THE
SCEPTICAL
CHALLENGE

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RUTH
WEINTRAUB

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THE SCEPTICAL CHALLENGE

Ruth Weintraub

To the memory of my father and for my mother

CONTENTS

ACKNOWLEDGEMENTS

ABBREVIATIONS

ATL   Sextus Empiricus, *Against the Logicians,* trans. R.G. Bury,
          the Loeb Classical library, London: W.Heine-mann, 1935.
CSM I, II Descartes, R. 1628. *The Philosophical Writings of Descartes,*
          trans. J.Cottingham, R.Stoothoff, and D. Murdoch.
          Cambridge: Cambridge University Press, 1985, 2 vols.
PH   Sextus Empiricus, *Outlines of Pyrrhonism,* trans. R.G. Bury,
          The Loeb Classical Library, London: W.Heine-mann,
          1933.

INTRODUCTION

Many philosophers suppose that scepticism is irrefutable; that we cannot vindicate our claim to know anything. But they are not all impressed by this very pessimistic conclusion. Some shrug scepticism aside as a mere curiosity; a philosophical aberration. Many factors combine to explain this seemingly puzzling attitude. The practical implementation of the sceptical doctrine encounters insurmountable difficulties. Its proponents are judged insincere and frivolous. Most importantly, there are ways of construing the sceptical challenge which render it self-defeating.

These issues must all be addressed by the sceptic. But when they are, and the sceptic's challenge is *properly* launched, it is a powerful one, to be contended with, rather than patronizingly dismissed. My first task in the book is to present a sceptical challenge which cannot be shrugged aside. But, then, once the nature of the challenge is understood, the pessimism it has engendered is seen to be misplaced: the *genuine* sceptical challenge is answerable. My second task, then, is to present a response.

The book, accordingly, is divided into two parts. The first presents the sceptical challenge, and the proper methodology to be adopted in responding to it. I begin by distinguishing various sceptical positions, and arguing that we need most urgently to contend with scepticism which denies the justification of our beliefs. Scepticism about knowledge, I suggest, is less worrying. The central role assigned to the concept of knowledge in epistemology is mistaken (Chapter 1).

I then go on to consider two problems which threaten to undermine scepticism. The first (Chapter 2) is practical, and concerns the sceptical life. Despite the impossibility of implementing the sceptical doctrine, I argue, we are not absolved from the need to contend with the sceptical position.

The second problem is theoretical, and pertains to the sceptical thesis. Notable sceptics have taken it upon themselves to adduce

rational arguments for their position, rather than merely challenging us to vindicate reason. The sceptic presents us with a paradox: a plausible argument with a repugnant conclusion. I illustrate this claim by considering the writings of Sextus Empiricus, Hume, and Descartes. My interpretation of Descartes is contentious, but it renders his sceptical challenge particularly formidable (Chapter 5). Since the rules of the engagement with him are the rules of rationality, we can only dismiss him on pain of being irrational ourselves. And his sceptical paradoxes are very hard to defuse.

This kind of sceptic faces a serious difficulty, however. If he attempts to adduce arguments in defence of a sceptical claim so as to be philosophically relevant, isn't his position self-defeating? For aren't the sceptical doctrine and the reasons cited in its support themselves unjustified? I consider various ways in which the sceptic might attempt to evade this dilemma (Chapter 3). Once the problem is solved, his challenge has been mounted, and the burden of proof is firmly on us.

In Chapter 4 I focus on the response to the sceptic. I criticize an influential school of thought which advocates the replacement of traditional epistemology with a naturalized version, rendering it part of science. The dispute with the sceptic, I argue, is philosophical, although we can also appeal to scientific considerations. But these do not suffice. We must rebut the sceptical arguments by locating the fault either in the premises or in the reasoning. To this end we must analyse epistemic concepts such as 'rationality', 'knowledge', 'justification', etc., and show that the seemingly plausible assumptions the sceptic invokes about them are, in fact, mistaken.

In the second half of the book, I respond to the sceptical challenge as it has been posed in the first part. I consider some sceptical arguments, and attempt to show that they are not cogent. I first present Hume's argument against induction in the most favourable light, obviating many objections which are often thought to vitiate it (Chapter 6). When thus presented, the problem of induction becomes but a special case of the ancient Greek criterion argument. But this sceptical argument can also be applied to deduction. Philosophers have ignored this analogy at their peril, tending to focus on inductive scepticism. By considering the argument in its full generality, we can see how it is to be rebutted in both cases (Chapters 7, 8).

In Chapter 9 I consider and reject various arguments (presented by Hume, Russell, van Fraassen, Williams and others) for scepticism about the unobservable: other minds, the external world, theoretical statements in science, etc. Finally (Chapters 10, 11), I consider how we might respond to Descartes' two famous sceptical legacies: the demon and dream arguments. I show how they can be rebutted by invoking various considerations pertaining to the nature of justification.

The strategy I employ against the sceptic is, on the one hand, modest: it does not yield a conclusive refutation of scepticism, but, rather, a piecemeal rebuttal of individual sceptical arguments. But, on the other hand, it shifts the burden of proof back to the sceptic. Pending further arguments, our claim to believe justifiably is vindicated. We can reject the unpalatable sceptical doctrine with a clear intellectual conscience.

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KNOWLEDGE, JUSTIFICATION AND TRUTH

INTRODUCTION

Scepticism can be more or less radical, or comprehensive, depending on the set of beliefs upon which it is directed. But it can also vary with respect to the *kind* of doubt it casts. One sceptic questions the justifiability (or rationality) of our beliefs, and another doubts our knowledge claims. In this chapter I shall argue that we should focus on scepticism about justification and need not separately address scepticism about knowledge. The argument proceeds in the following way. I first clarify the notion of justification which is most pertinent to our concerns, and explain its epistemic significance. Then, I consider various ways of interpreting knowledge attributions, and argue that even when very modestly construed, knowledge claims are undermined by justification scepticism, and, furthermore, that we needn't be perturbed by knowledge scepticism which leaves intact justification claims. Just fication, rather than knowledge, I shall conclude, is the central epistemological notion.

THE CONCEPT OF EPISTEMIC JUSTIFICATION

Our beliefs are the product of a complex causal process. The term 'justified' (and its synonym 'rational') is used to evaluate certain aspects of this process. This epistemic appraisal is made relative to one particular *desideratum:* maximal truth- and minimal falsity-content in our beliefs. To use Popper's (1972) terminology, we wish to attain a maximal degree of verisimilitude, our regulative ideal being 'the truth, the whole truth, and nothing but the truth'. With respect to this ideal, belief can be more or less successful, and in this it is akin to action. But

there are important differences. Most obviously, belief is less (if at all) voluntary.

An objection to taking verisimilitude to be our cognitive objective should here be preempted. It must be conceded that attempts to define verisimilitude have not been successful. But this, far from impugning the concept, shows it to be pretheoretically meaningful. If we did not have intuitions about verisimilitude, we couldn't cite counter-examples to proposed definitions. We need not wait for an adequate analysis, just as we do not eschew the concept of causation pending its analysis. One may not even be forthcoming (for verisimilitude or for causation): why think that every natural language expression can be defined?

There are other standards by reference to which belief can be assessed, most notably the practical one. There is some overlap between the practical and the cognitive value of a belief: plausibly (*pace* Stitch, 1990, ch. 5), our actions are (typically) more successful when guided by true beliefs. But some beliefs which hardly affect action may be theoretically important: we are interested in understanding reality, not just in controlling it. Furthermore, belief has consequences in addition to the actions it motivates. In terms of these consequences, a belief of greater verisimilitude may be less beneficial, as the following two examples show.

First, a terminally ill person may reasonably want to have a false belief about his situation: some beliefs are just too awful to have. Second, if persuaded by Pascal's argument, one will have a reason for believing in the existence of God, even if his existence is very unlikely. In both cases, the goal of the agent in forming his beliefs isn't cognitive (increasing truth and avoiding falsity). He aims to have beliefs which are—although false—practically beneficial.

I should make a terminological point here. One could restrict the use of the word 'justified' to express a purely epistemic evaluation, and use the word 'rational' more widely, to incorporate other considerations (practical, for instance). But my concern is epistemological, and I shall not be saying much about practical rationality. For stylistic reasons, therefore (so as to avoid repeating the word 'justified'), I use the words 'rational' and 'justified' interchangeably and narrowly: 'justified' ('rational') should be read as 'epistemically justified'.

Justification is most paradigmatically concerned with the inferential aspect of belief-formation. Rationality, after all, has

been defined as 'the use of reason or logic in thinking out a problem' (Collins English dictionary). But this does not mean that logical omniscience is required for rationality. Economists and philosophers who use the term 'boundedly rational' to characterize (human) agents who are not logically omniscient are departing from ordinary usage. We do not think ourselves irrational in failing to believe in Goldbach's conjecture even if it is a logical consequence of axioms (Peano's) which we accept. This is best explained, I believe, by the supposition that judgements of rationality are relativized to agents' cognitive capacities. It is the *agent's* reason, *his* intellectual potential for making inferences, which constitutes the standard relative to which assessments of rationality are made. If we ignore this relativization, we may be tempted to think of rationality as a skill (Reynolds, 1991). In fact, a rational person is more aptly likened to one who properly uses his (possibly mediocre) intellectual skill.

There is a difference between irrationality and stupidity. A logical wizard who hasn't drawn a logical conclusion from the evidence is failing to use his reason, and is properly branded irrational. The label 'irrational', on the other hand, should not be attached to someone who hasn't drawn the conclusion because the inference is beyond his logical capacity. *Ceteris paribus*, a consum-mate logician is required—if he is to be justified—to display greater logical coherence than the average person. He is blamed for harbouring 'remote' inconsistencies, for instance.

I have argued that judgements of rationality must be relativized to agents' (logical) capacities. But this claim engenders the following difficulty. Can one fail to use one's capacity to reason? True, psychologists suggest that people reason fallaciously even when they are capable of assessing the evidence correctly (Nisbett and Ross, 1980). But it might be objected that the only evidence we have for a person's reasoning ability is the way he actually reasons. Properly understood, the claim is correct but doesn't constitute an objection. We may grant the absurdity of ascribing a capacity the exercise of which is always curtailed. But when a musician (or an athlete) is 'off form', the quality of his performance falls short of the standard he can attain; a standard which is displayed on other occasions. In reasoning, admittedly, some fallacies are perpetrated by the same agent *regularly,* but they can still be characterized as lapses from a capacity (rather than cases of incompetence), whose ascription we

can substantiate by appealing to inferences of equal complexity which the agent draws successfully. Complexity can be characterized formally, independently of agents' inferential performance.

I now come to an additional feature of justification. To be rational, a belief should be properly caused. This consideration can be wielded in two kinds of case. First, an agent who believes something as a result of reading his horoscope isn't justified in his belief even if it is entailed by something else he believes: his belief is caused by something which isn't evidence for it. The second kind of case is that of a belief which is formed under pressure from some affective drive (wishful thinking, ideological bias, etc.).

THE CIRCLE OF BELIEF

The conception of justification which I have articulated contravenes the popular thought that '[t]here is no exit from the circle of one's beliefs' (Lehrer, 1974, p. 188). This vague intuition I shall construe as the claim that justification supervenes on the totality of one's beliefs: two agents with identical beliefs will be in-distinguishable with respect to which of their beliefs are justified and to what degree. More formally, a property, P, supervenes on a set of properties, X, if and only if for every y, if y is P, there are properties $Q_1,\ldots$ in X such that y has $Q_1\ldots$, and, necessarily, whatever has $Q_1,\ldots$ has P.

How is it that we can exit the circle of belief? If rationality depends not only on *what* the agent believes, but on how his beliefs are caused, then two doxastically identical agents may be justified to different extents. For instance, an agent who has repressed from consciousness all evidence against a proposition he wishes to believe has subverted reason so perfectly that detection from 'within' has been rendered impossible. His beliefs are evidentially coherent, but he isn't justified in them.

Bonjour (1985, pp. 150–1) would disagree. He points out that we judge justified the person who is deluded by an evil demon or a mad scientist. And the deluded person is analogous, Bonjour thinks, to the person who has repressed evidence. The fact that 'the mechanism which produces the beliefs is in some sense part of the person rather than an outside force', Bonjour claims, is irrelevant. '[In] relation to his overt cognitive processes his unconscious is an outside force.'

There seems, in fact, to be a world of difference between the two cases. It is the agent, and not his conscious mind, that we are

assessing. Epistemic rationality requires that one be motivated unitarily, one's goal being verisimilitude. And the wishful believer, the coherence of his (conscious) belief system notwithstanding, is being motivated by a competing goal. We needn't condemn him for this: truth-related reasons do not exhaust the field. But a belief which is motivated by other considerations just isn't *epistemically* justified, even if it is—everything considered—reasonable. Note that this is only a necessary condition. That an agent forms a belief with a view to improving verisimilitude isn't sufficient for justification. If a student lacking in self-confidence brings himself to believe he is capable of great things so as to overcome his anxiety and improve his capacity to learn, his belief is not (epistemically) justified (Smullyan, 1980, pp. 131–2). To be epistemically justified, a belief should be formed with a view to its own verisimilitude.

We can cite a second reason for thinking that justification does not supervene on belief; that irrationality, reason malfunctioning, needn't be discernible from the agent's beliefs. Perhaps below a certain threshold of logical competence the epistemic concepts 'justified', 'rational', etc., are not applicable. But above that threshold, appraisals of justification are sensitive not just to what the agent believes, but also to what he is inferentially capable of believing (see the previous section). And this means that two doxastically identical agents may be differentially justified. A fairly subtle inconsistency within a belief system may count as irrational in the case of the logical wizard while being perfectly justified in the case of a moderately intelligent person.

WHY DOES JUSTIFICATION MATTER?

Why should we care about being rational if our aim is truth (verisimilitude), for which justification is neither a necessary nor a sufficient condition? Well, justified beliefs are at least more likely (objectively) to be true than unjustified ones. This link between truth and justification has been contested. In defending induction, Goodman (1973, p. 62) has argued that we do not 'know that [our] predictions will turn out to be correct', nor even that they will turn out to be correct more often than random guesses. We are, nonetheless, justified in holding them, because they conform to our inductive practice.

Goodman's concession is fatal. Perhaps we cannot *persuade* the sceptic that our practice is truth-conducive, rather than a mere convention, but *we* believe that it furthers our cognitive aims. It would otherwise be arbitrary, and rightly denigrated by the sceptic.

That justification is truth-conducive doesn't quite explain its peculiar significance in epistemology. Rational people may do better vis-à-vis the truth, but so do people with good eyes. So what is special about justification? It may be thought that justification is important as a practical aid to belief-formation. Bonjour, thus, suggests that justification is a

> *means* to truth, a more directly attainable mediating link between our subjective starting point and our objective goal. We cannot, in most cases at least, bring it about directly that our beliefs are true, but we can presumably bring it about directly (though perhaps only in the long run) that they are epistemically justified.
>
> (Bonjour, 1985, p. 7, original italics)

We should presume no such thing. We do not have a privileged epistemic access to justification. It is quite easy for me, standing outdoors, to discover that your belief in impending rain is false, and rather more difficult to glean that it is irrational: caused by some wish, rather than being based on an erroneous weather report. The same is true in my own case. The causal psychological facts on which the rationality of a belief depend may be more difficult to discover than the state of affairs upon which it is directed.

Justification also depends on logical (deductive and inductive) relations between beliefs. But even if these are a priori, they can be more difficult to discover than the truth-value of the (empirical) propositions they relate. Were Galileo's opponents irrational and bigoted? Or was theirs a 'well-defined, sophisticated and empirically successful system [and Galileo's]…an unfinished and absurd hypothesis' (Feyerabend, 1975, p. 153)? This remains a controversial question even when the truth of the theory is no longer in doubt. The extent to which a scientific theory is justified depends on its simplicity, the variety of evidence in its favour, the plausibility of auxiliary theories jointly with which it gives rise to observable consequences, and its (perceived) fruitfulness in solving new puzzles (Kuhn, 1970). The assessment of these is highly susceptible to error.

One might suppose that our epistemic access to justification is privileged at least in the following modest sense. Isn't the belief in p justified, one may wonder, if the belief in 'p is justified' is? With respect to justification, it could be argued, there can be no gap between justifiably thinking oneself justified and actually being justified. This might be one instance where truth and justification go together.

In fact, they don't. If the belief in p is caused by a wish, the belief in 'p is justified' typically is as well (the same wish, in fact). But this is a psychological fact about wishful thinking (on which depends its efficacy). It need not always be the case that wishful thinking causes both beliefs. It is logically possible for an agent irrationally (wishfully) to believe p while rationally believing 'p is justified': judgements of rationality (those about oneself included), dependent as they are on subterranean psychological facts, are subject to rational error.

Justification isn't 'a path to truth' (Bonjour, 1985, p. 8) in the sense of being more easily recognized. So where does its significance lie? Well, from the first-person perspective justification and truth are very intimately linked. I cannot (rationally) judge false a proposition my belief in which I judge would be justified. In the case of someone else this possibility is quite straightforward: my two judgements (concerning the truth and rationality of his belief) are made in the light of something I know and he doesn't. But if it is myself I am assessing, then anything I know which undermines the truth of a proposition *ipso facto* undermines the rationality of my belief in it. The converse is also true: when I judge that my belief is unjustified, I *ipso facto* judge that my confidence is misplaced. This is not true when the judgements are made about someone else, where I may have information he doesn't.

Although justification isn't particularly easy to detect, it nonetheless has a regulative role with respect to belief modification and retention. To characterize one's belief as justified is to approve of it: to mark it as one which one ought to hold on to (or adopt) if one's aim is verisimilitude. Belief-formation goes by justification because, roughly, that is our best (cognitive) strategy.

These considerations suggest that we shouldn't be perturbed by Unger's (1975, p. 199) suggestion that 'there is at most very little in which one will be justified or reasonable'. According to Unger (1971), justification requires a logical guarantee of truth, and that is

why we are so seldom 'justified'. Now, we may wonder whether Unger is misusing the term 'justified'. But we needn't squabble about the 'real' meaning of the term. Unger's construal, even if it faithfully captures an existing concept, isn't regulative. It wouldn't be a sensible policy to use it as a doxastic standard, and be guided by it in forming our beliefs. Even if we can err, still it may be sensible to believe, risking the occasional error, so as to increase the truth-falsity ratio in our belief system. This is true even when we imagine ourselves to be conducting—in Descartes' manner—a 'pure inquiry' (B.Williams, 1978), unhampered by practical constraints. Even when our time and resources are unlimited, and we need not act on imperfect information, why hold out for ever? Since we cannot reach certainty, it is much more sensible to form fallible beliefs as best we can, modifying them as our inquiry proceeds, gradually getting nearer our goal. Unger's scepticism about justification can be conceded and shrugged aside: it doesn't impugn our doxastic practice, and we may be doing quite well even without his 'justification'.

Here we must part company with Descartes. He enjoins us to 'reject all such merely probable cognition and resolve to believe only what is perfectly known and incapable of being doubted' (CSM I, p. 10), because 'there are [many]…truths which suffice for the *sure* demonstration of countless propositions' (CSM I, p. 11, my italics). But this does not show that there is no need to settle for less than certainty, since Descartes immediately goes on to say that certainty can be attained only in mathematics. Adhering to his rule, 'there will be very few things which we can get down to studying'. The second reason Descartes gives for shunning merely probable knowledge is psychological. Men 'of learning' find it 'unbecoming…to admit to being ignorant on any matter', so they 'pass…off as true' their 'contrived doctrines', which are 'merely probable'. But surely the remedy against this tendency is not to shun probable knowledge altogether, but to distinguish it carefully from certain knowledge, and apportion one's confidence in accordance with the evidence.

We are also in a position now to assess Dancy's (1985, p. 39) analysis of justification, suggested in the wake of Nozick's (1981) theory of knowledge. A belief is justified, according to Dancy, if and only if it would be knowledge were it true. The analysis doesn't seem to capture the ordinary notion of rationality: Gettier's cases

(1963) are not failures of rationality, even if they do not constitute knowledge. But the considerations adduced above suggest also that Dancy hasn't captured a regulative concept of justification: his analysis admits of cases in which one may properly hold on to a belief one believes to be 'unjustified'.

The prominence of (regulative) justification from the first-person perspective explains its significance in our dispute with the sceptic: he challenges our beliefs, and can rest his case when he has shown that they are unjustified, and deprived us of the (normative) licence to hold on to them. By singling out the first-person perspective I do not mean to suggest that epistemology is egocentrically founded. The sceptic challenges *my* beliefs, but I may justify them by reference to experts, public criteria of meaning, rationality, etc. It is the *target* of the sceptic which concerns me here, not the proper *response* to it.

KNOWLEDGE WITHOUT JUSTIFICATION

Having argued for the epistemological importance of justification, I will now turn to consider knowledge. In this section I will show that in denying that a belief is justified, one is impugning its claim to be knowledge. This means that if knowledge is significant, so is justification. The discussion is complicated by the fact that the term 'know' doesn't have a unique meaning, so that we need to consider various legitimate construals.

Austin belittles the cognitive content of knowledge claims. He argues that in claiming to know one is simply expressing one's belief, with an additional *performative* component:

> …saying 'I know' is taking a new plunge. But it is not saying 'I have performed a specially striking feat of cognition, superior, in the same scale as believing and being sure': for there is nothing in that scale superior to being quite sure … When I say 'I know', *I give others my word: I give others my authority for saying* that 'S is p'.
>
> (Austin, 1961, p. 67, original italics)

Austin is wrong in claiming there is nothing superior to believing. For a start, a knowledge attribution, unlike that of a belif, includes a truth claim. It is easy to conflate the two when we focus on self-attributions of knowledge. As Moore points out, the assertion 'p is true

but I don't believe it' is pragmatically paradoxical. So in professing to believe p, I commit myself to p's truth.[1]

The second (admittedly contentious) reason for not assimilating knowledge to belief is that knowledge requires reliability in addition to true belief. The third is that, according to the traditional conception, knowledge requires justification (in addition to truth). By focusing on first-person attributions it is easy to miss the distinctness of the justification claim. There is, perhaps, something odd about the utterance 'I believe that p, but my belief is irrational'; but the initial feeling of oddness must be overcome. Usually, of course, in judging a belief to be unjustified, we are already disowning it, initiating its revocation. But Hume's sceptic judges as unwarranted many of his beliefs which he is (psychologically) incapable of giving up, and finds himself disbelieving the sceptical thesis which is based on arguments that 'admit of no answer and produce no conviction' (1777, p. 155, fn).

If, as Austin thinks, a claim of knowledge includes a performative component, a recommendation to others of the belief in question, then it is, surely, backed by the speaker's affirmation that he has the proper authority for it. It cannot merely be an affirmation of belief. It must include, as an *assertoric* component, a claim of justification.

Austin is wrong, then, in thinking that knowledge claims must be mere claims of belief; that there isn't a more stringent concept. Still, there is a sense of 'know' in which the concept may be applied where the question of justification doesn't arise at all. It is the sense in which the term is applied to animals ('Fido knows where the bone is'), and even to inanimate objects ('The electron knows where the slit is'). And even when applied to more reflective beings, knowledge doesn't always require justification. Often 'I know' is simply used to express conviction: 'I don't think p, I *know* it'. The lover who asks 'Does your husband know you're here?' is only interested in what the husband believes (and in how he will act), and not in whether his belief is justified.

If knowledge doesn't require justification, one might suppose that our knowledge claims aren't threatened by justification-scepticism, but this would be a mistake. So long as a knowledge claim entails a truth claim, it will be undermined (if not refuted) by scepticism about justification. My belief may well be knowledge (truly believed, reliably acquired, or whatever), but I am irrational in thinking it so, and in holding on to it.

KNOWLEDGE: BEYOND TRUTH AND JUSTIFICATION

The failure of the traditional analysis of knowledge (true justified belief) has led epistemologists to impose conditions in addition to truth, replacing or supplanting the requirement of justification: causation by the fact (Alvin Goldman, 1967), acquisition via a reliable method (Armstrong, 1973), truth-tracking (Nozick, 1981).

Craig (1987) discusses knowledge from a third-person perspective, investigating the function of knowledge ascriptions from the point of view of an inquirer seeking reliable informants. The concept of knowledge, he suggests, 'is used to flag approved informants'. To know p is, roughly, to satisfy any condition which is well correlated with telling the truth about p, and more accessible epistemically; more easily detectable than p's truth-value itself. Satisfaction of this condition confers a high probability on the informant's being right.

The 'practical explication' (Craig, 1987) of a concept is attractive. It enables us, if we wish, to assess analyses provided in the traditional vein—a specification of conditions necessary and sufficient for the application of the concept. For, although the approach is novel in its aim (understanding the purpose of a concept), it provides—as a byproduct—an alternative method for assessing truth-conditional analyses. We now ask whether a concept with such and such truth-conditions fulfils the role the analysed concept is deemed to have, rather than whether the truth-conditions match our intuitive judgements.

Thus, conditions cited in reliabilist analyses of knowledge (truth-tracking, causation by the fact believed, acquisition via a reliable method) virtually always go together with the reliability (from the inquirer's point of view) of a belief, and this explains, Craig argues, the successes of the analyses. Their failures are explained by the defeasibility of probability statements. Any condition which confers a high probability on the informant being right may be overridden. We can always find examples, albeit freakish, which aren't cases of knowledge, even though the proposed condition is satisfied.

Such considerations also explain the failure of the traditional analysis of knowledge (Gettier, 1963). If knowers are reliable informants, then divergence between justification and knowledge is to be expected in the case of other believers. We can see why, from the

perspective of an inquirer, a true belief which isn't justified may count as knowledge. If Jones believes that his belief is unwarranted, *he* ought to relinquish it. But *I* may have reasons which are unavailable to him (information about the method by which the belief was acquired, for instance), which lead *me* to adopt the belief myself, rendering him a reliable informant, and his unjustified (true) belief— a case of knowledge. For instance, *he* thinks the clock is unreliable, so his belief that it is now noon is unjustified. But I know that the clock has been fixed, so that he is (unbeknownst to him) a reliable informant, and does know the time.

We can see that justification may not be sufficient for knowledge either. Consider an agent whose (wontedly reliable) watch has stopped, showing the time of 12:00. Suppose it is noon, and the agent, who doesn't know his watch has stopped, looks at it, and forms the belief that it is noon. The belief is justified but isn't knowledge. Justification is an indication of reliability. But in our example, we have overriding information (unavailable to the hapless agent) in the light of which we judge that he is an unreliable informant, doesn't have knowledge.

Suppose (plausibly) Craig and reliability theorists are right in claiming that a true and justified belief may fail to be knowledge. What about first-person divergence? Can I characterize as a failure of knowledge my own justified belief? From Craig's perspective, the answer must be negative. A person who thinks his own belief is fully warranted cannot judge himself an unreliable informant with respect to it. If I cannot recommend the belief to others, I cannot embrace it myself. If Craig is right about the function of the concept of knowledge (labelling reliable informants), then the very divergence a theory (of knowledge) allows between first-person ascriptions of knowledge and justification will count against it.

Have we been too hasty in dismissing the possibility of a divergence between first-person knowledge and justification? The locution 'I justifiably believe p, but I do not know that p' does seem absurd. But intuitions waver, more so under pressure of examples (see the following section), and this is not surprising. There is, plausibly, no univocal sense of 'know' to be captured by a philosophical analysis. Still, we should at least, shouldn't we, demand that the sceptic's use conform to *some* existing notion of knowledge? If the sceptic is distorting our ordinary concepts, imposing inordinately stringent standards on knowledge (Austin, 1961), cannot he be ignored? Not really.

We may, in fact, bypass altogether the exasperating question as to the 'true' meaning(s) of the term 'know', and focus, instead, on the more important issue, as to *why* (and *which*) knowledge attributions matter. That the sceptic's concept is 'technical' doesn't by itself render his thesis inconsequential. If, for instance, we use the term 'knows' synonymously with 'confidently believes', then, of course, the demon argument couldn't be invoked to show that I do not know that I am sitting at my desk: the concept doesn't satisfy (at least) one of the argument's premises (I do not know that I am not dreaming). But if the sceptic can introduce a more stringent concept, 'know+', which does allow the derivation to go through, he won't automatically be confounded when told that the concept isn't ours. The question would have to be confronted as to the significance of knowledge+ attributions, and the implications of their scarcity. The sceptic could be telling us something about a more interesting concept.

Of course, such considerations could also militate against the sceptic: knowledge scepticism, even if it involves no warping of ordinary concepts, could turn out to be far less disturbing than the sceptic would like to think. Taking the sting out of scepticism is as effective as refuting it.

DOES KNOWLEDGE-SCEPTICISM MATTER?

The sceptic's claim that we have no warrant for many, indeed all, of our beliefs is one we should attempt to rebut. It impinges most directly on our conduct as rational inquirers. What about his denials of our knowledge claims? If we fail to know because we lack justification, that is worrying. But, I shall now argue, if a person can brand his own justified belief as a failure of knowledge he shouldn't be perturbed. To defend this claim I shall examine various ways of construing knowledge attributions, all of which allow such first-person divergence between knowledge and justification.

Consider Wittgenstein's statement:

Moore does not know what he asserts he knows, but it stands fast for him, as for me; regarding it as absolutely solid is part of our method of inquiry. When Moore says he *knows* such and such, he is really enumerating a lot of empirical propositions which we affirm without special logical testing; propositions, that is, which

have a peculiar logical role in the system of our empirical propositions.

<div align="right">(Wittgenstein, 1969, s. 136, original italics)</div>

According to Wittgenstein, propositions such as 'Here is one hand' are the framework ('hinges') for any inquiry; 'the element in which arguments have their life' (1969, s. 105). He is willing (arguably idiosyncratically) to apply the term 'know' only to beliefs for which one can cite grounds. The framework propositions cannot, therefore, be known, but they are not arbitrary, and enable others to be known. Clearly, Wittgenstein's denial of Moore's claim to know is not sceptical. Indeed, it is part of his attempt to vindicate our ordinary and scientific beliefs. His denial of knowledge poses no epistemological threat and we need not attempt to rebut it.

Another example of an innocuous form of knowledge-scepticism is Nozick's (1981). In his response to the knowledge-sceptic, Nozick concedes that he doesn't know he isn't being deluded by an evil demon. His belief that he isn't doesn't *track the truth:* if he *were* deluded, he would still think he wasn't. The concession is made palatable, Nozick points out, by the fact that many other beliefs ('There is a table in front of me') do constitute knowledge: if there wasn't a table in front of him (because, for instance, he was sitting in a different room), he wouldn't believe there was one. The sceptic is defeated on points, even if he isn't knocked out.

Has Nozick made a real concession to the sceptic? The fact that my belief in the negation of the demon hypothesis is not knowledge ought not to motivate me to revoke it. He is justified, Nozick himself claims, in the belief that he is not deluded by an evil demon: it is a belief got via the most reliable method available to him (1981, p. 265): deduction from other things he knows ('There is a table in front of me'). Not only is the belief justified, Nozick argues, he would stake his life on it (1981, p. 220). So what if it isn't knowledge? How is it inferior to a belief which is?

Consider, next, statistically based beliefs, which do not come out—on Nozick's analysis—as knowledge. Were I going to win the lottery, I wouldn't believe it, because the nearest worlds in which that happens are, plausibly, those in which I have the same (probabilistic) evidence about the lottery. My belief, 'I won't win the lottery', is, therefore, not knowledge, although it is justified.

Some may refuse to call this a case of belief. High as the number of tickets in the lottery might be, I will assign a probability less than 1 to the proposition 'I won't win the lottery', so it won't be a case of full belief. Now, this will be a departure from ordinary usage: partial belief shades into ordinary belief before becoming certain, and a sufficiently high degree of belief is belief *simpliciter*. But, anyway, whether or not this is a case of full belief, or just a high degree of belief, it is not (Nozickian) knowledge.

Or is it? Might not a statistically based belief track the truth? The world in which I win the (deterministic) lottery must differ from the actual one even before the lottery takes place. In such a world, the evidence I have regarding the lottery might be different, so the counterfactual 'Had I been going to win the lottery, I would have believed it' might be true. This way of resisting the sceptical conclusion with respect to statistically based beliefs is feeble even if determinism is assumed to be true. For surely even in a deterministic world I would have the same (probabilistic) evidence about the draw. But, anyway, this response is not available in non-deterministic contexts. One of the possible worlds in which I win in a non-deterministic lottery is physically identical with the actual world up to the time of the draw, and is, hence, unequivocally *the* unique nearest possible world in which I ought to believe in my winning if my belief in the actual world is to count as knowledge. Yet, the same objective chance will lead me in that world to believe that I won't win.

Belief, full or partial, in non-deterministic contexts, based on the best available evidence, won't be knowledge, because the evidence is ineliminably statistical. If quantum indeterminism pervades all macroscopic phenomena, there may not be *any* empirical knowledge pertaining to the future.

Has Nozick perverted the ordinary concept(s) of knowledge? Our intuitions aren't sufficiently clear cut. And, anyway, this is not the crucial question. Let us grant him the truth-tracking notion of knowledge. The important thing to note is that from our point of view, the distinction it marks may, on occasion, be one without a difference. For there is no reason for supposing that beliefs which are based on objective probabilities are less likely to be true than those which are truth-tracking. Not knowing the truth-value of a proposition, I do not necessarily prefer a truth-tracking informant to one who knows its (possibly very high) objective probability.

Nozick's knowledge, we can see, is not *regulative,* and its scarcity isn't worrying.

There are other concepts of knowledge which, like Nozick's, engender a distinction (among beliefs) which is sometimes trifling. If, as Plato (*Republic* 476–9) and Descartes would have it, knowledge must be infallible, there will be very little knowledge: we no longer think we have many (indeed any) infallible beliefs. Similarly, knowledge may require certainty of a strong kind, which we seldom, if ever, attain. If (Unger, 1971) 'hardly anyone …is certain that 45 and 56 are 101', then '…every human being knows, at best, hardly anything to be so'.

The natural response to Unger is to attempt to show that he has misrepresented the ordinary concept of knowledge. But this is not necessary. We can easily admit that we do not have knowledge, so long as 'there is much that many of us correctly and reasonably believe' (Unger, 1971). If such is our concept of knowledge, then knowledge-scepticism is no longer an 'unpopular thesis [which] simply must be false', but a true one, which we may embrace equanimously.

This ought to be our attitude to Descartes' scepticism. He concedes that his old opinions, which he subjects to a very exacting scrutiny, are *'highly probable'* and only 'in a sense doubtful' (CSM II, p. 15, my italics). What they lack is certainty, but this does not detract from their warrant.

Of course, it would be nice to know that a belief is logically incapable of being mistaken: that is the strongest kind of guarantee. But if we think that justification confers a reasonably high (although somewhat less watertight) assurance, we won't be too disconcerted when deprived of knowledge, so long as our justification isn't questioned.

Similarly, knowledge may be a scarce commodity if it requires warranted subjective certainty (Ayer, 1956, p. 35), because even if certainty is not always irrational, it very often is. Still, this is not worrying so long as a belief may be justified without being subjectively certain.

CONCLUSION

I have been discussing knowledge very eclectically, opting for no particular account. This omission is quite proper. In addressing the

sceptic, I have argued, it is justification on which we ought to focus. Knowledge—however construed—need not be considered separately. This (fortunately) means that we can bypass the vexing question as to its proper analysis.

This does not mean that the word 'knowledge' will not appear in the following pages. Much of the literature is (misguidedly, to my mind) concerned with knowledge-scepticism. I need to refer to it in illustrating various methodological points. Also, very often (if not invariably), arguments for knowledge-scepticism are equally cogent against the possibility of justification.

2

THE SCEPTICAL LIFE

RADICAL SCEPTICISM

Descartes makes 'a very careful distinction between the conduct of life and the contemplation of the truth' (CSM II, p. 106), and can act ordinarily even in the face of sceptical arguments, because his old opinions are *'highly probable'* and only 'in a sense doubtful' (CSM II, p. 15, my italics). What they lack is certainty, and this does not impugn their capacity to guide rational action.

According to the radical sceptic we have no (epistemic) reason to believe anything. Worse still, not only can't we rationally believe (fully), we cannot even judge propositions as more or less probable. This understanding of the sceptical doctrine differs from that given by the ancient Greek Pyrrhonist sceptics. Sextus Empiricus says that 'sense-impressions…are equal in respect of probability and improbability, so far as their essence is concerned' (PH, p. 139). In a similar vein, Stove (1973, p. 61) ascribes to Hume's sceptic the following probabilistic principle: unless a proposition (hypothesis), h, or its negation, is logically implied by a proposition, e, then $P(h/e)=P(h)$. That is, adding evidence, e, to one's premises has no rational bearing on h, and ought to leave unchanged its probability. But, Stove notes, this principle is inconsistent with the probability calculus.[1] Stove concludes that scepticism is incoherent. Would that scepticism were so easily refuted! In fact, both Sextus and Stove mischaracterize the sceptical doctrine.

Since the sceptic thinks that we can never have evidence for the truth of any proposition, he claims that we are not even warranted in assigning probabilities to propositions, in partially believing them. *Pace* Stove, the sceptic doesn't violate the probability

calculus. He doesn't think that rational degrees of belief are left unchanged by evidence. Neither (*contra* Sextus) does he think that all propositions are equally probable. Rather, he claims that there can be no rational degrees of confidence, probabilistic or otherwise. The probability calculus cannot be applied to constrain degrees of belief.

Thus characterized, scepticism is radical in three ways. First, it is comprehensive, and directed at all our beliefs. Second, it denies justification, not just knowledge. Denials of justification, I have argued (Chapter 1), pose a more serious threat. Given this reading, Sextus isn't prepared to count Arcesilaus, Carneades, and their followers in Plato's Academy as true sceptics:

> The adherents of the New Academy…differ from the sceptics…[in] describ[ing] a thing as good or evil… with the conviction that it is more probable that what they call good is really good rather than the opposite…whereas when we describe a thing as good or evil we do not add it as our opinion that what we assert is probable… And respecting the probable impressions they make distinctions: some they regard as just simply probable, others as probable and tested, others as probable, tested, and 'irreversible' [indubitable].
>
> (PH, p. 139)

The third and final feature of radical scepticism can be explained as follows. To deny that a belief is justified isn't necessarily sceptical. I may be told, for instance, that my belief in astrology isn't justified, no correlations having been established between the movements of the stars and the fortunes of men. Since I am told that the evidence in fact warrants a different belief, the claim that my current one is unjustified is not genuinely sceptical. And this remains so even if a large set of beliefs is thus criticized. What will make the criticism sceptical is the claim that with respect to the matter at hand, justification cannot be had, that one cannot form a rational opinion about the relevant proposition(s). Thus, when criticizing our beliefs about the future, Hume's inductive sceptic isn't suggesting we should believe that the sun *won't* rise tomorrow, or that we should be more hesitant and assign its rising a middling probability. Rather, he claims that *no* belief, not even a probabilistic one, about tomorrow's sunrise can be justified. Although in common parlance one might use the expression 'I am

sceptical about astrology' to express disbelief, this is not how the term 'scepticism' should be understood in epistemology.

Having characterized radical scepticism, we can now consider its practical consequences, and the significance these might have for the sceptical doctrine. To begin with, radical scepticism seems to require that we suspend judgement (eschewing even a probabilistic one), since it involves the claim that we are incapable of believing rationally. But it is not clear that we can acquiesce in this demand. Can the sceptic conduct his life without believing (see the following section)? And is it psychologically possible to suspend belief (see pp. 22–3)? Is the sceptical frame of mind more tranquil than the 'dogmatic' one? Despite the fact that the answer to at least some of these questions is negative, we are not absolved from the need to contend with the sceptical arguments (see pp. 23–30).

THE AGNOSTIC LIFE

We, non-sceptics, are guided in our actions by our beliefs. We leave a room in a high building through the door rather than through the window, because we believe the latter (and not the former) would lead to our death. How is the sceptic to live without holding any opinions? Hume thinks that the life of the agnostic, who has totally suspended belief, is one of inertia. Were the sceptic's doctrine accepted, '[a]ll discourse, all action would immediately cease; and men remain in total lethargy' (1777, p. 160). Here, Hume is at odds with Sextus Empiricus, who doesn't think the agnostic life is quiescent. 'Adhering…to appearances', he says, 'we live in accordance with the normal rules of life' (PH, p. 17).

Is Sextus really attempting to characterize the agnostic life? Hookway (1990, p. 14) thinks not. He suggests that Sextus doesn't really think he can suspend belief. He 'allows his life to be guided by the propositions that are naturally impressed upon him, while taking no responsibility for their truth or rationality'. To 'rely on appearances', Hookway suggests, isn't to rely on 'distinctive *objects* of knowledge', but is, rather, 'a distinctive sceptical manner of assent to propositions about ordinary things' (Hookway, 1990, p. 63, original italics). The sceptic is 'alienated' from judgements he finds himself compelled to make and act upon yet cannot rationalize.

This interpretation of Sextus isn't plausible. He equates (PH, p. 15) 'appearances' with 'phenomena' ('impressions'), and contrasts the sweet appearance of the honey with its being sweet 'in its essence'. He is sceptical about judgements regarding essences, and not about those regarding appearances. When 'we question whether the underlying object is such as it appears, we grant the fact that it appears' (PH, p. 15). But, anyway, whatever Sextus' intentions, we are looking for a characterization of the agnostic life. Can one be extracted from what he says?

There are, in fact, two strategies offered here for living agnostically, although Sextus seems to think they coincide: acting in accordance with appearances (sense-perceptions), and following prevailing norms. I shall argue that they are both problematic. The first strategy isn't even well defined, and the second is incomplete.

It is unclear how appearances can ground action. What is it about appearances that justifies my eating bread? I may feel hungry, but actions must be grounded in beliefs about future and hypothetical appearances: 'When I have eaten, I *will* no longer feel hungry'; 'Were I not to eat, my hunger *would* persist'. These are not beliefs about how things appear to me (now).

The second sceptical suggestion, viz. that one acts according to prevailing norms, 'laws and customs' (PH, p. 17), is a slight improvement, but it is incomplete. Convention doesn't dictate how one is to act in every situation. Perhaps it is customary to eat breakfast in the morning, but convention is silent about one's choice of a career, spouse, and holiday venue.

There *is* a complete agnostic strategy: acting randomly. But, like the two Pyrrhonist strategies mentioned above, it compromises the sceptic's agnosticism. If we toss a coin so as to decide between two courses of action, we must believe a proposition reporting the result of the toss. To follow custom, the sceptic must be able to identify particular actions as instances of conventions. This he can only do if he has beliefs about prevailing conventions.

Acting in accordance with appearances involves the belief that something seems to be the case: a truth claim, albeit a modest one. Admittedly, the ancient sceptics contrasted appearances with reality, 'identifying "true" with "true of a real *objective* world"' (Burnyeat, 1978, p. 121, my italics). But statements about appearances cannot be deprived of a propositional content, and thereby legitimized, by a

terminological fiat. The need to rely on appearances compromises the agnosticism of the radical sceptic, who thinks no belief is justified.

For the most part, Reid says approvingly, Pyrrho's 'life corresponded to his doctrine…[I]f a cart run against him, or if he came upon a precipice, he would not stir a foot to avoid the danger' (1764, p. 102). But, Reid adds mockingly,

> the great Pyrrho himself forgot his principles on some occasions; and is said once to have been in such a passion with his cook, who probably had not roasted his dinner to his mind, that with the spit in his hand, and the meat upon it, he pursued him even into the market place.
>
> (Reid, 1764, p. 102)

The mockery is misplaced. Pyrrho thinks the sceptic lives 'in accordance with the *normal rules of life*' (PH, p. 17, my italics). So he cannot be indifferent to precipices. The stories upon which Reid bases his objection aren't consonant with Pyrrho's well-articulated view of the sceptical life. Now, this view is untenable. Living according to custom, we have seen, isn't really agnostic. So instead of faulting Pyrrho for (occasionally) lapsing from the true agnostic life, Reid's objection ought to be that Pyrrho (consistently) adheres to a mistaken view about the agnostic life.

So what is the agnostic life? We seem to be left with Hume's suggestion that it is a life of inactivity. There is, perhaps, another alternative: acting according 'to our own instinctive feelings' (PH, p. 18), i.e., not really acting (intentionally) at all, but, rather, allowing our bodies to move as they will.

IS IT POSSIBLE TO SUSPEND BELIEF?

So the sceptic *can* live agnostically. But he will find it more difficult to contend with the second challenge his stance seems to pose. Here, again, Hume seems very convincing:

> [The] Pyrrhonian cannot expect that his philosophy will have any constant influence on the mind…all his objections …can have no other tendency than to show the whimsical condition of mankind, who must act and reason and believe; though they are not able…to remove the objections, which may be raised against them.
>
> (Hume, 1777, p. 160)

Perhaps sceptical arguments can wean us from unfounded (and unfoundable) theoretical speculations, and when the sceptic thus 'keep[s] within his proper sphere' (1777, p. 159) his argument will not only 'have ample of triumph' (in persuading its audience); it will also have beneficial consequences. But for all their cogency, the sceptical arguments cannot instill the slightest doubt about our ordinary, action-guiding beliefs.

Is the sceptic in trouble because he cannot suspend belief? Only if suspension of belief is the proper attitude in the face of evidential bankruptcy. But this supposition may be questioned. Even in purely theoretical contexts, where agnosticism does not lead to 'lethargy', and the sceptic can easily refrain from taking a stand, *must* he abstain? Further argument is, surely, required to support William James' contention (1956, p. 20) that the 'attitude of sceptical balance is…the absolutely wise one' when it comes to purely theoretical beliefs, which are not 'forced on us' by practical and moral exigencies. For Sextus, scepticism leads, as a matter of psychological fact, to 'a state of mental suspense and next to a state of "unperturbedness" or quietude' (PH, p. 7), which is independently desirable. 'The originating cause of Scepticism is …the hope of attaining quietude' (PH, p. 9). But *we* need an argument to show that agnosticism is the *epistemically* proper attitude for the sceptic.

It might be argued that the sceptic—at least of the radical stripe—isn't in a position to adduce such arguments. After all, he denies our ability to come to a reasoned judgement about anything. This is true, but works here in his favour. We are considering whether the unimplementability of scepticism gives us a licence to ignore the sceptical arguments. And if the sceptic cannot invoke arguments in favour of agnosticism, the practical difficulties it engenders are not his. Still, *we* need to consider them.

THE LOGICAL DEFENCE OF AGNOSTICISM

The logical defence of agnosticism invokes the principle of sufficient reason: if we have no more reason to believe a proposition than we do for believing its negation, we should refrain from believing either. The Pyrrhonist defence of *epoché* (suspension of judgement) thus rests on the existence of arguments of equal force for contradictory propositions. This logical defence of agnosticism fails.

Construed as a normative principle of action, the principle of sufficient reason is untenable. It isn't true that one should act (choose) only if one has a sufficient reason. A perfect balance of forces acting on a massive body will, indeed, result in inertia, but that does not mean that a balance of reasons *ought* (normatively) to lead to inaction. The inadequacy of the normative principle is attested by the (unnecessary) plight of Buridan's ass, who starved to death when confronted with two identical bundles of hay. It is plainly silly to refuse a very good (indeed perfect) option just because it is not superior to all the others.

The principle of sufficient reason fails as a principle governing rational choice. Let us, however, allow the sceptic to invoke the principle, and apply it to belief-formation. For this use of the principle we do not need to suppose that belief-formation is voluntary. When applied to (possibly involuntary) belief, the principle simply classifies as irrational a belief for which there is no sufficient reason. I shall now argue that the principle of sufficient reason cannot be used by the sceptic to uphold agnosticism.

When applied to a given set of alternatives, the principle alleges that only a choice of an alternative which is superior to all the others can be justified. It forbids a choice between options tied for first place, rather than positively recommending that no choice be made in the face of a tie. Buridan's ass must not choose between two identical bundles of hay; he is not *advised* to starve. No alternative can be justified by default, merely by being excluded from a set of alternatives, between whose members we are indifferent, and this applies equally when the excluded alternative is inaction. The (ir)rationality of starving must be assessed by comparison with the other alternatives. It is, of course, worse than either. The ass's predicament—according to the principle of sufficient reason—is that he has no rational choice.

Like starving, agnosticism must be viewed as just one (cognitive) alternative, in need of justification like any other. If no choice stands out above the rest, the most we can uncontentiously say is that they are all equally (ir)rational. We can compare the adoption of a belief to the purchase of a lottery ticket. The option of not buying a ticket may or may not be, in the absence of moral grounds forbidding gambling, the most appropriate. Whether or not it is will depend on the prize, odds, ticket price and one's attitude towards risk. For instance, it seems eminently reasonable to pay £1 for a ticket in a 100-ticket lottery whose prize is £1,000.

The same holds for cognitive 'gambles'. As Popper (1972) rightly suggests, elaborating on William James' (1956) insight, the goal of a belief system is verisimilitude, an optimal balance between truth- and falsity-content. The ideal belief system contains precisely all true propositions (see pp. 1–2). There are two kinds of doxastic errors, both of which one aims to avoid in formulating belief: failure to believe a true proposition (not possessing a winning ticket), and believing a false one (losing the price of a non-winning ticket).

The non-sceptic has a strategy he thinks conducive to the simultaneous minimization of both kinds of error: formulating his beliefs in accordance with the available evidence. The radical sceptic denies the reliability of so-called evidence, and the efficacy of our strategy. He can avoid error of the first kind (missing out on the truth) by believing all propositions; of the second (falling prey to falsity) —by remaining agnostic about all propositions. The strategy that precludes one error also guarantees incurring the other. Descartes has a much greater abhorrence of the second kind of error. '[T]he mere fact that [he] found…all [his] previous beliefs were *in some sense* open to doubt was enough to turn [his] absolutely confident belief in their truth into the supposition that they were wholly false' (CSM II, p. 41, my italics). Someone who has formed a fallible belief 'is less *wise*' than 'one who has never given [the matter] a thought' (CSM I, p. 10, my italics), i.e., the agnostic. But in fact the two errors are on a par with respect to the cognitive goal of maximizing verisimilitude.

Neither can the sceptic justify agnosticism over intermediate doxastic attitudes: believing *some* propositions. After all, one of these is the system of all truths (God's system), although, of course, the sceptic thinks we cannot tell which. By choosing agnosticism, we forfeit the chance of attaining it. We must conclude that agnosticism is not a privileged position, from which the sceptic can criticize others for their epistemological irresponsibility.

Similar considerations will show that scepticism doesn't prescribe that we should remain agnostic about probability assignments. Admittedly, by refraining from believing even partially one avoids the risk of (partially or wholly) believing false propositions; but, equally, one misses out on the chance of (partially or wholly) believing true ones. In a state of complete ignorance there is no epistemic reason for preferring a cautious (agnostic) strategy over a bold one.

Why are we tempted to think that agnosticism is the proper attitude for a sceptic? Our error can be diagnosed as follows. It is, of course, perverse to believe a proposition when one has equally good evidence for its negation. Agnosticism is the proper attitude in the face of perfect evidential balance. But that is because we, non-sceptics, have a (sufficient) reason for remaining uncommitted: the evidence warrants an equal distribution of confidence between the proposition and its negation.

The sceptical situation, on the other hand, is not one of perfect evidential balance. Admittedly, the sceptic attempts to marshal evidence for and against each proposition. But in doing so he aims to show that we can never have any evidence for any proposition. He aims, in fact, to discredit the very idea of evidence. But with its demise disappears the rationale (evidential balance) for agnosticism.

As James (1956, p. 23) points out, agnosticism is just another 'passionate' (non-intellectual) choice. Being very tolerant, he admits as rational any choice (including agnosticism), and gives us a licence to adopt on faith beliefs that cannot be grounded in reason; 'to believe at our own risk any hypothesis that is live enough to tempt our will' (James, 1956, p. 29). If we are more stringent than James, we will brand them all (including agnosticism) as irrational. We certainly cannot enlist the principle of sufficient reason to justify the claim that the proper attitude upon failing to justify any belief is to shun belief altogether. Here, as elsewhere, not having a reason for choosing is not a reason for not choosing; the latter may be the worst alternative, and the sceptic certainly cannot show that it is cognitively superior.

THE ETHICAL DEFENCE OF AGNOSTICISM

Descartes, following the Stoics, thinks error is sinful. We tend to think of belief less in ethical terms, judging as imprudent or irrational, rather than immoral, someone whose beliefs are unjustified. This is because we think belief to be, by and large, involuntary, and an ethical judgement of an action (belief) to presuppose freedom to do otherwise. Now, one can occasionally manipulate oneself into acquiring a belief for non-cognitive reasons (i.e., not because of its perceived truth). Thus, Pascal invokes his famous wager to persuade us that we ought to believe in the existence of God. There is a positive

probability that God exists, and one who believes in his existence receives an infinitely large reward (eternal bliss after death) if his belief is true, risking only a finite loss (living the life of a believer) should he believe falsely. The principle of expected utility maximization (which we can plausibly read into Pascal's reasoning) requires that one should believe in God. But, as Pascal realizes, one must resort to non-rational methods, such as going through the motions (acting as though one believed), or hypnosis, to bring about this conversion. Furthermore, beliefs acquired by such methods cannot easily be sustained (B.Williams, 1970).

Can a sceptic who thinks belief is voluntary defend agnosticism on moral grounds? Of course, his scepticism mustn't extend to morality: it might, for instance, be restricted to beliefs about the external world. But let us grant this. If, as Descartes thinks, one can choose to believe, if 'the will simply consists…in the fact that when the intellect puts something forward for affirmation or denial…we do not feel we are determined by any external force' (CSM II, p. 40), then the will *can* be blamed for our beliefs. When (if at all) *should* it?

According to Descartes, 'the perception of the intellect should always precede the determination of the will. In this incorrect use of free will may be found the privation which constitutes the essence of error' (CSM II, p. 41). '[I]t is…an imperfection in me to misuse that freedom and make judgements about matters which I do not fully understand' (CSM II, p. 42). Here, Descartes isn't reasoning like a sceptic. The correct use of the will is following the dictates of reason. And, of course, when reason tells us to suspend judgement (because the evidence is evenly balanced), we should. But if we are—as the sceptic thinks—irredeemably deprived of reason's guidance, we are equally culpable if we choose to believe groundlessly or to abstain: neither is recommended by (silent) reason. If moral blame can be levelled at the sceptic at all, because his stance lacks reason's approval, he is to blame *whatever* he does. Agnosticism is tainted alongside belief.

THE IMPLEMENTATION OF THE SCEPTICAL DOCTRINE

We can now see that one of the reasons adduced for thinking that scepticism engenders practical difficulties is specious. Our inability

to suspend belief constitutes a practical difficulty for the sceptic only given the assumption that the proper doxastic attitude for the radical sceptic is agnosticism. But, I have argued (see pp. 23–7), this assumption is untenable. The sceptical doctrine gives rise to no doxastic prescription: it renders *every* doxastic stance—including the suspension of belief—irrational. So the practical predicament of the sceptic isn't that he (psychologically) cannot implement his doctrine. Rather, there is nothing that would count as implementing the sceptical doctrine.

Similar considerations vitiate another practical objection to scepticism. The sceptic must acknowledge, Hume claims, 'if he will acknowledge anything, that all human life must perish, were his principles universally and steadily to prevail' (1777, p. 160). This objection fails as an *ad hominem* argument. For the sceptic has no reason for supposing that inertia is less conducive to human survival, or to the satisfaction of other human aims, than a life of frenzied activity. The sceptic denies the possibility of justifying any beliefs, including those pertaining to the causal effects of acting thus and so (or remaining inactive). Of course, neither can the sceptic properly view his agnostic stance as a means to attaining peace of mind. He cannot defend the Pyrrhonist claim that by suspending belief he will attain peace of mind *(ataraxia)* which he initially hoped to achieve by finding truth.

Hume doesn't just fail to show the sceptic that scepticism has detrimental consequences. He cannot even persuade *us* that this is so. For in addition to the plausible assumption that the agnostic life is inert (and, therefore, disastrously brief), Hume's reasoning also crucially depends on the false supposition that scepticism ought to lead to agnosticism.

Scepticism *does* have detrimental consequences. Sextus' claim that scepticism leads (via suspension of belief) to a calm state of mind is unpersuasive. Indeed, we believe Hume when he describes the 'wretched condition' to which his sceptical reflections have led him. The 'impossibility of amending or correcting these faculties, reduces [him] almost to despair, and makes [him] resolve to perish' (1739, p. 264). We also find plausible Peirce's claim that doubt 'is an uneasy and dissatisfied state from which we struggle to free ourselves and pass into the state of belief; while the latter is a calm and satisfactory state which we do not wish to avoid' (1965, 5.372).

THE SCEPTICAL DOCTRINE AND THE PRACTICAL DIFFICULTIES

We now have a better understanding of the practical difficulties the sceptical doctrine engenders. To begin with, it cannot (logically) be implemented: it rationalizes no doxastic stance. Secondly, belief in it has unpleasant psychological effects. The final difficulty, which hasn't yet been mentioned, is that the sceptical doctrine cannot lastingly and steadily be believed, and anyone who preaches it is being 'frivolous and insincere' (Russell, 1948, p. 9).

Great significance has been attached to the incredibility of scepticism. According to Russell (1948, p. 9), it justifies us in taking 'for granted that scientific knowledge, in its broad outlines, is to be accepted'. Reid thinks the sceptical 'philosophy is justly ridiculous even to those who cannot detect the fallacy of it. It can have no other tendency then to shew the acuteness of the sophist' (1764, p. 102). And Hume supposes that 'the most confounding objection to *excessive* scepticism' is its inability 'to produce a conviction' (1777, pp. 159–60, original italics). The sceptic is 'immediately at a loss, and knows not what to answer' when we ask him *'What his meaning is? And what he proposes by all these curious researches?'*, since he 'cannot expect that his philosophy will have any constant influence on the mind'.

The incredibility of scepticism cannot be invoked in this manner. To be sure, the sceptic wishes to persuade us that his doctrine is true. And one explanation for his inability to do so is that his doctrine is unwarranted, his arguments unsound. But we cannot simply reject his doctrine on the basis of the fact that we find ourselves incapable of believing it. We must contend with his arguments. We must show the reasoning to be invalid, or the premises unwarranted. If the arguments 'admit of no answer and produce no conviction' (Hume, 1777, p. 155 fn), the sceptic will be seen to have stumbled upon an incredible truth: 'belief is more properly an act of the sensitive, than of the cogitative part of our natures' (Hume, 1739, p. 183). We cannot reconcile the reflective, philosophical outlook with the practical one. Our beliefs and the actions they guide are not rational. This is certainly an uncomfortable position to be in—a kind of a 'cognitive dissonance' (Hookway, 1990, p. 55). But to suppose, on that count, that the sceptical thesis is false is to engage in wishful thinking.

Our conclusion must be the following. If scepticism is 'logically impeccable' (Russell, 1948, p. 9), the miserable state of mind it induces and the insincerity of its proponents provide no epistemological shortcut. They cannot absolve us from the need to contend with the sceptical arguments.

3

THE CHALLENGE

TWO TYPES OF SCEPTICISM

Two questions should be confronted if we are to assess the sceptical position. Who has the burden of proof, and what form is a 'proof' to take? If the burden is ours, and the task is characterized sufficiently onerously, we will not be able to carry it out. Hume's pessimism about the possibility of defeating scepticism which 'recommends an universal doubt…of our very faculties; of whose veracity…we must assure ourselves' (1777, pp. 149–50) seems well-founded. Any response to such scepticism would involve 'the use of those very faculties, of which we are supposed to be already diffident', and would, therefore, be ineffectual. This kind of scepticism is 'antecedent to all study and philosophy' (Hume, 1777, p. 149), and we can term it *antecedent* scepticism. Its essential characteristic, as its name suggests, is that it is a propaedeutic to any investigation, rather than its outcome. And, furthermore, it must be rebutted prior to and independently of any knowledge. Once we acquiesce in its demand, 'no reasoning could ever bring us to a state of assurance and conviction upon any subject' (Hume, 1777, p. 150).

We shouldn't concede defeat just yet. The (antecedent) sceptical challenge posed above isn't one we should rise to. The demand to justify reason without appealing to anything is absurd. Similarly, should the sceptic take the burden of proof upon himself, he cannot be expected to discredit reason from scratch. Reason isn't to be suspended until the dispute is settled. It must, rather, be employed in conducting it, at least if the aim is rational persuasion rather than conversion. And, indeed, notable sceptics have all *reasoned* in their attempt to undermine reason. They have made 'a very extravagant

attempt to destroy reason by argument and ratiocination… They endeavour to find objections, both to our abstract reasonings, and to those which regard matter (sic) of fact and existence' (Hume, 1777, pp. 155–6, italics omitted). Their brand of scepticism is *'consequent* to science and inquiry… [and invokes] profound *arguments* against the senses, which admit not of easy a solution' (Hume, 1777, pp. 150–1, my italics).

Consequent scepticism is based on reasons. These may be both empirical and a priori. To the first kind belong Sextus' ten tropes or modes of scepticism, which juxtapose opposing appearances to undermine judgements about reality. A tower looks square from a distance and round from nearby. This is an empirical fact, which the sceptic adduces to undermine the reliability of the senses.

The sceptic may invoke a priori considerations to undermine the possibility of knowledge. For instance, he may claim—on the basis of a conceptual analysis—that knowledge requires certainty, and that certainty is never warranted. This argument, note, illustrates that consequent scepticism may be sweeping, impugning *all* our beliefs. So the distinction between the two types of scepticism—antecedent and consequent—is not one of scope: antecedent scepticism is, of course, comprehensive as well.

The consequent sceptic accepts reason's authority. How can he then attempt to undermine reason? Well, in fact, he aims to show that reason gives us no guidance in forming beliefs; that there is no such thing as rational belief. He brings to our attention our 'unfitness to reach any fixed determination in all those curious subjects of speculation, about which [we] are commonly employed' (Hume, 1777, p. 150). Of course, in order to do this he has to appeal to our standards of rationality: these are the only standards for establishing *anything*. By appealing to them, he aims to show that many (if not all) of our beliefs are unjustified.

I have so far discussed the *kind* of proof involved in the debate with the sceptic. What about the question of burden? Perhaps there is an initial presumption against scepticism: it is, after all, extremely implausible. And anyway, proving one's case is more decisive than shifting the burden on to one's opponent, even if he should fail to come up with a convincing proof of his own. For the sceptic, then, burden-shifting can at best result in a stalemate. So he will certainly do better to take the burden of proof upon himself.

That this is the proper way for the sceptic to present his challenge can be illustrated by some historical examples. Sextus doesn't merely challenge reason's credentials. He uses various arguments to show that reason and the senses cannot be vindicated. The first Pyrrhonist strategy 'opposes appearances to judgements…with the result that, owing to the equipollence of the objects and reasons thus opposed, we are brought…to a state of mental suspense' (PH, p. 7). The ten tropes or modes of scepticism juxtapose opposing appearances to undermine judgements about reality. If 'we bend down over a book after having gazed long and fixedly at the sun, the letters seem to us to be golden in colour and circling round' (PH, p. 29). Whereas ordinarily, of course, the letters appear black and square.

The appearances of an object can differ within the same subject depending on his situation (distance from the object, for instance) or condition: 'waking or sleeping…hatred or love, emptiness or fullness, drunkenness or soberness' (PH, p. 61). Appearances can also differ between different subjects. 'Since…some animals possess…a natural brilliance in their eyes…so that they can even see by night, we seem bound to suppose that they are differently affected from us by external objects' (PH, p. 29).

The words 'we seem bound to suppose' suggest that the multiplicity of appearances provides an argument for the sceptical conclusion: the unreliability of appearances vis-à-vis objective reality. In fact, this Pyrrhonist strategy cannot, by itself, establish (the truth of) scepticism, although it might be psychologically efficacious n bringing us 'to a state of mental suspense' (PH, p. 7). The fac that the tower appears differently from different distances doesn't show that its true shape cannot be known. We might have a reason for thinking one appearance—the one we have when we are near, for instance—to be more reliable. So Sextus adduces an argument to show that there can be no way of deciding between opposing appearances. The argument is based on the problem of the criterion.

> Those who claim for themselves to judge the truth are bound to possess a criterion of truth. This criterion, then, either is without a judge's approval or has been approved. But if it is without approval, whence comes it that it is trustworthy? For no matter of dispute is to be trusted without judging. And if it has been

approved, that which approves it, in turn, either has been approved or has not been approved, and so on *ad infinitum*.

(ATL, p. 179)

To decide between opposing appearances, the argument goes, we need a criterion of truth. But we need, first, to decide between different criteria, and this choice must either be based on yet another criterion, engendering a vicious regress, or on the same criterion, resulting in a vicious circularity.

Hume initially shifts the burden of proof to those who believe in the existence of

a medium, which may enable the mind to draw such an [inductive] inference, if indeed it be drawn by reasoning and argument…[I]t is incumbent on those to produce it, who assert that it really exists, and is the origin of all our conclusions concerning matters of fact.

(Hume, 1777, p. 34)

Even here, Hume isn't demanding that reason be vindicated by reference to something else. Rather, he is challenging us to show that inductive inference really is sanctioned by reason; that it conforms to standards of reasoning that *we* accept, if only implicitly. And this is a legitimate challenge, because unlike reason, common sense (*pace* Reid, 1785, p. 439) isn't the ultimate arbiter. Common sense can be criticized by reason (and found wanting). But, anyway, Hume doesn't rest content with posing this (legitimate) challenge. He aims to persuade, and feels he has to take the initiative. Since he is contradicting common sense, he is at a polemical disadvantage. Even if we are unable to rise to his challenge to vindicate common sense, he will achieve a much more resounding victory if he *proves* that we cannot. So Hume goes on straight away to adduce his sceptical argument to show that induction isn't reasonable. And he subsequently presents various other sceptical arguments to undermine deduction (1739, pp. 180–7), our belief in the existence of external objects (1739, pp. 187–218) and a persisting self (1739, pp. 251–63).

Descartes presents sceptical arguments, so we can treat him as a consequent sceptic, and respond to his challenge thus construed. But Descartes has been interpreted as an antecedent sceptic. In order to rebut this interpretation, and show that Descartes doesn't merely rest content with challenging reason's credentials, we need, first, to

consider the proper response to consequent scepticism. This will be the task of the next chapter. Descartes' brand of scepticism will then be examined in Chapter 5. But before this, we need to consider how the consequent sceptic can mount his challenge.

THE SCEPTICAL DOCTRINE AND ITS JUSTIFICATION

The sceptic who takes upon himself the burden of proof *cannot* be shrugged off, as can one who merely challenges us to vindicate reason. We must rebut the sceptical arguments by showing the reasoning to be invalid, or the premises unwarranted. Pending that, we are obliged, by our own light (the light of reason), to admit that some of our beliefs about justification must be false, since they are jointly contradictory. But isn't the sceptic who takes the offensive doomed to fail? Isn't his position self-defeating? According to the radical sceptic we have no reason to believe anything. But if this applies to the sceptical thesis itself, then the sceptic cannot justifiably assent to his own doctrine. The difficulty is posed by Bourdin as a dilemma for the sceptic: if 'you had "powerful and well thought-out reasons"…, why renounce them?… If, on the other hand, they are doubtful and completely suspect, how have they managed to force or compel you?' (CSM II, p. 315).

The Pyrrhonian sceptic is well aware of the difficulty, but the various attempts he makes to meet it are inadequate. He first suggests that the sceptical thesis can be taken to apply to every proposition except itself.

> For many things are said which imply an exception; and just as we declare that Zeus is 'the Father of both gods and men,' implying the exception of this god himself (for, to be sure, he is not his own father), so also when we say that no proof exists we imply in our statement the exception of the argument which proves that proof does not exist.
>
> (ATL, p. 487)

This response is inadequate. If unsupported by an argument, the sceptical thesis—thus restricted—is unwarranted even if not self-defeating. And if backed by an argument, it will undermine itself, by showing the premises to be unjustified. Conceivably, there could

be an argument which showed the belief in every proposition except its own premises and conclusion to be unjustified. But no such argument has ever been formulated. The argument from the criterion (see the previous section), for instance, applies equally to all beliefs—at least those which aren't about appearances—and casts doubt on its own premises, and, consequently, on its own conclusion.

Some (less radical) forms of scepticism aren't self-defeating. They use premises and inference rules upon which they do not themselves cast doubt. Perhaps scepticism about ethical statements can be based on (non-ethical) claims about the necessity for knowledge of a causal interaction between the knower and the facts known, an interaction which does not exist between us and ethical facts. A sceptical argument against the possibility of empirical knowledge may be premissed on non-empirical (linguistic and epistemological) claims about the necessity of certainty for knowledge, and the unattainability of certainty about empirical matters.

In contrast, Hume's sceptical argument against induction might seem vulnerable to the difficulty. Familiarly, Hume's scepticism about induction (reasoning about 'matters of fact') vitiates his 'attempt to introduce the experimental method of reasoning into moral [i.e., psychological] subjects'. If we cannot justifiably generalize from particular instances, how can we formulate psychological laws? This is the (unresoluble) tension between Hume's naturalism and his scepticism. But doesn't Hume's scepticism undermine *itself* in addition to 'the science of human nature'? It would if his sceptical argument against induction had as a premise the theory of ideas, because this theory is an empirical theory of the mind, to be proved by an appeal to 'obvious, numerous, and conclusive' empirical facts (Hume, 1739, p. 4), precisely the sort of proof that Hume's sceptical argument discredits. And if—as Hume's sceptical argument would have it—the theory of ideas cannot be established, it cannot be adduced as a premise in Hume's sceptical argument.

Two ways of eliminating the difficulty for Hume's inductive scepticism suggest themselves. The first is to deny that the theory of ideas is an empirical theory, thus rendering it immune to Hume's sceptical argument. This response is not very plausible. Admittedly, Hume is insouciant (Stroud, 1977, p. 33) in the face of evidence unfavourable to his theory—the missing shade of blue (Hume, 1739,

p. 6). According to an important principle in Hume's theory, every simple idea arises in the mind only after (and as a result of) its corresponding impression. Yet he admits the possibility that a person who has never seen a certain shade of blue might nonetheless 'get the idea of that particular shade' when presented with 'all the different shades of that colour, except that single one…descending gradually from the deepest to the lightest'. This attitude to the counter-example might suggest that Hume doesn't view his principle as an empirical one. If he did, the missing shade would constitute falsifying evidence and oblige him to reject the principle. His dismissal of the example might suggest that he takes the principle relating simple ideas and impressions to be *definitive* of the term 'simple idea'. Since the idea of the missing shade of blue doesn't have a corresponding impression, it must be complex, rather than simple.

This interpretation is untenable. Hume doesn't think the principle is immune from falsification. He explicitly concedes that the missing shade is an exception, albeit 'so particular and singular', and this shows that he views his 'general maxim' as genuinely empirical (rather than analytic). And this means that Hume's argument against empirical reasoning can be applied to show that his theory of ideas cannot be justified. His argument against induction seems, consequently, to be undermined, as well.

The second, and more promising, way of defusing the difficulty for Hume's scepticism is to show that the theory of ideas is not required to ground scepticism about induction. Admittedly, in the lengthier exposition of the argument against induction in the *Treatise,* Hume invokes his (deflationary) account of causation, which is based on the theory of ideas. We have no idea of a necessary connection between events, Hume claims, there being no antecedent impression of necessity. All that is involved in A causing B, he continues, is the spatio-temporal contiguity of the two events, and the regular occurrence of events like B with events like A. We 'define a cause to be an object precedent and contiguous to another, and where all the objects resembling the former are plac'd in a like relation of priority and contiguity to those objects, that resemble the latter' (Hume, 1739, p. 172, italics omitted). But this means that 'we may easily conceive, that there is no absolute nor metaphysical necessity, that every beginning of existence shou'd be attended with such an object' (1739, p. 172). The law of causality is not a logical truth; 'the necessity of a

cause to every beginning of existence is not founded on any arguments either demonstrative or intuitive' (1739, p. 172).

This argument seems to rely on Hume's view of causation, and—consequently—on the theory of ideas. But in fact, Hume only adduces it 'to overcome all that repugnance, which 'tis so natural for us to entertain against the foregoing reasoning, by which we endeavour'd to prove, that the necessity of a cause to every beginning of existence is not founded on any arguments' (1739, p. 172). He has already (1739, pp. 79–80) provided an argument designed to show that the law of causality isn't a logical truth. Since we can 'conceive any object to be non-existent this moment, and existent the next, without conjoining to it the distinct idea of a cause', the 'separation…of the idea of a cause from that of a beginning of existence, is so far possible that it implies no contradiction nor absurdity'.

Hume's argument, we can now see, doesn't rely on the absence of an impression or idea of necessity. To show that the law of causality isn't necessary he invokes our ability to conceive of an event without a cause. And the principle according to which what is conceivable is logically possible is not an empirical principle. (Of course, it is very contentious, but that is another matter.)

I conclude that Hume's argument against induction doesn't undermine itself. On the other hand, radical scepticism (such as that based on the problem of the criterion) certainly seems to do so, and so does a more moderate (and recent) scepticism about the theoretical statements of science. Van Fraassen (1980) attempts to invoke scientific theories to show that belief in the claims they make about the 'unobservable' is unwarranted. The relevant chemical theory, van Fraassen suggests, tells us that what goes on when we detect particles in a bubble-chamber is not an observation: 'The theory says that if a charged particle traverses a chamber filled with saturated vapour, some atoms…are ionized. If this vapour is decompressed…, it condenses in droplets…, thus marking the path of the particle' (1980, p. 17).

Van Fraassen's attempt to ground scepticism about the unobservable scientifically seems self-defeating. When we use science to determine the boundary between the observable and the unobservable, we invoke beliefs about electrons to classify them as unobservable. How can we then brand as irrational these very beliefs? In order to decide which part of the theory we are debarred from believing, we have to rely on the truth of the theory *in toto*! And

once we realize we ought not to believe the theory's claims about the unobservable, we also lose our reason for so thinking.

FURTHER SOLUTIONS

Can such forms of scepticism (van Fraassen's and Sextus') be sustained or are they self-defeating? Sextus likens the sceptical argument to a ladder:

> [J]ust as it is not impossible for the man who has ascended to a high place by a ladder to overturn the ladder with his foot after his ascent, so also it is not unlikely that the Sceptic after he has arrived at the demonstration of his thesis by means of the argument proving the non-existence of proof, as it were by a step-ladder, should then abolish this very argument.
>
> (ATL, p. 489)

The analogy isn't apt. Having climbed a ladder, we can throw it away only if we can rest our weight on something else (a wall, say). But once the premises of the sceptical argument are undermined, its conclusion is left without logical support. Of course, it may have served its therapeutic purpose, and brought about suspension of belief and peace of mind, but that doesn't help to vindicate its ability to provide a rational basis for belief in the truth of the sceptical doctrine.

In the face of these difficulties, Sextus retreats, and refuses to assert the sceptical thesis, or even the evidence in its favour:

> [W]henever I say 'To every argument an equal argument is opposed,' what I am virtually saying is 'To every argument investigated by me which establishes a point dogmatically, it seems to me there is opposed another argument, establishing a point dogmatically, which is equal to the first in respect of credibility and incredibility'; so that the utterance of the phrase is not a piece of dogmatism, but the announcement of a human state of mind which is apparent to the person experiencing it.
>
> (PH, p. 121)

But of what philosophical relevance are those reports of Sextus' state of mind? Sextus abhors dogmatism, and wishes to refrain from affirming anything. But it seems as if he must choose between being

dogmatic and philosophically irrelevant. If scepticism is a technique, 'an ability, or mental attitude [to oppose] appearances to judgements in any way whatsoever' (PH, p. 7), rather than a doctrine, it is philosophically no more significant than juggling or cycling. And if the counter-arguments are to support the sceptical doctrine, they should be put forward as (rather than merely seeming to be) equally strong. That they so seem to Sextus will cut no ice unless Sextus is claiming to be a fairly reliable judge of arguments. And that is precisely what he is unwilling to do.

A WAY OUT OF THE PREDICAMENT

We must conclude that Sextus hasn't managed to show how one can (epistemically) justify scepticism. What, then, are we to make of arguments for radical scepticism? Can they simply be ignored as self-defeating? No. The sceptic is best construed as arguing *ad hominem*, being, therefore, entitled to invoke premises accepted (and considered justified) by his non-sceptical opponent. From these premises he hopes to derive a sceptical conclusion, which his interlocutor will be forced to accept. The sceptic himself need assent neither to the premises nor to the conclusion.

Will such a sceptical argument constitute a *reductio ad absurdum* of reason? Not quite. We cannot hold steadfastly to the sceptical conclusion, because it undermines the premises and inference rules that led us to accept it. Indeed, it might seem as if radical scepticism is self-defeating after all. If a belief can only be criticized in the light of others, mustn't these be held beyond reproach? In Wittgenstein's words (1969, s. 115): 'If you tried to doubt everything you would not get as far as doubting anything. The game of doubting itself presupposes certainty.'

Unfortunately, radical scepticism isn't so easily rebutted. If it cannot be rationally sustained, neither is it self-defeating. Hume (1739, pp. 186–7) beautifully characterizes the instability of the sceptical conclusion. Using reason, bowing to its authority, the sceptic undermines it from within. Once the ascendancy of reason is undermined, the sceptical argument itself loses its force, and reason regains its throne, only to become vulnerable again to the sceptical threat, and so on, *ad infinitum*. The philosophical argument cannot be settled, even if it is cut short by nature, which forces on us beliefs whose justification is forever being brought

into question. But even if we are not all committed sceptics in the end, our confidence ought to be undermined. The sceptic perennially prevents reflective persons from 'slumbering dogmatically', since they have 'no choice left but betwixt a false reason and none at all' (Hume, 1739, p. 268).

4

THE RESPONSE

OUR OBJECTIVE

We do not aim to persuade the sceptic. Our failure to do *that* may be due to his intransigence; our success, to our rhetorical prowess. Rather, we must contend with the sceptical *position*. (Of course, if the sceptic is reasonable, these two aims will coincide.) We must rebut the sceptic's reasons: a (seemingly cogent) argument cannot simply be shrugged aside just because its conclusion is implausible.

To be sure, sometimes we are faced with an argument whose conclusion is so preposterous that we just know it cannot be sound. In such a case we justifiably disbelieve the conclusion even though we think it is entailed by premises that we believe. This is the case with Zeno's ingenious 'proof' that there is no motion. Even before the fallacy was diagnosed it was reasonable to believe in the existence of motion. But even in such a case, the fallacy must be found. Pending such a diagnosis, we ourselves deem our belief system to be inconsistent: not an ideal cognitive position!

Like Zeno, the (consequent) sceptic presents us with a paradox: 'an apparently unacceptable conclusion derived by apparently acceptable reasoning from apparently acceptable premises' (Sainsbury, 1988, p. 1). Perhaps the sceptical conclusion is so implausible that we can disbelieve it, the arguments in its favour notwithstanding. I doubt it, but at any rate it is incumbent upon us to rebut the arguments, to find a fault in the premises or in the reasoning. Even if the sceptical argument doesn't threaten our rationality and the integrity of our inquiries, still it leaves our epistemological theorizing in a shambles.

OUR STRATEGY

How are we to rebut the sceptical arguments? What can we legitimately appeal to? A very austere response is given by Stroud, who claims that in responding to the sceptic we must 'put all that alleged [scientific] information into jeopardy and hence…render it unavailable' (Stroud, 1984, p. 221). The 'knowledge…repudiated [by the sceptic] could not be appealed to to meet the challenge' (Stroud, 1984, p. 231). In a similar vein, Hookway suggests that 'we should not rely upon beliefs which are threatened by a criticism in responding to it' (1990, p. 221). The stricture seems to reflect a laudable intention not to beg any questions against the sceptic, but in fact it concedes too much. We are not automatically debarred from invoking a premise (or a form of inference) just because the sceptic questions it. The rules of the engagement with the consequent (reasoning) sceptic are those of *rationality*.

When Sextus cites an 'old wife of Attica…[who] swallowed with impunity thirty drams of hemlock' (PH, p. 49) so as to show the dependence on our constitution of sense-impressions, we quite properly respond by denying the alleged evidence. Do we need to justify our denial? The claim may just seem preposterous, as it may have to Sextus himself; since his aim is '*conversion* rather than rational persuasion' (Hookway, 1990, p. 7, original italics), he allows himself (PH, pp. 511–12) to adduce bad arguments. The supposition that jaundiced people see things as yellow, put to a similar use by the sceptic (PH, p. 29), may also be rejected when exposed as a myth.

The same methodology should be followed when confronting Hume's sceptical argument against deduction (1739, bk I, pt IV, s. I). He suggests that 'if any single addition were certain, every one wou'd be so, and consequently the whole or total sum; unless the whole can be different from all its parts' (1739, p. 181). Since long sums aren't certain, even the simplest sum (1+1=2) must be (to some degree) doubtful.

The fallacy isn't hard to detect: Hume ignores the fact that when adding two large numbers (23+38, say) by a sequence of unit additions, one may fail to keep track of the number of steps one has already made. Each step (23+1=24) is, to be sure, obvious. But one needs also to know where, in the sequence of additions (23+1=24, 24+1=25,…) one must stop so as to get the requisite sum. And *this*

knowledge isn't trivial: one can certainly make mistakes in counting. Note, again, that in responding to the sceptical argument we do not shed our beliefs; we allow ourselves to appeal to various things we take ourselves to know: facts about the way sums are computed, the fallibility of memory, etc.

Two more examples illustrate the correct methodology to be employed against the sceptic. Consider first the sceptical argument which invokes the scientific method against itself. The sceptic claims that past scientific theories have all been falsified. The 'pessimistic meta-induction' (Putnam, 1978, p. 25), he continues, supports the conclusion that our current ones are also likely to be false. The scientific method, he concludes, is unreliable.

As most (if not all) sceptical arguments, this one goes back to Sextus Empiricus, who points out that

> [j]ust as, before the birth of the founder of the School to which you belong, the theory it holds was not as yet apparent as a sound theory, although it was really in existence, so likewise it is possible that the opposite theory to that which you now propound is already really existent, though not yet apparent to us, so that we ought not as yet to yield assent to this theory which at the moment seems to be valid.
>
> (PH, p. 23)

In response to the sceptical argument we do not eschew the scientific method. It has not *yet* been impugned: we are merely confronting a (pessimistic) view about it. Rather, we invoke the scientific method to vindicate itself. We point out, first, that the history of science is not a history of falsifications, and doesn't, therefore, support the conclusion that our present scientific theories are likely to be false. The following theories (and many others) have all withstood the test of time: water is a compound of hydrogen and oxygen, the tides are caused by the moon, blood circulates around our body.

We then go on to point out that the history of science also suggests that science is *progressive;* that even those theories that were falsified were closer to the truth than their predecessors; better predictively and more explanatory. The scientific method, we conclude, is conducive to the attainment of our cognitive and practical aims, and is therefore eminently rational. And this conclusion is reached by using the scientific method itself!

Consider, next, Nozick's (1981) attempt to reject the sceptical doctrine by invoking a counterfactual analysis of knowledge. According to Nozick, a belief in p is knowledge if it is true and 'truth-tracking': very roughly, if p weren't true, the agent wouldn't believe it. Now, Nozick argues, we do not know that we aren't being deluded by a demon: our belief that we aren't doesn't track the truth. If we were being deluded, we would still believe we weren't. But, he adds, this concession to the sceptic is mitigated by the fact that we know various other propositions, such as 'I am sitting at my desk'. These propositions are known, because they do track the truth: were I not sitting at my desk, I would (say) be lying on the couch, and believing myself to be doing so.

Nozick can show where the fault in the sceptic's reasoning lies. His analysis enables him to reject in a principled way one of the premises in Descartes' demon argument: If I do not know I am not being deceived by a demon, I do not know I am not sitting at my desk. This premise can be deduced from a seemingly plausible constraint on knowledge, closure under known entailment: if I know p, and I know that p entails q, then I know q. (My sitting at my desk knowingly entails my not being deceived by a demon.) But, Nozick claims, and cites his analysis as supporting evidence, knowledge doesn't satisfy the condition. Truth-tracking knowledge isn't closed under known entailment.

Whether or not Nozick's strategy is successful needn't concern us here. (That will be discussed in Chapter 10.) But I wish to uphold its very *propriety* against a prevalent criticism.

Johnsen (1990, p. 30) argues that Nozick's response is question-begging, since the sceptic thinks that we have 'no idea whether any of our beliefs constitute knowledge', and do not know whether the subjunctive conditionals by reference to which knowledge claims are to be assessed ('If p weren't true, I would not believe it') are true. In a similar vein, Craig argues that as 'a weapon against scepticism, [Nozick's] analysis is either impotent or redundant'.

> [If] we *are* in a position to assert that the actual world is not a sceptical world then the sceptic must somehow already have been defeated without recourse to the 'tracking' analysis; if we are *not* in a position to assert it, recourse to the analysis doesn't help.
> (Craig, 1989, p. 161, original italics)

Both objections seem to be made on behalf of an antecedent sceptic, who demands that we vindicate our knowledge claims from scratch. But this (antecedent) sceptic can be ignored (see pp. 31–5). The consequent sceptic, on the other hand, only demands that we relinquish our knowledge claims once we have been persuaded by his arguments: this will be the (unhappy) terminus, and not the starting point, of our epistemological inquiry.

> *If* we…reasoned as Descartes reasons and arrived by *reductio ad absurdum* at the conclusion that we know nothing of the physical world, and we found ourselves dissatisfied with that conclusion, clearly we could not go blithely on to satisfy ourselves and explain how knowledge is nevertheless possible by appealing to those very beliefs about the physical world that we have just consigned to the realm of what is not known.
>
> (Stroud, 1984, p. 229, my italics)

But Nozick is trying to *assess* Descartes' reasoning, and has not yet been forced to relinquish all his knowledge claims, or his ability to argue back at the sceptic.

If we are allowed to invoke every belief we deem reasonable, isn't our victory over the sceptic a foregone conclusion? For won't we then allow ourselves to assume that we know various things? And isn't this precisely what the sceptic is denying? To see that we are not giving the sceptic a raw deal even if we do not strip ourselves of every belief that he questions, consider Moore's 'proof of the external world'. When Moore holds his hand up to show that he knows there is at least one material object, he is accused of begging the question. But the objection mislocates the flaw in Moore's procedure. It is not Moore's very appeal to common sense that is illegitimate, but, rather, his refusal to take seriously the sceptical argument. While common sense affirms our knowledge of various propositions ('I have a hand'), it also finds persuasive (at least prima facie) the sceptic's premises: those of Descartes' dream argument, whose conclusion is that we do *not* know we have a hand.

Adding to the sceptical argument various premises about particular things we know doesn't prevent the derivation of the sceptical conclusion: the (deductive) validity of the argument isn't impugned by the addition of further premises. And that means that citing things we know doesn't constitute an adequate reply to the sceptic. Remember, he has presented us with a paradox. At best

Moore shows its conclusion must be false. But we can see that this is no solution by considering how paradoxes are to be solved.

We do not adequately respond to Zeno by pointing out that in real-life races Achilles does catch up with the tortoise, because that does not impugn Zeno's derivation. Our experience of races merely provides us with the impetus to expose the (subtle) fallacy in the reasoning by which Zeno purports to show that the tortoise will always remain ahead. Similarly, putting up our hand and affirming our knowledge that it exists doesn't solve the sceptical paradox. To do that we must reject (at least one of) the premises in a principled way or find the fallacy in the reasoning. This is what Moore declines to do. He simply dismisses the sceptical arguments:

> I think we may safely challenge any philosopher to bring forward any argument in favour either of the proposition that we do not know [that this is a finger]…which does not at some point, rest upon some premiss which is, beyond comparison, less certain than is the proposition which it is designed to attack.
>
> (Moore, 1959, p. 228)

But the sceptic *has* responded to Moore's challenge: he *has* produced an argument. And it is now incumbent upon Moore to show how the sceptic errs. Which of his premises is 'less certain' than the proposition that Moore knows he has a hand, and why? This obligation Moore does not discharge.

Unlike Moore, Nozick meets the (consequent) sceptic head on. If his analysis of knowledge is correct, he can show *where* and *how* the sceptic has gone wrong, and even explain our initial inclination to be impressed by the demon argument.

NATURALIZED EPISTEMOLOGY?

In responding to the sceptical challenge, Quine has argued, we 'may make free use of all scientific theory' (1973, p. 2). In fact, Quine continues, epistemology 'simply falls into place as a chapter of psychology and hence of natural science' (1969, p. 82). Perhaps Quine is too permissive in his attitude to science: surely we should rely only on scientific theories which are well established, rather than on those which are current. But, certainly, his conception of epistemology is too restrictive.

Quine's liberated epistemologist 'can appeal to physical receptors of sensory stimulation' (1973, p. 3), Darwinian theory, and empirical psychology. But why can he draw only upon science? He can accomplish very little if thus restricted.

'The Humean predicament is the human predicament' (Quine, 1969, p. 72), Quine himself has concluded. His pessimism is based on the fact that the appeal to any scientific theory such as Darwinism to justify induction is circular, since 'biological knowledge depends upon induction' (1975, p. 70). But if the predicament is our inability to rebut Hume's argument against induction, Darwinism won't extricate us. It may enable us to explain the survival value of induction, but it doesn't help us to rebut the sceptical *argument*. Even when 'we have stopped dreaming of *deducing* science from observations' (Quine, 1969, p. 76, my italics), we may still entertain Humean doubts about the possibility of *inducing* it. Those doubts are fuelled by Hume's argument, and won't go away until the reasoning is shown to be flawed. And that certainly cannot be done by citing scientific theories. Stroud is correct in suggesting that natural science doesn't even 'address…the philosophical problem' of 'traditional epistemology' (1984, p. 253).

Quine's pessimism is premature. We may yet be able to rebut Hume's argument against induction. To that end, we will not invoke the theory of natural selection, but, rather, focus on the concept of justification (Chapter 1).

We wish to adduce against the sceptic considerations pertaining to the nature of truth, meaning, knowledge and justification. If statements about physical objects are 'far in excess of any available data' (Quine, 1960, p. 22), then we need to rebut the sceptic's argument against the legitimacy of such inductive leaps. To this end Quine himself invokes simplicity and economy; not the physiology of the eye! Physical object theory, he argues, 'has proved more efficacious than other myths as a device for working a manageable structure into the flux of experience' (1951, p. 41); 'the smoothest and most adequate overall account of the world' (1960, p. 4).

Philosophy and natural science complement one another. We want to know how 'meager two-dimensional optical projections' get translated in the brain into 'a description of the three-dimensional external world' (Quine, 1969, p. 83). But to contend with the sceptical argument we also need to determine whether we must know we are

not dreaming if we are to know there is a chair in front of us. Or is it enough (say) that our belief be caused by the chair? We need to know whether the demon hypothesis is even coherent, and whether statements about external objects must always be inferred from statements about immediate experience. These questions are all distinctly philosophical, and not part of natural science or psychology.

Narrowly construed, science cannot supplant epistemology, but (*pace* Stroud, 1984, p. 221) this does not mean that traditional epistemological questions (unlike 'purely scientific' ones) are insoluble. For a start, some of the claims adduced in the dispute about scepticism may be analytic. The sceptic may argue that knowledge (analytically) requires certainty (Unger, 1971), while his opponent (Strawson, 1952, ch. 9) may cite as analytic the claim that induction is reasonable.

Even those who have resisted Quine's (1951) rejection of the distinction between the analytic and synthetic (Grice and Strawson, 1956) will find the status of epistemological considerations very confounding. Consider the verificationist principle of meaning, according to which statements are meaningful only if they are analytic or empirically testable. The principle is invoked to reject as meaningless sceptical hypotheses, which are supposed to be incapable of empirical tests. What is the status of the principle itself? Is it empirical, based on considerations pertaining to the acquisition of language? Or is it, as Ayer (1971, p. 21) thinks, 'a definition of the word "meaning"'? What about Quine's holistic view of science, or the principle of charity (Davidson, 1974), which restricts the extent to which our beliefs can be mistaken? We needn't decide the status of these principles. Our licence to invoke them depends on their being justified. If 'science' only includes natural and social science, then philosophy isn't 'scientific', but that does not impugn our attempt philosophically to rebut the sceptic: we needn't suppose that 'science' is all there is that is true or legitimate. Perhaps there is no 'cosmic exile'; no 'vantage point outside the conceptual scheme' that the philosopher can occupy (Quine, 1960, p. 275). But philosophical questions are distinct from scientific ones: science is concerned neither with the formulation of methodological rules nor with the explication of epistemic concepts. And even if philosophy is continuous with science, there are still considerations that it can call its own.

No doubt they cannot be wielded as effectively as can scientific ones. We won't reach an agreement about the standards (implicitly) imposed by the concept of knowledge, or about the correct theory of meaning, in the light of which we can adjudicate the intelligibility of the demon hypothesis. These are highly theoretical and controversial questions. Still, to reach even a partially warranted view about the sceptical thesis they must be confronted.

MODEST EPISTEMOLOGY

There are five ways in which epistemology can be viewed as a fairly modest enterprise. First, it can (and need) contend only with consequent scepticism. Second, it doesn't have to follow Descartes in his attempt to 'establish [something] in the sciences that was stable and likely to last' (CSM II, p. 12). Descartes thinks the considerations he adduces will provide beliefs with a kind of grounding they didn't have before: his project 'is…to demolish everything completely and start again right from the foundations' (CSM II, p. 12). But epistemology can be thought of less ambitiously. By explaining the nature of justification (and knowledge), it renders more perspicuous *existing* epistemic relations, and (hopefully) shows sceptical doubts to have been erroneous all along.

Descartes' epistemological overhaul isn't just immodest. It is also very comprehensive, aiming to redeem most, if not all, of his former beliefs. Seemingly, even his belief in his own existence is unjustified until he reflects upon the fact that the demon 'will never bring it about that [he is] nothing so long as [he thinks] that [he is] something' (CSM II, p. 17). But perhaps his existence was secure even before he began reflecting. At least, he cites it (CSM I, pp. 14–15) as an intuition, free of 'movement or a sort of sequence' (inference). And this suggests that it doesn't attain justification by being the conclusion of the *Cogito,* but was, rather, justified all along.

Hume agrees with Descartes about the pre-reflective epistemic status of our beliefs. Inductive reasoning, he argues, cannot be 'abstruse…since…it must be obvious to the capacity of a mere infant…[being] reasoning which engages *us* to suppose the past resembling the future' (Hume, 1777, p. 39, my italics). Since he has not yet discovered how induction is justified, he can conclude, without being 'guilty of unpardonable arrogance' (1777, p. 38) that

no justification exists. He would be a very 'backward scholar', he says ironically, if he couldn't 'discover an argument which was perfectly familiar to [him] long before [he] was out of [his] cradle' (1777, p. 39). But then, unlike Descartes, he adduces an argument to show that the pre-reflective situation cannot be improved: induction cannot be justified even by 'abstruse' and sophisticated reasoning. Neither the modest nor the immodest enterprise can be carried out, Hume thinks. In Chapter 8 I shall present a modest response to Hume's challenge.

Given this sense of 'modesty', how are we to construe the role of transcendental arguments? Their aim is to establish conditions necessary for experience, thought, or language. Thus, for Kant, our experience must be of spatio-temporally located, causally related objects. For verificationists, the possibility of deciding the truth-value of a statement is a necessary condition for its meaningfulness. Davidson (1986) cites a necessary condition for thought: we can only ascribe beliefs to an agent if they are by and large true.

If transcendental arguments purport to show that something is constitutive of (some aspect of) reality, one might attempt to wield them immodestly. If a condition, X, is necessary for experience (thought, language, etc.), then one could argue from the (very minimal) assumption that there is experience (thought, language) to the truth of X. Given that there is experience, Kant argues, the law of causality must be true. Given that we understand the statements in our language, verificationists reason, they must be capable of verification (or refutation). How are we to construe such uses of transcendental arguments?

Suppose the principle of charity (Davidson, 1986) is correct, and our beliefs must be by and large true. The principle can then be taken as justifying our beliefs; its use by Davidson would be immodest. That would mean that Davidson was unjustified in his beliefs about the external world (say) before he formulated (and defended) the principle, and that the beliefs of the philosophically naive, who cannot comprehend Davidson's argument, and of those who have understood it without being persuaded, are still unjustified.

There is an alternative, *modest,* way of viewing the principle of charity. Externalists think that beliefs are justified in virtue of facts of which the agent need not be aware. Thus, for a reliabilist (Armstrong, 1973), a belief is justified if it is acquired by a reliable method,

whether or not the agent knows this. The principle of charity renders us reliable believers. And the reliabilist may invoke his analysis of the concept of justification *modestly,* in conjunction with Davidson's argument. He won't provide us with justification we didn't have before he came on to the scene. Rather, he will show that we, naive folk included, have had it all along. I shall return to the question of how transcendental arguments are best construed (see pp. 120–1).

We have now arrived at the third way of construing the distinction between modest and immodest epistemologies. Epistemology can be conceived, following Descartes, as a practical '*technique* for acquiring beliefs' (Papineau, 1987, p. 130, original italics). This is an ambitious, immodest conception. Alternatively, and perhaps more plausibly, epistemologists' concern with knowledge and justification can be purely theoretical. The claim that knowledge is reliably acquired belief may be illuminating, but is fairly useless practically.

The fourth sense of 'modest' is illustrated by Michael Williams (1991, p. 42), who thinks we can at best show that 'the case for scepticism…[isn't] compelling'. But surely it is not enough to pit scepticism against common sense as equal contenders for our allegiance. That will engender a stalemate, and we want to *defeat* the sceptic. No modesty here!

Why is Williams pessimistic about the prospects for defeating the sceptic? He thinks we cannot '*prove* the correctness of [our non-sceptical epistemic] view' (M.Williams, 1991, p. 44, my italics). If a 'proof' has to be conclusive, this is true, but then we do not aim for proofs even in science, so why should the epistemologist set himself such an onerous task? In being modest in this (fifth) sense, and conceding that we cannot attain certainty in epistemology, we are not settling for a stalement. We might be able to show that our non-sceptical view is more reasonable than the sceptic's. If we cannot knock out the (consequent) sceptic, we may hope to defeat him on points.

5

DESCARTES' SCEPTICAL CHALLENGE

INTRODUCTION

I have distinguished two kinds of sceptical challenges—consequent and antecedent, and argued that we must take seriously only the former (Chapter 3). Having considered the proper response to it (Chapter 4), I can now defend my classification of Descartes as a consequent sceptic—an outstanding debt from Chapter 3. I propose to do this by considering the (in)famous 'Cartesian Circle', an apparent circularity in Descartes' response to the sceptic (see the following section).

If we construe Descartes as an antecedent sceptic, we can easily explain why he was bound to argue in a circle (see pp. 54–5). Since I wish to uphold a different interpretation, it is incumbent upon me to provide an alternative explanation for the circle. After pointing out the inadequacies of the antecedent interpretation (see pp. 55–60), I shall explain why Descartes, in confronting a consequent sceptic, a more amenable adversary, nonetheless gets embroiled in circular reasoning, and cannot be extricated (see pp. 60–4).

THE CARTESIAN CIRCLE

Arnauld notes a circularity in Descartes' attempt to 'establish anything at all in the sciences that was stable and likely to last' (CSM II, p. 12):

> we are sure that what we clearly and distinctly perceive is true only because God exists. But we can be sure that God exists only because we clearly and distinctly perceive this. Hence, before we

can be sure that God exists, we ought to be able to be sure that whatever we perceive clearly and evidently is true.

<div align="right">(CSM II, p. 150)</div>

Descartes' reasoning does appear circular. Admittedly, he seems to think he 'is able to lay it down as a general rule that whatever [he] perceive[s] very clearly and distinctly is true' (CSM II, p. 24) even before he proves the existence of God. But, more plausibly, he is merely considering the possibility of deriving the principle at this stage, only to reject it, pending a proof that he is not deceived. And, indeed, later, he says that after he has 'perceived that God exists', he has '*drawn the conclusion* that everything which [he] clearly and distinctly perceive[s] is of necessity true' (CSM II, p. 48, my italics).

As to the second arc of the circle, Descartes seems to invoke the principle of clarity and distinctness in proving God's existence. In the third *Meditation,* he makes an implicit appeal to the principle in defence of a premise he wishes to invoke: 'it is manifest *by the natural light* that there must be at least as much reality in the efficient and total cause as in the effect of that cause' (CSM II, p. 28, my italics). Descartes will then use this causal principle to argue that the cause of the idea he has of God must have at least as much reality as does the perfect idea of God, and must, therefore, be a perfect being, i.e. God.

Can the charge of circularity be rebutted? Descartes does argue in a circle, but must he? The answer, of course, depends on his aims.

THE SOURCE OF THE CIRCULARITY: ONE DIAGNOSIS

Many philosophers explain the circularity as stemming from Descartes' antecedent brand of scepticism. 'Cartesianism… teaches', Peirce claims,

> that philosophy must begin with universal doubt… But this initial scepticism will be a mere self-deception and not real doubt… A person may,…in the course of his studies, find reason to doubt what he began by believing; but in that case he doubts because he has a positive reason for it, and not on account of the Cartesian maxim.

<div align="right">(Peirce, 1965, 5.264)</div>

According to Reid, Descartes 'build[s] his scepticism upon this foundation, that all our reasoning and judging powers are fallacious in their nature, or…resolve[s] at least…to withhold assent until it be proved that they are not' (Reid, 1785, p. 447).

'The doubt', Austin complains against Descartes, must have

> a special basis, there must be some 'reason for suggesting' that it [the table, say] isn't real, in the sense of some specific way…in which it is suggested that this experience… may be phoney… If the context doesn't make it clear, then I am entitled to ask 'How do you mean?… *What are you suggesting?*'
>
> <div align="right">(Austin, 1961, p. 55, original italics)</div>

'The traditional Cartesian examination', Stroud claims,

> aims at an assessment of all our knowledge of the world all at once, and it takes the form of a judgement on that knowledge made from what looks like a detached 'external position'…[O]nce we [adopt] that lofty…philosophical standpoint,…scepticism can come to seem inevitable.
>
> <div align="right">(Stroud, 1984, p. 209)</div>

Antecedent scepticism, which Hume attributes to Descartes, has universal doubt as a starting point. This kind of scepticism, Hume points out, cannot be vanquished. Even if there was an 'original principle, which has a prerogative above others…[we could] advance a step beyond it, but by the use of those very faculties, of which we are supposed to be already diffident' (Hume, 1777, p. 150). If reason is gagged, it cannot defend itself. 'The Cartesian doubt', Hume concludes, is 'entirely incurable'. If one tries to argue one's way out of it, one is bound to reason in a circle or invoke arbitrary premises. Descartes, it seems to Hume, chooses to be impaled on the first horn of the dilemma.

DESCARTES IS NOT AN ANTECEDENT SCEPTIC

The imputation of scepticism 'antecedent to all study and philosophy' to Descartes (Hume, 1777, p. 149) is unfair. Austin misrepresents the Cartesian sceptic. Rather than faulting our grounds in the abstract, he has a very specific 'lack' in mind, a way in which our beliefs might be

false: being induced by a demon, or being dreamt. Moreover, the doubt must be well motivated: 'there is not one of my former beliefs about which a doubt may not properly be raised; and this is not a flippant or ill-considered conclusion, but is *based on powerful and well thought-out reasons*' (CSM II, pp. 14–15, my italics).

What counts as a reason for doubt? According to both Kenny (1968, pp. 18–20) and Frankfurt (1970, p. 20), Descartes' reason for universal doubt is provided by the 'large number of falsehoods that [he] had accepted as true in his childhood' (CSM II, p. 12). But in fact Descartes doesn't view this as a very compelling ground for universal doubt:

> although the senses occasionally deceive us with respect to objects which are very small or in the distance, there are many other beliefs about which doubt is quite impossible, even though they are derived from the senses—for example, that I am here, sitting by the fire,…and so on.
>
> (CSM II, pp. 12–13)

The claim that the senses are in general unreliable isn't warranted by citing the deceptive visual appearance of a stick immersed in water. Here, we judge appearances to be misleading because (we think) we know the truth about the matter: the stick is, in fact, straight. And even if, following Sextus, we avoid adjudicating between the opposing appearances (given to us by sight and touch, say), and invoke the law of contradiction to conclude that one of the senses is here unreliable, such cases cannot ground a *general* claim about the unreliability of the senses, because they are not *typical* (B.Williams, 1978, p. 51). When I observe the stick out of the water, I do not suppose its (straight) appearance is deceptive. So generalizing from the delusory cases would be analogous to inferring (Goodman, 1973, p. 82) from the fact that all my utterances hitherto antedated 1996 that everything I will ever say will be prior to 1996.

Recognizing that the falsity of some perceptual judgements cannot impugn the senses, Descartes goes on to adduce an argument that will—the argument from Dreams. If we designate by 'D' and 'Kp', respectively, the propositions 'I am dreaming' and 'I know that p', then the argument's two premises are:

(1) ~K~D
(2) ~K~D?~Kp.

 I shall follow the tradition in referring to the Dream argument. In fact, as I have presented it, this is not an argument, but an argument-*schema*. By substituting different propositions for p, we get *different* sceptical arguments, structurally alike. Why not replace the second premise with (p) (~K~D?~Kp) so as to get a single argument? The answer is that many instances of (2) do seem plausible, but the universally quantified statement fails: it has at least one false instance—'I am thinking'.

The second premise isn't explicitly stated by Descartes, but the first is a rendition of his observation 'that there are never any sure signs by means of which being awake can be distinguished from being asleep' (CSM II, p. 13). The lack of criterion, note, enables Descartes to claim that he may be dreaming at any time. To impugn the credibility of a belief, p, which he holds now, he needn't (*pace* Walsh, 1963, p. 91) invoke the stronger (and perhaps incoherent) possibility that his life may be a perpetual dream, although sometimes (CSM II, p. 408) he does.

Descartes subsequently adduces another sceptical argument: the demon. Its two premises are:

(1) ~K~DEM
(2) ~K~DEM?~Kp

Here, 'DEM' designates the proposition 'I am deceived by a demon'. Again, this is an argument-schema, engendering a host of (structurally identical) arguments, some more plausible than others. The second premise-schema is false when we substitute 'I am thinking' for p. But many other instances look plausible.

Why does Descartes need to supplement the dream argument? He thinks it leaves our belief in the propositions of mathematics intact, since they do not satisfy the second premise: 'whether I am awake or asleep, two and three added together are five, and a square has no more than four sides' (CSM II, p. 14). Perhaps he is mistaken in this. According to Hookway (1990, p. 53), the dream argument can be wielded more ambitiously, since we could be deceived in a dream about the validity of a purported mathematical proof.

It is clear, then, that Descartes does provide reasons for scepticism. But there are passages which seem to contradict my interpretation of him as a consequent sceptic. He seemingly adopts a less stringent

conception of the sceptical challenge when he likens himself to a person who wishes to remove all the rotten apples from a basket:

> Would he not begin by tipping the whole lot out of the basket? And would not the next step be to cast his eye over each apple in turn, and pick up and put back in the basket only those he saw to be sound, leaving the others?
>
> (CSM II, p. 324)

The analogy is invoked to motivate general suspension of belief independently of the sceptical arguments, and this is inconsistent with viewing the sceptical arguments as motivating the doubt: 'we need some reason for doubting; and that is why in my First Meditation I put forward the principal reasons for doubt' (CSM II, p. 270).

The more liberal attitude seems also to be manifested in Descartes' suggestion that the sceptical arguments are required to reinforce the resolution to suspend belief, since 'no matter how much we have resolved not to assert or deny anything, we easily forget our resolution' (CSM II, p. 270). Here, it seems as if the reason for suspending belief is provided independently of and prior to the formulation of the sceptical arguments. In the light of these two passages, how can I insist that Descartes' scepticism is consequent?

To answer this question, we should distinguish between the *motive* for embarking upon the epistemological project of assessing one's beliefs, and the *standard* by reference to which the assessment is conducted. To initiate the assessment of one's beliefs for their 'stability', one needn't actually think, or even suspect, that the 'structure' is insecure. It is enough that one find the question worth investigating. And perhaps, furthermore, one needs a psychological impetus, in the guise of the sceptical arguments, for maintaining one's resolve to conduct the inquiry. But what is crucial for interpreting Descartes' sceptic as consequent is that the assessment is conducted by reference to the sceptical arguments. Those beliefs, and only those, which withstand the arguments are worthy.

To be sure, the demon functions as a psychological aid to the conduct of the inquiry (Cottingham, 1976).

> [Since Descartes'] habitual opinions keep coming back,… and capture [his] belief,…it will be a good plan to [pretend] for a time

that these former opinions are utterly false… [He will] do this until the weight of preconceived opinion is counter-balanced and the distorting influence of habit no longer prevents [his] judgement from perceiving things correctly.

(CSM II, p. 15)

The way to do this is to 'suppose…that not God, who is supremely good and the source of truth, but rather some malicious demon of the utmost power and cunning has employed all his energies in order to deceive [him]' (CSM II, p. 15).

But this is not the demon's only role. The demon also engenders an epistemological standard by reference to which Descartes will assess his beliefs. This is why Descartes vows to accept—at least in the beginning of his inquiry—'only things of the kind which provide no possible scope whatever for that rascally demon to impose on [him], no matter how hard he tries' (CSM II, p. 314). Pending a refutation of the demon hypothesis, these are the only beliefs which withstand the argument: they do not satisfy its second premise. Of course, Descartes doesn't believe in the existence of the demon. He must 'follow the trail of truth… just *as if* there were some supremely powerful and cunning demon who wished to lead [him] into error' (CSM II, p. 314, my italics). He can happily acknowledge the possibility Bourdin raises that the demon 'is hooting with laughter because he has managed to persuade [him] that [he] sometimes dream[s] and [is] deceived, when this in fact never happens' (CSM II, p. 316).

At the end of his inquiry, Descartes has another strategy against the demon. He can now rebut the argument by rejecting its first premise (~K~DEM). It is incompatible with the existence of a veracious, benevolent and omniscient God.

The dream argument is confronted in a similar fashion. We do know we are not dreaming, because there is an intrinsic difference (coherence) between wakeful experience and that of a dream:

when I distinctly see where things come from and where and when they come to me, and when I can connect my perceptions of them with the whole of the rest of my life without a break, then I am quite certain that when I encounter these things I am not asleep but awake.

(CSM II, p. 62)

The criterion of wakefulness is underwritten by God's benevolence: 'from the fact that God is not a deceiver it follows that in cases like these I am completely free from error' (CSM II, p. 62). When applying the criterion, one is using one's cognitive faculties properly, and the truth of beliefs thus engendered, unlike those produced when the will overcomes reason, are guaranteed by God.

The second premise can also be denied. Some beliefs ('I exist') withstand it to begin with. Others do so only once the principle of clarity and distinctness has been established: 'Can one raise the objection…that I may be dreaming?… Yet even this does not change anything. For even though I might be dreaming, if there is anything which is evident to my intellect, then it is wholly true' (CSM II, p. 49). This appeal doesn't merely rebut the sceptical premise: it enables Descartes to *disprove* the sceptical conclusion, and show that he *does* know various things.

Having shown that Descartes provides sceptical arguments and attempts to rebut them, I conclude that his plight, the circle, is misdiagnosed by Stroud and Hume. The 'detached', 'lofty' standpoint—general doubt—is not Descartes'. His doubt is motivated by reason, and backed by arguments. And universal doubt isn't incumbent upon one who is using reason to show the inadequacy of sceptical arguments (Chapter 3).

A NON-CIRCULAR RESPONSE TO CONSEQUENT SCEPTICISM?

To count as knowledge, Descartes thinks, a belief must be certain. The 'wise' man 'reject[s] all…merely probable cognition and resolve[s] to believe only what is…incapable of being doubted' (CSM I, p. 10). In Archimedes' fashion, he has found 'one thing …that is certain and unshakeable' (CSM II, p. 16). He now wishes to attain further certain knowledge using his Archimedean point. He begins by proving God's existence.

To transfer certainty to a conclusion, the premises and inference rules invoked in the proof must themselves be certain. The Cartesian certainty, furthermore, isn't (*pace* Peirce, 1965, 5.264) merely subjective: Descartes is aware of having 'accepted as wholly certain and evident many things which [he] afterwards realized were doubtful' (CSM II, p. 24).

There are just two ways of avoiding the circle. The first is to justify the premises (and rules of inference) used in the proof of God's

existence independently of the principle of clarity and distinctness. I shall first, in the following section, consider two suggestions as to how this might be achieved. I will then (see pp. 63–4) examine the alternative strategy: proving the principle of clarity and distinctness without appealing to God's existence.

DOING WITHOUT THE PRINCIPLE OF CLARITY AND DISTINCTNESS?

According to Gewirth (1941, p. 383), the premises are psychologically certain: 'the mind cannot help assenting to them as true at the time it has [them]'. Now, one may well feel psychologically certain about the premises without appealing to the principle of clarity and distinctness. Unfortunately, psychological compulsion does not guarantee truth. And if the premises are merely psychologically certain, the conclusion isn't guaranteed to be true, either. So Gewirth cannot extricate Descartes from the circle.

The second way of implementing this strategy is the following. Descartes' premises would be justified independently of the principle of clarity and distinctness if they were *basic*—justified non-inferentially (van Cleve, 1979). And, indeed, Descartes suggests that 'first principles…are known…through *intuition*', and involve no mental 'movement or a sort of sequence' (CSM I, p. 15, my italics). In particular, they are not derived by an appeal to the principle of clarity and distinctness, although they can be subsumed under it. We subsequently discover that they are clearly and distinctly perceived, but this knowledge isn't our *ground* for believing them (van Cleve, 1979).

Van Cleve offers (indeed imputes) to Descartes the foundationalist view of knowledge, according to which the justification of some beliefs (and inference rules) isn't provided by others: the chain of justification can stop with them. I shall discuss foundationalism in Chapter 7, but for now we may grant it. Even if correct, foundationalism cannot extricate Descartes from the circle.

It is not enough that the premises and inference rules be justified independently of the principle of clarity and distinctness. Descartes has set out 'to reply to [these sceptical] arguments… and…to show the firmness of the truths which [he] propound[s] later on, in the light of the fact that they cannot be shaken by these metaphysical doubts' (CSM II, p. 121). If the premises are basic, are they invulnerable to such (metaphysical) doubts?

Descartes is ambivalent about the status of the premises. On occasions he is pessimistic. Has he not discovered grounds (albeit metaphysical) for doubting those various 'simple truths' once he has formulated to himself the sceptical hypotheses? Initially, these metaphysical doubts seem to assail all his beliefs. An exception is subsequently discovered: the belief in his own existence can withstand even the demon hypothesis. So Descartes is exaggerating when he claims that without the knowledge of God's existence '*nothing* can ever be perfectly known' (CSM II, p. 48, my italics). But he seems to think that even the inference rules and truths of logic are open to demon-induced doubt: 'may I not… go wrong every time I add two and three or count the sides of a square, or in some even simpler matter?' (CSM II, p. 14). Descartes doesn't explicitly say what the 'simpler things' about which the demon might deceive him might be, but what—other than a truth of logic or an inference rule—could count as simpler than '2+3=5' or 'the square can never have more than four sides'? Certainly the belief in the truths and inference rules of logic cannot be defended against the demon in the way that the belief in the proposition 'I doubt' can: 'let him deceive me as much as he can, he will never bring it about that I am nothing so long as I think that I am something' (CSM II, p. 17).

Arguing *ad hominem,* Reid (1785, p. 447) suggests that once the *Cogito* is accepted, so must the other statements doubted in the first *Meditation*. But Descartes' doubt isn't antecedent, and the discriminations he makes are perfectly legitimate: only beliefs which are not susceptible to the sceptical arguments can be retained.

Perhaps additional beliefs can at that stage be wrested from the clutches of the demon. We may come to see how merely 'attending' to them; acquiring 'a perspicuous representation of [their] functioning' (Hookway, 1990, p. 73) can render the demon incapable of deceiving us about them. But Descartes realizes that some of his premises can only be vindicated by an appeal to the 'natural light'. For instance, the principle according to which the cause has as much reality as the effect isn't indubitable.

If he is restricted to demon-proof beliefs, Descartes suspects, he cannot prove God's existence. That is why he appeals to the 'natural light'. Of course, the circularity of the appeal vitiates his reasoning.

I said Descartes wavers in his attitude to his premises. In his more optimistic moments, he thinks they are not only basic ('intuitive'), but perfectly secure, as well.

[I]ntuition is the indubitable conception of a clear and attentive mind which proceeds solely from the light of reason…*it…is not something a man can perform wrongly*. Thus everyone can mentally intuit that he exists, that he is thinking, that a triangle is bounded by just three lines.

(CSM I, p. 14, my italics)

[T]he proposition *Nothing comes from nothing* is…an eternal truth which resides within our mind… The following are [other] examples of this class: *It is impossible for the same thing to be and not to be at the same time*…and countless others.

(CSM I, p. 209, original italics)

Descartes' optimism here is, surely, misplaced. At least some of the premises invoked in the proof of God's existence ('A cause has at least as much reality as its effect', for instance) are not *certain*. (To us they do not even seem reasonable; we even have doubts about their intelligibility.) So as intuitions, lacking further support, they do not enable Descartes to attain his (ambitious) aim: (certain) knowledge.

BEGINNING WITH THE PRINCIPLE OF CLARITY AND DISTINCTNESS?

To be certain, we have seen, the premises invoked in the proof of God's existence must be justified by an appeal to the principle of clarity and distinctness. So on pain of circularity, the principle must be established before God's existence is proved. Can this be done? It might seem as if Descartes tries to do just this.

I am certain that I am a thinking thing. Do I not therefore also know what is required for my being certain about anything? In this first item of knowledge there is simply a clear and distinct perception of what I am asserting; this would not be enough to make me certain of the truth of the matter if it could ever turn out that something which I perceived with such clarity and distinctness was false. So I now seem to be able to lay it down as a general rule that whatever I perceive very clearly and distinctly is true.

(CSM II, p. 24)

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Descartes seems to be generalizing from a paradigm case—his belief in his own existence. This belief is certain and both clear and distinct. Now, the generalization from just one case of certain knowledge seems rash. There may be nothing common to all infallible beliefs; no criterion of truth. And even if there is a mark of truth, which of the *Cogito's* many features is it? 'Beginning with a "C"', perhaps? Descartes has singled out clarity and distinctness, but he could have made the wrong choice.

But perhaps Descartes is being too stringent in confining himself to the *Cogito*. Remember, Descartes' sceptic is consequent, and allows him to invoke reasonable premises in response. So are there no other things he can appeal to in establishing a criterion of truth? Can't he, for instance, rule out 'Beginning with a "C"'? There are, after all, false propositions which satisfy it ('Cats live for ever', for instance).

Unfortunately, Descartes' stringency *is* justified. If, in deriving a criterion of truth, he assumes the truth of 'Cats do not live for ever', the criterion, and beliefs subsequently established by reference to it, will not be known with certainty, even if they are reasonable. Pending a proof of God's existence, there is doubt, albeit 'metaphysical', attaching to the proposition 'Cats do not live for ever'. So Descartes is restricted to the *Cogito,* and, perhaps, a few other certainties. And, of course, without God's guarantee, doubt attaches to the very existence of a mark of truth. So we must conclude that the principle of clarity and distinctness cannot be established (with the requisite degree of assurance) before God's existence is proven.

AN ALTERNATIVE DIAGNOSIS OF THE CIRCLE

> The traditional Cartesian examination aims at an assessment of all our knowledge of the world all at once, and it takes the form of a judgement on that knowledge made from what looks like a detached 'external position'…[O]nce we [adopt] that lofty…philosophical standpoint,…scepticism can come to seem inevitable.
>
> (Stroud, 1984, p. 209)

Antecedent scepticism, Hume correctly notes, cannot be vanquished. But, *pace* Hume and Stroud, this does not explain Descartes' failure.

The 'detached', 'lofty' standpoint—general doubt—is not Descartes'. His scepticism is consequent, and he accepts the need to adduce reasons for doubt and the legitimacy of invoking counter-reasons. It is Descartes' stringent conception of knowledge that dooms his project to failure. Once certainty is taken to be necessary for knowledge, we are inexorably led to conclude that there is very little we can know.

Is Descartes' failure to respond to the sceptic in a non-circular way significant? To be sure, his sceptic is consequent, and his challenge is backed with seemingly cogent arguments. But is their conclusion so unpalatable? Hume's sceptic claims to have 'discovered either the *absolute fallaciousness* of [his] mental faculties, or [his] unfitness to reach any fixed determination in all those curious subjects of speculation, about which [he is] commonly employed' (Hume, 1777, p. 150, my italics). And Descartes is never a sceptic in Hume's (radical) sense of the word. Even when he starts his inquiry he thinks his beliefs are '*highly probable…* despite the fact that they are in a sense doubtful…[I]t is still much more reasonable to believe than to deny [them]' (CSM II, p. 15, my italics). He aims to improve upon a state in which his beliefs are already reasonable. And he fails to attain what might seem to us like an epistemological luxury: certainty.

We shouldn't be lulled into complacency just yet. Descartes himself underestimates the force of his scepticism. True, he purports to cast doubt only on the possibility of knowledge, and very stringently construed at that. But the dream and demon arguments seem compelling when couched in terms of *justification*. Am I justified in believing that I am sitting at my desk if I am not justified in believing that I am not dreaming? And am I justified in this belief? There are, after all, no intrinsic signs distinguishing wakefulness from sleep!

When directed against the possibility of justification, I conclude, Descartes' sceptical challenge is a formidable one. Of course, our beliefs may be justified even if they are not certain. So we may succeed where he failed. Descartes' (inevitable) circle need not be ours.

6

INDUCTIVE SCEPTICISM

INTRODUCTION

The problem of induction, 'the scandal of philosophy' (Broad, 1952, p. 143), is supposed to have originated with Hume. Indeed, Kant terms it 'Hume's problem' (Kant, 1783, p. 10). In this chapter I shall show that this attribution is historically wrong. Hume wasn't the first to cast doubt on the form of inference we call inductive: concluding, for instance, on the basis of past sunrises that the sun will rise tomorrow (see the following section). Neither was his argument original, initial appearances notwithstanding. By considering Hume's argument and reformulating it so as to meet various objections (see pp. 67–74), we recognize it to be a special case of Sextus' problem of the criterion (see pp. 74–6). My primary interest is not historical, however. This reconstruction provides a better understanding of Hume's problem, and suggests that his (inductive) sceptic can be confronted (Chapter 8) only after we have contended with Sextus' (Chapter 7).

WHOSE PROBLEM IS IT ANYWAY?

Contrary to prevalent opinion, Hume was not the first philosopher to think induction problematic. Sextus adduces an argument, in the form of a dilemma, to 'set aside the method of induction':

> [W]hen they propose to establish the universal from the particulars by means of induction, they will effect this by a review either of all or of some of the particular instances. But if they review some, the induction will be insecure, since some of the particulars omitted in the induction may contravene the

universal; while if they are to review all, they will be toiling at the impossible, since the particulars are infinite and indefinite.

<div align="right">(PH, p. 283)</div>

Sextus anticipates Hume in raising sceptical doubts about induction. But Hume adduces a different argument from Sextus, and his conclusion is more restricted. It pertains only to our attempt to infer a regularity on the basis of a *partial* survey of positive instances. Unlike Sextus, Hume says nothing about the (im)possibility of induction by a *complete* enumeration of instances. On the other hand, Hume's argument is more sophisticated and persuasive. To show that induction 'is insecure', Sextus invokes the fact that the universal rule can (logically) be false even when the particulars examined conform to it. Hume's argument is sometimes interpreted in this way, but this is a misrepresentation. His much more intricate argument is the following:

> (1) Concerning matter (sic) of fact and existence…there are no demonstrative arguments…since it implies no contradiction that the course of nature may change… and the trees will flourish in December…(2) If we be, therefore, engaged by arguments to put trust in past experience…these arguments must be probable only… (3) [A]ll arguments concerning existence are founded on the relation of cause and effect…(4) [O]ur knowledge of that relation is derived entirely from experience…(5) [A]ll our experimental conclusions proceed upon the supposition that the future will be conformable to the past. (6) To endeavour, therefore, the proof of this last supposition by probable arguments, or arguments regarding existence, must be evidently going in a circle, and taking that for granted, which is the very point in question.
>
> <div align="right">(Hume, 1777, pp. 35–6)</div>

Some of the argument's (many) premises (which I have numbered) may be questioned. When the argument is reformulated so as to withstand the objections (see pp. 67–74), its provenance can be better assessed (see pp. 74–6).

THE FIRST PREMISE

Does it really 'impl[y] no contradiction that the course of nature may change…and the trees will flourish in December'? Hume's reason for so thinking is that '[w]e can at least conceive a change in the course of nature;

which sufficiently proves, that such a change is not absolutely impossible' (1739, p. 89). Whatever we can conceive, Hume argues, is logically possible.

How are we to construe the conceivability which is supposed to be a criterion of logical possibility? If conceiving is 'entertaining in one's mind', then the criterion looks too liberal. It seems we can entertain both Goldbach's conjecture and its negation, yet one of them is impossible (Kneale, 1949, pp. 79–80). So even if Hume can conceive of trees flourishing in December, that does not show that it is logically possible for them to do so.

Perhaps, then, we are mistaken in thinking we can entertain both Goldbach's conjecture and its negation. But then, '"conceivable" is taken to mean "non-contradictory", [and]…no non-circular test for contradictoriness…or possibility has been given' (Stroud, 1977, p. 50), and Hume's claim that trees may flourish in December will not have been defended.

The inadequacy of Hume's criterion doesn't vitiate his argument. We don't need a *general* test of logical possibility to defend Hume's (eminently plausible) claim that a tree could (logically) flourish in December. It might be objected that anything that flourished in December wouldn't be a tree. But this ploy is both implausible and ineffectual (von Wright, 1965, pp. 48–50). True, what we would describe as a tree flourishing in December would have to be described—in the face of this manoeuvre—as something that *looked* like a tree and flourished in December. But now the argument can proceed much as before in terms of what things look like. There are no demonstrative arguments from past appearances to future ones: even if things have never looked as if trees flourished in December, it is logically possible that they suddenly should.

Admittedly, this new argument is directed at our inferring from the past to the future, rather than at our attempt to reason about matters of 'fact and existence'. But the latter is also impugned. If not flourishing in December is necessary for being a tree, there are certainly no demonstrative arguments to show that there will be trees in December, or, indeed, that there is one now in front of me.

THE SECOND PREMISE

Must we argue inductively if we are to put trust in past experience? Popper (1972) claims that induction is dispensable, science progressing by formulating bold hypotheses and subjecting them to

rigorous tests. The only mode of reasoning we require, Popper argues, is deduction. Predictions are deductively derived from hypotheses, and when observationally refuted, the theory is *deductively* falsified: if T?E, then ~E?~T.

This attempt to take the sting out of Hume's argument has been cogently rebutted (Newton-Smith, 1981, ch. III). Inductive considerations must be invoked in locating a theory's Achilles' heel, so as to determine whether a test is really severe. A theory's prediction is more vulnerable the less likely it is. But judgements about likelihood are based on inductive reasoning. Suppose I predict that the sun will from now on rise only on Tuesdays. Watching out for the sun on Wednesday morning (but not on Tuesday morning) is a severe test, because my prediction about that day contravenes the supposition that the sun rises daily. And this supposition is *inductively* based: inferred from past sunrises.

Induction also comes in at a later stage in Popper's scientific inquiry. We do not rest content with having 'on our books' a theory which has withstood rigorous attempts of refutation. If we rely on theories in action, we must at least believe (if only tentatively) their empirical consequences to be (very probably) true. And, similarly, we can only partake of our theories' non-practical virtues (verisimilitude, explanatory strength) if we take them to be (probably) true. But unlike the inference from the existence of a falsifying instance to the falsity of the theory, the inference from a theory's having withstood rigorous tests to its continuing to do so is not deductively valid. And it is the latter which is required to ground our (practical) reliance on the theory.

To sum up: Hume's claim that we essentially depend on induction, both in science and in everyday life, can be endorsed.

THE THIRD PREMISE

Doubts arise about Hume's claim that 'all arguments concerning existence are founded on the relation of cause and effect'. The inference from past sunrises to the next one isn't causal: today's sunrise does not cause tomorrow's in the way one billiard ball causes another to move.

The objection can be met. So long as 'experimental conclusions' rely on *some* principle which 'is derived entirely from experience', the argument can proceed in much the same way. Hume himself (in the fifth premise) suggests that it is the similarity of the future to the past which is presupposed when we reason non-deductively. And this

principle seems to figure in, and render Hume's argument applicable to, the inference from past sunrises to tomorrow's sunrise.

We still haven't got an argument against the justifiability of the inference from a sample (of black ravens, say) to the entire population: the inference is not only non-causal; it relates *contemporaneous* occurrences (facts). But an analogous argument can be constructed by supposing that this inference relies on the principle of uniformity of nature. In fact, Hume himself views the causal principle, 'Like cause—like effect', as a special case of the uniformity of nature (Hume, 1777, p. 36).

Thus construed, Hume's argument is independent of what may seem to be objectionable about his view of causality. According to Hume (1739, p. 172), the statement 'A caused B' is to be analysed (without remainder) into three components: (1) A preceded B; (2) A and B are spatio-temporally contiguous, (3) events of type A are regularly accompanied by events of type B. What is notably, and to many minds implausibly, lacking in Hume's analysis is some *connection* between cause and effect. But any tenable way of remedying the deficiency will not (*pace* Armstrong, 1983; Dretske, 1977) impugn Hume's argument against non-demonstrative reasoning. For Hume would still find 'nothing in any object, consider'd in itself, which can afford us a reason for drawing a conclusion beyond it' (1739, p. 139). This claim can be defended as follows.

Suppose the content of the statement 'The stone broke the window pane' is not exhausted by Hume's triad, but entails some further condition, C. ('Had the stone not been thrown, the window would not have broken' is the missing condition according to Lewis (1973).) Suppose, further, that C can be directly perceived in particular causal interactions. How do we infer—on the basis of observed instances of panes being broken by stones—that if this stone is thrown at the window, the pane will break? The impact and causal interaction is *subsequent* to the stone being thrown at the window, and (as yet) unobservable. We must infer from past occasions in which a stone is seen to cause a window to break that the *next* stone we will throw will also cause the window to break. And this inference is inductive: from observed causal interactions to future ones.

This is why Hume can allow his opponent to suppose 'that the production of one object by another in any one instance implies a power' (1739, p. 91). He thinks the supposition is unfounded, since

'our senses [only] shew us…two bodies, or motions, or qualities in certain relations of succession and contiguity' (1739, p. 88). But whether it is unfounded or not, the supposition will not block the sceptical conclusion: the 'appeal to past experience [of powers] decides nothing in the *present* case' (1739, p. 91, my italics). This claim does not rely on Hume's contentious view of causality. It (almost unobjectionably to modern readers) assumes that the law of causality isn't a logical truth. And this claim is made even more plausible when causality is understood robustly, as involving more than mere regularity.

The argument is still not general enough: it does not apply to conclusions based on inference to the best explanation, at least when the *explanans* is about the unobservable. When we infer at night the presence of a flower from its scent, we are applying the maxim 'Like effect—like cause' to an observed regularity: we can see and smell the flower during the day. But inference to the best explanation involving *unobservable* phenomena is different. Gravity best explains the tides, but isn't observed to be regularly conjoined with them. Now, Hume thinks the inference in the latter case is actually *impossible;* one cannot even entertain such conclusions.

> Any degree…of regularity in our perceptions, can never be a foundation for us to infer a greater degree of regularity in some objects, which are not perceiv'd; since this supposes a contradiction, viz. a habit acquir'd by what was never present to the mind.
>
> (1739, p. 197, italics omitted)

But one who thinks propositions about the unobservable *are* meaningful may wish to apply Humean considerations to show that belief in them is unwarranted. The principle invoked in their derivation, according to which nature is amenable to explanation, cannot be justified (van Fraassen, 1980).

Again, the requisite modification to the argument is easily forthcoming. To be sure, the justification of the principle of intelligibility isn't straightforwardly circular: the supposition that the world is intelligible doesn't explain anything. So Hume's sixth premise won't straightforwardly apply. But if, more plausibly, the principle of intelligibility is defended by an appeal to its past success (Laudan, 1987), this would be induction by enumeration—invoking

the principle of uniformity—at the level of inductive principles—and its warrant is threatened by Hume's original argument.

THE FOURTH AND FIFTH PREMISES

'[A]ll our experimental conclusions proceed upon the supposition that the future will be conformable to the past', Hume claims. And our knowledge of that supposition is empirical. To assess these premises, the principle of uniformity must be more precisely articulated.

We cannot construe the principle of nature's uniformity as merely requiring that there be *some* uniformities. This reading renders it too weak, and deprives it of the significance it is supposed to have as 'the supposition' upon which 'all our experimental conclusions proceed'. Mill notes the problem. The universe', he thinks, 'so far as is known to us, is so constituted, that whatever is true in any one case, is true in all cases of a certain description; *the…difficulty* is to find out *what description*' (Mill, 1884, bk III, ch. III, s. 1, my italics). The weak principle of uniformity doesn't solve this difficulty. It tells us that nature is uniform in some respects without saying what they are: is the colour of ravens one of them? Without an answer to this question, no inference will be forthcoming about hitherto unobserved ravens on the basis of the black ones we have observed.

I noted in the previous section that not all inductive reasoning is causal, and that Hume's causal principle should be replaced with a principle of uniformity, which is applicable more generally. But we now have grounds for doubting the significance of Kant's principle of causality even for 'causal reasoning'. Kant attempts to show that every event has a cause. But this principle is not strong enough to engender predictions. It doesn't, for instance, tell us what to predict—given our experience of stones regularly breaking panes—when we are about to throw a stone at a window. It guarantees that both possible outcomes, the pane breaking and failing to break, will not be without a cause. But it doesn't tell us which of the two to expect. Hume's causal principle, 'Like cause—like effect', is not vulnerable to this objection. From the fact that stones caused windows to break, in conjunction with Hume's causal principle, we can infer that the next one will.

Let us now return to the principle of uniformity. The principle guaranteeing the existence of uniformities, we saw, is insufficient to engender predictions. The difficulty would be solved if we could assume

nature to be uniform in all respects: having observed an A which is a B, we should expect all As to be Bs. But of course this principle is false: some days are rainy, and on others the sun shines.

The principle of absolute uniformity enables us to justify *any* (singular) prediction on the basis of some observed regularity (Goodman, 1973, ch. 3). Consider the prediction, P (made at t), that the next emerald will be blue. Define the predicate *grue* to apply to an object (at t) if it is green and $t < t_0$, or if it is blue and $t > t_0$. If nature is uniform with respect to grueness, all the emeralds hitherto observed will confirm—via the principle of uniformity—the generalization 'All emeralds are grue', which entails P. The (unrestricted) principle of uniformity can be made to 'confirm' other (incompatible) predictions about the colour of the emerald by the introduction of judiciously invented predicates.

We require a principle of uniformity which is strong enough to ratify precisely those predictions which we are willing to countenance. We need, in Goodman's (1973, ch. 3) words, to be told which generalizations are 'lawlike' ('Copper conducts electricity'), and which are 'accidental' ('All men in this room are third sons'). The former, but not the latter, are lent credence by their observed positive instances.

In order adequately to characterize the principle of uniformity it isn't enough to distinguish (Barker and Achinstein, 1960; Blackburn, 1973; Goodman, 1973) between legitimate ('projectible') and illegitimate predicates. The extent to which a generalization is confirmed by its positive instances depends not just on *which* predicates figure in it, but also on how they 'fit together'. The hypotheses 'Tall people have large feet' and 'Intelligent people learn languages rather quickly' are lawlike, whereas 'Tall people have a good memory' and 'Intelligent people have large feet' are (more) accidental. Being intelligent 'goes together' with a facility for languages, and that is why we will be easily persuaded by positive instances, whereas it will take many more to persuade us that intelligent people have large feet. The situation is reversed when we couple the predicate 'is tall' with the predicates 'has large feet' and 'acquires languages rather quickly', respectively.[1]

We must also take cognizance of the fact that unlike its deductive counterpart, inductive validity admits of gradations: some inductively based conclusions are better supported than others. Finally, what we learn from experience, *inter alia,* is *how to*

learn from experience. Scientific progress changes the way we classify things into natural kinds (Quine, 1976), and, correlatively, our judging hypotheses as (more or less) lawlike and susceptible to confirmation.

What of Hume's fifth premise? It is perhaps not very auspicious to think of induction as invoking a supreme 'supposition'. The problem is not that we cannot (as yet) explicitly characterize nature's uniformity in the light of the above considerations. Rather, such considerations are better viewed as constraining an *inference principle* rather than a supposition or premise we (implicitly) invoke in particular inductive inferences. This would make induction analogous to deduction, where the need for inference rules in addition to logical truths was brought to our attention by Lewis Carroll (1895). Hume's fifth premise should, of course, be modified accordingly. Our experimental conclusions, it will now assert, are the result of applying an inductive principle, whose justification must be 'derived entirely from experience'.

THE ARGUMENT NEWLY COUCHED

We can now formulate a sceptical argument against induction which is invulnerable to many of the objections that have been levelled against Hume's. Its premises are the following: (1) the justification of our inductive practice must be 'derived entirely from experience'; (2) such justification is circular; and, (3) therefore, illicit.

This argument against induction, distilled from Hume, is quite different from Sextus' (see pp. 66–7). Whereas Sextus thinks the logical gap between premises and conclusion suffices to vitiate the inference, Hume explicitly distinguishes between the two modes of reasoning, induction and deduction, and says that 'future judgement' must be based on 'arguments [which are] probable only,…according to the division' between 'demonstrative reasoning…[and] that concerning matter (sic) of fact and existence' (Hume, 1777, p. 35). He faults induction having eliminated purported ways of justifying it, and not because its premises may be true while the conclusion is false.

Hume's argument has often been misinterpreted by sympathizers and critics alike. His scepticism about induction is sometimes construed so that it becomes but an echo of Sextus'. Consider some followers first. According to Lipton, it is 'the *underdetermination* of our inferences by our evidence [which] provides the skeptic with his

lever' (Lipton, 1991, p. 22, my italics). In a similar vein, Popper suggests that 'any conclusion drawn in this [inductive] way *may* always turn out to be false: no matter how many instances of white swans we may have observed, this does not justify the conclusion that all swans are white' (Popper, 1972, p. 27, my italics).

Now to some of Hume's critics who impute his inductive scepticism to an assessment of induction by reference to deductive standards. Thus, Edwards claims that 'Hume…means by "reason" a *logically conclusive* reason and by "evidence" *deductively conclusive* evidence' (Edwards, 1949, p. 151, original italics). He goes on to suggest that a different standard must be used when assessing empirical predictions: '"reason" is not used in this sense when, in science or in ordinary life, people claim to have a reason for a prediction'. Thus, Edwards concludes that Hume's inductive scepticism is ill founded.

In the same way, Strawson points to the absurdity of 'the demand that induction shall be shown to be really a kind of deduction' (1952, p. 250). And Hume's scepticism, he claims, wrongly assumes that the demand is a reasonable one.

The criticism offered by Edwards and Strawson is appropriate if directed against Sextus. For he *does* rest content with pointing out that 'some of the particulars omitted in the induction may contravene the universal'; in effect complaining that induction isn't deduction. But Hume, we have seen, has quite a different reason for doubting induction.

Is Hume's argument against induction original? If he wasn't the first to cast doubt on it, did he at least provide new grounds for so doing? The answer is, no. Admittedly, Hume's argument is different from the one Sextus adduces *explicitly* against induction. But it is special case of Sextus' problem of the criterion (see pp. 33–4). Hume assumes that induction can be justified only inductively, and faults the purported justification because of its circularity. But the problem of the criterion is perfectly general: it applies to deduction as well. How is deduction to be justified? How do we know, for instance, that *modus ponens* is a truth-preserving rule of inference? If in the soundness proof for the prepositional calculus we invoke *modus ponens*, the justification is circular, and if we use some other rule, how is *it* to be justified? If by reference to *modus ponens,* the justification will, again, be circular; if by reference to yet another rule, we will have embarked upon a regress which must be either infinite or circular, and in both cases improper.

Hume doesn't present this argument against deduction (although he does adduce others). This must be because he does not believe the criterion argument is cogent in general: it may be deployed against induction, but not against deduction.

While rejecting Hume's scepticism about induction, many philosophers nonetheless (implicitly) assume *with him* that non-empirical assumptions can be justified. Kant attempts to show that the principle of causality, which he thinks is a presupposition of induction, is a priori, deducible from very minimal assumptions about the nature of experience. But of course the principle of causality will only ground induction if it is itself justified.

Carnap (1950) views prior probability relations as truths of logic. Carnap's c† confirmation function is (deductively) shown by Horwich (1982) to be 'demonstrably reliable', well calibrated. That is, among the propositions assigned probability x, the relative frequency of true propositions is x. If these are desirable features, he will have justified Carnap's function deductively. Reichenbach's (1949, s. 87) justification of induction invokes the claim—supposedly an a priori truth—that induction will succeed if any method will. Popper, persuaded by Hume's inductive scepticism, thinks scientific reasoning is rational because purely deductive: proceeding by rigorously testing hypotheses and rejecting those that have been refuted.

These claims—purported deductive groundings of induction—may be contested, but their significance should first be considered. Such (deductive) justifications presuppose the legitimacy of deduction, an unwarranted assumption pending a rebuttal of the criterion argument (in its full generality).

Scepticism about induction is, certainly, more compelling psychologically. Despite his sceptical arguments, one is inclined to find Hume's scepticism about deduction half-hearted. It is not presented in the *Enquiries,* the work Hume 'desires…may alone be regarded as containing his philosophical sentiments and principles' (1777, advertisement).

Confronting the criterion argument is one way of seeing whether or not this inclination is a mere bias. If it is, we may discover that scepticism is warranted with respect to both. But, equally, we might see how induction can be vindicated.

7

SCEPTICISM AND THE STRUCTURE OF JUSTIFICATION

INTRODUCTION

Having presented the sceptical challenge, it is now time to embark on the constructive task of responding to it. I begin by considering one of the most important sceptical arguments: the problem of the criterion.

Sextus uses the problem of the criterion in combination with the opposition of contrary appearances:

> Since [there is] a great divergency in the sense-impressions, …we shall be compelled…to end up in suspension of judgement. For…anyone who purposes to give the preference to any of these impressions will be attempting the impossible. For if he shall deliver his judgement simply and without proof, he will be discredited; and should he, on the other hand, desire to adduce proof, he will confute himself if he says that the proof is false, while if he asserts that the proof is true, he will be asked for a proof of its truth, and again for a proof of this latter proof, since it also must be true, and so on *ad infinitum*.
>
> (PH, p. 73)

There are two ways in which Sextus' argument can be improved. First, its scope could be increased. Sextus thinks the criterion argument can only be used to undermine beliefs about reality: when 'we question whether the underlying object is such as it appears, we grant the fact that it appears' (PH, p. 15). This is because he thinks a criterion is only required when we have conflicting evidence. The opposing appearances of the tower, for instance, support incompatible judgements about its real shape, and a criterion is required to decide

which one is veridical. There is, on the other hand, no divergent evidence as to how the tower *appears* (to me now); no need, therefore, for a criterion to ground my belief that it appears thus and so.

Sextus' use of the problem of the criterion is needlessly restricted. The existence of a consensus about a proposition does not mean that our (unanimous) belief is justified without a criterion. If the tower appeared the same from all angles to everyone, we would be relying on that fact in supposing that the apparent shape is also the true one. And consensus as a criterion of truth is vulnerable to the same trilemma: it is either dogmatic, circular, or leads to an infinite regress. I propose, therefore, to construe the criterion argument as applicable globally.

This unrestricted application of the criterion problem is not congenial to Sextus. He wouldn't wield the argument to cast doubt on appearances even if he thought this application valid, because that would be pointless from the point of view of attaining 'mental suspense and…a sense of "unperturbedness" or quietude' (PH, p. 7). Since 'sense-presentation…lies in feeling and *involuntary* affection' (PH, p. 17, my italics), the argument—no matter how cogent—would be psychologically ineffectual. But, of course, this is not an epistemological consideration.

On my construal, the problem of the criterion induces a very radical form of scepticism. It purports to show that no belief—no matter what its content—can be justified. It yields an equally pessimistic verdict about inference rules, rendering ineffectual Feigl's (1950) distinction between validation and vindication. Ultimate rules, Feigl argues, cannot be validated; defended by reference to more ultimate rules. But we can vindicate them: we can show that they will accomplish the purpose for which they are adopted. But the vindication of an inference rule avoids dogmatism, the sceptic will claim, only by appealing to another belief (about the utility of the principle). The problem of the criterion is, thus, transmitted to inference rules.

We now come to the second way in which the criterion argument should be modified, a way suggested by modern sceptics (Oakley, 1976). There are three ways in which beliefs can be structured, these sceptics argue, and they are all improper: infinite (non-terminating) chains of justification, chains of justification which terminate, and those which circle back upon themselves. This modern version of the sceptical trilemma is more persuasive, because it is plausible to suppose that beliefs are

justified by other beliefs, and not by criteria of truth. At least, it is not easy to formulate criteria by reference to which a belief in a scientific theory (say) is justified, whereas it is quite straightforward to cite other beliefs (evidence) which might be taken to warrant it.

To meet this hoary sceptical challenge I shall consider various arguments which jointly constitute the criterion problem (see pp. 80–7). There are no good reasons, I shall conclude, for thinking that any of the three possibilities denied by the sceptic are *intrinsically* (conceptually) impossible. Nor can any but one be ruled out on contingent grounds.

TERMINOLOGY

I use the terms 'groundless' (M.Williams, 1977) to denote the justification which accrues to a belief in virtue of its being embedded in a non-terminating chain of beliefs. I shall call a justified belief *basic* or *immediate* if its justification is not given (even partly) by other (justified) beliefs. Weak foundationalists (Alston, 1989) apply the term to beliefs which are justified—at least partly—non-inferentially. I have two reasons for adopting a stronger construal of the term 'basic'. The first is strategic. To show the legitimacy of terminating chains of justification we need strongly basic beliefs. A belief which terminates a chain of justification must be *completely* justified non-inferentially if it is to be capable of adequately justifying the other elements in the chain. The second reason for my terminology is that some of our beliefs are basic in the strong sense (see the following section).

When discussing the (im)propriety of chains of justification, it is important to be clear about their constituents. It is often supposed that justification can be ascribed to propositions (rather than beliefs), since it only depends on the *content* of a belief. A foundationalist may, thus, hold that propositions concerning sense-perception are immediately justified, and can justify propositions about external objects. Michael Williams (1991, p. 113) has objected that epistemological priority is context-dependent, and cannot be ascribed to propositions. The claim is plausible, although not for the reasons Williams cites.

To be sure, '*demands* for justification are raised and responded to against a background of specifically relevant error possibilities', and 'what is relevant will depend on…the context in which the claim is *entered*' (M.Williams, 1991, p. 113, my italics). But this context-

dependence pertains to the *activity* of justifying, and not to the *state* of being justified. And if in a particular context some belief does not have to be justified, still, the beliefs that would be cited in its justification in another context are available and provide it with justification even though none is demanded. So these facts about the activity of justification do not show that justification—as a property of beliefs—is context-dependent.

To see that the *state* of justification is context-dependent, consider the proposition, 'It is now cloudy'. Note that belief in it may be basic for an agent when he is outdoors, and inferential when he isn't. So it is a person's believing at t some proposition, rather than the proposition itself, that can be (un)justified.

Williams claims that recognizing the context-dependence of justification is the only way to defeat the sceptic. But this seems doubtful. For once the structure of justification is (contextually) determined, the question still arises as to whether the (more) basic propositions do, in that context, warrant the others. And from the fact that in another context the direction of justification will be reversed nothing follows about the extent to which justification is attainable in either context. Of course, to see whether context-dependence *is* crucial in the dispute over scepticism we have to consider whether sceptical arguments rely on the existence of a context-independent epistemological hierarchy. Those considered in this chapter certainly do not.

ARGUMENTS AGAINST TERMINATING CHAINS OF JUSTIFICATION

Michael Williams (1977) claims that justification can only be conferred by another belief, since only beliefs possess justification. But the argument is specious (Van Cleve, 1985). *If* a belief is justified by having justification *transferred* to it, then, of course, it must be justified by another belief. But justification isn't to be thought of as a relay race, the proponent of basic beliefs will contend. If a belief is justified in virtue of its reliable acquisition, for instance, it doesn't *receive* justification from that fact. Physical features, he may draw an analogy, do not transmit mentality (which they do not possess) to mental facts (if any) which obtain in virtue of them.

The objector to foundationalism will deny that a belief can be justified in virtue of non-doxastic facts such as reliable acquisition. He will appeal to an *internalist* conception of justification, which

requires 'the agent [to] have a "cognitive grasp" of whatever makes his belief justified' (Bach, 1985, p. 247). If a belief can be justified only if the agent believes it possesses the feature by virtue of which it is justified, it isn't basic: its justification depends on another belief (Bonjour, 1985, p. 32).

In support of internalism, Bonjour adduces two types of consideration. He argues first, that if the rationality of belief is akin to the morality of an action, then judgements of (ir)rationality must be made 'in the light of [the agent's] subjective conception of the situation' (Bonjour, 1985, p. 59). Three objections can be levelled against this defence of epistemological internalism.

First, it is not clear whether the analogy is apposite. The doubt isn't engendered by the fact that, unlike action, belief-formation isn't voluntary (Alston, 1989, p. 205): this would only render talk about epistemological duties and obligations inappropriate, leaving open the possibility that actions and beliefs are assessed from the same (subjective) perspective. Rather, since there are differences and similarities between belief and action, one has to be very careful when drawing epistemological conclusions from ethics, and make sure that the two cases are *relevantly* similar. And this decision, it seems, can only be made once we have determined the plausibility of epistemological internalism. So the analogy with ethics is dialectically ineffectual.

Second, if an analogy is to be drawn between beliefs and actions, the justification of beliefs would, more naturally, be assimilated to actions' rationality (rather than morality). A judgement of justification would, then, take into account the agent's beliefs, but might also depend on *their* justification. The appeal to the analogy with action in defence of epistemic internalism is, therefore, otiose.

Third, even if the justification of belief is modelled after the morality of action, the desired conclusion doesn't follow, because the corresponding (internalist) view in ethics isn't credible. Ought we to judge equally moral two doxastically identical agents who perform the same action? Are their beliefs the only morally relevant facts? Surely not. An agent is blameworthy who suppresses from consciousness a belief about his action's consequences to facilitate acting immorally. It may be even more reprehensible to act in the *conscious* knowledge that one is harming another, but an agent is completely absolved from moral blame only if he is innocently ignorant of the (harmful) effect of his action.

Bonjour also adduces (1985, ch. 3) examples which purport to undermine externalism, but they do not succeed. Consider the clairvoyant agent who believes against the evidence that he is clairvoyant. This agent is intuitively unjustified in his (reliably acquired) clairvoyant beliefs, and this suffices to vitiate reliabilism: the view according to which a belief is justified if and only if it is acquired by a reliable method. But the example does not show that every belief must be (partly) justified by a belief about what renders it justified (the reliability of its acquisition in this case). Let me elaborate.

We cannot demand that *any* justified belief, p, be accompanied by a (justified) belief in 'p is justified by X': that would engender an implausibly strong requirement that the agent believe an infinity of increasingly complex propositions: p, 'p is justified by X' '"p is justified by X" is justified by Y', etc. We *do* require the agent to believe 'p is justified by X' *if* he can entertain it.

We should distinguish the agent who believes *against* reasons from one who believes *without* reasons. The former is unjustified, because, as Bonjour plausibly claims, one shouldn't believe 'things to which one has, *to one's knowledge,* no reliable means of epistemic access' (1985, p. 42, my italics). But an (unsophisticated) agent who doesn't have the concept of reliability or any theoretical concepts pertaining to perception may be justified in his reliably acquired perceptual belief even if he has no ground for it, having no beliefs about the reliability of methods.

The point isn't simply that he doesn't have such justifying beliefs consciously, or has never explicitly invoked them to justify his perceptual beliefs (Alston, 1989, p. 335). Nor is it merely that he can be rational without being reflective. Everybody must allow justifying beliefs to be (sometimes) merely dispositional. But in allowing justification chains to terminate we go much further: we deny that justifying beliefs are always required. Of course, we must acknowledge (in the light of such examples) that a belief may be basic for one agent and mediate for another, and its status will depend on the concepts the agent possesses, and the propositions he is capable of entertaining.

We can now respond to Sextus' claim that a basic belief is dogmatic, a mere 'assumption'. When one believes dogmatically, one's belief lacks a reason it *could* have had. Thus, if I believe that the number of hairs on my head is odd, I believe dogmatically: I

know I haven't counted them, for instance. But if, as is surely plausible, nothing—no proposition I can entertain—counts as a reason for the law of contradiction, my belief in it isn't dogmatic, although it is unsupported.

AN ARGUMENT AGAINST INFINITE JUSTIFICATION CHAINS

Foley (1978) argues that to be legitimate, an infinite chain of justification,…r, q, p…, must be accompanied by a belief in an infinite proposition: 'p is justified by q, which is justified by r,…'. Since we cannot even comprehend an infinite proposition, the argument proceeds, a human chain of justification must be finite.

Why isn't the infinite justification chain genuine in the absence of the belief in the infinite proposition? Consider, first, the simple case in which one belief justifies another. If q is to justify p, Foley argues, the agent must believe 'p is justified by q'. He might, otherwise, 'believe that q justifies p in a manner that is radically mistaken'. For example, 'he might justifiably believe q, but believe p only because he believes the occurrence of p would promote his happiness and he has been told by a tea-reader that if q occurs, anything which will promote his happiness will also occur' (Foley, 1978, pp. 313–14). The argument can be generalized. A three-membered chain of justification, p, q, r, must be bolstered by the belief 'r justifies p through q', lest the agent believe p 'for the wrong reason', and so on. A belief in the infinite proposition, 'p is justified by q, which is justified by r,…' is required to sustain the infinite justification chain,…r, q, p…

What does Foley's example show? The agent he describes is, to be sure, unjustified in believing p. But he isn't rendered justified when merely assumed to believe in 'q justifies p' (call this belief z). To be justified, he must believe p *because* he believes q. But we cannot ensure the satisfaction of this causal condition by requiring the agent to have further beliefs. Indeed, each new belief brings in its wake further causal requirements on justification: the agent must believe p *because* he believes z.

It is plausible to require that the agent believe z *if* he can comprehend it. If he disbelieves it, or is even agnostic about it, his belief in p is inferentially inadequate. But since he cannot understand the infinite proposition 'p is justified by q, which is justified by r …',

the justification of p doesn't require that he believe it. So we do not have here a reason for ruling out infinite justification chains—even for human agents.

A SECOND ARGUMENT AGAINST INFINITE JUSTIFICATION CHAINS

In order for r to be justified, the objector argues, it must be inferred from q. But if q is to justify r, it must first be inferred from p, and so on *ad infinitum*. To be justified in a belief, the agent must perform an infinity of inferential moves, and this is impossible.

As it stands, the argument isn't cogent, because the premise is fallacious. Justification doesn't require the agent to make infinitely many inferences. He should just *have* those justifying beliefs. The dispute about the structure of justification is, after all, *normative,* and not *genetic* (psychological). Some beliefs must, of course, be acquired directly (non-inferentially): an infinite regress of belief acquisition is vicious (at least for humans). But that says nothing about their justification once acquired. A non-basic belief, even if acquired directly, may be (partly) justified by other beliefs.

Still, the really serious objection to infinite justification chains is that they require infinitely many beliefs, and this seems humanly impossible even if these are dispositional (rather than occurrent). Arguably, we do not even comprehend an infinity of propositions, let alone have an infinity of beliefs. To be sure, I believe the proposition (n) (n+1>n), but I only believe a finite number of its instances: the others are too complex. We must conclude that we have a (contingent) reason for ruling out infinite chains of justification.

ARGUMENTS AGAINST CIRCULAR JUSTIFICATION CHAINS

Circular justification is alleged to be illegitimate because the premise must ('already') be justified if it is to justify a conclusion. There is, it is then claimed, an absurdity—a violation of the asymmetry of the temporal order—involved in two beliefs mutually supporting (possibly indirectly) one another.

There are, in fact, two reasons for thinking that the relationship of justification is temporal, but they are both specious. First, it might be

thought that the justification of one belief, p, by another, q, requires that p be *caused* by q. If this is so, the asymmetry of causality induces an asymmetry in justification, thus ruling out circular chains (Audi, 1988; Kornblith, 1980).

The assumption is fallacious. Admittedly, it is not sufficient for p to be justified by q that the agent believe both, and that p justify q 'objectively' (constitute a ground for it). But to ensure that p be believed *because* q is, it suffices that the agent wouldn't—*ceteris paribus*—believe p unless he believed q. Unlike the causal relation, this counterfactual relation is not asymmetrical: it obtains, for instance, between my two beliefs 'It is Monday today' and 'It was Sunday yesterday'.

The second (bad) reason for thinking that justification involves temporal priority conflates persuasion and justification. If one is to argue from a premise, p, to another, q, p must (already) be accepted by one's interlocutor. One cannot, therefore, *argue* in a circle. But a circular chain of justification (even a small one) can be virtuous, as the following example shows. Suppose one witness to a crime testifies that the suspect had dark eyes, while another reports that his hair was dark. Denote by p and q respectively my beliefs 'The criminal has dark eyes' and 'The criminal has dark hair'. Since dark hair often goes together with dark eyes and vice versa, p and q provide some justification for one another.

IS GROUNDLESS JUSTIFICATION TRIVIAL?

Infinite justification chains are improper, it has been argued (Oakley, 1976), because any proposition, p, can be embedded in an infinite chain, each of whose members provides its successor with the strongest (deductive) justification: ..., $p \wedge q \wedge r$, $p \wedge q$, p. Circular justification is, similarly, alleged to render justification all too easy. If we allow circular justification chains, won't any belief be 'justified' by itself?

Is groundless justification trivial? The answer is 'Not necessarily'. To begin with, we have already (see pp. 83–4) seen that a belief, q, isn't always justified by another, p, which entails it: q must be believed *because* p is. Furthermore, even if the causal requirement is met in the above infinite chain of beliefs, the belief in p will be justified by the belief in $p \wedge q$ only if the latter is justified, and similarly for the other members of the chain. Thus, we have an infinite series of

hypothetical justifications, quite consistent with no member of the chain being actually justified.

Groundless justification will be trivial if *every* non-terminating justification chain is allowed, but it may be more demanding. For instance, it might be thought that only justification chains which form (part of) a coherent whole are legitimate, or that the only admissible circular chains of justification are those which are large enough. I am, in fact, sceptical about the possibility of defining justification, and don't accept these claims. But I can use them to show that there is nothing in the *idea* of groundless justification to render it trivial. But anyway, no definition of justification is required to show that in admitting the legitimacy of non-terminating justification chains, we are merely acknowledging that *some* are genuine. Even if we cannot define the term 'game', not every activity is a game.

IS TERMINATING JUSTIFICATION TRIVIAL?

Sextus mocks the supposition that we can 'assume as granted and without demonstration some postulate. For if the author of the hypothesis is worthy of credence, we shall be no less worthy of credence every time that we make the opposite hypothesis' (PH, p. 99). But this argument can now be very briefly dismissed. Like the other varieties, terminating justification can be discriminating. Allowing some chains of justification to terminate does not commit us to allowing all of them to terminate. It is not always 'absurd to assume the subject of inquiry' (PH, p. 99). Indeed, Sextus himself restricts his scepticism to 'those [things] which are non-evident', and is willing to 'state what appears to [him]' (PH, p. 123). '[M]ental states', he affirms, 'are apprehended' (PH, p. 129).

TRIVIALITY: A RED HERRING

It isn't triviality as such that ought to worry the proponents of groundless justification. To be sure, their critics are right in assuming that justification must be discriminating. If every belief can be 'justified', none are genuinely justified. But justification is a means towards an end—verisimilitude—and could be made stringent in the wrong way: suppose one were only justified in believing what one read in fairy tales! Trivial justification is worthless because it doesn't

promote our cognitive aim. But then, even if groundless justification isn't trivial, does it have any cognitive merit?

The architectonic terminology may foster the supposition that groundless justification cannot be linked to truth. Since a building cannot float in midair, groundless belief doesn't 'touch reality' (Alan Goldman, 1988, p. 81). But the analogy is not apt. There is nothing about groundlessness as such to rule out a link between justification and truth. To see that this is so, consider the following 'definition' of justification. A belief justifies another iff it entails it, and is justified *tout court* iff it is true and inferentially justified. Of course, this isn't an adequate definition of justification. But it embodies the closest link between justification and truth (rendering all and only true beliefs justified). And all of the justification chains it countenances are non-terminating.

8

INDUCTIVE SCEPTICISM
REVISITED

INTRODUCTION

We must justify induction, Hume claims. The only justification we can provide, he adds, is inductive, and its circularity is vicious. His conclusion is that induction is unjustified.

We have seen (see pp. 74–6) that Hume applies the criterion argument *selectively:* he thinks it can be invoked against induction, but not against deduction. Having considered the criterion argument, this can be further clarified.

In analogy with belief, there are three possibilities to be considered with respect to the justification of a form of inference. First, it can be *basic:* justified, but not by reference to another. Second, its justification might be *mediate*. Third, it may be *unjustified*. By ruling out the first two possibilities with respect to induction, Hume opts for the third possibility, the sceptical one. Induction is not basic. Neither can it be justified inferentially. This is because it has no deductive justification, and an inductive justification is circular, and therefore illicit.

Having examined the problem of the criterion, we can now confront Hume's inductive scepticism. We can also consider how deduction is to be vindicated, and whether the two cases are analogous.

WHAT JUSTIFICATION ARE WE SEEKING?

Let us for the moment grant Hume's first premise, and suppose that induction must be inferentially justified. Consider the form such a justification might take.

It has been claimed that the rules of inference we employ and particular inferences we make, whether deductive or inductive, are

adequately justified by 'being brought into agreement with each other' (Goodman, 1973, p. 64), the 'agreement achieved… [being] the only justification needed for either'. This claim, I have argued (see pp. 5–6), is untenable. Justification is not valued for its own sake. We wish our beliefs to be true, and the outcomes of actions they guide to be successful. We take it that inductively derived beliefs are at least more likely to be true than randomly chosen ones. If it is not known (nor even reasonably thought) to be more reliable than any other predictive method, of what significance is its 'justification'? And why should we reason in conformity with it?

Similar considerations will undermine the attempt to vindicate induction as a paradigm case of rationality, doubt about which is a manifestation of linguistic confusion (Edwards, 1949; Strawson, 1952, ch. 9). According to Strawson, 'to call a particular belief reasonable is to apply inductive standards, just as to call a particular argument valid…is to apply deductive standards' (1952, p. 249). '[P]lacing reliance on inductive procedures… is what 'being reasonable' (1952, p. 257) *means*. 'Induction is rational' is, Strawson contends, an analytic statement.

Strawson's account of deductive validity is inadequate. The term 'valid' when applied to arguments does not mean 'conforming to our deductive practice'; it means 'truth-preserving'. And even if we do not identify 'truth-preserving' with 'valid', the important question to settle when we attempt to assess our deductive practice is whether or not our inference rules are truth-preserving. An affirmative response to *this* crucial question certainly does not follow from their being *ours:* not every rule is sound. (Consider the rule which licenses the inference of any statement from any other.)

Similarly, the pertinent question concerning induction is whether it is sufficiently often truth-preserving. It is the answer to *this* question that guides us in deciding whether or not to reason inductively, given that our aim is truth rather than social conformity. Again, this question is not trivial. This can be seen when the vague term 'induction' is replaced with a more precise specification of (non-deductive) inferences which we are willing to countenance (see pp. 72–4), predicates we are willing to project to unobserved instances, and generalizations which we consider to be lawlike (Goodman, 1973). Conforming to one inductive practice in preference to another will certainly make a difference, possibly a considerable one, to the verisimilitude of the beliefs we form. This is why the problem of

induction is a pressing one: if the choice didn't make any difference, they would all be (trivially, but unobjectionably) justified, like the choice of any tin of soup from a host of qualitatively identical tins on the supermarket shelf.

The point can be reinforced by considering the analogy Strawson draws between induction and the law. It 'makes no sense', he claims, 'to inquire in general whether…the legal system as a whole, is or is not legal. For to what legal standards are we appealing?' (1952, p. 257). Analogously, he argues, the only standard by which induction can be assessed is the one set by induction itself, and relative to which it is trivially justified.

Strawson's analogy misfires. True, there are no standards by which to judge the law illegal. But the law, being a motivated practice (designed to achieve justice, public order, etc.), must be believed to be *efficacious,* well suited to its aims, rather than merely legal, if we are to be justified in adopting it. For instance, a system incorporating a law disenfranchising women is legal, but unfair. Now, substitute 'induction' for 'the law', 'truth' for 'justice', and see how Strawson's defence of induction (and deduction) collapses.

Similar considerations will serve to vitiate attempts by Wittgensteinians to ground (some) non-deductive inferences in conceptual truths. Thus, they claim, the inference from painbehaviour to pain isn't deductive, but the premises are nonetheless (defeasible) criteria for the conclusion. It is part of what we mean by ascribing pain that pain-behaviour is evidence for pain.

Even if true, the criteriological account of pain-behaviour doesn't suffice to justify our practice. As before, the crucial question is whether or not our practice is successful. What if we choose to employ an alternative concept, 'pain*', with different ascription-criteria? If we employ it in accordance with the criteria it incorporates, will our ascriptions be true more often than our pain-ascriptions? Those who ascribe pain in the absence of painbehaviour may be violating a rule of language, but is it a language worth using?

When we ask whether the inferences our application criteria sanction are reliable, we are told that 'a criterion…is concerned with the *normal* circumstances in which the concept gets application—circumstances that give sense to the concept itself and without which we could not properly be said to have the concept' (Hamlyn,

1970, p. 71, my italics). But then we are also told that 'to say that something is normally so does not entail that it is generally so… It is always possible in special cases for what is normal to become uncommon' (Hamlyn, 1970, p. 74). This means that the criteriological response has nothing to say about the *reliability* of our practice, and is, therefore, ineffectual.

Once we recognize that induction is a goal-oriented practice, conformity to the 'canons of induction' may no longer seem constitutive of being rational. Here the analogy with the law (and its legality) seems to break down. For there is no justificatory connection between the legality of the law and its efficacy: it wouldn't stop being legal if shown to have undesirable consequences. So the claim that the law is legal may well be analytic. But one who thinks that induction isn't reliable is irrational if he adheres to it. So the claim that induction is reasonable, even when made by those who *do* believe that it is reliable, cannot be analytic.

Note that I am not offering an externalist (reliabilist) analysis of justification. I am denying the analyticity of induction's rationality by pointing out that it is contingent upon what the agent *believes* about its reliability.

CAN INDUCTION'S RELIABILITY BE SHOWN DEDUCTIVELY?

If reflective people are rational in reasoning inductively, it is *(inter alia)* because they justifiably believe that induction is reliable. The same is, of course, true with respect to deduction. But there is a difference. The reliability of deduction, but not that of induction, can be shown deductively. A soundness proof for a deductive system is a deductive proof of its reliability: a demonstration that its inference rules are truth-preserving. But it simply isn't a logical truth that our way of proceeding inductively is truth-conducive.

We cannot even show, as Reichenbach (1949) tries to, that induction will succeed *if* any other method will. If nature is uniform, Reichenbach argues, induction will enable us to make veridical predictions on the basis of our observations. If nature isn't uniform, no method will succeed. So induction, Reichenbach concludes, is demonstrably the best method, although it may be quite poor (if nature is inauspicious). The argument fails because once nature's uniformity and our inductive practice are properly

characterized, neither premise can be demonstrated deductively. There is no way of showing deductively that the properties we choose to project on unobserved cases are those in terms of which nature is uniform. For instance, we will do less well than somebody who chooses to use the concept 'grue' (Goodman, 1973) instead of our 'green' if green objects in our world turn blue come the year 2000. The greater the disparity between our predicates and those which figure in 'the book of nature', the less successful our inferences will be, and the more successful those of some rival practice.

Subjectivist Bayesians (Savage, 1954, p. 49) attempt to demonstrate the reliability of 'induction' deductively. They conceive of an inductive practice as an assignment of prior probabilities to propositions (Horwich, 1982). Such an assignment determines how the agent 'learns from experience'. He will modify the probability he attaches to any proposition by conditionalizing on evidence he acquires: having come to believe a proposition, e (assigning probability 1 to it), he will asign to any proposition, t, the prior probability it had conditional on e: $P(t/e)$.

Savage cites a (stable-estimation) theorem of the probability calculus to support his claim that induction is reliable. Accumulating evidence swamps prior probabilities. In the long run, as the agent conditionalizes on more and more evidence-propositions, his probabilities will tend to be concentrated on true propositions. Since everyone will converge on the truth, we seem to have a deductive demonstration of the reliability of our inductive practice, and indeed of any other. But do we?

Unfortunately, the answer is negative. To begin with, the theorem does nothing to vindicate our confidence about our *current* predictions: different inductive practices may greatly diverge in the short run. And in Keynes' words, we are all dead in the long run. Worse, the theorem doesn't even provide a long-run deductive vindication of induction. If we observe *all* the instances of a generalization, we will establish its truth. But this will be deduction: in induction the conclusion goes beyond the premises. And so long as our evidence is incomplete, the result established with the aid of the theorem is probabilistic, rather than categorical. 'The theorem does not tell us that in the limit any rational Bayesian will assign probability 1 to the true hypothesis and probability 0 to the rest; it only tells us that rational Bayesians are certain that he will' (Glymour,

1980, p. 73). This is because we cannot deductively infer the truth of a proposition from its having probability 1.[1]

MUST INDUCTION BE NON-EMPIRICALLY JUSTIFIED?

We must conclude that induction's reliability—even when understood fairly unambitiously—cannot be deductively demonstrated. Neither can it be shown a priori. Is this worrying? Most philosophers would reply in the affirmative. Kant seeks to show that the principle of causality, which he thinks is a presupposition of induction, is a priori (or—synonymously for Kant (1787, B4)—certain and necessary). Otherwise, he will 'have nothing but opinion, it…all [being] merely a play of the imagination, without the least relation to truth'.

Bonjour (1985, p. 10) suggests that in the justification of an 'overall standard of empirical knowledge…no empirical premises can be employed… The argument will…have to be purely *a priori*', and goes on to mount an a priori defence of the standard of justification he favours—coherence. For Lipton it is 'the underdetermination of our inferences by our evidence [which] provides the skeptic with his lever' (1991, p. 22).

The thought that the justification of induction must be non-empirical is usually informed by a suspicion that induction really isn't as trustworthy as deduction. Michael Williams suggests that scepticism is unavoidable once we admit that claims about external reality must be based on claims about experience, since it is not an analytic truth 'that experience of a certain kind constitutes good evidence for the external world's being one way rather than another' (1991, p. 55). The implication is that if claims about evidential relations *were* analytic, they, and the non-deductive inferences they underlie, would be beyond reproach.

The mistrust of the empirical is evinced by epistemologists who attempt to justify our rejection of various sceptical hypotheses (evil demon, manipulative scientist, etc.) by showing that they cannot even be entertained (Putnam, 1981, ch. 1), or that they are a priori false. If they are so much as allowed to be possible, it is feared, they will continue to haunt us for ever.

Thus, Bonjour is puzzled as to how a basic empirical belief

is able to confer justification on other beliefs…*in spite of being empirical*…. How can a *contingent, empirical* belief impart

epistemic 'motion' to other empirical beliefs unless it is itself in 'motion'? (Or, even more paradoxically, how can such a belief epistemically 'move' itself?).

(Bonjour, 1985, p. 30, my italics)

The paradox of epistemological self-moved movers seems to be confined to empirical beliefs.

Popper suggests that 'any conclusion drawn in this [inductive] way may always turn out to be false: no matter how many instances of white swans we may have observed, this does not justify the conclusion that *all* swans are white' (1972, p. 27, original italics). Because induction cannot be deductively justified, and deductive justification is, according to Popper, the only justification there is, he must expunge inductive reasoning from science. Science, he must claim, proceeds purely deductively, by rigorously testing hypotheses and rejecting those that have been (deductively) refuted.

It is extremely doubtful whether science is purely deductive (Newton-Smith, 1981, ch. III). But is its rationality vitiated if it isn't? The preference for deduction isn't principled, but seems, rather, to be a bias. To be sure, induction isn't deduction. The conclusion of a true-premised inductive argument may be false, while that of a deductively valid argument (logically) cannot. But then, good inductive arguments more often than not yield true conclusions which we cannot derive by reasoning deductively. Of course, induction's utility cannot be deductively shown, but that will impugn induction only if we assume that deductive justification is the only ultimate form of justification; that deductive rules of inference can be circularly justified if they need any justification at all, whereas inductive ones must be non-circularly justified. But this assumption may be questioned. Pending further argument, the premises of the criterion argument must be taken to be equally applicable to deductive reasoning.

THE APRIORIST URGE

It is ironic that even those (Strawson, 1952, ch. 9) who view induction as an autonomous form of reasoning, with its own standards, still insist that it be ultimately justified by reference to conceptual (or at least non-empirical) truths. If we are to justify our inductive practice, two assumptions must be shown to be true: (1) induction is justified; and (2) justification promotes truth. If the justification is to be non-empirical,

the two claims must be shown to be not merely true, but a priori. I have argued (see pp. 88–91) that this cannot be done in the case of the first claim, and that this does not play into the hands of the sceptic (see the previous section).

We may resist the 'apriorist urge' with respect to the second claim (justification promotes truth), as well. We need not suppose that our justificatory practice is the best in all possible worlds so as to justify our adherence to it in this one. To be sure, we find justification valuable because (we believe) it increases the probability of truth (see pp. 88–91). But for this—justification doesn't have to be '*necessarily* truth-conducive' (M.Williams, 1991, p. 229, my italics). It is enough for fending off scepticism that it be shown to promote truth contingently.

CAN INDUCTION BE JUSTIFIED EMPIRICALLY?

If we cannot justify induction a priori, can it be justified by appealing to its past success (Black, 1958; Braithwaite, 1968)? Of course, with respect to induction we have nothing like the rigorous proof of soundness we have for deduction. We do not even have a (reasonably) precise specification of the practice whose reliability we are trying to show (see pp. 72–4). Still, we can say in a very rough and qualitative way that our predictions have been quite successful.

Is the empirical justification of induction illicit because circular? Black (1970, p. 174) defends induction on the basis of its past success, arguing that this involves no formal circularity: it is not assumed as a premise that induction will continue to work. It is not the fallacy of *petitio principii,* but 'a genuine inference, in which a *new* belief…is acquired' (Braithwaite, 1968, p. 276). But, of course, the real worry behind the objection is that the circular justification of a rule will be trivial even if the circularity is only 'pragmatic' (Dummett, 1991) rather than formal. Now, this worry can be quelled. The requirement that an inference rule be self-supporting can easily be seen to be non-trivial. We may not succeed in justifying a rule even if we are allowed to use it. The horoscope *may* recommend that we use an alternative method of prediction. And it is quite possible that the history of science would show it to have been unreliable or less reliable than some alternative. Indeed, the sceptical meta-induction (see p. 44) purports to do just this. And its failure is *contingent*!

We are not yet home and dry. The fact that circular justification of inference rules is not trivial does not suffice to allay our worries. Consider the inductive rule, I:

Most observed As have been Bs.
Therefore, (probably): the next A will be B.

The rule I can be defended by invoking the following inductive argument, utilizing the inductive rule itself:

Most applications of I have been successful.
Therefore (probably): the next use of I will be successful.

But competing rules can be invoked in their own defence (Salmon, 1957, p. 46). Consider the anti-inductive rule, AI, which allows us to infer from 'All observed As have been Bs' that the next A will *not* be a B. We can construct an argument utilizing AI to show that AI will be successful:

Most applications of AI have not been successful.
Therefore (probably): the next use of AI will be successful.

The same situation obtains with respect to deductive rules of inference. Thus, consider the 'anti-deductive' system (ACL), in which p can be derived from q if and only if ~p can be derived from q in classical logic (CL). We can prove the soundness of this system within it, since we can prove within classical logic that ACL is *not* sound. (Contradictions can be derived within ACL.) We must conclude that *if* we are to countenance the circular justification of deduction, we cannot balk at the inductive justification of induction.

THE JUSTIFICATION OF INDUCTION

Doubts remain about the possibility of justifying induction or deduction circularly. Must we then conclude that induction is unjustified? That this is not inevitable can be seen from Hume's attitude to deduction. It cannot be justified deductively, he thinks, because a circular justification is *never* admissible: 'The same principle cannot be both the cause and effect of another' (Hume, 1739, p. 90). An inductive justification is also impossible, since induction itself, Hume thinks, cannot be justified. But since Hume does not adopt a sceptical stance towards deduction, we must conclude—by elimination—that for him, its justification is basic (in his words 'intuitive').

Why not adopt the same attitude to induction? Hume thinks our inductive practice, our 'expect[ing] a similar effect from a

[similar] cause', must be argued for. If his opponent cannot produce a justifying argument, he will thereby be 'confess[ing] that it is not reasoning which engages us to suppose the past resembling the future, and to expect similar effects from [similar] causes' (Hume, 1777, p. 39). The argument must, furthermore, be simple, so as to be accessible to 'the most ignorant and stupid peasants…even brute beasts…[who also] improve by experience' if *their* expectations are to be rational. But a basic belief or inference rule has no mediate justification—simple or 'abstruse'. The use of *modus ponens* by a child is rational, even if he cannot adduce its soundness.

What of reflective agents? We may wonder whether the question of justification even arises for animals and young children. Perhaps the term 'justified' really comes into its own in the case of agents who are reflective enough to apply it to themselves, and can entertain doubts about the warrant of their inferential practice. But induction and deduction could be basic even for such agents. The soundness proof, it might be argued, doesn't provide a 'persuasive' argument in defence of deduction, but an explanation of its role (Dummett, 1973, p. 296); of what makes it reliable. The term 'persuasive' is unfortunate. The point isn't just that the proof won't persuade someone to adopt the practice of reasoning deductively. It doesn't provide the non-sceptic with a ground for reasoning deductively, either. But the reasoning is justified nonetheless.

9

TRANSCENDENT SCEPTICISM AND INFERENCE TO THE BEST EXPLANATION

INTRODUCTION

The transcendent sceptic denies the possibility of justifying beliefs about the 'unobservable'. The scope of his scepticism will, of course, depend on where the line between the observable and the unobservable—that to which we have 'direct access' and that to which we do not—is thought to lie. For instance, the sceptic can invoke a (very influential) doctrine about the mind—the representative theory—according to which we have immediate access only to our own mental representations ('ideas'), in order to show that our beliefs about the external world are unjustified.

Smith and Jones suggest that the

> representative theory immediately raises, in a virulent form
> …sceptical worries. If, strictly speaking, what we are directly
> aware of…are ideas or impressions in our own mind…how can
> we possibly prove that there really is a physical world outside
> the mind…?
>
> (Smith and Jones, 1986, p. 88)

Similarly, Stroud is puzzled as to how philosophers have failed to 'see the sceptical consequences of [the] sense-datum thesis', the thesis that 'nobody ever does know, by direct apprehension, of the existence of anything whatever except his own acts of consciousness and the sense-data and images he directly apprehends' (Stroud, 1984, p. 105, fn 12).

Transcendent sceptical doubts will be less radical when the observable realm is extended. Even if the sceptic doesn't think our knowledge is restricted to our own perceptions, he may still deny the

possibility of knowing the theoretical propositions of physics (whose truth must be inferred from propositions reporting observations of middle-sized objects), those pertaining to other minds (which must be based on propositions reporting behaviour), and those concerning the past (which must be based on records and memories).

Transcendent scepticism can be construed (Fogelin, 1985) conceptually—as scepticism about the meaningfulness of propositions about the unobservable—or epistemically—as scepticism about their justification. I shall focus on the epistemic version, which assumes such propositions to be meaningful. My concern is epistemological, and, anyway, the plausibility of the conceptual version of scepticism is largely dependent on that of the epistemic. Verificationism, which links meaningfulness to the possibility of knowledge (justification), will brand as meaningless propositions about the unobservable only *given* the assumption that they can never be justifiably believed. The Cartesian view, according to which we can only know directly the content of our mind, threatens our ability even to *refer* to (think about) external objects, 'fails to explain how beliefs about objects can be even intelligible' (Quinton, 1973, p. 173), only if propositions about the unobservable cannot be justified.

By itself, epistemological scepticism does not engender conceptual scepticism. Even Hume, who imposes stringent conditions on meaningfulness, admits the existence of verification- (and even confirmation-) transcendent propositions. The constraint he imposes on individual terms (the requirement that a simple idea be preceded by a corresponding impression) doesn't rule out as meaningless universal statements belief in which he thinks cannot be justified.

With these clarificatory points in mind, we can now consider various arguments for transcendent scepticism. I shall try to show that they all fail.

IS THE UNOBSERVABLE EPISTEMICALLY INACCESSIBLE?

Why should inferences to the unobservable be thought illegitimate? Does the 'doctrine of the epistemological priority of experiential knowledge over knowledge of the world…disconnect our beliefs about the external world from the only evidence available to support them' (M.Williams, 1991, p. 56)?

We are 'shut off from any External World…which is… necessarily and for ever hidden behind an impenetrable Veil of Appearance…confined to an immediate awareness only of our own…consciousness', Flew claims (1986, p. 15). This is because statements about appearances do not '*entail* any conclusions about a mind-independent public world' (Flew, 1986, p. 16, my italics). Our predicament stems from the fact that 'the relation between [beliefs about objects and beliefs about impressions is] *contingent*' (Quinton, 1973, p. 173, my italics). But then this is just Sextus' sceptical argument against non-deductive reasoning (see pp. 66–7). It is based on the fact that the conclusion of a non-deductive argument may be false even while the premises are true.

If the possibility of error suffices to engender scepticism, it is pointless (even if true) to insist (Flew, 1986, pp. 32–3) that we have direct or immediate beliefs about external objects. Direct or not—such beliefs are fallible. If scepticism about the external world is engendered by 'the fact that experience could be just what it is, even if the world were very different' (M.Williams, 1991, p. 68), then it will hardly go away when we deny 'the doctrine of the epistemological priority of sense experiences' (Stroud, 1984, p. 144). The alleged obstacle to our knowledge of the external world is the fact that the world could be different from the way we believe it to be, and this remains so whether our beliefs about it are immediate, or inferred from beliefs about immediate experience.

Perhaps the words 'see' and 'perceive' are properly applicable only to physical objects (Flew, 1986, p. 37), rather than to sensedata. But this insight does not help us vindicate a fallible belief about a physical object. A sceptic who requires certainty and agrees to apply the word 'see' only to physical objects will simply doubt whether we ever see anything. If we shouldn't take 'for granted that we have a firmer grasp of what is "in" the mind than of what is outside it' (M.Williams, 1991, p. 106), the sceptic will wonder whether we have a grasp of either. Since we are *not* infallible about the external world, we could be mistaken even about our sensations. Restoring the epistemological symmetry between experiential statements and statements about the external world (or eliminating the former altogether) does nothing to eliminate scepticism which is based on a requirement of certainty.

As ineffectual against this kind of scepticism is the (plausible) claim (see pp. 79–80) that epistemological priority is context-

dependent, and not properly ascribed to propositions (M.Williams, 1991, p. 113). For this scepticism is engendered by the logical possibility of error, a possibility which the context cannot eliminate. The proper response to transcendent scepticism which is based on the requirement of certainty is simply to reject the requirement, and remind the sceptic (and ourselves) that justified belief (perhaps even knowledge) can be fallible.

Is the representationalist courting scepticism once knowledge, or at least justified belief, is allowed to be fallible and non-deductive reasoning is admitted as legitimate (Chapter 8)? 'Vertical' inferences (from sense-data to external objects, for instance) have seemed peculiarly problematic. Transcendent scepticism is, psychologically at least, much more persuasive than total scepticism about non-deductive reasoning and fallible belief. Thus, the Stoics distinguish between commemorative and indicative signs. The former (smoke) serve to indicate the presence of objects (fire) *temporarily* imperceptible. The latter (bodily motion) indicate the presence of objects (the soul) which we are incapable of perceiving at all. Sextus Empiricus (ATL, p. 319) rejects the use of indicative signs, and affirms the legitimacy of commemorative signs. But his reason is pragmatic (rather than epistemological): he doesn't wish to attack 'ordinary life and all mankind'. Only commemorative signs, he (erroneously) thinks, are 'generally believed by all ordinary folk to be useful'.

Can this mistrust of the unobservable be justified? That beliefs about the unobservable go beyond the evidence does not show them to be irrational. The sceptic owes us a reason for 'discriminating' against non-deductive inferences whose conclusions are about the unobservable.

VAN FRAASSEN'S ARGUMENT

The relevant chemical theory, van Fraassen suggests, tells us that what goes on when we detect particles in a bubble-chamber is not an observation: 'The theory says that if a charged particle traverses a chamber filled with saturated vapour, some atoms…are ionized. If this vapour is decompressed…, it condenses in droplets…, thus marking the path of the particle' (van Fraassen, 1980, p. 17).

The instability of van Fraassen's doctrine—his appeal in its support to (theoretical) claims upon which he ends up casting doubt—cannot be invoked against his attempt scientifically to

ground scepticism about the unobservable. The sceptic, we have seen (see pp. 40–1), can handle such an objection. We must instead consider whether van Fraassen's reasoning is cogent. Does science show us that assenting to theories' claims about the unobservable is unwarranted?

Science tells us that our purported observation of an electron is different from the observation of a table. In the latter case, but not in the former, light rays are reflected directly from the object to our eyes. But what epistemological significance attaches to these causal facts about electrons, tables and light rays? If we think an evil demon is systematically misleading us, then we have a reason for mistrusting our senses and the information they convey about the external world. Similarly, if a scientific theory were to tell us that our vision was unreliable, that would give us a reason for not trusting our eyes. But chemical theory merely tells us that our causal interaction with the electron is more circuitous, and that doesn't impugn the warrant of the beliefs thereby acquired. We must conclude that van Fraassen hasn't managed scientifically to motivate scepticism about the unobservable.

HUME'S ARGUMENT

> By what argument can it be proved, that the perceptions of the mind must be caused by external objects, entirely different from them, though resembling them…and could not arise either from the energy of the mind itself, or from the suggestion of some invisible and unknown spirit, or from some other cause still more unknown to us?
>
> <div align="right">(Hume, 1777, pp. 152–3)</div>

Hume's objection, couched in the form of a rhetorical question, isn't that we cannot *deduce* the existence of external objects from our perceptions. By 'proof' he doesn't mean a logical deduction, but, rather, a reasonable inference from experience, like the one from past impressions of sunrises to tomorrow's. Whereas this non-deductive inference may be legitimate for Hume (when he isn't taking too seriously his scepticism about induction), the inference to external objects is crucially different, since 'the mind has never anything present to it but the perceptions, and cannot possibly reach any experience of their connexion with objects' (Hume, 1777, p. 153).

Inference to the unobservable, Ayer claims in a similar vein, is illegitimate.

> Experimental reasoning can carry us forward at a given level; on the basis of certain sense-experiences it allows us to predict the occurrence of other sense-experiences… What it does not permit us is to jump from one level to another; to pass from premises concerning the contents of our sense-experiences to conclusions about physical objects.
>
> <div align="right">(Ayer, 1956, pp. 77–8)</div>

'For what foundation could there be in such a case for our inductive arguments[?]', he asks rhetorically.

If we do not directly experience external objects, how can we know anything about them? Well, we cannot inductively confirm their 'connexion' (regular concomitance) with perceptions, because using induction (by enumeration) to infer the generalization 'All As are Bs' requires knowledge of some As and Bs regularly conjoined. But this fact doesn't suffice to show that the 'supposition of such a connexion is…without *any* foundation in reasoning' (Hume, 1777, p. 153, my italics). Hume has ruled out *one* possible foundation, induction by enumeration; others may remain. Rather than showing the inference to the existence of external objects to be irrational, Hume is best viewed as putting forward a challenge that its rational basis be made explicit.

The most promising response to this challenge is to claim that our beliefs about external objects are justified because explanatory of appearances (Russell, 1912, ch. 2). Similarly, we may argue that the theoretical claims of physics are warranted because they best explain observable phenomena, and those about mental states are justified by (because explanatory of) claims about behaviour. Inference to the best explanation (Harman, 1965) is the rational basis of at least some claims about the unobservable, we may argue. And to infer to the best explanation we do not have to experience the explaining phenomena directly; we just have to be able to recognize good explanations of phenomena we *do* experience.

Unlike deduction, this form of inference has not (as yet) been fully characterized. Philosophers of science ought, of course, to fill this lacuna. But we needn't wait until they do. Against the transcendent sceptic it is enough to point out that inference to the best explanation is a distinctive form of inference, and defend its

legitimacy at least in principle. Are there sceptical arguments to show that it is *always* irrational?

RUSSELL'S FIRST ARGUMENT

In inferring to the unobservable, Russell suggests, 'physics ceases to be empirical or based on experiment and observation alone', having recourse to an a priori principle, according to which 'our sense-data have *causes* other than themselves, and that something can be known about these causes by inference from their effects' (Russell, 1914, p. 108, original italics).

Now, if this objection were cogent it could be applied so as to rule out induction by enumeration as well. For this form of inference, Russell thinks, relies on 'the principle of induction', according to which the constant conjunction of As with Bs increases the probability that the next A will be a B (Russell, 1912, p. 66). And while this principle is, according to Russell, incapable of empirical (dis)confirmation (1912, p. 68), it is presupposed in our reasoning from experience. This *ad hominem* reasoning, of course, doesn't vindicate inference to the unobservable; it only shows that it is in the same (possibly leaky) boat with induction. But Russell's objection, I shall now argue, is not cogent.

We should first characterize more adequately the inference Russell faults. To begin with, we do not *assume* that 'our sense-data have causes other than themselves'. Rather, we *infer* the existence of these causes from the fact that they figure in a sufficiently good explanation of the phenomenon. One who thinks the explanation in terms of external objects isn't adequate ought to embrace phenomenalism.

What principle does the inference involve? Are we, for instance, assuming that the world is intelligible when we infer the truth of the best explanation? The analogy with induction and nature's uniformity should persuade us that this way of construing inference to the unobservable is inauspicious. Just as we do not suppose that the world is uniform *tout court,* we do not suppose that *everything* is explicable. Indeed, we suppose the contrary: we recognize that every theory will leave some things unexplained. Furthermore, we give theories credit for explanatoriness, but we sometimes forego explanation if it is too complicated or untestable.

Finally, again analogously with induction, just as we learn from experience the respects in which nature is uniform, so, perhaps, we modify our conception of what counts as a good explanation.

Russell's objection against inference to the best explanation is that it is not empirically justifiable. The response, again in analogy with induction and deduction, is that inference to the best explanation is a *basic* mode of inference, and one not in need of justification, empirical or otherwise.

RUSSELL'S SECOND ARGUMENT

The second reason Russell cites for shunning inference to the unobservable is that 'the inferred entities…[are] wholly remote from the data that nominally support the inference' (1914, p. 116). This claim is adduced in the course of Russell's attempt to persuade us that we should replace statements about the unobservable with statements about the observable in the spirit of his famous dictum: 'When possible, logical constructions are to be substituted for inferred entities' (1914, p. 115).

To justify the maxim, Russell appeals to a mathematical analogy. We construct the real numbers by identifying them with sets of rational numbers, and showing that the constructed objects have the 'right' properties: addition of these sets is commutative, for instance. We 'thereby gain a new and less doubtful interpretation of the propositions in question' (1914, p. 116). In physics we construct objects from appearances (sense-data). Each object is identified with the class of its appearances from all perspectives. I shall now argue that the analogy between mathematics and physics is not apt, and that Russell's methodological principle is untenable.

Why do we prefer the construction of irrational numbers from sets of rationals to inferring them as limits of series of rationals with no rational limit? The construction enables us to prove various claims about the constructed irrational numbers rigorously, whereas before the construction their very existence was doubtful, inferred 'as the supposed limits of series of rationals which had no rational limit' (Russell, 1914, p. 115).

Is there an analogy here with the physical case? The contrast Russell draws between inference and construction, is, in fact, specious. Both Russell (qua physicist) and the mathematician do not merely construct objects: they form beliefs about them, and do so inferentially. But the mathematical construction enables us to base the belief on a deductive inference (a proof); to replace a shaky belief with

a secure one. Analogously, Russell thinks, unlike the belief about the constructed physical object, the belief about the ordinarily understood physical object is 'wholly *remote* from the data that nominally support the inference' (1914, p. 116, my italics). What is this 'remoteness'? Since Russell thinks the cases are analogous, it must refer to the logical gap between the belief and the evidence for it. But, in fact, the analogy breaks down.

A statement about an external object, according to Russell (1914), is to be construed in terms of claims about appearances, some of which, namely 'the "sensibilia" which would appear from places where there happen to be no minds', are *no one's* data. This is designed, *inter alia,* to enable us to attribute existence to an object even while it is not perceived: Russell's aim, remember, is to interpret the statements of physics in as faithful a manner as possible.

This means that the proposition about the constructed physical object well transcends the evidence (our *actual* sense-data), and is justified—if at all—as the conclusion of a non-deductive inference. Furthermore, it is not clear that the inference to the 'constructed' object is less risky, involves a smaller inductive leap, than the one to the physical object ordinarily understood. To be sure, a statement about a 'bundle' of sensibilia does not entail anything about a physical object, but the converse is also true. If a particular table is 'defined as the *class* of its appearances' (Russell, 1914, p. 121, my italics), then any statement about it, 'The table is round', for example, will incorporate *all* the information as to how the table appears: its colour, smell and texture, for instance. And this information isn't part of the statement when it is construed as being about the physical object. It well transcends it.

We now come to the second disanalogy. The inferred entities, Russell says, should be 'similar to those whose existence is given' (1914, p. 116). Perhaps he has in mind Ockham's razor, the methodological principle which enjoins us not to 'multiply entities needlessly'. If we can make do with sense-data, we shouldn't commit ourselves to the existence of physical objects.

The appeal to Ockham's principle here is ineffectual. Perhaps we shouldn't 'do with more what we can do with less', but the principle only enjoins us to prefer a parsimonious theory if none of our 'needs' are thereby compromised. For instance, our theories must accommodate the evidence: Russell isn't suggesting that we deny—

for the sake of economy—the existence of sense-data. And there are other things we want to 'do' with our theories which cannot be done with 'less'. For instance, we may prefer a profligate theory because we can't explain as well without it.

In the mathematical case, there is no price to pay for economy, no explanatory loss involved in restricting ourselves to the constructed objects. Arguably, mathematical entities do not explain anything. And if they do, the constructed ones explain equally well. But the case is different in physics. Physical objects, ordinarily understood, are distinct from their appearances, and can be invoked to explain them. A bundle of appearances, on the other hand, cannot explain any of its constituents. So in eschewing belief about ordinary physical objects, we detract from our ability to explain. Russell's mathematical analogy, we must conclude, does not establish his stricture against believing in unobservable entities.

MODERATE SCEPTICISM?

If the representative theory doesn't preclude altogether the justification of beliefs about the external world, does it at least render them less warranted than does its rival? Does it 'degrade bodies into hypotheses, our assurance of the existence of the physical world [being] far stronger than any assurance we could obtain by indirectly confirming a theory' (Armstrong, 1961, p. 30)?

If the inference from sense-data to external objects *is* doubtful, we cannot vindicate our belief in the latter by jettisoning the representative theory. The belief in the external world will, surely, be in a worse position still if made basic, and deprived of even the (slender) evidential basis provided by sense-data.

But anyway, the worry is unwarranted. There is no reason for thinking that inferential justification is weak, giving rise to mere 'hypotheses'. Of course, if p is inferred from q, p cannot be more justified than q. In Berkeley's words, 'Nothing can give to another that which it hath not itself'. And p may well be less justified than q: inference is fallible. But this does not show that a basic belief is *in general* more warranted than one which is inferred. A belief might be inferentially justified by several others. And whereas each justifying belief can only pass on whatever warrant it itself has, the overall justification inferentially transmitted may well exceed the

justification possessed by each grounding belief, and amount to practical certainty.

Where does this leave us? We have only vindicated the *possibility* of inferring to the best unobservable explanation. In the next chapter I will defend its application to ground belief in the external world.

10

THE DEMON ARGUMENT
REVISITED

INTRODUCTION

The argument's two premises are:

(1) ~K~DEM (I do not know that I am not being deluded by a
demon).
(2) ~K~DEM→~Kp (If I do not know I am not being deluded, I
do not know that p).

We have seen (see pp. 56–7) that this is, in fact, an argumentschema,
yielding different arguments for different p's. But we needn't discuss every
argument separately. The plausibility of the second premise, the only one
in which p figures, can be assessed in a more wholesale manner. To
facilitate the discussion, however, let us take p to stand for 'I am sitting at
my desk'. It will be clear how the conclusions are to be generalized.

DENYING THE SECOND PREMISE?

Nozick (1981) responds to Descartes' argument by conceding its first
premise and denying the second. He thinks its plausibility derives
entirely from the principle of closure: K(r→q)→(Kr→Kq). The
rejection of this principle, he therefore thinks, deprives the second
premise of credibility, and blocks the sceptical argument.

The second premise can be derived from the principle of closure in the
following way. Substitute p for r, and ~DEM for q in the principle of
closure. Call the resulting conditional—K(p→~DEM)→ (Kp→K~DEM)
—CON. Now, propositions such as p entail the denial of the demon-
hypothesis: if there is a deceiving demon, there is no desk, and I am being

deceived into thinking I am sitting at one. This entailment is known, so
K(p→~DEM) is true, and the consequent of CON can be detached. The
second premise can now be derived by *modus tollens*.

Perhaps Descartes' second premise collapses once the principle of
closure is given up. But the principle is prima facie very plausible. So
how can it be denied?

According to Nozick (1981), one can know a proposition without
knowing one which knowingly follows from it. This is because truth-
tracking, the condition (in addition to truth) in virtue of which beliefs
are knowledge, isn't deductively transmitted (see p. 14).

Nozick is not the first to deny the closure of knowledge under
known entailment. Closure means that any (knowingly incompatible)
alternative to a known proposition must be known not to obtain. To
know that I am now in Jerusalem, I must know that I am not in
London, since I know being in Jerusalem entails (given some
background assumptions) not being in London. Now, Austin claims
that to know a proposition one must only be able to rule out *some*
alternatives. For instance, what is required if one is to know that there
is a goldfinch in the garden?

> Enough is enough: it doesn't mean everything. Enough means
> enough to show that (within reason, and for present intents and
> purposes) it "can't" be anything else, there is no room for an
> alternative, competing description. It does *not* mean, for example,
> enough to show that it isn't a *stuffed* goldfinch.
>
> (Austin, 1961, p. 52, original italics)

In a similar (but more precisely articulated) vein, Dretske (1970) suggests
that the *relevant* alternatives, those that *do* have to be ruled out, are those
which the agent takes to be the dominant contrast to the one he is
espousing. We can reject the sceptic's closure premise, because the
demon-hypothesis isn't deemed relevant to ordinary knowledge claims.

How effective is this strategy against the knowledge sceptic? Does
closure really fail? Austin, it might be objected, is conflating two
senses of the term 'rule out'. To be sure, one doesn't infer the bird's
being a goldfinch having independently established that it isn't a
stuffed goldfinch: the second alternative isn't epistemo-logically
prior to the first. But that doesn't mean that 'ruling out' the one
(knowing it to be false) isn't a (logical) condition for knowledge of the
other (M.Williams, 1991, pp. 331–2).

Nozick's analysis, Michael Williams (1991, 8.6) argues, is implausible precisely where it allows knowledge to fail of closure. Nozick considers the grandmother who believes her grandson is well, having seen him on his visits to her. Had he been ill, she wouldn't believe this, because the family would have kept it away from her. Her belief doesn't track the truth. Nonetheless, it is (intuitively) knowledge, and Nozick has to modify his account so as to reflect this intuition. The belief is knowledge, according to the new proposal, (roughly) because acquired via a reliable (truth-tracking) method: vision. But now, Williams argues, closure no longer fails. Our belief in the denial of the demon-hypothesis is acquired via a reliable method, deduction from beliefs acquired through the use of our senses, and constitutes knowledge. To be sure, it doesn't track the truth, but that is because were it false we would be using *another* method: deduction from beliefs we were *stimulated* into having.

> If knowledge is knowledge gained by methods available to us, it cannot matter what we would believe in situations where those methods were unavailable and in which we would have to rely on methods that would lead us to form beliefs that are not appropriately related to the facts.
>
> (M.Williams, 1991, p. 343)

Against Dretske, finally, one might wonder whether the relevance of alternatives is best construed in terms of the contrasting possibilities the speaker *has in mind*. Of course, the speaker may intend to rule out some alternative(s) in asserting a proposition. 'I am sitting at my desk' may ordinarily be asserted so as to rule out 'I am lying on the couch', and not 'I am being deluded by a demon into thinking I am sitting at my desk'. But the latter alternative doesn't have to be ruled out in ordinary contexts only because it has already been ruled out (as unlikely) by both speaker and hearer. But then, closure of knowledge under known implication doesn't fail in this case: my knowledge that I am sitting at my desk is closed under known implication, because in the relevant sense I *have* ruled out the demon-hypothesis.

Suppose, however, these qualms are met, and we can rebut the knowledge sceptic by denying closure. Still, we must focus (Chapter 1) on the sceptical argument couched in terms of justification, an argument whose premises are:

(1) ~J~DEM
(2) ~J~DEM?~Jp.

As before, this is an argument-schema, and Jp stands for 'I am justified in believing p'. For the sake of concreteness we can, again, let p stand for 'I am sitting at my desk'.

Can we emulate Nozick's response to the knowledge-argument by denying the second premise? This is not an attractive suggestion. One possible objection to it is that, whether or not knowledge is closed under known deduction, justification *is* closed under (justifiably believed) entailment and can legitimately be invoked in deriving the second premise. But this objection fails. The wishful belief in q isn't justified even if the agent justifiably believes p, p l-q: the belief is improperly caused. And initial appearances notwithstanding, this is quite consistent with the regulative conception of justification, which requires that one be guided in forming beliefs by what one believes about their justification (Chapter 1). If the agent suspects that his belief in q is caused by a wish, he ought to doubt his premises or the entailment: wishful believers typically 'manufacture' evidence, or distort their logic. If he is oblivious of the belief's aetiology, his belief in the premises and the entailment may well be justified. So may be his belief that q is justified. But he will be (justifiably) mistaken about his justification (see pp. 6–7).

The real objection to the above strategy is the following. Perhaps justification isn't deductively closed, but we have another reason for accepting the second premise. We want to deny the other premise, ~J~DEM: what we may be willing to concede in the case of knowledge is less willingly given up when it comes to justification. Perhaps we do not know that we are not deluded by an evil demon, but we are, surely, justified in the belief that we aren't! But the negation of the first premise entails the second premise! So the only plausible response to the demon argument when it is couched in terms of justification is to deny the first premise and (thereby) accept the second.

DENYING THE FIRST PREMISE

How is our belief that we are not deluded by a demon to be justified? It follows from premises (about external objects) we justifiably believe, and is believed *because* of the perceived entailment.

How are the premises justified? Well, for unsophisticated believers they are basic. Since foundations (if any) do not have to be infallible, there is, *pace* Alan Goldman (1988, ch. 4), no reason for insisting that beliefs about the external world must be based on other beliefs (perhaps about appearances).

What about more sophisticated agents? Let us consider, first, the possibility that beliefs about external objects can be justified as explanatory of appearances. I have argued (Chapter 9) that it must be shown why inference to the best explanation is illegitimate in *this* case, there being no reason for ruling out altogether inference to the unobservable.

WILLIAMS' OBJECTION

According to Hume, the external world is supposed to explain the coherence and regularity of experience: 'even in these changes, [bodies] preserve a coherence, and have a regular dependence on each other' (Hume, 1739, p. 195). But, Michael Williams argues, the presumption of coherence and regularity is simply false: 'purely experiential regularities collapse with every blink of the eye and turn of the head' (M.Williams, 1991, p. 55). The visual blue expanse I experience when looking at the sky will be replaced with a black one when I close my eyes, and then, again, by a green one when I open them and shift my gaze to a tree. There is no way of subsuming the changes under a purely experiential regularity; not even a statistical one. There is, therefore, nothing for the external world to explain!

The objection misidentifies the *explanandum*. To be sure, there are relatively few regularities within experience. But that is also something the 'external world hypothesis' may be invoked to explain. It is not only regularities that call for (and admit of) an explanation: we also explain the occurrence of particular events. *Ceteris paribus*, if a hypothesis best explains the data—however irregular—we may infer its (probable) truth. Now, we may invoke regularities in the *explanation*. Perhaps we explain an individual occurrence, whether characterized in purely experiential terms or in 'ordinary objects' language, by subsuming it under a law (Hempel, 1965). But the law(s) may involve concepts other than (additional to) those appearing in the *explanandum*.

When Hume says (of the fire in his chamber) that he is 'accustom'd…to see a like alteration produc'd in a like time,

whether [he is] present or absent, near or remote' (1739, p. 195), he is not reporting on a regularity in his *experience* of fires: the constant rate in which they abate. For how can he have an experience of a fire when he is absent from his room? But he can explain why the fire in his chamber is no longer in the same situation; why his fire-experience undergoes a sudden change when he leaves his chamber. He will cite a regularity between one's experience and one's circumstances (specified in terms of 'external objects'): one's distance from an unoccluded fire, the extent to which one's vision is unimpaired, etc. And its ability to explain his experience provides Hume with some warrant for believing in the (unobservable) regularity.

INFERRING THE WORLD FROM APPEARANCES

Even if we are willing to countenance beliefs which transcend appearances so as to improve our ability to explain, still there are alternatives to the real-world hypothesis. The relative merits of all the contenders must be assessed.

The external world is the best available explanation of appearances. As Bonjour points out, the real world hypothesis is more explanatory than the undeveloped, simple, demon-hypothesis, 'which postulates merely that there is an all-powerful evil demon who causes my experience…without saying anything more about the demon's motives and purposes or about what sorts of sense-perceptions and beliefs he is inclined to produce' (Bonjour, 1985, pp. 183–4). This hypothesis isn't minimally adequate as an explanation: it fails even to raise the probability of the *explanandum*.

Suppose we supplement the demon-hypothesis to include the supposition that the demon makes things appear *as if* there was a real world. Still, unlike the real-world hypothesis, it won't explain *how* our senses are affected to yield our perceptions. And in Lipton's words, 'we understand a phenomenon better when we know, not just what caused it, but how the cause operated' (Lipton, 1991, p. 118).

Note, furthermore, that when the demon-hypothesis is thus elaborated, it becomes less *simple* than the real-world hypothesis (Alston, 1993, p. 82). The real-world hypothesis, we must conclude, is the best explanation we have. And, furthermore, it is simpler than

any of its serious rivals: those whose explanation of appearances is not hopelessly inadequate.

As far as ontological economy is concerned, the real-world hypothesis fares ill. It is not just less economical than the assumption that appearances do not have *any* causes; it is less economical than all of its rivals that do purport to explain our experience. In Alston's words, it is 'recklessly prodigal in the kinds of things it invokes' in comparison with the demon-hypothesis, which 'postulates only one kind of explanatory factor' (1993, p. 78).

Should its ontological profligacy count against the real-world hypothesis? The answer, I think, is no. Prima facie, ontological economy is a factor in rational belief-formation. If we can explain the tides by invoking the gravitational pull of the moon and the mass of the water, we have no licence to postulate oceanic deities. But we should probe a bit deeper.

I submit that the only economy we should seek is *conceptual*. Very roughly, conceptual economy is determined by the smallest number of primitive concepts required to formulate the theory, concepts that are not definable in terms of the others. A theory's coherence (again, very roughly) depends on the existence of nomological connections between its concepts (and in the case of a quantitative theory, on the numerical relationships it posits between the magnitudes it relates, so that, for instance, a polynomial is simpler the lower its degree).

Ontological economy, the minimization of entity *types,* often goes with conceptual economy. But the two may diverge because of the distinction between sortal and non-sortal concepts. A sortal (individuative) concept ('horse', 'man') is used to say what something is, and to cover identity statements in the form 'x is the same f as y'. It supplies 'a principle for distinguishing and counting individual particulars which it collects' (Strawson, 1959, p. 168). A non-sortal concept 'blue', 'hot'), on the other hand, does not serve to objectify. It makes no sense to ask whether this is the same hot as that. The same *what,* we must be told.

Ontological economy is distinct from conceptual economy. The postulation of a new type of entity requires a new concept. But the converse is not true. The introduction of a non-sortal concept does not involve new ontological commitments. How does economy bear on simplicity? Well, we do not make a theory simpler just by replacing a sortal predicate with a non-sortal one, although we do make it ontologically more economical. The theory which ascribes to the

ocean a new property—'tidal tendency' (T_1) is not simpler than the one which postulates the existence of oceanic deities (T_2). Of course, to be worthwhile, T_2 must ascribe properties to the deities in addition to their being causally responsible for the tides: a bona fide entity cannot have just one property. And when thus elaborated, T_2 may (although it need not) be less simple than T_1, and not sufficiently explanatory to warrant the increased complexity. But then, it is by appealing to conceptual, rather than ontological, economy that it is being ruled out.

I conclude that the real-world hypothesis is the best available. But this is not enough to warrant belief in it: the best may not be good enough. This can be illustrated by the 'theological hypothesis': the supposition that God desires that the world should conform to the fundamental laws of physics. This provides (some kind of) an explanation for the laws of nature. Still, this fact doesn't warrant the belief in God's existence. Sometimes the first prize in a competition isn't awarded, because none of the competitors are judged worthy.

It might be thought that the problem with the theological hypothesis is that it doesn't engender novel predictions. But if that was a requirement on explanation, we would never be justified in inferring to the best *unobservable* explanation. As Alan Goldman (1988, p. 233) points out, to justify inference to the best unobservable explanation we must suppose explanation to be an *independent* virtue of a theory, over and above its predictive power. And this means that an explanatory hypothesis might be warranted even without engendering novel predictions. Indeed, this is precisely the case with respect to the real-world hypothesis. It explains appearances, but has no additional testable claims.

Descartes' attitude towards inference to the best explanation is more stringent. He allows himself 'to progress to the causes by way of the effects' (CSM I, p. 144), since 'the causes…explain [the effects]' (CSM I, p. 150). But the inference must satisfy very strict requirements. Not only are Descartes' premises (the *explananda*) perfectly secure, since 'experience makes most of these effects quite certain' (CSM I, p. 150), but when he infers to an explanatory hypothesis he must be able to eliminate alternative explanations experimentally. Since 'any particular effect…can be deduced from the principles [i.e., explained] in many different ways…[his] greatest difficulty is usually to discover in which of these ways it depends on them' (CSM I, p. 144). To vindicate a hypothesis, he must 'seek further

observations whose outcomes vary according to which of these ways provides the correct explanation' (CSM I, p. 144). He will not countenance a hypothesis whose rivals are empirically equivalent, even if it is more explanatory.

Now, Descartes doesn't need to infer the existence of God as explanatory of the regularities he sees in nature. He thinks he can prove it from 'first principles'. But his strictures on inference to the best explanation would rule out the inference to the theological hypothesis as explanatory of worldly regularities. *Our* scientific practice, on the other hand, is less stringent than Descartes', at least the one to which he officially subscribes. We 'reward' theories for explanatoriness as well as for testability and simplicity, so we cannot reject the theological hypothesis just because it engenders no novel predictions. On what grounds, then, do we fault it?

The theological hypothesis brings with it new mysteries. Why does God have the desires that he does? How does he impose his will upon the world (Alan Goldman, 1988, p. 212)? To say that he does it by 'a simple act of will' (Alston, 1993, p. 79) is not at all explanatory. Unlike the real-world hypothesis, the theological hypothesis doesn't engender enough of an explanatory gain to offset the decrease in simplicity. It isn't always preferable to have an explanation than none at all. That is why we infer the existence of the external world *and stop there*.

ALSTON'S OBJECTION

Inference to the real-world hypothesis as best explaining appearances, Alston claims (1993, 4.III), is unjustified. In order to justify a hypothesis via inference to the best explanation, he argues, we must 'be justified in supposing that…none of the other alternatives could be developed into something equally rich and detailed' (1993, pp. 80–1). But if this is so, Alston continues, the prospects for being able to infer the real-world hypothesis from appearances are dim. For we simply cannot justify this supposition. It 'is, in general, impossible to predict, or set limits on, theoretical developments' (1993, p. 84).

Two ways of answering Alston's objection suggest themselves. The first is to reject the requirement he imposes on inference to the best explanation. To be sure, it will be argued, once we are confronted with an equally good explanation of appearances, or even have a reason for thinking one exists, we shouldn't accept ours as true. But this is because we are only entitled to infer to the best

explanation if we do not have a reason for thinking a better one exists. *This,* and not Alston's, is a genuine requirement on inference to the best explanation. And, it is, surely, satisfied by the inference to the real world: we don't have a reason for thinking a better explanation exists.

This response is not satisfactory. To see why this is so, two ways of construing it must be considered. First, is it being assumed that in the absence of a reason for thinking a better explanation exists we are warranted in thinking we actually have the best one? This assumption is untenable. Perhaps some beliefs are prima facie justified: justified in the absence of countervailing evidence. But this is not the case in general. For instance, I am not warranted in believing that the number of hairs on my head is odd, although I have no reason for thinking it is even. The liberal attitude is justified only towards *basic* beliefs. And the belief that ours is the best explanation is, surely, not basic. It must be backed by a reason.

According to the second construal of the proposed response to Alston, we are entitled to infer the truth of an explanation when we have no reason for thinking a better one exists, without thereby assuming that no better one exists. But this licence is, again, unacceptably permissive. In deduction, for instance, we do not accept a conclusion just because we have no reason for thinking it doesn't logically follow from our premises: we require a proof that it does follow. Why should inference to the best explanation be governed by a less stringent standard?

A better response to Alston's objection is to accept his requirement on inference to the best explanation, and show that the inference to the real world satisfies it. How is this to be shown?

Note that if Alston is right, then it isn't only the inference from appearances to the real world that will fall by the wayside. Similar considerations will vitiate *any* inference to an explanatory theory, even in ordinary scientific contexts, in which background knowledge, far transcending appearances, is available. For why suppose we are in a better position to rule out the possibility of more explanatory alternatives to relativity theory, say? There is, according to Alston, a general difficulty about ruling out the existence of alternative theoretical explanations: 'to be in a position to predict successes and failures in theoretical developments…we would have to have already achieved those successes' (1993, p. 84).

The difficulty is specious. In order to know that our explanation is the best (*tout court*) we don't have to have formulated all the alternatives. There is a difference between knowing that there *is* an alternative, and knowing *what it is*. So there is nothing paradoxical about thinking that there is an as yet undiscovered theoretical explanation of anxiety (say) which is better than the currently available Freudian one, and, consequently, having little confidence in ours. And, conversely, we can reasonably, albeit defeasibly, suppose that the explanation future science will give of the tides (say) will not differ greatly from Newton's. Future science, we can very confidently predict, will not provide an alternative explanation of appearances; it will not deny the existence of the real world. So in inferring it from appearances we satisfy Alston's requirement.

FURTHER OBJECTIONS REBUTTED

It is often claimed that there are no objective criteria governing inference to the best explanation, and that, consequently, it can't be rationally justified: rationality, after all, *is* objective. But at least when the explanation proceeds by citing causes, being a good explanation *is* an objective matter (Lipton, 1991): thinking it is so doesn't make it so. Of course, not all explanations are causal, and it may not always be possible to rebut the charge of subjectivity. But that needn't concern us: the explanation of our experience in terms of external objects is causal.

The second objection is the following. The brain in a vat, who is stimulated into having massively false beliefs about its surroundings, may be justified in its beliefs about the external world. It is justified, for instance, in thinking it is sitting at its desk if it infers such propositions as explanatory of its appearances. But, the objection goes, we lose our 'contact with reality' by allowing the brain in a vat to be justified: we sever the link between justification and truth.

The charge can be rebutted. We maintain the link between justification and truth. We merely deny that it is analytic; that justification logically guarantees (probable) truth (see pp. 94–5). Even if justification doesn't *entail* reliability, so that the brain in the vat is justified (yet hopelessly mistaken), still, it is a (contingent) fact that our justificatory practice promotes the truth. Both we and the brain believe that justification (rationality) is truth-conducive. Since the belief is contingent, ours can be true while his is false.

The final objection is Flew's (1961). He argues that beliefs about appearances cannot ground belief in the external world since statements about appearances can only be formulated by reference to objects ('I have a catlike appearance'). Now, the premise may be correct, but the conclusion doesn't follow. We should distinguish epistemic from semantic priority. Even if statements about appearances invoke concepts of external objects, they need not presuppose that the concepts are instantiated (Alan Goldman, 1988, p. 192). So even if 'the application of words to the essentially private must be derivative from and secondary to their application to the necessarily public' (Flew, 1961, pp. 50–1), even if we must have concepts of external objects in order to talk about appearances, still, statements about the latter can epistemically ground statements about the former.

My conclusion is that beliefs about external objects can be justified as explanatory of appearances. This doesn't mean that they *need* be so justified: they could be basic even for sophisticated agents who have beliefs about appearances. In Alston's words,

> a belief is immediately justified…[if] there are conditions *sufficient* for its justification that do not involve any other justified beliefs of that believer. This condition could be satisfied even if the believer has other justified beliefs that could serve as grounds. Overdetermination is an epistemic as well as a causal phenomenon.
>
> (Alston, 1989, p. 45, original italics)

TRANSCENDENTAL ARGUMENTS

I canvassed (see pp. 51–2) two possible roles transcendental arguments could have in the dispute over scepticism. Kant puts his to an *immodest* use, to provide beliefs with justification they didn't have before. 'The demand for a deduction', he says, 'involves us in considerable perplexity, no clear legal title *to justify [the] employment [of a concept]*, being obtainable either from experience or from reason' (Kant, 1787, B117, my italics). Whereas geometry 'proceeds with security in knowledge that is completely *a priori*, and has no need to beseech philosophy for any certificate of the pure and legitimate descent of its fundamental concept of space' (1787, B120), '[everything] might be in

such a confusion that …in the series of appearances nothing presented itself which might…answer to the concept of cause and effect' (1787, B123). Kant's 'toilsome enquiries' are required to '[verify] the objective validity of the concept [of cause]' (1787, B120).

Having classified inference principles such as induction, deduction and inference to the best explanation as basic, we do not need Kant's transcendental deduction to justify our beliefs about the external world. Indeed, our beliefs about the basic categories of thought can never attain the same warrant as do our (mundane) beliefs about external objects. The former will depend on abstruse and contentious considerations. And Kant mistrusts the latter only because of his (misguided) predilection for the a priori (see pp. 51–2).

Could, then, Kant's transcendental argument be used *modestly,* to show that our beliefs about the external world, even those of unreflective people, were justified all along, being predominantly true? For this use, note, it is not necessary that the argument's conclusion be better grounded than beliefs about the external world. For the argument doesn't purport to ground these. It aims to rebut the sceptical doctrine, and will be quite successful even if its conclusion is—as is bound to be the case—(somewhat) contentious. Unfortunately, we cannot put transcendental arguments to this use either. It invokes a purely reliabilist conception of justification, which I have rejected (see p. 82).

If transcendental arguments cannot be invoked against the sceptic, what is their role? The account Kant provides of the basic categories of thought is, surely, interesting in its own right. And, furthermore, even if it cannot be used to ground our beliefs, nor even to show that they are justified, still, if successful, it *explains* why they are predominantly true.

11

THE DREAM ARGUMENT
REVISITED

INTRODUCTION

Since the dream argument resembles the demon argument in important respects, our response to it can be quite brief. The dream argument, recall, has two premises: (1) ~K~D; (2) ~K~D?~Kp. As before, the notation is the following:

Kp=I know p
D=I am dreaming
p=I am sitting at my desk

Many philosophers have objected to the second premise, faulting assumptions which purport to ground it. One way of deriving the premise is the following:

(1) D→~Kp	plausible assumption
(2) K(r→s)→(Kr→Ks)	closure principle
(3) Kp→KKp	iteration principle
(4) Kp→~D	1 contraposition
(5) K(Kp→~D)	4
(6) K(Kp→~D)→(KKp→K~D)	substitution in 2
(7) KKp→K~p	*modus ponens* 5,6
(8) [(r→s)^(s→t)]→(r→t)	tautology
(9) [(Kp→KKp)^(KKp→K~D)]→(Kp→K~D)	substitution in 8
(10) (Kp→KKp)^(KKp→K~Dp)	conjunction 3,7
(11)Kp→K~D	*modus ponens* 9,10
(12) ~K~D→~Kp	contraposition 11

Thus, the assumption of closure under known implication, K(r→q)→(Kr→Kq), and the iterative principle of knowledge, Kp®KKp, have

drawn fire. Perhaps these objections are cogent. But things are different when the argument is couched in terms of justification:

(1) ~J~D
(2) ~J~D®~Jp.

The same considerations I invoked in discussing the dream argument (see pp. 109–12) show that the second premise can be independently motivated. We want to reject the first premise: we think we *are* justified in believing we are not dreaming (even if we do not know it)! But the negation of the first premise entails the second.

Stroud has argued that we must reject both premises. Admittedly, he is concerned with knowledge, but, if cogent, his argument would be equally applicable to justification. If we accept the second premise, according to Stroud, we commit ourselves to the first. If 'it is a condition of knowing *anything* about the world beyond one's sensory experiences that one know that one is not dreaming', Stroud claims, one 'would have to have known at some time that [one] was not dreaming in order to get the information [one] needs to tell at *any* time that [one] is not dreaming—and that cannot be done' (1984, p. 21, original italics). But why can't it? Only if we interpret the condition as one of temporal or epistemic priority.

Interpreted temporally, it would be a condition on knowing that p that one *first* know that one is not dreaming. But then, in order to come to know that one is not dreaming one must *already* know it, and that is impossible. On the second interpretation, the belief that one isn't dreaming would be epistemically prior to the belief in p. This would, again, be absurd if p is the belief that one isn't dreaming: a belief can't be epistemically prior to itself. But, of course, the condition $Kp \rightarrow K\sim D$ ($Jp \rightarrow J\sim D$) is neither temporal nor epistemological. It is logical (M.Williams, 1991, p. 85), and trivially satisfied when the proposition known (justifiably believed) is ~D. And, of course, since the second premise does not *entail* the first, we can accept the former and reject the latter. Indeed, this is the only plausible response to the argument.

REJECTING THE FIRST PREMISE

So how is it that we are justified in believing that we are not dreaming? Bernard Williams (1978, appendix 3) suggests that one can sometimes

tell that p even if one couldn't tell that ~p if that were the case. Thus, I know I am not dead (or unconscious), although if I were I wouldn't believe I was: in fact, I wouldn't believe anything.

Williams appears to have preempted Nozick (1981): he seems to be claiming that knowledge ('being able to tell that…') does not have to be truth-tracking. A belief, p, tracks the truth if the agent disbelieves p when ~p is the case. And this is required, according to Williams, only if the belief formed when ~p is the case is rational. But the beliefs (if any) I form when dreaming are not rational. And that is why, *pace* Nozick, my wakeful belief that I am not dreaming may constitute knowledge.

Truth-tracking is inappropriate in the case of the belief that we are not dreaming (dead). We can tell that we are not dreaming, Williams suggests, because our (wakeful) experience is internally coherent, and we can explain the phenomenon of dreaming from the perspective of waking.

Williams' suggestion is untenable. It doesn't seem as if knowledge can be analysed in terms of coherence and truth-tracking, some beliefs requiring only the former (so as to constitute knowledge), others needing both. Williams is correct in thinking the requirement of truth-tracking is too stringent, but his manner of relaxing it is inappropriate. Plausibly, we shouldn't require a belief to track the truth too far afield (Alan Goldman, 1988). And if we don't, our belief that we aren't being deceived by a demon might count as knowledge. Equally plausibly, a belief should only track the truth to (close) worlds in which its method of acquisition is kept constant (Nozick, 1981): this is what we learn from the example of the grandmother (see p. 111). In addition, a belief shouldn't track the truth to a world in which the agent has *no* beliefs; those, for instance, in which he is dead. But *contra* Williams, the lesson to be learnt from these counter-examples to the necessity of truth-tracking isn't the sufficiency of coherence for knowledge: we have learnt from Gettier (1963) that that can't be right. Rather, these counter-examples suggest that the reliability of knowledge isn't to be understood in terms of truth-tracking. Perhaps it should be construed, as Nozick himself suggests, by reference to methods *actually* used in deriving a belief.

This would render our wakeful belief that we are not dreaming knowledge. For whatever our 'method' in forming beliefs when we are awake, when we dream we do not employ any method: our beliefs aren't rational. The fact (if it is a fact) that Descartes '[has] been tricked by exactly similar thoughts while asleep' (CSM II, p. 13), and

thought 'that [he was] here in [his] dressing gown …when in fact [he was] lying undressed in bed' shows the unreliability of the dreamer's belief. To show the unreliability of the *wakeful* belief, Descartes would have to cite wakeful occasions in which he mistook himself to be asleep. And, of course, there are very few, if any.

These considerations all pertain to knowledge. The requirement of truth-tracking, however restricted, certainly seems too stringent for justification. It would deprive the demon's victim of justification he surely has (see p. 112). Our wakeful belief that we are not dreaming is justified because it best explains our experience. (Indeed, it also explains the phenomenon of dreaming.) We can endorse Descartes' criterion without appealing to God to underwrite it: 'when I can connect my perceptions of [things]…with the whole of the rest of my life without a break, then I am quite certain that when I encounter these things I am not asleep but awake' (CSM II, p. 62). This is analogous to the demon case, in so far as we infer from our experience to the best explanation. But there is an important difference.

Both we and the demon's dupe are justified (see pp. 119–20). But the beliefs of the dreamer are not rational. This is due to three asymmetries between dreaming and wakefulness. First, no individual dream is internally coherent: even if logically consistent, the 'occurrences' within it are not explained by reference to lawlike regularities. Perhaps they *could* be, but the dreamer has (typically, at least) not formulated such an explanation. Second, dream-beliefs are not related in a coherent way across dreams: the 'events' in one do not lead in any systematic or predictable way to those of a subsequent one. Finally, we are unable to account for wakefulness from within dreaming. From our wakeful perspective, we have (some) understanding of dreaming, both physiological and psychoanalytical. And although we cannot predict the content of individual dreams, we at least know that they will only occur upon going to sleep. There is nothing analogous from the perspective of the dream: no theory, even rudimentary, of what wakeful episodes are, and no systematic way in which dreams lead to waking up.

SEXTUS' DREAM ARGUMENT

Sleeping and waking, too, give rise to different impressions, since we do not imagine when awake what we imagine in sleep, nor when asleep what we imagine when awake; so that the existence

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or non-existence of our impressions is not absolute but relative, being in relation to our sleeping or waking condition. Probably, then, in dreams we see things which to our waking state are unreal, although not wholly unreal; for they exist in our dreams, just as waking realities exist although non-existent in dreams.

(PH, p. 63)

Sextus' dream argument is one of his ten modes, or tropes, in which incompatible judgements are made from different perspectives. The way things appear to us depends on whether we are asleep or awake, healthy or ill, old or young, drunk or sober, at motion or at rest, etc. There is, furthermore, no way of deciding which of the perspectives is the reliable one.

Sextus' dream argument is different from Descartes'.[1] It relies on the *recognizable* difference between the two kinds of experience, and on there being no way of deciding which of the two judgements to which they give rise is objectively correct. Sextus knows he is awake, but doesn't think this perspective engenders reliable beliefs. Descartes, on the other hand, assumes that wakeful experience, unlike that of a dream, is (typically) veridical. But because there is no way of telling whether we are dreaming or awake, we cannot know whether our experience is a reliable indicator of the way things really are.

It is undeniably true that different perspectives give rise to incompatible judgements. But Sextus realizes that this doesn't suffice to show that we cannot form a justified opinion about how things are. There must be a reason for thinking that there are no grounds for choosing between the incompatible judgements; for taking one perspective to be more reliable than another. '[A]lthough it is easy to say what nature each of the underlying objects appears to…possess, we cannot…say what its real nature is, *since the disagreement admits in itself of no settlement*' (PH, p. 67, my italics).

One reason for thinking the disagreement is irresoluble is provided by the criterion argument: every dispute must be resolved by reference to a criterion, and there is none, Sextus thinks. But in this he is mistaken, I have argued (Chapter 7). Sextus adduces other reasons to support his contention. He argues, first, that there is no privileged vantage point from which the judgement can be made (PH, p. 67). When trying to adjudicate between the wakeful and dreaming experience, one must be in one of the competing states: 'to

declare that he is in no disposition at all—as, for instance, neither in health nor sickness…is the height of absurdity'. But then, one will be 'a party to the disagreement', and not an 'impartial judge'. Furthermore, Sextus argues, a judgement made from one perspective is liable to be confused: 'The waking person…cannot compare the impressions of sleepers with those of men awake…for we assent more readily to things present than to things not present' (PH, p. 67).

We can summarily dismiss the first reason for thinking that a judgement made from a perspective cannot be impartial. We do not have a vested interest in defending the reliability of our sense-impressions: we are interested in knowing how things are. As to the second reason, perhaps we 'assent to things present' before we reflect about our situation: we judge the square-looking tower to be square. But when we realize that the tower is very distant and the air is hazy, we withdraw our assent. The tower still appears square, but we judge its appearance to be misleading. Of course, we might not always realize our predicament. But this doesn't show that the two perspectives are on a par. Whatever our distance from the tower, we think its appearance to be a more reliable guide to its real shape the closer to it we are: this judgement is based on what we know about perception. Similarly, the drunk and the sober person alike judge the state of sobriety to engender more reliable judgements.

Admittedly, if we misidentify our perspective, we will form the wrong judgement: we will believe the tower is square because we think it is near, and its squarish appearance, therefore, is a reliable guide to its true shape. But this will only impugn our judgements when conjoined with the assumption that we are incapable of identifying our situation. This is quite implausible with respect to some perspectives: why suppose that we cannot know how distant we are from the tower? It is more plausible with respect to others: can we know that we are awake, sane, or sober? But if this is the worry, it is Descartes' sceptical argument which we are confronting. It is, to be sure, much more compelling. But, I have argued above, it can be answered.

CONCLUSION

The sceptical challenge is a formidable one. When properly mounted, it deserves our most serious attention. But equally, when its nature is understood, the pessimism often encountered about our

prospects for meeting it can be quelled. Our opponent reasons with us and is willing to listen to reason. Our polemical resources, therefore, shouldn't be underestimated. Indeed, having contended with the main sceptical arguments, we are justified in thinking scepticism to be false. Of course, we have not refuted scepticism. We have invoked contentious epistemological considerations, and have addressed scepticism only in a piecemeal fashion: more cogent sceptical arguments could be developed. Still, the burden of proof is now left (quite) firmly with the sceptic.

NOTES

1 KNOWLEDGE, JUSTIFICATION AND TRUTH

1   Dorothy Edgington has pointed out that this is not, strictly
    speaking, true. Unlike the paradoxical statement cited by Moore,
    the utterance 'I believe p and p is false' makes perfectly good sense,
    since one might discover that one unconsciously believes p without
    consciously believing it. One's unconscious self must be treated,
    in the present context, as doxastically distinct, on a par with one's
    past and future selves. One *can* judge false one's unconscious belief,
    but cannot thus distance oneself from one's *conscious* belief.

2 THE SCEPTICAL LIFE

1   If P(e)<1, P(h)>0, and h implies e, then
    P(h/e)=P(h&e)/P(e)=P(h)/P(e)>P(h).

6 INDUCTIVE SCEPTICISM

1   To illustrate the claim that lawlikeness depends on how the
    predicates of a hypothesis 'fit together', Davidson (1966) cites the
    hypothesis 'All emeroses are gred', where an object is an emerose
    (gred) iff it is an emerald (rose) and the time is prior to t, or it is a
    rose (emerald) and the time is after t. The example is supposed to
    be of a lawlike hypothesis, each of whose two predicates figure in
    hypotheses which are accidental: 'All emeralds are gred' and 'All
    emeroses are red'. But in fact, the hypothesis isn't lawlike in the
    requisite sense: it isn't confirmed by its instances. A green emerald
    doesn't add credence to the prediction that a rose examined after t
    will be red. The hypothesis *does* support counterfactual
    conditionals. If this were an emerose, it would be gred, since if it is
    before t it would be an emerald, and would be green—and,
    therefore, gred. And if it is after t, it would be a rose, and would be

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red—and therefore, gred. The hypothesis is logically equivalent to a conjunction of lawlike hypotheses: 'All emeralds are green', and 'All roses are red', both of which *are* confirmed by their instances. And this, of course, explains why it supports counterfactual conditionals although it isn't lawlike.

8 INDUCTIVE SCEPTICISM REVISITED

1   The inference is not a theorem of the probability calculus, because in any non-denumerable probability space there will be non-empty events assigned probability 0. For instance, suppose we draw at random a real number from the interval (0,1). The probability of any particular number being drawn is 0, but *some* number will be drawn!

11 THE DREAM ARGUMENT REVISITED

1   I owe this point to Vered Glickman.

BIBLIOGRAPHY

Alston, W. 1989. *Epistemic Justification,* Ithaca: Cornell University Press.

Alston, W. 1993. *The Reliability of Sense Perception,* Ithaca: Cornell University Press.

Armstrong, D.M. 1961. *Perception and the Physical World,* London: Routledge & Kegan Paul.

Armstrong, D.M. 1973. *Belief, Truth, and Knowledge,* Cambridge: Cambridge University Press.

Armstrong, D.M. 1983. *What is a Law of Nature?* Cambridge: Cambridge University Press.

Audi, R. 1988. *Belief, Justification, and Knowledge,* Wadsworth.

Austin, J.L. 1961. 'Other Minds', in *Philosophical Papers,* Oxford: Clarendon Press.

Ayer, A.J. 1956. *The Problem of Knowledge,* Harmondsworth: Penguin Books.

Ayer, A.J. 1971. *Language, Truth and Logic* (2nd edition), Harmondsworth: Penguin.

Bach, K. 1985. 'A Rationale for Reliabilism', *The Monist* 68, pp. 246–63.

Barker, S.F. and Achinstein, P. 1960. 'On the New Riddle of Induction', *Philosophical Review* 69, pp. 511–22.

Black, M. 1958. 'Self-Supporting Inductive Arguments', *Journal of Philosophy* 55, pp. 718–25.

Black, M. 1970. *Margins of Precision,* Ithaca: Cornell University Press.

Blackburn, S. 1973. *Reason and Prediction,* Cambridge: Cambridge University Press.

Bonjour, L. 1985. *The Structure of Empirical Knowledge,* Cambridge: Harvard University Press.

Braithwaite, R.B. 1968. *Scientific Explanation,* Cambridge: Cambridge University Press.

Broad, C.D. 1952. 'The Philosophy of Francis Bacon', in *Ethics and the History of Philosophy,* London: Routledge & Kegan Paul.

Burnyeat, M. 1978. 'Can the Skeptic Live His Skepticism?', in M.Burnyeat (ed.), *The Skeptical Tradition,* University of California Press, 1983.

Carnap, R. 1950. *Logical Foundations of Probability,* London: Routledge & Kegan Paul.

Carroll, L. 1895. 'What the Tortoise said to Achilles', *Mind* 4, pp. 278–80.

Cottingham, J.G. 1976. 'The Role of the Malignant Demon', *Studia Leibnitiana* 8, pp. 257–64.

Craig, E. 1987. 'The Practical Explication of Knowledge', *Proceedings of the Aristotelian Society* 87, pp. 211–26.

Craig, E. 1989. 'Nozick and the Sceptic: the Thumbnail Version', *Analysis* 49, pp. 161–2.

Dancy, J. 1985. *An Introduction to Contemporary Epistemology,* Oxford: Blackwell.

Davidson, D. 1966. 'Emeroses by Other Names', in *Essays on Actions and Events,* Oxford: Clarendon Press, 1980.

Davidson, D. 1974. 'Psychology as Philosophy', in *Essays on Actions and Events,* Oxford: Clarendon Press, 1980.

Davidson, D. 1986. 'A Coherence Theory of Truth and Knowledge', in E. Lepore (ed.), *Truth and Interpretation,* Oxford: Blackwell.

Descartes, R. 1628. *The Philosophical Writings of Descartes,* trans. J.Cottingham, R.Stoothoff, and D.Murdoch, Cambridge: Cambridge University Press, 1985, 2 vols, referred to as CSM I and CSM II .

Dretske, F. 1970. 'Epistemic Operators', *Journal of Philosophy* 67, pp. 1007–23.

Dretske, F. 1977. 'Laws of Nature', *Philosophy of Science* 44, pp. 248–68.

Dummett, M. 1973. 'The Justification of Deduction', in *Truth and Other Enigmas,* Duckworth, 1978.

Dummett, M. 1991. *The Logical Basis of Metaphysics,* Duckworth.

Edwards, P. 1949. 'Russell's Doubts about Induction', *Mind* 58, pp. 141–63.

Feigl, H. 1950. 'De Principiis Non Disputandum…?', in M.Black (ed.), *Philosophical Analysis,* Ithaca: Cornell University Press.

Feyerabend, P. 1975. *Against Method,* London: Verso.

Flew, A. 1961. *Hume's Philosophy of Belief,* London: Routledge & Kegan Paul.

Flew, A. 1986. *David Hume: Philosopher of Moral Science,* Oxford: Blackwell.

Fogelin, R.J. 1985. *Hume's Scepticism in the Treatise of Human Nature,* London: Routledge & Kegan Paul.

Foley, R. 1978. 'Inferential Justification and the Infinite Regress', *American Philosophical Quarterly* 15, pp. 311–16.

Frankfurt, H. 1970. *Demons, Dreamers, and Madmen,* New York: Bobbs-Merrill.

Gettier, E. 1963. 'Is Justified True Belief Knowledge?', *Analysis* 23, pp. 121–3.

Gewirth, A. 1941. 'The Cartesian Circle', *Philosophical Review* 50, pp. 368–95.

Glymour, C. 1980. *Theory and Evidence,* Cambridge: Cambridge University Press.

Goldman, Alan 1988. *Empirical Knowledge,* Berkeley: University of California Press.

Goldman, Alvin 1967. 'A Causal Theory of Knowledge', *Journal of Philosophy* 50, pp. 357–72.

Goodman, N. 1973. *Fact, Fiction and Forecast* (3rd edition), New York: Bobbs-Merrill.

Grice, H.P. and Strawson, P.P. 1956. 'In Defense of a Dogma', *Philosophical Review* 65, pp. 141–58.

Hamlyn, D.W. 1970. *The Theory of Knowledge,* London: Macmillan.

Harman, G. 1965. 'The Inference to the Best Explanation', *Philosophical Review* 74, pp. 88–95.

Hempel, C.G. 1965. *Aspects of Scientific Explanation,* New York: Free Press.

Hookway, C. 1990. *Scepticism,* London: Routledge & Kegan Paul.

Horwich, P. 1982. *Probability and Evidence,* Cambridge: Cambridge University Press.

Hume, D. 1739. *A Treatise of Human Nature,* ed. by L.A.Selby-Bigge (2nd edition), Oxford: Clarendon Press, 1975.

Hume, D. 1777. *Enquiries Concerning Human Understanding,* ed. by L.A. Selby-Bigge (3rd edition), Oxford: Clarendon Press, 1978.

James, W. 1956. 'The Will to Believe', in *The Will to Believe and Other Essays in Popular Philosophy,* New York: Dover.

Johnsen, B. 1990. 'Relevant Alternatives and Demon Scepticism', in M.D. Roth and G.Ross (eds), *Doubting,* Dordrecht: Kluwer.

Kant, I. 1783. *Prolegomena to any Future Metaphysics,* trans. P.G.Lucas, Manchester: Manchester University Press, 1953.

Kant, I. 1787. *Critique of Pure Reason,* trans. Norman Kemp Smith, London: MacMillan, 1990.

Kenny, A. 1968. *Descartes,* New York: Random House.

Kneale W. 1949. *Probability and Induction,* Oxford: Clarendon Press.

Kornblith, H. 1980. 'Beyond Foundationalism and the Coherence Theory', *Journal of Philosophy* 77, pp. 597–612.

Kuhn, T. 1970. *The Structure of Scientific Revolutions* (2nd edition), Chicago: Chicago University Press.

Laudan, L. 1987. 'Progress or Rationality: the Prospects for Normative Naturalism', *American Philosophical Quarterly* 24, pp. 19–31.

Lehrer, K. 1974. *Knowledge,* Oxford: Oxford University Press.

Lewis, D. 1973. 'Causation', *Journal of Philosophy* 70, pp. 556–67.

Lewis, D. 1986. 'Causal Explanation', in *Philosophical Papers* vol. 2, Oxford: Oxford University Press.

Lipton, P. 1991. *Inference to the Best Explanation,* London: Routledge & Kegan Paul.

Mill, J.S. 1884. *A System of Logic,* London: Longmans, Green & Co.

Moore, G.E. 1959. *Philosophical Papers,* London: Allen & Unwin.

Newton-Smith, W. 1981. *The Rationality of Science,* London: Routledge & Kegan Paul.

Nisbett, R. and Ross, L. 1980. *Human Inference: Strategies and Shortcomings of Social Judgement,* Englewood Cliffs, NJ: Prentice Hall.

Nozick, R. 1981. *Philosophical Explanations,* Oxford: Clarendon Press.

Oakley, I.T. 1976. 'An Argument for Scepticism Concerning Justified Beliefs', *American Philosophical Quarterly* 13, pp. 221–8.

Papineau, D. 1987. *Reality and Representation,* Oxford: Blackwell.

Peirce, C.S. 1965. *Collected Papers* (2nd edition), Cambridge, MA: Harvard University Press.

Popper, K.R. 1972. *The Logic of Scientific Discovery* (4th edition), London: Hutchinson.

Putnam, H. 1978. *Meaning and the Moral Sciences,* London: Routledge & Kegan Paul.

Putnam, H. 1981. *Reason, Truth and History,* Cambridge: Cambridge University Press.

Quine, W.V.O. 1951. 'Two Dogmas of Empiricism', *Philosophical Review* 60, pp. 20–43.

Quine, W.V.O. 1960. *Word and Object,* Cambridge, MA: MIT Press.

Quine, W.V.O. 1969. 'Epistemology Naturalized', in *Ontological Relativity and Other Essays,* New York: Columbia University Press.

Quine, W.V.O. 1973. *The Roots of Reference,* La Salle, Ill: Open Court.

Quine, W.V.O. 1975. 'The Nature of Natural Knowledge', in S.Guttenplan (ed.), *Mind and Language,* Oxford: Oxford University Press.

Quine, W.V. 1976. 'Natural Kinds', in *Ontological Relativity and Other Essays,* New York: Columbia University Press, 1969.

Quinton, A. 1973. *The Nature of Things,* London: Routledge & Kegan Paul.

Reichenbach, H. 1949. *The Theory of Probability,* Berkeley: University of California Press.

Reid, T. 1764. *An Inquiry into the Human Mind*, in *The Works of Thomas Reid*, ed. by Sir William Hamilton (5th edition), Edinburgh: McClachlan & Stewart, 1846.

Reid, T. 1785. *Essays on the Intellectual Powers of Man,* in *The Works of Thomas Reid,* ed. by Sir William Hamilton (5th edition), Edinburgh: McClachlan & Stewart, 1846.

Reynolds, S. 1991. 'Knowing How to Believe with Justification', *Philosophical Studies* 64, pp. 273–92.

Russell, B. 1912. *The Problems of Philosophy,* Oxford: Oxford University Press.

Russell, B. 1914. 'The Relation of Sense-data to Physics', in *Mysticism and Logic,* London: Allen & Unwin, 1963.

Russell, B. 1948. *Human Knowledge, Its Scope and Limits,* London: Allen & Unwin.

Sainsbury, M. 1988. *Paradoxes,* Cambridge: Cambridge University Press.

Salmon, W.C. 1957. 'Should We Attempt to Justify Induction?', *Philosophical Studies* 8, pp. 33–48.

Savage, L.J. 1954. *The Foundations of Statistics,* London: Chapman & Hall.

Sextus Empiricus . *Outlines of Pyrrhonism,* trans. R.G.Bury, The Loeb Classical Library, London: W.Heinemann, 1933, referred to as 'PH'.

Sextus Empiricus . *Against the Logicians,* trans. R.G.Bury, The Loeb Classical Library, London: W.Heinemann, 1935, referred to as 'ATL'.

Smith, P. and Jones, O.R. 1986. *The Philosophy of Mind,* Cambridge: Cambridge University Press.

Smullyan, R. 1980. *This Book Needs no Title,* Englewood Cliffs, NJ: Prentice Hall.

Stitch, S. 1990. *The Fragmentation of Reason,* Cambridge, MA: MIT Press.

Stove, D.C. 1973. *Probability and Hume's Inductive Scepticism,* Oxford: Clarendon Press.

Strawson, P.F. 1952. *Introduction to Logical Theory,* London: Methuen.

Strawson, P.F. 1959. *Individuals,* London: Methuen.

Stroud, B. 1977. *Hume,* London: Routledge & Kegan Paul.

Stroud, B. 1984. *The Significance of Philosophical Scepticism,* Oxford: Clarendon Press.

Unger, P. 1971. 'A Defense of Skepticism', *Philosophical Review* 80, pp. 198–218.

Unger, P. 1975. *Ignorance,* Oxford: Clarendon Press,

van Cleve, J. 1979. 'Foundationalism, Epistemic Principles, and the Cartesian Circle', *Philosophical Review* 88, pp. 55–91.

van Cleve, J. 1985. 'Epistemic Supervenience and the Rule of Belief', *Monist* 68, pp. 90–104.

van Fraassen, Bas c. 1980. *The Scientific Image,* Oxford: Clarendon Press.

von Wright, G.H. 1965. *The Logical Problem of Induction,* Oxford: Blackwell.

Walsh, W.H. 1963. *Metaphysics,* London: Hutchinson University Library.

Williams, B. 1970. 'Deciding to Believe', in *Problems of the Self,* Cambridge: Cambridge University Press, 1973.

Williams, B. 1978. *Descartes,* Harmondsworth: Penguin.

Williams, M. 1977. *Groundless Belief,* New Haven, CT: Yale University Press.

Williams, M. 1991. *Unnatural Doubts,* Oxford: Blackwell.

Wittgenstein, W. 1969. *On Certainty,* Oxford: Blackwell.

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