception, and perhaps even supply an alternative we can understand, without falling once again into a form of idealism...[74]

There is an unfortunate tradition in Western philosophy, which runs from as early as Empiricus, resurfaces through Descartes, and gains a foothold in the work of the British empiricists, that there is a fundamental distinction to be made between the appearances and that which underlies them.[75] But more than this, there is a suggestion, made manifest in the work of Hume and Mill, that the foundation of our knowledge is at the level of the 'appearance'. *That at this level, one of 'being hot' and 'seeing red', for instance, no deception is possible.* Indeed, it is telling that Descartes did not consider whether he could be deceived as to that which he perceived: he was concerned, rather, about whether *what appeared to be the case* was, in fact, the case. Only hence did he invoke the possibility of the *malin genie.*[76]

Now I do not have the space to chart the development of this idea in full, although such a process would likely prove enlightening. Instead, I will provide a brief overview of how widespread it was, and is, in order to motivate my contention that the dubious distinction between *that which is sensed* and *that which is responsible for that which is sensed* – be that thing-in-itself, interaction between thing-in-itself and *a priori* internal categories, or what have you – has not been given the critical examination it deserves. This, although the distinction is a prime motivator for scepticism, and Kant's work was designed to

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[73] Kuhn [1977], p.298   –

[74] Stroud [1984] , p.255-274

[75] I deal with Empiricus in greater detail later: see III.4.

[76] Yet even this ought not to arise unless a *genuine* possibility can be arrived at, as I discuss in III.1.

address precisely such a problem, as I have already mentioned in I.2.2. To remind, I there supported Stroud's position that: 'the Kantian view would block scepticism and supplant the traditional conception, but only by giving us a 'transcendental' theory which... seems no more satisfactory than the idealism it is meant to replace.'[77] In short, Kant's move was precisely to shift the doctrine of the epistemic priority of *representations* to the transcendental level, and thereby achieve empirical realism; this, as against those such as Berkeley, who preferred transcendental realism, but coupled it with empirical idealism. There is a curious to-and-fro here, which is reminiscent of the variety of responses to another pseudo-problem: "Which came first, the chicken, or the egg?"

Now this distinction became so endemic, after the time of the British empiricists, that it wound a route through the work of Mach, the Cambridge School, the Vienna Circle, and Ayer: the presumption was that there are 'sense-impressions', or 'sense-data', which have epistemic priority, and are directly 'known' (in the sense of 'knowledge by acquaintance'). An early discussion of Russell encapsulates this popular viewpoint particularly clearly:

> Let us give the name of 'sense-data' to the things that are immediately known in sensation: such things as colours, sounds, smells, hardnesses, roughnesses, and so on...whatever else may be doubtful, some at least of our immediate experiences seem absolutely certain...[I]t is our particular thoughts and feelings that have primitive certainty...Here, therefore, we have, for what it is worth, a solid basis from which to begin our pursuit of knowledge.[78]

Furthermore, Thomson suggests that the aim of early experimental psychology was precisely 'to make the old associationist-empiricist philosophy of mind into an empirical and experimental science.'[79] And while it is true that associationism has diminished in popularity, in part because of the failures of introspectionism and behaviourism – perhaps the arguments from Popper, which I outlined in I.2.3, had some effect on this score – it remains the case that the notion of representations has underpinned most approaches to experimental psychology, save perhaps the psychometric one.[80] As a recently written psychology textbook puts it: 'Modern approaches to thinking and reasoning focus primarily on the mental representations and processes that underlie our ability to think, and the limitations on it.'[81] In other words, *mental representations* and *processes involving them* are just assumed: the contemporary debate in psychology is about *what* processes might be involved, and *what* representations might be.

However, whatever predictive success a computational approach to the mind might enjoy, the mere fact that one can *model* behaviour by employing the notion of information-processing does not decisively answer the deeper ontological and epistemic

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[77] Stroud [1984], p.274

[78] Russell [1912], pp.12-19.

[79] Thomson [1968], p.89

[80] The Gestalt school also deserve a mention, at least in so far as they held that mental contents could not be derived, or understood, by a component-by-component analysis such as that advocated by Wundt, and the introspectionist school.

[81] Garnham and Oakhill [1994], p.10

questions. Indeed, the issue of the underdetermination of theory by evidence is particularly important here, and even those who are scientific realists when it comes to mature sciences – physics, chemistry, and biology – might pause before endorsing ET with respect to such a young discipline; effectively, a child *of* empiricism. In fact, there is a deeper irony in all this, for empiricists often want to tell us that we should suspend judgement in so far as theoretical entities are concerned, but *mental representations* would seem to be theoretical entities themselves. And as such, there would seem to be inconsistency at the heart of the research programme: such empiricism can only be fleshed out, and be made fruitful, by making posits about putative entities that it tells us not to commit to belief in.

Today, judging by the number of times I have been asked "If a tree falls in a forest when no-one is around, does it make a sound?" upon telling a stranger I was a philosopher, I would say that this way of thinking is dominant in Western thought. (Agassi calls it 'sensationalism', and it is *sensational* in more than one sense.[82]) It is the most entrenched of ontological assumptions, having transcended disciplinary boundaries with remarkable ease, even though it would now seem that some philosophers of mind – say those that prefer disjunctive theories of perception – are finally giving it a more thorough examination than it has previously enjoyed. I, on the other hand, think that we should put it to the sword.

But I should add that I only mean 'put it to the sword' in so far as its putative *epistemic* consequences are concerned, because our *actual dilemma* is an intersubjective one, and the truth or falsity of this sort of distinction is itself up for grabs; as Popper puts it: '*We move, from the very start, in the field of intersubjectivity, of the give-and-take of proposals and of rational criticism.*'[83] In other words, I do not have a problem with someone working on the idea – using a metaphysical research programme involving as one of its core assumptions – that we possess *internal representations* of *external objects*. It may, after all, be true. What I object to, rather, is the curious leap from the recognition that this may be the case, or even the belief that this is the case, to some sort of retrodiction about how we came even to entertain said hypothesis. Moreover, I see no reason why the fact that we require internal representations in order to see, *if we do*, leads into the idea that all we see are internal representations. As a more concrete example, let us accept that without an image on my retina – a 'representation of something' – I cannot see, or have a so-called 'veridical perception'. Why then say that all I can *see* are such images? This seems to me to be an entirely different line of argument, and a rather dubious one if we are to take experiments such as Stratton's, with the 'inverting goggles', seriously.[84] After all, it may very well be the case that I cannot see without a brain. It does not follow that in seeing, I see my brain. Furthermore, it may well be the case that I cannot see without an object of the perceptive act; and the object perceived may be a necessary component of any perception.

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[82] See Agassi [1966]
[83] Popper [1983], p.87
[84] Stratton [1897]. See also Carr [1935], pp.18-57.

A further analogy might be useful, here. Imagine I am looking at myself in a mirror – notice that the language we naturally employ suggests, precisely, that I am looking at *myself* – and asking whether it might be the case that I am viewing a mere image, or a 'representation of my appearance'. How to answer? Well in physics, there is an important distinction to be made between a *real* image, and a *virtual* image: *but both can appear precisely the same.* Consider the following figures:
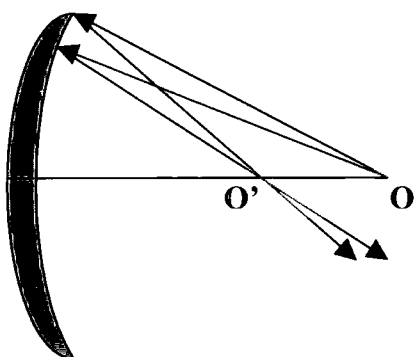
FIG. II.1 – REFLECTION OF POINT OBJECT AT A SPHERICAL SURFACE

The mirror is concave, and the image O' is *real.*
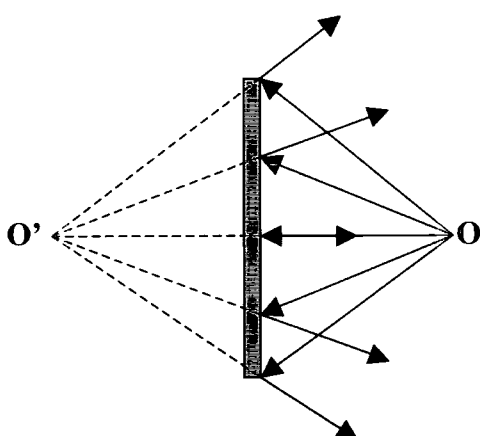
FIG. II.2 – REFLECTION OF POINT OBJECT AT A PLANE SURFACE

The mirror is flat, and the image at O' is *virtual.*

In the first case, fig. II.1, rays of light are said to converge upon the point O', and thereby form a real image (which would be seen on a screen, were it to be placed there). But in the second case, fig. II.2., the image appears to be behind the mirror: it is precisely as if it were projected onto a screen at a point behind the mirror, equidistant from the mirror's surface with the object being reflected. In other words, in the second case, what is being *represented to me* is what would be seen by an observer who was looking at me, with eyes at the same height as mine, and at the point of the virtual image, were there no intervening mirror. (This, save that it is inverted.) It is a 'representation' only in so far as I cannot step outside myself, and take a look at my body while swapping the positions of my left and right eyes; 'representation' has a modal underpinning.

Now this suggests that one may hack away at the concept of 'representation', in so far as it makes sense with respect to perception (or indeed conception, if we are to believe in such a faculty). I understand well, I would hazard a guess, what it is for a photograph to be a representation of a scene: a potential (historical) perceptual encounter. (We all

understand, of course, that it is insufficient in many respects.) But the notion that *any* given thought (or thought-content), let alone a given perceptual experience, is a representation of that which is (or may be)?

Well think of it this way: we can surely think about thought. So what then? We presumably have a representation of a representation, which would seem to suggest that we cannot understand thought as being representational, rather than representationally representational, after all. (I don't think this is a sophistic point.[85]) What's worse, the reader may now be thinking about thinking about thought, as I was when I wrote the above. And then he might think about thinking about thinking about thought, and wonder whether this suggests that an infinite regress of representations of representations is not on the cards, here. He might think about whether his thinking about thinking about thinking about thought suggests this.

The invocation of representations seems to amount to hand-waving desperation: one has some sort of idea that content emerges out of thought kind of mirroring, or sort of representing, the way that 'other things in the world' sort of, really, perhaps, are. But the mirror analogy seems to fail given its modal foundation, as I have argued above, and the invocation of this comes across to me as a smokescreen for a fundamental mystery, that would be better stated explicitly.

In fact, I wonder if there are actually such things as mind-independent representations. Go back to the example of the photograph. When we say it is a representation, do we not mean that *it represents something to us*, rather than that *it has the property of being a representation*? But then, no representation without perception, and indeed the perceiver! If this line is accepted, then the claim that there are mental representations seems to become almost Pickwickian; to be a colourful, but misguided, bit of tropology. Why say that thought (or perception) represents, rather than *presents*? (Remember now the mirror analogy.) How else, after all, is anything then to be *presented*? And what would it mean to represent that which cannot be presented? I am afraid that the answer seems to be nothing whatsoever, as far as I can fathom; it simply makes no sense.[86] If this is just a failing on my part, then so be it: I would prefer to say that I have no idea what is being suggested here, rather than feign insight on the basis of crude figures-of-speech.

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[85] Richard de-Blacquiere Clarkson has suggested to me that this is a similar point to one made by Wittgenstein, as part of his private language argument. He goes on to suggest [personal correspondence]: 'the problem is that many representational theorists subscribe to this metarepresentational view explicitly in combination with a frequently implicit constitutive analysis of belief: subpersonal (representational) beliefs are partly constituted by the existence of a personal (metarepresentational) belief.' My general objection to this view, which is championed by those such as Perner, Currie, and Leslie, is provided by my comments about presentation versus representation, below.

[86] I do see that it is possible to represent something to another that is presented (or has been presented) to one's self, but cannot be presented to the other, say via a picture drawn from memory, of a long dead relative. Further, I understand how one might try to represent colours to a person born blind, by reference to different tones (or frequencies of sound, rather than frequencies of light). But none of this will do, for in these cases there is one who has the initial *presentation*.

Swings and Roundabouts: On Empiricism, Physicalism and Subjectivism

But there is, of course, a different route for the seeker of 'solid foundations' for 'knowledge'. To reject the notion of sense-data, at least ostensibly, and instead go for the primacy of observables, construed as physical (viz. material) objects. And although I think the distinction between 'observable' and 'unobservable' is quite misguided – I come to discuss this later, when I tackle van Fraassen's view of observation, in III.4 – let us accept, for the moment, that it is sound. What motivates the account?

Well unfortunately, I take it that the fundamental point – the underlying starting point between physicalism and sense-data empiricism, and the unspoken assumption – is that each of us is alone in the world, having come into being as a *tabula rasa* and been suddenly confronted by experiences, understood as something like 'raw feels', which we subsequently attempt to make some sort of sense of. Yet although I would not say this is conclusively wrong – it is, though, wrong – it seems to me that it cannot just be presupposed that it is correct. In other words, I think it entirely mistaken to adopt subjectivism as a *dogma* – but not as an explicitly stated research programme, embarked upon tentatively – when there are other approaches, most notably intersubjective ones, which seem to fit the very nature of inquiry far better. Indeed, surely it is the case that there is an argument to be had against physicalism, as I shall show, and hence our inquiry should not presuppose physicalism. For that matter, I hold it should not presuppose anything else, other than that we might get to the truth by stating our problems clearly, proposing solutions, and pouring the strongest criticism we can onto those tentative solutions, to see if they hold up. But I explain this in II.3 and II.4, below.

So let me move on to offer some direct criticisms of physicalism, since the antecedent metaphilosophical point may seem, taken alone, to be too cheap. And let me choose Carnap as my point of engagement, since he came to recognise the inadequacies of the idea of 'sense-data', and adopted precisely a physicalist line, in order to shore up the positivist programme.[87] He rarely discussed this in a great deal of detail, but the most pertinent and clear passage I have been able to uncover comes in a reply of his to a piece by Feigl, and starts as follows:

> At the present time I prefer not to emphasize the requirement of intersubjective confirmability as much as we used to previously, but rather to consider it to be of secondary importance. I regard as meaningful for me whatever I can, in principle, confirm subjectively. This statement may be taken as a rough formulation of the *principle of empiricism*.[88]

Now this makes it clear that the subject, the individual, is at the very core of Carnap's philosophy – in this period, at least – and, he agrees with me, empiricism. As such, 'confirmability' as a thoroughly intersubjective notion, founded in the ability of an idea

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[87] It is worth noting that the move from verificationism on the basis of sense-impressions, to probabilism on the basis of physical objects, would seem to illustrate a degenerating research programme if ever there were one! But much credit is due to Carnap, for his willingness to be responsive to criticism.

[88] Carnap [1963], p.882.

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(or hypothesis) to stand up to criticism, is already out. (We are lonely little selves, in a fearsome battle with 'the world', struggling for subjective confirmation.) But still further, Carnap has an empirical notion of 'confirmability' at work, here. One option would be to go for the epistemic priority of representations already discussed above, as did many of the Vienna Circle, but he takes another. Thus, he continues:

> Now we come to *physicalism*. In this world, I find the following features which are empirical but, unlike single facts, belong to what is sometimes called "the all-pervading fundamental features" of the world and of the language in which the world can be described:
>
> (1) There are beings similar to myself with whom I am able to communicate by language.
>
> (2) I find myself able to give to others a signal indicating any kind of experience which I have; or, in case I should be unable to give an intentional signal, others could, under suitable circumstances, infer my state from observable symptoms.
>
> (3) Therefore, everything I know, including what I know by introspection, is in principle confirmable by others on the basis of their observations.
>
> (4) Therefore it is possible, and convenient for practical purposes, to begin the construction of the language with primitive predicates designating properties of things that are intersubjectively observable (e.g. "red, "hot").
>
> Predicates designating properties only subjectively observable, though intersubjectively confirmable (as e.g. "angry", "having a toothache") may be introduced derivatively. It should be emphasized that the difference between these two kinds of thing properties is not a matter of principle, but merely of degree.[89]

However, I take this argument to be enthymematic to a vitiating degree, even under the most favourable interpretation. Most vitally, the talk 'all-pervading fundamental features' of the world which are all *empirical* strikes me as being curious, given the invocation of knowledge and introspection in (3). In other words, the invocation of 'all-pervading fundamental features' seems to be a fig-leaf for his particular (materialist) metaphysical outlook; the very same outlook that he might not even have realised he had, and could very well have been ruled out as 'meaningless' according to his own principle of empiricism. I should add that it would not have done for him to have suggested "I only believe (1)-(4) because science says so", for reasons already stated in I.2.5, although I doubt whether it would even have occurred to him, *qua* genuine philosopher, to do so.

To his specific formulation, though. (1) I agree with, but the question is whether his principle of empiricism has the resources to deal with it, in so far as making it 'subjectively confirmable'. I suspect not, since 'beings similar to myself' are hardly confirmable *by* – although they are surely *consistent with* – 'observations' in Carnap's sense, by which I think he means perceptions (or *sensory experiences*) alone. At least, not if perception is to be based on 'raw feels', as he suggests with his mentions of 'red'

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[89] Carnap [1963], pp.882-883.

and 'hot' in (4).[90]

(2) is dubious, and seems insufficient for Carnap's purposes. First, for I am not sure how one is supposed to 'give a signal' that one *is* having a (visually based) dream, or how one's state could be 'inferred from observable symptoms' of the sort that he seems to have in mind. Even if a subject is wired up to a machine monitoring 'brain waves', the only means by which it can be properly tested whether a particular pattern (or cluster of activity) corresponds to a dream-state is to wake the subject and ask "Were you just dreaming?"; any future 'inference to the subject's state', with respect to similar tests, is *parasitic* on this sort of desideratum. Second, even if it is right that one can always give to others a signal indicating any kind of experience which one has, it does not actually follow that this is the *basis* of our ability to communicate. Taken alone, it would be insufficient to allow communication at all, in so far as this is an *interactive* process.

(3) does not follow from (1) and (2). 'Know' has suddenly entered, not too surreptitiously, yet no mention of 'knowing' has been made in (1) or (2). But how would one 'give a signal' – or how is it to be inferred from one's 'observable symptoms' – that one *knows* a given proposition, p? Or that one has a true belief, or a justified belief, or a justified true belief, in p?[91] In extreme, is there a brain state for every true proposition such that when one believes in said proposition, its truth is evident *from that specific state*? This is extremely implausible, and highlights how Carnap's use of 'know' conceals a shift in what is being discussed. In place of (3) should be something like this:

(3*)   Therefore, everything I experience, including what I experience by introspection, is in principle confirmable by others on the basis of their observations [which are their experiences?].

Yet even this is dubious without strong modal force being given to 'in principle'. How a modern person could get anywhere near confirming 'on the basis of their observations' that the Roman Empire existed would seem to be a complete mystery. Indeed, even that someone else *once* experienced something they called 'The Roman Empire' (or just a part of something called that) would not seem to be a potential item of contemporary knowledge under this view.

In (4), we have a mention of 'the language'. What Carnap means is something like *the* object language, but without a metaphysically robust account of propositions – and their relation to sentences, utterances, and so forth – his view would seem unmotivated. Besides, could this language really be devoid of reference to non-experiences (which is all that is *actually* consistent with what he has said in the foregoing quotation), or devoid of reference to non-physical objects (which is what he *really* wanted to argue for, as far as I can tell)? The project to produce such would seem to be a fool's errand, but far be it from me to disallow others from pursuing it. The challenge: show me the language. (And until you can show me the language, do not say you can show me 'inductive logic', please.) Show me a language – a useable language – without terms that refer to relations,

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[90] If perception is concept-dependent, then this problem disappears. But then, so does physicalism.

[91] I prefer a distinction between 'true' and 'reasoned', as I suggest in II.3, below.

properties, universals, abstracta, numbers, geometrical figures, and so forth. And show me a *tenseless* language, too: for time is not a physical thing, nor indeed an 'experience' *qua* raw feel.

In all, then, I opine that Carnap did not come to terms with his deeply held metaphysical views, because he never came to the recognition that natural science has limits as a mode of inquiry, and is parasitic on distinctive disciplines, such as mathematics and ontology. He never saw that natural scientists cannot be 'natural scientists' in the sense that he would have had them be, since they are men and women of passion: lovers, murderers, dreamers, and philosophers.

Still, it may be asked whether I do not have my own dogmas. To this, I should say that I am strongly convinced about a particular view of the world, and our place in it (or rather the pragmatic and epistemic value in adopting such a view, *ceteris paribus*): truth is absolute (AT), language can successfully map onto aspects of the world (ST), some of the distinctions we employ are not just conventions (a version of MT), we have a chance to find truths, and it is rational to hope for 'the best'. Yet while I here defend some ideas I have (or others have had) – I am giving them the best run I can, to test them out – I am also prepared to commit this thesis to the flames. This is the spirit I advocate, and *this*, I will not willingly give up. But I take it to be precisely an *anti-dogmatic* spirit, although it might be suggested that to adopt it come what may is, at some level or another, a kind of *dogma*; a matter of blind faith (from an immanent perspective, at least). I try to explain why I do not think this is the case in II.4, but to give an early flavour of where I am leading:

> We can assume or be *convinced* of the truth of something without being *committed* to its truth...[A] pancritical rationalist, like other people, holds countless unexamined presuppositions and assumptions, many of which may be false. His rationality consists in his willingness to submit these to critical consideration when he discovers them or when they are pointed out to him.[92]

To sum up, it might be suggested that our metaphilosophy should be the same as our meta-metaphilosophy, in order to prevent an infinite regress of strategies – in particular, appeal to foundational authorities such as 'sense experience', 'intuition', or 'common sense' – for dealing with disputes. And while such a statement may seem cryptic, or even gnomic, I shall endeavour first to motivate it, and then to explicate it, in the remainder of this chapter. Here, I have only provided a sketch of my disenchantment with particular programmes, and their overarching approach. I have said what realism is not, in so far as it does not assume any 'ultimate barriers' between us and other components of the world: that we see or conceive of things in a 'skewed', or 'represented' fashion. And I take this to tell us pretty nearly what realism, *qua* understanding of the τέλος of inquiry, involves. The explicans is epistemic, rather than metaphysical, just because it would be a *petitio principii* to rely on one's *take* on metaphysics – but not on metaphysical arguments – in order to support it. In short, one ought not to freely help oneself to the resources that are provided by-a-realist-understanding-of-metaphysics in-order to somehow-*justify*-that same

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[92] Bartley [1984], pp.121

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outlook. Nor indeed, to attempt to immunise that outlook from criticism, since its mere self-consistency is not sufficient for preferring it with respect to absolute truth. Rather, what *is* a reason for preferring it – the one I have argued for here – is its optimism from an epistemic perspective: its value *qua* regulative ideal in order to motivate inquiry, when coupled with AT. Popper expresses this succinctly: 'Rational discussion, that is, critical argument in the interest of getting nearer to the truth, would be pointless without an objective reality, a world which we make it our task to discover: unknown, or largely unknown: a challenge to our intellectual ingenuity, courage, and integrity.'[93]

3. THE ABANDONMENT OF KNOWLEDGE

I can well conceive a man without hands, feet, and head (for it is only experience that teaches us that the head is more necessary than the feet). But I cannot conceive man without thought... Man is but a reed, the most feeble thing in nature; but he is a thinking reed... All our dignity, then, consists in thought. By it we must elevate ourselves, and not by space and time which we cannot fill. Let us endeavour, then, to think well; *this is the principle of morality.* – Pascal[94]

Despite my antecedent mentions of 'knowledge' – and previous acceptance of its standard philosophical definition, namely 'justified true belief'[95], in order that I might argue concisely against justificationism – I think it behoves us not to attach any genuine philosophical significance to the term. So here, I shall attempt to show why an essentialist approach towards 'knowledge' is liable to lead philosophical debate, and more generally inquiry, astray.[96] In part, this may be thought of as an illustration that it not merely unwise, but also futile, to undertake epistemology without a carefully considered ontology.[97] However, more fundamentally this discussion is important, in context, because of my views on (so-called) ampliative inferences, and on explanation generally. In particular, my rejection of abduction and induction has epistemological (or rather *anti*-epistemological) consequences, which I wish to argue for.

But before I begin this process, it is important to make it clear exactly what I am inveighing against when I refer to 'epistemology', or 'epistemologists', in what follows; for it is indubitably the case that there are those who call themselves 'epistemologists' and do not undertake to study *knowledge*, but rather *inquiry*. And while I have few problems with the latter process, an initial exemplar of what I oppose is the following view:

> The theory of knowledge... inquires into the *nature* of knowledge and the *justification of belief*....if philosophy is the quest for truth and wisdom, then we need to know how we are to obtain the truth and *justify our beliefs*... Epistemology is primary interested in...

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[93] Popper [1983], p.81

[94] Emphasis mine. Pascal [1660], part VI, 339-347.

[95] Gettier [1963].

[96] Note, however, that I argue primarily against the idea that knowledge is *analysable*, should there be such a thing.

[97] To lay my cards on the table again, and hark back to the introduction to this chapter: I hold that epistemology is a sub-domain of metaphysics.

propositional knowledge... The field of epistemology seeks to throw light on the following kinds of questions:

1. What is knowledge? That is, *what are the essential characteristics of this concept?*

2. Can we know anything at all? Or are we doomed to ignorance about the most important subjects in life?

3. How do we obtain knowledge? *Through the use of our senses, or our intellect, or both?*[98]

Now to make my position clear, I disagree that the first question is important at all, in so far as it assumes there are essential characteristics of 'knowledge'. I disagree that the second is pressing, in so far as it might be rational to hope that we can acquire true beliefs, and strive to do so, irrespective of whether we are 'doomed to ignorance' or not. And I disagree with the presuppositions underlying the third question, in so far as it implies that context of discovery is important; that there are some sort of solid foundations for 'knowledge' (or more properly, acquisition of true belief, true statements in a given language, or so forth).

If any more evidence is required to suggest that this is really how 'epistemology' is sold, then let us look to a contemporary textbook:

Epistemology is the study of knowledge and justified belief. To engage in this study is to seek answers to the following questions:

Q1   What is knowledge?
Q2   What do we know?
Q3   What is it for a belief to be justified?
Q4   Which of our beliefs are justified?[99]

Since such views seem prevalent – indeed, it is somewhat ironic that these self-styled 'epistemologists' take it that there is something *essential* to 'epistemology', for which, see my discussion in II.4 – I will make no further apology for my use of the term below. But it is interesting to note, nonetheless, that the individual who coined the term, namely Ferrier, did not mention justification at all. And still further, he held that *agnoiology*, the study of ignorance, was equally as revealing, and was wont to defend the idea that 'All ignorance is *possibly* remediable'; that to speak of 'ignorance of p' would have no sense, were it the case that p could not be 'known'. In his words: 'No kind of knowledge is absolutely inconsistent with the nature of all intelligence. But unless all ignorance were *possibly* remediable, some kind of knowledge would be inconsistent with the nature of all

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[98] Emphasis mine, added to pick out particular presumptions. Pojman (ed.) [1999], pp.1-2. I should also add thanks to Tony Booth, for some strong (and deserved) criticism about my unfair treatment of 'epistemologists', in earlier versions of this section.
[99] Steup [1996], p.1

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intelligence, to wit the knowledge by which the ignorance in question might be remedied.'[100]

Against Essentialism with respect to 'Knowledge' – A Nominalistic Approach

In natural language, in our daily lives, we do, indeed, employ the verb 'to know', the noun 'knowledge', the adjectives 'knowledgeable' and 'knowing', and the adverb 'knowingly'.[101] But let us now consider the senses in which it is permissible to use each of these forms in various contexts, in order that we might examine what, if anything, they genuinely have in common.

The verb 'to know' has three main uses. First, it can be employed in a sentence that has as its object a noun, or clause, which refers directly to a non-propositional entity, or group of entities. Its purpose, in such cases, is to indicate that the subject of the sentence is *acquainted with* said entity, or entities.[102] Simple examples are: 'I know Jonathan Lowe'; 'Jonathan Lowe knows the lecturers in Durham's philosophy department'; 'We know a blue car'; and 'The dog knows where its bone is'. Of course, understanding the last two examples in such a fashion is not entirely straightforward. 'We know a blue car' is ambiguous, because it might mean that 'we' – taken as the given group, determined by context, to which such a deictic, or indexical, term refers – are each familiar with a blue car, but not *the same* blue car. And to rephrase the last example, which might seem to be radically different from the rest at first sight, 'The dog is acquainted with the location of its bone'. That is to say, in metaphysical terms, the dog is acquainted with a particular state-of-affairs, for example, such that its bone is buried under the tree in the garden. It is important to recognise that said dog need not have the concepts of 'bone', or 'tree', or 'garden', in order to have such acquaintance; similarly, I may be acquainted with the most talented philosopher in Durham, and it would not seem unreasonable to suggest that I am, without realising *that* she is the most talented philosopher in Durham.

Second, 'to know' can be used in order to denote competence, or the possession of a skill, or ability, on the part of a subject; in such cases, the object of the sentence is an activity. Most typically, such uses involve 'how' clauses, and indeed we speak of individuals having 'know-how'. Examples are: 'I know how to ride a bicycle'; 'Karl Popper knew how to play the piano'; 'We know how to walk'; and 'Darrell knows French'. In the final case, we might say, more precisely, that 'Darrell knows how to speak French', and thus are not implying a mere acquaintance with French (in the sense that one could be aware of the existence of French, and recognise it when one heard it being spoken, without understanding a single word of the language). And note, further, that one can 'know

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[100] Ferrier [1854], pp.402
[101] One may also talk of 'knowingness', but I leave this ugly construction aside, since it is fundamentally derived from the adjective 'knowing'.
[102] Richard de-Blacquiere Clarkson has suggested to me that 'the first use is more or less dispositional – certain entities under propositional description interact in characteristic ways independent of those descriptions'. [Personal Correspondence] Indeed, this is pretty close to what I want to suggest here: for instance, an animal may have the ability to recognise a particular type of entity without having a concept of their being such an entity. See the example of the dog, below.

French' without being an expert in French – we often say that one knows French *without knowing it well*. (Interestingly, the same is true when we speak of 'to know' in its first sense, as 'to be acquainted'; one can be well acquainted with a person, or a place.) This is the sense in which I hold that one can know philosophy; it is not a proposition or a body of propositions – although some unfortunately treat it as such – but a way of thinking to which one may, or may not, be predisposed.[103]

Third, and finally, the verb 'to know' may be employed with a sentence, or proposition, as its object. And this is all-too-often the philosopher's sense of 'know'; typically such usage involves a 'that' clause, which is taken to be the sentence, or disclose the proposition, which is 'known' by the subject. Some examples: 'I know that Paris is the capital of France'; 'Karl Popper knew that it was a moral imperative to refrain from threatening visiting lecturers with pokers'; 'We know that this sentence is here in order to serve as an exemplar'; and 'Eudoxus knew [that] the Sun revolves around the Earth'. Again, I suspect the final example will seem somewhat curious, from a modern perspective. The Sun does not revolve around the Earth, so how could Eudoxus have *known that* the Sun revolves around the Earth? Well let us imagine being contemporaries of Eudoxus, and entering into a discussion of the finer points of his system of astronomy. It is quite plausible that one of us might say, perhaps after making a critical point, 'Well, at least he *knows that* the Sun revolves around the Earth'. But would we be mistaken to use the word 'know' in such a context? This is a core puzzle, to which I shall return again in what follows. Yet we should note, even at this early stage, that we might say, instead of the above, 'At least he *holds the belief that* the Sun revolves around the Earth, as we do'. And this is an important recognition, because it emerges that 'to know that p' is *not* obviously to have a propositional attitude toward p. That is to say, 'knowing p' does not seem to be akin to 'fearing p', 'wishing p', or indeed 'believing p'. Yet most philosophers would accept that one should 'believe p' in order that one should 'know p': this, without necessarily implying any relation of supervenience between 'known' and 'believed', or indeed that 'knowledge' even exists.[104]

I expect the foregoing classification to seem plausible to the reader, on the assumption that she is prejudiced by contemporary positions in analytic philosophy. But even this much is not manifestly clear. Take, for example, Darth Vader's infamous utterance to Luke Skywalker in *The Empire Strikes Back*: 'Search your feelings, you *know* it [that I am your father] to be true'. This does not seem to be an inappropriate use of language, in context, even though Luke clearly does not believe (at that time) that Vader is his father. And Vader's suggestion might be precisely that Luke *knows* this (as well as fears this), but does not believe it, so 'to know p' would, then, be to hold a distinct propositional attitude toward p after all. I am inclined to say that we should take this idea seriously, but would point out that the analysis of 'to know' would then enter into the realm of psychology, and perhaps phenomenology – this is not my project. (Perhaps one could talk of beliefs and repressed beliefs, or appeal to the Freudian notion of the unconscious?)

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[103] The *goal* of the activity may well be to separate true propositions from false propositions. See my discussion below, in II.4.

[104] I return to discuss whether 'to know p' can be to hold a propositional attitude toward p when I discuss Williamson's work, towards the end of this section.

This aside, it remains unclear that 'to know' in the sense suggested by Vader would have consequences for considered action. For it is what we believe – even in the sense of Sartre's 'bad faith', if we admit such a notion – that seems to determine how we behave, and what we expect (or would predict).[105] After all, Sartre's point would seem to be precisely that we should 'believe' what we 'Vader-know'. (And we might need to make room for self-deception, in epistemology.)

So far, then, it has been suggested that this simple word root has a multiplicity of senses, none of which is obviously more basic (or important) than the others, and most of which are expressible in other terms, such as 'belief' and 'acquaintance', which might be candidates for more fundamental entities, such as 'belief states', and 'acquaintance states'.[106] (I am not presuming, or even suggesting, that there *are* such states. The point is that under a state-model, 'knowledge' seems defunct.) Note further that while 'p but I don't believe that p' seems paradoxical (when p is asserted) – indeed, this is Moore's paradox – the same does not seem to be translucently true of the following: 'p but I don't know that p'; 'I know that p [think Vader-know] but I don't believe that p'; and 'I believe that p but I don't know that p'. Thus, it seems that what we are willing to assert, and to act upon, are our beliefs. But in spite of this, let us continue this ordinary language investigation – while remaining cognisant that the foregoing senses of 'to know' are not straightforwardly exhaustive[107] – in order to see whether anything else of significance can be gleaned.

The noun 'knowledge' has two fundamental senses. First, we may speak of the knowledge of, or possessed by, an individual or group (such as a community), in a manner suggested by sentences such as: 'I have knowledge of quantum mechanics'; 'The Ancient Greeks had knowledge of astronomy'; 'Doctors have knowledge of medicine'; and 'Witchdoctors have knowledge of magical potions'. The first three examples seem reasonably uncontroversial, but the fourth might seem dubious, just because there are not – at least, according to orthodox Western science – such things as 'magical potions'. However, this intuition is not, we might think upon further reflection, manifestly correct. For surely it is the case that one can 'have knowledge' of the creatures of Greek Mythology: the Cyclops, the Pegasus, and so forth. Similarly, one can 'have knowledge' of logic, without there being such a thing as *logic itself*. Caution is, therefore, advisable in the evaluation of such claims, particularly – although not peculiarly – in respect to natural philosophy. Indeed, it would be strange to say that Sir Isaac Newton did not have knowledge of mechanics, or that he only had knowledge of mechanics *in so far as* he understood a particular fictional theory that he constructed, because classical mechanics is, strictly speaking, false. (An analogous claim may not seem unreasonable in the case of the Witchdoctor, it is agreed. But we need to carefully consider the causal factors

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[105] One might, for example, 'know' that one is homosexual, and *be* homosexual, but 'believe' that one is not homosexual.

[106] In the 'know-how' case, we might also speak of proprioceptive acumen, hand-eye coordination, and so forth, say in the example of being a skilled tennis player. None of this seems obviously reducible to propositional attitudes, or acquaintance states, though.

[107] For example, the OED suggests 'Distinguish; be able to distinguish (one thing) *from* another'. In this sense one may 'know right from wrong', but I would be inclined to place this under the notion of competence.

responsible for what he would call his knowledge. We should also be willing to entertain the notion that many of the things that the Witchdoctor believes in virtue of his professional training may be true, or verisimilar.[108] Remedies of traditional African medicine should not be dismissed merely upon a whim.) Here, then, comes a suggestion that what constitutes knowledge need not be true, or true literally construed. In the example involving Newton, we notice that what counts as 'knowledge' is just *reasoned*, and *based on experience* (it is impossible to tell *conclusively* whether it is truth-like, or how truth-like it is); this is what seems to characterise the use of the word, in such a context.

But what of the second sense of 'knowledge'? Well, we may speak of 'the knowledge in the library', or 'the knowledge contained in Bacon's *Novum Organum*'. Again, *prima facie*, such usage could seem perverse. But consider a different claim: 'If you seek knowledge, then go to the library'. Now it may be argued, reasonably enough, that such a conditional expresses the idea that in order for *you* to attain knowledge, you must undertake an activity, namely reading, and it is only the tools that enable said activity which are to be found in the library. But why not call the information that really is in those books – the many arguments rehearsed there, and assertions made therein – 'knowledge', as Popper suggested? We can say 'It is known how to make bread', without supposing that any individual in fact has the skills necessary to make bread. Or even more persuasively, we can say that π is known to at least 1.2411 trillion decimal places, without implying that any one person can, or could ever, recite the value of π to that number of places.[109] So this 'objective knowledge', as Popper would have it called, is neither a vague, nor mysterious, posit. We might call it 'information which is the product of human activity' if we like, and prefer the tag 'intersubjective' to 'objective', but should hardly say that such is not worthy of the name 'knowledge' because *that* can only be – for want of a less vulgar phrase – in our heads. (Few metaphysicians suggest that propositions are 'in our heads', if they believe in said entities, for that matter.)

Interestingly, the evaluation criteria for this very PhD thesis involve two mentions of 'knowledge'. First, 'candidates are required to show ability to...understand the relationship of the theme of their investigations to a wider field of knowledge'. I will confess that I have no idea whatever why this should be important. (Does the university not want the candidate to understand the aforementioned relationship, and show understanding of the same, rather than merely *show ability* to understand it?) In any event, it would make better sense to speak of a field of reasoned beliefs, (putatively) true beliefs, reasoned propositions, or (putatively) true propositions. Second, 'the thesis...should include an original contribution to knowledge'. But I should say that what is intended here is not 'justified true belief', and that this will be clear to the

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[108] There is an allusion, here, to something like, but weaker than, Feyerabend's 'meaning variance', which bears some similarity, in turn to Kuhn's 'incommensurability'. Feyerabend suggests: 'observations are not just theory-*laden* (the views of Toulmin, Hanson and apparently also Kuhn) but *fully theoretical* and the distinction between observation statements... and theoretical statements is a pragmatic distinction, not a semantic distinction; there are no special "observational meanings".' Feyerabend [1975], pp. 211-212.

[109] I do not mean to suggest that objective knowledge is *entirely* independent of human capability; in this example, pi has actually been calculated to that number of places, by a computer *which was programmed and designed* by humans.

examiners, *inter alios*. Else, my original contribution to knowledge could be this: 'It is not the case both that I am a Cadbury's chocolate bar, and that I am not a Cadbury's chocolate bar'. For this statement is not found in any of the literature, and is 'certainly true' by the guns of those such as Hume and Carnap, be it understood as a necessary relation between ideas, or as a preclusion relation between propositions.

At this point, it seems almost unnecessary to point out that an individual may be fairly described as 'knowledgeable' without there being any implication that said individual has a large set of true beliefs. Rather, her beliefs might have been acquired via a process of careful reasoning (and observation); she might simply have *done her best* (and might be continuing to do her best) to separate the true from the false. Indeed, she might even have (or at least, strive to have) the *skill* to do so; that is, possess the 'know-how' of philosophy. And this returns us to the astute quotation that heads this section. What is really being discussed here is wisdom, in the Socratic sense: 'rational decision-making...not rational-decision making'.[110] Indeed, even Ferrier, who coined the term 'epistemology', recognised that: 'a system which is reasoned, but not true, has always some value. It creates reason by exercising it. It is employing the proper means to reach truth, although it may fail to reach it.'[111]

'Knowledge', then, does nothing.[112] That is, beyond being a convenient word. Rather, there are quite distinct properties of true and reasoned (viz. reached by the application of critical thought), both of which can apply to beliefs, objects of belief (i.e. propositions), and that which discloses objects of belief (e.g. a copy of Kant's *Critique of Pure Reason*).[113] What else should we need to discuss, save experience and categories, in examining the foundations of inquiry? Our quest is for the truth, and we employ reason – but not good reasons! – in this quest. And we seek the truth because it is sufficient to see that we are never led astray, in deed or word. Indeed, it seems as if Plato was painfully close to this fundamental recognition, as the following passage in his *Meno* shows:[114]

Socrates:	But that one cannot guide correctly if one does not have knowledge; to this our agreement is likely to be incorrect.
Meno:	How do you mean?
Socrates:	I will tell you. A man who knew the way to Larissa, or anywhere else you like, and went there and guided others would surely lead them well and correctly?
Meno:	Certainly.

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[110] Miller [1994], p.43.

[111] Ferrier [1854], p.3.

[112] Note well that I do not add the qualification 'As far as practice is concerned', which Miller appears to want to. (I suspect this may simply be a matter of context.) Miller [1994], p.66.

[113] A *caveat*: I do not literally mean that 'reasoned' is a property of *propositions*, although 'true' may very well be. It is, rather, a property of specific sentence *tokens*, or utterances.

[114] Plato, *Meno*, 97a-d.

Socrates:	What if someone had had a correct opinion as to which was the way but had not gone there nor indeed had knowledge of it, would he not also lead correctly?
Meno:	Certainly.
Socrates:	And as long as he has the right opinion about that of which the other has knowledge, he will not be a worse guide than the one who knows, as he has a true opinion, though not knowledge.
Meno:	In no way worse.
Socrates:	So true opinion is in no way a worse guide to correct action than knowledge. It is this that we omitted in our investigation of the nature of virtue, when we said that only knowledge can lead to correct action, for true opinion can do so also.
Meno:	So it seems.
Socrates:	So correct opinion is no less useful than knowledge?
Meno:	Yes, to this extent, Socrates. But the man who has knowledge will always succeed, whereas he who has true opinion will only succeed at times.
Socrates:	How do you mean? Will he who has the right opinion not always succeed, as long as his opinion is right?
Meno:	That appears to be so of necessity, and it makes me wonder, Socrates, this being the case, why knowledge is prized far more highly than right opinion, and why they are different.

Plato then has Socrates suggest that true opinions are not liable to linger, but instead to leave one, unless one 'ties them down by an account of the reason why'. However, how precisely we should understand this point is not clear, since it is bound up with the earlier discussion of recollection. It seems as if Socrates is pointing to the value of explanation, and claiming that it is by providing an explanation for a true belief that one cements it in mind. (As such, explanation facilitates proper recollection, and that which is properly recollected constitutes knowledge.) But against this, I should say that an explanation can serve to cement any belief, true or not, although I would gladly concede that a fleeting opinion is liable to be quickly disregarded if it does not seem, upon reflection, to be explicable (and thus defensible). Besides, Socrates continues by suggesting that it is wisdom, rather than knowledge, that is 'the guide in public affairs'; that virtue is a gift bestowed by the gods, rather than aught else. And might this not also be the case, talk of gods aside, with respect to *the ability to acquire true belief* in general? That one either has it, or one does not?

Going by the *Phaedo*, which is usually taken to have been written after the *Meno*, this understanding seems sustainable, since Plato has Socrates say 'I know that arguments of

which the proof is based on probability are pretentious and, if one does not guard against them, they certainly deceive one, in geometry and everything else.'[115], and 'No sensible man would insist that these things [on the soul] are as I have described them, but I think it is fitting for a man to risk the belief – for the risk is a noble one...'[116] This draws out two important strands of thought. First, the dominance of a belief in a community, or its widespread acceptance, should not suffice alone for one's own acceptance, even when said belief is seemingly trivial. (But *nota bene*: I think it unwise to impute the modern sense of 'probable' to the text. It is better read in the archaic sense of 'probable doctor', viz. 'respected' or 'approved'.[117] Hence, Plato's Socrates inveighs against undue respect for, or the employment of argument from, authority.) Second, it is quite permissible to 'go out on a limb' by adopting a belief which is neither proven, nor indeed provable. Thus, by extension, it may not be wrong to 'go out on a limb' with a belief that is unsupported. For example, although we may not have conclusive reasons (or even *good reasons*) for saying that the application of critical thought is, in fact, sufficient for discovering the truth, we may accept that it is; this, as a strategic device, and a rational hope. Miller, advocating the critical rationalist perspective with which I have some sympathy, puts it so:

> [S]cience is a collection of statements... the business of science is the discovery, as far as is practicable, of the truth values (and perhaps 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.[118]

Now what I have said here is, of course, extremely controversial. However, in order to defend it, and flesh out my line, I should like to examine a recent account of 'knowledge', namely that of Williamson: in doing so, I will make some concessions that should serve to clarify my position, and serve to highlight the relationship between externalism and critical rationalism, prior to a further discussion on the issue of demarcation. Beforehand, however, a few words about general issues in contemporary epistemology would seem to be in order.

Theories of Justification

The foregoing discussion is liable to irritate the professional epistemologist. I have not mentioned foundationalism, coherentism, foundherentism, contextualism, or the like. I have neither mentioned the distinction between externalist and internalist accounts, nor naturalistic epistemology.[119] And thus, it would be easy to presume that I am completely unaware of these distinctions, and inveighing against an important area of inquiry of which I have no *knowledge* (in the epistemologist's sense). But I should like to

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[115] Plato, *Phaedo*, 92d.

[116] Plato, *Phaedo*, 114d.

[117] See the useful discussion in Hacking [1975], ch.3

[118] Miller [1994], pp.11-12.

[119] However, my earlier critique of naturalism, in I.2.5, should make it clear that I would want to dismiss the latter.

emphasise, against the appearances, that I believe most of these discussions are rather pointless (although not without intrinsic interest), because they serve to polarise the examination of belief formation in an entirely inappropriate fashion. And what's more, I'll hold that belief forth as true, but *utterly unjustified* in the senses of 'verified by my evidence', 'probabilified by my evidence', 'confirmed by my evidence', 'partially verified (?) by my evidence', 'entailed by my evidence', 'partially entailed (?) by my evidence', 'necessitated by my evidence', or 'partially necessitated (?) by my evidence'. Instead, I shall say that I have made my decision on the basis of (what I understand to be the relevant) experience, and with hard thought. I shall say that I have little else to give, and find myself incapable of clinging onto the cuddly toy of 'justification', even though to do so would enable me to sleep more restfully. (Dogmatic slumbers may be blissful, after all.)

As a case in point, take the debate between the externalist and the internalist. The former might say that 'justification', or more properly *warrant*, is provided by a reliable process; for example, for one to 'know' that one is seeing one's hand, it is necessary not only that the belief that one is seeing one's hand is true, but also that said belief has arisen via a process suited to 'reliably', or trustworthily, producing *true* beliefs (in this example, in the relevant domain, viz. with respect to perception).[120] (Another externalist option, that I put aside here, is to adopt a causal theory of justification.) The latter, on the other hand, holds that there is an internal component to knowledge other than, and in addition to, belief. Steup's rough characterisation is as follows:

> Our epistemic duty is to believe in accord with our evidence: believe p only if p is supported by our evidence, disbelieve p only if p is contradicted by our evidence, and suspend judgement about p if our evidence neither supports nor contradicts p.[121]

Now I must admit that those who undertake these sort of discussions are smart; their analyses are extremely detailed, and worthy of admiration from the point of view of a philosopher's philosopher, as exercises in reason. But what, one might ask, are they doing? They are not asking how it happens that *true beliefs* come about. They are not asking how we might achieve *true beliefs*, or even how we ought to go about achieving *true beliefs*. No, they are asking, to use Steup's neologism, what *epistemizes* belief, or what can *epistemize* true belief. And their answer is not Plato's, discussed above. They hardly seem to be talking about explanation at all (although they employ it), and do not seem to be concerned about how we might *fix* true beliefs, when we have them.

To the reliabilist, then, I would say that there may well be reliable means by which to establish beliefs; reliable, in so far as *truth-conducive*. But what are they? How are we

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[120] There are, of course, many variants. For example, Lehrer writes: 'What is essential is the reliability or trustworthiness of the evidence for what we accept to guide us to acceptance of what is true rather than false. The trustworthiness of the evidence makes us trustworthy in the matter, whatever our general defects.' Lehrer [1990], p.173. This is wrong, I say, on two counts: first, since what we take to be evidence is theory-dependent (and there is no such thing as 'objective evidence', for there are no entities that constitute 'evidence'); second, since our favoured modes of inference can be faulty (at any given point in time).

[121] Steup [1997], p.377.

to recognise these processes? Can we determine which they are? Indeed, how are we to evaluate the verisimilitude of the proclamations that the epistemologist makes, given the means by which she goes about her work? Do her proclamations even have a chance of being true, even though they would seem to be *reasoned*, and based on *experience*? When Freddy tells me, in the pub, "I know that God exists", what on earth does he really mean? Both that "God exists" is true, and that he formed his belief in 'God exists' by a reliable process? (There is no trick here, where I move from knowing to 'knowing that one knows'; Moore's paradox suggests that Freddy would readily assert, "I believe that I know that God exists". Hence, that he believes he arrived at 'God exists' by a reliable process, if the externalist account of 'knowledge' is right, and he understands what 'know' really means.[122])

With the internalist – who would presumably agree with the spirit of the questions I pose to the externalist – I agree that we do have epistemic duties, which respect to that which we have consciously available. If Freddy were to tell me "I know that God exists", I would expect him to have done his best to work out whether "God exists" is true, and to have paid due attention to *what he took to be* his relevant evidence.[123] If he had not, I would think he had done himself a disservice, *inter alios*, for the epistemic buck stops with each of us, as free agents, exercising our wills.[124] But why should the fact that Freddy had fulfilled his duties be sufficient for his establishment of a true belief, in this instance or any other? How is Freddy to determine that which constitutes relevant evidence, let alone *evidence*, in the first place? Is 'evidence' simply handed to him on a plate, for him to pick at as he wishes? (Is it *drip-fed* to him?) And even if it is given to him in such a fashion, how much of it should be pick at (or be fed), if he is after true beliefs? Still further, how does evidence serve to 'support' beliefs in a non-psychological sense? (We have seen that the probabilistic approach is no good.) And is it really right that Freddy ought to suspend a belief if he has no evidence to 'support' or 'contradict' it? Why can he not have evidence consistent with the belief (x) that having another belief (y) has pragmatic benefits, and therefore commit to belief in y, without any evidence to support y? (Pascal's wager springs to mind.) Why, for that matter, can he not just 'take a chance' on y, provided he admits to himself that he is so doing, and is happy to give y up if (what he takes to be) evidence against it happens to come his way?

Agreed, I simplify. Agreed, moreover, that the foregoing 'quick-fire' questions would seem, taken together, to resemble a rant. But let me emphasise that it is the *spirit* of my objection to contemporary epistemology that I wished to convey above. It is to its very futility that I point, as I should like now to affirm in a more conciliatory tone, by providing five specific objections to the epistemologist's project.

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[122] It may be objected that Freddy might not be an externalist, so might not be making such a claim. As a matter of fact I agree. But my conclusion is just that 'to know' is hardly ever used in the externalist's sense, which makes one wonder if the externalist is discussing what it is 'to know' at all.

[123] The point here, which I come back to, is that reason has to determine what counts as evidence or not; I thoroughly reject the notion that evidence is handed to us.

[124] I cannot really be expected to defend the idea that we have free wills here, so ask the reader to let this claim ride. After all, what is important in context is that the internalist would generally accept such a claim; see Steup [1997] on doxastic voluntarism.

First, we should note that epistemologists are typically concerned with a somewhat disembodied, even dehumanised, notion of 'justification'. Take a young man with a pervasive developmental disorder, who is struggling to fulfil his epistemic duty, which he in fact recognises despite his condition. (I say condition, implying essence, and *not* illness or disease.) Let's call him Nicholas, and let us imagine – as is, I should think, plausible – that Nicholas makes his decisions, and his choices to commit to beliefs, in a way that I cannot fully comprehend. In some situations, Nicholas does or says things that I would gladly label, in casual conversation, as 'unreasonable' or 'irrational'.[125] But is it, then, genuinely the case that Nicholas is irrational, or indeed unreasonable? Is it not possible that Nicholas could equally judge much of my behaviour to be pretty weird?

Now what I am trying to get at, here, is that there is some sort of notion of the *Ubermensch* lurking in the background, when we start to compare the behaviour of different individuals with respect to 'rationality'. Many, I think most, would say that one such as Nicholas is 'reasoning defective', whereas I am not. Yet while such a belief may well be true, and I respect the right of others to hold it – I would not begrudge Nicholas his beliefs either, since he fulfils his epistemic duty – I do not think it quite follows that one such as Nicholas makes *unreasonable decisions* in an objective sense.[126] What is reasonable for Nicholas is, frankly, not what is reasonable for me. But likewise, what is reasonable for the reader may not be what is reasonable for me, and what is reasonable for David Lewis may not be what is reasonable for Bas van Fraassen. As a community – of which Nicholas may be a part, as may I – we can, of course, compare what we think is reasonable. But who are we to listen to, and who are we not to listen to? I should say that if we were able to distinguish between those who were doing their epistemic (viz. ethical) duty and those who were not, this would be enough. (Take the intelligent, devious, and persuasive man, who does not do his duty, but seeks to convince others of the truth of his pronouncements. Is *he* not the dangerous one?) Further, notice that to label another 'reasoning defective' is precisely to accept that the epistemic buck stops with one's self. It will not do to say that it is really one's community that does the labelling, because *one* has to choose, for oneself, whether to accept that such a label has been correctly assigned. And I find it bizarre that any academic philosopher should disagree. It is ironic that there should be journals filled with discussions about what is and is not objectively reasonable, when it seems so clear that the participants disagree so widely just because there is no objective standard. Faced with a paper, the philosopher takes herself to be able to make up her own mind about the truth or falsity of the claims therein. But where is the philosopher who would say that she does not make *reasonable* decisions? And if there are so many *reasonable* philosophers about, then why isn't it patently obvious what is *reasonable*?

Second, and along related lines, we should note that the thought-experiments which are discussed by epistemologists tend to be radically insufficient to their intended task, even if we assume there is such a thing as the 'objectively reasonable' decision, or set of decisions, for any given situation. And this is precisely because the bulk of these are not sufficiently specified *with respect to the individual, and state of the individual, in the*

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[125] Note, here, that I do not want to use 'reasonable' in the sense of 'appropriate', in that which follows.
[126] He may, of course, adjust his beliefs in a fashion that is not *truth-conducive* in an objective sense.

situation. Typically, they are of the following form: in situation σ, with experience set ε, belief set β, and desire set δ, then it is reasonable, rational, or maximally rational, to adjust β so as to include/preclude belief B (or set of beliefs {B}, which may or may not be a sub-set of β), to perform action α, or so forth. But please, let us try to be serious about this. Is it genuinely the case that one can list even one's own beliefs, without writing vastly more than a journal article, or even a book? And when we approach such examples, do we not tend to project ourselves into them; that is, to try to place *ourselves* in the situation σ?[127] (How else to get a handle on what seems 'reasonable' or not? What is 'reasonable' is not necessarily what leads to true belief formation, after all.) I think the answer is in the affirmative, and that in doing so we tend to import, unavoidably, many of our own beliefs and desires.[128] Admittedly, this does not render all such thought-experiments pointless – we might hope that what many of us import is often similar enough to allow us to perform *a limited comparison of reasoning strategies* – but it severely limits their generality.

Third, there is a curious circularity that is inherent in the project of the epistemologist. After all, what she would want us to believe is precisely that her pronouncements about 'knowledge' are *true* – it is *that* which she argues for. She cannot ask us to evaluate whether we 'know' (or could know) that such pronouncements are *true*, on pain of incoherence. Hence, evaluation with respect to truth holds primacy. Return to Steup's claim, mentioned earlier, that 'Our epistemic duty is… to suspend judgement about *p* if our evidence neither supports nor contradicts *p*.' And now let *p* be precisely "Our epistemic duty is to suspend judgement about *p* if our evidence neither supports nor contradicts *p*." Should we not, then, suspend judgement about *p*? What could actually count, even in principle, as evidence that either supports, or contradicts, such a normative claim? (It is far from obvious that one who is charged with a crime should not be presumed innocent.) And should we not end up suspending judgement about whether or not 'we should suspend our judgement about *p* if our evidence neither supports nor contradicts *p*', and so on? The lesson seems to be that 'taking a risk' on a belief – that is, taking the risk that it is true – is not irrational (nor indeed rational!).

Fourth, it might be said that classical epistemologists tend to rely on an extremely narrow view of rationality, such as that mentioned briefly in I.2.4. In particular, their focus on beliefs is dubious, when it might also be the case that there are rational hopes, fears, and even desires. Further, there may even be such things as rational hope-making processes, and so forth. The issue is one of 'direction of fit', and it remains unclear to me why merely understanding *how* things are is sufficient to enable reasonable interaction with our surroundings, particularly with respect to our fellows: indeed, surely it is the case that we seek to understand how we would prefer for things to be, for ethical reasons or otherwise, as well as how we would prefer for things not to be. Agreed, we need to understand what is possible if we are to understand what we can achieve, and the

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[127] It may be thought that I assume the truth of something like 'Folk Psychology' or 'simulation' here, but I do not. (And in so far as those who theorise about 'rationality' tend to, I would add that this sort of -assumption may be found wanting. This is just another potential argument against their project.) For one may very well say that we import our understanding of social norms into such examples, and point out that these can hardly be fully specified either. I am grateful to Matthew Ratcliffe for pointing this out.

[128] I have not even mentioned moods, which might also be relevant.

potential dangers we face, but merely understanding that which is possible does not, in itself, tell us what to fear and what to hope for. Moreover, it tells us nothing about what attitudes we ought to adopt. In the terms in which Macleod puts this, I am therefore in favour of a 'type 3' analysis of 'rationality', in contrast to the 'type 1' and 'type 2' analyses which seem to have been historically favoured:

> Theories (Type 1 theories, let us call them) for which it is beliefs and belief alone to which terms like *rational* and *irrational* are properly applicable will be very different, certainly in scope and probably also in structure, from (Type 2) theories for which actions and decisions can also be said to be rational and irrational. And both will be different from (Type 3) theories, which seem to make room in addition for rational appraisal of an agent's desires and preferences and of the ends, ideals, and principles associated with them.[129]

Finally, and most importantly, there are serious doubts about whether it is worth undertaking epistemological inquiry without being in possession of a sound metaphysical possibility space. For discussing narrow logical possibilities – using loose talk of 'reliable processes', 'mental states', 'beliefs', and so forth – is all very well, but it is hardly going to shed any light on how we genuinely interact, and how we should best interact, with the rest of the world. For example, how are we to understand 'reliable processes'? Presumably such things would come about due to laws governing the interaction of entities, perhaps both mental and physical, but then our understanding of laws becomes relevant. Is there a plausible example of such a process? One could simply say 'perception', but this is rather unenlightening unless it is understood precisely what the (metaphysical) possibilities pertaining to perception are. For instance, if perception is not a causal process, then could it still constitute a 'reliable process'? And how, precisely, can the mental and the physical interact? Or is there not really a proper ontological (viz. non-phenomenological) distinction to be drawn between the mental and the physical? In short, it is not productive to bracket metaphysics and ask questions like 'What is knowledge?', or even 'What is belief?', for such questions are to be answered, if they are answerable at all, on precisely such a basis; that is, in such a possibility space. As such, to make the claim that '*x* is knowledge' while bracketing metaphysics is about as convincing as making the claim that 'the actual world is deterministic', while bracketing physics. About as convincing, that is, as Bill Clinton's claim not to have had "sexual relations with *that* woman".

It might be said that what all this amounts to, and what I am arguing for, is *irrationalism* (underpinned by a degree of doxastic voluntarism, and indeed voluntarism *period* as the prop for active inquiry). However, if this is right then I am in fine company: that of 'the irrational rationalist' (Popper) and 'the passionate liberal' (Feyerabend).[130] For I agree with the former that the only method – the one true method of philosophy, nay all inquiry – is stating one's problems clearly (or as clearly as one can), and tackling them critically (or as critically as one can).[131] And I agree with the latter that there is nothing wrong with 'epistemological anarchism', *provided* such 'anarchism' is *governed* (or limited) by

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[129] Macleod [1986], p.59
[130] Newton-Smith [1981].
[131] I take the opportunity to argue this in further depth hereafter, II.4.

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the aforementioned method.[132] Indeed, the thread that binds these two views together – and it is there in Popper's work, under a sympathetic reading – is precisely a deontological one. It amounts to what might be called a categorical epistemological imperative: do your best to find the truth, given the capacities at your disposal. *It is epistemological cum ethical, and urges us to employ our wills to a mutually beneficial end.* And as such, truth is properly *an aspiration*, rather than *an aim*; I am content to admit this.

The cat is out of the bag. But I have discovered, to my chagrin, that this sort of admission – one that was demanded of Popper by Newton-Smith [1981], O'Hear [1980], Salmon [1981], and so forth – seems equally as strange to the scientific realist as it does to the empiricist. After all, the scientific realist seems to want the admission just because she thinks it makes a mockery of the anti-justificationist (and anti-inductivist) project. (It does not. It merely leads to a clear statement of its Socratic root.) The empiricist wants to ask why we should still cling on to truth even as aspiration for natural science. But I want to emphasise what I have *not* denied, because otherwise it may appear that I am advocating some sort of radical relativism:

(i)   Natural sciences do have their own distinct *methodologies* (involving many local methods, specific to particular experiments, etc.), which are subject to critical scrutiny, and revision with a view to improvement. These have significant sociological and practical elements; for example, there may be standard practices involving paper writing, presentation of data, and so forth.

(ii)   Theories and facts *can* (together) be evaluated with respect to their verisimilitude, albeit that such evaluation is fallible, and never conclusive. (And what counts as a fact is a theory-dependent issue; 'non-conceptual experience' is a misnomer, as I argue later, in III.4.) This is a piecemeal process: there is no true dictum such as 'the theories in mature science are generally highly verisimilar', or ET.

(iii)   Local methods *can* be evaluated with respect to their suitability to achieve a task (in an efficacious manner), or provide true conclusions. Further, formal systems of inference *can* be evaluated with respect to their ability to preserve truth. Both can be revised with a view to improvement, in epistemic and pragmatic senses. (Remember, I agree with the externalist to the extent that there may be reliable processes. Finding which they are, on this assumption, is the tricky bit.)

(iv)   We can look for suitable means by which to find the truth – *intersubjective ones* – and call such means 'rational', if liked. But then we would be discussing precisely how it is *best* to think, or to behave, not how it is *reasonable* to think or behave. What it is *right to do* and what it is *reasonable to do* (not 'appropriate to do') are not the same; we can strive to apply reason with a will to discover the former, but there are no guarantees it is sufficient to the task.

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[132] Feyerabend took rather a curious tour in his philosophical life, but towards the end there were indications that relativism no longer appealed to him, although his anarchistic spirit was still intact. See, for example, Feyerabend [1995].

Against 'Knowledge' as a Propositional Attitude

> On the standard view, believing is merely a state of mind but knowing is not, because it is factive: truth is a non-mental component of knowing. – Williamson[133]

I mentioned much earlier that it was not only unclear, but also dubious, that 'to know' a proposition can be understood as 'to take an attitude towards' a proposition. Yet to my surprise I have found this point to be contentious in my discussions with fellow philosophers, and would thus seem to owe a detailed argument to this effect.

So let us have it that we really possess propositional attitudes, and list some undisputed examples thereof: belief; hope; fear; and intuition. And let us notice that the objects of these attitudes are precisely, by definition, *propositions*; they are not a particular sub-set of propositions. One can believe that p irrespective of whether p has any particular property (save perhaps 'believability', if one wants to commit to such a notion); one can hope that p whether or not p is (objectively) desirable; one can fear that p without it being (objectively) fearful; and one can intuit that p (or have the intuition that p) without it being the case that p. In all these cases, one can take an attitude to a proposition in spite of its properties, unless we want to attribute trivial properties to the propositions that are proper objects of these attitudes – to say that they need be, variously, 'believable', 'hope-able', 'fear-able', and 'intuit-able'. (In a similar fashion, in order that I be able to see x, one might say that x need be 'see-able'. But it does not follow that 'see-ability', or 'observability', is really a simple property possessed by any thing. For one may equally say a table is red without meaning that a table has the fundamental property of redness, and on a side note I cannot resist adding that van Fraassen's talk of 'observables' seems very metaphysical to me.)

But now let us consider 'knowledge', and allow that – following the trivial attribution of properties above – for one 'to know p', p must be (*sotto voce*) 'knowable'. Can we take a basic attitude towards p, such that we *know* p? Well let us note under the standard philosophical account of 'knowledge', it is a prerequisite of 'knowing that p' that one believes that p. But if one believes that p, one takes precisely a propositional attitude to p; the consequences are typically that p is worthy of assertion, and action upon, pedantic cases aside.

So if one knows that p, it is necessarily the case that one believes that p; believes, precisely, that p is true (or possesses the property of truth, *qua* truth-bearer). And to believe that p possesses the property of truth (or lacks the property of falsehood) is not to require that it does, in fact, possess such a property. Yet in the case of 'knowing that p', if 'to know' is to be taken to be a basic attitude, it is required that p *does possess* the property of truth, and perhaps even an additional property of 'being justified'. And we might ask, then, why 'knowing' should be the odd attitude out – in comparison to the

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[133] Williamson [2000], p.22

other propositional attitudes agreed upon – in so far as it requires that p have non-trivial properties.

That which we 'know', if there is 'knowledge' in the sense of 'justified true belief', is precisely the *sub-set* of that which we believe – take the *attitude of belief to* – which just so happens to be true, and just so happens to be justified. But what sense does it make to speak of a 'justified' proposition? What we surely mean is that *how we have come to believe in p* (adopt the propositional attitude of belief to p) is sufficient to *justify our taking the propositional attitude of belief* to p. Thus, we might say that 'to know p' is simply to take the propositional attitude of *belief* toward p, when it just so happens that p is true, and that the propositional attitude of *belief* toward p was arrived at by a means sufficient to *justify* our taking said attitude toward p. What we 'know' would than be a sub-set of what we believe – the propositions we take *that* attitude towards, first and foremost. As such, the notion of 'knowledge' as a distinct propositional attitude seems extravagant; indeed, Williamson himself acknowledges that 'believing p truly is not a mental state, at least, not when p is an ordinary contingent proposition about the external environment'.[134]

Of course, a counter argument could be made on the grounds that one only can be 'sad that p' and 'happy that p' if one 'believes that p'. But these attitudes do *not* require that p possess a non-trivial property such as truth, rather than 'potential object of happiness'. (I can believe my mother has been cured of cancer, and be happy that my mother has been cured of cancer, when she has not, in fact, been cured of cancer.) As Williamson puts it, 'knowing... is a *factive* attitude... Other factive attitudes include perceiving that something is so, remembering that it is so, and regretting that it is so.'[135] However, Williamson's examples are not persuasive in the slightest. First, one can perceive that something is so without it being so; one can see that 'there are sheep jumping over a fence' in a dream, when there are actually no sheep jumping over any fences, anywhere. (Remember, *seeing* is not *seeing that*.) Second, one can remember that something is so without it being so – Popper remembered his meeting with Wittgenstein at the Moral Sciences club in Cambridge very differently from the others there present, and the accounts are mutually incompatible. Third, and finally, one can regret never having done something when one has, in fact, done it – amnesia is a common enough occurrence, after all. What I am suggesting, here, is that there is a *qualitative feel* associated with attitudes such as 'seeing', 'remembering', and 'regretting'. But as I shall endeavour to urge below, it would be strange to say there was a qualitative feel to 'knowing', or even 'believing with a high degree of belief'.

Now we might say that 'sadness' and 'happiness' are akin to second order propositional attitudes, in so far as they are attitudes towards propositions that are also objects of belief, but even then they would still be distinct from 'knowledge' in so far as *it* requires a genuine property of the belief object, namely *truth*. Furthermore, if we are to be realists about truth, then we must hold that p can be true whether it is 'believable' or not. But

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[134] Ibid., p.27.
[135] Ibid,. p.21

then, p can be true whether it is 'knowable' or not, precisely because if it were to be 'knowable' it would *have to be* 'believable'.

So to repeat, there is an obvious phenomenological distinction between being happy that p, fearing that p, believing that p, and so forth. But is there any phenomenological distinction between believing that p and knowing that p? One might want to answer in the affirmative by claiming that 'knowing p' involves a stronger feeling than 'believing p', but this approach is doomed to failure just because attitudes admit of degree. For example, one may have a mild fear of open spaces, but an intense fear of spiders, and thus fear 'I am in an open space' less than 'There is a spider crawling on my arm'. And likewise, one may have a strong belief that one exists, but a less strong belief that universals exist, and so forth; indeed, one may even be *certain* that p, viz. assign it a subjective probability of one, when it is not the case that p. Besides, as Ramsey has insightfully pointed out:

> We can, in the first place, suppose that the degree of a belief is something perceptible by its owner; for instance that beliefs differ in the intensity of a feeling by which they are accompanied, which might be called a belief-feeling or feeling of conviction, and that by the degree of belief we mean the intensity of this feeling. This view would be very inconvenient, for it is not easy to ascribe numbers to the intensities of feelings; but apart from this it seems to me observably false, for the beliefs which we hold most strongly are often accompanied by practically no feeling at all; no one feels strongly about things he takes for granted.[136]

There is, however, an ingenious last-ditch defence for the advocate of 'knowledge' as a propositional attitude. For what if there were a group of propositions such that, for any possible world in which a human could come to believe them (or choose to believe them), they would be true? The idea would be precisely that there are some beliefs which simply *could not be formed* (or come about for any human) in any world where their (propositional) objects were false. Now this claim strikes me as bold, and I am inclined to agree that such objects of belief would genuinely be worthy of the name 'knowledge', but it still does not strike me that this model is sufficient to suggest that 'knowing' is a fundamental propositional attitude. Again, it would seem to be parasitic on belief, and to have no distinctive phenomenological character. And as such, it would seem that those who would want to save 'knowledge' as an attitude would do better to suggest its objects are *facts*, *states-of-affairs*, or other non-propositional entities.[137] (This, although one might still be able 'to know p' in the sense of 'being acquainted with p', rather than 'acquainted with the truth-value of p'.) But then we would no longer be discussing the propositions, statements, or arguments, which are the lifeblood of the *activity* of inquiry. We would be back to the notion of acquaintance, with which I started this section.

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[136] Ramsey [1926], p.169
[137] This is suggested by Vendler [1972].

4. ON DEMARCATION AND CRITICAL RATIONALISM

The importance lent to the falsifiability criterion and demarcation problem by Popper and others distorts his thought – Bartley[138]

[T]he demarcation between science and metaphysics is a special case of the wider problem of demarcating criticizable from non-criticizable theories – Popper[139]

What demarcates 'science'? This is now a middle-aged question, which has been tackled – albeit with difficulty – by many a philosopher. For the Vienna Circle – and following a particular understanding of Wittgenstein's *Tractatus Logico-Philosophicus* – it was the verifiability of the statements upon which it was founded. For Popper, it was the falsifiability of (many of) those statements, and associated theories; at least, going by a superficial view of *Logik der Forschung*.[140] For Kuhn, it seems to have been a particular set of virtues which are sought of theories, or models, by certain communities of thinkers.[141] And today, we find those such as van Fraassen implying, along related lines, that there is something special about its method.[142]

However, the approaches of several of these philosophers have a significant similarity. They start by examining actual scientific practice, be it in a philosophical, psychological, sociological, or historical, manner. And that which does not conform to their findings is labelled, variously, as 'meaningless', 'speculation', 'heuristic', or 'word play'. The word 'metaphysics' can then be casually attached to that domain, sometimes as if there is nothing more to be said. The problem is simple: they seem to make the unspoken assumption that there is something special, or important, about this thing called 'science'. Then they go out looking for it, think that they find it (because they expect to), and seek to denigrate, in varying degrees, that which falls outside *its* scope. (But is essentialism about 'natural science' right? I think not, and am not eager to merely assume, in any event, that disciplines such as chemistry and physics are not autonomous, or that the former will at some stage become a sub-domain of, or reduced to, the latter.)

This should be no surprise. For the problem is set up in a way which is conducive to making value judgements about (allegedly) different forms of activity, or the products thereof; as if one could compare Renoir's *Sur le Terrasse* with Newton's *Principia Philosophiae Naturalis*, and declare that one was an objectively *better* piece of work, on universal assessment criteria. (Indeed, even Popper, who believed that 'metaphysics' is meaningful and indispensable, at least in his later days, still occasionally argued that 'science' has a greater epistemic status: 'In science (and only in science) can we say that

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[138] Bartley [1968], p.43.

[139] Popper [1968], p.95.

[140] Commonly, this is taken to be falsification by *observation statements*, but I shall say more on this below.

[141] These are 'accuracy, consistency, scope, simplicity, and fruitfulness'. Kuhn [1977], p.322. See my discussion in Rowbottom [2002], pp.46-53.

[142] See the subsequent discussion in III.5-III.5.4.

we have made genuine progress: that we know more than we did before.'[143])
Unfortunately, however, it is not clear that our lot is so simple, although the question of
demarcation is, as I mentioned in the prologue, important in several non-academic senses.

Now I do not want to argue that natural philosophy and metaphysics are radically
incomparable. Rather, to emphasise that any reasoned and considered comparison should
involve talk of value judgements; much as van Fraassen's recent talk of 'stances' might
suggest. Or to return to Popper:

> [A]t any moment we are prisoners caught in the framework of our theories; our
> expectations; our past experiences; our language. But we are prisoners in a Pickwickian
> sense: if we try, we can break out of our framework at any time. Admittedly, we shall
> find ourselves again in a framework, but it will be a better and roomier one; and we can
> at any moment break out again... a critical discussion and comparison of the various
> frameworks is always possible... The Myth of the Framework...simply exaggerates a
> difficulty into an impossibility.[144]

In this case, the metaphysician might 'turn the tables' on the debate, start with the
assumption that there is this thing called 'metaphysics' which is 'special' – but not,
please, in an epistemic sense – then try to demarcate it, and see where that leads. Such an
analysis might then be compared with that of a Popper or a Kuhn, and prove fruitful in
clarifying the debate. (It might emerge, for example, that neither approach tackles an
underlying problem situation, which has not yet been unveiled and examined.) However,
I shall not make it my task to undertake this here, because I put forward my view of
metaphysics in the next chapter, in part by building upon the historical theses developed
in sections 1-1.3 above. Rather, what I want to examine at this stage is the very question
of how we should expect to demarcate *any* discipline. And I think that this broader
question is important to examine precisely such that one is not tempted to make *ad hoc*
alterations in one's account in order to preserve putative 'disciplines', or 'areas of
investigation', with which one takes oneself to be associated. In my case, these might be
'physics', 'mathematics', 'philosophy', 'epistemology', 'philosophy of science', 'history
of science', and 'metaphysics'. Indeed, I would not have 'aesthetics' or 'ethics' in my
mind as I wrote, simply because I am not terribly interested in them, and even have
doubts, perhaps foolishly, about whether they *are* distinctive disciplines.

It should go without saying that it is far from obvious that it only makes sense to carve up
activities into scientific and non-scientific, or metaphysical and non-metaphysical. For
example, mathematics might be a domain which is not easily subsumed under either
'metaphysics' or 'science'; it might be radically distinct from both, although of great
interest, use, and value – in somewhat different ways – to practitioners of either. Just as a
painter might employ simple geometry as an end to representing a scene in proper
perspective, a physicist might employ vectors in order to determine the resultant force on

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[143] Popper [1970], p.57. One way of understanding this claim is that Popper thinks of truth as being
grounded-in-the-actual,-as-well-as-being-'absolute':-the-wording of Tarski's-theory-of-truth – 'it-is-the-case
that' – might suggest this. However, it is unclear that one cannot say 'It is true that it is possible that *p*', in
a substantive sense.
[144] Ibid., pp.56-57. I build upon this in the next sub-section.

an object with both mass and charge, situated in both an electric field and a gravitational field.

Demarcating Proper Inquiry

On the question of demarcation, my starting point is to recognise that there are two core questions that might be asked, which are strongly suggested by the quotations that open this section. The first is "What are the necessary features of any proper form of inquiry?", and 'proper' has normative force. The second question, which is subsidiary, is "In virtue of what should we demarcate different forms of inquiry?" Further, I hold that the former is the more important, and that to attempt to fuse the two is a serious error. Indeed, it seems to me that those who are eager to jump to the claim that the second question can be answered by appeal to method, or worse method alone, are only conflating it with the first. For while it seems very plausible that inquiry ought to involve particular sorts of activity – and here, by 'inquiry', I mean *genuine inquiry*, rather than merely what one group or another does in the name of 'inquiry'[145]– it is unclear that there *ought to be* any essential difference between the fundamental approach of those properly investigating, period; that is, be they anthropologist or zoologist.

Now in this regard, I might return to my brief comments about 'normal science', and the important debate in *Criticism and the Growth of Knowledge*, in II.1.[146] For notice that Popper agrees with Kuhn that there really is such an activity as 'normal science', but simply disagrees that this activity is *proper*, let alone characteristic of good science. In his words:

> 'Normal' science, in Kuhn's sense, exists. It is the activity of the non-revolutionary, or more precisely, the not-too-critical professional: of the science student who accepts the ruling dogma of the day; who does not wish to challenge it; and who accepts a new revolutionary theory only if almost everybody else is ready to accept it – if it becomes fashionable by a kind of bandwagon effect… I believe, and so do many others, that all teaching on the University level (and if possible below) should be training and encouragement in critical thinking. The 'normal' scientist, as described by Kuhn, has been badly taught. He has been taught in a dogmatic spirit: he is a victim of indoctrination. He has learned a technique which can be applied without asking for the reason why (especially in quantum mechanics). As a consequence, he has become what may be called an *applied scientist*, in contradistinction to what I should call a *pure scientist*. He is, as Kuhn puts it, content to solve 'puzzles'… it is not really a fundamental problem which the 'normal' scientist is prepared to tackle: it is, rather, a routine problem, a problem of applying what one has learned…[147]

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[145] Of course, it may be the case that there are some people in physics departments, say, that are not really engaged in genuine inquiry at all. And it may also be the case that some people do particular things with their aspiration as truth, although they are insufficient to the task of achieving it. In short, the mere appearance of inquiry-occurring is not sufficient to make it the case that inquiry is really occurring.
[146] For more on 'normal science', see Rowbottom [2002], *'Normative Methods and Normal Science – Logic of Discovery'*, pp.57-61.
[147] Popper [1970], pp.52-53

This passage is extremely poignant from my personal perspective, and I remember clearly the moment when I first read it, just under five years ago. For I believed, and still believe, that I was taught in the dogmatic fashion that Popper inveighs against. Yet for whatever reason – by an implausible innate disposition to be questioning, or more cogently an inquisitive disposition fostered by a mother who always did her best to answer even my most difficult questions, in my early childhood – I thankfully came to appreciate just that. That I was being subjected to a textbook diet of de-historicised 'reconstructions', which were devoid of deep insight, and penned by individuals who had been through a similar rigmarole in their training, more often than not. Indeed, it is a striking coincidence that my encounter with quantum mechanics was precisely the curtain call for my time in physics; this, since the Copenhagen view, so uncritically asserted in lectures, seemed to me to be utterly nonsensical. I became fascinated with the underlying problems – of determinism, measurement, non-locality, and so forth – to the exclusion of everything else.[148] I sought an answer, and when I eventually encountered the work of Bohm it began to dawn on me that this had been marginalised for nothing more than political, and social, reasons. (It had literally been written out of the textbooks, as a matter of 'historical contingency', as Cushing so plainly puts it.[149] And as Bohm writes: '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.'[150]) Worse, I soon discovered that none of my lecturers had even read the original work of Bohr, Heisenberg, or Born. No, they were, rather, reporters on the work of previous reporters! And so, a philosopher was born. A philosopher that was to find great sympathy with the following words of Feyerabend:

> The idea that science can and should be run according to some fixed rules, and that its rationality consists in agreement with such rules, is both unrealistic and vicious... vicious, since the attempt to enforce the rules will undoubtedly erect barriers to what men [and women!] might have been, and will reduce our humanity by increasing our professional qualifications.[151]

> [I]f normal science is *de facto* as monolithic as Kuhn makes it out to be, then where do the competing theories come from? And if they *do* arise, then why should Kuhn take them seriously and allow them to bring about a change of the argumentative style, from 'scientific' (puzzle solving) to 'philosophical'? I remember well how Kuhn criticized Bohm for disturbing the uniformity of the contemporary quantum theory. Bohm's theory is *not* permitted to change the argumentative style. Einstein... *is* permitted to do so... Does this mean that proliferation is permitted as long as the competing alternatives are firmly entrenched? But pre-science which has exactly this feature is regarded as inferior to science.[152]

Now while this digression might seem self-indulgent, it flags up a very serious concern. For while it would be easy for me to pour scorn upon much of what occurs in

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[148] It is ironic, of course, that it was precisely the inconsistency of this theoretical framework with others in physics – particularly those of a more classical, deterministic, vein – that prompted my rejection of it.
[149] See Cushing [1994]
[150] Bohm [1987], p.39.
[151] Feyerabend [1970a], p.91.
[152] Feyerabend [1970b], p.206.

contemporary physics – I will come to this – it might seem to be the case that learning in a didactic fashion is necessary simply to get to grips with the problems that contemporary physicists are concerned with. And this, I take to be the thrust of Kuhn's view: that there is a point at which a discipline goes through a transition in order to attain maturity, only after which progress becomes an 'obvious characteristic' thereof. At that stage a genuine 'scientific theory' is at hand, and in his words, 'once hope for a therapeutic prescription is abandoned, there is no reason to expect anything less [than normal science]...with such a theory in hand the time for steady criticism and theory proliferation has passed.'[153] Yet I am not sympathetic to this view, and while I will agree that it has considerable descriptive accuracy, with respect to the latter half of the previous century and the *status quo*, I find it normatively unpalatable. For whereas it must be right that one will need to understand a great deal of material in order to be able to engage in contemporary work in science, the important question is where the emphasis *should* lie in the teaching process, and what sort of activity *should* be encouraged: critical examination, or puzzle-solving? The answer for Kuhn is clearly the latter, for he continues:

> Even given a theory which permits normal science, however, scientists need not engage [sic] the puzzles it supplies. They could instead behave as practitioners of the proto-sciences must; they could, that is, seek potential weak spots, of which there are always large numbers, and endeavour to erect alternate theories around them. Most of my present critics believe they should do so. *I disagree but exclusively on strategic grounds*...I confessed to Feyerabend that I shared Bohm's discontent but... No one, I suggested, was likely to resolve the paradoxes of the quantum theory until he could relate them to some concrete technical puzzle of current physics...Because they can ordinarily take current theory for granted, exploiting rather than criticizing it, the practitioners of mature sciences are freed to explore nature to an esoteric depth and detail otherwise unimaginable.[154] [Emphasis mine]

I disagree, but exclusively on epistemic, and hence deontological, grounds. First, because it is surely not right that one has to 'take current theory for granted' in order to be 'freed' to employ it *howsoever* one likes, whether 'esoteric' or not; rather, one may simply *accept* a theory for particular theoretical, practical, or predictive, purposes.[155] Just consider that many physicists still employ classical mechanics from time to time, say when concerned with terrestrial projectile motion, although they are under no illusion that it is true, or even highly verisimilar. Second, because theoretical frameworks in different domains of a discipline might surely be compared with respect to their consistency, each with the other; and it is very hard to see how 'a critical mass of anomalies' – which is what Kuhn says is generally required for theory-change, and is *right* to be required – could account for, or is needed to prompt, the processes of reduction or integration.[156]

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

[154] Ibid., p.246.

[155] The distinction between acceptance and belief is, of course, at the core of van Fraassen's approach. In the following example, one might 'accept' Newtonian mechanics in precisely *his* sense.

[156] To be entirely fair to Kuhn, though, it should be added that he mentions external consistency as a theoretical virtue. See Kuhn [1977], ch.13. Furthermore, he also discusses three types of 'articulation' in his *Structure of Scientific Revolutions*, the third of which may be relevant in this regard, and 'resembles exploration'. Unfortunately, however, the discussion is simply too brief to be convincing: it reads precisely

Third, because putting all one's eggs in one basket does not prepare one for the day when the fox comes, and gobbles them all up; and 'periods of crisis' should, I think, be *avoided*, rather than invited.[157] And fourth, last but not least, creativity should be fostered, not quashed, if our objective knowledge is to be increased; for when crisis comes, as it is bound to from time to time, despite our best efforts to prevent it, who are we going to look to in order to deal with it? Those who have been indoctrinated into a disciplinary matrix, been taught not to rock the boat, and had their capacity for independent thought eroded by years of monkey practice (viz. 'puzzle-solving')? It seems to me that if Kuhn is descriptively correct that 'Lifelong resistance, particularly from those whose productive careers have committed them to an older tradition...is not a violation of scientific standards'[158], then this is a sad indictment on science (as is, *inter alia*). Indeed, it seems, further, that the favourite child of the relativist, namely 'incommensurability', might really be an illusion generated by the exchanges of dogmatic thinkers who have become set in their ways, and are no longer willing to fulfil their epistemic duty.[159]

Overall, then, I should say that students of natural sciences simply *ought* to be taught differently than they presently are (at least from my experience). To be encouraged to critically engage with original scientific works, and examine them in historical context, while also being expected to understand the course of development of the subject, and to attain mastery in the employment of various theoretical frameworks, past and present, in order to solve *problems* (in particular, what were once considered to be pressing problems). Furthermore, I say that anyone who was successful in such a programme would prove to be capable of doing what Kuhn calls 'puzzle solving' – I prefer Popper's 'applied science' – perfectly well. (After all, it is hardly the case that a philosophy student need think that classical logic is absolutely perfect, in order to be proficient in employing it!) But what is more, I suspect that such an individual would be *better* at applied science than one who had merely been indoctrinated, if for no other reason than this: she would have been versed in a wide variety of problem solving strategies, and have become skilled in mixing and matching them.[160] And better still, she would have

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as if he is discussing critical exploration, rather than a simple semblance thereof. See Kuhn [1996], pp.29-30.

[157] My point, here, is that if there are several theories on the go, with respect to a particular domain of investigation, there is breathing space in the event of systematic failure of one of those theories. And I hold this to be a blessing; one which the open-minded individual can be thankful for.

[158] Kuhn [1962], p.151. See also Kuhn [1962], p.90, where he writes: 'Almost always the men who achieve these fundamental inventions of a new paradigm have been either very young or very new to the field whose paradigm they change.' Yet he admits, in a footnote: 'This generalization... is so common as to be a cliché...Nevertheless, the generalization badly needs systematic investigation.'

[159] Remember, in this regard, my discussion in I.2.3.

[160] Popper quotes Oersted in this regard, in opening his *Realism and the Aim of Science*: 'So much is certain: that nothing is better adapted to form a mind which is capable of a great development, than living and participating in great scientific revolutions. I would therefore counsel all those whom the period they live in has not naturally presented with this advantage, to procure it artificially for themselves, by reading the writings of those periods in which the sciences have suffered great changes. To peruse the writings of the most opposite systems, and to extract their hidden truth, to answer questions raised by those opposite systems, to transfer the chief theories of the one system into the other, is an exercise which cannot be sufficiently recommended to the student. He would certainly be rewarded for this labour, by becoming as independent as possible of the narrow opinions of his age.' Popper [1983], p.3.

been prepared to make her own innovations: have gained a grasp of the weak points of favoured contemporary theories, understood potential ways in which they may be corrected, and so forth. She would be prepared for, and equipped to deal with, crisis. She would be sent forth as scientist proper, rather than as a fettered hack; as a potential contributor to *our* ongoing critical debate, not just a data-collecting parasite replete with a misplaced, but deliberately instilled, certitude about 'textbook science'. (If empirical evidence is wanted, just watch a few documentaries about work in modern science, or an Open University programme, and listen to the scientists speak. All-too-often they have 'proved this', 'proved that', and 'proved the third thing'. Invincible but incapable, their self-professed power is breathtaking, but also deeply disturbing.) To sum up, then, I find that Watkins is right in writing:

> [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.[161]

Yet remember that what I am about, here, is a discussion of the issue of demarcation. And what I take myself to have argued for – but not proven, thank goodness! – is precisely that it would be wrong to say, with Kuhn, that 'Only when they must choose between competing theories do scientists behave like philosophers'[162]; rather, it should be said that "*Bad* theoretical scientists only behave like *good* philosophers when they must choose between competing theories." (To which it might be added that I suspect there is a closer link between theory-choice and theory-construction than this quotation from Kuhn suggests.) What is the lesson? That the means by which to demarcate genuine 'inquiry' from non-inquiry – or bad inquiry, for I should not want to quibble over a word – is very, very, simple.   It is a matter of attitude, and Popper hit the nail on the head with the humble (but perversely controversial) platitude:

> Scientific theories are distinguished from myths merely in being criticisable, and in being open to modifications in the light of criticism… [T]here is only one way to science – or to philosophy, for that matter: to meet a problem, to see its beauty and fall in love with it; to get married to it, and to live with it happily, till death do ye part – unless you should meet another and even more fascinating problem, or unless, indeed, you should obtain a solution. But even if you do obtain a solution, you may then discover, to your delight, the existence of a whole family of enchanting though perhaps difficult problem children for whose welfare you may work, with a purpose, to the end of your days.[163]

In other words, *the method*, of all inquiry, is simply criticism, and this must be tempered by a willingness to participate in discussion, in a genuine attempt to pursue the truth. (Here is the deontological aspect: one must try one's hardest, *qua* critic, even though there is no guarantee that even *this* will lead to success.) To this we may, if we like, add

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[161] Watkins [1970], p.31

[162] Kuhn [1965], p.7

[163] Popper [1983], pp.7-8. I can recommend this short introduction to anyone who wants to understand what I take to be the genuine nature of Popper's view, so often obfuscated by placing too much focus on *Logik der Forschung*.

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some simple rules about presenting one's opinions clearly, stating the problems they are intended to tackle as pellucidly as one can, and respecting the views of others – although not blithely agreeing with them – in order not to become distracted from the dialectical process through personalising the debate. The notion is that we should stick to world 3 – that of propositions, and their relations – rather than indulge in world 2 issues, *in the process of inquiry*. This is the Socratic Popper, and I, too, am a disciple of Socrates. As Boland puts it, in a useful paper that dispels the pervasive image of Popper as a naïve falsificationist, which was cultivated to some extent by the machinations of Lakatos and Feyerabend:

> It is difficult to see how we could have the current textbook-based education system without Kuhn's view being correct. It is exactly the textbook based education system that presents an overwhelming obstacle to the appreciation of the Socratic version of Popper's view of science that the disciples promote... The presumption taken for granted by all followers of the normal view [of Popper as falsificationist] says that we would have to justify our knowledge before we can claim to know anything... What is most disturbing for Popper's disciples is the presumption that any success in science must be due to a practiced scientific methodology. Again, the disciples take the view that methodology has no more guarantees than a Socratic dialogue. Unfortunately, proponents of the ordinary view of science seem to want more.[164]

Furthermore, it is worth noting that at least one critic of Popper, namely O'Hear, seems to get to grips, albeit partially, with this aspect of his thought. The quotation which heads the next sub-section is therefore his, and it is quite refreshing in comparison to the discussions in Stove [1982], Newton-Smith [1981], and Putnam [1969].

On Criticism, Criticisability, and Fideism

Rationality, then, consists primarily in eliminating errors and learning from them. This process is not peculiar to empirical science, nor is empirical science furthered by empirical testing alone, although it is undoubtedly true that change because of empirical testing is one important mark of the scientific and serves to distinguish it from conservative and empirically closed systems such as African magic... the importance of distinguishing between science and non-science seems to diminish in comparison with the importance of distinguishing what counts as a critical as opposed to a dogmatic approach in each particular field of activity. – O'Hear[165]

Platitudes are helpful, particularly when they seem more plausible, and flexible, than the strong dictums offered by justificationists. But there is much more to be said on the purpose of critical rationalism, and indeed on its proposed procedures.[166] In particular, I

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[164] Boland [1994]. On the charge levelled at Lakatos and Feyerabend, I take it that their recently published correspondence leaves this matter in somewhat less doubt than beforehand. But in any event, it is remarkable how reliant on Popper's thought they both are, particularly with respect to the notion that rationality must be linked to criticism, if it is to be linked to anything. This, despite whatever personal problems they may have had with him. See, for instance, the table in Lakatos and Feyerabend [1999], p.216.

[165] O'Hear [1980], p.111

[166] Here, it should be noted that I am using 'critical rationalism' broadly, to illustrate approaches to rationalism that emphasise *criticism*. As such, I take Bartley's 'comprehensively critical rationalism'

follow Bartley in thinking that the trick is to *avoid appeals to authority*, and *to banish dogmatism*, while simultaneously *making no concessions to irrationalism or Fideism*. And this is what is so beautiful, so wonderful, about the critical rationalist approach, if it can be made to work. There is almost a Stoic flavour to it, which is summed up elegantly by Marcus Aurelius Antoninus:

> Consider that everything is opinion, and opinion is in thy power. Take away then, when thou choosest, thy opinion, and like a mariner, who has doubled the promontory, thou wilt find calm, everything stable, and a waveless bay.[167]

Indeed, it might be said that the purpose of critical rationalism is precisely to defend reason, while not succumbing to the prominent objection to justificationist programmes, which Bartley – in his marvellous *The Retreat to Commitment* – sums up as being a *tu quoque*. In his words:

> In sum, the belief that rationality is ultimately limited, by providing an excuse for irrational commitment, enables a Protestant, or any other irrationalist, to make an irrational commitment without losing intellectual integrity. But at the same time, anyone who makes use of this excuse may not, in integrity, *criticize* the holder of a different commitment. One gains the right to be irrational at the expense of losing the right to criticize. One gains immunity from criticism for one's own commitment by making any criticism of commitments impossible... Moreover if everyone *has* to be a subjectivist, there is a sort of consolation: nobody can look in from the outside. Everyone is alone, inside his own mirror cage, staring at his own face. No wonder the existentialists are bored... The fact remains that anyone who is bored of being bored must answer the *tu quoque*.[168]

And central to all this, as I hope I have shown throughout my foregoing discussions, particularly in I.2.2-2.5, II.1.3, II.2 and II.3, above, is the rejection of appeal to justification, *qua* authority, or indeed *any other form* of authority. If I may again quote Bartley at some length, since he sums up what I have been arguing against so pellucidly, under the flag 'comprehensive rationalism' (which was earlier Popper's phrase):

> The most common conception of rationalist identity, comprehensive rationalism, combines two requirements. (1) A rationalist accepts any position that can be justified or established by appeal to the rational criteria or authorities; and (2) he accepts *only* those positions that can be so justified... In the stereotyped way in which it is usually told, the history of modern philosophy focuses attention on a number of basically subordinate questions that arise only if comprehensive rationalism is assumed to be possible. Among these, the most important has probably been: What is the nature of the rational authority or criterion to which a rationalist appeals to justify all his opinions? The various theories of knowledge are functions of the answers philosophers have given to this question. These answers fall into two main categories:

(Bartley [1962]), or 'pancritical rationalism' (Bartley [1984]), to be versions of critical rationalism, broadly construed as the position advocated in Popper [1945b]. I come on to discuss differences between Bartley and Popper in short order.
[167] Antoninus XII, 22.
[168] Bartley [1962], p.103-104

(1) According to the intellectualists (or Rationalists – with a capital "R"), the rational authority lies in the intellect (or Reason). A rationalist justifies his beliefs by appealing to intellectual intuition.

(2) According to the empiricists, the rational authority lies in sense experience. An empiricist justifies his beliefs by appealing to sense observation.

The history of these answers is one of failure.[169]

I take this to show that one of the central contentions of this thesis is *deeply* unoriginal – it is all there in this magnificent work of Bartley's[170] – and will therefore take this opportunity to make a confession. All that has gone before is so many words, so many little quibbles, chipping away at the dogmatic façade that is contemporary philosophy; and none of it really matters. *It ought never to have mattered.* For while I came into this through a philosophical issue about science – mark well how much I have read, and all the 'authorities' I have cited, faithful examiners! – it may very well have been an issue about theology, or even carpentry. And there is a strong sense in which I have contempt for much of this work, precisely because of what is expected of it, and therefore found within on the basis of a practical recognition, on my part, about how to best get 'a job in philosophy': lengthy references, 'clever' use of words, and discussions of facile distinctions entrenched by 'great philosophers'.[171] But where is the *integrity* in all this? It is a constant struggle not to 'sell out', only occasionally tempting to break the unspoken 'rules' (which seem, more often than not, to be a pile of fluff designed to make us all think we are 'special', *qua* academics), but nonetheless I continue, despite what might be labelled 'an unfortunate – but bloody honest – outburst'. Some unpublished comments of Sir Karl spring to mind here:

> This very fact that this… has so many pages and so many people and so much literature, I mean all that is somehow really, really wrong, not only an error and not very clever and so on, but morally wrong, because it destroys the moral aspect of that attitude.[172]

Now putting all this to one side, we might first want to ask whether there is really a form of critical rationalism that is immune to the *tu quoque* move that Bartley explains – that is fully resistant to the lure of irrationalism – and here we are moved into a rather interesting, and long-standing, difference of opinion between Bartley, and the father-figure Popper. For what Bartley singles out for attack, particularly in his [1965] and [1984], is the following passage from Popper's *Open Society and its Enemies*:

> [W]hoever adopts the rationalist attitude does so because he has adopted, without reasoning, some proposal or decision, or belief, or habit, or behavior, which therefore in its turn must be called irrational. Whatever it may be, we can describe it as an irrational

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[169] Ibid., pp.109-110

[170] Incidentally, Popper also held it to be 'extraordinary beautiful book' (here, referring to the first edition). See appendix B.

[171] What makes the philosophers in question 'great' is that they are vehicles for our learning how to reason, and that they flag potential problem-situations. But part of the very process we must undertake is to decide what is, and what is not, a genuine problem. And no-one, but no-one, is an authority on that.

[172] See appendix C.

faith in reason... Accordingly, our choice is open. We are free to choose some form of irrationalism, even some radical or comprehensive form. But we are also free to choose a critical form of rationalism, one which frankly admits its limitations, and its basis in an irrational decision (and so far, a certain priority of irrationalism).[173]

And further, we should note that Bartley's criticism seems to have had some effect, albeit minor, *prima facie*, since there are alterations to this passage in a later addition, which reads instead:

> W]hoever adopts the rationalist attitude does so because he has adopted, **consciously or unconsciously**, some proposal, or decision, or belief, or behaviour, **an adoption which may be called 'irrational'. Whether this adoption is tentative or leads to a settled habit**, we may describe it as an irrational *faith in reason...* [Bold typeface illustrates changes][174]

What does Popper think about all this, though? Well, note first that in reply to Bartley's charge that 'the problem lies not in the demarcation of the scientific from the non-scientific, but in the demarcation of the rational from the irrational, the critical from the uncritical'[175], he writes 'I can only say that I have constantly suggested this (ever since 1937) to my readers and to my students... I suggested to my students on countless occasions that it is greatly clarifying to identify 'the rational' (I prefer the term 'the attitude of rationality') with the critical attitude, with the critical approach to science *and* philosophy.'[176] So whatever else – I do not wish to enter into a 'guessing game' about the personal relationship between Bartley and Popper, or whether this is really a 'veiled confession' on the latter's part – it seems that they fundamentally agreed on this core issue. Where they seem to have *disagreed* is primarily with respect to the means by which this attitude need be adopted; and while Popper seems to have understood Bartley's project of avoiding the *tu quoque* in his approach to a critical version of rationalism, he had his doubts about whether it could be achieved.

Now in this regard, some interesting letters from Popper have come into my possession, which were written only two years before his death. These are reproduced in full, in appendix B, but some of the most vital passages are as follows:

> I pointed out that Fideism – with a capital 'F' – was a simple consequence of the fact that you cannot rationally establish *any* theory... Therefore, if you accept (other than tentatively) *any* theory, it can only be on Faith. I then pointed out that my "critical rationalism" was in no sense a theory, and I gave them its formulation (O.S. p.225) 'I may be wrong and you may be right, and by an effort, we may get nearer to the truth'; and I pointed out the effort was a (problem-oriented) critical discussion (as opposed to a person-oriented criticism)... the F̲ideist argument (as opposed to what one might call my̲ f̲ideism) was everywhere criticized in my theories... that a scientist should not believe in

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[173] Bartley adds: 'Popper happened to be admirably open and forthright about his fideism, whereas the fideistic character of Ayer's and Putnam's positions is not displayed and may not even be recognized by them. This openness does not, however, solve the problem.' Bartley [1984], p.104-105.
[174] Popper [1945b], p.255
[175] Bartley [1965], p.64
[176] Popper [1965], p.99

his theories (though he <u>may</u> believe that they are <u>preferable</u> to certain alternative theories)... I recommended my ~~theory~~ attitude (!) to my readers; just as Bill did... because I believed it was a god [sic] attitude – indeed, a morally good attitude... Bill, obviously, recommended his pancritical attitude to his readers, and that he would have, if asked why, to say something similarly <u>f</u>ideistic... I should say, now, that the last paragraph on p.231 of O.S. <u>may</u> be misleading. I should, instead, have said something about Fideism (as here) and pointed out that my (of course, tentative) belief that the attitude of critical rationalism should be accepted, although it <u>is</u> a belief, is obviously not a form of Fideism... but a form of tentative fideism which, whenever we recommend anything, we <u>should</u> hold, if we <u>are</u> intellectually honest...<u>this</u> improvement, in verbal formulation, I gladly admit, I owe to Bill. Moreover, I ought to eliminate my concession to irrationalism, since it misled Bill. (Although I am doubtful on this point: there <u>is</u> an irrational element – though not a Fideistic one – in the adoption of <u>any</u> moral attitude – even if the adoption is, clearly, tentative.)...it is clear that, according to Bill's terminology, I always was what he calls pancritical.[177]

So to sum up, Popper's idea is that since there is no justification, the choice to adopt a particular theory – including, as is relevant here, the theory that one ought to adopt a particular attitude, such as the critical attitude – is always 'fideistic' of a fashion. But this 'fideism' is not necessarily a bad thing, and this goes back to my earlier discussion in II.3, where I argued 'that 'taking a risk' on a belief – that is, taking the risk that it is true – 'is not irrational (nor indeed rational!)' In other words, I think that Popper's use of the word 'fideism' here is still extremely misleading, precisely because the view of rationality being promoted is based in the *willingness to test, and give up* that which one believes, or that which one accepts. So I would formulate the line so: the ability (or willingness) to adopt some beliefs (or classify some statements) and the adoption (or classification) thereof is *a prerequisite for being rational*, in the critical rationalist's sense. But in itself, it is neither irrational nor rational: it is, rather, non-rational, in the sense that it has nothing to do with *being* rational. (If clarification is wanted, I take it that having a brain might be necessary for being rational, but not that having a brain is *being* rational. *It is non-rational.* It is this confusion that I think is central.) On the other hand, to be *irrational* is to do a particular thing with the non-rational beliefs that one has adopted: to cling on to them, to seek to preserve them at all costs, or to cherish them in virtue of their being 'one's own'. That really is Fideism, in Bartley's sense of *commitment* to an idea.

If I am right, here, then the point is that one should not be *committed* to critical rationalism (or, if preferred, 'pancritical rationalism'), and provided that one is not – provided that one is ready to have it tested, examined, and found wanting – then one can avoid the *tu quoque* with which Bartley is, quite rightly, concerned. The key is that the proper critical rationalist allows his own weapon – that of stating a problem clearly, and tackling it critically – to be used against him, and even against his theory that this weapon itself *ought to be* adopted. In particular, he will point out that there are at least two ways in which to attack this form of rationalism: (a) 'to produce an argument showing that at least some of the unjustified and unjustifiable critical standards necessarily used by a pancritical rationalism were uncriticizable to boot, that here, too, *something* had to be

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[177] Appendix B.

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accepted as uncriticizable in order to avoid circular argument and regress'[178]; (b) 'If it could be shown that justification and criticism are generally, or even largely, inseparable in principle'.[179] Indeed, all this might be summed up by saying that critical rationalists are really serious about talking to other people, respecting their views, and trying to find the truth by this process: *deadly* serious, in a way that seems to me to be difficult to even want to fault, from a deontological perspective. Yet even this *can* be faulted. For instance, it might be shown that a critical rationalist could do so not without relying on authorities; that appeal to authority is necessary for inquiry, or that the adoption of authority structures is *best* (or failing that necessary) for human societies, or social groups. Indeed, the critic interested in such a line might look to the work of Milgram, who suggests:

> (1) organised social life provides survival benefits to the individuals who are part of it, and to the group; (2) whatever behavioural and psychological features have been necessary to produce the capacity for *organised* social life have been shaped by evolutionary forces; (3) from the standpoint of cybernetics, the most general need in bringing self-regulating automata into a coordinated hierarchy is to suppress individual direction and control in favour of control from higher-level components; (4) more generally, hierarchies can function only when internal modification occurs in the elements of which they are composed; (5) functional hierarchies in social life are characterised by each of these features, and (6) the individuals who enter into such hierarchies are, of necessity, modified in their functioning.[180]

This said, let us now look to the modes of criticism which are suggested by Bartley, to give an overview of how the critical approach might proceed. This, for different disciplines might involve different modes of criticism.

Modes of Criticism and the Friesian Trilemma

> Statements, in short, are not criticisms; methods of attack are. – Miller[181]

One of Bartley's most interesting complaints about Popper's position – one with which I heartily agree, and is highly relevant to this thesis – is the latter's invocation of what would seem to be 'conventions', in his discussion of 'The Empirical Basis'. Specifically, Popper writes:

> From a logical point of view, the testing of a theory depends upon basic statements whose acceptance and rejection, in its turn, depends upon our *decisions*. Thus it is *decisions* which settle the fate of theories. To this extent my answer to the question, 'how do we

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[178] Bartley [1984], p.120

[179] Ibid., p.120, f.11. For even more potential criticisms, see Miller [1994], pp.81-93

[180] Milgram [1974], p.149-150. Yet note that I would want to contest this line, for even if Milgram is right that 'There is a fatal flaw nature has designed into us, and which in the long run gives our species only a modest chance of survival' – see p.205 – it might be said that we should do our best to strive against allowing ourselves to be manoeuvred into a situation where this flaw has the chance to do damage. And here is a link into Popper's political philosophy; the advocacy of the Open Society.

[181] Miller [1994], p.85

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select a theory?' resembles that given by the conventionalist... I hold that what characterizes the empirical method is just this: that the convention or decision does not immediately determine our acceptance of *universal* statements but that, on the contrary, it enters into our acceptance of the *singular* statements – that is, the basic statements.[182]

Now this approach has been criticised by Ayer, in particular, who writes that 'if observation can only motivate but never justify the acceptance of any statement, this principle becomes entirely arbitrary'.[183] Yet Popper's reply is curious in so far as it seems to flirt with 'justification' (construed in an externalist fashion): 'Our experiences are not only motives for accepting or rejecting an observational statement, but they may even be described as *inconclusive reasons*. They are reasons because of the generally reliable character of our observations; they are inconclusive because of our fallibility.'[184] He does not say "They *count* as inconclusive reasons because we have a *theory* that our observations have a reliable character", and therefore it strikes me that there is a hint of vacillation, here, such that this appeal to reliability cannot hold if critical rationalism is to be taken seriously. And if such an appeal is disallowed, then are we not flirting with irrationalism, once more? Even in the terms that the critical rationalist should, I think, prefer: why treat the *theory* (T) that our observations have a reliable character any more seriously than a given theory (T2) that some observations are being used to test? Is the suggestion that we should be *committed* to T, or accept it on faith? That we should dogmatically, or at the very least arbitrarily, accept (some form of) *empiricism*?

Bartley's suggestion is that the confusion, here, is due to the fact that Popper framed his initial discussion of this issue by employing the trilemma of Fries, which is better understood as one dilemma which then induces another. In short, for Fries this runs as follows: if one is not to be a dogmatist, then one must be able to justify the statements one employs. However, any given statement can only be justified by another statement (or statements), thus an infinite regress beckons. Hence, the only remaining alternative is to adopt psychologism, and appeal to something like 'knowledge by acquaintance' of 'facts', 'sense-impressions', or what have you.[185] But all this, Bartley suggests, is a toy: the point is not to play with it, but to understand how its employment might serve to promote a search for firm foundations – or a 'ground of knowledge' – in the sense that I have attempted to reject, earlier in this chapter.[186]

As it happens, however, the solution is remarkably simple, and involves absolutely no appeal to authority, or any slip into arbitrary theory selection. Rather, we return to the simple idea of assessment of the current state of the ongoing debate, and preference on the basis of that assessment. That is, in so far as there is to be 'faith' in anything, this would be 'faith' in ourselves: but that can hardly be any sort of 'faith', since we don't really have any option on this level. (It will not do to disagree by saying we could place faith, instead, in 'God's word', or what have you. For we would still need to assume that we were capable of identifying said word, understanding it, and so forth! In this sense,

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[182] Popper [1959], pp.108-109
[183] Ayer [1974], p.687
[184] Schilpp [1974], p.1114
[185] On this, I refer the reader back to my discussion in II.2.
[186] See Bartley [1984], appendix 3.

the critical rationalist approach is at the very least minimal in so far as 'faith' is concerned.) The core idea is that we can 'step outside' of theories, with respect to any irrational *attachment* (or commitment) we may have to them. Bartley puts it so:

> The test statements are intended to be hypothetical, and criticisable and revisable, just like everything else in the system; there is no justification, no proof, no fixed point anywhere. There is nothing "basic" about basic statements. And hence no possibility of dogmatism with respect to them. If such basic statements happen to be incompatible with a theory, then the theory is false *relative to them*; and they are false *relative to the theory*. There is no question of theory proving reports wrong, or reports proving theory wrong. *Both* could be wrong: neither is "basic"...One contributes nothing to this situation by adding a requirement that one needs to decide by agreement which reports to accept. Moreover, Popper is wrong in claiming that the test will have "led nowhere" without such an agreement: it will have led either to problematical or unproblematical basic statements... one steps outside the positionality of the theory to commit on the state of examination of the theory, treating the theory as an object, not as one's point of view, and oneself coming from beyond the theory...[187]

Now my only objection to this is that Bartley does not, I think, go quite far enough. A theory can be 'false' *relative to some basic propositions* (expressed by statements, or sentence types, no doubt), yes, but this 'falsity' is in its own turn *relative to a particular system of logic*. (I say propositions, because there is the additional issue of falsity with respect to hermeneutic appraisal of sentence types within a language. And indeed, matters become even more complicated when translation functions between languages are required – say when the basic statements are in one language, and the theory is in another. Failing to mention such difficulties will not, alas, make them disappear.) Still further, even at this level – of recognising an inconsistency between a class of basic statements and a theory, given a system of logic – *we are still fallible*. For if there were to be any suggestion that we were not, then some sort of 'ground' to inquiry would be advocated here, after all. What we need, instead, is a frank admission that we can change the logic, change the basic statements we hypothesise as being acceptable, change the theory, change the language in which we work, change our interpretation of the basic statements or theory, or throw a whole system out as entirely unworkable. ("There's something wrong here, let's try a different approach!", "Maybe the way we've set this problem up is wrong?", "Maybe there is no real problem here, and we are asking an inappropriate question?", and so forth.) But I take this to be precisely the strength of a critical approach. It has no boundaries, yet no infinite regress is on the cards: its potential is unlimited.

Before I move on to discuss modes of criticism, however, I should add that I find it curious for Miller to characterise the foregoing quotation in a negative light, by writing: 'For all Bartley's interest in the truth, and his disdain for justification, he seems, by dubbing as irrational those unjustifiable decisions that we make in our quest for truth, to have lost his nerve at the very last minute.'[188] This, since I think that Bartley does no such thing; rather, he endeavours to show precisely how the 'decisions' about *what to*

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[187] Bartley [1984], p.215
[188] Miller [1994], p.93

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accept as basic statements are *neither* arbitrary, *nor* justifiable, against two understandings – 'willkürliche Festsetzungen' (arbitrary stipulations)[189], and then 'based on general reliability of observation' – suggested by Popper's writing at different points. Miller is absolutely right that 'the decisions we make about what to accept and reject...are constrained throughout by our desire to discover the truth'.[190] (This is the deontological backbone of critical rationalism.) Yet Bartley himself says: 'One may go on to conjecture about which of the reports are accurate: but this is a conjecture, not a decision, and may itself be tested accordingly. Hence a theory may be *provisionally* and *conjecturally* rejected because it conflicts with some less problematic view.'[191] So I think the confusion here is just over the word 'decision'. Of course Bartley doesn't rule out our 'deciding' in the sense of 'making conjectures about what is right and wrong on the basis of discussion', let alone rule this out as *irrational*, but he does rule out our 'deciding' in the sense of 'treating something as if it is *conclusively* false'. This, since treating something as if it is *just* false – until it comes up for re-examination – is sufficient for practical purposes. Thus, I take it that Miller and Bartley – and I, for that matter – would agree here, after all. A quick way of putting this is just that all decisions are hypothetical, and open to criticism. But all decisions, themselves, are non-rational, *which is not to say that decision-making is non-rational, or irrational, or that requiring that decision-making take place (from time to time) is non-rational, or irrational.*[192] If this is still unclear, imagine a woman who has had a theory 'pop into her head', and has classified it as true in order to test it (and subject it to criticism), versus a man who has classified the same theory as true due to a difficult, and drawn out, process of decision-making (and is still willing to have it subjected to criticism). Does this mean the theory is 'rationally held' by the man, but not for the woman? No, it might be rationally held by both, and this is the sense in which the *decision itself* (but not *making that decision*) is non-rational. For we ought not to say the woman *should not* instead have classified the theory as false, but been willing to test it, and willing to revise *that classification* (by appropriate means). And for that matter, the man may have assessed things differently, but that would not be *irrational*. In sum, it is not how we *have* classified statements (or theories) that matters, but how (and when) we go about re-classifying that which we have classified: we ought not to swap classifications on mere whim.[193]

Let us move on to modes of criticism, though, given that some potential objects of criticism – statements, theories, systems of logic, and so forth – have been identified above. And here, I want to concentrate on *theories* as objects, since it would seem to be a

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[189] Bartley [1984], quoting Popper, p.214
[190] Ibid.
[191] Bartley [1984], p.216
[192] Remember my discussion of the dispute between Popper and Bartley, and introduction of the 'non-rational' element, above.
[193] There is a small possibility that Miller thinks we can just *choose* to make no decisions, or *choose* to have no beliefs whatsoever. But I do not believe we can make such choices (except in the trivial sense that one can kill oneself, or what have you); choices which would remove, as I see it, the very prerequisites of rationality. -Killing oneself does-not-make-one-irrational, but denudes one of the *potential for being* rational, although the choice to kill oneself may not be rationally made (on any given occasion). In other words, doxastic voluntarism can be true to a degree, without it being true that one can choose to believe literally nothing, or choose to make no decisions whatsoever.

reasonable posit that any discipline will involve these: metaphysics, as much as physics or chemistry.

Now as our starting point, we might look at a suggestion of Bartley's:

> We have at least four means for eliminating error by criticizing our conjectures and speculations. These checks are listed in descending order according to their importance and to the rigor with which they may be applied:
>
> (1) The check of *logic*: Is the theory in question consistent?
>
> (2) The check of *sense observation*: Is the theory *empirically* refutable by some sense observation? And if it is, do we know of any refutation of it?
>
> (3) The check of *scientific theory*: Is the theory, whether or not in conflict with sense observation, in conflict with any scientific hypotheses?
>
> (4) The check of the *problem*: What problem is the theory intended to solve? Does it do so successfully?[194]

This is some sort of start. But first, I do not agree that any one of these tests is more 'important' than any other; this, for a theory is either true or false, and any means by which it can be excluded, if false, is surely as 'important' as any other. Second, I am not really sure about what role the 'scientific' plays in the third check: indeed, in so far as there is an implication that checking any theory versus a 'scientific' theory is more important than checking it against any other theory held in the light of critical discussion, I do not like the implications. Why not make mention of metaphysical or mathematical theories, in this regard?

Before being too unfair to Bartley, though, I should mention that in the second edition of *The Retreat to Commitment*, he admits that 'Where such theories are brought into clash with scientific theories, and thus are criticizable in terms of these scientific theories, one must not assume too readily, however, that the observation-irrefutable but theory-refutable statement is wrong and the observation-refutable scientific hypothesis is right.'[195] This is what one would expect from the architect of pancritical rationalism. Indeed we might also expect, and be glad to find from his personal correspondence, that he believed 'there is a "check of metaphysics", as well as a check on metaphysics'.[196] And this being the case, we might well want to change (3) to a check on the *external consistency* of a given theory. Call this (3*).

Still, I take it that this model of modes of criticism might be improved by invoking the distinction between *immanent*, and *transcendent*, criticism. Indeed, checks (1) and (4) might be thought of precisely as *immanent* (when we have opted to hold the system of

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[194] Bartley [1962], p.158
[195] Bartley [1984], p.204
[196] Appendix A.

logic constant), whereas checks (2) and (3*) are *transcendent*.[197] And we might ask, further, whether there could be other forms of immanent or transcendent criticism. Here are a few suggestions, and I draw on Kuhn's notion of 'theoretical virtues', mentioned earlier, in order to make some of them:

(5) The check of simplicity. Does the theory constitute the simplest available way to solve the problem it is designed to solve?

(6) The check of fruitfulness. Has the theory proven fruitful, in conjunction with other theories, in suggesting new approaches, disclosing unexpected observations, or serving as an auxiliary to allow their testing by observation?

(7) The check of conceivability (or intuition). Does the theory have any consequences which do not, in the current status of the debate, seem conceivable (or to be intuitable)?

(8) The check of possibility. Is the theory possibly true, in a *non-epistemic* sense?

Let me now say a few words about these suggestions, although more (say about scope, or accuracy), could be made. With respect to (5), which can be understood immanently and transcendently, simplicity is, at the very least, a *pragmatic* virtue. This is to say, at a bare minimum, that the simpler of two theories is better, but only *ceteris paribus*. But might simplicity not also be an epistemic, in so far as truth-conducive, virtue, even if only in *ceteris paribus* cases of comparison? I think the critical rationalist can remain neutral, but entertain the possibility. (There is nothing 'inductive' about such a suggestion.)

As to (6), which is a transcendent check, it is sometimes the case that a particular theory seems to be responsible for great progress, and hence there is a sense in which it is natural to want to stick with it for longer, *ceteris paribus*. So this check is somewhat Lakatosian: it is effectively a check on whether the theory constitutes, or is a part of, (what currently seems to be, and have been) a progressive research programme.[198] Is the check merely pragmatic, though? I think the answer seems to lie in the affirmative, since it is clear that any false theory can nonetheless be useful in a limited class of applications. Yet it is equally clear that as long as its employment leads to great successes, it is not unreasonable to continue to employ it, *even if there are some serious suspicions about its falsity*. It should be added that there is no tension with respect to the truth-goal, here. For example, we might be interested in the truth of the theory (T') that 'Use of theory T is still driving technology ahead'. And we might want to invite those with counter-theories (i.e. incompatible theories) to start employing those, if we suspect T to be false on the basis of other checks, in order that we see whether those can be equally as fruitful, or even more so.

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[197] Other than (4), there is the check of whether there is a better available solution. But I take that to be transcendent.

[198] There is some controversy about whether Lakatos 'stole' the idea of metaphysical research programmes from Popper's postscript to the Logic of Scientific Discovery, to which he had access before its publication. But his treatment does, at the very least, bridge some gaps between Popper and Kuhn; it is almost a *synthesis* of their views.

(7) and (8) I take to be linked, somewhat, since I think that there is – or rather, we ought to behave *as if* there is – a link between conceivability and possibility. This is a subject dealt with more fully in the next chapter, but it is worth noting that such a suggestion is not against the spirit of critical rationalism – since it does not involve the advocacy of radical intellectualism (*qua* the positing of 'firm foundations' for knowledge) – as Popper himself makes clear in writing:

> My opinion is that we can readily admit that we possess something which may be described as 'intellectual intuition'; or more precisely, that certain of our intellectual experiences may be thus described. Everybody who 'understands' an idea, or a point of view, or an arithmetical method, for instance, multiplication, in the sense that he has 'got the feel of it', might be said to understand that thing intuitively; and there are countless intellectual experiences of that kind. But I would insist, on the other hand, that these experiences, important as they may be for our scientific endeavours, can never serve to establish the truth of any idea or theory, however strongly somebody may feel intuitively, that it must be true, or it is 'self-evident'... Such intuitions cannot even serve as an argument, although they may encourage us to look for arguments.[199]

Prima facie, the final comment might seem damning. But it is not, because 'sense impressions' cannot serve as arguments *either*, even if there are such things, from Popper's, or more generally the critical rationalist's, perspective. On the contrary, it might be suggested that arguments *emerge* out of either intuition, sensory experience, or even a subtle interplay of the two. (Indeed, it may be doubted whether *observation* is possible without intuition, or by a being incapable of intuition. But then, the inverse may also be doubted, although I am, personally, somewhat less dubious on this score. Call it an intuition.) If our focus is to be on arguments, then let them be assessed on their own merits, no matter what their source. But if there is to be a check (2), then why not a check (7)? Or more plausibly, a check (8) in so far as what is intuitable *serves to reveal or enable* arguments for what is possible? Analogously, it may be said that 'sense-impressions', were there to be such things, could only *play a part* in allowing observation *statements* to be made; that is, only *play a part* in allowing a check (2)! I shall try to build on this in the next chapter, but should add right now that it seems to me that check (1) is a only special case of check (8), in so far as it is a check on *strict, or narrow* logical *possibility*. Check (8) is, it seems to me, far more fundamental. But I have said enough, here, to conclude the discussion of demarcation of *inquiry*, and will now return to the issue of *disciplines*.

Demarcating Disciplines

[M]y subject does not exist because subject matters in general do not exist. There are no subject matters; no branches of learning – or, rather, of inquiry: there are only problems, and the urge to solve them. A science such as botany or chemistry (or say, physical chemistry, or electrochemistry) is, I contend, merely an administrative unit... even serious students are misled by the myth of the subject. – Popper[200]

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[199] Popper [1945b], p.18
[200] Popper [1983], p.5

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For once, I employ the foregoing quotation because there is an important sense in which I strenuously disagree with it. Right, there are no 'subject matters' – subject matters do *not* exist – but I do think that there are different sorts of entities in the world. That is, entities which fall into *different ontological categories*, and not just different 'categories' *qua* classificatory 'concepts' of a merely conventional bent. Hence, I hold that there may be, and plausibly are, different 'forms of inquiry' in so far as they have different sorts of object. Yet further, the nature of the very objects under investigation must, I think, partially determine the suitable *practical means* by which the investigation of them is to be undertaken – this, given in addition, our capacities and limitations, *qua* human persons. In a trivial sense, I cannot undertake pure mathematics by wandering around looking for triangles to examine, or for infinitely long parallel lines to walk between. Nor, indeed, can I attain understanding the pharmacological effects of 3,4-methylenedioxymethamphetamine (a.k.a. MDMA) without partaking of it, or reading the report(s) of one who has. Indeed, in the latter case we might think I need to make observations, or examine observation reports – broadly, employ sensory faculties, examination of mood, and so forth – whereas in the former, these do not seem to be required.[201]

It would seem appropriate, here, to engage with Popper's arguments against 'essentialism', if only because many of the views I advocate have been prompted by his work. And although I do not want to do this at great length, because it will detract from the positive aspects of what I suggest, I think that his position is summed up rather well by the following:

> Methodological essentialism, i.e. the theory that it is the aim of science to reveal essences and describe them by means of definitions, can be…understood when contrasted with its opposite, *methodological nominalism*. Instead of aiming at finding out what a thing really is, and defining its true nature, methodological nominalism aims at describing how a thing behaves in various circumstances, and especially, whether there are any regularities in its behaviour. In other words, methodological nominalism sees the aim of science in the description of the things and events of our experience, and in an 'explanation' of these events, i.e. their description with the help of universal laws. And it sees in our language, and especially in those of its rules which distinguish properly constructed sentences from a mere heap of words, the great instrument of scientific description; words it considers rather as subsidiary tools for this task, and not as names of essences. The methodological nominalist will never think that a question like '*What is* energy?' or '*What is* movement?' or '*What is* an atom?' is an important question for physics; but he will attach importance to a question like: 'How can the energy of the sun be made useful?' or 'How does a planet move?' or 'Under what condition does an atom radiate light?'[202]

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[201] One might argue that an individual without sensory faculties could not come to grasp the notion of a triangle, but this is beside the point, and I might accept it despite being doubtful. For once 'triangle' is grasped, the examination of triangles, *qua* geometrical entities, becomes possible without observation, or employment of observation statements.

[202] Popper [1945a], p.30

I suspect, however, that I am not the only one to find this discussion somewhat confusing, particularly because Popper advocates what he calls 'metaphysical realism' – something like transcendental realism – elsewhere, such as in *Realism and the Aim of Science*. But we must notice that there are two distinct components to what he calls 'methodological essentialism': first, the view that things have a genuine essence, but second, that one should proceed by attempting to *define* the true nature of things. Yet I think that these two component views ought to be separated, as I shall now try to explain.

On the face of it, it would seem that there are three different ways to ask a 'what is' question. First, one may ask this in the sense that one *presumes* (is committed to the idea that) there is such a thing to be inquiring into the nature of. Yet second, one may ask this in the sense that one wonders if there really *is* such a thing to be inquiring into the nature of: one is trying to work that out. And third, one may ask it simply in the sense that one wants to understand the use of the relevant word, say in a particular context; this use is generally translatable into 'What do you *mean* by x?', or some such.

In order to disentangle this potential mess, though, we should note that to ask 'On the assumption that there are such things as electrons, what are their essential and accidental properties?' – the second sense of the 'what is' question – surely does not seem unreasonable. Indeed, it would seem curious to want to prevent scientists from making posits such as 'A necessary property of an electron is that it has a definite rest mass, y, the value of which we can investigate by means of an arbitrary unit of mass'; to suggest that there is something radically wrong about such an approach. But on the other hand, we can see the danger in one physicist claiming that another simply *wasn't talking about electrons any more*, because in their theory, 'electron' did not 'fit the definition' of 'having a definite rest mass, y'. Further, it would be danger for a physicist to just presume, or be committed to the idea, that there *really are electrons*, in the sense of the first question.

The point would seem to be, then, that we might agree with the thrust of Popper's objection to a particular *methodological* approach which is rather uncritical; but in spite of the problems with such an approach, this does not speak as to whether there *are* essences for us to discover. And to be more specific, we might discuss the potential uses of terms in language. For instance, is it unclear to me that particular terms can never *properly refer* to specific entities, perhaps even *rigidly designate* as suggested by Kripke. Yet likewise, it is unclear to me that sometimes we cannot use a term in the *sense of* an (implicit) definite description (viz. with such a description in mind, in a Neo-Russellian fashion), or even in the *sense of* the particular types of entity referred to by the term, *in virtue of their potential uses* (which would seem to be somewhat Wittgensteinian). Let us take a specific example: imagine I tell you that I can conceive of water not being H_2O. One of the first questions that should be asked is whether I take the term to be *reference-fixed*, or not.

There are difficulties here, no doubt, but I do not think it would do to exaggerate these. For instance, were I to be asked "Do you have a dog?", I would not reply "Actually, I have several", in virtue of the fact that I own several stuffed toys which are, in one sense

of the word, 'dogs'. (And the question 'Are toy dogs really dogs?' is, I think, silly.) Likewise, I do not know of any metaphysicians that suggest we ought to find a mere definition of 'property', say in the sense of 'real estate'! Instead, (revisionary) metaphysicians tend to abhor the notion that they are playing mere 'language games', or are involved in the simple 'clarification of concepts'. (They are violently opposed to the Wittgensteinian view of philosophy.) On the contrary, they take themselves to be engaged in the task of *distinguishing* between distinctions which are merely conventional and those that are not: and no-one, to the best of my understanding, is currently claiming that this is a matter of presenting mere 'definitions'. This aside, though, can they really do what they think they are doing? A reasonable view would seem to be that they cannot do any harm in the attempt, provided it is undertaken critically. And besides, even metaphysical *nominalists* need to posit *some sort* of ontological foundation(s); without doing so, they would find themselves precisely in the infinite regress (or circularity) of definitions which Popper employs as one of his arguments against methodological essentialism.

A clear example of what Popper inveighs against was presented to me this very day, while watching *Newsnight*. With respect to the contemporary crisis in Darfur, some politicians seem obsessed with working out whether what is happening fits the definition of 'genocide' adopted by the U.N. But this sort of semantic nonsense is abhorrent, given the situation; it doesn't even approach the real problem, or how we *ought* to respond to it. Indeed, one even has suspicions that to indulge in such a process – a quibble over 'genocide' – is part of the process of deflection: a flimsy veil thrown over a washing of hands, and an unwillingness to tackle the matters of genuine importance.

But putting this to one side, it amused me somewhat when I found the following footnote, which would seem to state clearly where Popper stood, and put an end to any confusion:

> I do not take up any position towards the *metaphysical* problem of universals, i.e. towards the metaphysical problem of nominalism versus essentialism (a term which I suggest should be used instead of the traditional term 'realism'); and I certainly do not advocate a metaphysical nominalism, although I advocate a methodological nominalism. The opposition between *nominalist* and *essentialist definitions* made in the text is an attempt to reconstruct the traditional distinction between 'verbal' and 'real' definitions.[203]

So by extension, we might think that Popper did not want to advocate any *metaphysical* position at all, with his comments on 'essentialism': not on the question of properties, but also not on the questions of relations, natural kinds, and so forth. All this would seem to remain open, and thus I will now return to the issue of demarcation, taking it that what I suggested in the first paragraph of this sub-section, above, is in no way in conflict with the critical rationalist view advocated beforehand.

But thankfully, I take it that there is now little need to say too much more. A proper discipline – not a 'discipline' *qua* useful administrative unit – ought to have its own *distinctive objects. This is the fundamental 'demarcation criterion' for disciplines,*

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[203] Popper [1945b], p.322, f.38.

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although such 'demarcation' is, I think, hardly very important. But further, in so far as there may be methodological differences, the nature of those objects (or more pertinently, our possible means of investigating them, in virtue of our nature and theirs) will serve to partially determine the *types of checks* employed in the discipline. This is a subsidiary 'demarcation criterion', in so far as it is parasitic on the first: for instance, there is no 'check of observation' (2) in pure mathematics, nor need there be. In fact, I take it that this should be pretty uncontroversial: we adapt our tools, critical or otherwise, to the task at hand (or the investigation being attempted, on our understanding of what might be out there). We have to have some idea of what we are looking for, before we work out how to look for it.

However, a result that I did not expect is that it seems plausible, under such a view, to say that there is no such discipline as 'science', and indeed no such discipline as 'philosophy', although there *is* history of those who have been labelled "scientists", and likewise "philosophers". (One can, of course, use discipline words in order to pick out groups of thinkers who work on similar sorts of problems, engage with one another's work, and so forth; but I am not interested in a socio-historical account of how the mere *conventions* of 'disciplines' emerge.) Yet on the other hand, there might be 'physics', 'metaphysics', 'chemistry', and so forth; that is, there would seem to be such distinctive disciplines, going by the status of our ongoing critical debate. And I should also point out that there are resources in my account to explain reduction: how 'acoustics' could have been found to have no distinctive object, in so far as one that was not already being studied in a different area of physics. But there couldn't be any *essential* 'scientific method' because there is no science in an *essential sense*. There couldn't be any *essential* 'philosophical method' because there is no philosophy in an *essential sense*. Rather, *inquiry is continuous*. And in saying this, I hope that I could hardly be accused of partisanship.

DESTRUCTIVE REALISM: NATURAL SCIENCE WITH A METAPHYSIC OF IMPOSSIBILITY

INTRODUCTION

The time has now come for me to attempt to be more creative, and this is an early indication that I will say much that is false – but mistakenly think is true – in this final chapter. After all, 'throwing spanners into the works' is the task of the critical rationalist, or more generally the Socratic philosopher, and this is really the limit of my skill, if I have any; only if I am lucky, will I chance upon a few hypotheses that are true.

Herein, I shall attempt to fulfil two distinct tasks. First, to give an overview of how metaphysics might be possible, and what it might involve, building upon the findings in the previous chapter. Second, to employ this view – which will be (and ought to be) little more than a sketch[1] – in order to provide a counter to some of the contemporary 'anti-metaphysics' arguments offered by van Fraassen, and in particular his *Empirical Stance*. And while I shall end without offering a tiresome definition of 'Destructive Realism', it should emerge that it involves a meeting of pancritical rationalism with a realist take on metaphysics – one which is deeply modal – but leaves much room for manoeuvre with respect to the fine details. After all, what I seek to motivate is a particular view on natural science, via something like an election manifesto: I do not want to suggest that it is entirely unproblematic, or to make it so specific that only I could find it appealing, or worthy of a bet.

I start by examining whether there might be any link between conceivability and possibility, and try to motivate not only the notion that the metaphysically possible is ideally conceivable, but also that the ideally conceivable is metaphysically possible. This, when that which is *ideally conceivable* is construed as that which is not only conceived of (by whomever), and formulated for public examination, but also stands up to all the criticism that is heaped upon it. My argument for this will be primarily epistemic, to wit that we ought to accept that this is the case – although it might not be – not only in order to motivate inquiry, but also to provide ourselves with a possibility space for our empirical investigations, aimed at uncovering what is *actually* the case. For were we to take (strict or narrow) logical possibility as our starting point, our task – our quest for the truth – would seem to be insurmountable. And needs must we try to eliminate some logical possibilities, as well as some potential actualities, if we are to have any chance of pressing ahead: of generating theories of greater verisimilitude, or at least recognising more false theories for what they are. The significance of all this might be summed up quite neatly in modal logic: $\sim\Diamond p \rightarrow \sim p$.

Second, I attempt to motivate the view that one of the things we ought to hold open to examination is our very modes of explanation, and provide a defence of the claim that

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[1] Like Popper, I think it best to oversimplify in order to give an outline of a methodology, rather than become embroiled in technicalities that distract attention away from the broader issues.

Aristotelian αιτία would still seem to be up to the task, when understood as *broad* 'becauses', as may have been implied by II.1. And if I am right, then it is interesting to note how long we have been employing these, and how there is a metaphysical heart to the very search for explanation, rather than mere *description*. As such, this would be another nail in the coffin of anti-metaphysical approaches to *inquiry*, let alone those to natural science: conventionalism, instrumentalism, constructive empiricism, and so forth.

Third, I should like to tie up my view of metaphysics, by providing an argument that metaphysical systems constitute one of the most useful means by which to criticise logical systems – in terms of II.4, that we can apply a 'check of metaphysics' to our logic (or logics) – just because logic is underpinned by metaphysics. This emerges through a discussion of the truth goal, *and in particular the theory of truth that we are to adopt*, which I take to have a radical effect on our potential models of how truth-preservation comes about, and how we might achieve this. In short, I shall urge that since the debate about *substantive* theories of truth can only be conducted on fundamentally metaphysical grounds, and the matter of which theory of truth we accept will affect which arguments we prefer, it turns out that logics, and particularly revisions to a given system of logic, are subsidiary to metaphysical considerations.

With my view of metaphysics motivated and partially outlined, I shall move on to tackle some of van Fraassen's recent arguments for empiricism, and the adoption of an *Empirical Stance*. In the fourth section, I will tackle the theoretical-observable distinction which has been historically central to empiricist accounts of science, and is vital to motivate either instrumentalism or constructive empiricism. Through a discussion of perception, I shall argue that this distinction has no significance whatsoever – indeed, that it is fallacious – although I shall maintain that there is an *ontological* distinction between 'observable entity' and 'non-observable entity', as well as an entirely different *epistemic* distinction between 'theoretical entity' and 'observed entity'. There is an extent to which this argument builds upon that in II.2, in so far as I take it that to appeal to sensory faculties as epistemic *authorities* is unacceptable.

In the final section, I will attempt to argue that realist metaphysics is in fact *compatible with* not only constructive empiricism, but also some sort of empirical stance, even if my arguments about perception are incorrect. In particular, my goal will be to defuse van Fraassen's arguments against such metaphysics from an immanent perspective, and show further how his invocation of 'stances' is insufficient to tackle the view of the pancritical rationalist who is also a metaphysician. As such, I shall endeavour to show that the empiricist toolbox provides no specific ammunition to tackle destructive realism, as opposed to scientific realism (which the destructive realist opposes for some shared reasons): indeed that destructive realism is a more austere position, in so far as it involves a complete rejection of ampliative inferences with respect to context of justification.

1. THE POSSIBILITY OF CONCEIVABILITY

I know that everything which I clearly and distinctly understand is capable of being created by God so as to correspond exactly with my understanding of it. – Descartes

The time has now come for me to offer my account of how metaphysics is possible. And since this account will itself be metaphysical, which is alas unavoidable, it must be taken as a premiss that we can and do perform metaphysics, as I have tried to suggest in the previous chapter.

However, my angle differs from that of many metaphysicians, for instance Lowe, whose approach is to 'get on and do' metaphysics, in the belief that the proof lies in the pudding.[2] For what I wish to do is to champion realist metaphysics from an epistemological, and rationalistic, perspective. To argue that reason is the *sine qua non* of not only agency, but also inquiry, in the manner so beautifully suggested by Blanshard:

> You are under the constraint of an order that you did not make. To the extent that you succeed in thinking rationally, your reasoning is under the control of a reason, in the sense of a framework of necessary relations, that is independent of you. You think as you do because it is not merely you that guides the thinking; you have surrendered yourself to an external and logical order along whose lines you find yourself carried... As Plato saw, [man] is in process of escape from a realm of flux into a timeless realm of essences, of mastering nature and his own human nature through understanding it.

> Philosophy, as I conceive it, is a persistent attempt to satisfy the theoretical impulse; such satisfaction is achieved only when the question Why? has been finally answered; and that answer in turn is achieved only with the grasp of necessity. But granting that thought is thus seeking necessity and can be fully satisfied with nothing less, how do we know the world will supply it? The fact is that the hunger for understanding no more guarantees an intelligible world than a hunger for caviar ensures all we want of it. May we not at any time stumble upon a thing or event that is so unconnected with anything around it as to be unintelligible – not only beyond our grasp at the moment, but essentially and forever inexplicable? The existence of such an element can hardly be denied a priori. On the other hand no hopelessly opaque surd seems yet to have appeared... And it would be gratuitous defeatism to launch the enterprise of understanding the world with the assumption that it cannot succeed. In raising the question Why? it is surely reasonable to assume there is an answer to be found, whether in fact we find it or not... unless at every step of the inquiry one can assume that intelligibility lies ahead, there would be no point in going on. The ultimate intelligibility of things is thus the working postulate of a rationalist philosophy... as Locke put it, God did not make man merely two-legged and leave it to Aristotle to make him rational...[3]

Now all this is just to provide some sort of orientation point; to make it clear which stance it is that I find myself in, although this is not entirely by design. Rather, it is as

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[2] In short, persuasion of 'the enemies of metaphysics' is not his end, nor does he think it ought to be. This is suggested by the manner in which Lowe [1998] and Lowe [2002] begin, and corroborated by personal discussions.

[3] Schilpp (ed.) [1980], pp.130-134.

a result of my optimistic approach to our epistemic predicament, and my views on perception, the means by which we learn, and (the limitations of) natural science. In particular, to pre-empt III.4, I hold that if perception fails us systematically but reason does not, then we are in a better position than we would be if reason were to fail us, just because perception *requires* reason; this, for as Blanshard puts it, 'perception is judgement... not mere sensation'.[4] Indeed, any mechanical thing can respond successfully to stimulae, with respect to preserving its function, or maintaining its existence, through its nature or pure 'Darwinian luck'. But we are not machines, or sophisticated forms of virus. We are sentient. We have a choice about what we do and do not believe, how we do and do not act, which brings with it responsibility.[5]

So in short, I would say it is rational to hope – on fundamentally pragmatic grounds – that we are powerful but nonetheless fallible, rather than powerless and infallible, like a stone or a Kantian human would seem to be. I would also add that rational hope-making is not rational-belief making: the direction of fit between hope and belief is quite different, albeit that one might very well want to trim one's hopes to believed possibilities, in most circumstances.

Conceivability and Transworld Modality

Oftentimes, arguments in philosophy – even in natural philosophy and everyday life – involve the following *a priori* move: one can conceive that p, therefore it is possible (in some sense) that p. Plausibly, such moves motivate many investigations, and are critical in decision-making: consider what motivated (in part) the search for Neptune, or what the man who wishes to park his car is doing when he slows it, and looks at the gap between two parked vehicles. Indeed, in the case of acts of imagination, which might be taken to be a sub-set of conceptions proper – here, I classify the former, for convenience, as acts involving the mental construction of one, or more, potential sensory perceptions – recent work in child psychology, involving children as young as two years old, has suggested that they are indispensable:

> it is not likely that children engage in imaginings that are not inferentially constrained and then learn somehow to constrain them... In one experiment children are shown a range of soft toy animals. There is also an empty cup which the children are encouraged to imagine is full of water. The cup is up-ended over one of the animals: the lion, say. The children will spontaneously imagine that the lion is wet, though the animal is in fact dry (since the up-ended cup was in fact empty) and no one has mentioned the idea of it being wet.[6]

But allow me to put such empirical claims aside, and start my task by introducing two operators, for much of that which follows will be couched in modal (or doxastic) logic, for purposes of clarity.[7] Let $\Box_t$ mean 'it is transworld necessary that', or 'it is the case in all possible worlds that'. Let $\Diamond_t$ mean 'it is transworld possible that', or 'it

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[4] Ibid., p.134.

[5] I am aware of the issue of doxastic involuntarism, but agree with Steup [2000] that we exert indirect control over our beliefs through many of our actions.

[6] Currie [2002], p.207.

[7] A *caveat*: I must emphasise that this is *purely* for purposes of clarity; as an accompaniment to the arguments in the text. I do not value formalism for its own sake, do not require the formalism for my arguments, and do not want to create the impression that I am *doing* formal logic, here.

is the case in some possible worlds that'. Now let S5 hold as far as these operators are concerned. For instance,

$$p \rightarrow \Diamond_t p$$
$$\Box_t p \rightarrow p$$
$$\Box_t p \rightarrow \Diamond_t p$$
$$\Box_t \Box_{t\dots} p \rightarrow \Box_t p$$
$$\Diamond_t \Diamond_{t\dots} p \rightarrow \Diamond_t p$$
$$\Box_t \Diamond_t p \rightarrow \Diamond_t p$$
$$\Diamond_t \Box_t p \rightarrow \Box_t p$$

Now all this is, I should hope, reasonably uncontroversial. (I am not going to enter into detailed criticisms of S5 logic here, for reasons that I hope shall become clear. For these operators, I will just take it that it is 'pretty much' right, since even if a slightly different system is required, this would not have a great deal of impact on my overarching model.) I call this S5 transworld modality, or S5TWM for short.

But next I want to introduce another operator, and specify its relation to those outlined above. I call it the conceivability operator, ©, and it stands for 'It is clearly and distinctly conceivable that'. Fundamentally, I have it that it obeys the following rule:

$$©p \rightarrow \Diamond_t p \qquad \text{Weak Conceivability-Possibility Axiom (WCPA)}$$

I call this a 'weak' axiom because it is not as strong as the one that will follow: it simply says that *if* something (say a state-of-affairs, which can be represented by a proposition) is clearly and distinctly conceivable, then it is the case in some possible worlds (or both some possible worlds and all possible worlds, since $\Box_t p \rightarrow \Diamond_t p$). For example, if a blue swan is clearly and distinctly conceived here, then the truth of 'There exists a blue swan' is thereby conceived, and hence there is at least one possible world in which 'There exists a blue swan' is true, because there is a blue swan therein. For if it is (actually) conceived that p, then it is clearly conceivable that p.

The most obvious objection to this is an old one: what distinguishes a 'clear and distinct' conception from one that is not, in fact, 'clear and distinct'? My answer is rather brief: all proper conceptions are 'clear and distinct', in the same way that all proper perceptions are 'clear and distinct'. And to this, I must add that I cannot easily explain in words what it is for one to have a 'clear and distinct' conception, any more than I can specify what it is for one to have a 'clear and distinct' perception. I cannot properly describe how foggy our experiences are when dreaming, or having partaken of LSD, any more than I can describe what it is to watch the sun setting over Durham cathedral while as sober as a judge. (This, although were I a sufficiently skilled writer, I could no doubt *evoke* the image for some readers.) Nevertheless, it might be thought that this claim is deeply problematic, in so far as one might claim to conceive of something – say the truth of a proposition – and therefore that it is possible, when one has not. To which I reply that there are many people who claim to see abominable snowmen, ghosts, or the Loch Ness monster, and therefore that these are *actual*; we do our best to test such claims as a community, and those about what can be conceived are open for investigation just as are those about what can be perceived.

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(Besides, I hold that perception requires a conceptual component in experience *anyway*. No perception without conception.) It should also go without saying that some may be able to perceive better (or less well) than others, and the same is the case with conception.

A second objection might be that I would suggest we are near-omnipotent, or that I would have us be more powerful, and insightful, than we really are. But to this it may be said that I have not directly mentioned humans at all, just yet: the axiom refers to genuine acts of conception, which may or may not be human in nature. And still further, in continuing to answer this second objection while tackling a potential third, I should answer those who would make the accusation that 'conceivable' is precisely a *modal notion* – Ladyman levels a similar charge at van Fraassen for his use of 'observable'[8] – by pointing out that I thoroughly agree. All that WCPA says is that *if* there is a possible world such that p is conceived in that world, atemporally speaking, then p is the case in some possible worlds. At the bare minimum, then, if p is conceived in just one world then p must be the case in at least one world – and this *may or may not* be the world in which p is, in fact, conceived.

A final potential objection is one to which I will return, and will continue to dog my efforts. How could it be the case that we could have a faculty such that we could, *inter alia*, stretch out to other 'possible worlds'? (That is, given that I will obviously want to introduce humanity into my picture at some stage.) At first blush, it might seem that this would necessitate the genuine existence of possible worlds, say as maximally consistent sets of propositions, maximally consistent possible states of affairs (as Plantinga would have it), or even spatio-temporally bounded separate 'universes' (as Lewis would have it). And in some of these cases, it might seem that we would need to have some sort of extra spatio-temporal ability, or power, to delve into these different worlds simply by entering into reflection, or by doing, for example, geometry. (Properly conceiving of particular possibilities may not always be a matter of mere reflection, of course.) I will put this aside for the moment, however, since I actually wish to go further than endorsing just the weak conceivability-possibility axiom, which can stand or fall irrespective of the following strong version:

$$©p \leftrightarrow \Diamond_t p \qquad \text{Strong Conceivability-Possibility Axiom (SCPA)}$$

Of course, if the WCPA raised the reader's eyebrow, then this is liable to raise both, and perhaps even the edge of the lip. For the SCPA says, *inter alia*, that if there is a possible world such that p, then there is also a possible world in which it is conceived that p. But before I continue, let me pause to reiterate what I said at the beginning of this section, and remind the reader of Blanshard's principle of rationalism: to emphasise, again, that I want to make the recommendation that we accept this thesis as true on an optimistic and pragmatic basis. So while I am quite happy to confess that I personally believe in the truth of this axiom, I do not hold forth my arguments as being sufficient to demonstrate such. Rather, I seek to motivate its *acceptance, on methodological grounds*, and the positive argument for this, which I call 'the argument from the killer possibility', is as follows:

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[8] Ladyman [2000].

If we take seriously the notion that there is a possibility that we could not ever recognise, then we must entertain the notion that its consequences might, likewise, be entirely inconceivable. But then, they might literally be anything; anything, that is, in so far as conceivable and inconceivable simultaneously – whether we could ever challenge the law of non-contradiction is irrelevant – or an amalgamation of conceivable and inconceivable elements. Now one barely conceivable consequence of all this is that we have opened up a can of worms: to admit just one inconceivable possibility can be, in virtue of our very inability to conceive of that possibility, to admit an infinite number of inconceivable possibilities. This, for that very inconceivable possibility might be precisely such that it entails, in a fashion we cannot comprehend, an infinite number of inconceivable possibilities. And so forth.

So the point, here, is that to admit just one inconceivable possibility might as well be to reject any link between conceivability and possibility, *even a partial one*. And as such, *even* scepticism about what would seem to be actual, Cartesian scepticism, would not seem to be a relevant concern. For why bother to talk about anything, from physical object to *malin genie*, when it is not a genuine possibility? Why even try to inquire, when inquiry is not even a genuine possibility, as far as we can tell? Why even give up on inquiry? Radical relativism would seem advisable (and perhaps not advisable); mysticism preferable (and perhaps not preferable).

With this infuriatingly weird argument given, however, let me first try to show a couple of basic results that follow from SCPA when it is combined with S5TWM:

$\Box_t p$	Premiss	
$\Box_t p \to \Diamond_t p$	S5TWM	Necessity-Conceivability
$©p \leftrightarrow \Diamond_t p$	SCPA	
$\therefore ©p$		

p	Premiss	
$p \to \Diamond_t p$	S5TWM	Actuality-Conceivability
$©p \leftrightarrow \Diamond_t p$	SCPA	
$\therefore ©p$		

I think that these theses speak for themselves; they knit together in a fashion that is elegant (even if completely wrong). So second, it will be useful to pick up on the effect of introducing negations into the SCPA, and to show precisely how this is not problematic, while also taking the opportunity to cite some specific examples of (what are plausibly) transworld laws. I will be concerned, here, with the following two results:

$©{\sim}p \leftrightarrow \Diamond_t{\sim}p$	SCPA Negative (SCPA-)

${\sim}©p \leftrightarrow {\sim}\Diamond_t p$	Negative SCPA (-SCPA)

(N.B. The negative sign at the end of SCPA denotes negation introduction with respect to the operands, and the negative sign at the beginning of SCPA denotes negation introduction with respect to the operators. Neither SCPA- or -SCPA should be confused with axioms themselves.)

Now let p be the following: 'There is an entity, such that said entity is a triangle, and said entity does not have three sides'. So if T stands for 'is a triangle', and 3 for 'has three sides', then p may be expressed as: $\exists x(Tx \ \& \sim3x)$. And let us take it – as is plausibly the case – that it is both conceivable that not-p and indeed not-conceivable that p. (Here, I make some sort of non-truth-preserving leap from 'it has not been conceived that' to 'it is not conceivable that', in the latter case, yet we must remain aware that I have not said anything which would support such a move, just yet. This is an outstanding issue.) Now from SCPA-, it follows that it is possible that 'There is not a triangle without three sides', and hence that there is at least one possible world in which there does not exist an entity such that said entity is a triangle, and does not have three sides; in logical notation, $\Diamond_t\{\sim\exists x(Tx \ \& \sim3x)\}$. But from –SCPA, it also follows that it is not transworld possible that 'There is a triangle without three sides'; in other words, that there is a triangle without three sides in any possible world; in logical notation $\sim\Diamond_t\{\exists x(Tx \ \& \sim3x)\}$, and from S5TWM, $\Box_t\{\sim\exists x(Tx \ \& \sim3x)\}$. In this case, then, $\Diamond_t\{\sim\exists x(Tx \ \& \sim3x)\}$ because $\Box_t\{\sim\exists x(Tx \ \& \sim3x)\}$, as we should expect from S5TWM.

But what of a statement necessarily true due to a law of logic, such as that of non-contradiction? Let p be 'Ferdy is a female fox and Ferdy is not a female fox', and our premises be $\copyright\sim p$ and $\sim\copyright p$. We have two results. From SCPA-, it follows that $\Diamond_t\sim p$; that is, it is the case in some worlds that 'It is not the case that Ferdy is a female fox and Ferdy is not a female fox'. From -SCPA, it follows that $\sim\Diamond_t p$; it is not the case in any possible worlds that 'Ferdy is a female fox and Ferdy is not a female fox'; thus, $\Box_t\sim p$.

Of course, all this should draw to our attention that the introduction of negation with respect to both operator *and* operand has a similarly useful, and interesting, result:

$$\sim\copyright\sim p \leftrightarrow \sim\Diamond_t\sim p \qquad\qquad \text{-SCPA-}$$

This means that if it is not conceivable that it is not the case that p, then it is not the case that it is transworld possible that not-p. (And vice versa.) But this amounts precisely to saying that it *is* transworld necessary that p; that is to say, that it is the case that p is true in *all* possible worlds. (It is not conceivable that not-p, if p is true in all possible worlds.) For in logical terms, it is a consequence of S5TWM that $\sim\Diamond_t\sim p$ – there is not some world such that not p – entails $\Box_t p$. And hence that -SCPA- may be rewritten as $\sim\copyright\sim p \leftrightarrow \Box_t p$, just as -SCPA may be rewritten as $\sim\copyright p \leftrightarrow \Box_t\sim p$.

However, even given these examples, it remains plausible that there are entities (or states-of-affairs) that can be conceived of yet would seem to some to be impossible, and to be referred to as 'impossible' in common parlance. For example, I have been asked whether a two-headed swan (or 'There exists a two-headed swan') is conceivable, and when I affirmed that this is so, I was then asked a more interesting question: 'Is a swan with the same physical composition as a duck conceivable, and thus possible?'[9] My answer was unhesitatingly in the affirmative, since the question was open to several interpretations (and I selected the most lenient). What am I getting at? This brings me on to the next section of my account.

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[9] I owe these examples to Richard de Blacquiere-Clarkson.

How could 'a swan with the same physical composition as a duck' be construed? Well first, this may be taken to suggest that there is a possible world in which swans have the same physical composition as ducks (or in which one swan has the same physical composition as one duck) because the laws therein make this, or are consistent with this being, the case. I say this is possible, for example, because it could be a world such that a particular physical composition (or range of compositions) created a system whereby there was a propensity for the emergence of a swan, and a propensity for the emergence of a duck. (That is, such that one, or the other, must emerge.) Whatever else, this is not ruled out on *a priori* grounds.

However, this sort of claim brings issues of reference to bear. For by using 'swan' the questioner might take it that he rigidly designates – in a Kripkean sense – a particular natural kind in *this world*. And he may go on to claim that it is precisely an essential property of a swan (hence any swan) that it is has, say, 'swan DNA'. Likewise, he may take 'duck' in a similar fashion.

My answer to this begins with my pointing out that we were all quite capable of picking out swans, and referring to swans, before it was established – if it has been established – that swans have DNA. That is to say, swans were proper objects of inquiry well before DNA had even been conceived of, and there were no significant problems (as far as we are aware) in recognising them. So my point (in answering in the affirmative to the overarching question) must, then, be understood as follows: we could have found out that swans did *not* have DNA. (I must emphasise that I am not conflating epistemic and metaphysical possibilities here; by 'we' I mean something like 'our counterparts in different possible worlds'.) That what we called 'swans', or even better 'the bodies of swans' were, instead, otherwise composed. Against the Kripkean quibble, I can simply say that *if* it is supposedly the case that it is 'metaphysically impossible' (*a posteriori*) for what we once meant by 'swan' to not have DNA, then there could only ever have been one metaphysical possibility to consider, in this regard. As such, it would be a mystery to me why anyone should want to say that realist metaphysics had anything to do with, let alone was necessary for, the practice of natural science. Rather, would it not be the case that people conceived of a lot of metaphysically impossible things, but could only discover they were impossible *by experience*? If so, then 'metaphysically impossible' would seem to be readily substituted by 'false' – at least in this sort of instance – and this is precisely what the austere empiricist wants to hear. (If *this* is the account, then I think one may as well be an empiricist. One may say that we have *mere imaginings* that are confronted by experience, and that experience is sufficient to enable us to rule some of them out *as false*. No 'special science' of metaphysics, based on distinct capacities, in required in such a picture.[10])

My point is that swans admit of a great deal of variance. Not all swans need have feathers, have two wings, and so forth, in order to be swans. So why, then, should it

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[10] This is only supposed to be an argument for my view when it is opposed to the Kripkean or Lowean alternatives, of course.

be metaphysically necessary that they have DNA? (It is not even as if this alone marks out a swan, rather than the dead body of a swan, after all.) Might it not be the case, instead, that there is an ideal form of the swan, and a variety of entities that resemble that form to a greater or lesser extent? (This is meant to be as Platonic as it sounds, although I do not want to suggest that such forms are world transcendent, or supernatural.) Under such a view, the important question is one of how far an entity must diverge from such an ideal, with respect to its properties (and so forth), in order to cease to be a swan. And although there are grey areas here, some things do seem to be clear. For example, a change of colour, or size, is not obviously sufficient – and I urge the reader, again, to cast aside any presuppositions about DNA, or so forth – to make a swan into a non-swan. Removal of *anima*, or the ability to 'self-move', as well as undergo any substantial change whatsoever, would seem on the other hand to deny the entity in question its very classification as an organism, as Lowe argues.[11] Further, and again with sympathy to Lowe – he makes this point in his [1987] – it might be added that to be a swan is precisely to have a particular class of dispositions, and then asked whether those dispositions are dependent upon a particular (and just one) class of physical compositions. Yet while the answer seems to lie in the negative, *in so far as conceivability is concerned*, it is plausibly the case that such dispositions *are* associated with a particular class of physical compositions in this, the actual world, and may be associated with different such classes in others. (To which I add that I cannot conceive of a swan that does not exist 'in' space and time – that is not substantial.)

It might be thought that I have not taken the possibility of semantic externalism seriously, but have instead assumed semantic internalism. However, I hold that such an accusation could only be founded on the dogma that externalism – the view that some concepts (or, according to Haukioja [2006], propositional contents of intentional states) are partially (or wholly) constituted by factors external to an agent – is a thesis about factors that are not only *external* to the agent in a straightforward mereological sense, but also a *physical* sense. In other words, I take it that contemporary externalism is founded on the idea that it is *causal interaction* with instances of physical objects which are required to grant concepts of those very same objects. Against this, I have urged that the factors – e.g. the *form of swan-hood* – can be perfectly external, as well as immutable, and fully grasped not only on the basis that a particular *which instantiates that form* is encountered, but also on other experiential bases. I do not need to have encountered a Cyclops to grasp the form thereof, any more than I need encounter a cow in order to grasp the form thereof. If I can grasp the form of unicorn-hood without encountering unicorns – and notice that I say this because if possibility is to map onto conceivability, we must account for the possibility of unicorns, given the apparent fact that we can conceive of unicorns – then I am already in a position to recognise a unicorn.[12] But I cannot have grasped that form on the basis of anyone encountering a unicorn, provided it is correct that there are not, and have never been, any unicorns in our world (as of yet). In short, then, what I am suggesting might be thought of as a semantic externalism about *forms*, but not necessarily particulars, although it might be the case that an encounter with a particular that instantiates a given form is responsible for helping one to come

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[11] See, for example, the discussion in Lowe [1998], pp.185-187.
[12] Compare Kripke [1972], p.4 and Kripke [1980], pp.157-158.

into contact with the form.[13] Notice that one of the consequences of my view is that there can be a form (or kind) in a world where there is nothing that instantiates it (or are no members of the kind), although this might seem somewhat counterintuitive. My (Platonic) view is that the *metaphysical potential* for there being such instances is provided by the forms, the existence of which is necessary.[14] But this is not enough to establish the *physical* (or nomological) *potential*, as should become clear.

To return to the initial question, then, under different interpretations. Is there a possible world where there are ducks that have the same physical composition as swans do here, or vice versa? My answer is yes. Is it possible that a swan *here* have the same physical composition as a duck *here*? No, if the laws of physics (and chemistry and biology) here are similar (or the same as) those still unfalsified in contemporary science. This leads me into *nomological possibility*, which is a notion that Lowe would thoroughly, but I think wrongly, reject.[15]

It is time to return to modal logic, in order to explicate my view. Let $\Box_n\{w\}$ denote 'it is nomologically necessary, in w, that', which means 'There is a law in possible world w such that'. Let $\Diamond_n\{w\}$ denote 'it is nomologically possible, in w, that', which means 'It is consistent with the laws in possible world w that'. Now let us have it that (something like) S4 modal logic holds, with respect to these operators, and that A denotes the actual world. What I am saying, *inter alia*, is that:

$$p \to \Diamond_n\{A\}p$$
$$\Box_n\{A\}p \to p$$
$$\Box_n\{w\}p \to \Diamond_n\{w\}p$$
$$\Box_n\{w\}\Box_n\{w\}\ldots p \to \Box_n\{w\}p$$
$$\Diamond_n\{w\}\Diamond_n\{w\}\ldots p \to \Diamond_n\{w\}p$$

First, it will be noticed that A is employed in the first two equations, whereas w is employed in the remainder. Why is this? It is because 'p', as typically employed in such logical systems, is normally taken to mean *precisely* 'it is actually the case that p', whereas here this is insufficient. So I shall adopt the following revision: $p\{w\}$ means 'it is the case in world w that p' (or p is true in world w). This, in order that I might add the following:

$$p\{w\} \to \Diamond_n\{w\}p$$
$$\Box_n\{w\}p \to p\{w\}$$

Second, I should also like to add that I want to have it that $\Box_n\{w\}\Diamond_n\{w\}p \to \Diamond_n\{w\}p$ (based on S5) does hold, but on the other hand that I am not so sure about $\Diamond_n\{w\}\Box_n\{w\}p \to \Box_n\{w\}p$. This is because a law in world w such that 'It is consistent

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[13] Of course, it is completely open to dispute whether I can, in fact, conceive of unicorns. It might be that I am conceiving of horses with horns, or some such. So all I am doing here is developing a system that is consistent with what would seem to be conceivable, intuitively speaking; and in doing so, I take it that I am making my task harder than it would otherwise be. Of course, it should be added that there are other potential routes to address these problems. For instance, one could invoke a distinction between relational and notional contents of thoughts, or something similar, as does Chalmers [1995].

[14] One of the consequences of this view is that I reject the Aristotelian picture according to which the existence of primary substances, or physical objects, is necessary.

[15] I base this on personal discussions.

with the laws in world w that p' seems defunct (and hence it might as well be said 'It is consistent with the laws in world w that p'), but 'It is consistent with the laws in world w that there is a law in world w such that p' is more tricky, *when it is taken to be atemporal talk.* For it could be suggested that any countable (even infinite) number of 'levels of laws' is allowed *in principle* by refusing to allow reduction here, and I think this is an option worth leaving open. (To allow the system to deal with this is not to suggest that there really are multiple 'levels of laws', of course.)

But now I should like to raise a problematic idea. It is as follows: if it is *just* consistent with the laws in a given world (w) that p, then it might be thought that it ought also to be consistent with those same laws that not-p. Indeed, in a court case a lawyer might say 'It is consistent with the forensic evidence that the accused committed the crime' – for example, let us say that the accused has group A blood and blonde hair, and that a few specks of group A blood and a number of blonde hairs were found at the scene of the crime of which she is accused – when the evidence (or the accepted basic statements) is insufficient to exclude the possibility that she did, in fact, commit the crime. But then again, it is also insufficient, in the example given, to show that she *did* commit the crime; this, given the reasonably obvious premiss that there are many people with group A blood and blonde hair. So it might be argued, on the basis of this sort of intuition, that the foregoing system is incorrect. Instead, it might be thought that the following holds:

$$\Diamond_n p\{w\} \leftrightarrow \Diamond_n \sim p\{w\} \qquad \text{Consistency Axiom (CA)}^{16}$$

I take this quite seriously, in the same way that when someone says it is possible for me to catch a flight, I take it that it is also possible for me *not* to catch said flight, and it is neither necessary that I do, or that I do not. Yet it is clear that CA violates the foregoing system based on S4, and I should therefore like to suggest that an entirely different system of logic is required in order to capture the notion of *mere consistency.* This is as follows, given both CA and $\Box_n\{w\}\Diamond_n\{w\}p \rightarrow \Diamond_n\{w\}p$:

(i) $p\{w\} \rightarrow \Box_n\{w\}p \lor \Diamond_n\{w\}p$

(ii) $\Box_n\{w\}p \rightarrow p\{w\}$

(iii) $\Diamond_n\{w\}p \leftrightarrow \sim\Box_n\{w\}p \ \& \ \sim\Box_n\{w\}\sim p$

Let me explain this briefly. According to (i), if it is the case that p (in a given world), then there must be a law (or cluster of laws) in that world which *make it true*, viz. necessitate its truth therein, or it must merely be consistent with the laws in said world. And if it is merely consistent with those laws then p or not-p are mutually exclusive but *genuine possibilities* in that world; for from CA, if it is consistent that p then it is also consistent that not-p, and from (iii), if it consistent that p then there is no law (or cluster of laws) that necessitates either p or not-p. But perhaps I should explain more clearly why it is that I should advocate such a system:

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[16] Another way to think of *consistency* in this sense is *permissibility.* A legal analogy may be useful: the laws in the U.K. permit us to drink coffee, and permit us not to drink coffee.

If it is just nomologically possible that p, in a given world, then there must be another world that differs *only* in respect of the truth-value of p (or p until a specific time therein) – this is a *façon de parler* to the extent that there will be something different about that world which *makes it the case* that p has a different truth-value – but is otherwise maximally similar. For example, if we adopt an indeterministic interpretation of non-relativistic quantum mechanics, such that the Stern-Gerlach measurement of the spin of an electron along a particular axis has a propensity of 0.5 to give each of the two possible results (positive or negative spin of ½), we might say that for each such experiment, there is a world in which each of the results occur. (Imagine there were only one such experiment ever performed here: there would be another world that had an identical history up to the time the experiment was performed, *and in which the same laws held*, but in which the experiment gave a different – the other nomologically possible – result.) In short, it might be consistent with the laws herein that event *a* (or outcome *a*) occurs in a given experiment, and it might also be consistent with the laws herein that event *b* (or outcome *b*) occurs in that very same experiment. (And if the two outcomes are collectively exhaustive and mutually exclusive, *according to a law*, then it cannot be the case that 'Event *a* occurs and event *b* occurs'.)

Henceforth, I shall call this system R.[17] And let me continue by introducing some rules that interrelate R with S5TWM, since we already know how SCPA is related to this:

(α) $\Box_t p \rightarrow (\Box_n\{w\}p \lor \Diamond_n\{w\}p)$ & $(\Box_n\{w_l\}p \lor \Diamond_n\{w_l\}p$ & ...

What (α) says is that if it is the case in all worlds that p, then in each world it is either consistent with the laws therein that, or necessary given the laws therein that, p. (If preferred, we may express this as $\Box_t p \rightarrow \Box_t(\Box_n p \lor \Diamond_n p)$.) This includes the actual world, such that if it is the case in all worlds that p, then the laws in the actual world *must* either necessitate, or be merely consistent with, p. If preferred, the point may be stated (indeed, derived from the axioms in S5TWM and R) as follows: $\Box_t p \rightarrow p\{A\}$, $p\{A\} \rightarrow \Box_n\{A\}p \lor \Diamond_n\{A\}p$, thus $\Box_t p \rightarrow \Box_n\{A\}p \lor \Diamond_n\{A\}p$. This is also equivalent to $\Box_t p \rightarrow \sim\Box_n\{A\}\sim p$, since $\Diamond_n\{A\}p \rightarrow \sim\Box_n\{w\}\sim p$, from (iii); to there not being a law in the actual world which makes p not the case.

But what if it is nomologically necessary that p, in a given world; if there is a law in that world such that p (or that makes p the case)? Well, we might notice that since p will therefore be the case in that world, from (ii), it must follow that there are *some* worlds such that p. This result may be expressed as follows:

(β) $\Box_n\{w\}p \rightarrow \Diamond_t p$

We might expand upon this by noting, further, that if there is a world *w* in which there is a law such that p, and another world w_l in which there is a law such that not-p, then it follows that it is not the case that p in all possible worlds, but is the case that p in some possible worlds. In logical terms: $\Box_n\{w\}p$ & $\Box_n\{w_l\}p \rightarrow \Diamond_t p$ & $\sim\Box_t p$. (And we can imagine a wide range of such formulae, each spanning a different number of worlds, which can be constructed in a similar fashion.)

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[17] I have no idea whether it is novel, but suspect it is not.

When it comes to considering transworld possibility, though, the required axiom would seem to be rather uninteresting. For from the mere fact that there are some worlds in which it is the case that p, it need not follow either that p holds in any given world, or indeed that p is consistent with the laws therein. To return to the earlier example of the blue swan, the mere fact that there is a world containing such a swan does not mean that it is possible *in a nomological sense* for there to be such a swan in the actual world. The following would seem to capture this intuition (and *w* may be substituted for *A*):

(γ) $\Diamond_t p \rightarrow \Box_n \{A\}p \vee \Diamond_n \{A\}p \vee \Box_n \{A\} \sim p$

Yet on the other hand, if it is nomologically possible that p, in a given world, does this entail anything more substantive with respect to transworld modality? *Prima facie*, it might seem not. In particular, and in close analogy to (γ), it might be thought that the following axiom ought to hold: $\Diamond_n \{A\}p \rightarrow \Box_t p \vee \Diamond_t p \vee \Box_t \sim p$. I want to deny this, however, as should be suggested by my earlier example of the Stern-Gerlach experiment:

(δ) $\Diamond_n \{A\}p \rightarrow \Diamond_t p \ \& \sim \Box_t p$

If preferred, we may write this as $\Diamond_n \{A\}p \rightarrow \Diamond_t p \ \& \ \Diamond_t \sim p$. But hold on. If this is correct, then (α) is too weak. Why? Again, because for each world where $\Diamond_n p \ \& \ p$, there is a (maximally similar) world where $\Diamond_n p \ \& \sim p$. So if it is the case that p in every world, then there cannot be a world in which it is nomologically possible that p. In other words, (α) must therefore be replaced with:

(α*) $\Box_t p \rightarrow \Box_n \{w\}p \ \& \ \Box_n \{w_l\}p \ \& \ ...$

This is a rather interesting result, which allows us to claw back some hope for determining nomological necessities.

Prima Facie Conceivability versus Ideal Conceivability

Even given S5TWM, SCPA, and R, it is clear just how difficult our task of discovering laws – those nomological ones which just so happen to hold here, and those transworld considerations (which may be thought of as 'higher level laws') that constrain them – would seem to be. Yet miraculously, it would also seem that we enjoy considerable successes in our inquiry. And if those apparent successes are genuine, then it would seem that we are either extremely lucky – that we are regularly struck by lightning – or really are gifted with some form of intuition that serves to allow us to delimit possibilities, in addition to those sensory faculties that allow us to select between delimited possibilities. If liked, I invoke something similar to Putnam's 'No Miracles' argument, here: if inductivism is wrong, conventionalism is false, observations are theory-laden such that 'facts' are not just handed to us on a plate, and 'the truth is hidden in the deep', then how do we so well without an intuitive grip on possibility space? That is, without something a little special, at the level of the *context of discovery*, with respect to modality?

Of course, I frankly admit that we might just be lucky, and maintain that only *context of justification* is relevant with respect to the logical analysis of objective knowledge. But I believe in intuition, given that I also believe we have made considerable progress in discovering truths, particularly in non-empirical domains such as pure mathematics; indeed, such a 'No Miracles' argument seems to me to be persuasive, although it is clearly not compelling. But if I am right in the foregoing, how should we proceed in metaphysics? How are we to perform the check on conceivability (7), and thus the check on possibility (8), which I suggested in II.4? Allow me to return to the analogy between observation statements and conception statements, in order to answer this:

Imagine an individual were to claim that she had seen an abominable snowman, while walking in the Alps. How are we to test this statement? Well first, we might ask for a specification of the alleged 'snowman'. How did it appear? Was it tall, or short? Was it of anthropomorphic form? What colour was it? Second, we might ask about the circumstances involved in the sighting. Was anyone else there to see it? How close was it, when seen? Was it seen clearly, and distinctly? (Was it foggy?) And third, we might ask about other possibilities that this woman had considered: about her personal level of conviction that she saw an abominable snowman, and about where possibility of error in this statement might lie. For instance, had she been drinking?

But the purpose of all these questions is just to understand, first, what precisely the 'abominable snowman' amounts to: to understand precisely what she is claiming. Second, to get some grip on the possibility space involved: to attempt to ascertain whether she may have seen something else she mistook for an 'abominable snowman', for instance a human. At this level, we are trying to work out whether the hypothesis that she really saw an 'abominable snowman' – understood in the fashion revealed by her answers to some of the questions – can stand up to non-empirical criticism, or empirical criticism based on observation statements already possessed. However, we are also trying to work out in what way the associated claim – there is an abominable snowman in the Alps – might be independently tested. For instance, were she to say it appeared to be a large hairy beast, around eight foot tall, we might extrapolate to the likely size of its footprints, and perform a search for them in order to test the hypothesis. Further, we might expect that such a creature would need to eat and sleep, *qua* animal, and consider what its potential food sources in the area could be, what sort of lair would be suitable, and so forth. In the end, we may find that what was *prima facie* observed was in fact not observed at all. But if it was observed, we would be sure it was observable.

Now analogously, an individual might claim that she had conceived (of the possibility) that the morning star was not the evening star. How are we to test *this* statement? Again, we might ask for a specification of the alleged stars in question. Does she intend 'evening star' to rigidly designate, or is she using it as a sign for a particular definite description which we will need to understand in order to fully grasp the putative conception? For instance, does 'star' really refer only to 'celestial body', in this instance? Again, we might ask about the circumstances involved in the conception: what else was envisaged?; were the same laws of nature that are currently thought to hold here also holding in the situation conceived of?; and so forth. Finally, we might also ask whether something else might, in fact, have been conceived of. For

example, might it have been the case that she was merely conceiving of 'evening star' having a different referent to 'morning star': that the possibility she was envisaging was *purely linguistic*?

Our means of testing will involve employing our own faculties of conception, just as our means of testing in the case of an observation statement (or an existential claim on the basis of such a statement) will involve employing our own faculties of perception (provided it passes the non-empirical tests). For instance, we might try to examine whether the consequences following from what was putatively conceived of cannot themselves be conceived of. In logical terms, we may determine that $\Diamond p \rightarrow \Diamond q$, but $\sim\Diamond q$ therefore $\sim\Diamond p$, since q cannot be conceived of, and is therefore classifiable as inconceivable. And naturally enough, what was *prima facie* conceived of may turn out not to have been conceived of at all; instead, it may be the case that something else was really conceived of, or even that there was no genuine act of conception at all. (Of course, in either of the foregoing examples, the individual could simply be lying.)

In closing, however, I ought to explain why I think it reasonable to classify a situation – one that might be represented by a proposition or collection of propositions, no doubt – as *inconceivable* just because it cannot, at a particular point in time, be conceived of. And this is where the analogy I have used above breaks down, in so far as it is not necessarily reasonable to classify something as unobservable just because it has not been observed. Indeed on the contrary, sometimes the very fact that we have not observed an entity that is metaphysically classified as observable, despite attempts to do so, is our very ground for believing that it does not exist.

What to say, then? Well it has to be frankly admitted that the move from 'It has not been conceived that p' to 'It is inconceivable that p' is out, even as a fair gamble. However, there is an alternative move, namely from 'It is not *presently* conceivable that p' *and* 'It has not been conceived that p' to 'It is inconceivable that p' which is somewhat better, in so far as to have grounds for saying the former, one would have to really *try* to conceive that p. And while this move is clearly not truth-preserving, I contend that it is pragmatically required *qua* epistemic heuristic. The primary argument for this claim is a *reductio* along the lines of my 'argument from the killer possibility', above. We all recognise that we might be mistaken in classifying p as inconceivable on the suggested basis, and should therefore be willing to reclassify p as conceivable if it is conceived of (or seems to be conceived of). This is vital. But if we shirk from the process of such classification then we will achieve only paralysis in inquiry. So as I suggested in chapter II, the lesson is just that we have to be willing to take some risks. And the risks are allowable just because we can make such classifications *tentatively*, and remain receptive to criticism.

I might add that those who would want to disagree with what I advocate will have to put something in its place, for we cannot really do without classifying some things as possible and others as impossible (in either transworld, or nomological, senses). I simply do not understand how one could say with their hand on their heart that they had no interest in what was possible, and how such an individual could genuinely -function-in-everyday life. For who is not interested in whether it is possible to survive after drinking a pint of household bleach, or to warm oneself by sitting close to a fire, and so forth?

2.   THE ROLE OF ARISTOTELIAN AITIA IN THE METHODOLOGY OF METAPHYSICS

[A] certain fatigue and boredom may have set in… the dispute has taken on a somewhat ritualistic air where neither party seems to gain any decisive advantage – Armstrong[18]

The debate between nominalists and metaphysical realists, which is still ongoing, has a long and distinguished history. Yet rarely, it seems, do metaphysicians engage in open discussion about the proper methods of metaphysics; instead, most are content to employ a somewhat loose form of argumentation, based primarily upon informal logic, and manipulation of language.

Oliver, however, is a welcome exception to this general rule.[19] For he has recently re-emphasised that the mere manipulation of language is surely insufficient to the task of deciding between metaphysical theories, just because to say that one sentence may be rewritten as another, e.g. 'This is a red apple' as 'This apple exemplifies redness', does not provide any reason to favour the first form over the second. Rather, one must look elsewhere for criteria of theory-choice in metaphysics: in particular, Oliver suggests, to considerations of explanatory power and economy.

One might wonder, here, if some sub-set of the virtues which Kuhn suggests for theories in natural philosophy – accuracy, simplicity, consistency, scope, and fruitfulness – might do the job.[20] In so far as metaphysics has an empirical dimension, qualitative accuracy of a theory is vital. Simplicity, although obviously a pragmatic consideration at the very least, might be thought by some to be a guide to truth-likeness.[21] Internal logical consistency of a theory is a basic requirement for our accepting it as a candidate, and external consistency between one metaphysical theory and the others which one holds is likewise expected. On the other hand, a theory with too great a scope is liable to make some metaphysicians suspicious; consider God as a truth-maker in Berkeley's philosophy. And similarly, few would suppose that metaphysical theories are often, or should be, fruitful in a Kuhnian sense: that they 'should disclose new phenomena or previously unnoted relationships between those already known.' That is, although *some* applied metaphysical theories can be fruitful, in common parlance, of new research findings.[22]

However, the appeal to theoretical virtues, taken alone, is surely not sufficient to tease out the salient differences between the disciplines of metaphysics and natural philosophy. For example, were one to ask a neuroscientist why it is that we sleep, one might expect a purely physiological answer: gamma-aminobutyric acid is released

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[18] Armstrong [2003]
[19] Oliver, A. [1996]
[20] Kuhn [1977], pp.321-322.
[21] Sober supports this view, although he 'denies that the justification of simplicity depends on this fact'. Sober [1975], p.168.
[22] For example, Shimony correctly describes the Aspect experiments, based on the EPR paradox, as 'experimental metaphysics'. Shimony [1989], p. 27.

from the ventrolateral preoptic area of the hypothalamus, and this inhibits brain activity. Were one to ask a philosopher, such as Aristotle, one might expect a very different sort of answer: 'its goal is the conservation of animals'.[23] But here, one should note that both sorts of answer are equally legitimate. For although it might seem curious for an individual to reply to the question 'Why are you tired?' by stating that a chemical had been released in her brain, rather than referring to circumstances in her life (such as having been awake for a long period of time, etc.), it is nonetheless a permissible, if pedantic and gauche, answer.

This suggests that the acceptable modes of explanation in natural philosophy need not exhaust those acceptable in metaphysics. Certainly, the metaphysician need not restrict herself to the form of explanation advocated by Hempel[24], nor indeed to an event based, or inference related, conception of causation; after all, this word has its root in the Latin 'causa', which may reasonably be understood as 'for the sake of', such as in the phrase 'honoris causa'. (As van Fraassen points out: 'The idea of causality in modern philosophy is that of a relation among events. Hence it cannot be identified with efficient causation, its nearest Aristotelian relative.'[25]) Indeed, the claim that explanation, in particular what is taken to constitute a good explanation, is based on not only fact and theory, but also *context*, seems reasonable enough.[26] For Bacon, for example, 'the final cause rather corrupts than advances the sciences', at least when assigned to other than 'human action[s]', although he nevertheless believes 'It is a correct position that 'true knowledge is knowledge by causes [in the sense of 'αιτία', presumably]'.[27] Yet I, for one, have no problem with the claim that flowers turn towards the sun in order that they might better nourish and support themselves; to maximise the energy that they derive from the light thereof. (This form of explanation is complementary to that involving photosynthesis, and the mathematical consideration that maximising the area of petals exposed to sunlight will maximise energy gained, etc.) Moreover, in point of fact, I simply do not know whether flowers have souls, but I remain open-minded on the issue. Etymologically speaking, the language that we employ is riddled with indications that such was once thought so; consider the root of 'animation'. And Kepler's idea of an *anima motrix* emitted from the sun did not seem to seriously impair his ability to make significant progress in natural philosophy.[28]

Of course, it cannot be denied that the prevalent form of explanation in the academy, at present, has a scientistic basis. And one finds many modern philosophers who are all too eager to reduce philosophy to the status of handmaiden to 'science'. Historically, such a move has precedence, for in the Middle Ages philosophy was expected to serve the needs of theology and the Church. However, as I argued in II.1, the move also has precedence in failure; and I would opine that critical thought, and trust in the faculties of the individual agent and communities of such agents – viz. in the capacities of human, *qua* reasoner – should remain our leading light. The only metamethodology suitable for our making progress, if we are capable of such, is

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[23] Aristotle, *On Sleep*, 455^{b}20-25.
[24] I refer to Hempel's deductive-nomological model of explanation. See Hempel [1965], pp.335-359.
[25] van Fraassen [1980], p.113.
[26] However, it should be emphasised that one need not accept van Fraassen's account of causation in order to accept such a claim. Ibid., ch. 5.
[27] Bacon [1620], Book II, Aphorism 2. (Bacon quotes from Aristotle's *Posterior Analytics*.)
[28] See Kuhn [1957], pp.214-216 & 245-249.

criticism; that is, in the sense of critical reflective thought, in which all putatively 'obvious' truths are ultimately questionable.

Furthermore, a hearty dose of humility would seem to recommend itself, at least if one thinks that lessons can profitably be drawn from history. I daresay that many Romans thought themselves to be highly 'advanced', in comparison to members of earlier societies which had faded away; indeed, the Roman invasion of Britain is still seen by many to be an act of 'civilisation'. All this is mistaken, although tempting. For one doubts the ingenuity of ancient humans at one's peril, if modern archaeological findings, such as those in Skara Brae, are to be lent any credence. Thus, cautious optimism seems a prudent compromise, and the proper role of metaphysical inquiry should be taken to be the critical examination and re-examination of all claims about the nature of being, *without* systemic bias.

Unsurprisingly, this brings us back to *Philosophus*, to the distinction between θεωρία and τέχνη, and to the notion that having explanations is constitutive of genuine knowledge (and hence indicative of wisdom).[29] To remind:

> With a view to action experience seems in no respect inferior to art, and we even see men of experience succeeding more than those who have theory without experience...But yet we think that *knowledge* and *understanding* belong to art rather than experience, and we suppose artists to be wiser than men of experience...and this is because the former know the cause, and the latter do not. Hence we think that the master-workers in each craft are more honourable and know in a truer sense and are wiser than the manual workers...we view them as being wiser not in virtue of being able to act, but of having having the theory for themselves and knowing the causes.[30]

> Knowledge is the object of our inquiry, and men do not think they know a thing till they have grasped the 'why' of it.[31]

Now these quotations suggest that 'αἰτία' should certainly not be read as '*cause*', rather than '*ground*' or '*explanation*'. That is, in so far as it is highly dubious to plug '*causa*' into natural philosophy of today, thereby derive what 'causation' really is, and then pronounce, in a naïve scientistic fashion, that Aristotle posited 'Four Causes'. (After all, Aristotle clearly states that Plato used only essential and material causes; and the latter posited Forms.[32]) So my claim is that an αἰτία is a *because*; and such an interpretation does, as I shall attempt to show, prove to be enlightening in a deep metaphysical sense.[33]

An Elucidation of the Four Becauses in a Metaphysical Context

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[29] There is an important *caveat*: a crude reading of Aristotle as a justificationist, in the sense that knowledge is 'justified true belief', is not advisable.

[30] Aristotle, *Metaphysics*, 981^{a}10-981^{b}10.

[31] Aristotle, *Physics*, 194^{b}15-25.

[32] Aristotle, *Metaphysics*, 988^{a}1-15.

[33] Ultimately, if my interpretation of Aristotle is held wanting, I still contend that it is valuable. After all, I am interested more in ideas than people here; and just as a poem may impart much that the poet did not intend, so may the work of a philosopher.

Typically, outlines of the four *becauses* involve examples drawn from a physical, or biological, context. For example: the material cause of the strength of a bridge might be the concrete from which it is constructed; the formal cause of a correct classification of a tasteless, transparent, and odourless substance as 'water' might be that it is a compound of hydrogen and oxygen, in a ratio of 2:1; the efficient cause of a cigarette's ignition might be one's holding it to a flame; and the final cause of sexual intercourse might be the propagation of one's species. However, it is clear that a more careful account of the *becauses* will be required if they are to be applied in metaphysics, and this is what I shall try to provide.

First, the material *because* concerns the internal nature of an entity (or category of entity) under discussion. Is it primitive, or compound? For example, how would an entity such as a given apple have to be, in order to display the properties, such as colour and shape, which it does? Need it merely be a bundle of particular things, viz. tropes, such as a particular shape, and particular colour? Or might it, instead, be composed of a substratum which partakes of, or displays, other entities, viz. universals? If the former, is some metaphorical 'glue' necessary to hold the particulars together? If the latter, then will one not need to consider the other entities, viz. universals, in a material sense?[34] In either case, how is it that the material of an apple is sufficient to support change?

Second, the efficient *because* concerns why an entity remains the same, or undergoes change, and the explanans is always *other entities* (and perhaps their material, etc.). For example, why does the relation 'greater than' necessarily obtain between the numbers two and one (as an ordered pair)? Is the answer not because two and one are themselves unchangeable? What of the relation 'distance between', when considered of Durham and London? Does this not change, given that Durham and London change in area? (Consider the merging of Buda and Pest.) Would the relation cease to exist, were Durham to suffer the fate of *Calleva Atrebatum*?[35] However, yet more fundamentally, to state an efficient cause might be to point to an entity (or category of entity) without which another could not *be*; that is, could not remain the same, in the more essential sense of *remaining period*. Could the triangle *be* without the line? Could the line *be* without the triangle? Is a green book dependent for its existence upon the universal of green? Or is the universal of green dependent for its existence upon at least one concrete particular that might exemplify it? (Perhaps the two are co-dependent.) The point, here, is that the notion of efficient *because* is sufficient to capture what might also be referred to as metaphysical dependency; indeed, along related lines, also to suggest the idea of 'supervenience'.

Third, the formal *because* concerns the identity-conditions of an entity (or category of entity), with regard, often, to generation and corruption. Why could it be that an apple can only support certain forms of change, while remaining an apple? That is, why is it that an apple is destroyed when eaten, but not in undergoing a change of colour while ripening upon a tree? (Is to say that an apple is rotten to point to changes in a related group properties of the apple, such as water content, shape, and colour, or is 'rotten' a property in itself?) What is essential to being an apple, or the ship *of Theseus*, or water? Or on a more fundamental metaphysical level, what is essential to

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[34] The idea, here, is that universals could be efficient *becauses* of properties; hence, their 'material' nature would also need to be considered. (Although the inverse is also a possibility.)

[35] This question leads into the formal *because*, as will become clear.

being a relation? Are all relations not dependent upon at least two other entities, for their existence? Do we not say that *Calleva Atrebatum* was, but is not, west of *Londinium*? And would we not want defend the claim that *Londinium* became, or is, London? (Change in designator need not, and plausibly does not, correspond to change in a metaphysical sense.)

Fourth, and (as is appropriate) finally, the final *because* concerns the ends for which, if any, something is true of an entity (or category of entity). Why does the apple provide nourishment, yet fall from the tree and decay if not eaten? Consider that it contains seeds, which may be spat out, or excreted, by one who eats the apple; else, they may find their way into nearby soil when the flesh of the apple decays. Similarly, what is the explanans of the shape of the sycamore seed? Is it not disposed to 'fly', viz. be easily carried over considerable distances by the wind? And might it be that these dispositions can be properly explained by appeal to the ends of trees: their will to reproduce?[36] Yet more, is it not the end of all life to reproduce? (Is there a lesson, here, to be learned about life itself?) If anything is to provide an ultimate account for the nexus of entities that are, their relationships, and how they integrate, then it is final *becauses*. Indeed, when one is asked a seemingly trivial question such as 'Why do we exist?', the only recourse seems to be to offer a final *because*, or to deny the question its intended meaning.[37] (Similarly, in dealing with theological problems such as the 'problem with evil', whether with respect to natural events, or the acts of humans.) *Prima facie*, it may seem strange to demand a final *because* in fundamental metaphysical matters, for example to attempt to answer the question 'To what end are universals exemplified?' The proof is in the pudding, however, and a claim such as 'universals will themselves to be exemplified, since that is the only manner in which they can reveal themselves to beings in space and time' is certainly compatible with the scholastic claim that a universal could not exist without something to exemplify it.

3.   ON INFERENCE, LOGIC, TRUTH, AND METAPHYSICS

All men have opinions, but few men think – Bishop Berkeley

Earlier herein, I have argued not only that it is doubtful that inductive practices exist – and dubious, therefore, that a Humean model of learning is sustainable – but also that inductive inferences are invalid, even if it a psychological fact that we are predisposed to employ them.[38] This said, I thoroughly reject the notion that we *must* use them, at least when we are engaged in critical reflective thought, on the rather straightforward

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[36] The other alternative, of course, is that a given tree exists just because it can reproduce successfully, and that the mechanisms employed were 'selected' in a Darwinian sense. Yet the plausibility of such a claim rests, *inter alia*, on a privileging of event causation, in a modern natural philosophical sense; and the metaphysician need not be bound by such a notion.

[37] Many a philosopher will have been asked such a question, say in a public house. Yet what I have found, in attempting to answer it in informal discussions, is that answers based on aught else than final *becauses* do not often seem to satisfy the questioner. (The slip to 'aliens put us here' merely shifts the question of the final *because* to their doing so. Of course, it then raises the question of why those aliens exist/existed. An infinite regress might follow.)

[38] Remember the *caveat*, that the latter is only (clearly) the case if truth is construed in a non-epistemic sense.

basis that I do not. It might be added that there is no reason to suppose that all our activities are geared towards discovering the truth, and it may well be the case, psychologically speaking, that we are prone to bouts of 'lazy thinking', which we should strive to guard against.

This aside, in considering inference the key insight is that it involves performing operations on truth-bearers – utterances, sentences, or propositions[39] – in order to issue, in result, different truth-bearers. The operands, in any given instance, are referred to as premises; the results of the operation are referred to as conclusions. Thus, a *rule of inference* is a means by which to move between one group of truth-bearers, and another. Here are two candidate rules, and examples of inferences based upon those rules:

Rule (1):   For any proposition p, if it is the case that p, then it is the case that not-p.

Premiss: The sky is green.

Conclusion: The sky is not green.

Rule (2):   For any conditional, if it is the case that its consequent is true, then it is the case that its antecedent is probably true.

Premises: If Ludwig Wittgenstein was a philosopher, then I am the Queen of Sheba.

    I am the Queen of Sheba.

Conclusion: Ludwig Wittgenstein was probably a philosopher.

The structure of argument is, then, clear. Or is it? Don't the rules, above, appear to be truth-bearers themselves? And might they not be thought of as premises? Although this might seem to be the case, *prima facie*, it is really a category error; a simple misunderstanding. For rules of inference are indubitably *expressible* by truth-bearers, but are not themselves truth-bearers. Similarly, what makes it right to assert "The sky is blue", in the actual world, is not just 'The sky is blue'.

This leads us to consider the τέλος of argument. For its purpose is not merely to generate new sentences, or propositions; a monkey sitting at a typewriter and pressing keys at random could succeed in such a task, yet haphazardly. As such, there is a great deal more to be said, and we might start with the recognition that when we offer an argument, we are often interested in the premises for one reason or another: we consider them to be special, of a fashion. But how so? Typically, because either we, or someone else, would hold that they were worthy of asseveration: sincere and solemn declaration. In other words, because someone holds them forth as 'true', and I

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[39] My personal view is that propositions are primary truth-bearers, and sentences are only derivatively truth-bearers, in so far as they provide the means by which to pick out propositions. Here, however, it is too early to draw this view into the discussion.

use the scare quotes to indicate that I do not want to assume any particular theory of truth, just yet. This is a useful starting point, and we might now look to the end for which rules of inference were formally laid down, by taking a brief detour through the history of ideas.

From Parmenides to NATO: The Significance of Truth

Since antiquity, it has been held by many scholars that the pursuit of truth is not only an imperative for the individual, but derivatively for humanity. Perhaps the first suggestion of this view may be found in Parmenides' poem *On Nature*, where a distinction is made between 'the Way of Truth', and 'the Way of Opinion':

> And you must ascertain everything –
> both the unmoving heart of well-rounded truth,
> and the opinions of mortals in which there is no true trust (*pistis*).
> But nevertheless you will learn these too.[40]

For Socrates, if one is to take Plato's *Apologia* to be significantly representative of his thought, there was an important distinction between being 'an accomplished speaker', viz. an expert in persuasion, and a 'man who speaks truth', the latter being preferable in an axiological sense.[41] And this thread is woven through Plato's dialogues, in which it is further developed. The mere aim of 'winning a dispute', and the acquisition of the associated art which enables this, is implicitly frowned upon by the philosopher from Elea, in the *Sophist*; he helps Theaetetus to reach the conclusion that the sophist only has expertise in 'the word-juggling part of production that's marked off as human and not divine.'[42] But in stark contrast, as beautifully expressed in the *Phaedrus*:

> The reason there is so much eagerness to see the plain where truth stands is that this pasture has the grass that is the right food for the best part of the soul, and it is the nature of the wings that lift up the soul to be nourished by it.[43]

This is a core aspect of the intellectual legacy which Aristotle inherited, and systematised, with a specificity which was thitherto unparalleled, in his *Sophistical Refutations*. And the significance of this work, the *locus classicus* of the claim that all truth seeking requires a specific method, a particular 'set of tools', so to speak, is not to be underestimated. As I argue elsewhere herein, the promotion of this belief in the Latin West, and the associated move from faith in authority to trust in reason, was vital to prepare the ground for the 'Enlightenment era'.[44] (To reiterate, briefly: there are significant respects in which the Enlightenment involved movement *towards*, rather than away from, the Aristotelian method of inquiry, and its Socratic root, where this is not taken to be the use of syllogism specifically, but rather, more generally, advocacy of a critical approach to knowledge claims, involving deductive logic.) Moreover, indeed, any student of contemporary Western philosophy will still cover

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[40] See the discussion in Barnes [1979], ch. IX, section (a).
[41] Plato, *Apology*, 17b.
[42] Plato, *Sophist*, 268d.
[43] Plato, *Phaedrus*, 248b-248c.
[44] See II.1-II.1.3

highly derivative material regarding what constitutes a fallacy (or 'contentious argument') – the *petitio principii*, *ad hominem*, etc. – and be *instructed*, in a fashion which Aristotle might have described as didactic, that it is *essential* to the activity of philosophy that fallacies be avoided. In essence, these fallacies are based upon the core laws of Aristotelian analytics: the law of non-contradiction, the law of identity, the law of the excluded middle, and the law of rational inference (from that which is known, to that which is unknown). They are principles that underpin all inquiry, for Aristotle, *only* since the goal of inquiry is naught other than truth. That is to say, he holds that logic forms the core of all other disciplines, although each involves other principles, 'appropriate to... [the] subject'.[45] And 'that... which involves no action, i.e. that which is true or false... is absolute.'[46]

Given this, one might imagine my surprise at several conversations which I have had with fellow philosophers who have disputed the claim that the aim of philosophy is truth, yet have still appeared to abide, in non-trivial respects, by the same set of rules which were suggested by Aristotle. They are oft lightning-quick to detect a fallacy, and expose it, yet will hold that the aim of the activity is *not* truth. For example, according to Dr. X – the name has been omitted to protect the guilty – it is "the resolution of disagreements".

Now upon hearing this, my first reaction was to retort: "Is it *true* that the aim of philosophy is the resolution of disagreements?" However, while this response is a reasonable one, it remains insufficient in so far as it presupposes that truth is not constituted by something akin to 'resolution of disagreements'; and it is only fair to presume that Dr. X was aware that I used the word 'truth' in an absolute sense. (He should have a pretty good idea of my line.) But it seems to me that if one were to accept his point – his understanding of 'truth' – then the avoidance of fallacies would be of little concern; for plausibly, there are many rhetorical techniques that are suited to resolving disagreements, and bringing about consensus. Indeed, another strategy to achieve this aim would be to impose one's own beliefs on others. To deprive one's students of sleep, or to submit them to a regime of starvation, might aid in this goal; certainly some might believe that such actions were immoral beforehand, but they would soon 'learn' otherwise, under the tender ministrations of such a consensus seeker.

Granted, this example is rather extreme. But the lesson to be drawn from it is, I think, of paramount import. To a first approximation, it is as follows: what is a legitimate *rule of inference* is determined by the purpose for which argument is employed. And to a second approximation, if it can be agreed that the aim of argument is truth: what is a legitimate *rule of inference* is partially determined by what truth is, viz. by which theory of truth is itself true. But here, there might seem to be a problem. For is it not the case that any coherent theory of truth must be true, but only so according to its own account of truth? It seems so, and thus, talk of '*theories* of truth' appears to be misguided, if we are to avoid being pluralists about truth, given that theories are usually taken to be able to be compared on the basis of their relative verisimilitude. (The point: any one understanding of truth will be incorrect according to another.) Thus, I propose the following formulation:

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[45] Aristotle, *Sophistical Refutations*, 165^{b}1.
[46] Aristotle, *De Anima*, 431^{a}10.

First Principle of Truth-Seeking: One's understanding of truth partially determines the rules of inference that one takes to be acceptable in the pursuit of truth.

Now while I have not seen this claim made explicitly in any text – or if I have, I have forgotten – it seems so fundamental that I cannot bring myself to believe that it has not been explicitly stated before. And one of its most significant consequences is that how we understand truth-makers has a radical effect on how we think we should try to get at the truth. To hark back to my earlier rejection of ampliative inferences, for example, remember that I went to some effort to emphasise that my claim would not necessarily hold if truth were based purely upon coherence. Indeed, I argued only $AT \rightarrow \sim ET$.

Truth-Makers for the Alethic Realist: Logic as a Slave to Metaphysics

Nowhere in this thesis do I defend alethic realism, as such. But I do contend that since it is a prerequisite for any serious realism about natural science, *pace* the later Putnam, this is not strictly necessary. Rather, what we should be interested in, which accords with the project undertaken herein, is what *rules of inference* are appropriate for moving from truth-bearers that could be made true by the mind-independent and objective, to further truth-bearers that *would* be made true by the mind-independent and objective *were* the former to be made true by the same. This might seem like a clumsy way of writing, but the point is just that what we should be interested in is how *beings*, and *sortally distinct beings* (viz. beings falling into different categories), interrelate.

Now this being the case, mere concentration upon the possible *forms* of truth-bearers, and rules that relate those *forms*, will prove to be radically insufficient. Consider, for example, the following inference:

X is red all over
X is green all over
X is red all over and green all over

Deductively speaking this is valid; it involves use of the *conjunction introduction* rule, often denoted (&I). In terms of form alone, and in accordance with the canons of classical logic, it is an acceptable inference. However, most of us would be willing to concede that the two premises cannot both be true; that is to say, they are mutually exclusive. How so? Well we should notice that this is not simply a result of the definitions of 'red' and 'green', nor indeed 'red all over' and 'green all over', plus the laws of logic.[47] Rather, classical logic allows us to perform an operation that we should not be allowed to perform, when taking into account the *content* of the two propositions (or more generally, truth-bearers) – in particular, the possible states-of-affairs to which they refer, and would have as their truth-makers, were they to be true.

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[47] It might be thought, here, that I 'gloss over' this possibility. But I would encourage the reader to buy into the assumed account of truth, and try to provide a definition of colour which is not parasitic on claims about (posited) mind-independent and objective entities. (Reliance on contemporary physics is already ruled out.) For more on this, see Ewing [1951], ch.2.

Of course, it might be argued that this is no problem for logic: that if the premises were true, the conclusion would be true. But against this, I would point out that other examples, equally ridiculous, are not hard to find:

I believe that Frodo is a hobbit   (*Existential introduction*)
There is something that I believe is a hobbit

All dodos are extinct
Daisy was a dodo
Daisy was extinct

Jonathan Lowe is a good philosopher
Jonathan Lowe is good

We might want to adjust classical logic, but then we would find ourselves with an inference system that did not merely account for relations between sentences in virtue of their structure (or form). (Nothing like 'Free Logic', viz. logic 'free of existence assumptions with respect to its terms, general and *singular*', could help us here![48]) For all that could motivate such adjustments would be considerations relating to being, *qua* being; again, remember the understanding of truth operating here. Imagine, for a moment, that propositions exist, and are abstract entities. Then would not the relations between them, with respect to truth, be utterly dependent upon the relations between their truth-makers? (Even if propositions do not exist, the same would plausibly be the case for sentence types, although there would be pressing problems to be tackled – one of the explanatory benefits of propositions is that they account for syntactic and semantic ambiguities in the sentence types that we employ, another that they account for the inter-translatability of our languages.) Under such an understanding, it could *never* be the case that 'I am sitting at this moment and I am standing at this moment', because the state-of-affairs (or *fact*) that would make 'I am standing at this moment' true precludes the state-of-affairs (or *fact*) that would make 'I am sitting at this moment' true. In a similar fashion, a given triangle could not be both right-angled and isosceles, or both acute and obtuse.

And while we are thinking of what might exist, we might be rather suspicious about the existence of analytic propositions; it might seem that they could only ever be useful fictions. For example, the sentence 'All bachelors are unmarried men' really reduces to 'All unmarried men are unmarried men', and ends up being an consequence of a simple identity law, $P \equiv Q \rightarrow \forall x(Px \leftrightarrow Qx)$. The fact that there are two different ways of saying the same thing, in this case, is plausibly just an artefact of language; this, although it may have some pragmatic benefits. Philosophers often use words with the suffix '-ism' – alas, this thesis is full of them! – on pragmatic grounds; for while they detract from the perspicuity and precision of discourse, they facilitate concision.

To these issues – how we should understand truth-bearers and truth-makers – I will return. But what I wish to emphasise, here, is that *rules of inference* are, properly construed, just rules for moving from true premises to true conclusions; as such, they

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[48] See Lambert [2003], ch.8.

are not just within the province of logic, for the alethic realist. And let me marshal one further argument to this effect, which is due to Lowe:[49]

'It is not the case both that Daisy is a ditzy dodo and Daisy is not a ditzy dodo' – call this proposition α – is an instance of the logical law of non-contradiction. However, α is not itself a statement of said law. Thus, we need to think about the status of a further proposition, call it proposition β, 'α is an instance of the logical law that for any proposition p, it is not the case both that p and not-p.' Yet β is not a law of logic. And if β were to be taken to be a consequence of the laws of logic, then we could consider another proposition, γ, of the form 'β is an instance of the logical law(s) that $p_1, ..., p_n$.' An infinite regress looms, and Lowe concludes that:

> Strictly logically necessary propositions are ones that are true in virtue of the laws of logic alone – and sometimes this is a matter of the propositions in question being logical consequences of those laws: but true propositions stating the existence of logical consequence relations between other propositions are only *broadly* logically necessary – they express *metaphysical* necessities. The lesson would seem to be that logic, in the strict sense, is undergirded by metaphysics – as indeed is every other intellectual discipline.

Lowe goes a long way, here, but I do not have the faintest idea why any alethic realist should genuinely want to disagree, *even when alethic realism is understood in the minimal sense advocated by, say, Alston.*[50] Anyone who attaches a thesis to their understanding of truth that puts up a 'barrier' to truth-makers – rendering the identification of true statements, under their own conception, radically impossible – might as well be a sceptic. This, or they might want to rethink their account of truth; I offer this advice to the transcendental idealist – consider noumena – in particular.[51] "The truth is out there, but we cannot reach it" is verging on the incoherent, and only seems to motivate us to hang up our gloves. And not only is it an irrefutable thesis – a vice, not a virtue – but also thoroughly undesirable in a pragmatic sense; it is the philosophy of the pessimist, who has given up on all that he once held dear.

4.   THE OBSERVABILITY CRITERION

Is all that we see or seem
But a dream within a dream?
– Poe

We move, from the very start, in the field of intersubjectivity, of the give-and-take of proposals and of rational criticism. – Popper[52]

Appeals to the distinction between that which is 'observable' and that which is not were first employed by analysts of natural science, such as Mach and Poincaré, in order to challenge what is now called the semantic thesis (ST) of scientific realism.

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[49] Lowe [1998], ch.1, section 6.
[50] Alston [1996]
[51] See also the discussion towards the beginning of I.2.2.
[52] Popper [1983], p.87.

(In the literature, this is sometimes referred to as 'semantic realism'.[53]) And although the history of the resultant debate is long, convoluted, and rather painful to recount[54], it is clear that this distinction underpinned the approach to epistemology of science which dominated the first half of twentieth-century Western philosophy, particularly with respect to the realism issue. For the Vienna Circle, only verifiable statements were meaningful, and only 'observation statements' were verifiable; hence, only 'observation statements' were meaningful. Elsewhere, Ramsey undertook the task of showing how a scientific language devoid of 'theoretical terms' might be developed, and why such was to be preferred. And later, Craig's theorem was employed by some philosophers in order to argue that *any* class of terms that was deemed unwanted could indeed be dispensed with in scientific discourse.[55]

Even as late as 1966, we find a congruous position being advocated by Carnap. According to his syntactic view, the terms employed in scientific discourse are divisible into 'observational' and 'theoretical' groupings; 'observational' terms mapping onto 'observable' entities, and 'theoretical' terms mapping onto 'unobservable' (or 'non-observable') entities. Further, he distinguishes between 'empirical' and 'theoretical' laws. In his words:

> Empirical laws...are laws containing terms either directly observable by the senses [sic] or measurable by relatively simple techniques... The laws relating pressure, volume, and temperature of gases are of this type [e.g. Boyle's law]... The terms of a theoretical law do not refer to observables even when the physicist's wide meaning for what can be observed is adopted. They are laws about such entities as molecules, atoms, electrons, protons, electromagnetic fields, and others that cannot be measured in simple, direct ways.[56]

For Carnap, 'theoretical laws' are only testable in so far as they allow 'empirical laws' to be derived; it is 'empirical laws' that must then be compared with 'facts'.[57] And what is a 'fact'? Not much is clear other than it must be 'spatiotemporally specified', Carnap's only example being: '*the* expansion of this iron bar observed this morning at ten o'clock when it was heated'.[58]   No doubt this is a neat little ontology, which fits quite nicely with the way that many scientists talk, casually, about their practices. Unfortunately, as I shall subsequently show, it is not even wrong; rather, it is so vague as to be devoid of substance or value, at least as aught other than a heuristic for secondary school pupils, who are just coming to grips with laboratory work. Indeed, Carnap's obvious error in the antecedent passage seems to mirror the underlying mistake of language-based philosophy: he writes of terms when he should be writing of entities. He also writes of theoretical entities being 'measured', which seems to preclude their being *fictional*. Besides, neither the total volume of water in my body, nor the distance between the Earth and Pluto, can be measured in a 'simple' and 'direct' fashion. Are this volume and this distance therefore 'theoretical entities'?

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[53] Nagel [1950].

[54] Psillos [1999] provides a broad, if partisan, overview: ch.1-3.

[55] Craig [1956]; see also Hempel [1965], ch.8. It should be noted that this is only true in principle, not in practice; it is a logical result.

[56] Carnap [1962], pp.226-227.

[57] Carnap also claims that 'Correspondence Rules', such as 'The temperature...of a gas is proportional to the mean kinetic energy of its molecules', are vital in order to connect 'nonobservables' to 'observables'. Ibid., ch.24.

[58] Ibid., p.230.

Yet it would be wrong to suggest that there are many contemporary philosophers of science who are 'syntactic instrumentalists', or even 'neutralists' in the (later) Carnapian vein.[59] In part, this is because a semantic view of scientific theories has become more popular. Further, because it is widely accepted that the arguments for the semantic thesis of scientific realism (ST), such as those offered in Psillos [1999], are sound.

However, soon after these 'anti-realist' avenues were considered to have been closed, it became apparent, through the work of van Fraassen, that another was available.[60] Instead, one might take a semantic view of scientific discourse, and understand it literally, but hold that it is always permissible to remain agnostic about the existence of posited 'non-observables'. One might hold that it is possible to 'accept' such posited entities for all practical purposes (viz. behave *as if* they exist) without really committing to belief in them; further, that inference to the best explanation (or abduction) does not function when dealing with the 'non-observable' realm (e.g. when inferring from the 'observable' to the 'non-observable'). As such, my subsequent attack on the observable-unobservable distinction is an attack on both instrumentalism *and* constructive empiricism; said distinction underpins both, and neither can stand against the storm of realism, without this foundation.[61]

Now with respect to van Fraassen's position, consider first his statement of 'constructive empiricism': '*Science aims to give us theories which are empirically adequate; and acceptance of a theory involves as belief only that it is empirically adequate.*' Second, his statement that a theory is 'empirically adequate' if and only if it accounts for the observable(s): '…a theory is empirically adequate exactly if what it says about the observable things and events in this world is true – exactly if it 'saves the phenomena'.'[62]

But even upon repeated reading, I believe that these claims should seem very strange indeed, for what is true of the table that I am observing presently might be precisely that it is constructed, *inter alia*, of electrons. Indeed, I would suggest that normal language fails van Fraassen for a reason. A table is an observable thing, we all agree. A table might be composed of electrons, we all agree. (Van Fraassen endorses ST.) So van Fraassen cannot mean 'empirically adequate if… what it says about the observable things… is true', without countenancing the idea that the aim of science is to discover the truth about entities that he would classify as 'theoretical', e.g. electrons. Does he mean 'observable properties of observable things'? Well now we have a rather strange ontology beginning to emerge, which looks decidedly metaphysical. Moreover, notice that 'empirically adequate' cannot stretch to 'how observable things interact', but can involve only 'how observable things appear to interact'. This, for observable things may interact precisely in virtue of their unobservable components.

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[59] The latter position was a forerunner of contemporary structuralism. See Psillos [1999], p.41.

[60] I refer to both positions as 'anti-realist' because this is standard practice in contemporary philosophy of science. See, for example, van Fraassen [1980], section 1.2.

[61] It should be noted, however, that van Fraassen's new position, outlined in *The Empirical Stance*, might still be thought tenable. This is tackled in the next section, III.5.

[62] Ibid., p.12.

All this raises the suspicion that there is some sleight-of-hand at play here, whereby 'observable things and events' must be taken to be some sort of internal representations, or at least to dwell in a distinct 'realm of appearances', if van Fraassen's point is to be comprehensible; this, for that which is true of a representation need not be what is true of that which it represents. And even though Ladyman suggests that van Fraassen is really a *direct realist* about perception, we might remain somewhat suspicious, for he still seems stuck on the distinction between *that which is sensed* and *that which is responsible for that which is sensed*, and the epistemic significance of this, which I argued against in II.2.[63]

In any event, if the distinction between 'observable' and 'unobservable' collapses, at least in so far as it pertains to entities existent in space and time, then so does van Fraassen's account of science.[64] Therefore, I shall now take the opportunity to develop the discussion in the previous chapter, by tackling the issue of perception more directly.

New Dog, Old Tricks – The Empiricus Stance

The basis of van Fraassen's view is not as new as it might seem. Rather, modern empiricism –'constructive empiricism' being the exemplar – has much in common with the scepticism written about by Sextus Empiricus, in late antiquity.[65] For this scepticism, which might be better labelled *Pyrrhonism*, is nothing like the following, modern, notion:

For a given proposition, p, on philosophical grounds, one cannot decide (rationally) whether p or not-p.[66]

Instead, Empiricus urges that it cannot be doubted that there are *appearances*. What may be doubted is that which grounds what is φαινόμενος (apparent) – perhaps even *if* anything grounds it – and how one appearance relates to another:

> When we say that Sceptics do not hold beliefs, we do not take 'belief' in the sense in which some say, quite generally, that belief is acquiescing in something; for Sceptics assent to the feelings forced upon them by appearances – for example, they would not say, when heated or chilled, 'I think I am not heated (or: chilled)'.[67]

In other words, Empiricus does not advocate the idea that there is equipollence (ἰσοσθένεια) between arguments for, and against, there being appearances; that is,

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[63] Ladyman [2000]

[64] It might not seem unreasonable to suggest that abstracta are unobservable, if they exist; indeed, there is a strong metaphysical basis for such a contention, which presents a distinct line of demarcation between the 'observable' and the 'non-observable'. I return to this point later.

[65] Empiricus [≈30]. It should be noted that Empiricus may just have been cataloguing different forms of pre-existent scepticism, and the arguments employed therein, rather than advocating an original, and coherent, viewpoint.

[66] This is rough, and might need to be extended to sets of propositions, etc.; nonetheless, it serves its purpose in context. A similar view is put forward by Barnes in his discussion of Empiricus' philosophy. Ibid., pp.xix-xx.

[67] Empiricus [≈30], I, 13. See also I, 29: '...we say that they [Sceptics] are disturbed by things which are forced upon them...'.

although his typical strategy to diffuse philosophical arguments is precisely to show the equipollence – viz. equal rational force, or at least power to convince[68] – of arguments for opposing claims (or positions). (The law of non-contradiction seems to be a given for Empiricus.) The goal of establishing equipollence is to enable εποχή, or suspension of judgement; this achieved, ἀταραξία, a profound sense of tranquility, may be attained; and maintained by immersing oneself in *appearances*, and 'everyday life', but naught else. Indeed, philosophical argument is only good for the purpose of therapy, according to Empiricus: it may be justly employed only in order to bring oneself or another to suspension of judgement, and hence tranquility.

Now there is an everyday sense in which 'appearances' may deceive, whereby one might claim to have been 'misled by the way things seemed', but it must be emphasised that this is not the sense of 'appearance' intended by Empiricus. For example, although it might 'appear' to be evening at midday, due to heavy cloud cover, this is not what he has in mind: rather, this would be an obvious example of a theory-laden mistake. Instead, he has in mind that one might '*see* a tree' when confronted with something else, e.g. a sword, or nothing whatever.[69] (Considering appearances in a dream context might prove illustrative.)

Furthermore, and second, Empiricus does not clearly state that being a sceptic should involve having no beliefs whatsoever. Instead, one might understand his position as follows. What is taken to be 'belief' in common parlance can be divided into 'belief' and 'acceptance' – to accept something is not necessarily to believe it, and vice versa. So to suspend one's judgement (achieve εποχή) might involve a refusal to move from acceptance to belief; and this reading, suggested by Barnes, is reminiscent of van Fraassen's agnosticism about the existence of theoretical entities.[70]

To extend upon this analogue further, it might be emphasised that Empiricus is not willing to countenance the use of the indicative sign, but is willing to give his imprimatur to the use of the associative – or what Barnes calls the 'recollective'[71] – sign. For whereas the former (indicative) sign would supposedly allow us to infer the existence of entities *unobservable-even-in-principle*, the latter (associative, or recollective) sign would only allow us to infer the existence of that which may be unobservable at a particular point in time (or perhaps space), but is *observable-in-principle*. Empiricus writes:

> There being two different sorts of signs, as we have said, we argue not against all signs but only against indicative signs, which seem to be a fiction of the Dogmatists. For recollective signs are found convincing by everyday life: seeing smoke, someone diagnoses fire; having observed a scar, he says that a wound was inflicted. Hence not only do we not conflict with everyday life, but we actually join the struggle on its side, assenting without opinion to what it has found convincing and taking a stand against the private fictions of the Dogmatists.[72]

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[68] He writes: 'By "equipollence" we mean equality with regard to being convincing or unconvincing...' Ibid., I, 10.

[69] See Empiricus [≈30], I, 44-52. This makes it amply clear that he has something like sense-data, or at least sensory experiences, in mind.

[70] Ibid., p.xxiv.

[71] Ibid., p.xxiii.

[72] Empiricus [≈30], II.102.

In other words, Empiricus is neither opposed to induction, *nor indeed to inference to the best explanation in the realm of the observable*. And this brings to mind a passage from van Fraassen:

> I hear scratching in the wall, the patter of little feet at midnight, my cheese disappears – and I infer that a mouse has come to live with me. Not merely that these apparent signs of mousely presence will continue, not merely that all the observable phenomena will be as if there is a mouse; but that there really is a mouse.[73]

The point is that mice are observable-in-principle: the wall could be demolished by use of a sledgehammer, or a trap could be set, in order to find the mouse in the example above. But likewise, one could walk towards the smoke in Empiricus' first example, in order to see the fire. (Moreover, one could also legitimately infer to observable-in-principle events in the past, such as in the example of the scar. One could have seen the wound inflicted.) Thus, Empiricus is beginning to look rather like the first constructive empiricist, largely because of his acceptance of what we now call the 'theoretical-observable' distinction. Hacking would agree: '[Empiricus'] scepticism anticipates many of the concerns of a modern logical empiricist, but we do not find him... enunciating any sceptical problem about the future.'[74] But against this, I seek to motivate the view that induction and inference to the best explanation need either to be accepted as legitimate practices, or abandoned wholesale: this, precisely because the aforementioned distinction is not a genuine one. (It is worth adding that to reject the idea that 'inference to the best explanation' is worthwhile is *not* to reject the demand for explanation. III.2 should be seen in this light, as I further explain in III.5.)

It is not unreasonable to characterise the Greek philosophy in the time of Empiricus as having undergone a considerable shift from that in the time of Plato or Aristotle. The shift was this: Plato and Aristotle – central in the *academic* tradition – put metaphysics first in philosophy, whereas the sceptics and dogmatists tried to put epistemology first.[75] Of course, some would say the same thing has happened again, post-Enlightenment: van Fraassen's 'anti-metaphysics', tackled in the final section, seems to be a case in point.[76]

How then is the metaphysician to engage with, and attempt to persuade, an Empiricus or a van Fraassen? (Of course, the latter does not directly say 'It cannot be doubted that there are appearances', but does mention an 'observable world', and implies that it is composed of 'observable things'.[77] He also uses an even more perplexing phrase on occasion, 'observable phenomena', the meaning of which is opaque.[78]) Let her start by examining precisely the claim that supposedly cannot be doubted: 'There are appearances'. What questions might she ask?

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[73] van Fraassen [1980], pp.19-20.

[74] Hacking [1975], p.179.

[75] Plato is still seen by many as an ethicist, first and foremost: however, the core of his ethical system is based upon considerations relating to *essence*, in particular that which lies outside the Cave.

[76] Further, I will argue that both Empiricus and the later empiricists were motivated by subjective epistemologies, and that this contributed to their 'inside-out view'. There is a link, here, to their underlying take on justification, which I have argued against in chapter two.

[77] van Fraassen [1980], pp.54-55.

[78] Ibid., pp.57 + 64.

Well first and foremost, she might ask 'Are there appearances?' in the sense of 'To what does "appearance" refer?' Are appearances things-in-themselves? They had better not be, without the claim being ontological, in so far as it is founded on the category of 'entity'. Yet even if this is accepted, appearances could not have a 'grounding' either, without there being things-in-themselves; a claim such as Empiricus' – that there are such things – would then be that *appearances* are not ontologically fundamental, or basic. For example, one might see them as involving relations (a category), between us (*qua* members of a natural kind, another category), and other particulars (yet another category).

Building on this, she might point out that were we to be denied access to categories, discourse would be rendered impossible. For example, 'discourse' would not refer – or at the very least, we could not take it do so – in the previous sentence. (This holds even if meaning is determined by *use*, following the later Wittgenstein, for it would not be possible for us to *use* anything without identifying some things, and we could not identify any things whatsoever without categorial access.) In other words, even the reasonably innocuous claim 'There are appearances' is indefensible without appeal to ontology, be it implicit or explicit. Hence, it would seem to be the case that there are arguments for and against the notion that there *are* appearances. So why should these not be equipollent? What makes arguments *for* appearances so remarkably strong? Empiricus has no straightforward answer to this question, and more surprisingly, I do not think that van Fraassen does either. In short, this is since appearances are precisely *theoretical* entities according to the very observable-theoretical distinction that underpins his empiricism, as I shall now argue.

Maxwell's Challenge

The *locus classicus* for the challenge to the epistemological significance of the 'observable-theoretical' distinction – although as van Fraassen happily admits, 'such expressions... are on the face of it, examples of category mistakes'[79] – is a paper by Grover Maxwell, named *The Ontological Status of Theoretical Entities*. And although this paper might seem a little confused from a contemporary perspective, since it has so many targets, it is not too problematic to draw out the core arguments therein. They are as follows:

> (a)   A form of observable-theoretical distinction is accepted, but its 'ontological significance' is denied. According to Maxwell's understanding of perception, 'theoretical entities are no worse off than so-called observable physical objects' because we may come to see that which we previously only theorised about. In particular, he holds that we can, with sufficient effort, 'train ourselves to "observe directly" what were once theoretical entities'.[80] One of his examples is as follows:
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>> After listening to a dull speech while sitting on a hard bench, we begin to become poignantly aware of the presence of a considerably strong gravitational field, and as Professor Feyerabend is fond of pointing out, if we

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[79] van Fraassen [1980], p.14.
[80] Maxwell [1962], p.1060.

were carrying a heavy suitcase in a changing gravitational field, we could observe the changes of the G$\mu\upsilon$ of the metric tensor.[81]

(b)   Maxwell accepts that there are such things as sense contents, but believes that we usually do not observe them – rather, we observe physical objects and 'other publicly observable entities'.[82] He argues that we may see a car without seeing its colour, or indeed its shape. A further example (mine): the victim of a crime who is asked to describe the perpetrator may be confident that he was a man, and even be able to describe the shape of his face in great (and accurate) detail, without being able to report the colour of his eyes. Maxwell's claim is that such occurrences are not due to memory lapses.

Indeed, Maxwell goes further by suggesting that sense data may be thought to be theoretical entities. He writes:

> If theoretical physics, psychology, neurophysiology, etc., were sufficiently advanced, we could give satisfactory answers to these questions, using, in all likelihood, the physical-thing language as our observation language and *treating sensations, sense contents, sense data, and "inner states" as theoretical (yes, theoretical!) entities.*[83]

It is unfortunate that he adopts a physicalist line, but his point seems well made: again we see the rather infuriating see-saw effect which I argued against in II.2, whereby one might do away with 'inner states' by appealing to observable 'physical objects', just as one might do away with theoretical 'physical objects' by appealing to observable 'inner states'. It seldom seems to occur to participants in this sort of discussion that there might be both, and that both might be neither observable nor theoretical in the relevant *epistemological* sense (viz. as 'foundations of knowledge').

(c)   The epistemic significance of observation is not denied, but it is claimed that it is only important in so far as it provides a 'base' to allow us to confirm particular statements. (Maxwell mentions statements 'which... refer to entities which are unobservable at a given time', although it is far from obvious precisely what he is getting at. One plausible interpretation is that he is trying to collapse the distinction between indicative and associative/recollective signs, as mentioned in the foregoing sub-section.) However, Maxwell does not want to accept the idea that it is the *observation term* which provides such a base; rather, it is the *quickly decidable sentence*. In his words, this is:

> ...a singular, nonanalytic sentence such that a reliable, reasonably sophisticated language user can very quickly decide whether to assert it or deny it when he is reporting on an occurent situation.[84]

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[81] Ibid., p.1061.

[82] This is an old notion; see Russell [1912], ch.2.

[83] Maxwell [1962], p.1061.

[84] Ibid., p.1060.

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Examples are easy to provide, for such sentences are the stuff of simple discussion, in our daily lives. Upon entering a pub with a friend, I might say "There's a table over there", meaning (more properly) 'There is a table over there which is surrounded by unoccupied chairs.' My friend might then state, without hesitation, "Yes, but I don't want to sit there. It's too near to the toilets.", and I might swiftly agree to the latter sentence (i.e. that the table is close to the toilets).

Unfortunately, though, Maxwell does not make it clear whether he wants to have it that what is considered a quickly decidable sentence is partially a matter of convention, fundamentally dependent upon tradition, or whether it is grounded in the nature of human, *qua* natural kind. Nevertheless, a reasonable middle ground might be adopted, where it could be recognised that such sentences are dependent upon both considerations. Thus, it is consistent with his account that one group of humans might find it easier to 'spot' some things than another, because of different theories which are intersubjectively shared within that group.

Most important is that such a basis allows us to get moving in the business of inquiry – to issue snap judgements, which are nonetheless susceptible to future critical re-evaluation, about whether some sentences (or propositions) are true or not. And in so far as this involves a traditional component, this in no way necessitates that we be trapped with just one set of quickly decidable sentences. Rather, it could still be possible to undergo shifts, as suggested by Popper's arguments against 'The Myth of the Framework', with which I expressed some sympathy in II.4.[85]

(d)   That which is considered unobservable at one point in our investigations is subject to be considered observable at a later stage. For example, although there may have been a time at which it was right to say that bacteria were unobservable, it may now be the case that they are observable. Further, that they have, in fact, been observed. Maxwell writes:

> the line between the observable and the unobservable is diffuse... it is constantly being pushed toward the "unobservable" end of the spectrum as we develop better means of observation – better instruments.[86]

Here, it is important to understand the distinction between observable-in-principle and observable-in-practice. His point is best understood as follows: we might posit something which is not observable-in-practice, but may well be observable-in-principle. Thenceforth, activity might be geared toward observing it in practice.

Maxwell's argument is rather muddled, and there are some ambiguities. For instance, it is not obvious how point (a) – that 'we can train ourselves to "observe directly"

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[85] See Popper [1970], pp.56-57.
[86] Maxwell [1962], p.1060.

what were once theoretical entities' – could coherently relate to point (d), above; surely no amount of training will alone develop a 'better means of observation', or 'better instruments'. Thus, it seems that the training would have to come *after* better instruments were developed, and this would suggest that only the instruments could allow *direct* observation; that is, enable the move from observable-in-principle to observable-in-practice.

Nevertheless, Maxwell does provide a foil for van Fraassen, who considers his paper to be worthy enough to require engagement with. And the examination of his response will provide a useful platform for me to launch my critique of his alternative.

Van Fraassen's Reply

Against Maxwell, van Fraassen draws our attention to the distinction between *observing* and *observing that*, and claims that the former is possible without the latter. His example involves an individual playing with a tennis ball, but not observing that it is a tennis ball, due to a lack of awareness that there is such a game as tennis:

> He cannot get that information through perception; he would first have to learn a great deal. To say that he does not see the same things and events as we do, however, is just silly; it is a pun which trades on the ambiguity between seeing and seeing that.[87]

Prima facie, this does seem persuasive. Most of us can remember pointing at an object in our childhood, and asking "What is that?" Indeed, I have often seen a bird without seeing what genus it was a member of; I recently saw an ibis without *seeing that* it was an ibis, as I have subsequently learned from a book. Yet I would contend that van Fraassen's argument is too quick, and only skims the surface of what is, in truth, a far more complex issue. Consider the following example:

While on a recent trip to South Africa, I was a passenger in a car that was being driven by my father. And as we were travelling through the Drakensberg mountains, he suddenly exclaimed "Did you see that snake?" At this I paused, uncertain about what I should say – that is, although I was not moved to thinking reflectively – and could only mumble "On the road?" Yet even when he replied in the affirmative, I was still not sure how to respond. I suppose this might seem strange, but let me try to explain.

Over the course of the journey I had been reading a novel, and only sporadically looking up in order to take in the view, and appreciate the terrain; I was quite aware of the road when looking up, although not directing my full attention toward it. What is more, I had been looking up from my novel just before my father's exclamation, and had registered what might be described as a 'dark patch on the road'. But had I *seen the snake*? My answer at the time was in the negative, after a pregnant pause. Instinctively this seemed to be correct, although van Fraassen would presumably want to have it that I saw the snake, although not *that it was a snake*.

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[87] van Fraassen [1980], p.15.

However, while there is a sense in which I would want to agree with van Fraassen, it is only in so far as I would agree that a snake may have been *partially responsible* – perhaps causally – for that which I saw. And *what I saw* was a dark patch on the road. In natural language, this is the only genuine account that I can offer of my sensory experience: to say more would be to lie.

It is true, of course, that I might see van Fraassen without *seeing that* he is van Fraassen. Still further, I might see this man without *seeing that* he is a man; he might be disguised as a woman. All this is accepted, as is the underlying notion that one has sensory experiences which need not, and should not, be confused with the inferences made on the basis of, or beliefs arrived at from, those experiences. But in this sense, I think it is even true that one could see a colour without *seeing that* it was a colour, or a shape without *seeing that* it was a shape, and would therefore want to resist the following dogma: *that sensory experience is possible without beliefs or expectations on the part of the perceiver, which serve to partially constitute such experience.* This returns us to my point about the snake on the road, assuming that there really was one: we may say that *I saw it* in so far as my sensory experience would have been different had it not been there, *but in doing so we are holding my expectations/beliefs constant.* Had my expectations and beliefs been different at that moment, it is possible that I may not have been able to report a 'dark patch on the road'; indeed, I may just have reported the road, while looking in precisely the same direction.

Now the crux of this thought is that it is not possible to see anything without *seeing that* something. Consider a man who claims to have seen something while asleep: the report can only be made because he could *see that* there was something. And no-one, I contend, ever asseverates 'I saw something' when they can offer no further description of what was seen, e.g. movement, or a swirl of colours. Consider a solider on watch, in this regard. Or even more controversially, consider a non-human animal. For I think it would be extremely strange to want to say of an animal that it could genuinely see, while simultaneously positing that it had no awareness of anything outside itself – that it had no ability to *see that* (anything). For instance, is a typical dog not at least capable of *seeing that* a bone is food, even if it cannot *see that* it is a bone? And is it not in virtue of this that we can say that such a dog is capable of seeing a bone? Coates gestures in this direction:

> For *seeing* to occur, in the basic *extensional* sense… the subject must, at most, entertain some minimal conceptual component in experience – perhaps merely to the effect that there is something present in the surroundings…[88]

At this juncture, I should emphasise that this suggestion is neutral on the question of whether we forge internal representations of external objects, and whether it is those that we truly perceive. Furthermore, it is neutral on the question of whether an entity perceived (*qua* external existent) is constitutive of the sensory experience that allows such perception, and uniquely determines said experience.[89] (As Coates also writes: '…there is no obvious path leading from claims about the epistemic character of

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[88] Coates [1998]; to be fair, it is unclear whether Coates wishes to include animals other than humans.

[89] In other words, nothing I have said has yet favoured a causal theory of perception over a disjunctive one. To return to my first example: all I have said is that were a snake not there, *ceteris paribus*, I would have seen something different; that is the sense in which the snake was responsible for what I saw.

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experience to claims about its ontological nature.'[90]) However, as I shall show, a full account of what perception entails is *vital* to a defence of the 'theoretical-observational' distinction; indeed, even to suggest that the distinction exists. But let me work backwards, and try to show why a concept empiricist – one who believes in sense data – would commit to belief in it.

According to the concept empiricist, or 'sense empiricist' as Popper puts it, 'to see X' is 'to see a mental representation of X'. Thus, all the things that one sees or touches, hears or smells, are inside oneself. (Here we see how such a view is liable to lead to idealism, even of an extreme form such as Berkeley's. In Popper's words: 'the world becomes the totality of my ideas, of my dream.'[91]) The scheme is essentially as follows: something 'out there', viz. not inside oneself, may stimulate one. Based on that stimulation, a model of the stimulator is constructed inside (and/or by) one's mind, and it is that model (or picture) one is aware of. In other words, as I have already argued in II.2, there is a sense in which one is radically detached from the world: all one's interaction with it is mediated by a process of 'internal representation'. Of course, the distinction between 'phenomena' and 'noumena' – that of Kant, who clearly states 'representation... is in us'[92] – almost falls out. The phenomena are like little internal pictures, constructed by some mysterious process in our bodies and/or minds of which we are entirely unaware. Things-in-themselves are somehow responsible for what the little pictures look like, but we can't really be sure how. Indeed, this was precisely why Kant wanted to hold that things-in-themselves were *radically* inaccessible to us; that we could not even conceive of how they might be.

But let us pause, here, to draw a breath and attempt to return from the clouds. For what has been described is a complex and extremely shaky theory about the way things are; about our place in the world, and the nature of our interaction with it. If it were right, then we would do well to treat claims about things-in-themselves with a pinch of salt. But it is wrong. It is based on a false theory of perception, motivated by the addiction to justificationism – the quest to preserve certainty, or near-certainty in the guise of 'probable truths' – and arrived at via a detour through amateur psychology. Cast your mind back to the view of Russell, mentioned in the previous chapter:

> [I]t is our particular thoughts and feelings that have primitive certainty. And this applies to dreams and hallucinations as well as to normal perceptions... the certainty of our knowledge of our own experiences does not have to be limited in any way to allow for exceptional cases. Here, therefore, we have, for what it is worth, a solid basis from which to begin our pursuit of knowledge. The problem we have to consider is this: Granted that we are certain of our own sense-data, have we any reason for regarding them as signs of the existence of something else, which we can call the physical object?[93]

However, this account must be wrong, for unless being asleep and being awake were qualitatively different in an experiential sense, we would not even be aware of the

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[90] Coates [1998]
[91] Popper [1983], p.82
[92] See Kant [1997], A378.
[93] Russell [1912], pp.19-20.

distinction – we would not even posit it. (Likewise, we can only posit 'hallucinations' if we assume we can genuinely perceive external entities some of the time.) Further, we could not establish them as qualitatively different, in an experiential sense, on the basis of sense-data alone, even were we to have this. (This is even more obvious in the case of hallucinations.) Thus, perception understood as 'access to sense-data alone' cannot be established as a basis without appealing to something more basic: in this case, Russell appeals to a wake-dream distinction which he cannot hold that he is certain of. His argument evanesces because he tries to tackle Cartesian scepticism via a subjective epistemology, and seems to miss the fact that his starting-point is underpinned by a rich, and prejudiced, metaphysic. He is a victim of the highly infectious 'Epistemology as First Philosophy' pathogen which I have repeatedly inveighed against herein.

But back to van Fraassen, who unfortunately does not deign to share his theory of perception, and remember the earlier quotation:

> ...a theory is empirically adequate exactly if what it says about the observable things and events in this world is true – exactly if it 'saves the phenomena'.[94]

We now see that the best strategy to illustrate the problematic nature of this claim is *not* to discuss causal theories of reference, *pace* Psillos.[95] Rather, it may be supposed that van Fraassen would grant that we can successfully refer to objects such as 'tennis balls', and events such as 'car crashes' – that is, objects and events that we can *see*. It may then be urged that we can *see* entities such as electrons, if they exist; that much scientific activity has been geared towards creating conditions in which they may be seen. In other words, it may be emphasised that "We can see tables and chairs" is no more or less problematic, in either an epistemic or metaphysical sense, than "We can see electrons." (With Maxwell, 'theoretical entities are no worse off than so-called observable physical objects', although not for the reasons he gives.) Ultimately, it may even be quite defensible to suggest that we, *qua* humans, could *in principle* see any entity which exists in space and time.

So in the next sub-section, I shall examine van Fraassen's argument to the effect that we cannot see electrons – the 'jet argument' – and defuse it. This achieved, I shall elucidate how a correct understanding of perception reveals the 'theoretical-observable' distinction to be underpinned by a conflation between metaphysical and epistemic issues. Specifically, I shall argue that it is a bizarre juxtaposition of two separate distinctions – the first between the observable and the unobservable, and the second between the theoretical and the observed.

Beforehand, though, it is worth adding that the empiricist who wants to defend the 'theoretical-observational' distinction could, as a first effort, try to appeal to the claims of contemporary natural science. That is, he could talk of three types of photoreceptor cones in our eyes, each containing a different visual pigment attached to 11-*cis*-retinal, and disposed to absorb photons of differing energies; further, he could account for deuteranopia and protanopia as genetic abnormalities.[96] Yet none of this will do, for to rely on such talk would be to appeal to the very 'theoretical

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[94] Ibid., p.12.
[95] Psillos [1999], ch.12.
[96] This could support the claim that 'blue' was an internal response.

entities' which we are not supposed to want to commit to belief in: light, molecules, and so forth.

The Jet Argument

For van Fraassen, 'observable' is a vague predicate; he claims that it is only usable in virtue of it having 'clear cases and clear counter-cases.'[97] A clear case of observation would be employing the naked eye to read a book. Furthermore, and derivatively, watching the moons of Jupiter through a telescope, just because they would be able to be seen by the naked eye of an astronaut who was close to them (at least, according to contemporary theory). But both Maxwell and I would accept these as clear cases, so what van Fraassen needs to provide, as he correctly recognises, is a clear counter-case. His attempt involves a comparison between the effect of a charged particle passing through a cloud chamber, and that of a jet flying through the sky:

> The resulting silver-grey line [when a charged particle passes through a cloud chamber] is similar (physically as well as in appearance) to the vapour trail left in the sky when a jet passes. Suppose I point to such a trail and say: 'Look, there is a jet!'; might you not say: 'I see the vapour trail, but where is the jet?' Then I would answer: 'Look just a bit ahead of the trail...there! Do you see it?' Now in the case of the cloud chamber this response is not possible. So while the particle is detected by means of the cloud chamber, and the detection is based on observation, it is clearly not a case of the particle's being observed.[98]

Now I find this line of argument quite baffling: it seems to establish precisely nothing, other than what our conventions are. In particular, because in reply to van Fraassen's "Do you see it?", I might say: "I'm not sure. Maybe I see a jet, but not *that it is a jet.* I see a dark patch moving, against the blue of the sky, which is consistent with the presence of a jet." And now let us turn this around, imagine that I were stood next to a cloud chamber, pointing at the front of a trail developing in real-time, and asking "Do you see the charged particle?" of van Fraassen. The character of his reply would not seem to be any different from this: "I'm not sure. Maybe I see a charged particle, but not *that it is a charged particle.* I see movement about a point – a perturbation, in a background that is relatively static – which is consistent with the presence of a charged particle." The point is simple: in the first case, I may remain agnostic because although what I see is consistent with there being a jet present, I might be misled. I might be seeing a rocket. Why, then, should the second case be any different?

In the first case, van Fraassen could pass me binoculars or a telescope. While looking through them, towards the dark moving patch, I might then see something else *consistent with there being a jet in the sky.* But so what? In the second case, we could repeat the experiment with a large metal block in the middle of the cloud chamber, and notice that the trail would not penetrate it; but surely this is something else *consistent with there being an electron in the cloud chamber.* So what is there of significance here, other than that when one sees a jet, it is easier to *see that* it is a jet, given that the theories required are commonly held and instilled in us from an early

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[97] Van Fraassen [1980], p.16.
[98] Ibid., p.17.

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age? (Of course, it is less common to find oneself in a situation where one can see an electron, given the status quo. But that is contingent.) And what is there to stop me from saying I'll accept the claim that jets exist, because it saves the colours and shapes I *see that* there are, but I won't commit to belief in them? It is not as if I somehow *grasp jets in their completeness*, on the assumption that such things exist: I do not have *privileged epistemic access to jets*. And to baldy assert otherwise seems to be to appeal to an empiricist dogma, which is based on veiled metaphysical assumptions about other entities that exist in the world, and the range of our possible interactions with them.

If this does not convince, let me put it another way. Imagine I will grant that the sky exists, that tables exist, and that almost all 'normal' objects of perception ('publicly neutral objects') – those that we refer to in common discourse, or 'everyday life' – exist. But I will be a local sceptic about jets. I will accept, of course, that the way everything else that I believe in *is affected* is perfectly consistent with the existence of jets. Yet I will refuse to *commit to belief* in jets, and hold that I am perfectly rational to do so. No metaphysical argument will deflect me: I scoff at metaphysics, and hold that the only necessity is verbal necessity. Besides, why bother to convince me that there are jets, when I can behave *as if there are jets*, and reap all the pragmatic rewards that believing in jets would bring?

As a rebuff, it might be objected that I am being silly. What's the harm in believing in jets? My answer, delivered with a straight face, is that I don't need to believe in such occult entities – 'flying machines' – and that by remaining agnostic I will be less liable to have made a mistake. Sure, the position of the individual who believes in jets is as rational as mine, but I'm just that little bit cleverer; I won't look like an idiot if everyone else changes their mind on this.

To rejoin, my opponent could ask on what principled basis I distinguish between the jet and 'everything else'. I will repeat that I scoff at metaphysics. I reject that sort of talk, because I reckon epistemology comes first, and there is an epistemic distinction between jets and non-jets. Challenge me a bit further, and when I get tired of finessing my way out of your arguments – remember, I am clever after all – I'll end up telling you that I've got an 'anti-jet stance': "You know, that's just the way I see the world!"[99]

But enough polemic, for van Fraassen's position is really an anthropocentric one. In particular, he seems to think that what is observable is somehow determined by 'everyday life', or is forthrightly obvious, because he regards 'what is observable as a theory-independent question'.[100] So the real problem, I think, is that van Fraassen does not recognise that what we think is going on is almost always theory-dependent; outside, as well as inside, contemporary natural science. Take this example: were I to turn off the light-switch in my room, now, I would hold that my bookcase was still there, although I could not see it. This, *because I believe I could see it were the light on*. But would I want to hold that my shadow, a great candidate for an observable we could agree on, was still there? I say not. Still further, I would mention holes (and could, further, talk of gaps). I don't believe, for one second, that holes are things-in-

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[99] I argue against the invocation of 'stance' in the next section.
[100] Van Fraassen [1980], p.57.

themselves, but I'm pretty sure that I can point at holes, and talk about holes – "Look, there's a hole, can you see it?" – without seeming mad. I can accept hole talk, and behave *as if* there are holes. The silver bullet cometh: *I can rationally hold an 'anti-hole' stance, while holding that holes are observable in van Fraassen's sense!* But this seems to me to make a mockery of the claim that his distinction has any genuine epistemic significance.

A Reconstrual of the Observability Criterion

All this said, there remains a significant distinction between that which is observable and that which is non-observable. This is metaphysical, or ontological. There is also an important difference between a theoretical entity and an observed entity. This is epistemic.

As far as the first is concerned, we may make metaphysical posits about entities that do not exist in space and time, and could not, therefore, be the objects of perceptual awareness. That is, on the reasonable assumption that we exist in space and time, and that perception – being radically dependent, *inter alia*, on our embodiment – only grants direct awareness of other entities in space and time, viz. concreta. (Notice it is not precluded that abstracta – e.g. laws of nature, under some accounts – be involved in the process of, or governance of, acts of perception.) The thought, simply, is this: if entities such as universals had causal effects, then we might be able to set up a situation suitable for 'viewing them' (that is, in a possible world in which they existed). However, any thing eternal and unchanging, located literally nowhere (in a proper spatial sense), is clearly not a potential object of perception. There is no sense in which any (sane) individual would wander off in search of blueness and circularity, unless they were something like a devout Platonist, and intended to 'explore' by dying, and having their soul released from its fleshy prison! Here is a demarcation criterion between the observable and non-observable in emergence: since all abstracta are unobservable, might it not be that all concreta are observable? Might it not be reasonable to deny that '[T]he term "observable" has logically nothing to do with existence', at least under a broad construal of 'logically'?[101]

This further claim might seem, at first glance, to be extremely dubious. For instance, might one not conceive of an inert substance, existent in space and time yet undergoing no interactions whatsoever? The reply is emphatically in the negative, and not just because we should not assume *a priori* that there is no 'hopelessly opaque surd'.[102] For to be in time is precisely to be capable of change; time is the dimension of change, or even *is* change. To be in space is precisely to be in a position – no pun intended – to interact with (a set of) other entities. (In both cases, relations are entered into.) Allow me to borrow from contemporary science in order to persuade, since it provides some of our best current guesses about how things are.

Any 'inert entity in space and time' would need to have substance without volume or mass, if it were not to 'resist' the intrusion of other objects (due to the space it occupied) or undergo (and exert) gravitational influence. Yet it could not be

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[101] Ibid., p.18.
[102] I refer back to the quotation from Blanshard in III.1.

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something like an electromagnetic wave, without violating Einstein's infamous equation, $E^2 = p^2c^2 + m_0^2c^4$, by being capable of *changing* momentum (*inter alia*); this since a photon has no rest mass, and the equation reduces to $E = pc$. In short, this suggests that anything existent in space and time can be *seen*, in the correct conditions. But this in no way implies that it is a trivial task to spot each and every concrete thing, or each and every sort of concrete thing – to get into the right situation to observe the aforementioned either in a physical, or psychological, sense. For example, to observe 'dark matter', if it exists, is no easy business for the moment. But it may well be that case that black holes have already been observed, through the x-rays emitted by the accelerated gases that form the accretion disk about them.[103] This, in the same way that one might locate a plughole in a bath filled by murky water which is draining away, by noticing a whirlpool.

Second is the distinction between a theoretical entity and an observed entity, or more properly a theoretical entity and an entity *believed to have been observed*. And needless to say, this is purely epistemic. For instance, imagine I was to posit that there is a small planetoid which orbits the Sun on a path that lies within the volume formed by a solid of revolution (about the Sun, at the origin) of the 2-d area bounded by the orbital paths of Earth and Venus. But imagine, further, that I roughly specify its orbit, by arriving at an approximate solution to the many body problem involved, and tell you where and when you will have to point a telescope, in order to attempt to view it. Would it not be reasonable to *classify* it as 'observed', and no longer 'theoretical', provided such attempts to view it were successful (viz. provided the relevant observation statements were classified as true)? Or to put it differently, would a particular set of observation statements – treated as such because they deal in terms that refer to entities already classified as observable-in-principle, because classified as observed[104] – not serve to corroborate the hypothesis that there existed such a planetoid, and thereby allow for a classification of that hypothesis as *true*? And would the classification of that theory as true not be because of the classification of the planetoid as *observed*? On this level, it strikes me that there is no greater epistemic security in classifying any one entity as 'observed' than any other. Rather, it just so happens that it is much harder to classify some entities as 'observed' than it is others, because we do not often find ourselves in situations suitable to do so, *in terms of entities already classified as 'observed'*, and *sometimes hence 'observable' (in the ontological sense)*, according to dominant traditional theories. But it strikes me that asking "How would this snow on the ground be affected were an abominable snowman to walk over it?" is very similar to asking "How would the washing-up liquid in this tank be affected were a neutrino to be absorbed by it?"; the fact that an abominable snowman could also be seen by the naked eye *in a particular class of circumstances* does not seem to have any epistemic significance at all. Consider, in particular, the reconstruction of the appearance of a dinosaur such as a diplodocus, on the basis of the discovery of bones: plausibly no human will ever see such a creature, but we do not classify it is theoretical. Besides, the fact that I cannot see the chair I am sat on when I am not in the room does not make me doubt its existence, and the fact that I cannot feel that chair when I am asleep on it does not make me doubt its

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[103] It is difficult to know whether van Fraassen would want to have it that a black hole is observable, but I should say it would not be, were his (imputed) view of perception to be correct.

[104] Here I gesture at the following: to make the test possible, particular theories about the existence of telescopes, the effects of looking through them, their very existence, their observability, and so forth, must be accepted.

existence. Nor, indeed, does the fact that I have never smelt it, or tasted it.

5.   METAPHYSICS, CONSTRUCTIVE EMPIRICISM, AND 'THE EMPIRICAL STANCE'

The Empirical Stance has considerable scope, yet it clearly builds upon van Fraassen's earlier work, in particular *The Scientific Image*. Its central question is clear: 'What is empiricism, and what could it be?'[105] And while some of the tactics that van Fraassen employs in order to answer this should be familiar by now – for example, he maintains the view that explanatory power should only be construed as a pragmatic virtue for theories, that the only forms of possibility and necessity are verbal, and that there is a radical distinction between acceptance and belief – his overall strategy has developed considerably. Most noticeably, he now has metaphysics firmly in his sights, whereas beforehand, he has only hinted at such antipathy.[106]

It is to this move that I wish to object, and I shall endeavour to fulfil two distinct tasks in this section, in order to set the stage for subsequent internal and transcendent critique. First, I want to elucidate the notion of 'stance', which is bound up with van Fraassen's view of philosophy *qua* activity, and the pragmatic/existentialist vein in his thinking. Second, to provide an overview of his arguments against metaphysics, with particular focus on disclosing what he understands 'metaphysics' to be.

The Notion of Stance

In attempting to establish what empiricism might be, van Fraassen first considers it as an historical movement, and draws the conclusion that it is characterised by 'a recurrent rebellion against the metaphysicians.'[107] However, since this is precisely the point at issue – and it will be contended that van Fraassen's view of metaphysics is too narrow – it cannot bode well for us to accept this, as it stands. Instead, this claim might be moderated somewhat, and we might accept, pending further investigation, that empiricism is at least characterised by its opposition to certain *forms* of metaphysics. (As will be explained below, van Fraassen does tend to use the term 'metaphysics' in a rather lax fashion, and this leads to considerable confusion about what, precisely, he is targeting.)

More revealing and cogent, however, is his subsequent analysis of what empiricism cannot be. In brief, he argues that the empiricist cannot engage in a critique against metaphysics – or for our purposes, any *form* of metaphysics – if she accepts a 'Principle Zero' such that:

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[105] van Fraassen [2002], p.xiii.
[106] See the introduction to van Fraassen [1980], where it is implied that Aristotelian philosophy inhibited progress in the Middle Ages, and that there is strong similarity between the reasons for which nominalists oppose metaphysical realism, and those for which empiricists oppose scientific realism. See also the opening chapters of van Fraassen [1989], and van Fraassen [1991].
[107] van Fraassen [2002], p.36.

> For each philosophical position X there exists a statement X+ such that to have (or take) position X is to believe (or decide to believe) that X+.[108]

Why? Just because to hold that there is a factual thesis which is not only immune to empiricist criticism, but is also the foundation of the empiricist's attack on metaphysics, is quite incoherent; for instance, it might be open to metaphysical criticism. Either this, or the empiricist must confess to being basely dogmatic, but no more so than the metaphysician with an opposing foundational claim need be: a stalemate ensues.

But how might we view a philosophical position if 'Principle Zero' is to be violated? The answer: as a 'stance', which involves beliefs and opinions, but also 'involves a great deal more, will not be identifiable through the beliefs involved, and can persist through changes of belief.'[109] And this knits nicely with van Fraassen's suggestion that the history of ideas plays an important role in giving us an orientation: 'In every century we must reinterpret ourselves to ourselves. We do not come into our century with a tabula rasa.'[110]

Unfortunately, van Fraassen's characterisation of 'stance' is still very vague; we are left with only two and a half pages of prose to tell us what philosophy could be.[111] What *is* clear is that a 'stance' is radically dependent on the value-judgements and attitudes of one that holds it: 'toward life, love, and laughter'.[112] Yet it is also supposed to be reasonable to compare 'stances' on some sort of intersubjective basis: for van Fraassen, they are not 'purely subjective'. For example, in comparing the 'stance' of materialism with that of empiricism, van Fraassen thinks it is illuminating to examine how proponents of each view (empirical) science. But an obvious objection still looms:

> Is there a difference that makes a difference between assuming a stance and acquiring a set of beliefs, except perhaps for what Peirce called "a certain contrite fallibilism"? If a student emerges from Philosophy 101 convinced that there is no point in trying to describe either the world or knowledge in the wholesale ways characteristic of metaphysics and epistemology, does her acquisition of that negative belief count as taking the empirical stance?[113]

Now I do not want to guess how van Fraassen would want to answer. Rather, I simply want to emphasise that Rorty does not do him a disservice by suggesting that one must be a fallibilist in order to hold *any* 'stance'; van Fraassen wants to avoid dogmatism at all costs, and this is his motivation for rejecting the aforementioned 'Principle Zero'.

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[108] Ibid., p.41.

[109] Ibid., p. 62.

[110] Ibid., xvii.

[111] Ibid., 'What Could Philosophy Be, Then?', pp.61-63.

[112] Ibid., p.62.

[113] Rorty [2002]

In order to understand van Fraassen's argument against 'metaphysics', we first need to understand what he takes 'metaphysics' to refer to. He inveighs against, variously, 'a seventeenth-century style of metaphysics'[114], '[Quinean] analytic ontology'[115], and 'materialism'[116]. But hidden away in a footnote is a more substantive definition of that which he opposes:

> The type of metaphysics to which I refer, and which I take to be the enterprise engaged in by, for example, Descartes and Leibniz, is characterized by the attempted construction of a theory of the world, of the same form as a fundamental science and continuous with (as extension or foundation of) the natural sciences.[117]

This is fair enough, if again undesirably vague. But what leaves a bad taste in the mouth is van Fraassen's declaration in the introduction, 'I do not reject all metaphysics'[118], coupled with his apparent association of 'metaphysics' with 'trivial pursuits'[119] at the end of chapter one, and thenceforth. On the one hand, his point might be understood as this: many activities that involve 'puzzle-solving' (in Kuhn's sense) are trivial, but we need not reject them; all metaphysics is 'word play', but need not be rejected as long as it is not accompanied by 'false consciousness' (e.g. the belief that it is disclosing some special knowledge about the actual). On the other, it might be thought that he owes us an explication of what sort of metaphysics is meaningful, and useful, under his painfully brief account of what constitutes philosophy. Besides the recognition that van Fraassen is opposed to philosophical approaches that are overly respectful of the *content* of natural science, rather than its methodology, we are at an impasse. Needs must we look beyond *The Empirical Stance*, to van Fraassen's earlier work, in order that we might progress.

Unfortunately, it has to be noted such a move confronts us with a serious obstacle, for in his earlier scholarship van Fraassen offers a dogmatic characterisation of empiricism, far removed from the notion of 'stance':

> By empiricism I mean the philosophical position that experience is our source of information about the world, and our only source.[120]

Here we have a substantive thesis – an X+ to the X of empiricism, if 'Principle Zero' holds – with which many a metaphysician would want to strongly disagree. This, because it seems to hark back to the naïve associationism of Hume and Mill, which Pinker has recently argued 'became the core of most [subsequent] models of learning...'[121], and against which I argued at length in I.2.3 (and to a lesser extent, II.2). But for the moment, let us just accept that there is room for serious debate here, and that one might fairly asseverate '[C]ulture is crucial, but culture could not exist without mental faculties that allow humans to create and learn culture to begin

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[114] van Fraassen [2002], p.4.
[115] Ibid., p.11.
[116] Ibid., pp.49-61.
[117] Ibid., p.231.
[118] Ibid., p.xvii.
[119] Ibid, p.30.
[120] van Fraassen [1989], p.8.
[121] Pinker [2002], p.19.

with.'[122] This is an explicit nod to van Fraassen's belief that 'we do not come into our century with a tabula rasa', but need not result in the acceptance of a Kantian (or Neo-Kantian) conception of metaphysics, founded on categories of thought about being, rather than categories of being, and resulting in a robust empirical realism constructed upon the swampy ground of transcendental idealism. 'What are our faculties, and their limitations?' is a question that I wish to keep open, and will return to towards the end of this thesis. (A similar question, about whether we have *a priori* knowledge, also looms large.) I have digressed, here, only in order to suggest that it is unclear how it is not relevant for the 'new model van Fraassen': the one with 'the empirical stance'.

So, back to the main thread of this sub-section: 'metaphysics' for van Fraassen. Let us first look to *Laws and Symmetry*, and second to *Quantum Mechanics: An Empiricist View*. In the former, we find van Fraassen arguing that 'the end of the eighteenth century marks a great turning-point in philosophy'[123], which involved a desire, on the part of empiricists, to separate science from not only theology, but also 'metaphysics'.[124] (I have already denied this in II.1.) As if to corroborate the importance of the brief digression above, he continues by citing Kant's *Critique*, and makes it clear that the sort of metaphysics being attacked therein involved the dubious claim that 'reason can bring us to logical... certainty of truths that transcend experience'.[125] Further, we find a clue as to what form of metaphysics may be acceptable to van Fraassen. One that bears surface similarity to the Kantian conception, in so far as it only allows for:

> the critical archaeology of ideas to uncover the actual presuppositions in actual history of science, plus the analysis of possible presuppositions that could constitute a foundation for science.[126]

In the latter, we find a further development of this position, whereby van Fraassen expresses his dislike for scientism, according to which science 'is elevated (?) to the status of metaphysics.'[127] Again, the point seems to be that obsession with the content of contemporary science, if that content is taken to disclose the truth or approximate truth about the 'way the world is' (viz. the actual), is inadvisable: as such, his arguments against scientific realism and metaphysics of a certain sort do seem to come together. But note that being interested in the content of science in another way is perfectly permissible for van Fraassen:

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[122] Pinker [2002], pp.viii-ix.

[123] van Fraassen [1989], p.8.

[124] This might seem curious because traditional metaphysics was arguably a core component of theology from the Middle Ages onward, as I argued in II.1, given that medieval theologians had eagerly discussed issues such as whether God could create a universal without anything to instantiate it. It remains unclear whether metaphysics is supposed to be some sort of *method* for van Fraassen, or whether it can legitimately be taken to be a discipline in its own right. I will argue that these two conceptions are deeply intertwined: that metaphysics mirrors mathematics, in so far as it has pure and applied domains.

[125] van Fraassen [1989], p.8.

[126] Ibid., p.9.

[127] Van Fraassen [1991], p.17.

> When we come to a specific theory, the question: *how could the world possibly be the way this theory says it is?* concerns the content alone. This is the foundational question *par excellence*, and it makes equal sense to the realist and empiricist alike.[128]

This is a striking statement, because van Fraassen clearly wants to have it that it is, after all, perfectly reasonable to be interested in, and even carefully examine, the content of contemporary science. What is wrong is to be an epistemic realist in the mould of those that march under the flag of 'scientific realism'; as Psillos puts it, to regard 'mature and predictively successful scientific theories as well-confirmed and approximately true of the world.'[129] (Here, it is worth re-emphasising that a realist conception of truth is assumed by all parties in this discussion.[130] To opt for an anti-realist account of truth, such as that of Dummett, would be to avoid the debate between 'scientific realism' and 'constructive empiricism'.) The point: van Fraassen fails to tell us why the metaphysician should need to commit to belief in scientific realism. Does she need to? This is a question to which we will return in due course.

Despite what has been gleaned, it remains the case that the van Fraassen of *The Empirical Stance* neither succeeds in providing a rigorous account of metaphysics, nor considers a representative sample of the contemporary takes on the discipline. Understandably, given the nature of his project, van Fraassen goes to great pains to distinguish *his* empiricism from that of Locke and Mill, in answering what empiricism is.[131] But the Principle of Charity would seem to suggest that he should have isolated his target with equal care, by also providing an account of the development of metaphysics in the history of ideas, similar to (but perhaps broader than) the one I offered in II.1. Still, allow me to put that line aside, and concentrate on a more direct philosophical argument.

One overarching problem is this: can questions about the kind of knowledge metaphysics attempts to provide be cleanly separated from those about its very scope? The answer, unfortunately, seems to be that the two concerns are intertwined: thus, we might do well to start by asking just what kind of knowledge it *does* attempt to provide. We might remain extremely cautious about attributing any particular view of metaphysics to van Fraassen, instead concentrating on what *sorts of activity, sorts of epistemic moves*, and *attitudes*, he associates with 'metaphysics'. If we can then show that there is a special science that deserves the name 'metaphysics', but doesn't involve those *activities, moves*, or *attitudes*, then it will emerge that van Fraassen's 'anti-metaphysics' arguments are ineffectual, because they target too small a domain.

5.1 ABDUCTION AND 'METAPHYSICS'

From the foregoing overview, two salient conclusions have already been gleaned:

(A)   That any 'stance' must involve fallibilism; thus, that metaphysical knowledge must be understood to be fallible in order for there to be a 'metaphysical stance', or for metaphysics to be compatible with the 'empirical stance'.

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[128] Ibid., p.4.
[129] Psillos [1999], p.xix.
[130] See, for example, Alston [1996], ch.1.
[131] Van Fraassen [2002], ch.2.

(B)   To inquire into the content of a scientific theory is perfectly permissible, according to van Fraassen, as long as it is not just assumed that such content will be a key to successfully carving the (actual) world at its joints, at least in so far as that should concern the unobservable.[132]

For the moment, however, showing how metaphysics can be understood to be fallible – as suggested by (A) – is best left on hold, until the objects of its inquiry can be delimited. This is where (B) comes in, for it does not explicitly disallow metaphysics from having the content of empirical science as one of its objects. Keeping this in mind, as a suitable background to the ongoing discussion, let us now examine one of van Fraassen's central arguments against 'metaphysics':

(α)   Objection to the use of abduction, in the context of ontology: 'Where is the metaphysician who shows us how likely it is that inference to the best explanation in ontology will lead to true conclusions?'[133]

Now (α) should be familiar, for van Fraassen also uses a similar argument against inferences to the unobservable from the observable in *The Scientific Image*, employing the memorable example of 'a mouse in the wainscoting'.[134] But let us accept that he is right about this – that there is a sound epistemic distinction between the observable and the unobservable, that what is observable is not a theory-dependent question, etc.[135] – and ask "Are all the objects of ontological inquiry unobservable?" Plausibly not, for this can involve the investigation of categories such as 'event', and there would seem to be 'observable things and events', at least in some manner of speaking, by van Fraassen's own admission.[136] But hold on. What does it mean to posit a category? Doesn't the metaphysician want to infer the existence of the (unobservable) category of events, from the existence of events? The answer is emphatically negative. Categories do not exist, rather 'entities belong to different ontological categories on account of their different existence-conditions and/or identity conditions.'[137] To do ontology is to enter into a process of classification, whether concerning observable entities, or (posited) unobservable entities. And this being the case, there is no obvious motivation for van Fraassen to want to reject it wholesale.

'No obvious motivation' does not mean 'no motivation', however. Still restricting the domain of ontology to the observable – an ephemeral conceit – why would an empiricist want to inveigh against the project of category classification? Not, I think, on the grounds that it is useless to inquire into the identity-conditions of objects such as chairs, tables, and desks. Rather, she might have some serious doubts about what

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[132] This is a consequence of constructive empiricism, and is a part of van Fraassen's empiricism, although it is unclear whether this need be part of the empirical stance.
[133] I mentioned this in III.4: see van Fraassen [2002], p.16.
[134] van Fraassen [1980], pp.19-23.
[135] For clarification on this issue beyond my discussion in III.4, see Monton and van Fraassen [2002].
[136] van Fraassen [1980], p.12. Remember also that van Fraassen professes to be a direct realist about perception of everyday objects, for he writes: 'we can and do see the truth about many things: ourselves, others, trees and animals, clouds and rivers – in the immediacy of experience.' van Fraassen [1989], p.178.
[137] Lowe [1998], p.179.

an *existence condition* is supposed to be; how such a notion could be useful, let alone interesting. But allow me to give an example.

Take a group of entities that both the metaphysician and the empiricist should have little problem committing to belief in: humans, trees, apples, swords, and statues. Is it the case that there is a significant distinction between swords and statues on the one hand, and trees and apples on the other, in so far as their *existence conditions* are concerned? An ontologist might want to answer in the affirmative, just because swords and statues – call these artefacts – are dependent for their existence upon that of humans, whereas trees and apples are not. (More carefully, it might be conceded that other sentient life-forms could craft artefacts, so we might want to suggest a category of sentient life-forms to place humans in, and then relate *that* to the category of artefact.) Not dependent, of course, in the sense that were all sentient life-forms to be suddenly extinguished, the swords and statues now in existence would suddenly cease to exist. The point is that the category of sentient life-forms would have to be non-empty in any possible world in order for the category of artefacts to be non-empty in that world. And here, please understand the talk of 'possible worlds' to be no more than a convenient fiction; this, since van Fraassen argues that '"world" is not a count noun', and this argument needs to be confronted before an alternative account can be maintained.[138]

However, even given that some sort of sense might be made of the notion of *existence conditions* – the metaphysician needs to motivate a *de re* account, against van Fraassen's *de dicto* account[139] – the fact of the matter is that many metaphysicians want to allow for the inclusion of entities in their ontology which they would candidly admit to be unobservable, should they exist: the most obvious cases are abstracta, such as universals. Is it not the case that they need to employ abduction, or inference to the best explanation, in order to posit the existence of such things?

Well, we might start to tackle such a question by recognising that there are metaphysicians who would want to answer in the negative. For example, Lowe writes:

> [W]hat is central to the concept of rational inference is the notion that if the premises of an argument are true, no alternative is *possible* but that the conclusion is also true. This view is confirmed by the fact that we all naturally consider it to be a fatal weakness in someone's argument if he has failed to perceive that other alternatives besides his conclusion are compatible with his premises. But this means that what distinguishes demonstrative inference from non-demonstrative inference is the *kind* of 'possibility' involved.[140]

So here, we have a realist metaphysician who would want to declare that all ampliative inferences are invalid, even irrational, but does not want to endorse the pure deductivism of Popper, and subsequent critical rationalists such as Miller. How does he want to have it that he can do this, while maintaining that 'metaphysics can be genuinely concerned with the structure of reality itself...and at the same time can

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[138] van Fraassen [2002], pp.18-25.
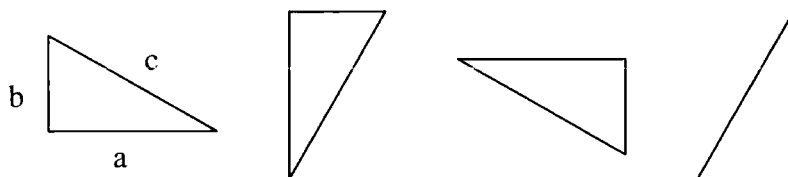
[139] van Fraassen [1977]

[140] Lowe [1975], p.3. I should add that this remains his view: see also Lowe [1987].

have a non-empirical character which distinguishes it from natural science'?[141] It is to the investigation of this question that our attention should now turn.

A short answer might simply be that Lowe believes we have *a priori* knowledge, which provides us with the ability to delimit categories of being; that he denies 'experience is our source of information about the world, and our only source.'[142] However, this is far too quick, albeit verisimilar, for it runs the risk of caricaturing his position, which has developed from his early thoughts about the structure of argument and modality. At the core of his philosophy is the idea that there is a form of necessitation that is neither strictly logical, nor narrowly logical; that is, neither a result of the laws of logic, nor the laws of logic plus the meanings/definitions of non-logical terms. This is broadly logical necessitation, or *metaphysical necessitation*, an example of which is "All water is H_2O".[143] And although the empiricist is liable to want to interject with "If that can be known, it can only be known *a posteriori*", Lowe does not want to disagree. He wants to admit that *some* metaphysical necessities cannot be established *a priori*, and clarify his position by adding that 'metaphysics by itself can only tell us what is metaphysically possible, not which of various metaphysical possibilities actually obtain.'[144] This statement requires unpacking and refinement, though. How should one understand this notion of metaphysical possibility? The answer:

> Metaphysical possibility is... the possibility of a *state of affairs* (one which is *representable*, no doubt by a proposition): and so in this sense it is a 'real', or *de re*, possibility. 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*, whereas logic, properly understood, does not concern being in general but, rather, the formal properties of and relations between propositions (which constitute only a small part of what there is).[145]

So what Lowe has in mind – talk of possible worlds still held as a convenient fiction, for the reasons mentioned earlier – is that we may be able to establish *a priori* that there are, *inter alia*, impossible states of affairs. And it would follow, of course, that such states of affairs could not obtain in the actual world. Are there any simple examples? Let us take one from mathematics. Start with four congruent right-angled triangles, with sides of length a, b, and c, as depicted below:

Then place these triangles together so that their longest sides form a square, as follows:
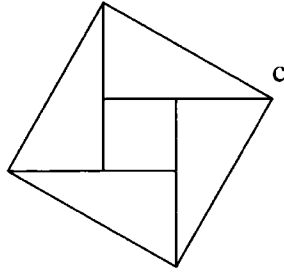
[141] Lowe [2002], p.11
[142] van Fraassen [1989], p.8.
[143] A word of warning: Lowe's position should not be confused with that expressed in Kripke [1980]. For discussion of the similarities and differences, see Lowe [1998], pp.13-27.
[144] Lowe [1988], p.22
[145] Ibid., pp.9-10

Now if we consider the enclosed gap between the triangles, we notice that it is a square, with sides of length (a-b). And the sum of its area, $(a-b)^2$, and the area of the four triangles, 2ab, must be equal to the area of the square formed by the longest sides of the triangles, c^2. Thus, by simple arithmetic,

$$c^2 = (a-b)^2 + 2ab$$
$$= a^2 - 2ab + b^2 + 2ab$$
$$= a^2 + b^2$$

This is, of course, a simple yet elegant proof of Pythagoras' theorem. But it is far from obvious that said theorem is true just in virtue of the definitions of terms such as 'angle', 'triangle', 'square, 'line', and 'area', plus the laws of logic. (Indeed, many of us will remember a time when we understood such terms, and possessed an intuitive grasp of simple logic, but did not see the truth of the theorem.[146]) Rather, it might be true in virtue of the *nature* of the right-angled triangle: we might want to say it is an *essential property* of any right-angled triangle that the square of the length of its hypotenuse is equal to the sum of the squares of the lengths of its other sides. And this being the case, we would not want to countenance empirical research into (drawn approximations of) right-angled triangles. That is, we would not want to be a part of a research project where right-angled triangles of different areas and shapes were drawn, day in, day out, in the search for a triangle which violated Pythagoras' theorem.

It is open to dispute whether any triangles exist (in the actual world). However, there is a metaphysical necessity that does not depend on assuming that they do: the point is that *if* right-angled triangles exist in the actual world, they all have the essential property described above. This is a direct consequence of the fact that the set of possible worlds in which there are right-angled triangles that do not obey Pythagoras' theorem is empty. As Lowe puts it: 'the conclusions of metaphysical arguments will often have the form of *conditional* statements, which are themselves shown by such arguments to be unconditionally true. For instance, such a conclusion might be that *if* time is real, *then* some persisting substance must exist.'[147] The rub is that the metaphysically possible relates to the actual only in so far as it delimits it; it partially determines, but does not uniquely determine, that which is actual.[148] We return to the notion of *existence conditions*, which Lowe takes it that it is the role of metaphysics to

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[146] Let us also remember that the project to reduce mathematics to logic – that of *Principia Mathematica* failed.

[147] Ibid., p.22.

[148] But note that strict and narrow logic also serve to delimit the actual: e.g. for any x, it is not the case that x does exist and x does not exist.

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examine; we have come full circle, although perhaps we now have a better understanding of where we find ourselves. Let us pause, and take stock.

We started by asking whether it was the case that the metaphysician needed to employ abduction in order to make her pronouncements, or appeal to it in order to defend her conclusions. But it has emerged that she does not need inference to the best explanation at all; rather, she wants to delimit the metaphysical possibilities (necessities, and impossibilities) about the objects of our experience and objects of our conceptions. It is plausibly true, of course, that she is interested in *forms of explanation* that van Fraassen does not favour, but this is a distinct issue. In short, inferring to the possible explanations is not inferring to the best possible explanation, and (α) misses the mark. What is particularly illustrative is that Lowe would not even want to countenance the use of abduction (with respect to *context of justification*) in the realm of the observable. Nor, indeed, would I.

There is more than this that we can take from this discussion, however. First, it has been suggested that metaphysics need not have the content of contemporary science, and only the content of contemporary science, for its object. Rather, metaphysics can investigate mathematical claims, and indeed (some) claims about the observable directly; the prior examples concerning Pythagoras' theorem and the category of 'artefact', on both of which contemporary science has little to say, were cases in point. Second, it has become apparent that a central issue in the debate about the status of metaphysics is modality; it is precisely the modal realism of Lowe – his belief in possibilities *de re*, but not necessarily possible worlds – which furnishes his philosophy with its distinctive character. Third, it has emerged that the metaphysician is liable to want to have it that we possess *a priori* categorial knowledge, whereas the devout empiricist is liable to want to deny this (although is not bound to, if construed as having a 'stance', since to hold a 'stance' is not to commit to belief in any particular dogma.). Fourth, and finally, that the forms of explanation we want will also affect our views on metaphysics.[149]

Now it is not possible to examine these issues in greater depth here, let alone to engage van Fraassen on so many fronts; this, although I should like to draw attention to Ladyman's ongoing challenge on the core issue of modality.[150] Rather, I want to show how one who has a view of metaphysics similar to that of Lowe can be both a constructive empiricist, and hold what would seem to be an empirical – but not van Fraassen's personal – stance.

5.2   REALIST METAPHYSICS AND CONSTRUCTIVE EMPIRICISM

Lowe holds that the role of metaphysics is to examine the possible. But how are we to determine which metaphysical possibilities actually do obtain? His answer is as follows:

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[149] For example, the realist metaphysician may be inclined to think of Aristotelian αἰτία as being important forms of explanation, as I argued in III.2. See Aristotle, *Physics*, book II, chapter 3. Barnes [1984], pp.332-334.

[150] See Ladyman [2000], Monton and van Fraassen [2003]

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> In a word [sic]: by experience. Knowing how the world *could* be in respect of its fundamental structure, we must judge as best we can how it *is* by determining how well our experience can be accommodated with this or that alternative metaphysical possibility as regards that structure. This may appear to give metaphysical theorizing a status similar to that of scientific theorizing, but the similarity is only superficial. A judgement that the world actually exhibits a given metaphysical feature – for instance, that it contains substances or that time is real – will indeed be an a posteriori judgement, being responsive to the evidence of experience. But the content of the judgement still retains its modal character as expressing a genuine metaphysical possibility, albeit one judged now to be actualized.[151]

Now according to this account, experience is extremely important as a source of knowledge; indeed, the ultimate purpose of metaphysical theorising is to enable us to get at how the world actually is, *via experience*. And this being the case, we might think that there is a genuine sense in which Lowe is an empiricist, albeit in the Aristotelian, rather than British, tradition. There is nothing to stop Lowe from committing to belief in a thesis such as this: 'Experience is our most important source of information about the (actual) world.' This, although he would want to defend the idea that experience requires categorial access; that it is *metaphysically laden*, so to speak.

But does Lowe – or more importantly the Lowean metaphysician – need to be a scientific realist? In particular, does his account of metaphysics imply that he should commit to belief in the claim that the (allegedly 'well-confirmed') theories of contemporary science are approximately true, literally construed? It seems not, and on several grounds. First, he is an anti-inductivist, and would reject the notion of confirmation (but could allow for 'corroboration' in something like Popper's sense); his views on what constitutes a rational argument make that clear. Thus, for Lowe, it follows that successive generations of theories in science may often move further away from the truth, rather than closer; as Miller puts it: 'we are in permanent peril of classifying statements incorrectly or doing things wrong.'[152] Second, he is not prevented from holding that the distinction between the observable and the unobservable is important in a *purely epistemic sense*; that empirical adequacy is much easier to achieve than the unvarnished truth.[153] Third, and finally, he might want to argue that certain theories are metaphysically impossible, although they 'save the phenomena'. The Copenhagen interpretation of quantum mechanics might be a suitable 'theory' to level such a charge at.

Might Lowe be a constructive empiricist, then? Might he assent to the claim that: 'Science aims to give us theories which are empirically adequate; and acceptance of a theory involves as belief only that it is empirically adequate'[154]? Well he does have an auspicious starting-point according to Monton and Van Fraassen, because '[it] is certainly much easier for a modal realist to be a constructive empiricist than anyone else.'[155] However, much might seem to depend on whether the second use of 'theory', in the definition of constructive empiricism above, refers only to 'theory in empirical science'. Is there any reason to think that it should? Again considering

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[151] Lowe [1998], p.23.
[152] Miller [1994], p.70.
[153] Here, I put my arguments in III.4 aside.
[154] van Fraassen [1980], p.12
[155] Monton and Van Fraassen [2003], p.406.

pure mathematics, it at least becomes plausible that van Fraassen could not have been referring to *all* theories, in all forms of inquiry, since such an enterprise has a distinctive non-empirical character. Therefore, it remains unclear that one needs to believe that acceptance of a *metaphysical* theory 'involves as belief only that it is empirically adequate' in order to be a constructive empiricist; what constitutes acceptance in a metaphysical context might be an entirely different matter. And further, there is nothing to stop Lowe from having it, with deference to van Fraassen, that acceptance of a metaphysical 'theory of the world, of the same form as a fundamental science and continuous with (as extension or foundation of) the natural sciences'[156] should involve only belief that it is empirically adequate, and *hence possibly true in the same epistemic sense that any unfalsified theory is.*[157] For one would have to be very bold indeed, to expect any more from a theory with such wide scope; to think that metaphysics, even with the benefit of the content of empirical science, enables us to posit cosmological theories which are approximately true. There is no harm in the speculation, though, as van Fraassen candidly admits: 'the term "metaphysical baggage" will, of course, not be used when the detour pays off... even the useless metaphysical baggage may be intriguing... because of its potentialities for future use.'[158] Metaphysical analyses may have pragmatic value in natural science, although there is never any guarantee that a given metaphysical analysis *will*; but this is something in favour of metaphysical analysis. If we can all agree that '[t]heories with some degree of sophistication always carry "metaphysical baggage".'[159], then it is arguably important to have some people with expertise in metaphysics: this, just because they might provide heuristics for teaching, understanding (viz. organising relationships between phenomena in manner which is easy to grasp), and motivating new research projects. As Feynman puts it:

> [E]very theoretical physicist who is any good knows six or seven different theoretical representations for exactly the same physics. He knows that they are all equivalent, and that nobody is ever going to be able to decide which one is right at that level, but he keeps them in his head, hoping that they will give him different idea for guessing.[160]

In short, it is not necessary to concede that metaphysics *only* has pragmatic value, but the fact that it does is something in its favour, when it is performed humbly, and without any pretensions of establishing anything with certainty, or even 'high probability'. Mistakes are liable to be made in any form of inquiry, and we should remain ever cognisant of this. This recognition brings us on to the discussion of how, and why, metaphysics should be thought to be fallible, in accordance with (A).

5.3 FALLIBLE METAPHYSICS AND 'THE EMPIRICAL STANCE'

On the one hand, in order for a metaphysician to be recast and reconstituted as holding a 'stance', she must accept that her discipline is fallible. But on the other, to

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[156] van Fraassen [2002], p.231.
[157] Here, the underlying notion is that being empirically adequate is a necessary (but not sufficient) condition for a theory to be true.
[158] van Fraassen [1980], p.68.
[159] Ibid.
[160] Feynman [1965], p. 168.

claim that reason can provide one with certain truths is to fall foul of the Kantian arguments against traditional metaphysics, understood as investigation into categories of *being*, with which van Fraassen would seem to have sympathy. However, as I argued in I.2.2, this is not a problem for the realist metaphysician just because adopting fallibilism is precisely the response to Kant's *Critique*; indeed, because this move plays a vital role in deflecting the force of Kant's arguments against transcendental realism. But let me briefly survey this argument again, and ask the reader to reconsider this passage:

> For since it will not be supported on grounds of experience, but everything that is necessary should be cognized *a priori*, the principle of connection requires universality and necessity, thus complete certainty, otherwise no guidance to the truth is forthcoming at all. Hence it is absurd to have an opinion in pure mathematics: one must know, or else refrain from all judgement.[161]

Now I argued, first, that Kant's requirement for 'judging from pure reason' is too stringent, giving the examples of Fermat's last theorem, two parallel lines in non-Euclidean geometry, and the Dirac delta-function. Second, that Kant was trying to *preserve* the 'absolutely certain and non-empirical character of metaphysical knowledge'[162], and this was why he argued that metaphysical claims were claims about the structure of our ways of thinking. I agreed with Lowe's reply to Kant, which might be understood as a plea for us not to confuse *a priori* knowledge with *certain* or *necessary* knowledge:

> [E]ven granting the truth of this metaphysical assertion [that metaphysical claims must be claims about the structure of our thought about reality, if they are all to be certainties], why shouldn't we respond to it by saying *not* that metaphysical knowledge as traditionally conceived is impossible (itself a self-defeating claim, inasmuch as it is precisely a metaphysical claim as traditionally conceived), but rather that metaphysical knowledge is almost never *certain* knowledge – that is, that metaphysical knowledge-claims can almost never be absolutely invulnerable to falsification or disproof?[163]

Naturally, the instances of 'almost' would probably cause van Fraassen, or an empiricist in the British tradition, to balk. Yet Lowe would want to rejoin with 'I included the "almosts" because not to have done so would have smacked of the very dogmatism that I am opposing.'[164] And this is an incisive point. By his own credo, it would be 'false consciousness' for van Fraassen   understood as holding an 'empirical stance' – to rule out such a possibility. Think of it this way: if more than one sort of metaphysics is compatible with the phenomena, and ampliative inferences do not provide us with a means by which to choose between those systems, then we are not obliged to choose one over the other. None of this makes it irrational to commit to belief in one, rather than another, as a leap of faith, or even on pragmatic grounds. And such metaphysics would not obviously be 'trivial', nor would it obviously be mere 'art'.[165] Rather, it would involve an honest attempt to work out how the world – understood in a common sense fashion, as yours and mine – *might*

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[161] Kant [1787], A822-A823.

[162] Lowe [2002], p.9.

[163] Ibid., p.9.

[164] Personal correspondence.

[165] See van Fraassen [2002], p.30 and van Fraassen [1989], p.9

be. It might not only be commendably optimistic, but also vital as an activity at the nexus of modes of human exploration, which strives to discover the common rules underlying those modes, and to draw ostensibly different strands of endeavour together.

So realist metaphysics, in particular *the form of realist metaphysics advocated by Lowe*, can be understood to be fallible. This, and it can be understood to be compatible with constructive empiricism, if the argument in the preceding section goes through. But are these facts, taken alone, sufficient for it to be compatible with an 'empirical stance'? It seems not, for van Fraassen writes:

> [I]f empiricism is a stance, its critique of metaphysics will be based at least in part on something other than factual theses: attitudes, commitments, values, goals.[166]

Thus, what is needed, in addition, is a basis of considerable agreement, in terms of these four factors, between the empiricist who repudiates realist metaphysics, and the empiricist who endorses it (should this be allowed). In other words, for pro-metaphysics empiricists and anti-metaphysics empiricists to be able to disagree on a variety of factual theses, without significant disagreement on the basis of attitudes, commitments, values, and goals. We should look, then, to how van Fraassen characterises the empirical stance in these respects.

First, and negatively, he denounces two forms of deference that he thinks are shown to science by materialists: 'the belief that the scientific description of the world is true, in its entirety or near enough, and at least a strong inclination towards completeness claims for the content of certain sciences. "This is true, and nothing else is true" would express such claims.'[167] But little needs to be added to the prior discussion of these issues, where it has been shown that metaphysics can have more than the content of any given science as its object, and that there is no radical link between scientific realism and realist metaphysics, let alone one between naturalism and realist metaphysics (for which, see I.2.5). Realist metaphysicians need not be deferent in the way that materialists are.

Second, and positively, he writes that the empiricist takes an admiring attitude towards (natural) science, but in virtue of its methods, rather than purely its content; this point should seem familiar from the earlier discussion. He writes: 'Science is a paradigm of rational inquiry. To take it as such is precisely to take up one of the most central attitudes in the empiricist stance.'[168] But let us not forget point (B), in particular that it is perfectly permissible to inquire into the content of empirical science with *the correct spirit*, which would seem to be best encapsulated as follows:

> All our factual beliefs are to be given over as hostages to fortune, to the fortunes of future empirical evidence, and given up where they fail, without succumbing to despair, cynicism, or debilitating relativism.[169]

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[166] van Fraassen [2002], p.48.
[167] Ibid, p.63.
[168] Ibid.
[169] Ibid.

However, this spirit should also seem familiar: it reminds us of the quotation from Miller, in the foregoing argument for the compatibility of realist metaphysics and constructive empiricism. And this might suggest that the realist metaphysician who is also an empiricist may take an admiring attitude to the methods of natural science for different reasons than van Fraassen. She may agree with Popper that:

> [T]here is a method which might be described as 'the one method of philosophy'. But it is not characteristic of philosophy alone; it is, rather, the one method of all *rational discussion*, and therefore of the natural sciences as well as philosophy. The method I have in mind is that of stating one's problems clearly and of examining its various proposed solutions *critically*.[170]

In other words, it may be held that *criticism* is at the core of inquiry, and this goes for determining what counts as 'empirical evidence' as much as it does anything else, as I argued at length in II.4. And while there are consequences of this view, for example that 'normal science' in Kuhn's sense is undesirable[171], it would seem rather strange to classify Popper as anything other than an admirer of science, *when it is done properly*.

There are internal disputes in metaphysics, about how the discipline should be approached. Charges of dogmatism are sometimes levelled, as is evinced by a recent paper of Oliver's.[172] But there are similar disputes in the natural sciences: take the Solvay Conference of 1927 as a case in point. And it would be a mistake for the metaphysician to issue the charge at physicists, *en bloc*, that they are instilled with a positivistic spirit, just because the Copenhagen Interpretation of non-relativistic quantum mechanics became entrenched in the discipline (due to Bohr, Heisenberg, Born, and Pauli): the work of men such as Schrödinger, de Broglie, Einstein, and Bohm, also needs to be taken into account.[173] However, this cuts both ways. The natural scientist, as well as the philosopher of science, should be wary of picking out one group within another discipline as representative of the rest.

5.4   CONCLUSION

The overarching aim of this concluding discussion is to suggest that empiricists can be metaphysicians too, and that neither natural science, nor philosophy of science, provide a stick with which to beat metaphysics. But upon reflection, this should be no surprise. For mathematical assumptions, like their metaphysical counterparts, are almost invariably a part of scientific theories, yet few would seriously contend that an examination of their role therein could ever provide ammunition for the opponents of mathematics, or give imprimatur to the issuance of prescriptive comments about mathematical practice. And none of this is to deny that there is an important interaction between mathematics and natural science. They can lead one another: complex numbers were discovered in mathematics before they were used in physics, just as the Dirac delta-function was discovered in physics before it was given careful

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[170] Popper [1980], p.16.
[171] See Popper [1970]
[172] Oliver [1996].
[173] See Bohm [1987] and Cushing [1994].

mathematical examination. There has been similar interaction between natural science and metaphysics, and will no doubt continue to be.

It is admitted, of course, that *van Fraassen* has some powerful arguments against realist metaphysics. Four questions that are key to the debate about its status, on each of which van Fraassen has a considered opinion, have been identified: "What are its proper objects?"; "How should we understand modality?"; "What are the sources of knowledge?"; and "What is explanation?". So if there is to be a debate between the proponents of revisionary metaphysics and its detractors, then let us concentrate on these factual issues. Let us engage in rational discussion, in the hope that we might get closer to the truth of the matter. But let us not pretend that there need be any serious disagreement on the basis of attitude – let alone that philosophy of science is 'First Evaluative Philosophy' – for a practitioner of any discipline can be instilled with the critical spirit.

EPILOGUE

There might seem to be something rather strange about the position that I have argued for here. Am I pancritical rationalist, or a metaphysician? And would I give up pancritical rationalism for realist metaphysics, or realist metaphysics for pancritical rationalism? Whence the link? In concluding, I think I owe a simple and direct answer.

As a pancritical rationalist – I prefer 'Socratic philosopher', since it is less pretentious – there is part of me that wishes I did not need to depend on metaphysics, or even logic, in order to proceed in my efforts to inquire, particularly with others. In this sense, I am a Socratic philosopher more than anything else. But I am also a Socratic philosopher who currently believes that inquiry requires logic, and requires metaphysics, and hence that we are stuck with them. Moreover, I am also a Socratic philosopher who currently believes that realist metaphysics is the only self-consistent form of metaphysics on the table. But this is not necessarily a good thing.

The overarching point is that if we *have to do* metaphysics, and *have to do* logic, then we had better take them seriously. But this need not bind us, in principle, to any particular system of metaphysics, or any particular system of logic. And likewise, absolute truth is a nice regulative ideal, but it may not be the only one. Hence, I can only think that we have many exciting discussions ahead of us, to which I look forward. Let us begin, having cast our commitments into the wishing well, with the hope of achieving not only wisdom, but also good will to our fellows. We may be doomed to fail, but optimism is the master aptitude.

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APPENDIX A

HOOVER INSTITUTION
ON WAR, REVOLUTION AND PEACE

Stanford, California 94305-2323

As from Professor W. W. Bartley, III
33 Castle Lane
Oakland, California 94611
May 3, 1986 (Saturday)

Mr. Rafe Champion
77 Holt Avenue
Cremorne
New South Wales 2090
Australia

Dear Rafe,

I am just back from six weeks in Freiburg and Vienna, and am writing this in my sleep.

First to your question: Agassi's work on metaphysical research programmes, which certainly uses those terms, although he also speaks of "principles of interpretation", appears in his thesis, which I mentioned in an earlier letter to you. And I believe that Watkins gave him a footnote on this in his Mind article.

I suppose you are right to be persistent about the MRP issue.
I can accept that KRP and I have constructed an "evaluational and methodological research program" that differs from all earlier such "evaluational and methodological" programs, and would moreover maintain that much of philosophy since the 17th century has been devoted to the development of such evaluational programs. Moreover there is indeed a close connection to metaphysics, since these programs were very often intended to weed out metaphysics. But my view, in and of itself, is not metaphysical; whereas a metaphysical reseach program in Popper's sense is an untestable view of the world that guides and steers research in the sciences.

As to your historical question, I wouldn't call Agassi and Watkins's behaviour "inactivity" (Worrall comes much later, and has virtually nothing to do with the Popper story, only with the Lakatos aftermath), but we can go into all that if it interests you when we meet next summer. For as you will know from Greg Lindsay I had a confirmation of the invitation, and expect to be in Australia for part of next August. You will know the details better than I do, but I believe the meeting is in Sydney, which I know only from its airport.

Yes, there is a "check of metaphysics", as well as a check on metaphysics.

Yes, the ideas of the old programme need to be brought together and refuted as a set. I started to gather them together years ago

5

when I was at the Warburg Institute and lectured on this at the LSE and at Berkeley and have some manuscripts and tapes talking about such things, but I never had the time to carry out the task prior to the Popper-Lakatos fight, and I was too demoralized afterwards, and let it all drop.

Shock-horror? Or the desire to create the impression that one is deep, profound, different, non-superficial — that one has seen through everything, that nothing matters (indeed that nothing exists), that morality is an illusion, that all human striving and elevated sentiment amounts to no more than plastic flowers all too inadequately covering over feces and semen?

We must talk about Hayek. Must as I'd like to dissolve everything in my metacontextual pudding, I think there are real tensions in Hayek. Interesting ones. But I'll be interested to see what you come up with in your prize paper.

All the best,

Ever,

Bill

W. W. Bartley, III

P.S. I liked your "Essentialism and the Organic State", and have not finished the other things you sent. A propos of "The Austrian School of Economic and Social Theory", I learned in Vienna two weeks ago from Popper's young friend Karli Milford (son of Peter Milford and grandson of Karl Hilferding: see preface to the Poverty), that Carl Menger's two fundamental problems in his Methodenlehre were, guess what?, the problem of induction and the problem of demarcation. Writing about Tony O'Hear, who is a sweet boy whom we have all spent years trying to educate, is a waste of time. He can't learn, or rather, he is one of those many people who are in a sense clever but who can't think.

Popper is thriving in Vienna, where he is now Honorary Professor in the University of Vienna and head of the new Ludwig Boltzmann Institute for Theory of Science. (He has also just accepted a Senior Research Fellowship at the Hoover Institution, and will spend some time each year here.) He has written a brilliant paper on "Active Darwinism", with quite new and important ideas. If you read German, write to him to ask for a copy. Otherwise, you will have to wait until he writes a version in English.

HOOVER INSTITUTION

ON WAR, REVOLUTION AND PEACE

Stanford, California 94305-2323

As from Professor W. W. Bartley, III
33 Castle Lane
Oakland, California 94611
24 May 1986

Mr. Rafe Champion
77 Holt Avenue
Cremorne
New South Wales 2090
Australia

Dear Rafe,

I got your pile of manuscripts and offprints.

I particularly enjoyed the article on psychology in **The American Psychologist**, which is quite well done and very useful in getting this information to that particular audience.

About the second article: "Rafe Champion on Popper, Campbell and Stove", I wonder whether you can advise me, à propos of Anderson and the Australian invitations to Popper, whether there are archives in Australia, for example the papers of Anderson, that might be worth consulting in connection with my Popper biography when I come to Australia next summer? Or memoirs, whether in book or in essay form? I found a few of these in New Zealand, but of course Popper actually lived there for nine years.

Your article on "The Philosophy and Economics of Liberalism" is excellent, and I shall be happy to forward a copy to Hayek. But you must not expect Hayek to reply, or for that matter, to read it. As I think you know I have just returned from a month in Freiburg with Hayek, and he is in rather bad shape physically - at the grand old age of 87 - and also must cope with bouts of depression that lead him to expand his immense knowledge of English and French literature - the novel and history - but to shrink from philosophy and economics as if it were the plague (which it is). Hayek does however enjoy getting letters, and you should not hesitate to write to him, or to send your stuff to him directly, if you like, at the following address: Professor F. A. von Hayek, Urachstraße 27, D78 Freiburg im Breisgau, West Germany.

Now a few critical comments about the latter essay. These are, incidentally, not made out of modesty, for I have a high opinion of my work, and haven't any doubt that I can outdo most of my "professional" philosophical contemporaries. They are made, rather, out of a sense both of realism and of truth. In saying things like "Popper, Hayek and Bartley are perhaps the three leading liberal thinkers of modern times", you of course please me: I would love to be in that category. But what you say is not true and cannot be defended successfully; and hence you, by writing such things, reduce your credibility. I don't want that to happen.

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Stanford, California 94305-2323

A few words about how things actually stand. And these are said confidentially. I do think that my old work on rationality revolutionizes epistemology and theory of rationality and does generalize Popper's work in an exceptionally important way. I'd love more recognition for that, and I'd like it if my work eventually had some more impact and made the areas it touches more interesting. But this - although it makes a fundamental contribution to the theory of liberalism (in the European sense) does not put me in the same league as Popper and Hayek. There is perhaps a sense in which I am in the same league as Hayek: I don't doubt that I am every bit as "original" as he is, and in many ways we have the same sort of minds. But our achievements don't compare. He has written fourteen quite important books, plus a number of historical studies (of which most people don't even know) that would alone secure him an important niche. Moreover he was one of the main antagonists in the very important practical and theoretical battles of the thirties, and is one of the most influential people in the world today. And you should add to this a generosity of spirit and truly noble character that I don't have at all.

As to Popper, there is simply no comparison. I have had one really important idea and a lot of middling ideas. But he has enriched everything he has touched, and he has touched almost every area of human thought. He has many human faults, but is also a true genius, one of the towering intellects of all time. Moreover, you don't fully get this from his writings - here he differs from Hayek: everything that Hayek has to say he has written - Popper creates constantly, and effortlessly, as he talks, and the world would be a greatly richer place intellectually had a tape-recorder been running at his side for the past seventy years. It is this - and this I haven't yet published - which accounts, really accounts, for the great hostility to him.

Such richness calls up intense envy in anyone who has intellectual ambitions (and you should read Helmut Shoeck's book **Envy** to understand how these things work; Shoeck is one of the few sociologists who has seen how things are in "society"). I think you can perhaps get a glimpse of Popper's effect (if you haven't met him or met him only briefly) if you have seen the play **Amadeus** (or even the movie, although the movie does not do it so well). There is a scene where Mozart is to be presented to Joseph II, and the court musician, Kapellmeister, or whatever, Salieri, produces a little march to welcome Mozart to the audience chamber. Mozart enters, and after the presentation turns to Salieri and says something like: "That is a very nice little march you have composed for me. May I try it?" Mozart sits down at the harpsichord, plays back Salieri's march perfectly, looks up, and says: "Let's try it a little faster." He then plays it twice as fast, but reaches a chord and pauses, saying: "Well, that's not quite right, is it?" He then starts to improvize, and as you watch - or listen - as something that was competent and ordinary is transformed into something of the most exquisite beauty and originality. While Salieri writhes in pain and envy. This is the only portrayal on stage of which I know (and one of the few in literature) of true transformation, of World 3 creation, and of the ambition and envy that accompany it. Popper has this same effect - both in his conversation and in his writing.

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Stanford, California 94305-2323

Now I have already said enough things that shouldn't be said for one letter.
I genuinely appreciate what you said, and am grateful for it and touched by
it: but next time tell the truth.

With very best wishes,

Yours,

W. W. Bartley, III

APPENDIX B

Sir Karl Popper, CH, FRS

136 Welcomes Road,
Kenley, Surrey
CR8 5HH

19—11—92

Leslie Graves,
R.R. 3, P.O. Box 279, Spring Green, Wi 53588, U.S.A.

My dear Leslie,

I am sure you will agree that we adopt the American custom, and that I drop "Graves" and you "Sir". I was very happy that you have taken to Anthony Trollope: I can book this as one of my really good deeds — although you would, undoubtedly, have found him out in any case.

About the third paragraph of your letter dated November 10 (for which I thank you very much — I read it today, after returning from Japan only half alive) I have to say the following:

Your paragraph made me look up the first edition of Bill's <u>Retreat</u>, 1962; <u>the only one that I have read</u> (see below). In this edition it is <u>only</u> the note [136] that discusses our differences. Bill gave me a copy of his 1984 edition, and said that I should only read a note (which one, I have forgotten) and tell him what I thought of it; and so I did. The result was that I pointed out to him that the (weak) criticism of <u>myself</u> that was contained in this note was based on a mistake, which I pointed out to him; he accepted this. Only now (after reading your letter) did I look at this edition again and saw that it still . . . . . .

edition totally, as far as Bill's treatment [1] of myself was concerned. But I had assumed it was essentially the same book, and I had asked him ~~to~~ to tell me

where

/its main differences were — /in reply to this

and

question he had referred me, only, to that

footnote!

When I was in Japan, there was a "Workshop" or "Seminar"; and two Japanese Professors, one of whom attacked me, said that Bill, in his book Retreat..., had called my position a fideist one! It was the first time I ever heard of such an accusation by Bill. Moreover, one of the two also said that I was the main target of the ~~book~~, and that it was I whose Retreat to Commitment /the book its

gave

title! So it became clear to me that they were speaking of a/different from that which I knew;

book

and totally different as far as I was concerned! I must ask you ~~to~~, in view of your third paragraph, to read all the references to myself in the first edition; and

especially [the note 7~~X~~ to p 132.

In the Workshop~~-Seminar~~ in Japan, [I was asked for a reply. I said that Bill was one of my best pupils — perhaps the best — and stressed his interest in Protestantism. And I pointed out that Fideism — — with a capital "F" — was a simple consequence of the fact that you cannot rationally establish _any_ theory (other than a tautology). Therefore, if you accept (other than tentatively) _any_ theory, it can only be on Faith.

I then pointed out that my "critical rationalism" was in no sense a theory, and I gave them its formulation (O.S.p.225) '_I may be wrong and you may be right, and by an effort, we may get nearer to the truth_'; and I pointed out, the effort was a (problem-orien- ~~tat~~ ted) critical discussion * (as opposed to a person-oriented criticism).

Moreover, I pointed out (as I think I ~~did~~

'failing my discussion with you) that the
failed to do so

fideist argument (as opposed to what
one might call my fideism) was every-
where criticized in my theories, since I
had
allways/stressed, <u>hundreds</u> of times
(without exaggeration) that acceptance
of a theory ought to be consciously ten-
even
tative only — and, that a scientist should
<u>not</u> <u>believe</u> in his theories (though he
<u>may</u> believe that they are <u>preferable</u> to
certain alternative theories). as

I further pointed out that, I re-
attitude!!
commended my ~~theory~~ to my readers,
just as Bill did; and that, if asked
why, because I believed it was a good
attitude — indeed, a morally good atti-
tude. [I think that I said to you the
same as I say in this paragraph.]

I also said that Bill, obviously,
recommended his pancritical attitude
to his readers, and that he would
have, if asked why, to say some-
thing similarly fideistic.

I said, finally, that my formulation
(i.e. I may be wrong....) makes it really
impossible to accuse me of any form
of _dogmatism_ — which is, of course,
the characteristic aspect of all
Fideism. And that my formulation
was not abstract, need not "define-
able", which I thought was impor-
tant.ₓ

Now, after speaking to you, and
after having in Japan for the first time publicly
defended my position against Bill,

I should say, now, that the last paragraph on p. 231, of O.S. ~~may~~ be misleading. I should, instead, have said something about Fideism (as here), and pointed out that my (of course, tentative) belief that the attitude of critical rationalism should be accepted, although it is a belief, is/ ^obviously^ not a form of ~~Fideism~~ (since it is not an irrational acceptance) but a form of ^tentative^ fideism, which, whenever we recommend anything, we should hold, if ~~we are~~ as intellectually honest ~~people hold~~. Of course - at the time we recommend the ^tentative^ acceptance of that attitude; and of course, only as long as we recommend it.

All this seems to me, obviously, meant by what I said — unfortunately, /so mislead- ingly to Bill.

So this improvement, in verbal formulation, I gladly admit, I owe to Bill.

Moreover, I ought to eliminate my concession to irrationalism, since it misled Bill. (Although I am doubtful on this point: there is an irrational element — though not a fideistic one — in the adopt- ion of any neutral attitude — even if the adoption is, clearly, tentative.)

With all good wishes,

Yours sincerely

Karl (Popper)

P.S. From the reaction of the two Japanese Professors to my

formulation of critical rationalism

it seems to me that Bill never divulged this formulation! He certainly never did in the 1st edition of his book. But that was his right: he pointed out in note 7 on p. 132 that that his "critical rationalism" was a bit different from mine (which he was not discussing; and therefore he was under no obligation to explain mine. I should like to hear from you whether he quoted my formulation in the 2nd Edition in which, as I only now know, he criticized me severely. From your own reaction I suspect the answer is "no". But if it is "yes", then please give me the page reference!

Yours KRP

P.P.S It is clear that, according to Bill's terminology, I always was what he calls paucritical. This fact explains why I found his attempted to scholastics much worse

(margin, left side, vertical text)
my own — and why I offered him to improve my own formulation;

Letter the second! [1] 20-11-92

Dear Leslie,

Although I am not yet recovered from my extremely tiring trip to Japan, I used the night from the 19th (yesterday's letter) to the 20th (today) to re-read Bill's _Retreat_ in its first edition. I had not looked at it for 30 years! I found it an excellent, and moving book, extremely well-informed about the movements in the protestant Church, brilliantly written; a _very_ high level for a Ph.D. thesis. It is very appreciative of my own work (which is, indeed, his basis). And it carries it on, and applies it to a problem — Protestantism — which does not really interest me at all. (I think that all theology is blasphemy.) But Bill does make these problems interesting! I think I told you so when you were here. The book is also very appreciative about my work — except "critical rationalism" which he attributes to people like A.J. Ayer. (By not mentioning me at all in connection with "critical rationalism", except in Note 7 on p 132, he certainly did confuse things; but these

things happen.)

You should use this appreciation of
Bill's really extraordinary, beautiful book. None
of my students has written a similar-
ly good book, and a very readable
book.

As to the second edition, he told me
except for a footnote,
not to read it (as he also told me of
his three (?) papers in an Israeli Journal).
And I don't intend to read it, or these papers.
It _seems_ to me a rather different
work, with great claims, and quite
misrepresenting my "critical rational-
ism". I should be grateful if you
beautiful
would read the first edition, and
give me your impression. (It almost comes
near to Barchester Towers!)

There is now some material for you to
use — and a lot of work.

Yours sincerely
Karl

(It now comes, somehow, as an

APPENDIX C

[He is clearly feeling very disturbed, so I urge him to continue reading which, however, he does not do at this time.]

Sir Karl: I want actually to say something about, about the thing which led Bill on to get, in my opinion, let us say, trapped in this debate about CCR. See, it...I could have left it, and perhaps should have left it, by saying: I mean by rationalism the attitude, "you may be right, I may be wrong and together we may get nearer to the truth by discussion." I could have left it with that. Perhaps it would have been the best. I said the addition to it was something which, I think, is the point which Bill really didn't like, namely, that one, I, or...perhaps I had the attitude of faith in reason. Now instead of saying this, I could have said that I believe or I have to face that this attitude "you may be wrong[quickly correcting himself] right, I may be wrong and together we may come to the truth," is, let us say, a good attitude or an attitude which I would like to propagate among people, especially among intellectuals. That is all I meant. Now, in propogating this attitude, let us call it, let us call mine CR, "I may be wrong and you may be right and together we may get nearer to the truth," let us call this CR. Now, it isn't enough just to formulate such an attitude, it is also that one adopts the attitude.

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The adoption of an attitude of course is nothing absolutely finer. I adopt this attitude and I try to propogate it, but if somebody comes, if Bill comes and says this isn't quite the right attitude: you should say also that even in your adoption of this attitude you are open to criticism, you should add that. Then I would have said, yes, yes, but does that not lead, if one goes on, to an infinite regress? That is to say, if I say yes to you and say: now, I adopt CR but I adopt CR with a comment that I'm quite ready to learn that CR isn't the best, is not then what about if another Bill comes and says that's not quite in order: you should say that even this attitude isn't quite the best so we go on to infinity and that is dangerous and therefore it's just as well we go back to CR. It isn't a dogmatic attitude. I haven't said, "I may be right and you may be wrong." I have started with, "I may be wrong and you may be rightm" and this prevents dogmatism. And, is itself enough to show that we should be open. So this in a nutshell, now, is really why I wasn't in favor of CCR without going in all these things. And I do think actually that the debate with all its unpleasantness is just what we have now said. That is to say, CCR if improved in this direction, CR if improved in this direction and if we add to it that we are really not even completely bound to CR is not

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better because if we accept that this is an essentially improvement we are directly accepting an infinite regress. So, and it isn't necessary and so on. Now, in Bill's form it was much more complex, and this complexity in itself, is I feel not right in questions which are fundamentally moral questions. And in these, really it is actually that the adding of another phrase is something which has, for totally different reasons, not for logic reasons, but for totally different reasons, is bad. Namely, it excludes people who could...perhaps...find CR very attractive, but who would...CR with that new addition...with that new addition, find a bit too, how shall I say? Too intellectual. And this is a quite _different_ criticism and, in my opinion, that is a very _important_ criticism. Not criticism now in Bill's sense but criticism in sense of human effectiveness and human attractiveness. And I think really that as far as the group of my students concerned who became involved in this debate: they were all damaged by it. All of them, not only Bill. One shouldn't be too wise, too clever... better. I better say, one shouldn't be too clever. It is the kind of cleverness which repels not only simple people but I would say, for example, scientists. A scientist is not the man who is for such finesses, you see. It's a finesse and one shouldn't have finesses. And I tried

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to explain this to Bill and I'm sure it hurt him and of course I didn't want to hurt him, but I was after all his teacher and had to tell him what I thought. And anyway, I do think...how shall I say? It was...what he wanted was implicit in saying "I may be wrong," instead of, "I may be right and you may be wrong but together we'll find it," by starting with "I may be wrong and you may be right." This prevents the kind of thing which the additional phrase would have prevented, let us say, would have prevented, namely: What can it prevent? Dogmatism, even...how shall I say? logically seeing it the two formulas, "I may be right and you may be wrong," logically but by putting it in the way, "I may be wrong and you may be right," by putting it in this way, I imply in a non-logical way that this is under no circumstances to become a dogma. This is implied   and so I even now think he was wrong. I may have been wrong to say faith in reason. Now let me say something about that, somewhere it has to be admitted that this is an attitude one adopts. This question of adoption, of adoption, this is a special question, quite different from the formulation of what I adopt. He discussed only the formulation of what he thinks we should adopt. The difference between CR and CCR is only a difference of the attitude which we should adopt. But why should we adopt it? See, the

question of truth makes it clear. If because it is true, but that we don't want to say, we don't want to be dogmatic. So, he can only answer: why should we adopt it? Why not? But why not is not good enough. He can then say: all right, why not is not good enough, I adopt it. But that is, there we are faced with a dogma. You can say, "why shouldn't I adopt it, I'm free to adopt anything." But because I'm free to adopt anything, I can just as well adopt the opposite attitude. So we are back to that question. So the adoption question is a separate question and it is this question which I meant when I said faith in reason. All right, I admit I shouldn't have said faith in reason and I wouldn't have said it if I had his background of discussion of Protestant faith and so on. But in this point, it did not help me. I would now say, faith in reason, I mean no more and it could be replaced by the following: This is an attitude which first of all I adopt, and secondly I would recommend my readers to adopt. And if my readers ask me why do you recommend me to adopt it, of course, I simply say, because I believe it's a good attitude and there comes the word belief in of faith and I felt I should not subverse such a point even though I'm not for faith in theories. But after all, faith in an attitude, let us say, in the attitude of friendliness, to take something

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very simple. I believe in the attitude of
friendliness. Start with, start if you meet a person
for the first time, assume that he or she is a nice
person and be friendly to him or her. That is, we
can't get away from such things. So I think, I think,
somehow or other that he did not achieve, even with
everything, apart from all that there is written about
what is rationalism, there is a question, do you
yourself adopt the attitude of rationalism? To which
he probably would have said: provisionally. Oh yes,
yes, I wouldn't have said anything else. (Note: last
sentence not entirely intelligible) I adopt it at
present provisionally. Why do you adopt it at present
provisionally and not the other attitude? I can't get
around it because I think it's the right thing to do.
Why do you think it's the right thing to do? We can't
go on like that forever. It's just so, that is what we
need. It is, faith in faith different from the faith of
the religious people of the protestant with a small "f"
rather than with the big "F"? I don't think that this
is a faith in which the fact that it is a faith means
anything very specially great or good or admirable, or
anything like that, but it is just that I think it is
the best thing to do and we... beyond that we can't
go.[2]

All this I have tried as well as I could to explain to
Bill. Probably I can, it perhaps...now better.[3] That
is possible. But it goes in a completely different
direction from what Watkins and what is his
name...Post...have discussed and it remains very much
near to what I have said in the Open Society and it is
not because I am dogmatic and stick to what I have said
in the Open Society. I'm, I'm....how should I say? I
explain it to Bill. I'm happy for any improvement and

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[2] Or, as someone else once put it, "what we cannot talk about we
must pass over in silence." Or, as the same author wrote, "The
book's point is an ethical one. I once meant to include in the
preface a sentence which is not in fact there now but which I
will write out for you here, because it will perhaps be a key to
the work for you. What I meant to write, then, was this: My
work consists of two parts: the one presented here plus all that
I have not written. And it is precisely this second part that is
the important one. My book draws limits to the sphere of the
ethical from the inside, as it were, and I am convinced that this
is the ONLY rigorous way of drawing those limits. In short, I
believe that where many others today are just gassing, I have
managed in my book to put everything firmly into place by being
silent about it."

I have accepted his improvements and so on. It wasn't any dogmatism. It was really...and, and David's paper has the same fault. It's the same fault. It gets into intellectual questions and so on and behind these questions is this infinite regress, of course. And, how shall I say? One need not to be clever to see this infinite regress? And the infinite regress may be turned into something else and, but I hate to be so clever as to bring Godel in here [this refers to David's paper. You understand. So I think all this is really an error in...how shall I say?...an error just in that way in which it damages rationalism. It tries to make rationalism stronger than it can be made and of course thereby <u>really</u> makes people say tu quoque and so on. You get into that mess, and he did get into that mess. And so did David and everyone else. So I think this part apart from, apart from, from eliminating repetitions and such things or where I was ungrammatical, I think you [He is referring to what he wants Leslie to do in editing this for publication] should really, essentially, adopt this I have said. Not too long.

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[3] If it's not clear, he means that in 1992 he is better able to explain this than he was able to thirty years earlier.

Leslie (spoken):   No, I understand.

Sir Karl: But anyway, the question of which he so to speak had

forgotten, there is an attitude and the question is why

do you adopt the attitude? Then you can either say:

why not, why shouldn't I adopt it? If you criticize

it, I won't adopt it if your criticism is good, but

still, there are so many other opposed attitudes which

one could adopt, so you have to say "I like it," at

least, "I like this attitude." Perhaps that is better

than to say I have faith in reason..."I adopt this

attitude because I like it." But I...it's a mistake

to overlook that we have to confess that we adopt the

attitude. Hm?

Leslie (spoken):   I understand what you're saying. I'd like you

to finish reading this and then we'll go on to some

other things.

[But Sir Karl continues his train of thought.]

Sir Karl: It is perhaps the most important point that it is very

sad if a man like Bill gets lost in that. And I really

did my best to prevent him and he only felt that I for

some probably unconsious reasons, I didn't want him to

make this important discovery. And I really was, even

then, afraid...not very afraid because I thought he

will get it, get over it, but I was afraid that he

---

would lose himself in this nonsense. So I felt it was, you see? And I mean, all this is theory, but my theory is that he thought I wasn't ready, I was too dogmatic, I wasn't ready to admit that somebody else like he could make a great contribution to a very important subject. Very sad. Really very sad. And this is the first time that I will publish a work about it, direct about it. But I [he now continues reading my previously prepared comment, reading part of it out loud]

Sir Karl: yes, 'what I would like to say is that Bill cannot have'... (is reading my question, or statement, reads the part "his life cannot have been a failure in any way"...

Leslie (written): Anyway, what I'd like to say is that Bill cannot have personally regarded his life as having been a failure in any way.

Sir Karl: I'm afraid, I really am afraid, he had regarded it a bit like that, but this we shall not say, I...perhaps failure is too much, but I do think he knew he could have or should have made a contribution to philosophy rather than to, to, biography. That is, I mean it is more creative to have new ideas rather than to describe the life of a man even if the man is more important than Mr. Erhard...or what was it? is it? (Leslie:

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Erhard is correct) Hmmm? (Leslie: Erhard's correct)

Yes, yes. (Leslie: read this) I think, I think, he

somehow felt it. That is very...very likely to be

true, what you say here and yet, there, I am sure there

was an element of disappointment in what he has been

able to do. You must read this book, this thing, book,

where Braithwaite plays a role. (Leslie: OK.) You

must read this book. There are ideas developed which

perhaps are not entirely his but which I'm sure have

never been so well put and which as I say were over my

head. That is to say, I would never have looked at

religion in this way. It's a real, very important,

book, and if a man can do that, he could do more than

that about, this man, Werner Erhard, who after all is a

bit of a swindler. I mean, the whole organization, all

that, it is a very dubious affair. That again is

between me and you.

Leslie (spoken): Right

Sir Karl (reading out loud some sentence fragments):

"he was interested in..."

Leslie (written): He didn't regard any part of it as having been

wrecked, and certainly not by anything to do with you.

Bill was interested in learning new things; in the

and music played a, the, major role in that school and
she was very happy, all through her school years, so
(Leslie: we've done all those) Yes. (Leslie: Now I
have to give you this, and I will take this) This very
fact that this [Sir Karl is holding and referring to
David Miller's contribution to the memorial volume,
which David had faxed to him on the previous day] has
so many pages and so many people and so much
literature, I mean all that is somehow really, really
wrong, not only an error and not very clever and so on,
but morally wrong, because it destroys the moral aspect
of that attitude. Do you...? I couldn't read it
beyond page 3. The paper is for the book? (Leslie
laughs, somewhat hysterically, and says: well, anyway,
here is a present for you. I found it in the bookstore
this morning. She gives him a copy of Eric Lerner's
book about the big bang, which he had expressed an
interest in on Tuesday.) Does it say what he is? Yes.
(Leslie: And here's one more, The Creative Moment)
I...I believe not that the big bang never happened, but
that we don't know anything about it. I mean, I have
no reason to believe in the big bang. Who's Joseph
Schwartz? I believe that Einstein's theory has been
now refuted. It was a very interesting theory, and it
was the first theory since Newton and it had
interesting aspects which made it ...tape runs out and

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is changed... yes, yes, that is right.

unintell....interesting. He got a good publisher,

Jonathan unintell...the great, yes...seems to be good,

probably better than that. This [referring to Lerner's

book] is too big. It really is. This is not true,

Orsted did not discover the field concept. It is true

that Orsted is very important, but he did not do that.

Of course, one can, he could talk himself out of it,

but...I'm sure I should have to object to more in that

even than in the other. He is very dogmatic in his

formulations. He writes dogmatically. He makes all

sorts of assertions. I mean, just opening it I found

at least three assertions which all three are not quite

in order, you see, yes, he's very dogmatic. Thank you

very much.

[Leslie writes a note to the effect that she feels it is time to

wrap things up.]

Sir Karl: It is true that I am not well, you see, I don't know.

Do you know about blood pressure? (Leslie: Yes) Yes.

I had on the night you left, I had what I have

sometimes, a bad tachychardia, with I have a machine to

measure it automatically which I rang the Mews and Mr.

and Mrs. Mew came and they brought the machine down and

measured and I had 183 and the blood pressure was 70

over 40 which is almost nothing, it is really very

very bad. I, so to speak, really almost died, because

it was practically nothing. And, I didn't dare to lie

down before they came because of I didn't know that I

could get up again but then they came, I lied down and

then it settled itself soon. So in a way it was

nothing, but it made me feel very weak. I mean that I

wouldn't have believed it if the machine hadn't shown

it. The pulse, also the machine measures the pulse,

you see, I couldn't feel it any longer. It was really

pretty bad, so it is wise if I don't do much work, and

I thank you, I'm sorry that you are leaving tomorrow,

and I can't, I hope I have also encouraged you a

little.

Leslie (spoken): Yes.