Hume's problem, epistemic deductivism and the validation of induction

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ABSTRACT

Contrary to Owen (2000), Hume's problem is, as has traditionally been supposed, a problem for the justification of inductive inference. But, contrary to tradition, induction on Hume's account is not deductively invalid. Furthermore, on a more modern conception of inductive or ampliative inference, it is a mistake to suppose that the proper construal of an argument explicating the supposed justification for such inferences should in general be non–deductive. On a general requirement for argument cogency that arguments should be suitably constructed so as to make it clear to the audience that the subject is justified, on whatever basis is cited, in regarding the hypothesis with whatever epistemic attitude the arguer purports to be so justified, arguments in general, fully explicated and properly construed, should be deductively valid.

Hume's problem does not prevent such justification because his crucial argument establishes only that our basic assumptions cannot be justified, in the sense of being 'proven', or shown by non–question–begging argument to be just. It does not establish that our basic assumptions, properly explicated, are not just, or that they are not (at least to the satisfaction of most of us) clearly so. Nor does Goodman's 'new riddle' of induction pose a serious problem for the justification of our inductive inferences, as is still commonly suggested, since Jackson figured out the solution to the riddle thirty years ago. There is an analogous problem to Hume's for the provability of principles or claims of deductive inferability, and if my analysis of the proper construal of the structure of argument (in the natural sense) is correct, this will block Howson's (2000) proposed escape route. Nevertheless, as with the case of induction, the unprovability of basic claims and principles of deductive inferability does not bar their deployment in cogent justifications.

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Declaration: None of the material in this thesis has previously been submitted for a degree in this or any other university.

INTRODUCTION
WHAT IS A GOOD ARGUMENT?

In 'What is a good argument?' (1992) Trudy Govier presents a critical survey of a number of different accounts of what it is for an argument to be cogent. This article, along with her elaboration of some of the key points in her earlier book *Problems in Argument Analysis and Evaluation* (1987) – and especially chapter 3 on 'The Great Divide' (between deductive and inductive arguments) – constitute a convenient point of reference for introduction of the topic of this thesis, and identification of the general subject area within informal logic. My concern in this thesis is with the concept of induction, its relation to deduction, its logical structure, and the significance of philosophical problems that have been raised against its cogency.

Govier introduces the basic concept of the 'cogency' (or 'goodness') of an argument in terms of its being 'epistemically and logically adequate'. By this she means that it satisfies two types of condition, one concerning the premises (which we might call *premise*–adequacy) and one concerning the relation between the premises and conclusion (which we may call *logical* adequacy). Different theories about argument cogency offer different accounts of what these conditions are. Before proceeding to discuss the relative merits of alternative accounts of argument cogency, Govier sets out three conditions that she believes ought to be met by any adequate theory of argument cogency. These are basically as follows:

Govier's conditions for an adequate account of argument cogency

(1) The theory should set conditions for cogency that are neither too strict nor too lenient – as judged by the standards of (a) common intuitions about examples, (b) philosophical opinion, and (c) the widely accepted opinions of influential scientists.[1]

(2) The theory should be 'reliable' – in the weak sense that it can be used by different people to get the same result[2] – *except* when different results can be explained by

[1] Govier presents this latter standard as if it were meant to exemplify the aforementioned. But since it seems to represent an appeal to scientific authority, in contrast to common intuition or philosophical opinion, I think it stands better as a supplement.
[2] Which we may distinguish from a perhaps more natural strong sense, that application of the theory always or mostly identifies arguments that actually *are* cogent.

differences of opinion about whether the premises satisfy the respective premise conditions.[3]

(3) The theory should be fairly easy ('uncumbersome') to apply.

While I have no particular problem with the third condition of (reasonable) ease of application,[4] I am somewhat puzzled, with regard to condition (2), why Govier thinks that while scope for differences of opinion about whether the respective premise condition is satisfied in any particular case need not compromise the adequacy of a theory, she does not appear to permit any scope for differences of opinion about whether the respective logical condition (in her terminology 'inferential condition') – i.e. the condition relating to the appropriate relation between premises and conclusion – is satisfied. This would be understandable if, for any serious candidate account of logical adequacy, it would be unlikely for there to be any scope for difference of opinion in particular cases about whether it is satisfied. But of course that is unlikely to be the case with some serious candidates – notably with regard to accounts of logical adequacy that permit some non–deductive relation between premises and conclusion – not only because some of us, as Govier acknowledges, are deductivists, but because even among those who are not, there often *is* scope for disagreement about whether a particular conclusion is or is not inductively inferable (even granted any particular interpretation of the latter) from given premises.[5] In view of this just as we are prepared to admit that there may be scope for disagreement over satisfaction of the *premise* condition (as distinct from the applicability or legitimacy of the premise condition) it seems that we should likewise be prepared to admit that there may be scope for disagreement about satisfaction of the *logical* condition. But if we make the appropriate revision, the 'reliability' condition doesn't seem to amount to very much at all, since then it simply amounts to the condition that we should agree about the result *if* we agree that the premise condition *and* the logical condition are *both* satisfied. But that would seem to be nothing more than a truism that would apply to *any theory whatsoever* – if indeed as Govier suggests it is simply in respect of satisfaction of these two conditions that we *call* an argument 'cogent'. In that case the condition (as amended) would appear to be otiose.

[3] Actually Govier refers to difference of opinion about 'the *warrantedness* of the premises'. But that would appear to presuppose a particular account of the condition for premise–adequacy. I guess she would agree the amendment that I propose.

[4] at least as a principle of preference for choosing between otherwise equally appealing theories. But of course it must go without saying that apparent *correctness* of a theory, which will be determined by other criteria, must take precedence over relative ease of use.

[5] For example proponents of the logical theory of inductive probability generally conceded that, since we are not logically omniscient, we may often be mistaken about the value of a specified probability, and that our personal judgements as to the respective values may differ. Nor is this just a matter of academic dispute among philosophers, practitioners – even expert practitioners in agreement about the evidence

As regards condition (1) – seeming to be right by the standards of intuition, philosophical opinion, and scientific endorsement, these are all worthy aims, but of course we might anticipate some problems of consistency between standards that may be perceived to be appropriate from each of these perspectives, and of course even from the viewing area of *one* of these perspectives there will be considerable scope for disagreement, since there will be disagreement in *all* areas about what legitimately follows *granted* certain given premises in many cases, as well as disagreement about the *criteria* for the logical adequacy of arguments in those areas where this relatively theoretical question is considered. Fortunately though, despite these apparently overwhelming problems for the conditions of theory adequacy proposed by Govier, I believe there is a line of approach to this issue that enables us to establish an account of argument cogency that on reflection should warrant a broad range of agreement from each of these perspectives, and moreover which should satisfy Govier's condition of reliability as *originally* specified (a requirement which is certainly *not* otiose) by justifying a simple and easily applicable criterion for logical adequacy – a criterion which, if my arguments concerning the appropriate structure of argument are right, should be generally agreeable to deductivist and inductivist alike.

As regards condition (3) – easy applicability, as I have said, this will be readily met in respect of establishing whether the condition of *logical* adequacy is satisfied – which I regard as a major merit of the approach I shall propose. But the problem of establishing agreement about *satisfaction* of the *premise condition* is always going to pose a formidable difficulty for application of any serious theory of argument cogency – whatever the proposed condition. Moreover I shall argue that this is in fact more of a problem than is traditionally accounted for: If my arguments in what follows are right, unless we want to commit ourselves to a strictly *logical* theory of inductive probability – a theory most commonly regarded as untenable – then we need to acknowledge that our disagreements about what is non–deductively 'inferable' from what are (or at least appear to be) substantive disagreements about logically contingent conditionals.[6] And as such their proper *logical* position in any properly formulated argument is

base and from the same school of statistics – may often disagree about the probability of a hypothesis or whether a particular conclusion may be drawn.

[6] Although I personally accept the objections that have been raised against the logical theory of probability, as set out e.g. in Gillies (2000, Ch. 3), in fact I do not intend my general theory of argument structure and evaluation to rule out that theory. Even if there are strictly logical inductive probabilities, *and* these have a consequence that the respective conditionals I refer to here are in fact, unbeknown to most of us, analytic. No harm is done to the cogency of an argument merely by stating among the premises a proposition that is in fact analytic. The only problem for my account of argument structure and cogency, even if that were the case, would be that it is slightly less economic than in principle it should need to be – if we were all logically omniscient and no–one was in any doubt about the analyticity of the respective conditionals.

among the premises that are to be acknowledged and evaluated prior to, or independently of, the relatively unproblematic question of *logical* adequacy.

Despite the shift, as distinct from relief, of burden in this regard, I believe that this line of approach is nevertheless of significant importance, since it clarifies the proper *logical* status of the divisive issue of inductive inferability. If I am right, this should *not* to be confused with the question of the logical adequacy of an argument, as it most commonly and traditionally has been. If we can agree at least this much, which seems on a reflection to be a quite *simple* point about inductive inference (a point moreover which I shall argue was not, as is commonly supposed, disputed by Hume) then perhaps we may at least begin to look in the right direction in seeking to resolve our differences about inductive inferability. If my arguments are good enough, then perhaps many more of us will no longer feel the need to divide ourselves into the traditionally *opposing* camps of the deductivist and the inductivist. If the proper structure of argument is as I shall argue it is, and the corresponding conditions for both premise and logical adequacy are as agreeable as I think they will be, once that proper structure is recognised, then all of us should be able to present our respective arguments to one another, without any distortion or omission of our intentions, in a logical format that none of us should have any particular difficulty endorsing – at least so long as we don't have any difficulty with the logical adequacy of deduction Of course philosophical problems have been raised with regard to deduction – one of which has indeed been claimed to be a precise analogue of the classical problem of induction. And I shall make some comment on the significance of such problems in the final section of the thesis.

In further mitigation for the above–mentioned shift of burden, in the latter part of this thesis I shall also attempt to offer some considerations in support of the view that more recent, and potentially more serious, problems related to Hume's might not be quite as intractable or hopeless as some philosophers would have us suppose. In particular, I shall comment briefly on the infamous 'new riddle' of induction posed by Goodman. Since the issue of particular concern with regard to argument cogency is the question of the logical adequacy of inductive arguments, the proper place to begin my argument is with an examination of the concept of induction and the fundamental problem that has traditionally been associated with it. Thus I shall approach this broader issue in the first instance via an interpretive analysis of Hume's famous, but all too commonly misrepresented, account of inductive inference and the problem he found with it.

PART ONE

HUME'S PROBLEM, AND THE CONCEPT OF INDUCTION

CHAPTER ONE
HUME'S PROBLEM FOR THE JUSTIFICATION OF INDUCTION: HIS CLAIMS, HIS ARGUMENTS, AND THEIR CONTEXT.

David Owen has recently argued that the classical arguments of Hume – which have commonly been interpreted as arguments for the conclusion that our inferences from what we have observed to what we have not observed are not, and could not possibly be, *justified* – have been misunderstood (Owen, 2000[7]). Owen claims that Hume does not argue for such an unjustifiability conclusion in these arguments, and that his apparent claims to that effect in the respective passages should be understood rather in terms of the negative *explanatory* claim that our beliefs in the unobserved are not *produced* by reasoning – thereby leaving open the possibility of justification. My preliminary aim in this chapter is to clarify the context and significance of Hume's traditionally supposed arguments for unjustifiability, and in particular to defend the traditional view that the arguments in question are *indeed* arguments for the *unjustifiability* of induction – in a particular sense of the term. Following these preliminary considerations, we shall examine the finer details of the unjustifiability thesis and Hume's argument for it, in the following chapter, with particular consideration of the common claim that Hume presents a problem for the validity of inductive inference.

The Humean concept of inductive inference

'Induction', in a traditional sense associated with certain issues addressed by Hume in his *Treatise of Human Nature* (1739) and *Enquiry Concerning Human Understanding* (1748), is generally characterised basically as inference (or argument) from the *observed* to the unobserved. For example, Wesley Salmon, in his classic textbook *The Foundations of Scientific Inference* (1966) describes the problem of inductive inference as

> the problem of determining whether the inferences by which we attempt to make the transition from knowledge of the observed to knowledge of the unobserved are logically correct (Salmon 1966, p6)[8]

[7] The main arguments of Owen in support of this claim are centred in chapter 6, p113–146, and his attempt to render his interpretation compatible with Hume's remarks in his Conclusion to the first book (*Treatise* , 1.4.7) is presented in chapter 9, p197–223.

[8] Note however that by 'logically correct' Salmon does not intend 'deductively valid' which is the standard of 'logical correctness' for deductive arguments. He sees explication of exactly what would

while acknowledging that determining the logical *structure* of 'inductive' inference, so construed, in Salmon's words 'the logical relationship between evidence and conclusion' is an essential preliminary to answering that question. Hume himself however made it clear from the outset that the defining feature of the type of inference with which *he* was concerned, at least in the context of his primary arguments on these issues, was *causal* inference:

> I shall proceed to examine ... Why we conclude, that such particular causes must *necessarily* have such particular effects; and what is the nature of the inference we draw from the one to the other (*Treatise*, Book 1, part 3, section 2, paras. 13–15)

However, the common emphasis on *observation*, rather than causation, as the essential foundation of inductive inference in Hume's theory is explicable in the light of Hume's view that our inferences relating to causation are founded on *experience*:

> as the power, by which one object produces another, is never discoverable merely from their ideas, 'tis evident *cause and effect* are relations, of which we receive information from experience, and not from any abstract reasoning or reflection. (*Treatise* 1.3.1.1.)

Hume describes the relevant aspects of our experience, and the relation between this and our inferences with regard to cause and effect, as follows:

> 'Tis ... by EXPERIENCE only, that we can infer the existence of one object from that of another. The nature of experience is this. We remember to have had frequent instances of the existence of one species of objects; and also remember, that the individuals of another species of objects have always attended them, and have existed in a regular order of contiguity and succession with regard to them. ... We likewise call to mind their constant conjunction in all past instances. Without any further ceremony, we call the one *cause* and the other *effect*, and infer the existence of the one from that of the other. (*Treatise* 1.3.6.2. Hume's emphases)

It is notable that one of the points Hume had already noted in his preliminary consideration of the concepts of cause and effect, is that the terms 'cause' and 'effect' are logically related. For example, he pointed out that those 'who say, that every effect must have a cause' are 'frivolous'

> because 'tis imply'd in the very idea of effect. Every effect necessarily presupposes a cause; effect being a relative term, of which cause is the correlative. But this does not prove, that every

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constitute the appropriate relation of 'logical correctness' for inductive inferences as an essential part of the problem.

being must have a cause; no more than it follows, because every husband must have a wife, that therefore every man must be marry'd. (*Treatise* 1.3.3.8.)

The implication of this, in the context of the inference described above (*Treatise* 1.3.6.2.) where 'Without any further ceremony, we call the one *cause* and the other *effect*, and infer the existence of the one from that of the other' is that *in so doing,* i.e. by *calling* them 'cause' and 'effect' – without first establishing any justification for so doing – we are effectively *assuming* (as distinct from proving) that the one will (and must) be followed by the other. And as we shall see, it is this key point that Hume proceeds to develop and utilise throughout the course of his analyses of the logical structure and epistemological credentials of inductive inference.

Comparison with other concepts of induction

Before proceeding much further though, first we ought to note at the outset that the term 'induction' or 'inductive inference' was not systematically employed by Hume, who generally used terms such as 'probable reasoning' and 'arguments from experience'; although the traditional use of the term may be traced to his suggestion in the *Enquiry* that from objects, which appear similar (in respect of features with regard to which, in our experience, they have been frequently and constantly conjoined with certain 'effects')

> we are *induced* to expect effects similar to those, which we have found to follow from such objects. (*Enquiry* section 4, part 2, para. 7, my emphasis).

We also need to note at the outset that Hume's basic account of the concept of 'inductive' inference needs to be distinguished from other and more recent senses in which the term has been used:

(i) *Humean induction v. non–deductive inference*

This Humean causation/observation–based concept of induction needs to be distinguished in particular from a more recent sense of the term, as typified by the text–book definitions of Skyrms and Swinburne:

> I shall say that an inductive argument is an argument which is *not deductively valid* but one in which, it is claimed, the premises 'make it reasonable' for us to accept the conclusion. (Swinburne 1974, p2, my emphasis)

> An argument is *inductively strong* if and only if it is improbable that its conclusion is false while its premises are true, and it is *not deductively valid* (Skyrms 1975, p7, my emphasis)[9]

What is notable about these definitions is that they omit any reference to foundation on observational premises and, in particular, that they presuppose (or stipulate) that an inductive argument *cannot*, by its nature, be deductively valid. I draw attention to the distinction mainly in order to alert the reader to the possible error of projecting such a relatively modern conception of 'induction', understood specifically in terms of *non–deductive* inference, onto their interpretation of *Hume's* statement about our causal expectations being 'induced'. As we shall see, it is by no means uncommon for commentators to fall into the error of regarding *Humean* induction as a special case of 'induction' in this anti–deductive sense.

On the face of it, Hume's claim that we are 'induced' to expect certain effects would not appear to demand anything other than the natural interpretation that something *makes* ('determines') or *inclines* us to, expect a continuation of the conjunction of certain types of event when they have hitherto (in our extensive or experimental experience) been constantly conjoined. In his discussion of the problem of induction, Hume considers in some detail *what* leads us to this expectation, i.e. by what *process* of 'inference' we *arrive* at this belief, and considers in particular 'whether we are *determin'd* by reason to make the transition', or by something else[10] (*Treatise* 1.3.6.4. my emphasis).

If there were any more dedicated *logical* significance that Hume intended to be attached to his use of the term 'induce' here, rather than the apparent equivalence to this *broad* use of the term 'determin'd', compatible with natural or psychological explanation as well as it is with logical explanation, we would certainly expect him to make more common use, and to provide some explication, of the term 'induce' in his analysis of the logic of this particular kind of inference. But unlike his key logical terms, such as 'demonstrative' and 'probable', there is no apparent replication or clarification of the term 'induce' in his extended analyses of the logical structure and justificatory credentials of this form of inference. On this natural interpretation of 'induce',

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[9] Corresponding accounts of inference may of course be had by substituting 'inference' for 'argument', and perhaps 'assumptions' for 'premises', and 'supposed' for 'claimed'.

[10] specifically, the alternative Hume specifies here regarding what 'determines' us to make the inference we do is 'a certain association and relation of perceptions'. However, Hume's psychological *explanation* of what makes or 'induces' us, to make the inference we do, is to be distinguished from his claims about the *logical structure* of the inference, which is our concern here. Thus the mentioned 'association of perceptions' is presumably meant to *explain why* we make the *presumption* that Hume goes on to insist that we make. Whereas all Hume requires with regard to the logical status within inductive inference of the proposition in question is the acknowledgement that it *is* a presumption.

Hume is here simply acknowledging that something (perhaps mysteriously) *causes* or *compels* us to make the inferences we do when we observe a constant conjunction of events, and wants to know whether what does so is the compulsion of compelling *reason*, or rational justification.

In order to pursue the explanation of *why* we make the inductive inferences we do – what makes or 'induces' us take such a the train of thought, and the associated question of whether the chain of thoughts or beliefs (about the relevant facts and the relations between them) that constitute the inference make up a satisfactory justification for the ensuing beliefs, it is of course a necessary preliminary for Hume to examine the *nature* and in particular the *logical structure* of inductive inference. This question of the logical structure of induction involves consideration of the logical status of the beliefs invoked in the inference – distinguishing in particular between those which are 'taken for granted', or 'presumed' and those which are justified in the course of the inference – and the inferential relations between them – in particular what is inferentially dependent on what (i.e. which beliefs would not be inferred, or held, without other prior beliefs). Our concern is with the latter logical and epistemological issues, or more specifically *Hume's* view of them, rather than with Hume's positive views about the psychological *explanation* of induction.[12]

(ii) *Russellian induction:*
sense–data v. the Humean presumption of the veracity of observation

Another notable variation on the concept of induction is that presented by Bertrand Russell in his essay 'On Induction' (1912), wherein our (private) 'sense–data' are taken to be the foundations of our inductive inferences, rather than our beliefs about the supposedly observed external objects that we naturally take to be associated with them.

> What things are there in the universe whose existence is known to us owing to our being acquainted with them? So far our answer has been that we are acquainted with our sense–data, and, probably, with ourselves. These we know to exist… But if we are to be able to draw inferences from these data … we must know general principles of some kind by means of which such inferences can be drawn. / The principle we are examining may be called the *principle of induction* (Russell 1912, p19 / p23)

[12] Hume's *negative* view about the explanation of inductive inference – that it is *not* to be explained by compelling *reason* for making the inferences, does of course fall within the scope of our logical and epistemological concerns.

In Russell's view, it would appear that the basic aim of induction is to 'extend our knowledge beyond the sphere of our *private* experience' (Russell 1912, p19, my emphasis).

Hume in contrast begins with the supposition that it is 'impossible to decide with certainty' whether the impressions (of observation) that arise from the senses 'represent nature justly, or be mere illusions'. What Hume more modestly concerns himself with in this context is the *inferences we draw* from our perceptions – 'whether they be true or false; whether they represent nature justly, or be mere illusions of the senses.' (*Treatise*, 1.3.5. para. 2). The question Hume addresses then, with regard to inductive inference, is basically as follows: even *leaving aside* the justificatory problems noted with respect to our observational presumptions, *why* do we draw the conclusions we do about the unobserved, and by what *process* of inference do we arrive at our conclusions? (*Treatise*, 1.3.2.13–15, above).

'Hume's problem':

Hume's claims and the traditional interpretation of them.

Although there may be scope for disagreement about the finer details of interpretation of Hume's classical account of what is generally believed to constitute a fundamental problem for the justification of inductive inference, there are a number of points in Hume's *Treatise* where he expresses, in terms that at least appear to be reasonably clear, the core claims of the *particular* problem of induction that has become his namesake. One of the core claims of what is (almost) universally referred to as 'Hume's problem' for inductive reasoning is Hume's claim that *we have no reason* to make the inductive inferences we do:

> *even after the observation of the frequent or constant conjunction of objects, we have no reason to draw any inference concerning any object beyond those of which we have had experience; …* [This principle] we have found to be sufficiently convincing, even with regard to our most certain reasonings from causation (*Treatise*, 1.3.12.20)

Moreover, when Hume says that we *have* no reason to draw these inferences, he doesn't *just* mean that we haven't thought hard enough about their justification, and haven't quite clarified, articulated, or identified the reasons that do or would support them. He means that the evidence we have could not *possibly* provide us with satisfactory reasons for believing the conclusions we draw from it. When we look at the details of his argument for the above claim it becomes apparent that the basic problem perceived by Hume in this regard is that it is *impossible* for us to establish any reason for believing the conclusions of such inferences – even in what appear to be the most compelling of cases (i.e. 'our most certain reasonings from causation', based on observation of the 'constant conjunction' of objects). Thus:

> even after experience has inform'd us of their *constant conjunction*, 'tis **impossible** for us to satisfy ourselves by our reason, why we shou'd extend that experience beyond those particular instances, which have fallen under our observation (*Treatise*, 1.3.6.11, my emphasis in bold.)

In short it would appear that Hume has objections to raise against any suggestion either that we *do* or even that we could *possibly* have or establish any (satisfactory) *rational justification* for drawing the inferences we do on the basis of what we have observed.

Owen's challenge to the traditional view

But is Hume's problem *really*, as it appears, about the *justification* of inductive inference, as is traditionally supposed? David Owen has recently argued that

> It is tempting to read into Hume a more current problem: given that we have such beliefs [in the unobserved], how, if at all are they justified? Hume's problem is more one of explanation than justification: given that we have such beliefs, what is their nature and how is it that we come to have them? (Owen, 2000, p118)

> I am not denying that there is a problem of induction that is largely concerned with justification. I am only claiming that it is not Hume's problem. (Owen, 2000, p139)

> The problem is how these beliefs are produced, not how they are justified. (Owen, 2000, p137)

More specifically, Owen claims that even *after* the passage that is generally regarded as containing the first and fundamental instance of Hume's basic line of argument against justifiability (including *Treatise* 1.3.6, quoted above)

> Hume has not yet *raised* the issue of warrant (Owen, 2000, p137, my emphasis)

Moreover, Owen makes it quite clear that not *only* does he believe that the traditional view – that 'Hume is saying … that conclusions of probable reasoning are without justification or warrant' – is *mistaken*, but he believes that that view is *absurd*:

This would be bizarre. ... To say that every instance of probable reasoning was fallacious would be to reject probable reasoning and would force us to treat Hume's frequent references to probable reasoning ... as mere instances of *facon de parler.* (Owen, 2000, p137)

As we shall see, in presenting his case for a radical re–interpretation of Hume's *apparent* arguments against our having any *justification* for our inductive inferences, Owen makes much of the broader *context* within which these apparent justificatory denials, and the arguments underlying them, are utilised. I shall argue that in attempting to support his apparently (and purportedly[13]) controversial interpretation, Owen instead falls guilty of conflating the *particular*, and in Hume's view psychologically devastating claim,[14] and the arguments *underlying* it that have came to be called '*Hume's problem*' – a problem for which Hume does not purport to have any solution[15] – with a further and less worrying claim that is *derived* from the justificatory problem – a problem regarding which he *has* a positive theory to offer – and which has *not* became his namesake.

In order to clarify these points, and to appreciate the background to Owen's challenge, let us first examine the broader context of the argument in the *Treatise* that is commonly regarded as constituting Hume's first and main argument for the unjustifiability of inductive inference.

The first unjustifiability argument and the use Hume makes of it.

The first argument Hume presents in support of his above cited 'no–reason–*to*–infer' conclusion (*Treatise*, 1.3.12.20) – relating to our 'most certain' causal inferences crops up in a somewhat earlier context. That initial and main component of his larger 'no–reason–*to*–infer' argument is introduced as *one* of the lines of argument that Hume uses in support of his answer to a subtly distinct question about the actual *process* of production of our 'most certain' causal inferences, i.e.

> whether we are determin'd *by* reason ... (*Treatise*, 1.3.6.4, my emphasis)

to make such inferences, or whether these beliefs are produced by some process *other than* the comprehension of compelling *reasons*. We may describe this question of the process of belief

[13] 'The point is controversial' (Owen, 2000, p118.)

[14] The significance for Hume of the justificatory problem is brought out in extensive detail in his Conclusion to Book 1 of the *Treatise* – discussed below.

production as Hume's *explanatory* problem, and we may call his negative answer to the explanatory question, of whether such non–observational convictions are produced *by* our understanding of compelling reasons for belief, the 'not–*by*–reason' conclusion.

In order first to establish that the explanatory problem *is* a problem, Hume shows that the traditional supposition that we *are* compelled by reason to make our causal inferences – on account of our acknowledgement of compelling reasons for believing the respective conclusions – is untenable. This is where the *justificatory* question comes in – whether we have any reason to believe the conclusions of our causal inferences. Hume argues that we couldn't possibly have satisfactory reason for drawing these inferences and so no proofs[16] for the conclusions of our causal inferences are even *possible*. He expresses this initial *unprovability* conclusion as follows:

> even after experience has informed us of their *constant conjunction*, 'tis impossible for us to satisfy ourselves by our reason, why we shou'd extend that experience beyond those particular instances, which have fallen under our observation. We suppose, but are never able to prove, that there must be a resemblance betwixt those objects, of which we have had experience, and those which lie beyond ... (*Treatise*, 1.3.6.11. Italics are Hume's emphasis. Underline is mine.)

On the basis of his negative answer to this justificatory question – specifically a question of provability – Hume then *proceeds* to the negative answer to the explanatory question – the 'not–*by*–reason' conclusion:

> When the mind, therefore, passes from the idea or impression of one object [which has fallen under our observation] to the idea or belief of another [which has not], it is not determin'd by reason ... (*Treatise*, 1.3.6.12)

Furthermore, Hume is quite clear in *introducing* this strategy of approaching the explanatory issue *via* consideration of the justificatory issue:

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[15] 'For my part, I know not what ought to be done in the present case. I can only observe what is commonly done; which is, that this difficulty is seldom or never thought of; and even where it has once been present to the mind, is quickly forgot' (*Treatise*, 1.4.7.7)

[16] There is some scope for debate about how strict a sense of 'proof' Hume is employing here. Although 'proof' *could* be interpreted as argument that warrants *certainty*, Hume is not so much concerned with the degree of *intensity* of our belief in this context as with the question of whether our belief in any case can be *justified* by reason. But even if provability were interpreted in a strong sense, it remains quite clear that whether or not a proposition is *provable* is a matter of (a particularly strong kind of) justifiability – not a matter of explanation.

> If reason determin'd us [to make inductive inferences], it wou'd proceed upon that principle, [the 'continuity' or 'uniformity' principle] *that instances, of which we have had no experience, must resemble those, of which we have had experience, and that the course of nature continues always uniformly the same.* In order therefore to clear up this matter [of whether we are determined *by* reason to make such inferences], let us consider all the arguments on which such a principle may be suppos'd to be founded … and see **whether they afford any just conclusion** of this nature. (*Treatise*, 1.3.6.14., italics Hume's, bold mine.)

In order to understand the significance of this passage we need to ask ourselves *how* (in Hume's view) would finding out whether any of the alternative candidate *justifications* for this principle are in fact genuine justifications clear up the matter of whether we are *determined by reason* when we make the inferences that *depend* on it? In other words, what is the connection between the justifiability of the principle on which our inductive inferences depend, and the explanatory question of whether we are determined by reason when we make those inferences? In this passage Hume appears to be suggesting that reason will determine us in the drawing of our conclusions *only if* the principles employed in the drawing of those conclusions are themselves justified. Hume's basic rationale here then seems to be that if no argument (or line of reasoning) we may have to offer in support of the continuity principle could justify our acceptance of that principle, then no argument employing that principle could possibly justify our acceptance of the conclusions that depend on it. The crucial link then between the justificatory and explanatory issues that Hume is exploiting here, is quite simply that if we cannot establish any line of reasoning that will *justify* the conclusions of our inductive inferences, then it cannot be the case that we are *compelled by reason* to draw those conclusions. And as we have seen above, such a reading of this introductory passage appears to be confirmed by the logic of Hume's ultimate and penultimate conclusions, relating respectively to the explanatory *and* justificatory questions.

The difference between these claims, and the direction of derivation, are quite clear from the fact that the converse implication does not hold. Supposing only that we *were* led to draw our inductive inferences by some influence *other than* the comprehension of satisfactory or compelling reasons, *that* in itself *would* appear to leave open the possibility that we might nevertheless be able to justify those inferences retrospectively. But in contrast the *impossibility of justification*, or the lack of any reasons *to* make the inference, *rules out* the hypothesis that we have in fact been compelled *by* our powers of reason to draw them. And it is precisely this incompatibility that is the *key* to this *particular* argument (the argument from *unjustifiability*) that Hume offers in support of his not–by–reason conclusion. Hume's argument here, in contrast to what Owen suggests, quite plainly does *not* leave 'wide open' the justificatory question (Owen, 2000, p146).

Hume also has *additional* arguments to support that 'not–*by*–reason' conclusion, besides the 'no–reason–*to*–infer' line of argument, and these are particularly prominent towards the end of the relevant section in his later *Enquiry*.

The distinction between justifiability and justness

It is natural and common to interpret the no–reason–to–infer conclusion as implying that we are not and could not possibly be rationally *justified* in making the inductive inferences we do. However we need to take note of a distinction between two possible *interpretations* of justifiability, on only one of which this appears to be a direct implication of Hume's claim here. The sense of 'justifiable' on which this appears to be a direct implication is a sense that may be associated with *provability*, since he paraphrases his conclusion with the claim that

> We suppose, but are never able to prove, that there must be a resemblance betwixt those objects, of which we have had experience, and those which lie beyond the reach of our discovery. (*Treatise*, 1.3.6.11)

In *this* sense of justifiability, a belief is justifiable iff it is possible to *provide*, i.e. to set out or present (or perhaps at least to comprehend) satisfactory reasons for holding that belief. However the term is sometimes used in a stronger sense, whereby when someone denies that a belief (or an action) is justifiable, they intend to deny that the belief (or action) is *just* – i.e that it is a belief or an action that we *ought* to (or at least may) hold (or take) – not merely that we are able to *provide*, to set out or identify, satisfactory reasons for holding the belief or taking the action. Although some interpreters might want to take Hume's conclusion in the latter stronger sense, on examination of his argument for the conclusion it would not appear in itself to sustain such a strong conclusion, since Hume simply proceeds by eliminating any possible lines of proof, or satisfactory argument, for the respective conclusions. If indeed he does intend the stronger 'not–possibly–just' conclusion, as distinct from the weaker 'no–possible–proof' conclusion he *overtly* presents here, he offers no bridging arguments for the further step.[17] The important point of distinction with regard to our appraisal of Owen, is that this is a claim relating to *the possibility of justifying* – in the weaker sense of proving – the conclusions of our

[17] Of course he might be assuming that such an implication is too much of a platitude to warrant mention, but since he provides no argument for it we need not concern ourselves in our analysis of Hume with a critical appraisal of any such inference.

inductive inferences: It is *not* merely the claim that we *don't proceed* in our inductive inferences by justifying or proving the conclusions, and it does *not* leave open the possibility that such logical justification or proof might be attainable.

In summary then the main preliminary points we need to note, before attending to the details of Owen's objections, are:

(a) This initial occurrence of a negative *justificatory* claim (concerning the possibility of proof), and the argument for it, contributes to the later and *more general* 'no–reason–to–infer' conclusion (*Treatise*, 1.3.12.20, above) but is related specifically to the possibility of proof for our 'most certain' *causal* inferences.

(b) This initial occurrence of a justificatory 'no–reason–to–infer' argument arises *within the broader context* of consideration of a prior explanatory issue, and constitutes part of a *larger* argument for a negative 'not–by–reason' conclusion to that explanatory question.

(c) The *general*, and profoundly problematic, negative *justificatory* claim that we have *no reason to draw any* inductive inferences, and the arguments Hume presents in support of it (which *include* the initial unprovability argument for causal inference) is what has become generally referred to as 'Hume's problem'.

If Owen has a substantial *interpretive* point[18] to make on this matter, as he alleges, it is not about *which* argument here *ought rightfully* to be called 'Hume's problem', it is about the proper *interpretation* of the particular argument that *is* called 'Hume's problem', and which is commonly regarded as an argument for the claim that we cannot possibly establish any logical justification for our inductive inferences.

For convenience of reference I have provided an illustration of the broader context in which this initial unprovability argument arises, in the form of summary quotations, in figure 1.

Does the initial unprovability thesis relate *only* to causal inferences?

Referring to figure 1, it is notable in the argument Hume presents here for unprovability with respect to causal inference, that although he does not make explicit the *general* 'no–reason–to–infer' conclusion (for *all* inductive inferences), that stronger conclusion would nevertheless appear to be *derivable* from this line of argument granted premise (3) – where Hume claims, in line with his prior considerations, that only the relation of causation can 'lead us beyond' the immediate evidence of our memory and senses: If *all* inductive inferences require *causal* inference and causal inference cannot be justified, then it would seem to follow that *no* inductive inference could be justified. We shall find that this interpretation is confirmed when Hume later turns to the issue 'Of the probability of chances' (*Treatise* 1.3.11), but since he has made explicit his view that all inductive inferences depend on causal inferences, it would seem that Hume is content for the moment to focus on this fundamental case in its own right, granted the clear implication that if *this* kind of inductive inference cannot be justified, none can.

Of course the stronger, more general, conclusion might still have been intended to be implicit in Hume's argument here. The significance of '*even* after experience … of constant conjunction' in the unprovability conclusion (7) might well be taken to imply something like 'in this strongest case and therefore in the weaker dependent cases too'. But that semantic question is relatively unimportant here, since it seems to be sufficiently clear that the stronger conclusion is a valid implication of Hume's argument, and that it is understood by him to be so at this point although the full clarification of the point is reserved until later. In any case however, the essential issue at stake between Owen and the traditionalist here is not so much about whether the (supposed) argument for the unjustifiability of inductive inference in general is *complete* at this stage, but simply whether it is an argument about the *unjustifiability* of the type of inference under consideration as distinct from merely the *explanation* of the type of inference under consideration. Thus, although this issue will be relevant to our response to *one* of Owen's supposedly controversial claims, our defence of a justificatory interpretation of this early argument for unprovability will *not* depend on this issue of completeness.

What is clear though is that this argument is *at least* intended to apply to (purely) *causal* inductive inferences, and that Hume does later supply further arguments that relate specifically to weaker and dependent forms of inference. In this case, for our purpose, we may regard this early argument as an argument relating specifically to causal inference, albeit carrying broader implication for inductive inference in general. It is notable that Hume begins and ends the

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[18] as distinct from a relatively trivial evaluative point, suggesting for example that the one was 'more

section in which the argument falls with explicit reference to 'the inference we draw from cause to effect' and 'our reasonings from that relation [of cause and effect]' respectively (*Treatise*, 1.3.6. 1&15.). Examining the immediate context of the argument, as illustrated in the text boxes below, it appears then that it is with regard to our *causal* inferences that Hume is addressing his explanatory question in this passage, and so it is natural that the unjustifiability line of approach to elimination of the 'by reason' hypothesis at this point in his argument should be related to the same type of inference.

Summary quotation of the initial unprovability argument in context.

THE CONTEXT OF THE INITIAL 'NO–REASON–*TO*–INFER' ARGUMENT (FOR CAUSAL INFEERNCES):– In *Treatise*, 1.3.6. (para. nos. in subscript)	The '*by*–reason?' question: Since it appears, that the transition from an impression present to the memory or senses to the idea of an object, which we call cause or effect, is founded on past *experience*, and on our remembrance of their *constant conjunction*, the next question is, whether experience [and our remembrance of their constant conjunction] produces the idea [of the cause or effect] by means of the understanding or [by] the imagination; whether we are determin'd by reason to make the transition, or by a certain association and relation of perceptions?$_4$
THE STRATEGY OF APPROACHING THE EXPLANATORY ISSUE *VIA* CONSIDERATION OF *JUTSTIFIABILITY*:–	If reason determin'd us, it wou'd proceed upon that principle, [the principle of 'continuity' or 'uniformity'] *that instances, of which we have had no experience, must resemble those, of which we have had experience, and that the course of nature continues always uniformly the same.* In order therefore to clear up this matter, let us consider all the arguments on which such a principle may be suppos'd to be founded; and as these must be deriv'd either from *knowledge* or *probability*, let us cast our eye on each of these degrees of evidence and see whether they afford any just conclusion of this nature.$_4$

important' to Hume than the other. However we shall also consider this weaker suggestion below.

THE ARGUMENTS:–

(For unprovability, and for the 'not–*by*–reason' claim)

THE '*NOT-BY-REASON*' ARGUMENT (FROM UNPROVABILITY)

THE ARGUMENT *FOR* UNPROVABILITY (w.r.t. CAUSAL INFERENCE)

(1) If reason determin'd us, it wou'd proceed upon that principle, [the principle of 'continuity' or 'uniformity'] *that instances, of which we have had no experience, must resemble those, of which we have had experience, and that the course of nature continues always uniformly the same.*[4]

(2) there can be no demonstrative arguments[1] to prove, *that those instances, of which we have had no experience, resemble those, of which we have had experience.*[5]

'Tis ... necessary, that in all probable reasonings there be something present to the mind, either seen or remember'd; and that from this we infer something connected with it, which is not seen or remember'd.[6] [Hume's concept of probable inference]

(3) The only connexion or relation of objects, which can lead us beyond the immediate impressions of our memory and senses, is that of cause and effect; ... 'tis the only one, on which we can found a just inference from one object to another[7]

(4) The idea of cause and effect is derived from *experience*, which informs us that, such particular objects, in all past instances, have been constantly conjoin'd with each other: And as an object similar to one of these is supposed to be immediately present in its impression, we thence presume on the existence of one similar to its usual attendant[7]

[But] *Why from this experience* [should[2]] *we form any conclusion beyond those past instances, of which we have had experience?*[10] [Interjected question]

(5) [We have seen that] probability is founded on the presumption of a resemblance betwixt those objects of which we have had experience, and those of which we have had none[7] [From the *second* clause of (4)]

(6) therefore 'tis impossible this presumption can arise from[3] probability [7]

(7) Thus ... even after experience has inform'd us of their *constant conjunction*, 'tis impossible for us to satisfy ourselves by our reason [i.e. *either* by demonstration *or* by probable argument], why we shou'd extend that experience beyond those particular instances, which have fallen under our observation. We suppose but are never able to prove, that there must be a resemblance betwixt those objects, of which we have had experience, and those which lie beyond[11] [From (2) and (6)]

(The *'no–reason–to–infer'* or more specifically 'unprovability' conclusion – for our 'most certain' causal inferences)

(8) When the mind, therefore, passes from the idea or impression of one object to the idea or belief of another, it is not determined by reason[12] [From (1) and (7)]

(The '*not–by–reason*' conclusion)

NOTES ON 'THE ARGUMENTS'

[1] I have omitted all details of the arguments against demonstrability, since this is the least contentious part of Hume's argument, and it is the explicit reference to the justificatory issue of *provability* that is my concern here.

[2] This question actually occurs in the context of an interjection into Hume's main argument, following his specification of the problem of circularity, where he is 'examining' a purported line of justification for causal inference to see whether it yields a conclusion which is indeed 'built on *solid* reasoning'. I present the question at *this* point in Hume's main *background* argument, immediately *prior* to his specification of the problem of circularity, because I think it is also applicable at this particular point: It helps to clarify Hume's introduction of the problem of circularity by focussing attention on the significance of his use of the term 'presume' rather than 'infer' in the preceding statement. It would seem that his use of 'presume' here is intended to suggest that the specified basis of the inference does *not*, in itself, *justify* the conclusion – a point which is highlighted by the question I have brought forward. My insertion of the word 'should' here, suggesting that this *is* a question of justification (rather than inserting 'do', which would suggest a question of explanation) is based on (a) the immediate context of the *actual* point of occurrence of the question (specified above): also note that Hume adds here that if this question is answered in the same manner as previously it would become apparent that the foregoing reasoning had no *just* foundation' (my emphasis), (b) Hume's subsequent use of the word 'shou'd' in the 'no–reason–to–infer' conclusion (7) that he immediately draws from this, and (c) the immediate context within which I suggest the question also applies (as described above).

[3] Hume is clearest about the perceived circularity underlying this step of inference in the *Enquiry*: 'To endeavour … the proof of this last supposition [of continuity] by probable arguments … must be evidently going in a circle, and taking that for granted, which is the very point in question.'. (*Enquiry*, 4,2,30). It may well be that, at this point in the *Treatise*, Hume is leaping to the explanatory or 'process' conclusion (with respect to probable arguments) with his use of the term 'arise from' rather than 'be proven by', and that he is suppressing the apparent (and intermediate) unprovability implication. But in any case it becomes apparent (from (7)) that he does believe he *has* established *unprovability* with respect to probable arguments as well as with respect to demonstration, and so, even if that *were* the right interpretation of 'arise from' the line of implication at this point would appear to be that probable reasoning cannot explain the presumption of continuity *because* dependence excludes the possibility of any (non–circular) probable *proof* of continuity.

The traditional interpretation of the unprovability argument

What I have presented here is a traditional interpretation of Hume's early argument for the unjustifiability of casual inference – which provides a fundamental part of the support for his general 'no–reason–to–infer' conclusion relating to inductive inference in general – along with clarification of the broader explanatory issue within which context this particular argument arises. Although there is scope for the view that Hume's *entire* argument for the unjustifiability of induction is not *complete* at that early stage, this is nevertheless, as is traditional, a *justificatory* interpretation of the argument that Hume employs at this early stage: Even that early argument for the unprovability of the conclusions of our causal inductive inference is an argument about logical *justifiability*, and specifically about the *possibility of proof.* The unprovability thesis that Hume establishes in that early argument is *not* in itself a thesis about how our inductive beliefs are or are not *produced.* The negative explanatory conclusion – which *is* about belief production – is a further and weaker conclusion that is derived *from* the famous unprovability thesis.

It does not appear that there is any case to be had here for a denial that that early argument for unprovability concerns the logical *justifiability* of causal inferences, and that it forms the essential basis of Hume's later claim concerning the justifiability of *any* inductive inferences. The fact that the early unprovability argument is initially utilised to form part of a larger argument concerning the explanation of causal inference in no way undermines the justificatory interpretation of the unprovability conclusion that is derived therein, or the premises on which that conclusion is based. In fact, in order to make sense of this *particular* line of argument for the explanatory 'not–by–reason' conclusion, the justificatory interpretation of the unprovability *basis* for that conclusion appears quite clearly, to be the only natural and reasonable interpretation.

Nor is there any case for denying that this early unprovability argument constitutes *an essential part* of the support for Hume's later and more general 'no–reason–to–infer' conclusion. In fact we have seen that he explicitly refers retrospectively to the earlier argument *as* a fundamental part of the support for his later claim.

[20] i.e. in the initial form Stove gives them, on p31.

Stove's misinterpretation of Hume's psychological claims.

David Stove (1973, p33) apparently failed to appreciate the strategic position of Hume's justificatory argument as an argument *within* a larger argument concerning the psychological explanation of induction, and consequently misinterpreted the overtly psychological references where these arise in the relevant passages. In this regard it is notable that instead of distinguishing between the two subtly different conclusions set out in (7) and (8) above (*Treatise*, 1.3.6.11 and 12), Stove conjoins the two in a *single* conclusion that *reads* like the psychological conclusion

> (j) Even after we have had experience of the appropriate constant conjunction, it is not reason (but custom, etc.) which determines us to infer the idea (e.g. of heat) from the impression (e.g. of flame). (Stove, 1973, p31)

but which he insists is to be *interpreted* in justificatory terms as

> (j) All predictive–inductive inferences are unreasonable. (Stove, 1973, p45)

The situation is similar with respect to his account of a purported intermediate conclusion to a supposed '*Stage I*' of Hume's argument:

> (d) The inference from the impression to the idea, prior to experience, is not one which reason engages us to make. (Stove, 1973, p31)

Again this reads like a psychological claim, but Stove ultimately interprets it as an 'evaluative' claim equivalent to

> (d) All *a priori* inferences are unreasonable. (Stove, 1973, p45)

Stove makes explicit and justifies this interpretation as follows:

> The reason why (j) and (d) require at least some translation at present[20] is that they appear on the surface to be propositions of a kind which *it is certain they really are not.* Hume's two conclusions appear to be factual, and in particular, psychological propositions: as though (j) for example, were Hume's answer to a causal question, 'What faculty of the mind is it which is at work in us when we make predictive–inductive inferences?' But *to any philosophical reader of Hume it will be obvious* that this appearance is misleading. Here, at any rate, Hume's interest in

the inferences he discusses is not empirical and psychological, but rather the kind of interest which a philosopher usually takes in inferences: viz. an evaluative, and in some sense, logico–philosophical interest. In the conclusions (j) and (d) as they stand, we have in fact just another instance of what we know to be true in general of Hume (as of most philosophers between the seventeenth and twentieth centuries) that he asserts logico–philosophical theses in the guise of remarks about the constitution of the human mind.

> ... *there can be no doubt* that Hume intends *by (j)* an extremely unfavourable evaluation of the inferences which are its subject. After all, (j) *is* that famous sceptical conclusion which Hume came to about inductive inferences (Stove, 1973, p31, my emphases.)

As I have indicated by highlighting with italics, Stove's argument here for the conclusion – that these apparently psychological claims *aren't* in fact so intended, but are rather logical evaluations – rests heavily on appeal to prior conviction, rather than on close examination of the textual context. In this regard, the basic failings of Stove's approach become all too obvious; Stove avoids employment of *actual* quotations from Hume's argument in his initial representation of it, and avoids any close analysis of the immediately surrounding context in which it arises. Once the finer details of what Hume actually says in these passages, and of their context, are carefully attended to it becomes apparent that Hume *is* indeed concerned here with the question 'What faculty of the mind is it which is at work in us when we make predictive–inductive inferences?'. In fact Hume's ultimate conclusion in the relevant passage of the *Treatise* is precisely that the mind is not determined by reason to make inductive inferences, but by the operation of the *imagination*:

> When the mind, therefore, passes from the idea or impression of one object to the idea or belief of another, it is not determined by reason, but by certain principles, which associate together the ideas of these objects, and unite them *in the imagination*. Had ideas no more union in the fancy than objects seem to have to the understanding, we cou'd never draw any inference from causes to effects, nor repose belief in any matter of fact. (*Treatise*, 1.3.6.12, my emphases)

Since Hume also immediately proceeds to explain *how* he thinks the faculty of the imagination (as distinct from the faculty of reason) induces the mind to form the expectations it does, Stoves claim that Hume's *apparent* concern with the question of *which faculty of the mind* is in operation here is in fact illusory is quite incredible. Likewise with the *Enquiry*:

> If the mind is not engaged by argument to make this step, it must be induced by some other principle ... What that principle is may well be worth the pains of enquiry.

> … Whenever any object is presented to the memory or senses, it immediately, by the force of *custom*, carries *the imagination* to conceive that object, which is usually conjoined to it (*Enquiry*, 5.1.2, 5.2.2.)

Similarly in the Abstract:

> 'tis *custom* alone, not reason, which determines us to make [experience of effects] the standard of our future judgements. (*Abstract*, 25, my emphasis)

> The presence of this visible object, and the constant conjunction of that particular effect, render the idea different to the *feeling* from those loose ideas, which come into the mind without any introduction. …
>
> … whatever name we may give to this feeling, which constitutes belief, our author thinks it evident, that it has a more forcible effect on the mind than fiction and mere conception. This he proves by its influence on the passions and on the imagination; which are only moved by truth and what is taken for such. (*Abstract*, 21, 22.)

It seems quite incredible to suggest that these are *not*, as they appear, psychological conclusions rather than logical conclusions, as Stove suggests they are. But they are ran together with Hume's logical conclusions in these passages simply because, as Hume explains at the outset (*Treatise*, 1.3.6.4., figure 1 above), his strategy is to approach the psychological issue *via* initial consideration of the logical issue, leading thereby to elimination of the psychological hypothesis that we proceed *by* employment of our faculty of reasoning and the establishment of proof.

Stove notes (p34) that there are points where Hume does express his claims in overtly logical or justificatory terms (in this case quoting *Treatise*, 1.3.12.20, from a later point in the *Treatise* where Hume reiterates and comments on his earlier justificatory conclusions), but still fails to appreciate that these actually *are* subtly different claims to the overtly psychological claims that arise within the passages where the justificatory argument occurs. Thus, despite the fact that Hume does satisfactorily distinguish his psychological and justificatory conclusions, both with regard to his advance account of the strategy of his larger (psychological/explanatory) argument, and with regard to the terminology he employs in his expressions of the respective points, Stove insists on conflating these claims with the result that when the *actual* evaluative (logical or justificatory) conclusions are (directly and clearly) expressed as such he confusedly describes them as 'having been shorn of their usual pseudo–psychological air' (Stove, 1973, p34).

Stove suggests, in the above passage, that it was not uncommon in Hume's time for logical points to be expressed in psychological terms. But of course that point could hardly be maintained without also acknowledging that it will nevertheless usually be possible to *distinguish* by the context, between cases where a genuine psychological claim is being made and cases where a psychological turn of phrase *is* employed in a sense that is indicative of a logical point. But, as we have seen, it is clear from the present context that Stove has failed to make the relevant distinction in this case. In the penultimate sentence of the above passage Stove claims that 'there can be no doubt that Hume intends by (j) an extremely unfavourable evaluation of the inferences which are its subject.' (Stove, 1973, p31). In subtle but significant contrast, what does seem clear from this context is that Hume does indeed intend (and express) an unfavourable evaluation of these inferences, but that *(j)* – or at least Hume's actual statement that approximates to (j) (i.e. *(8)* as distinct from (7) in my figure 1 above) – is *not* the statement that is intended by Hume to express that evaluative/logical conclusion. (j) is, as it appears, a *psychological* conclusion that is inferred *from* the immediately prior justificatory conclusion.

Owen's misinterpretation also appears to be due to a failure to appreciate that what we have here is an argument (the famous justificatory one) *within* an argument (the less famous psychological one) – not a simple argument which is just about one or the other issue. But in Owen's case, the justificatory claims and conclusions that arise within the core justificatory argument are mistakenly interpreted in terms of the psychological issue that is the subject of the broader argument that Hume builds on the basis of the initial justificatory conclusion in this passage, rather than the other way round as in Stove's misinterpretation.

Owen's mistaken perception of the traditional interpretation of Hume

Owen points to a number of points in the text of the *Treatise* where Hume expresses a concern with the *transition* from impressions of the observed to ideas of the unobserved, and the question of 'whether we are determin'd *by* reason' or by something else when we make this transition. And he alleges that

> the traditional view requires us to treat all this [i.e. the references to the question of how our beliefs are *produced*] as a concern with the justification of a belief whose existence is somehow unproblematic. (Owen, 2000, p138)

But that is simply not true. The undeniable fact that the question of *how* our beliefs are produced *is* a major concern of Hume within Book 1 of the *Treatise* is neither news nor controversial for the traditional interpreter of Hume. In fact, it would be difficult to find any *other* interpretation of Hume's various questions claims and theories that are referred to quite explicitly as questions claims and theories '*Of the causes of belief*' (Section title, *Treatise* 1.3.8).

Nor, for anyone who actually reads Hume's surrounding text, should there be any question about the fact that the unprovability thesis is actually argued and used *in order to support* the further and weaker conclusion that we *don't* make our inductive inferences on the basis of proofs. It is quite clear within the text that Hume *immediately* makes the inference *from* the unprovability conclusion *to* the weaker conclusion that our fundamental inductive belief (the belief in the continuity of causal regularities) *isn't* based on proof and is simply taken for granted. In the version in the *Abstract* of the *Treatise*, he even runs the two propositions together in a single sentence, as this final step of inference seems to be so obvious:

> This therefore is a point, which can admit of no proof at all, *and which we take for granted without any proof.* (*Abstract* of the *Treatise*, para.14, my underline, my italics)

Having eliminated these potential points of confusion, and erroneous allegations by Owen, as to what precisely he thinks the difference might be between his own and the traditional interpretation of the unprovability argument, it would seem that we still need to identify some substantial claim which Owen believes to be representative of his position and which, once his terms of reference are understood, the traditional interpreter of Hume actually *would* find objectionable.

An open question? Hume's extension of his argument to 'the probability of chances'

However there *are* some explicit points that Owen makes on which the traditionalist *would* disagree. As we have noted, Owen claims that even *after* the specification of the unprovability argument, and the ensuing elimination of the 'determined–by–reason' production hypothesis:

> Hume has not yet *raised* the issue of warrant[21] (Owen, 2000, p137, my emphasis)

Similarly, but less strongly, in the closing statement of his conclusion Owen says

[21] Presumably Owen intends this as a synonym of 'justification', since the latter is what he originally claimed to be at issue between himself and the traditionalist. In any case what matters with regard to the question of the controversiality of his view is whether he intends this statement about warrant in some sense in which traditionalists in general, or at least in the main, would want to deny it.

> Far from being settled by the end of Hume's negative argument, the question of the warrant of probable reasoning remains wide open. (Owen, 2000, p146)

Considering the stronger claim first, so long as Owen does intend 'warrant' in its normal sense as synonym for 'justification', it would seem that he is quite mistaken if he thinks that Hume does not even *raise* the question of warrant in his lead up to the unprovability argument. As we have noted, and as illustrated in 'The Strategy' section of figure1, Hume makes it quite clear that he intends to approach the issue of belief production, and specifically the 'by reason' hypothesis, *by consideration of the question of justifiability.* He explains quite explicitly that he intends to examine each type of argument on which it might be supposed that the principle of continuity has been established by logical reasoning – in order to see whether either type of argument is *capable* of providing rational *justification* for such a conclusion (*Treatise*, 1.3.6.4). The intent here is clearly that if we *can* provide rational *justification* for this belief, then that might well explain *why and how* we have come to believe it. But if we can't, then of course it won't.

What then of Owen's weaker claim here, that the question of warrant remains *open* after this early argument relating to causal inductive inference? We have already noted that, even on a justificatory interpretation, the justificationist might want to agree that Hume's justificatory argument is not strictly *complete* at this early stage, since it is designed to relate specifically to our 'most certain' inductive inferences from causation, and in that respect the issue might appear to remain *to some extent* open, with regard to *weaker* probabilistic arguments. However, we have also noted that Hume appears to think that the more general justificatory denial is, on the basis of his considerations at this stage, a foregone conclusion, since he has already argued that *all* our inductive inferences are based on cause and effect. And indeed this is a point Hume clearly confirms when he later takes up the issue of completeness[22] with regard to '*the probability of chances*' (Section heading, *Treatise* 1.3.11.1), and 'that evidence which is still attended with uncertainty' (*Treatise* 1.3.11.2) where he explains how chances depend on association with causal factors that constrain the relevant range of possibilities:

> 'tis impossible for us to conceive this combination of chances, which is requisite to render one hazard superior to another, without supposing a mixture of causes among the chances, ... Where nothing limits the chances, every notion that the most extravagant fancy can form, is upon a footing of equality; ... Thus unless we allow, that there are some causes to make the dice fall, and preserve their form in their fall, and lie upon some one of their sides, we can form no calculation concerning the laws of hazard. (Treatise 1.3.11.6)

[22] 'in order to bestow on this system its full force and evidence' (*Treatise* 1.3.11.1)

So, despite his needing to fill in the story in this regard, it becomes quite clear at this point that on Hume's understanding of the situation, having shown that our causal inferences are unjustifiable, he had in fact done enough to establish that even our expectations based on probability in the sense of chance were likewise unjustifiable.

Furthermore, in this regard it also needs to be noted that it is not only the justificatory issue that requires further *clarification* after the initial argument: Hume also acknowledges that his *explanatory* argument is to some extent incomplete at this stage, and indeed in this regard he proceeds by essentially the same strategy in the case of inference relating to chance as he did in the case of causal inference:

> we may repeat all same arguments we employ'd in examining that belief, which arises from causes; and may prove after the same manner, that a superior number of chances **produces** our assent neither by *demonstration* nor *probability*. (*Treatise*, 1.3.11.7, my emphasis in bold)

Once again Hume employs a negative *justificatory* argument, *in order to reach* this negative explanatory conclusion – arguing that

> *'tis impossible to prove* with certainty that, that any event must fall on that side where there is a superior number of chances (*Treatise*, 1.3.11.7)

and furthermore that considerations of probability, interpreted in terms of chance, can provide us with no more reason to expect any particular outcome than can a tautology:

> when we say 'tis likely the event will fall on the side, which is superior, ... we do no more than affirm, that where there is a superior number of chances there is actually a superior, ... which are identical propositions, and of no consequence. (*Treatise*, 1.3.11.8).

Having established *in* these points that we *cannot possibly justify* our beliefs or expectations about matters of chance by logical reasoning, Hume as before immediately concludes that those beliefs cannot have been *produced* by such logical considerations. And following this, in just the same way, Hume proceeds to develop his positive account of how our beliefs relating to chance *are* produced. In this case it is quite clear that any objection to the *justificatory interpretation* of the earlier argument for unprovability, on the basis that the justificatory issue is *not entirely settled* at that point would equally apply to Owen's suggestion that it should then instead be interpreted as an *explanatory* argument about how our beliefs are produced, since that question *equally* is not entirely settled at that point (if not more so, considering Hume's point relating to the decisiveness of the case for general unjustifiability on the basis of the result relating to causal inference).

This should in any case be clear in virtue of the above–noted *justificatory dependence* of his production theory on his negative answer to the justifiability question within this particular line of argument for the 'not–by–reason' conclusion – i.e. arguing *from* the impossibility of logical justification to the negative explanatory thesis that we do not *acquire* our inductive beliefs by logical justification. In view of this it is clear that, as far as *this line of argument* for the negative explanatory thesis is concerned, if Hume's argument for the justificatory thesis – that it is impossible *to* logically justify our inductive inferences – did leave us with any remaining doubt about *that* impossibility, then that line of argument would fail to clinch the case for the negative explanatory conclusion that *therefore* we cannot have *acquired* our inductive beliefs by any such process of logical justification.

Hume's explanatory theory, and the question that still needs to be answered

Hume's positive view of the psychological process of inductive inference is that this propensity or inclination to believe (or expect) what we do not see is a result of the operation of experience and habit on the imagination, influencing the forcefulness or intensity of our ideas:

> Experience is a principle, which instructs me in the several conjunctions of objects for the past. Habit is another principle, which determines me to expect the same for the future; and both of them conspiring to operate upon the imagination, make me form certain ideas in a more intense and lively manner, than others, ... Without this quality, by which the mind enlivens some ideas beyond others (which seemingly so trivial, and so little founded on reason) we cou'd never assent to any argument, nor carry our view beyond those few objects, which are present to our senses. (*Treatise*, 1.4.7.3)

Even with regard to 'the most usual conjunctions of cause and effect' *Hume* concludes

> the ultimate principle, which binds them together, ... proceeds merely from an illusion of the imagination; (*Treatise*, 1.4.7.6)

Owen suggests that

> Hume does *begin* to face the question of warrant at this point [section 7 ('Conclusion of this book')], *and gives the beginning of an answer* (Owen, 2000, p211, my emphases)

But for *Hume*, at this stage in his analysis

> the question is, how far we ought to yield to these *illusions*. ((*Treatise*, 1.4.7.6., my emphasis)

not how far we might still be able to establish some logical justification or proof that these 'illusions' are *true*. The situation now facing Hume is this: since we have seen that it is not possible to provide any logical justification for the extra–observational beliefs produced in us by the processes of nature – how far should we be prepared to go along with these natural belief–inducing processes? Should we simply accept *whatever* we are naturally inclined to believe, or should we sometimes resist our natural belief inclinations – and if so under what circumstances? Thus, not only is Owen's claim here quite clearly a misinterpretation of the question that Hume is *now* struggling with, but Owen's failure to realise that Hume has long since ruled out the possibility of logical justification prevents any proper understanding of the background context that *makes sense* of the question Hume is now considering.

The 'textual evidence' for Owen's view:
Owen's conflation of content with context

Owen suggests (p138) that there is 'ample textual evidence' for his view that Hume does not even *raise* the question of justification in the passage we have examined. The first piece of 'evidence' he offers is that of Hume's predominant concern with the question of 'whether we are determin'd by reason' to move from our observations to our non–observational beliefs, 'or by a certain association and relation of perceptions'. But we have already dealt with the question of *which* of the arguments Hume presents is the subject of 'Hume's problem', in the sense in which this phrase is generally intended – the argument which the traditional interpreter *believes* to be intended by Hume as an argument for unjustifiability. And we have noted that the arguments that form the basis of Hume's denial of justifiability are presented *within the context* of Hume's arguing for a negative conclusion to that broader question of belief production.

In view of this it would appear that Owen's perception of these references to the issue of belief production as evidence for his view that Hume *does not even raise*, let alone answer, the issue of justification at this point amounts to nothing more than a fallacy of conflating the *content* of

Hume's unjustifiability claims (and his argument *for* them) with the broader *context* in which they arise.

The use Owen makes of the second quotation he takes to support his view further illustrates the type of interpretive error he is making in this regard:

> In the *Abstract*, Hume says: "However easy this step may seem, reason would never, to all eternity, be able to make it" ... Hume's point is, 'How *do* we make this step?'
>
> (Owen, 2000, p138, my emphasis, my double quotation marks to mark the *actual* quotation of Hume, as distinct from Owen's interpretation in single quotation marks)

The actual *content* of Hume's statement here, as distinct from the broader use he makes of it, is of course just what Hume *says* in the statement. Although there is some play with language here, in that Hume talks about whether '*reason*' would be able to make the inference, it is quite clear that what he is talking about is whether *we*, however long and carefully we thought about it, could *possibly* make the inference by (satisfactory) logical reasoning. This is of course much stronger than simply suggesting that we *don't* make such inferences by logical reasoning when we make them, and in contrast to Owen's view, clearly *rules out* the possibility that nevertheless we *might eventually* be able to justify such inferences by logical reasoning.

Of course Hume does go on to consider by what process we do proceed to such beliefs. For those of us who always thought that there *were* good reasons for making the inductive inferences we do – and that we made them *because* we *understood* that there were good reasons for doing so – the question of origin *becomes pertinent*, once the point made about *unjustifiability* is *accepted.* But as we have clarified at some length, these two issues are nevertheless distinct, and in this particular quotation the point actually *made* is quite clearly distinct from the question that then arises. But the simple fact that Owen's interpretation of Hume's 'point' here is a *question*, while Hume's perfectly clear point is a *statement* starkly illustrates the fact that Owen is here once again losing sight of the true significance of what Hume actually *says*, by focusing his interpretation of Hume's explicit claims on the surrounding *context* in which the claims arise, rather than on their actual *content*.

The 'evidence' of the thread of Hume's argument

Owen (2000, p134) suggests that 'the best defence of this [view] of Hume's negative argument involves a look ahead to his positive claims'. On considering the thread of Hume's early argument through his initial ('negative') argument for the 'not–by–reason' conclusion, and the immediately following development of his positive account of belief production, Owen notes that the latter does not make any pretension of attributing justification as an inherent part of the process. He then goes on to argue as follows:

> If [Hume's] negative account is as I have described it, lacking concern with justification and warrant, then Hume's positive account [of belief production] is right on target. We may disagree about whether it is the best account or whether it is any good at all. But at least it squarely addresses the problem as posed and attempts to find an adequate solution for it. But if the problem is really a problem about justification and warrant, then Hume' solution is hopeless. It makes no mention of warrant or justification and thus completely avoids the allegedly central issue. (Owen, 2000, p136)

But we have seen that in fact Hume *uses* his stated problem for the justification of causal inference specifically as a means to the end of *eliminating* the 'by–reason' production hypothesis, in the broader context of context of considering *how* our causal beliefs are produced. Since answering that broader question is the initial theoretical aim at this early stage, it is theoretically appropriate for Hume to continue his answer to that explanatory question *before* discussing the psychological and practical implications of the negative justificatory conclusion established in the process of examination of the broader theoretical question – even despite the relative seriousness of the latter (as drawn out at length in his Conclusion to Book 1).

The fact that Hume's positive account of belief production answers the explanatory question that this passage is designed to tackle in no way supports Owen's view that the argument 'lacks concern' with justification of warrant, and that the issue of justification 'does not enter the picture'. On the contrary, as we have seen, Hume's case for the elimination of the 'by–reason' production hypothesis in this passage proceeds quite specifically *by* consideration of the *justifiability* of our causal inferences. Moreover, as we have seen, in this particular line of argument for the elimination of the 'by–reason' hypothesis (in contrast to the supplementary arguments in the later *Enquiry*) the famous argument for unprovability *does all the crucial and substantial work* for the support of that weaker conclusion. Not only does it 'enter the picture' in Hume's broader discussion of the explanatory issue, but as far as Hume's line of argument on the explanatory issue in the *Treatise* is concerned, the negative justificatory conclusion is *essential* to that broader line of argument. In order to *properly* understand the context of Hume's development of his positive theory of belief production, it is necessary to understand not only *that* he has by this stage (effectively) eliminated the 'by–reason' hypothesis, but *how* he has eliminated it – and it is clear enough that here in the *Treatise* Hume does this by establishing that *it is not possible to justify* or prove our extra–observational beliefs.

The cool continuation of the implications of the unjustifiability conclusion for the initial question about the *process* of inductive inference within the main text of the *Treatise*, in contrast to the impassioned reflection on its more serious practical implications in the Conclusion to Book 1 (*Treatise*, 1.4.7.) can make one wonder for a moment if Hume *really did* mean what he appears to have just said. But the eventual disclosure of his emotional response to this problem *after* the completion of his purely theoretical deliberations ultimately eliminates any prior grounds for suspicion that perhaps he did not.

Owen's theoretical analysis in support of his interpretation

In purported support of his controversial interpretation of Hume's unprovability argument Owen suggests that what, in the context, the unprovability argument is *designed to show* is that our belief in the continuity principle (or as he, in common with many others, calls it – the 'uniformity principle') cannot be used to explain *how* we reason probabilistically:

> The concern [of the unprovability argument] is not whether *the uniformity principle* is justified. The concern is whether it is something we can *believe* or know, *prior* to our engaging in probable reasoning, so that we might explain how we do in fact reason probably. If it were available to us, it would serve, much as the idea of necessary connection might have served, to facilitate the transmission from the impression to the idea. Moreover [1] *[the uniformity principle] would facilitate the transition in such a way that in making the transition, we would be 'determin'd by reason'.* Hume's conclusion is that [2] *it is not something that we believe or know prior to our engaging in probable reasoning*, and so [3] *it cannot be used to explain the origin of that practice.* ... Hume argues that after we do engage in the practice of probable reasoning, explained in Hume's way, we do come to believe in the uniformity principle and make use of it in some of our more reflective causal reasonings. (Owen, 2000, p141–2, my emphases, my numbering.)

It would seem that what Owen is trying to do here is to support his interpretation of the unprovability argument by providing what is supposed to be an illuminating or at least plausible account of Hume's underlying theoretical intentions at this point. However there are two compelling lines of objection that need to be raised against Owen's suggestions in this passage. The first point to note, as with the arguments considered above, is that this purported analysis of Hume's underlying line of thought does not in any way reflect what Hume's actually *says* in the unprovability argument. What the unprovability argument (or at least the part of it relating specifically to *probable* reasoning) actually *says* is that the 'uniformity principle' *cannot be proved* by any probable arguments because all probable arguments require a presupposition of it. It does not *say* that the principle *can't explain the origin* of probable reasoning *because we didn't believe it* before we first engaged in probable reasoning.

In addition to this crucial point of the *non–identity* of Owen's expressed interpretation here with the explicit content of the unprovability argument *itself*, we may also note that he even appears to misinterpret the *broader* argument, within which Hume makes use of the unprovability conclusion in order to establish the weaker conclusion that we *aren't* determined by reason when we make our inductive inferences. Not only is this *not what Hume says* in this particular argument for the 'not–by–reason' conclusion, but Hume *would not* even subscribe to the claims that Owen attributes to him here.

Owen suggests in the passage above [2] that Hume is claiming here that we *don't believe* the uniformity principle prior to engaging in probable reasoning. But Hume, in stark contrast, is claiming quite the contrary. Hume is claiming that the uniformity principle is an actual, and fundamental *presupposition* of any probable argument or inference:

> ... (*Abstract*, para 14)
>
> For all inferences from experience suppose, as their foundation, that the future will resemble the past, and that similar powers will be conjoined with similar sensible qualities. (*Enquiry* 4.2.8.)
>
> This is the inference from cause to effect; and *of this nature are all our reasonings in the conduct of life: On this is founded all our belief in history* ... It follows, then, that all reasonings concerning cause and effect are founded on experience, and that all reasonings from experience are founded on the supposition, that the course of nature will continue uniformly the same. ... All probable arguments *are built on the supposition*, that there is this conformity betwixt the future and the past (*Abstract*, paras 10,13, &14, my emphasis)

Moreover, this is not the *only* claim that Owen attributes to Hume here but which Hume would not subscribe to. Owen also suggests (statement [3]) that Hume is claiming that a prior belief[23] in the uniformity principle *can't be used* to explain the origin of probable reasoning. But in Hume's account of probable reasoning, a prior belief in the uniformity principle plays an

[23] Compare Owen's second sentence in this passage, plus statements [2] and [4] to confirm this interpretation of statement [3] in terms of *belief* in the uniformity principle.

essential role in explaining the origin of probable reasoning. In Hume's view, acknowledgement of a prior belief in the uniformity principle is an essential requirement for *any* satisfactory explanation of probable reasoning precisely because (in his view) probable reasoning cannot happen without it.

Thirdly, Owen *also* wrongly suggests here (statement [1]) that Hume would have us suppose that if a prior belief[24] in the uniformity principle *were* available to us 'it would facilitate the transition [from the observational impression to the non–observational idea] in such a way that in making the transition, we would be '*determin'd by reason*''. But Hume's argument is, quite to the contrary, that although we do (and must) base our probable *arguments* on a prior belief in the uniformity principle, that does *not* amount to supplying a *proof* of, or a *reason* for believing, the conclusion of the inference (because there is no possible proof or reason for believing that crucial presupposition). What Hume actually says in this regard in the *Treatise* is that

> According to this account of things, which is, I think in every point unquestioinable, *probability is founded on the presumption of a resemblance betwixt those objects, of which we have had experience, and those, of which we have had none*; and therefore 'tis impossible this presumption can arise from probability. (*Treatise* 1.3.6.7., my emphasis)

Hume is not saying that it is impossible we can *presume* the unobserved will resemble the observed – on the contrary he here quite explicitly insists that we *do* so in all our probable reasonings. His complaint is rather that this presumption cannot *justly*[25] arise from probable reasoning (the problem of course, taken for granted here, but made more explicit in the *Enquiry* being that such a justification would be circular). That this *is* the significance of the problem here is further clarified in the immediately following paragraph, which Hume begins as follows:

> Shou'd any one think to elude this argument; and ... pretend that all conclusions from causes and effects are built on *solid* reasoning: ...(*Treatise* 1.3.6.7., my emphasis)

Hume's reference here to the 'solidity' of the reasoning described clarifies the point that this complaint is a complaint about the *justification* for the cited presumption – *not* about whether the presumption is actually, or possibly, *made* – as Owen's interpretation demands.

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[24] Again, compare the context for confirmation that Owen's point essentially concerns prior *belief* in the principle.

[25] This being clarified in the introductory context – 'let us cast our eye on each of these degrees of evidence and see whether they afford any *just* conclusion of this nature.' (*Treatise* 1.3.6.4., my emphasis.)

Thus, in stark contrast to *Owen's* suggestion, Hume's (broader) argument here is that *even though we do* have a prior belief in the uniformity principle this will *not* facilitate the transition in such a way that we are *determined by reason* – in other words *compelled by rational proof* – to draw the conclusions we do about the unobserved[26]

The extent to which Owen appears determined to *resist* seeing what Hume is actually *saying* in this argument, and to read something quite different into it, is quite remarkable.

Owen's final misunderstanding: the 'tu quoque' argument

Finally (with reference to Baier[27], 1991) Owen offers the argument that

> [probable] reasoning is the sort of reasoning that makes up the bulk of Hume's *Treatise*. ... So it will not do to treat Hume as entirely negative about [the warrant of][28] probable reasoning, if we are to avoid saddling him with a virtual contradiction at the heart of his enterprise. Far from being settled by the end of Hume's negative argument, the question of the warrant of probable reasoning remains wide open. (Owen, 2000, p146)

But even granted the traditional view that Hume is (virtually[29]) entirely negative about the logical justifiability of probable reasoning, there is nothing like a contradiction to be had here: Hume does not *purport* to have any genuine logical justification for his inductively based opinions, despite the surface appearance of some of the phrases he uses. In fact Hume was perfectly aware of the threat of an objection along just these lines and responded to it at the end of his Conclusion to Book 1:

> [I]f we are philosophers, it ought only to be upon sceptical principles, and from an inclination which we feel to employing ourselves after that manner. ,, [W]e should yield to that, which inclines us to be positive and certain in *particular points*, according to the light in which we

[26] In the *preamble* to the unprovability argument in the *Abstract* Hume begins with consideration of the situation of Adam *prior to any experience* – in which situation we might suppose that he did not have a belief to the uniformity principle, since Hume says that belief in it is based on experience. But as he approaches the unprovability argument itself Hume makes it quite clear that he is about to show that *even with the benefit of experience*, and moreover *even with the belief in the uniformity principle that naturally arises in the course of experience*, Adam could never have *proven* the principle, *nor* have been *determined by reason* in the conclusions he draws from the use of it.

[27] The quotation Owen mentions is 'If Hume really distrusts causal inferenece, then he must distrust his own *Treatise*' (Baier, 1991, p55).

[28] The traditionalist does not suggest that Hume is entirely negative *in everything he says* about probable reasoning: he claims that Hume is (at least virtually) entirely negative about the (logical) *justifiability* (or in Owen's terms 'warrant') of probable reasoning. That this is the position that Owen intends to target in his objection here is evident from the reference to warrant in his conclusion as quoted.

survey them in any *particular instant.* ... On such an occasion we are apt ... to forget our scepticism, ... and make use of such terms as these, *'tis evident*, *'tis certain*, *'tis undeniable*; ... I may have fallen into this fault ... but I here ... declare that such expressions were extorted from me by my present view of the object, and imply no dogmatical spirit, nor conceited idea of my own judgement $_{15}$ (*Treatise*, 1.4.7. paragraph numbers in subscript)

Hume's position then is that we formulate inductive beliefs in accordance with the continuity principle of probable reasoning because we are irresistibly compelled to do so by the force of natural habit (along with the associated apparent consequences of doing otherwise: 'it costs us too much pains to do otherwise'). But he is also quite explicit in distancing himself from any claim, despite appearances to the contrary, that he is able to provide any *proof* for his view. It is simply his *belief*, which he holds without any apparent hope or claim of *logical* justification. Hume does not *purport* to compel *the reader* to follow his theories in the *Treatise* by provision of unobjectionable argument. He merely *invites* the reader to follow him in his 'speculations' on these matters – *if he is so inclined.*

> If the reader finds himself in the same easy disposition, let him follow me in my future speculations. If not, let him follow his inclinations (*Treatise*, 1.4.7. 14)

The fact that Hume feels the need to provide such a radical *response* to this *tu quoque* line of objection – a line of objection that, in Owen's own admission, would appear to be in order *if* Hume had argued that probable inference was unjustifiable, – provides additional support *in favour* of the view that Hume has indeed propounded such an argument, and recognises that his arguments *are* subject to this objection – that he cannot provide any ultimate justification for his beliefs or for the inferences that he makes, despite his natural inclination to think and sometimes say otherwise.

Hume's apparent reservations about the impossibility of a logical justification of induction in the *Enquiry*

However, despite the force of these considerations in favour of the traditional view, that is not the end of the story with regard to this interpretive issue. What we have seen is that this appears to be Hume's position at the time of composition of the *Treatise*. But of course Hume's later *Enquiry Concerning Human Understanding* (1748) was intended mainly as a more careful and concise revision of the ground covered in the *Treatise*. And Hume did say that he wanted his

[29] Barring the occasional apparent admission of the possibility that he is mistaken in his beliefs and arguments.

arguments to be judged on the basis of this rather than the *Treatise*: In the opening *Advertisement* to the 1777 edition of the *Enquiries* Hume says

> the Author desires, that the following pieces may alone be regarded as containing his philosophical sentiments and principles.

And although Owen does not make particular use of the point, opting to base his argument on the original text of the *Treatise*, Hume does appear, at certain points in the text of the *Enquiry*, to treat with some *reservation* the apparent claim that there is *no* possible argument that could support our inductive inferences.

Hume's reproduction of the unprovability argument in the *Enquiry*

Before proceeding to consider the significance of these apparent reservations, let us first confirm that Hume does nevertheless *reproduce* the argument for *unprovability*:

> all inferences from experience suppose as their foundation that the future will resemble the past, … It is *impossible*, therefore, that any arguments from experience can *prove* this resemblance of the past to the future; since all these arguments are founded on the supposition of that resemblance. (*Enquiry*, 4.2, para 8, my emphasis)

Immediately following the repetition of the demonstration that no arguments could possibly prove the inductive inferential principle (or presupposition) of past/future continuity, Hume says, with (at least initially) apparent sincerity[30]:

> as a philosopher … *I want to learn the foundation of this inference.* No reading, no enquiry has yet been able to remove my difficulty, or give me satisfaction in a matter of such importance. Can I do better than propose the difficulty to the public, even though, perhaps, I have small hopes of obtaining a solution? We shall at least, by this means, be sensible of our ignorance …
>
> I must confess that a man is guilty of unpardonable arrogance who concludes, because an argument has escaped his own investigation, that therefore it does not really exist. (*Enquiry*, 4.2, 8–9, my emphasis)

[30] Perhaps these statements should not be taken at face value. But in either case, sincere or ironic, the argument I shall present in defence of the traditional interpretation of the unprovability argument will stand without an assumption of irony. The attribution of a degree of confusion in the line of argument that Hume presents in this passage of the Enquiry is unfortunately, in any case, inevitable.

It would seem then that the difficulty 'of such importance' to which Hume refers here is rather the difficulty that appears to arise from Hume's immediately preceding demonstration that there is no possible way of *proving* the continuity principle. Certainly it would seem from what Hume is saying that *he* can't see any way it would be possible to *provide* any satisfactory proof of it, and doubts that anyone else can either. The point expressed in this passage then appears to be the suggestion that perhaps Hume may have been guilty of an oversight within his argument for unprovability, and that he might in fact be mistaken in drawing such a pessimistic conclusion: perhaps there might after all be some *possible* way of proving the continuity/resemblance principle that he has failed to consider.

Hume's purported elimination of these doubts, and his apparent confirmation of Owen's interpretation

Immediately following this suggestion though, it would seem that Hume is about to provide decisive confirmation that he is *not* mistaken about this, when he adds:

> But with regard to the present subject, there are some considerations which seem to remove all this accusation of arrogance or suspicion of a mistake. (Enquiry, 4.2.9)

Surprisingly though, as it turns out, the following passage provides instead what on the surface appears to be a compelling piece of textual evidence for *Owen's* view, since it is at this point that Hume brings in two considerations that are used to provide independent confirmation of the *weaker* conclusion that '*it is not reasoning which engages us* to suppose the past resembling the future': (a) the point that an infant or animal that is *incapable* of logical reasoning naturally develops inductive expectations when faced for example with objects that have previously caused pain, and (b) the fact that even we cannot immediately *report* the reasons on which our inferences are supposed to be based, but need instead to try to *construct* an appropriate argument. Having first suggested that there might be some *possible* proof of the uniformity principle which he has overlooked; Hume explicitly introduces these final considerations with the suggestion that they will 'remove' any such doubt.

It seems quite clear that what Hume was talking about in the lead up to this supposed settlement of the issue was the question of whether, despite his failure to find or imagine any argument that would satisfactorily (without circularity) support the continuity principle, there might nevertheless be some *possible* line of argument *hitherto unthought of* that would support it. So unless Hume is actually going to *change the subject*, what appears to be *promised* here are some considerations that will remove any suspicion that some *possible* line of logical support for the continuity principle may have been overlooked. But that is *not* what he provides. *These* arguments *do leave open* the question of whether we might, with sufficient logical ingenuity and painstaking research, be able to think of an argument that would provide satisfactory logical support for the continuity principle. *Their* conclusion *is* merely that we don't *use* any such argument when we make our inductive inferences.

Moreover – just in case we might suspect that perhaps Hume is merely appealing to (extraordinarily) poor or weak arguments in support of the strong unprovability conclusion – Hume immediately makes it clear that the conclusion of these arguments is the relatively weak explanatory conclusion – that

> it is not reasoning *which engages us* to suppose the past resembling the future,

and moreover emphasises that:

> *This is the proposition which I intended to enforce in the present section*

and furthermore that it *is* a relatively weak conclusion:

> If I be right, I pretend not to have made any mighty discovery. (*Enquiry*, 4.2.10, my emphases)

It would appear then that in the *Enquiry*, which Hume specifically asked to be regarded as the definitive account of his philosophical views and arguments, he is in the end insisting that we interpret the conclusion of what has traditionally been interpretd as a strong justificatory argument for *unprovability* in precisely the way that Owen is suggesting – rather as a negative explanatory argument for a weak 'not–by–reason' conclusion. What then is the traditionalist to make of this? Could Owen be right after all?

The context of the unprovability argument,
and Hume's fallacious defence against the objection
that it might be based on an error of oversight.

There does of course remain the problem that the two respective statements in Hume's text (the unprovability statement and the 'not–by–reason' statement) do not in any way *appear* to be identical, and the fact that, as we have seen, the unprovability claim, as we have (traditionally) understood it appears to be an *essential component* of the unprovability line of argument for the not–by reason conclusion. And of course this line of argument still *appears* to be present – and indeed the primary line of argument – presented in the *Enquiry*. In view of this it would seem that the right way to attempt to make sense of this puzzling passage is to seek some account of Hume's intentions, and possible errors, in this passage that is compatible with the results of our analysis thus far.

I think that the *charitable* interpretation of the situation here, is that Hume is using the supplementary arguments not simply to confirm the *particular claim* regarding which the question of possible oversight has been raised, as his connecting statement might *appear* to suggest. Rather, he is using the supplementary arguments to confirm his *ultimate* conclusion concerning the broader theoretical issue which is the subject of this 'present *section*' as a whole. That is why he emphasises just that point immediately following the conclusion of those additional arguments.

However, even though this may be a perfectly reasonable strategy for him to take under the circumstances of suspicion about this particular (unprovability) *line of support* for the conclusion of ultimate theoretical concern at this point in the *Enquiry*, the problem nevertheless remains that Hume *does* appear, by his choice of words in the connecting passage, to suggest that the arguments to follow will clear up 'this' *immediately forementioned* 'suspicion of mistake'. And this immediately forementioned suspicion of mistake was specifically a suspicion that some *possible* proof for the continuity thesis might have been overlooked, i.e. that there might be some mistake about the *unprovability* conclusion. In this case, despite the legitimacy of Hume's use here in the *Enquiry* of additional arguments in response to the problem of doubt about the unprovability line of argument for the *not–by–reason* conclusion (i.e. provision of *alternative* arguments for the *ultimate* conclusion that it was intended to support) he is nevertheless – with respect to his implicit promise to eliminate any scope for doubt *about the unprovability thesis* – guilty of the fallacy of changing the subject (to a similar one more easily defendable). To be specific, Hume's fallacy here is that he is guilty of changing the subject of *what it is that*, on this suggestion, *might have been overlooked*. The initial objection was that Hume might have overlooked some *possible* line of argument that *could* be used to prove the continuity principle. But the only issue of possible oversight that Hume addresses in these closing arguments is the question of whether there might possibly be any argument that actually *has* been used or understood by us within the course of our inductive inferences. On this understanding of Hume's puzzling line of argument at this point in the *Treatise*, it would seem that what might look on the face of it like a confirmation of Owen's interpretation of Hume's problem, is merely a case of Hume having himself fallen uncharacteristically into the fallacy of momentarily conflating the two closely related but subtly distinct conclusions for which he argues in his discussion of the justification and explanation of inductive inference.

Conclusion

In this chapter I have clarified the context of Hume's problem for the justification of induction, and in so doing I have defended the traditional view, against the objections of Owen, that Hume's apparent arguments for the unjustifiability of our inductive inferences are indeed arguments for their unjustifiability. We have seen that Hume's unjustifiability thesis is employed by Hume in the context of a broader argument concerning the process of our inductive inferences. Hume argues that *since* our inductive inferences cannot possibly be *justified* by any line of reasoning, in the sense that they cannot be 'proven' or shown to be true, they are not *arrived at* by our comprehension of any satisfactory line of reasoning.

With the benefit of these fundamental and contextual considerations, in the following chapter I shall consider in more detail the precise interpretation of Hume's unjustifiability thesis, with regard to the question of whether Hume is suggesting that justification fails on account of a failure of premise–adequacy or a failure of logical adequacy. In particular I shall approach this issue by consideration of the common supposition that one of the core strands of Hume's argument is a claim that our inductive inferences cannot be *deductively* valid. I shall argue that this interpretation of Hume's unjustifiability argument is a mistake, and that Hume simply made a case for the unjustifiability of inductive inference on the basis that a crucial *assumption* of our inductive inferences and representative arguments is unjustifiable. Hume had no complaint to make about the *validity* of inductive inference once its fundamental logical structure is properly understood.

CHAPTER TWO
WAS HUME'S PROBLEM A PROBLEM FOR THE VALIDITY OF INDUCTIVE INFERENCE?

It is commonly claimed, and taught, that in Hume's view the crucial problem for inductive inference was that such inferences are logically invalid.[31] Musgrave (1993) for example presents the following account of Hume's objection to inductive reasoning:

> Hume thought that as a matter of psychological fact we cannot help reasoning inductively. ... Nor did he dispute (at least in this context) that the premises of such arguments could be known for certain to be true from past experience. *His objection is that inductive arguments are logically invalid*: the truth of the premises does not guarantee the truth of the conclusion, it is possible for the conclusion to be false even though all the premises are true (Musgrave, 1993, p152, my emphasis)

In this chapter I shall argue that, contrary to popular opinion, Hume did not find a problem with the validity of the inferences we are 'induced' to make about things we have not observed. Furthermore, close attention to the details of Hume's account of the logical structure of inductive inferences reveals this as a form of inference that *is* in fact deductively valid. The justificatory problem of induction, as Hume understood it, does not present us with any reason to question the validity of inductive inference – not, that is, unless we should wish to contend *Hume's* account of its logical structure.

Colin Howson's initial account of induction in his recent discussion of *Hume's Problem* (Howson 2000) reflects Hume's emphasis on observation as an essential and basic feature of inductive inference, but also draws attention to one or two other significant aspects of induction:

> The inferential process by which observation, suitably controlled, is regarded as conferring an affidavit of reliability on what in a strict logical sense extends beyond it philosophers have traditionally called *induction*. (Howson 2000, p6)

It would appear from the context of this introductory characterisation that Howson's use of the qualification 'suitably controlled' is intended to relate to the context of controlled *experiments*, since he goes on to elaborate this in terms of 'observational data obtained in suitably rigorous

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[31] See below for further examples.

ways'. Hume himself sometimes spoke of the outcomes of induction in terms of our 'experimental conclusions', and stressed that

> It is only after a long course of uniform experiments in any kind, that we attain a firm reliance and security with regard to a particular event. (*Enquiry* 4.2.7.)

However it is worthy of note that there is a *further* sense in which the qualification 'suitably controlled' could, and should, be taken to apply in the context of inductive inference, and which applies in the case of *Hume's* understanding of the nature of inductive inference. Even granted that controlled experiment has been utilised in obtaining the observational information on which our inductive inferences are founded, *what is inferred* from that information *also* needs to be 'controlled', in the following sense: Whatever the structure of inductive inference might be, it *needs* to include – in addition to the mere content of the initial observational information – some crucial component or feature which effectively controls what is and what is not inferred from the content of that information via that controlling factor. One thing I intend to show in what follows is that Hume expresses and maintains a particular view as to just what that *inferential* controlling factor is, and makes a particular point of insistence on its precise logical status within the inference.

Another notable point about Howson's initial characterisation of induction at this point is that, in emphasising that the conclusion 'in a strict logical sense extends beyond' the *observational* information on which it is based, this properly represents a true characterising feature of Humean induction. Unfortunately, this correct account of the character of Humean induction is all too often mistaken for a subtly but importantly different account, and moreover, at least on the face of it, it seems that Howson also falls foul of this interpretive fallacy in his ensuing representation of Hume's account of induction and the problem associated with it.

Did Hume deny the possibility of deductive induction?

It is commonly supposed that it is quite obvious that inductive inference, as Hume understood it, *cannot* be deductively valid – that Hume acknowledged this – and moreover that this constituted a basic premise of the problem that Hume perceived to threaten the *justification* of inductive inference on his understanding of it. I shall argue that on all these points this common perception of *Hume's* understanding of the logical structure of inductive inference and *his* problem for the justification of such inference is mistaken.

This common perception of Hume's view appears to be endorsed by Howson,[32] when he suggests that

> Hume's argument ... is very simple. Hume commences by pointing out that no inference from the observed to the as–yet unobserved is deductive: (Howson, 2000, p10)

– presuming of course, as one naturally might, that Howson here intends 'inference from the observed to the as–yet unobserved' implicitly to be understood as a reference to inductive inference as Hume understands it.[33] Howson quotes Hume's *Enquiry* to illustrate the point:

> That there are no demonstrative arguments in this case seems evident; since it implies no contradiction that the course of nature may change, and that an object, seemingly like those we have experienced, may be attended with different or contrary effects. (*Enquiry* 4.2.5.)

But is Howson right to suggest that Hume's point here is intended to imply that our inferences from what we have observed to what we have not are not *deductive* (or, more to the point, deductively valid)? There are problems with Howson's interpretation of this point of Hume's.

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[32] Another recent example is to be found in Alexander Bird's account of the nature of induction and Hume's problem, in his recent contribution to the series 'Fundamentals of Philosophy' *Philosophy of Science* (1998) p10–17. Here Bird takes care to distinguish '*Humean* induction' from a 'broader' sense 'in which it means just "non–deductive"', and describes Humean induction as a form of argument 'where we argue from [our observations of a limited range of] several particular cases to the truth of a generalization covering them [all, not just the ones we have seen]' (p13, parentheses quote elaborations in example on p14). Bird specifically frames his account of Hume's problem 'in terms of *Humean* induction' (my emphasis) pointing out that 'those observations along with the form of the inductive argument should justify the [conclusion]', but nevertheless goes on to argue that this 'cannot be the sort of justification that a deductive argument gives to its conclusion' since 'in inductive arguments it is logically possible for the conclusion to be false while the premises are true' and refers in elaboration to 'The very fact that inductive arguments are not deductive' (p15).

Salmon (1966) also appears to hold that Humean induction, by its nature, is not deductively valid. Salmon holds that 'All valid deductions are demonstrative inferences' (p8), that 'All ampliative inference is non–demonstrative' (and therefore not deductively valid) (p10), and admits that 'In presenting Hume's argument, I was careful to set it up so that it would apply to any kind of ampliative or non–demonstrative [and therefore non–deductive] inference' (p20). Salmon supposes that 'Any type of logically correct *ampliative* [hence, on Salmon's terms, non–deductive] inference is induction' and that 'the problem of induction is to show that some form of ampliative inference is justifiable' (p20, my emphasis and parentheses).

Ladyman (2002) also takes this view of induction: 'Induction in the broadest sense is just any form of reasoning that is not deductive' (p28), 'By definition, in inductive arguments, it is possible the premises may all be true and the conclusion nevertheless false' (p39).

[33] In discussion on this point Howson has in fact indicated that he is largely in agreement with my interpretation of Hume here, and that he simply means that Hume is saying that there is no deductive argument from *just* our observations to what is unobserved. I of course accept that Hume would surely *agree* such a claim – and indeed, as I explain below, he does even seem to invoke something like that point in response to considered line of objection at one point in the *Enquiry*. However my argument is that this is by no means the central claim Hume is making in his account of the problem of induction. Moreover, understanding exactly what Hume *is* saying here helps us to get a more accurate picture (than the traditional view) of Hume's understanding of the logical structure of inductive inference.

One problem, which I shall take up below, is that what Howson appears to interpret this claim as saying does not follow from the grounds Hume offers for the claim. But a more basic problem concerns Howson's apparent interpretation of Hume's term 'demonstrative' here as equivalent to 'deductive'.

Hume's concept of 'demonstration'

It is not uncommon for commentators to read Hume's account of the problem of induction with a modern interpretation of the concept of 'demonstration' in mind, whereby 'All valid deductions are demonstrative inferences' (Salmon, 1966, p8). But on consideration of the textual evidence, of which this statement is a prime example, there appear to be a compelling case for thinking that this interpretation of Hume's use of the term 'demonstrative' basically as 'deductive' is mistaken. From the statements Hume makes about demonstrative inferences, it would appear that Hume would regard *only* arguments whose conclusions are logically (or semantically) necessary – i.e. imply a contradiction – as *demonstrations*. This is apparent from a number of passages, and in particular from the following clause from paragraph 11 of the *Abstract* of the *Treatise*.

> whenever a demonstration takes place, the contrary is impossible, and implies a contradiction. (Hume *Abstract*, para. 11)

The natural reading of this statement is that the contrary of *that which is demonstrated* (i.e. the conclusion of the demonstration) is impossible. To force an interpretation of 'demonstration' as 'valid deduction' in our normal semantic sense of the term as an argument for which the *conjunction* of the premises *with* the contrary of the conclusion implies a contradiction, would require a much more convoluted reading of *what it is*, whenever a demonstration takes place, the *contrary* of which is supposed to be impossible. On an interpretation of 'demonstration' as 'valid deduction' we would need to read this as 'the contrary of *the negation of the conjunction of the premises with the negation of the conclusion* is impossible' or something equivalent.

This reading is so convoluted that it is not particularly easy even to *see* the point without the use of symbols: Where A is the assumption (or the conjunction of the assumptions) and C the conclusion, what by definition is *impossible* in the case of a *valid deduction* is A&~C. Employing the traditional symbol ◇ for 'It is possible that ...' (~ as the sign for negation, and & for conjunction) this impossibility may be symbolised as

$\sim\Diamond$ (A&~C)

But in Hume's account of 'demonstration', he is saying that 'the contrary' of something is impossible. If indeed then he intends this in a sense equivalent to 'valid deduction', we need to find an equivalent expression *for the clause in brackets* (A&~C) where the *main* operator is negation. An easy way to do this is to apply double negation, thus:

$\sim\Diamond$ ~~(A&~C)

On this interpretation then the unspecified 'something' the *contrary* of which is impossible, in symbols, would need to be ~(A&~C) or some equivalent. In comparison with the far simpler natural reading on which the contrary of *the conclusion* is impossible in the case of demonstration, it appears at least on this initial analysis, that there is a compelling case for going along with Stove's view that

> to suppose that Hume used 'demonstrative argument' in this sense [equivalent to 'deductively valid'] would be to impute to him an error unbelievably gross and often repeated. For he would then be saying ... that there cannot be a valid argument which has a contingent *conclusion*! (Stove, 1973, p35–6, my emphasis)

Stove, quite naturally, appears simply to *assume* that the unspecified 'something', the *contrary* of which Hume claims to be impossible in the case of 'demonstration', is simply the conclusion. And our initial comparison with what would be needed to fill that space on an interpretation as 'valid deduction' would appear to rule out that alternative as a construction too complex and abstract to be taken by Hume as being sufficiently obvious from the context as not even to need mention.

However, with regard to this question of comparative simplicity of interpretation, this initial comparison is somewhat misleading, since the relatively complex construction ~(A&~C) can be significantly simplified, and furthermore, what it reduces to is not such an implausible candidate for the space–filler in this context after all. ~(A&~C) is of course equivalent to the material conditional A→C (If A then C).[34] In order to appreciate the significance of this in the context, we may note that Hume thinks of a 'chain of reasoning' as a sequence of suitably related propositions, whereby the relations between directly inferred propositions are apparent either from experience or (in the case of relations of ideas associated with demonstration) by intuition, so that by following the chain of individually apparent connections it *becomes clear* that there is a satisfactory, if relatively complex, inferential connection between the first and last proposition in the sequence. Owen rightly emphasises this general clarificatory function with regard to the case of demonstration, describing the latter as a 'process *whereby we become aware* that one idea stands in a relation to another' (2000, p91). But he fails to make it quite clear exactly *what* relation must hold in general between the terminal propositions in the context of a *propositional proof* that something is the case (whether demonstrative or otherwise). With regard to the demonstrative case, Owen simply says

> If the two ideas that stand in this indirect, demonstrative relation are themselves propositional, we will have a demonstrative inference *from* one proposition *to* another. (Owen, 2000, p91 my emphasis)

But what exactly is this 'from ... to' relation that is supposed to hold between the terminal propositions in the general context of a proof? In order to argue satisfactorily from a satisfactorily evident or otherwise uncontested proposition A to a categorical conclusion C initially under question, what must thereby be made satisfactorily evident[35], is that since A is the case, so is C – or more specifically (since A is already evident or granted)[36] *if*, A is the case, then so is C. Thus the crucial *relation* that ultimately needs to be shown to hold between the terminal propositions – in the case of demonstration as well as in any other kind of proof – is that *if* the initial proposition is true *then* the conclusive proposition will also be true.[37] In other words the *ultimate* relation that needs to be established as holding between terminal propositions in the case of a demonstration is that of the respective *conditional.*

For some ordered pairs of propositions the connecting conditional may be immediately apparent either from logical intuition (if the connection is logical) or from direct observation. In such cases there will be no need to explain how the two propositions are connected in the course of the argument – the implication will be immediately obvious, and the one may be directly inferred from the other. In other cases however the connection may not be immediately apparent, and in such cases a 'medium' or intermediate proposition (or sequences of

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[34] That the material conditional $A \rightarrow C$ is equivalent to $\sim(A \& \sim C)$ is straightforwardly evident from the truth table for the material conditional, where $A \rightarrow C$ is *false* iff A is true and C is false (i.e. iff $A \& \sim C$) and hence it is *true* iff it is *not* the case that $A \& \sim C$.

[35] In this case, sufficiently evident to warrant categorical assertion.

[36] I take it that '*Since* A, C' implies A in addition to *if* A then C.

[37] This will be the case even when independent assumptions are added in the course of the proof, although $A \rightarrow C$ will in such cases be dependent on those additional assumptions, and consideration of that additional information in such an argument will be instrumental in making it evident that $A \rightarrow C$.

propositions) needs to be provided which clarifies the link (or chain of links) between the initial proposition and the conclusion.

In the case in question (above, *Enquiry* 4.2.5.) Hume leads up to the point with the observation that

> The connexion between these propositions is not intuitive. There is required a medium, which may enable the mind to draw such an inference, if indeed it be drawn by reasoning and argument. (*Enquiry*, 4.2. para. 3)

And it is precisely in context of the question of 'What that *medium* is' that the above reference to demonstrative arguments arises. Now if I am right in suggesting that the medium is essentially a (satisfactorily evident) proposition (or a sequence of propositions) 'connecting' the two terminal propositions A and B, on consideration of which it becomes evident that *if* A *then* B, perhaps it might not be out of the question to suppose that the unmentioned object in Hume's statement about demonstration is actually supposed to be understood as this *purported connection* between A and B – or at least the required logical connection, which is basically that *if* A is the case *then* so is B. But if this conditional *were* the unspecified 'something' the *contrary* of which is impossible in the case of demonstration, then, according to our analysis above, Hume's notion of 'demonstration' as (partially) described here *could* after all be equivalent to 'deductively valid proof'.

Moreover, if we examine carefully the context of the above account of demonstration in the *Abstract*, we find that, even in the same paragraph, Hume appears to suppose that if there *were* a satisfactory relation of ideas *between* the terminal propositions in an experientially barren inductive inference, then we would have a case of *demonstration*:

> Here is a billiard–ball lying on the table, and another ball moving towards it with rapidity. They strike; and the ball, which was formerly at rest, now acquires a motion. [...] And when I try the experiment with the same or like balls, in the same or like circumstances, I find that upon the motion and touch of the one ball, motion always follows in the other. [...]
>
> This is the case when both cause and effect are present to the senses. Let us now see upon what our inference is founded, when we conclude from the one that the other has existed or will exist. Suppose I see a ball moving in a straight line towards another, I immediately conclude, that they will shock, and that the second will be in motion. [...]
>
> Were a man, such as *Adam*, created in the full vigour of understanding, without [prior] experience, he would never be able to infer motion in the second ball from the motion and impulse of the first. It is not anything that reason sees in the cause, which makes us *infer* the

effect. Such an inference, were it possible, would amount to a demonstration, as being founded merely on the comparison of ideas. (Hume *Abstract*, paras. 9–11, my clarificatory parenthesis)

Hume's point here is that if we imagine someone, in a position of having no relevant *background* experience, observing for the first time one billiard ball strike another, that person would be unable to predict, *from* the fact that the first ball strikes the second, that the second will begin to move. There is no suggestion here that the conclusion, if it is drawn will be drawn *without* the initial (observational) information that the first ball strikes the second (hence my clarificatory parenthesis in the quote). The point concerns an inference *from* an (isolated) piece of *observational information*. So when Hume says here that 'the inference' would be 'founded merely on the comparison of ideas', he does *not* intend that to be interpreted as implying that there would be *no observational premises*: what he means is that the *connection* between the observational premise and the conclusion would be founded merely on the comparison of ideas. Likewise, when he goes on to say

> But no inference from cause to effect amounts to a demonstration. Of which there is this evident proof. The mind can always *conceive* any effect to follow from any cause, and indeed any event to follow upon another: Whatever we *conceive* is possible, at least in a metaphysical sense: But whenever a demonstration takes place, the contrary is impossible, and implies a contradiction. There is no demonstration therefore for any conjunction of cause and effect. (Hume *Abstract*, para. 11)

it is (or certainly should be) quite clear that Hume's point is not that such an inference must fail to be a demonstration simply because there is no contradiction in supposing that ball 1 did *not* strike ball 2 at t_1 – in other words because the *premise* is not a necessary truth – which on Stove's account of Hume's view would be sufficient for an argument to fail to be a demonstration.[38] Nor is the point that it must fail to be a demonstration simply because there is no contradiction in supposing merely that ball 2 will not be in motion at t_2, i.e. because the *conclusion* is not a necessary truth. Hume's point here is surely that such an inference must fail to be a demonstration because it is possible *that ball 2 be struck by ball 1 and not begin to move*. In this case, the mystery item the *contrary* of which Hume claims here is not impossible (in other words possible) is the *negation* of the latter, and this interpretation conforms with our earlier hypothesis that the mystery item in Hume's account of demonstration might be the connecting conditional. To clarify this, it is apparent from the text here that what Hume is in

[38] 'Hume meant by 'demonstrative argument' a '(valid) argument *from necessarily true premises*'.' (Stove, 1973, p35, Stove's emphasis).

fact claiming at this point is that it is not impossible that ball 2 is struck by ball 1 and ball 2 does not begin to move. In symbols, and applying double negation to the clause in brackets, this claim is equivalent to

$$\sim\Diamond \sim\sim(A\&\sim C)$$

where A is the observational premise – that ball 1 strikes ball 2 (at t_1), and C is the prediction or conclusion – that ball 2 *will* be in motion at t_2. On this analysis of Hume's claim, the unstated item, the *contrary* of which Hume claims is not impossible in a demonstration *is* in this case equivalent to $\sim(A\&\sim C)$ – which, as we have noted above, is logically equivalent to the material conditional $A \rightarrow C$. Thus in this central example, despite the potentially misleading blanks in Hume's expression of the point, and despite Stove's dismissal of the possibility, it appears – at least in this particular instance, that Hume is in fact using the phrase 'demonstrative' in a sense that is compatible with the conclusion, and indeed the premises, being *synthetic* propositions (as they clearly are in this case). The point Hume is stressing, when he says that the inference in this case cannot be a demonstration because 'the contrary' does not imply a contradiction, does after all appear to be simply the point that this inference is not deductively valid, since close analysis reveals that it is the contrary of the *connecting conditional* (i.e. if the stated premise(s) then the conclusion, or its logical equivalent) which Hume claims does not imply a contradiction – and that is just what would be required for an argument to be deductively valid.

The trouble though is that this interpretation of Hume's concept of demonstration does not seem to be compatible with some other statements he makes about demonstration. In other cases it appears to be quite clear that Hume thinks that the contrary of the *conclusion* must be inconceivable in the case of a demonstration. For example, just a little further on in the same passage of the *Abstract*, Hume adds

> When a demonstration *convinces me of any proposition*, it not only makes me conceive the proposition, but also makes me sensible, that 'tis impossible to conceive any thing contrary. (Hume *Abstract*, para. 18)

This would appear to suggest that the proposition that is supposed to be a necessary truth in the case of a demonstrative argument is that proposition the truth of which the argument is supposed *to convince* the audience – which we would naturally interpret as a reference to the *conclusion*. Consideration of this in conjunction with the previous example might incline us to suppose that Hume was somewhat ambiguous in this respect, sometimes using the term in the one sense, and sometimes in the other. However this reference need not be interpreted in such an uncharitable manner, if we bear in mind Hume's understanding of the primary function of argument as clarifying the *connection* between one proposition and another – the connection needs to be seen *before and in order that* the conclusion may be seen to be true (or acceptable). Since the connection in a propositional argument will itself take the form of a proposition, the primary function of a demonstration is to convince the audience of *the connecting proposition*. In view of this, this reference is quite compatible with the interpretation we have found to be appropriate in the details of the initial example.

Although Owen is right to point out that, on account of the requirement of making the connection *apparent*, it would strictly be a mistake to suppose that 'any deductively valid argument with necessarily true premises is a demonstration' (p90) as Stove suggests, Owen is nevertheless wrong to suggest more strongly that 'any account of Hume's notion of demonstration that *includes* the notion of 'deductively valid' … is … anachronistic', since any argument with a premise P, a conclusion C, and a *connecting conditional* 'If P then C' whose negation implies a contradiction must be a *deductively valid* argument. However, it is also notable that it is not necessary for a connecting conditional (that is understood to be a premise of the argument) to be a logical truth (i.e. for its negation to imply a contradiction) in order for an argument to be deductively valid. So what Hume is arguing here is nevertheless *not* that our inductive inferences are *not deductively valid*, but merely that if there is any satisfactory *connection* between our observational evidence and our predictions, it is *not a logical truth*. So, Hume argues, if any satisfactory connection *can* be established by reason to be true, it must be established by the evidence of experience.

Hume's analysis of inductive inference

In addition to the problems outlined above with regard to Hume's concept of demonstration, we may note that Howson's interpretation of Hume's claim as implying a denial of the possibility of deductive induction is not in any case particularly plausible, since such a denial does not even seem to be a valid implication of the quotation. Even granted that, as Hume claims, it is logically *consistent* to suppose that the course of the universe may change, and moreover that it may do so in precisely those respects that we take to be relevant to our predictions, it does not appear to follow from *that* that we cannot construct a *valid* deductive *argument* to a conclusion about something we have not observed, on the basis of what we have observed. A simple and indeed quite plausible line of objection to the suggestion that we cannot would be the consideration that, in the context of our *actual* inferences from what we observe to what we do not, our observational premises might well be *supplemented* by further operative assumptions[39] which exceed the content of our observations.

As a purely logical point this possibility is of course quite obvious, and the natural response by those who take the anti–deductivist position is that what they *mean*, when they say that it is not possible to argue deductively from what we have observed to what we have not, is simply that it is not possible to do so *without* any further assumptions. *That* of course is a point that no–one would want to deny, and on which Hume himself would undoubtedly agree:

> Let the course of things be allowed hitherto ever so regular; *that alone*, ... proves not, that for the future, it will continue so. (*Enquiry* 4.2.8. my emphasis)[40]

The important question though, with regard to *Hume's* view of the problem of induction, is how this point relates to Hume's understanding of the actual *structure* of inductive inference, and to the problem *Hume* had for the justification of induction *as he understood it*. Did *Hume* think that our presumption of the veracity of our observations is the *only* basic assumption that we make when we make inductive inferences?

It is my view that on a careful and unbiased examination of the relevant texts it would seem to be quite clear that he did not.[41] At the centre of his main argument on this issue in the *Treatise*, Hume sets out quite explicitly, and in some detail, his view that inductive inference is based on a *further* presumption of the *continuation* of the (apparently) hitherto constant conjunctions of events that we regard as cause and effect:

> The only connexion or relation of objects, which can lead us beyond the immediate impressions of our memory and senses, is that of cause and effect; and that because 'tis the only one, on which we can found a just inference from one object to another. The idea of cause and effect is

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[39] By (operative) 'assumption', in the context of an inference, I mean a proposition that is believed to be true (by the person or persons making the inference), whose own justification is taken for granted, rather than established or clarified, in the course of the inference, and without which the conclusion of the inference would not be inferred.

[40] I do not intend to insist here that Hume is employing a sense of 'prove' that implies deductive validity. We may interpret Hume's point here as the broader claim that past regularity in itself is insufficient for any form of proof that it will continue – deductively valid or otherwise.

[41] It is interesting to note that in recent tutorials I have had the opportunity to present exactly this question to a number of groups of first year students of epistestemology and philosophy of science, shortly after their first lectures on Hume. Without any prior input from myself, presented with copies of the relevant section of the *Abstract*, and invited to examine the text in pairs and consider whether or not Hume claimed that we *assume* that observed regularities will continue when we make inductive inferences (along with other related questions) – the students were practically unanimous in agreeing that he did – and included this proposition as a premise in their sketches of an example of a Humean inductive

> deriv'd from *experience*, which informs us, that such particular objects, in all past [observed] instances, have been constantly conjoin'd with each other: And as an object similar to one of these is suppos'd to be immediately present in its impression, we thence presume on the existence of one similar to its usual attendant. According to this account of things, which is, I think, in every point unquestionable, probability is *founded on the presumption* of a [continued] resemblance betwixt those objects, of which we have had experience, and those, of which we have had none (*Treatise* 1.3.6.7. latter emphasis mine)

This crucial presumption is referred to repeatedly by Hume in the course of his discussions of inductive inference, and is referred to by a variety of abbreviations, not all of which make their essential relation to causal regularity patently clear.[42] Unfortunately, it is with reference to one of the relatively opaque variations – 'that the course of nature will/must continue uniformly the same' (*Abstract*, paras. 13/14) – that this assumption has perhaps most commonly been dubbed by commentators – as the 'principle of the uniformity of nature'. However, I think the relevant supposition is best understood when it is interpreted in the context of Hume's emphases (a) on continuation of the '*constant conjunction*' (of types of object encountered, in our experience, under certain conditions) associated with the concept of *causation*, as made explicit in his more detailed accounts of the relevant presumption in passages such the above – as the defining feature of the 'uniformity', or 'resemblance' referred to in the more opaque abbreviations, and (b) on the logical status of the supposition within the inference as a *founding presumption*, or what in more modern logical terms we would call an 'assumption'. In view of these emphases, a more appropriate name might be something like the *assumption of causal continuity*.[43]

Despite the opacity of most of his abbreviated expressions of the relevant presumption, the same emphases are also clearly present in the contexts of the relevant passages in the other documents in which these expressions occur. For example in the *Abstract* Hume stresses that:

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inference according to Hume's account. (I did of course alert them that to the point that was not the traditional interpretation).

[42] I discuss three variations, and the question of their equivalence, in the context of my analysis of a potentially confusing passage in the *Abstract*, in the Appendix.

[43] There is also an element of variation in Hume's representation of the *content* of the presumption. The minimal version of the assumption is simply that the *constant conjunction* (in the cases we have observed) of the types of object, event, or property or whatever that we *regard* as causally related *will* be maintained in cases we have not observed. However, Hume sometimes presents a variation that also appears to imply that the conjunction of the respective objects is indeed causally necessitated. This variability is reflected for example in Hume's treatment of the non–modal 'will' and the modal 'must' as interchangeable in the abbreviatory form, 'that the course of nature will/must continue uniformly the same' (*Abstract*, paras. 13/14). Of course the basic condition of *continuity* is sufficient for the purpose of *prediction* with regard to the unobserved, but it is not implausible to suppose that Hume is right in supposing that we make that presumption *only* when we think the observed constancy of succession *is* due to a *causal* link of some kind. (Hume does not explicitly address the possibility of a common cause as distinct from a direct causal link in the case of constant conjunction, but in either case it is supposed to

This is the inference *from cause to effect*; and of this nature are all our reasonings in the conduct of life: On this is founded all our belief in history ... (*Abstract*, para. 10, my emphasis)

that all reasonings *concerning cause and effect* are founded on experience, and that all reasonings from experience are *founded* on the supposition, that the course of nature will continue uniformly the same.[45] (*Abstract*, para. 13, my emphasis, my interpretation in parenthesis)

Essentially the same view of inductive inference is maintained in Hume's account in the *Enquiry*:

We have said, that all arguments concerning existence are *founded on the relation of cause and effect*; ... and that *all our experimental conclusions proceed upon the supposition,* that the future will be conformable to the past. (*Enquiry* 4.2. para. 6, my emphases and interpretation.)

It seems clear then that Hume thinks that our inferences about the unobserved are based not *merely* on what we presume from our experience to be *observed*, but *also,* and *crucially,* on a supplementary presumption of the continuity of (apparent or supposed) causal regularities.

On Hume's account of induction then it would seem that it is this *additional assumption* of causal continuity that *controls* the inferences we draw from our *other* basic assumption of the veracity of our supposed observations. In this case, it is not unreasonable to suppose that if Hume were equipped with our modern conception of 'deductive' inference, he would have no particular difficulty in acknowledging that on the basis of *these* assumptions, the conclusions we draw in respect of similar outcomes, unobserved at the point of inference, may in fact be *validly* drawn. *Hume's* problem for inductive inference then *does not rely on any supposition that our inductive inferences are not, or cannot be, deductively valid.*

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be causal necessitation that is responsible for the constant conjunction, albeit in the former case joint necessitation by some other, possibly unknown, factor.)

[45] Hume continues: 'We *conclude*, that like causes, in like circumstances, will always produce like effects.' (*Abstract*, para. 13, my emphasis). In the Appendix I defend my view that despite the misleading terminology Hume also regards this proposition as a founding assumption in the context of our inductive inferences, that in fact he regards it as equivalent to the immediately forementioned 'founding supposition', and that his use of the phrase 'we conclude' here is therefore indicative of nothing more than tautological implication.

Hume's view of the problem for the justification of induction

If this analysis is right, if Hume's problem for the justification of induction is not essentially a problem about the *validity* of inductive inference, then what is it? From Hume's account it would appear that his problem for the justification of inductive inference is rather simply that a crucial *assumption* that we make when we make inductive inferences, namely the assumption of causal continuity, *cannot be rationally justified*:

> 'tis impossible for us to satisfy ourselves by our reason ... *We suppose, but are never able to prove*, that there must be a resemblance betwixt those objects, of which we have had experience, and those which lie beyond ['which we *call* cause or effect'] (*Treatise* 1.3.6.11. [ref: 1.3.6.4.], my emphases)

It is with regard to this *assumption* of inductive inference that Hume claims, in the *Treatise*[46], that no 'demonstrative' proof can be had:

> there can be no demonstrative arguments to prove, that those *instances,* ['which we call cause or effect'] *of which we have had no experience, resemble those, of which we have had experience.* (*Treatise* 1.3.6.5. [ref: 1.3.6.4.])

and Hume *rightly* argues *that* claim on the basis of the consistency of supposing that past regularities will not continue. Hume's point here is simply that this commonly presumed fundamental connection between the observed and the unobserved is *not a logical truth.* This is *not* equivalent to a claim that our inductive inferences cannot be deductively valid. Indeed, as we shall see in the following section, granted Hume's view of the *structure* of inductive inference, as founded on this assumption *in conjunction* with the assumption of the veracity of our perceptions, it would appear that inductive inferences so construed *will* in general be valid.

Hume's problem for the justification also of course involves consideration of the possibility of a non–demonstrative proof of this presumption of continuity. Thus in the *Abstract* he goes on –

> Nay, I will go further, and assert, that he could not so much as prove by any probable arguments, that the future must be conformable to the past. All probable arguments are *built on the*

[46] The precise context of interpretation is however different at the point in the Enquiry from where Howson takes his similar quotation. We shall examine Howson's line of argument at that point shortly.

supposition, that there is this conformity betwixt the future and the past, *and therefore can never prove it.* (*Abstract*, 14)

Here Hume again makes it quite clear that he sees this as a fundamental presumption that we employ when we make inductive inferences – something that we simply *assume* or *suppose* to be the case – which we '*take for granted* without any proof' (*Abstract* 14). And of course *the problem* on Hume's account, with such inferences, is not that the inference to the conclusion of the inductive inference isn't what we would call 'valid' – however Hume might have chosen to phrase such a claim if he did indeed think that something like that was the case and *wanted* to make it – but quite simply that this fundamental presumption *cannot itself be proven.*

A Humean, deductively valid, model of inductive inference

The general structure of inductive inference, on the basis of the above–described account offered by Hume, with only modest interpretive speculations, would appear to go along something like the following lines:

(1) An instance a of the particular type ['species'] (of object, event, or property of an object) F has been observed, but we have not observed whether it has been (or will be) succeeded by an instance of type G.[47]

(2) In all of the many ['frequent'] cases where we have observed whether an instance of F has been followed by an instance of G, they *have* always been followed by instances of G, and our experience of these cases has been of a kind (sufficiently numerous, suitably varied, etc) that inclines us (reasonable, and scientifically–minded people) to *regard* their succession as due to causation.

(3) *Whenever* all of the many observed instances of A (whose succession or otherwise by an instance of another type B has been confirmed by observation) *have* been succeeded by an instance of B (and our experience of these cases has been of a kind that inclines us to regard their succession as due to causation), any observed instance of A whose succession or otherwise by B has not been confirmed by observation *will* be succeeded

[47] Note that this entails the presumption of the *veracity* of our apparent observation (with respect to a). Similarly for (2).

by an instance of B. [The (general)[48] assumption of the continuity of (apparently) causal regularities]

(4) *Therefore*, object a will be succeeded by an object of type G.

Whether this or something like it, as Hume appears to have thought, is an *authentic* representation of our actual inferences from the observed to the unobserved is another matter. But it is apparent from the passages we have examined that at least the main features of the model outlined above would need to be incorporated in any reasonable construal of Hume's view of the basic structure of inductive inference:– As we have seen, the key point emphasised and maintained by Hume is that in addition to the basic *observational* assumptions, as represented in my (1) and (2), there is *also* a *founding presumption* (whether general or specifically relating to the types mentioned in (1) and (2)) that [certain] types of object hitherto (in our experience) 'constantly conjoined', and regarded as being so by virtue of causal necessitation, will *continue* to be so – a presumption that is, in the context of the inference 'taken for granted', and in that respect accorded the status of an assumption – an assumption which I have attempted to represent schematically, and in its general form, in my (3). The conclusion of a Humean inductive inference is of course the prediction that the expected but unobserved 'effect' will in fact occur, as represented by my (4).

What is particularly notable about this *Humean* model of the logical structure of inductive inference is that it *is deductively valid* even if, as Hume argued, no 'proof' or satisfactory argument can be provided in *support* of the crucial assumption (3). To clarify the validity of this form of inference we may first note that the particular implication of the general assumption of causal continuity (3), that applies to the particular types of object mentioned in any particular instances of the other premises, may be expressed schematically in the conditional form:

(3a) *If* all of the many observed instances of F for which succession or otherwise by an instance of G has been confirmed by observation *have* been succeeded by an instance of G (and our experience of these cases has been of a kind that inclines us to regard their succession as due to causation) *then* any instance of F whose

[48] The idea here is that to obtain an instance of this argument schema, that employs the *general* principle of continuity, *F* and *G* will be substituted by specific values such as 'a body of like colour and consistence with that of bread which has been eaten' and 'nourishment', while (3) will be read as a universal statement applying in the case of these values as well as any others. Of course, Hume also conceives of *particular* versions of this generalised assumption. The schematic account of the particular version may be represented by the corresponding conditional, and is presented in (3a) below.

succession or otherwise by G has not been confirmed by observation will be succeeded by an instance of G.

Since the antecedent of this conditional is equivalent to premise (2), the consequent

> any instance of F whose succession or otherwise by G has not been confirmed by observation will be succeeded by an instance of G

follows (from (2) and (3a) by modus ponens). And from this in conjunction with premise (1) the conclusion (4) follows by universal elimination.

The ontic status of the principle of continuity

(3) does look somewhat odd once we spell out the pertinent details as I have in the above. In particular it looks odd because, once we are quite clear about what it implies, it doesn't look quite so *innocuous* as it appears (at least on the surface) in its abbreviated forms – 'the course of nature will continue uniformly the same', 'the future will resemble the past', 'like causes will always produce like effects', and so on.[49] In fact we know that *sometimes* such apparently causal constant conjunctions in our past experience are followed by exceptions. In other words we know that as it stands, this *general* assumption is strictly (and relatively obviously) *false*. The *temptation* of course is to interpret the principle in a more charitable manner, and the natural and simple amendment in this respect would be to rephrase the principle in epistemic terms – basically by adding some appropriate epistemic qualifier to the consequent, thus '*it is reasonable to believe that* any observed instance of A whose succession or otherwise by B has not been confirmed by observation *will* be succeeded by an instance of B. Furthermore, such an epistemic interpretation might appear to be suggested by Hume's own description of the situation in a particular instance, in the case of the nourishing powers of bread:

> If a body of like colour and consistence with that of bread, which we have formerly eaten [and by which we have always been nourished] be presented to us, *we ... foresee, with certainty*, like nourishment and support. (*Enquiry* 4.2.3., my emphasis).

Now such an interpretation of the principle of continuity would be particularly appealing to me from a theoretical point of view, since I ultimately want to argue that our inductive inferences and arguments generally *need* to be represented in just such epistemic terms. And it would be particularly helpful to my case if I could find support for this *further* point about the logic of induction in the arguments of Hume. However, I do not think the textual evidence concerning Hume's perception of the ontic–or–epistemic status of the principle supports such an interpretation. What is clear about the above–cited passage is that it is an account of Hume's view of the *psychological* situation when we engage in such inferences. But it does not appear to be intended to be read as a description of an *instance of the presumed principle* itself. Rather it simply appears to be, as it reads, a psychological account of the *inference*. When we look at the logical form of his unequicocal descriptions of *the principle* – 'the course of nature will continue uniformly the same', 'the future will resemble the past', 'like causes will always produce like effects', and so on – they appear in general to be straightforwardly *ontic* propositions. They are *not*, in any case, presented as *epistemic* propositions.

Of course in the end Hume wants to argue that arguments based on this principle won't satisfactorily account for our confidence in their conclusions, since we cannot provide any *justification* for the principle. And in order then to account for our confidence he appeals instead to psychological 'principles of union among ideas' (*Treatise* 1.3.6.13.). But the two should not be conflated. In Hume's view then it would appear that (what we might call) the *logical* principle of continuity on which our *arguments* are based – that hitherto constant conjunctions in our experience *will* continue – is an *ontic* assumption. And this needs to be distinguished from the *psychological* principle – that *we habitually presume* that long standing conjunctions in our experience will be continued.

A tempting response to Hume's view of induction, and Hume's rebuttal of it.

As Hume acknowledges, and discusses at some length in the *Enquiry*, we might like to think that the crucial presumption (3) (or some variant of it) 'may justly be inferred' from its observational antecedent (2), and indeed one of Hume's own comments in this respect might by some interpreters be regarded as supporting Owen's suggestion that Hume's problem of induction leaves open the question of justification:

> I have found that such an object has always been attended with such an effect, and I foresee, that other objects, which are in appearance, similar, will be attended with similar effects. I shall

[49] I am grateful to Jonathan Lowe for alerting me to the need to attend to this difficulty with my construal.
[51] This mirrors Hume's comments on other alternative proposals about the logical structure of inductive inference discussed and dismissed in the Treatise: 'Should anyone think to elude this argument; and … pretend that all conclusions from causes and effects are built on solid reasoning: I can only desire that this reasoning may be produc'd, in order to be exposed to our examination.' (*Treatise* 1.3.6.8.)

allow if you please that the one proposition may justly be inferred from the other. (*Enquiry* 4.2.3.)

However, close attention to the context confirms that this may most appropriately be interpreted, as indeed it appears to be phrased, as a 'for the sake of the argument' hypothetical concession relating specifically to the sake of the (broader) argument about the *explanation* of our inductive inferences, rather than a *genuine* concession on the (hereby momentarily set aside) issue of justifiability, as is illustrated by the continuation of his point:

> But if you insist that the inference is made *by* a chain of reasoning, I desire you to produce that reasoning. … and it is incumbent on those to produce it who assert that it really exists. (*Enquiry* 4.2.3. my emphasis)[51]

In other words what Hume appears to be saying here is something like this: Let us set aside for the moment the question of whether our inductive inferences may somehow be justified. Suppose for the sake argument there *is* some justification for such an inference. Nevertheless the problem for the traditional explanatory view that we proceed in our inductive inference *by* reasoning remains: If we *did* proceed by reasoning we should then be able to provide a satisfactory account of *how* our inductive inferences are justified, i.e. by what process of reasoning we *do* in fact justify the conclusion.

In contrast Hume *genuinely* concedes merely that we *make* such inferences (whether or not justly)

> I know *in fact* that it always is inferred (*Enquiry* 4.2.3., my emphasis)

But what Hume means by this, in line with his earlier account of 'inference' as a 'transition', is simply that the learning of the first proposition somehow causes, is followed by, or results in, an associated 'belief' of the second. The process by which this transition takes place might or might not involve a process of *reasoning* or rational justification. Hume's view on this, as detailed above is that although the learning of the first proposition somehow causes or 'induces' us to believe the second, it does so only via a presupposition of continuity which is 'taken for granted' without justification in the context of the inference – and which is thereby properly represented within the *logical* structure of the inference as an *assumption*.

What Hume does *not* mean by this, is that the inference is, or may justly be, drawn *without* association with any *supplementary* factors. In fact he goes on to insist that

> There is required a *medium* [a 'connexion between these propositions'] which may enable the mind to draw such an inference, if indeed it is drawn by reasoning and argument. (*Enquiry* 4.2.3., my emphasis)

Such a connecting reason or 'medium' would consist in an additional intermediate belief which, in association with the initial proposition, provides us with a satisfactory reason for believing the second.

The trouble that Hume finds with such a theory though is that, as he has already argued, there is no logically true proposition that will facilitate such an inference, and *any* inference to a proposition beyond the logical implications of our observational information must be *founded* on an assumption of causal continuity.

> You say that the one proposition is an inference from the other.[52] But you confess that the inference is not intuitive; neither is it demonstrative: Of what nature is it then? To say it is experimental, is begging the question. For *all inferences from experience suppose, as their foundation, that the future will resemble the past, and that similar powers will be conjoined with similar sensible qualities.* (*Enquiry* 4.2.8. my emphasis)

Once again Hume here *reasserts* his central claim (italicised) that causal continuity is in fact an indispensable and *fundamental assumption* of any inductive inference. The clear implication is that *as such* it is *not* (and could not be) supported by any independent (non–circular) justification

It is in the context of this *defence* against the suggestion that things might be otherwise – that perhaps, on the contrary, we *can* justify the principle of continuity – that we find the quotation presented by Howson (2000, p10, as above):

> That there are no demonstrative arguments in this case seems evident; since it implies no contradiction that the course of nature may change, and that an object, seemingly like those we have experienced, may be attended with different or contrary effects. (*Enquiry* 4.2.5.)

[52] The two propositions actually mentioned at this point (*Enquiry* 4.2.8.) are slight variations on the original propositions mentioned (above) at *Enquiry* 4.2.3. These later variations (4.2.8.) are 'I have found, in all past instances, such sensible qualities conjoined with such secret powers' and 'similar sensible qualities will always be conjoined with similar secret powers'. In each case I think that the representative expressions employed in the prose may reasonably be construed to encode something like the underlying logical structure and relations indicated by my (2) and (3).

As we noted at the outset, Howson (along with many others) appears to take this quotation to support the view, and moreover an attribution to Hume of the view, that our inductive inferences (as these were understood by Hume) cannot be deductively valid. But if our analysis of the context of the quotation is correct, both that view, and the attribution of it to Hume, are a mistake. The assumptions that constitute the logical foundations of our inductive inferences, which according to Hume include a presumption of the principle of continuity as well as the content of our relevant observations, do in fact yield a valid argument for the predictive conclusions of those inferences. Hume does not raise any objection against that. He does not even raise that *issue* in the context of his discussion. What Hume has a problem with is the justifiability of our (alleged) *assumption* that the regularities we observe will continue. The point of the above quotation is simply to set out the straightforward point that this assumption cannot be justified on the basis of any proof that it is a *logical truth* – or in Hume's terms that it can be justified by a 'demonstrative' argument.

As we noted earlier there is a related but crucially different point that Hume *does* hold – which is the obvious point that the premise, (roughly) that two types of object have hitherto been frequently and constantly conjoined, does not, *on its own*, imply that such hitherto conjoined types of object will *continue* to be so conjoined.

> Let the course of things be allowed *hitherto* ever so regular; *that alone*, ... proves not, that for the future, it will *continue* so. (*Enquiry* 4.2.8. my emphasis)

But Hume here merely takes that to be part of the case for his *rejection* of the suggested response to his central claim – i.e. the claim that no justification can be had for the fundamental assumption of continuity on which all our inductive inferences depend. As Hume claims here, past experience alone is insufficient to prove the continuity principle, and as he has already argued, considerations of logical possibility don't prove it, and it cannot be proven by invoking the principle itself, as that would obviously beg the question. What proof then, Hume asks, could there possibly be?

Stove's response to Hume: Logical probabilities

In view of this conclusion it would seem that Stove is mistaken when he attributes to Hume what he calls a 'Deductivist thesis' that

> *all* [deductively] *invalid arguments are unreasonable* (Stove, 1973, p50)

Whether or not Hume might have been inclined to agree this thesis, it is an issue he does not address and on which he does not comment.

HoweverStove ultimately interprets this thesis in terms of a concept of *logical probability*:

> My identification of [Hume's] argument [for inductive scepticism] (and in consequence my evaluation of it) involves the identification of Hume's sceptical conclusion, as well as some of his premises, as being *statements of logical probability*. (Stove 1973, p 1, Stove's emphasis)

as the thesis that 'For all [deductively] invalid arguments from e to h, P(h/e&t) = P(h/t)' (where t is a tautology). He then goes on to employ an argument attributed to Von Thun to show that this does not hold when h implies e, and so concludes that this so–called 'Deductivist thesis'[55] is not true.

Stove at first makes his account of logical probability sound innocuous:

> A statement of logical probability is made *when and only when* we assess the probability of one proposition in relation to another proposition, which picks out possible evidence for or against the first. (Stove, 1973, p7, my emphasis)

However, despite the explicit biconditional, it would seem Stove is not content to settle with this relatively innocuous account and adds a further and highly contentious condition on what, on his interpretation of the term (following the tradition of Carnap and Keynes), is to count as a statement of logical probability. Stove insists further that what determines the truth or falsity of a statement of *logical* probability, as such, is a particular *kind* of relation between the hypothesis and evidence statement. Specifically, he thinks that it is (what on a natural interpretation of his account amounts to) a *semantic* relation between the two propositions that determines the truth

[55] This equation is equivalent to an assumption of *independence* of h from e, and so in the context of a logical theory of probability will amount to an assumption that h is logically probabilistically independent of e. Typically deductivists will not even acknowledge a viable concept of logical probability, let alone define their own position in terms of such probabilities.

or falsity of the 'evidential' relation that is asserted when a proposition of *logical* probability is asserted:

> That relation is fully fixed once the content of each primary proposition is fixed; and what those contents are, is not a question of fact, but a question of *meaning*. (Stove 1973, p8, my emphasis)
>
> With these assertions I think everyone would agree (Stove 1973, p9)

However Stove does not seem to realise that the logical theory of probability, even at his time of writing, was largely regarded as discredited:

> there seems at present little hope of successfully rehabilitating the Principle of Indifference as a logical principle. ... Altogether the difficulties in the logical interpretation had, by the 1920s, reached such a level that the Bayesians really needed a new interpretation of probability (Gillies, 2000, p49)

As we have seen, *Hume's* understanding of the then prevalent concept of probability looks a lot like the classical theory, so it is not entirely inconceivable that his comments on its significance in the context of probabilistic induction may have been intended in the closely related logical or semantic sense.[56] But in any case, even if we do interpret his concept of probability in this sense, what Hume says *about* probabilistic inference does not appear to be anything like what Stove suggests he claims.

There are two main aspects to the logical theory of probability, when it is taken to have any *epistemic* implications. The basic (logical) aspect is the analysis of the respective logical possibilities logically permitted by the evidence. The crucial epistemic aspect is the assignment of epistemic probabilities (degrees of belief or expectation) to the respective possibilities, generally in accord with some form of principle of indifference. This principle, in such epistemic application, tells us that if there is no known reason for expecting one possibility more than another of several alternatives, then each of these possibilities should be regarded with equal expectation.[57] But Hume's argument against such an approach is basically that the partition of the logical possibilities provides us with no rational basis for *any* such assignments of epistemic probabilities. For Hume we have a matter of chance when causation does not

[56] For a comparison of the two theories see Gillies (2000, chs. 1 and 2).
[57] c.f. Gillies (2000, p35).

determine the matter, and there are various *possibilities*. Thus Hume emphasises that the *question* in hand is

> by what means a superior number of equal chances [or possibilities] *operates upon the mind*, and pronounces belief or assent (*Treatise*, 1.3.11.8, my emphasis)

In this case, Hume's point is that not *only* do we never have any (non–question–begging) reasons for expecting one of the possibilities more than another (as is required for application of the principle of indifference) but *nor* do we ever have any *non–question–begging* reason for regarding all such possibilities with *equal* expectation (as the principle would then have us suppose).

Conclusion

Following the thread of Hume's analysis of inductive inference through the *Treatise*, the *Abstract*, and the *Enquiry*, we have seen that Hume insists throughout that the supposition of causal continuity is an indispensable and founding *assumption* of our inductive inferences. On this, Hume's, understanding of the logical structure of inductive inference, the justificatory problem for inductive inference is *not* that our inductive inferences are not or cannot be *deductively valid*. In stark contrast, on Hume's account of their logical structure, it would seem that in fact our inductive inferences *are* deductively *valid*. The *justificatory* problem, as Hume sees it, is precisely that this is an *indispensable assumption*, and unfortunately one for which we are unable to present any non–question–begging justification, and for which, it would seem, none could possibly be provided.[58] Hume finds basically the same problem for probabilistic inductive inference as he does for causal inference, in that no non–question–begging argument

[58] This is to be distinguished from the Stove/Mackie view that Hume believes that the only authentic form of justification is deductively valid argument, and for that reason our inductive inferences *must* be deductively valid if they are to be justified. On *that* view, the justificatory problem that *Hume* is supposed to find for inductive inference is (in accordance with the common but mistaken account of Hume's view of the structure of induction that we have exposed) that inductive inference is *not* deductively valid (and therefore, on the Stove/Mackie premise, cannot be justified). In contrast we have seen that on Hume's view of the logical structure of inductive inference it *is* in fact deductively valid, and there is no reason to suppose that Hume would not have acknowledged that.

can be provided in support of the assignment of probabilistic expectations to the various logical possibilities permitted by the evidence.

CHAPTER THREE
WAS HUME RIGHT ABOUT THE STRUCTURE OF INDUCTIVE INFERENCE?

The main aim of my analysis of Hume's problem was to establish that, according to this classical account of (causal) induction, it would be a mistake to identify inductive inference with 'non–deductive' inference, or any particular type of non–deductive inference, and to clarify the point that Hume at least perceived the problem of induction as an issue concerning the ultimate justification of the *presumptions* underlying our inductive inferences, rather than any problem with their *validity* as such. We have established this much in our analysis thus far. However, despite the unquestionable importance of Hume on the issue of induction, we cannot take this to be the end of the story with regard to the issue of the logical adequacy of induction. One question we need to consider is whether Hume was (at least roughly) *right* about the basic structure of inductive inference and arguments. And although it is not my intention to attempt to address the epistemological issue of the justification for the basic assumptions and prior expectations we employ in induction (as distinct from the validity of the inferences *granted* those assumptions), it would be inappropriate to proceed without placing this issue in the context of some comment on the question of whether Hume might be mistaken in his view that we do in fact make such assumptions.

In a recent article Samir Okasha (2001, p307) argues that Hume *is* mistaken in this regard, and although Okasha does not present any detailed interpretive analysis of Hume's text, his outline of the basic elements of the argument, as he understands it (p312) is in close agreement with that defended in my own justified analysis above. Although Okasha bases his analysis on the version in the *Enquiry* it is notable that his summary of the argument also appears to provide a good match with our analysis of the version of it which Hume presents in the *Treatise*.[59]

> Hume's sceptical argument can … be broken down as follows:
>
> 1. Arguments from past experience, or 'probable arguments' proceed upon the supposition that nature is uniform[60]

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[59] In my (3) – (4), figure 1 summarising Hume's argument in the *Treatise* above, I have also presented some of Hume's background considerations underlying Okasha's (1) (my (5).

[60] Okasha makes it clear that he is using the phrase 'nature is uniform' to abbreviate the claim that Hume himself abbreviates (with a variety of expressions) after initial elaboration in the context of both the *Treatise* and the *Enquiry*, and which relates to the *continuation of past regularities* that we expect: 'The description is simple: we observe past regularities, and project them into the future. [Hume] expresses this by saying that all our experimental reasoning 'proceeds upon the supposition' that nature is uniform' (Okasha, 2001, p312). This phrase is clearly related to the second part of the clause as quoted in my (1) in figure 1.

2. Unless we have reason to believe that nature really is uniform, we have no reason to believe the conclusions of arguments from past experience
3. The proposition 'Nature is uniform' cannot be established by demonstrative argument, as its negation is not self–contradictory
4. The proposition 'Nature is uniform' cannot be established by probable argument, as we only have reason to believe the conclusions of probable arguments if we already have reason to believe that nature is uniform, as (2) says
5. If a proposition cannot be established by either demonstrative or probable argument, then we have no reason to believe it
6. So we have no reason to believe that nature is uniform [from (3)–(5)]
7. So we have no reason to believe the conclusions of probable arguments [from (6) and (2)], i.e., past experience gives us no reason to believe anything about the future

(Okasha, 2001, p312)

In particular it is notable that Okasha, in contrast to Howson (2000) appears (at least at this initial point of interpretation) to acknowledge that Hume's problem is essentially a problem concerning the *justification for the presumption* of continuity (or 'uniformity') that Hume insists we employ in inductive inference (rather than suggesting it is a problem with the validity of the inference as, we have seen, many authors do). Okasha then attempts to rebut Hume's argument for his sceptical conclusion by denying Hume's basic premise that inductive inferences 'proceed on the supposition that nature is uniform'. He claims in contrast that we might proceed by Bayesian conditionalisation rather than by any question–begging presumption of regularity continuity, or by any associated 'inductive rules' that might be taken to reflect such substantive presuppositions. Furthermore, Okasha concludes that on such a view of inductive 'inference' Hume's charge of begging the question would not apply. This conclusion stands in stark contrast to that of Howson's detailed discussion of Hume's problem, from a Bayesian point of view, that

> *no* theory of rationality that is not entirely question–begging can tell us what it is rational to believe about the future, whether based on what the past has displayed or not. ... what [evidence] does tell us cannot be unmixed from what we are inclined to *let* it tell us. ... Hume was right. (Howson, 2000, p239–40, my emphases)

While it is not my intention to discuss the various response that have been made to Hume's problem, I think it is worth giving some consideration to this particular line of response, particularly in view of the current popularity of Bayesian theory of probability, in order to provide some support for my agreement with Howson that Hume's broader problem for inductive inference in general applies just as well to the view that we proceed in our inductive predictions by Bayesian conditionalisation.

Okasha notes that it is generally acknowledged that Hume's account of our inductive practice – of the presumption of the 'uniformity of nature' – is a considerable simplification ((b), p308), but that it is also commonly supposed that the basic line of argument does not depend on the finer details of that matter ((d), p310). He also notes that there are two fundamentally different views of the nature of our inductive practice. One is the view that we do employ *some* rules or 'principles' in our inductive inferences, which may be regarded as equivalent (or at least correspond) to an empirical assumption which says 'The world is such that the rule of inference is truth conducive' (p314). Another is the view that we don't employ any such rules of inference at all. Okasha suggests that updating one's beliefs in the light of incoming evidence on the basis of Bayesian conditionalisation does not involve a commitment to any such rules or associated empirical assumptions. The crucial difference according to Okasha is that

> a rule of inference is supposed to tell you what beliefs you should have, given your data, and the rule of conditionalisation does not do that (Okasha, 2001, p316)

Given the correspondence of such rules of inference with empirical assumptions, Okasha argues that whether or not Hume's problem would survive the correction of his account of our inductive practice *would* depend on whether we do employ such (I suggest we call them) 'substantive' rules of inference (or their associated empirical assumptions). His basic argument goes as follows:

> Hume argued that the reasonableness of our opinions about the unobserved is conditional on an empirical assumption for which we have no non–circular justification. ... Hume's inductive scepticism would be quite right, if we did use rules of inductive inference [that correspond to empirical assumptions]. But if we do not, then a Humean sceptical argument will not go through. (Okasha, 2001, p320, my parenthesis)

We shall examine his elaboration of the point shortly. Before proceeding though we may note that Okasha somewhat oversimplifies Hume's view of the empirical assumptions we employ when we engage in *causal* inference (which is the only part of Hume's broader argument that Okasha addresses) and completely ignores Hume's comments on the 'assumptions' we make when we engage in *probabilistic* inference. For the moment I shall leave aside Goodman's problem for Hume's *alleged* claim that we presume that regularities *in general* will continue, since this warrants a dedicated chapter. However, with regard to our current concern with the exposition of Hume's concept of induction and his problems or it, we should note here that it is by no means clear that Hume did intend to claim that we presume that *all* regularities will continue. In fact it seems quite clear that he did not intend any such absurd suggestion.

Hume's original description of the nature of causal inference in *Treatise* 1.3.6.2 may undoubtedly be interpreted as a description (whether or not simplified and recognised as such by Hume) of what takes place *whenever we make causal inferences*. But that does not imply that *we make causal inferences whenever* we make any observations that conform to Hume's (possibly simplified) description of certain observational conditions that hold whenever we make causal inferences. Furthermore, if we suppose merely that Hume's account of those conditions is at least *to some extent* simplified and recognised as such by him, then he *could not consistently also* think that we make inductive inferences *whenever* those conditions hold.

Add to this certain comments by Hume that appear to make it clear that he fully acknowledges that there *is* more to be said about the *precise* conditions under which we concede casual conclusions. In particular Hume makes a number of references to our 'experimental' conclusions, and makes his appreciation of the complexity of the conditions for projectibility quite clear:

> Nothing so like as eggs; yet no–one, on account of this appearing similarity, expects the same taste and relish in all of them. It is only after a long course of uniform experiments in any kind that we attain a firm reliance and security with regard to a particular event. (*Enquiry* 4.2. para 7)

This would seem to place beyond reasonable doubt that Hume was fully aware that it is not *merely* the regularities that arise in our experience themselves that are relevant to causal inference, but the *background conditions* – notably, in the context of scientific investigation, controlled experimental conditions – *under which those regularities are observed* that are *also* relevant.[61] In view of this it should be quite clear that Hume neither thought nor intended to suggest that *all* the regularities we experience are projected.

One thing Okasha seems to fail to realise (going by his discussion of the 'popular opinion' (b) on p309) is that the suggestion that they are *all* projected was *not* an assumption of Hume's problem, and indeed that Hume's cavalier approach to the various and obviously simplistic

[61] It is for this reason that I insert the clause 'and our experience of these cases has been sufficient for us to regard their succession as due to causation' in my earlier account of the general principle of continuity, (3) in my analysis of Hume's view of the logical structure of (causal) inductive argument.

descriptions of the principle of continuity in the course of his presentation of the problem only serves to illustrate the point that *Hume himself* understood that the problem he was presenting was *not dependant* on the finer details of the projection conditions. In this particular respect Howson is more accurate in his interpretation than Okasha since he acknowledges that Hume intends merely that

> all inferences from past experience suppose as their foundation, that the future will resemble the past
>
> – *in some way or other.* (Howson, 2000, p11, my emphasis)

and *not* in *all* respects. Okasha, seems to think that to say that would be 'to say nothing' since

> Nature … cannot fail to be uniform in some respects if it is to be describable at all. (Okasha, 2001, p309)

But this is to fail to grasp the fundamental *point* of the 'uniformity' or (more to the point) 'continuity' assumption. That nature *has* displayed some regularities or uniformities *thus far* is a condition that *is* necessary for nature to be describable. But the crucial feature of the continuity assumption is the implication that these regularities will *continue* to hold in the future. And, whichever regularities may be predicted to continue, that those particular regularities *will* continue is *not* a necessary condition for the fact that nature is describable. To illustrate the point, one of the implications of the continuity assumption is that the sun will continue to rise each day. But we are nevertheless perfectly able to *describe* the hypothesis that it will *not* rise tomorrow.

Similarly, Okasha's purported counterexamples to Hume's claim that we proceed by a presumption of continuity illustrate the extent to which he has failed to grasp the considerable subtlety and relative modesty of Hume's actual assumptions in this regard. For example Okasha complains that

> All of the Costa Ricans I have ever met are philosophers, *but that does not lead me to believe that all Costa Rican's are philosophers*, nor that the next Costa Rican I meet will be a philosopher. Expectation–formation takes place in the light of a vast store of information. (Okasha, 2001, p309, my emphasis)

But there is no reason to suppose that Hume would have intended that the limited kind of experience that underlies regularities such as this are of the kind that lead us to suppose that the regularity is the result of a casual connection. In fact there is every reason to suppose that he did *not* intend that regularities such as this are generally taken to be the result of causal connections. The above qualification relating to extensive experimental grounding (*Enquiry* 4.2. para 7) makes that clear enough.

Furthermore, Hume's problem does not depend, and is not supposed to be framed, on an *individualist* position with regard to what observational data is permissible in our inductive inferences. Hume makes no objection to the standard supposition that we *share* observational information. In this particular example then, in contrast to Okasha' suggestion, there is no need to appeal to 'a vast store of background information' to block the causal inference and associated universal projection (even if it were a *potential candidate* for projection, which as we have noted above it is not). The only information to which we need appeal in order to block this universal projection is the common observation–based knowledge that some Costa Ricans are not philosophers.

On the basis of this profound misunderstanding of Hume, Okasha goes on to argue as follows

> The inductive principle which [Hume] says we employ is one which we most certainly do not employ, as Goodman showed. We believe that some observed regularities will continue into the future, but not others. The first two premises of Hume's argument are therefore false. (Okasha, 2001, p315)

But all this shows is that Okasha has simply failed to realise that *Hume did not deny* that we believe that *some* regularities will continue into the future, but not others, and furthermore that his argument does not depend on any such denial. Okasha has seriously misunderstood Hume's basic premise, and the point noted (and obvious enough without reference to Goodman) serves only to falsify an absurd claim (the claim that we believe that *all* observed regularities will continue into the future) which Hume did not make, and clearly enough did not intend.

However Okasha also has a more serious contention to make. In genuine contradiction to Hume, Okasha wants to claim furthermore that *some* cases of causal prediction do '*not involve* the extrapolation of a past regularity into the future' (p309, my emphasis). And he thinks that an example (which he attributes to Putnam) 'neatly illustrates' this:

> Usually when you slam two small objects together, nothing in particular happens. However when you slam two high speed neutrons together, you get an atomic explosion. This is not a chance event: scientists predicted that it would happen. Their prediction was entirely 'theory–driven': it clearly did not involve the extrapolation of a past regularity into the future, as per Hume's account. (Okasha, 2001, p309)

But Okasha here appears to have failed to grasp Hume's preliminary clarifications in the *Treatise* of the depth of dependency of our causal predictions on assumptions of certain matters of continuity. To apply Hume's basic observations to this example, even the supposition that performing certain actions will result in two high speed neutrons 'slamming together' involves for example the presumption that the neutrons will continue to exist, and will remain in (an at least approximately continuous line of) motion throughout the 'slamming' process. Such background presumptions, explicitly acknowledged by Hume, are by no means 'entirely theory–driven': Small objects *in general* tend to continue to exist when fired towards one another (at least up until the point of contact) and they also *generally* tend to follow a continuous line of motion (thereby ensuring that they collide if the lines of motion converge on a single point in space–time). Likewise with regard to the outcome: Surely Okasha cannot expect us to believe, as he suggests, that scientists established their findings about atomic reactions *entirely* by hypothetical theory construction and *without any employment of repeated trials of controlled experiments*, as on Hume's account would be required. It is of course common knowledge that a great deal of repetitive experimental research went into the development of atomic theory.

Particular rules

Despite his former suggestion that to presume that *some* regularities will continue 'is to say nothing' (p309), it would seem that Okasha nevertheless wants to allow (what in fact Hume claims) that we believe, in advance, that *some* observed correlations between events will continue to hold in the future (presumably since clearly we do). In defence of his position, then he needs to be able to maintain that a presupposition that any such *particular* regularities will continue does not amount to following a (substantive) rule of inference. (In fact what Okasha ultimately requires is even more basic and more obviously problematic than this – a point we shall follow up below. For the moment though let us consider his argument with respect to inductive rules.) On this point Okasha begins his argument as follows:

> If we believe that some observed correlations between properties will continue to hold in the future, that does not mean that we are using the inductive rule 'Infer that properties correlated in the past will be correlated in the future' (Okasha, 2001, p318)

All that this point shows of course is that a presumption of the continuation of *some* (particular) observed regularities does not correspond to the *universal* rule which says that *all* observed regularities should be projected, it does not show that such an assumption does not amount to following *any* rule. Okasha then proceeds to argue, via criticism of contrary suggestion by Jackson, that such a partial belief does not amount to 'following a rule' at all, and can be explained without any such implications, by reference to Bayesian conditionalisation. However, on Okasha's own account of the basic *relation* between substantive presumptions and rules of inference, the belief that some *particular* observed correlations will continue to hold in the future *should* be equivalent to the rule 'Infer that those *particular* (hitherto) correlated properties will be correlated in the future.', since it is an empirical assumption which

> In effect ... says 'The world is such that the rule of inference is truth conducive'. (Okasha, 2001, p314).

Thus as far as Okasha's own account of this alleged relation is concerned, it would seem that such a presupposition *is* equivalent to following a rule of inference.

However, Okasha has a conceptual point to make on the matter. Here he picks up on a turn of phrase employed by Jackson, and insists that

> talk of 'using a rule on occasion' stretches the concept of a rule beyond breaking point. One might just as well say that a heavy smoker obeys the rule 'Don't smoke' simply because he takes breaks between cigarettes. (Okasha, 2001, p318)

But what Okasha fails to appreciate is that this is just a bad way of describing the use of a *particular* rule, since it is misleadingly phrased in terms that suggest occasional use of a universal rule. To illustrate the distinction, it is clear that a light smoker breaks the *universal* rule 'Don't smoke – *at all*'. But that doesn't mean he is not thereby obeying *any* smoke–restriction rules. Clearly he *is* obeying the *non–universal* (or 'particular' – relative to 'smoke') rule 'Don't smoke *heavily*'. Similarly the rule of (limited) charity 'Make donations to *some* charity appeals' is a rule that may be either followed or broken, despite the fact that it is not a universal rule – it does not say '*Whenever* there is an appeal for charity, make a donation'. That this is all that Jackson intends by his unfortunate turn of phrase in this context is sufficiently clear, not only by the absurdity of the interpretation favoured by Okasha, but also from

Jackson's own expression of the point, since he spells it out in explicitly particular terms, referring to an inference from '*certain* Fs being G to certain other Fs being G'.[62]

Furthermore, despite the potentially confusing phraseology employed by Jackson in the context of this initial statement, it is clear that the substance of his position in this article is supplied in the course of his *argument*. And in fact Jackson goes on to supply a typically insightful and invaluable defence of the common and plausible view that we *do* apply certain criteria as a matter of principle in our selection of the particular cases in which we project common properties from samples to populations, and that these standard criteria satisfactorily account for the puzzling cases of 'grue' and its like.

In any case, what Okasha needs for his argument expressed in terms of *rules* to go through is that there will be no *particular* rules that correspond to a belief that some particular regularities will continue. But in this passage Okasha simply fails to observe the distinction between the legitimate concept of using a particular rule and the absurd notion of 'using a rule on occasion'. And by conflating the two while discrediting the latter he illegitimately dismisses the former, thereby making way for the unsound inference that (since clearly we don't use the (universal) rule '[Always] Infer that properties correlated in the past will be correlated in the future') 'Jackson's 'undeniable fact' [that we all 'project common properties from samples to populations' on occasion (Jackson, 1998, p249)] lends no credence to the notion that we use rules of inference.' (p318).

Is it irrational to follow an inductive rule?

In an attempt to further support his argument that we don't follow rules in induction, Okasha goes on to argue that we certainly *shouldn't* because

> there is something inherently irrational about following an inductive rule: it deprives one of the flexibility needed for rationally adjusting one's opinion to experience. To use an inductive rule is to assume that the world is arranged in a particular way, as I have stressed. But, presumably, for any assumed arrangement of the world, it should be possible to *imagine* the world's not being like this – that is the nature of empirical assumptions. If such evidence materialized, the rational thing to do would be to give up the assumption that the world is arranged in the way in question,

[62] Clearly this is intended to make it clear that the described rule is only partially specified. The description implies that an inference is drawn under 'certain' conditions, but does not state exactly what those conditions are. The basic point of Jackson's article is to reject Goodman's view of one of the necessary conditions ('entrenchment'), and to clarify a condition that more satisfactorily accounts for our

i.e. to stop using the inductive rule. So following any particular inductive rule does seem less than fully rational. It embodies a fixed commitment to the world's being in a certain state. (Okasha, 2001, p321)

However this argument is unsound, and the problem with it stems from a misleading interpretation of Okasha's simplistic description of the relation between empirical presumptions and inductive rules. To commit oneself to an inductive rule – which will relate a variety of *possible* bodies of evidence with associated predictions – is *not* to commit oneself to an assumption about the structure of the world *that is insensitive to developments in the evidence* – which is what Okasha is arguing here. It is a basic and well–known feature of inductive rules that they induce *defeasible* predictions. Even on the most simplistic interpretation of Hume's principle of continuity, it is clear for example that the universal projection that all ravens are black, based on an initial body of evidence on which all observed ravens are black, will be rejected when subsequently a *white* raven is observed. *The rules themselves* (whatever the finer details) cater for such adjustments in the light of developments in the evidence, since the rules dictate *under what evidential conditions* certain projections will be made. One of the provisions for a universal projection that is clear enough in Hume's basic account of the principle of continuity is that there are no observed exceptions to the regularity.[63] Further and more sophisticated provisions for projection are identified by Jackson. What is clear though, on any conception of the rules of induction, is that when the evidential conditions change the predictions endorsed by the rules *can and will* change accordingly.

Okasha comments on the Lewis/Teller diachronic Dutch–book argument which has been interpreted as showing that pre–planned violations of conditionalisation are irrational (Teller 1973, 218–58). Although he ultimately declines to take a position on that issue, Okasha *mentions* in support of his argument for the irrationality of inductive rules Van Frassen's argument (1989, Ch. 5) that it follows from the Lewis/Teller theorem that inductive rules actually violate the demands of Bayesian rationality. I do not intend to discuss this issue in any detail here either, since the basic problem with Okasha's position does not depend on this claim. However it is worth noting that although it is relatively uncontroversial that overly simplistic rules of induction could result in violations of conditionalisation, or even synchronic probabilistic consistency, Van Frassen's inference would not hold for any rules of induction that were sufficiently sophisticated to *incorporate* a requirement of conditionalisation – or more basically, it will not apply to any rules of induction that *don't* violate conditionalisation. It is a

discrimination with regard to 'grue'. See Jackson's Summary section (1998, p267) for details. I have reserved my discussion of Goodman's problem and Jackson's solution for a later chapter.

basic principle of probability assignment in general that initial rough estimates (reflecting relatively raw estimates of relevant relative frequencies in our case) are 'adjusted' (c.f. for example O'Hagan, 1988) for consistency in order to ensure synchronic consistency. The (likewise practically limited)[64] requirement of conditionalisation for the epistemic frequentist then operates in much the same way as it does for the subjectivist, in that *in principle*, the frequentist will have anticipated the various possible ways that the frequency evidence will unfold, and will associate with each potential development in the evidence a corresponding conditional probability for a hypthesis. Thus the epistemic frequentist also aims to maintain diachronic consistency in the main (with the kind of admissible exceptions cited by the subjectivist). Of course he is unlikely in practice to do so consistently, as indeed is the subjectivist, but his basic principles of frequency–based probability evaluation need not offend the general principle of conditionalisation, and should in principle be expected to uphold it.[65]

Furthermore, even if we were to suppose that our principles of induction *may* at least sometimes demand such violations, it might nevertheless be the case that rationality requires that in such situations the substantive rules should override the otherwise normal policy of conditionalisation. In this regard it is notable that Dawid (1982) has advocated the aim of generally endeavouring to maintain a reasonable level of calibration over one's assignments of probability, while noting that the adjustments that will sometimes be made in an attempt to improve calibration (re–calibration') *will* invoke violations of conditionalisation.

To get back to the point of real importance here, Okasha stresses that his crucial claim is simply that

> there is no *need* for rules of inference in order to get from data to opinions not entailed by the data, whether or not it is true that there is no *room* for rules. (Okasha, 2001, p317)

provided one goes along with van Fraassen in supposing that we, at least usually, conditionalise. The trouble with this suggestion, as far as it goes, is that given *only* the rule of conditionalisation, plus a growing body of observational 'certainties', or in Okasha's terms

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[63] c.f. for example *Treatise* 1.3.6.4. for reference to the condition of the '*constant conjunction*' of the respective properties in our experience.

[64] Van Frassen or Okasha, since they both take the view that *some* violations of conditionalisation need to be allowed, on the Bayesian theory, in order for example to cater for the invention of new hypotheses that we had not previously considered.

[65] I think this is basically the same point, albeit applied specifically to epistemic frequentism, as the comment made by Howson and Urbach (1993, p103): 'if the restriction to updated rates you currently think fair is imposed … the Lewis–Teller argument is otiose.'.

[67] i.e. beliefs or expectations that supplement our observational beliefs.

'data', we don't get to *any* opinions not entailed by the data. Something more is required. Whatever we might make of Okasha's arguments for the suggestion that it isn't rules of inference that take us there, quite obviously more needs to be said about the unmentioned ingredients that, on the Bayesian theory, are supposed to provide the essential substance that fuels our inductive predictions. And we need to see how well the Bayesian theory stands up to Hume's problem in the light of these considerations. In order to approach this issue, it will be worth first clarifying the key features of Hume's argument, interpreting him charitably rather than insisting on any avoidable absurdities, and furthermore (since Okasha is now clearly talking about a probabilistic theory of induction) taking into account Hume's extended argument relating to 'the probability of chances' or 'that evidence which is still attended with uncertainty' (*Treatise* 1.3.11.).

The basic problem

To recap on the key points of Okasha's argument so far, he has noted that Hume's (initial) argument depends on the assumption that we presume that observed regularities will continue. He observes that it would be absurd to suppose that *all* observed regularities will continue, but accepts that we believe that some regularities will continue. He rephrases Hume's problem in terms of rules of induction on the basis of the supposition that a rule of induction will tell you to hold certain beliefs about the unobserved given certain observations, and so is equivalent to a substantive presupposition about what is not observed. He then aims to argue that we don't need inductive rules to make inductive predictions, and so Hume's claim that we depend on such substantive presuppositions about the unobserved in order to make our predictions about the unobserved does not hold. However, it is difficult to avoid the suspicion on reading Okasha's argument that the extended discussion of inductive *rules* ultimately serves rather to cloud than to clarify the real issue at stake here – particularly in view of Okasha's initial presentation of the key feature of Hume's problem precisely in terms of our *belief that* nature is unifiorm. A *belief that* something is the case simply is *not* a *rule* of inference – whatever other relations one might claim to hold between the two.

The real issue becomes all the more clear when we place it in the original context of Hume's expressed premise basically *our causal inferences proceed on the basis of a fumdamental presumption that (long–standing, experimentally tested, etc) exceptionelss regularities will continue*. Okasha then interpreted this in more modern terms as equivalent to a premise that we follow some related *rule of inference*. But clearly the basic issue here can be expressed quite straightforwardly in the original terms, and it is simply whether Hume was right in assuming that we proceed on the basis of substantive *presumptions* about the unobserved when we make inductive predictions. In this case the question about whether such a prior belief *can or cannot properly be construed as equivalent to a rule of inference* is, in any case, nothing more than a distraction from the fundamental issue. Hume didn't *claim* that it could, and his argument clearly does not depend on the presumption that it can.

Let us then refocus on this basic issue – i.e. whether Hume is right in supposing that we hold beliefs that amount to substantive presumptions about the unobserved when we make inductive predictions, and consider whether Bayesianism saves us from this Hume's *actual* premise, rather than Okasha' distortion of it. Moreover, since Okasha frames his objection in terms that relate to considerations of probability (in the modern sense of the term), for the sake of fairness to Hume, we should take into account the suplementary details of his argument relating to *the probability of chance* (*Treatise* 1.3.11). In this regard we may frame the key points of Hume's more general problem with reference to probabilistic *expectations* as well as outright beliefs (or those matters we regard as 'most certain').

Hume's general justificatory problem of induction is not merely to explain how *some* of our beliefs and expectations about the future might be justified – granted the assumption that *other* of our beliefs and expectations about the unobserved are justified. The problem Hume is tackling is an (almost) global one (he is allowing us to leave aside any problem concerning the justification of our observational 'certainties'). In the context of the justificatory problem of induction the justification of *any* belief or expectation about the future (or more generally the unobserved) is *part of the problem* and therefore something that is *in question*. Hume's *general* problem for induction then goes something like this:

Hume's general problem for induction

> Any predictions, beliefs, or expectations we form about the unobserved, on the basis of what we *have* observed, depend on supplementary[67] background beliefs and/or expectations *about the unobserved*. Our predictions will not be justified unless these supplementary beliefs and expectations, on which they depend, are justified. Simply to assume that any of these underlying beliefs and expectations are justified is to beg the question, since whether or not *any* of our non–observational beliefs or expectations are justified is the fundamental epistemological issue.

Okasha thinks that if in fact we proceed in our inductive inferences by conditionalisation then we are exempt from Hume's problem. But once we acknowledge that the particular aspect of Hume's problem that is relevant to probabilistic judgement is not so much his primary argument concerning causal induction, but his extended argument as applied to probability, then it is clear how his basic point, despite its formulation in terms relating to the classical theory of probability, would apply just as well to the probability judgements based on Bayesian conditionalisation. When I update my initial (unconditional) probability for an event P(A) in the light of new evidence E, in accord with conditionalisation, what I do is to set my new unconditional probability P'(A) to a value r *previously determined* by my *prior* conditional probability – relating precisely to the contingency that just such evidence turn up P(A|E) = r. Now in order to appreciate the significance of Hume's problem in this context, we do not need to question the justification for maintaining such a probabilistic principle for *updating* our prior credences in this way – just as we do not need to question the *logic* of the move from our observational evidence *plus the assumption of continuity* to the predictive conclusions of our inductive inferences in order to appreciate Hume's problem for the justification of our 'most certain' causal inferences. The problem in the latter case concerns the justification for my founding *belief* that the exceptionless law–like regularities I have observed will continue. The corresponding problem in the former case is basically the problem cited by Hume with respect to 'the probability of chance' – and is simply the problem of how we justify any such prior *credences* (*degrees* of belief or expectations) in the first place. Conditionalisation doesn't justify them – as De Finetti pointed out (1969) 'initial probabilities' are an 'essential pre–requisite' for conditionalisation.[68] In this case subjective conditionalisation provides no escape route from Hume's problem. On the contrary, it affords a prime modern example of Hume's basic contention with regard to probabilistic induction – once the proper interpretation of Hume's problem, as illustrated above, is understood

Having clarified this problem with Okasha's line of response to Hume's problem, as mentioned in my earlier discussion of the interpretation of the principle of continuity, I am nevertheless inclined to agree with Okasha that Hume was not quite right in insisting that we rely in our inductive inferences on a straightforward assumption of the (ontic) principle of continuity. As I suggested at that point, I think it is considerably more plausible to suppose that we are inclined to agree and employ an *epistemically qualified* version of the principle – basically that under the evidential circumstances described we *ought to expect* the observed regularity to continue. However it is not difficult to see that there will be a corresponding problem for any attempt to 'prove' or establish by any non–question begging argument that *this* is the case. In fact we may

[68] Of course De Finetti might well disagree with Hume that we *need* to justify our prior credences in order to justify the posterior credences that depend on them. But then an ardent subjectivist might not even feel the need to maintain the view that our posterior credences require any justification in any case, since these are in any case generally regarded on the subjectivist view merely as matters of *subjective opinion*. Hume's problems for the justification of any kind of prediction will of course only be a problem for those of us who seek justification for our beliefs and expectations – and particularly for those of our beliefs and expectations on which many others depend.

interpret Howson's recent analysis of Hume's problem (2000) which discusses the epistemological significance of Hume's problem in considerable detail, as a critique of just such an epistemic construal of Hume's problem, on which he concludes that

> *no* theory of rationality that is not entirely question–begging can tell us *what it is rational to believe* about the future, whether based on what the past has displayed or not. ... Hume was right. (Howson, 2000, p239–40, my emphasis)

CHAPTER FOUR

AMPLIATIVITY AND THE MODERN CONCEPT OF INDUCTION

Anti–deductionism[69] and ampliativity

We have seen that Hume's characterisation of inductive inference, properly understood, may be contrasted with other more modern accounts where the emphasis on observation reports as essential and characteristic premises is absent, and is replaced by an emphasis on conditions relating to the logical relationship between premises and conclusion. In these cases inductive inference is characterised explicitly, more or less simply, as arguments in which the premises provide *non–deductive* justification for believing or accepting the conclusion (or regarding it as likely). For example:

> I shall say that an inductive argument is an argument which is *not deductively valid* but one in which, it is claimed, the premises 'make it reasonable' for us to accept the conclusion. (Swinburne 1974, p2, my emphasis)

> When an argument is *not deductively valid* but nevertheless the premises provide good evidence for the conclusion, the argument is said to be inductively strong. (Skyrms 1975, p7, my emphasis)

What is notable about these definitions is that they omit any explicit reference to foundation on observational premises[70] and, in particular, that they presuppose (or stipulate) that an inductive argument *cannot*, by its nature, be deductively valid. Jonathan Lowe for example, acknowledging Kneale (1949) insists that

> 'inductive' reasoning is *non*–deductive: this much almost everyone would concede – provided it is understood that we do not have in mind here such procedures as so–called 'mathematical' induction, but rather only what is sometimes called 'ampliative' induction. The premises of an inductive argument do *not* entail its conclusion. (Lowe, 1987, p325)

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[69] I use this term 'anti–deductionism' in this context to refer to the view that no inductive arguments can be deductive, in preference to 'anti–deductivism' which more naturally suggests a view that is opposed to deductivism (which latter in its simplest and most extreme form is the view that all good arguments are deductive).

[70] The term 'evidence' as employed in Skyrms' characterisation is sometimes taken to imply that any information so designated is of an observational nature. However there is a broader sense in which it is simply taken to mean information that makes some proposition 'evident' in the sense of (justly) perceived to be true (or likely). Moreover Skyrms appears to intend something more like the latter interpretation since he immediately paraphrases his characterisation simply in terms of the *probability* of the conclusion granted the premises (Skyrms 1975, p7).

The motivation for the former, i.e. the relaxation of insistence on observational premises, is relatively straightforward. We are not always engaged in examination or hypothetical questioning of the epistemological *foundations* of our beliefs. In contrast to Hume's chosen epistemological remit, we commonly *take for granted* beliefs that, while not directly verified by observation, are nevertheless regarded as *legitimate* assumptions in the context, and proceed to make further inferences on the basis of those assumptions. Many such inferences are commonly regarded as *inductive* inferences, in a sense that we associate with the term 'ampliative', which we shall examine in the following – even if *none* of the premises would be regarded as observation reports. Thus for example Salmon identifies 'induction' with (logically correct) *ampliative* inference.[71]

> Any type of logically correct *ampliative* inference is induction ... the problem of induction is to show that some form of ampliative inference is justifiable (Salmon 1966, p20, my emphasis)

Similarly, L. J. Cohen says that 'ampliative' induction

> *amplifies our knowledge.* And it contrasts with the 'summative' induction that, rather unproblematically, establishes a generalisation on the basis of what are known to be all its instances (Cohen 1989, p1–2, my emphasis)

Rescher (1980) describes the ampliative role of induction as follows:

> Questions arise most pressingly where the information–in–hand does not suffice – when they are not answerable in terms of what has already been established. ... The definitive task of induction is to provide an ampliative methodology for acquiring information in the domain of "matters of fact and existence" extending our informational horizons ... The crucial thing about induction is its movement beyond the [information] in hand Rescher (1980, p6–7)

We may qualify this slightly in respect of the fact that the primary function of rational *arguments* would not appear to be simply a matter of belief *acquisition*, but is essentially concerned with *justification* for beliefs – whether in the context of acquiring new beliefs in the light of the respective justification for so doing, *or* clarifying or confirming that beliefs already held are indeed justified. In what follows we shall consider carefully exactly what are the

[71] although as we shall see below, we may find fault with his particular line of definition for the term 'ampliative'.

essential characteristics of the *ampliative* inferences we commonly describe as 'inductive', and whether it is correct to think that, in virtue of their ampliative nature, the corresponding arguments, i.e. the arguments that set out the purported justification for the inferences cannot be deductively valid.

Why exclude deduction?

In order to approach this issue we may first consider exactly *why* Skyrms, along with Swinburne, (and of course many others) should have wished to insist that inductive arguments *cannot* be deductively valid. We have already examined in detail one prime contender for an, at least partial, explanation of this – which is the common thought that inductive inference and argument, at least as classically characterised, *according to Hume*, cannot be deductively valid – and we have established that to think so on that basis would be a mistake.

Of course even if that *were* Hume's claim, in the context of his epistemological concern with the justification for any of our beliefs about the unobserved (and even if on that interpretation he would have been right in that context) it would not necessarily follow that the same would hold on a *broader* concept of induction where that particular epistemological constraint is relaxed. But if I am right and that was *not* Hume's claim – if, as I have argued, the logical form of inductive inference as Hume understood it was in fact deductively valid – then it would seem that any broader conception of induction should at least be *compatible* with this supposedly paradigm case of Humean induction, and therefore compatible with deductive validity. In view of these considerations then it might seem peculiar then that neither Swinburne nor Skyrms appeared to feel any compulsion to provide *any* justification whatsoever for their stipulations in this matter. It is as if they felt that the underlying reasons for the deductive exclusion clause are somehow *obvious*. Let us then consider whether there might be any other explanations for this supposedly uncontentious claim regarding the logical structure of ampliative or inductive inference.

Mill's extension of Hume's problem to syllogistic deduction

On our understanding of Hume's problem, it would seem that one philosopher who understood Hume's complaint quite well was Mill. Mill's complaint[72] about the epistemological usefulness of syllogistic logic (or lack of it) may be regarded as, in effect, an extension of Hume's complaint about induction, since the paradigm case Mill takes to illustrate his point – an instance of the first figure mood 'Darii', the traditional precursor of universal elimination – mirrors the problem that Hume raised against induction, on our understanding of it.

> It must be granted that in every syllogism, considered as an argument to prove the conclusion, there is a *petitio princippi.* When we say,
>
> All men are mortal,
> Socrates is a man,
> therefore
> Socrates is mortal
>
> it is unanswerably urged by the adversaries of the syllogistic theory, that the proposition, Socrates is mortal, is presupposed in the more general assumption, All men are mortal: that we cannot be assured of the mortality of all men, unless we are already certain of the mortality of every individual man: that it be still doubtful whether Socrates, or any other individual we choose to name, be mortal or not, the same degree of uncertainty must hang over the assertion, All men are mortal (Mill 1843, p20)

This is essentially the same problem that Hume identified with respect to inductive inference, since Hume believed that we make just such a general *assumption* about *all* cases, despite limited evidence of verified cases, and in particular including the unverified case of the *hypothesis*, when we make inductive inferences. Mill however wants to apply the same epistemological point to syllogisms in general, since when an argument is deductively valid, we cannot – or at least in principle *should* not – be certain about the conjunction of the premises *unless* we are already certain about the conclusion.

But of course this does not mean that deductive arguments are useless, since it is nevertheless possible to have *sufficient evidence* to support a conclusion (and the conjunction of a set of premises that imply it) while still being *in doubt* about the conclusion. And this will be so when we fail to notice or realise that the evidence satisfactorily supports the conclusion. As Augustus DeMorgan pointed out in response to Mill,

> the presence of the premises in the mind is not necessarily the presence of the conclusion (DeMorgan, 1847, p254)

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[72] It is clear here that Mill does not suggest the complaint originates with himself, but attributes it to 'the adversaries of syllogistic induction'. Walton (1991, p17–19) finds a lucid account of the same point presented by Sextus Empiricus (OP II, 195–7), in the second or third century.

In other words we do not always live up to the logical ideal of automatically believing all the logical consequences of what we believe jointly[73] to be true. So deductive arguments can *enlighten* us about the conclusion of a deductive argument even when we already (justly) believe all the premises to be true.[74] Of course this point is particularly relevant to the case of more complex deductive arguments, when the implications of the premises may need some clarification. But even then the larger argument may be composed of smaller steps of inference for which each individual implication is relatively obvious, hence the seemingly trivial simple steps of deduction can play a significant role. This point in itself seems to be sufficient to establish that an argument may be 'ampliative' in the important *epistemic* sense of expanding our *knowledge* or *justified belief* – associated with the primary function of argument – while nevertheless being deductively valid. Nevertheless, at least in the standard sort of cases we are considering here, ampliative deductive inferences such as this will not be inferences that have any particularly distinctive characteristics in virtue of which we might want to call them 'inductive'. The cases we are considering here are just those cases of deductive arguments that happen to be opportunistically pertinent, presented in order to mend contingent failings in the audience's deductive analysis of the evidence or assumptions.

In view of this, the fundamental motivation underlying the deduction exclusion clause would appear to be nothing more contentious than the simple point that we need to employ principles of inference other than the relatively well understood principles of deduction *only* in cases where the proposition at issue *cannot* be deductively inferred (or disproved) from what is already known or accepted, and it is precisely in an attempt to understand the principles involved in *those* problematic cases that we need to develop an *alternative* kind of logic to deduction. On the surface then the underlying motivation for the deductive exclusion clause *does* appear to be quite unobjectionable and obvious. However, as we shall see, this line of thinking, although *based* on an uncontentious point, is not in fact valid, and results in a summary dismissal of important possibilities beyond the straightforward cases intended.

For the moment, in order to clarify the point that there *is* an error in this line of thinking, we may note simply that from the unobjectionable point that (a) we need to employ principles of

[73] I use the term 'jointly' here in the sense that I jointly believe a set of propositions S iff *I believe that all* the propositions in S are true – as distinct from the claim that *I believe that* member S_1 is true *and I believe that* S_2 is true, … and so on. I take Kyburg's Lottery Paradox to illustrate that the former is required for the epistemic adequacy of a deductively valid argument, while the latter is insufficient.

[74] There are of course further epistemological benefits of the use of deductive arguments. For one thing the premises themselves can provide new information to the audience when the arguer is taken on trust. And as Jackson has argued (1987, 104–7) presenting an argument to a conclusion in such circumstances may be of more use to the audience than simply asserting the conclusion, since for one thing the audience will typically learn more (relevant) information that way, and learn more about the nature of the evidential support for the conclusion.

inference other than the principles of deduction *only* in cases where the proposition in question cannot be deductively inferred from what is initially accepted, it does *not* follow that (b) we *do* need to employ principles of inference other than the principles of deduction in cases where the proposition in question cannot be deductively inferred from what is initially accepted.[75] The significance of this erroneous line of thinking in this context will become clearer in what follows.

Logical ampliativity

How then are to understand the *appropriate* qualification that may be applied to the basic epistemic account of ampliativity, as exemplified in Cohen's account above, in order to account for such exceptions in the case of our simply having overlooked the fact that the hypothesis is a straightforward deductive implication of our prior beliefs? The right way to approach this issue is to consider first how we may properly characterise our concept of inductive inference, before we address the issue of the logical structure of arguments that are designed to explicate the justification for our inductive inferences. In order to approach this issue we may consider the case of Hume's concept of inductive inference for the sake of comparison. In the case of *Hume's* concept of 'induction', the respective inferences may be regarded as '*observationally* ampliative' in that the hypothesis in this case is not a deductive implication of our *observational* assumptions. As we have seen, that does *not* imply that the hypothesis is not a deductive implication of the full set of assumptions, since as Hume emphasised repeatedly, there is a 'founding presumption' that is common to all our inductive inferences but which is *not* observationally verified.[76] The more modern conception of induction, which has typically but mistakenly been seen as a 'broader' concept that *encompasses* Hume's as a special case, may more appropriately be understood as a *distinct* concept within a broader *family* of concepts of induction (including the Humean concept of 'induction' as observationally ampliative inference).

On the modern concept of inductive inference, which we might call 'logical' induction, there has been wide–ranging doubt that deduction is even a candidate logic for proper representation of the inference, and it is certainly the case that the concept with which we are dealing in this case is not intended to admit such *straightforward* deductive inferences as exemplified on Hume's account of induction – even though, as we have noted, such inferences may in some contexts be *epistemically* 'ampliative' in the sense as described (above) by Cohen. More specifically we may admit that it is not intended to admit what we might call 'ontic' deductive

[75] The logical point here is simply the platitude that 'P *only* if Q' does not imply 'If Q then P'.

inferences, where the premises and conclusion consist of unqualified factual (or more precisely non–epistemic) statements. We may also note that this is in any case how arguments by tradition are *commonly* construed, since epistemic qualifiers and connectives in a natural expression of an argument are typically[77] interpreted, in the process of construal, as merely indicative of which of the more basic 'ontic' or 'object' propositions that they qualify or connect are to be counted as 'premises' or 'conclusion', or in some cases – for instance when the connective is relatively weak (e.g. 'This inclines me to suspect that …') – as an indication of whether the argument is to be construed as deductive or inductive.

However, if we observe the distinction between the respective concepts of the 'hypothesis' (i.e. the proposition toward which some epistemic attitude is supposed to be justified in the inference) and the 'conclusion' of an argument that is presented in supposed clarification of the supposed justification (i.e. the proposition that occupies the respective place as indicated by the main inferential connective, or 'illative particle',[78] in the argument) – and if we likewise distinguish between the stated premises of a presented argument, and the pertinent *assumptions* of the arguer – i.e. the ontic propositions accepted by the arguer, and appealed to by the arguer in their attempt to clarify the justoification for the hypothesis – then we may more appropriately characterise what would commonly be taken to be an 'inductive' inference in the modern sense (which we might call 'logical' induction) as an inference in which the *hypothesis* is not (and is not purported by the arguer to be) a deductive implication of the *assumptions* on which the perceived justification for the inference is based. For inferences *other than* logical inductive inferences in this sense, there need not in general be any particular problem about how we ought to construe the *logic* of the respective inferences. Typically such inferences may be straightforwardly construed as deductive inferences where the conclusion is the hypothesis and the premises are just the justificatory assumptions. My central aim in this thesis is to illustrate how we ought to construe those inferences where the question of the appropriate logic is generally regarded as more problematic, where there is *no* such deductive connection *between the assumptions and the hypothesis* – namely cases of what I have called *logical* induction – where the inference is, in *this* respect *logically* ampliative.

[76] even though, in Hume's view, it is experience that, by force of habit, 'induces' us to believe it.
[77] Typically but by no means always. In epistemic logic and epistemology, in particular, epistemic qualifiers and such–like are often essential to the topic of argument, and are not in such cases 'construed away'.
[78] e.g. 'Therefore', c.f. Lambert and Ulrich (1980, p49).
[80] I paraphrase somewhat liberally here.

Swinburne's account of induction applies directly to arguments (as distinct from psychological inferences) and stipulates that only arguments that are not deductive in form[80] can be 'inductive' arguments in his sense of the term. For the sake of a suitable terminological distinction while acknowledging the popular tradition of using the word 'inductive' in such application, we may call such arguments, i.e. arguments that (are supposed to) offer some support for a hypothesis, but which are *not deductive* in form, '*formally* inductive'. However, in what follows I shall argue that it would be a mistake to suppose that arguments that are designed to explicate the justification for *logically* ampliative inferences, as defined above, should properly be construed in the form of *formally* inductive (i.e. non–deductive) arguments. It can *seem* to be a platitude that in fact they should be so construed, because it is not uncommon to use the terms 'conclusion' and 'premises' relatively loosely, presumably more or less as synonyms for 'hypothesis' and 'assumptions' in the context of describing a psychological *inference*. For example Salmon uses the former terms, despite their primary association in the context of logic with formal analysis of *arguments*, to define an ampliative inference, thus:

> an ampliative *inference* ... has a *conclusion* with content not present either explicitly or implicitly in the *premises*. (Salmon 1966, p8, my emphases)

And if we adopt that way of talking about the respective features of a psychological inference (i.e. calling the hypothesis the 'conclusion' and the (ontic) evidential propositions the 'premises') then it might seem to go without saying that in an appropriate propositional explication of the justification for the inference, in the form of an argument, the conclusion of the respective argument should properly be construed as the 'conclusion' of the inference, and the premises of the argument should be the 'premises' of the inference. However if we bear in mind that epistemic qualifiers (and indeed connectives) in the natural expression of an argument (or implicit in the supposed justification of the psychological inference) will in some cases play an essential role in the justification for the respective inference, and will in such cases need to be properly represented in an appropriate construal of the argument, then it would be a mistake in such cases to stick to such a simplistic policy of construal.

In what follows I shall argue that such epistemic qualifications and connectives do indeed play an essential role in the justification of logically inductive inferences, and that such arguments generally ought to be construed in epistemic form. Furthermore it will become apparent that when the supposed justification for a logically ampliative inference is thus fully and properly explicated, the resulting construal will be an argument which is deductively valid in form. The view that this is the right approach to the construal of logically ampliative arguments may be termed *epistemic deductivism*. If this view of induction is right, then there is no place in the logic of induction for the *formally* inductive (i.e. non–deductive) arguments defined by Swinburne.[81] As we shall see, logically inductive arguments, properly construed, do in fact have a quite simple deductive structure that properly reflects, as it should, whatever epistemic connection is purported by the arguer to hold between the evidential data and the hypothesis in question.

Summary and conclusion to Part One

As we have noted, it is uncontroversial that deductive inferences and their associated arguments *can* extend our knowledge, or justified beliefs, by helping us to acknowledge initially unseen logical implications of our initial beliefs, and in this respect might be termed '*epistemically* ampliative' even though their conclusions are logically *entailed*, albeit covertly, by the premises, or in other words logically 'implicit' in the premises. But in contrast to such unexceptional deductive inferences, we might say that inductive inferences are intended to be '*logically* ampliative', in that the particular type of extensions to our beliefs that we seek to justify (or justifications for pre–existing beliefs) in the case of induction are extensions (or justifications) where the belief that is intended to be added (or justified) – i.e. the 'hypothesis' or 'target proposition' – is *not* deducible from our initial beliefs (or at least in the latter case not from our *other* initial beliefs, besides the hypothesis). Of course, when the proposition of epistemic concern, *can* be deduced from a given set of categorical (unqualified) assumptions, we do not have a problem with regard to a suitable logical form for representation of the argument. A simple (non–epistemic) deductive argument will do the job, and the basic principles of deduction are relatively well understood. On a superficial comparison of such logically unproblematic cases with the more problematic case of logically ampliative inference, it can *appear* that *logically* inductive arguments must employ logical principles other than those of deduction. However we have seen that this is a mistake – albeit an easy and common mistake to make – to the extent that just such a misconception of induction has practically became textbook tradition, exemplified for instance in Salmon (1966), Swinburne (1974), Skyrms (1975), and more recently in Bird (1998) and Howson (2000).

On our earlier analyses of Hume, it would seem that in Hume's view, the kind of inductive inference with which he was concerned – what we might call '*observationally* ampliative' inductive inference – is not of the logically ampliative kind: Hume's notion of induction requires only that the hypothesis is not deducible from our *observational* assumptions. But

[81] Except perhaps if interpreted as truncated expressions of some standard form of epistemic deductive argument, e.g. where it is supposed that the evidential propositions are 'certain', that there is no further

Hume insists that we employ an additional and indispensable presumption when we make such inferences, and moreover on his apparently ontic account of the additional general assumption that is made, it would seem that the hypothesis is then deducible from the full set of assumptions. From close examination of the immediate context of Hume's problem, we have eliminated the theory recently advanced by Owen that Hume's is not a justificatory problem at all, but merely an explanatory problem, and we have clarified the fact that Hume's problem for causal inference is more specifically the problem that the 'principle' of continuity, that he claims to be an fundamental and indispensable assumption of our inductive inferences, cannot be *proven* or 'justified' – in the sense that no argument, that does not rely on the general principle, or some particular instance of it, could possibly be provided that would satisfactorily establish that the same proposition is true.

We have also examined Hume's associated problem for the justification of probability judgements or partial expectations, and I have argued that Hume's problem cannot be so easily overcome by consideration of modern (Bayesian) theory of probability as Okasha (who at least appears to acknowledge the proper interpretation of Hume's problem for causal inference) has recently argued. However we may concur to some extent with Okasha in that Hume's (ontic) representation of the general presumption that he claims we employ when we make (causal) inductive inferences does not seem to be entirely plausible. I have suggested that this might be better represented as an epistemic assumption to the effect that in evidential circumstances of the relevant kind *we ought to believe* that the respective regularity will continue. If we modify Hume's account of inductive inference in this regard, we will then an account of observation–based induction that yields a *logically* ampliative inference, in the sense described above, since the *hypothesis* – that the regularity will continue – will not be a deductive implication of the assumptions. Such a move however will not save us from Hume's problem for the justification of the non–observational assumptions that we make in our inductive inference, since his problem is equally well expressed in relation to the question of justification for such an *epistemic* assumption:

> even after experience has inform'd us of their *constant conjunction*, 'tis impossible for us to satisfy ourselves by our reason, <u>why we shou'd extend that experience</u> beyond those particular instances, which have fallen under our observation (*Treatise*, 1.3.6.11, my underline)

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relevant information, and the implicit claim is that since all that is the case then the hypothesis ought to be 'accepted', in some standard sense of the term.

in other words, we cannot justify the supposition that, when we know that every F that has been observed has been followed by a G, *we should expect* that the next F that is observed will also be G.

Some of the comments Hume makes on the implications of his problem of unprovability seem to indicate that Hume thinks that if we cannot comprehend any satisfactory rational explanation as to *why* we ought to believe P, then that should at least shake our confidence that indeed we *ought* to believe P.[83] We may regard this as related to a rule of thumb employed in everyday reasoning, that we generally suppose to constrain the limits of rational credence, dividing reasonable belief from credulity – that we should believe only what we are able to provide satisfactory reasons for believing. But, even if Hume's unprovability argument is correct – taken in conjunction with his additional arguments in the *Enquiry*, simply that that we *don't* comprehend any rational justification for our basic non–observational assumptions – this line of argument *could* be regarded as a reduction ad absurdum of the view that such a practical rule of thumb (for application to the kind of hypothesis typically under consideration in the context of everyday reasoning) should be understood to apply just as well to the very basic beliefs that may be called into question in the context of philosophical epistemology. In other words, the mere fact that I cannot *establish by rational argument* that P – or *that I ought to believe that P* – does not *in general* mean that it is *not the case* that I ought to believe that P.[84] In particular this does not hold with regard to the basic and common beliefs which, along with our observational beliefs, make up the fundamental assumptions on the basis of which we make the inferences and predictions we do in everyday reasoning. The issues which are generally at stake in *practical* applications of induction, for example in the context of weather forecasting or weighing of evidence in the process of law, do not preclude the employment of various assumptions representing prior knowledge of relevant causal principles which are not under examination. Such assumptions will entail more than the strict implications of any actual *observational data*

[83] For example: 'The *intense* view of these manifold contradictions and imperfections in human reason has so wrought upon me, and heated my brain, that I am ready to reject all belief and reasoning, and can look upon no opinion even as more probable or likely than another.' (*Treatise*, 1.4.7.8). and 'let men be once fully convinced of these … principles, and this will throw them so loose from all common systems, that they will make no difficulty of receiving any, which may appear the most extraordinary.' (*Treatise*, 1.3.12.20)

[84] Although, as I have argued, Musgrave was mistaken in his account of Hume's argument for unprovability, he does (in my view rightly) observe that Hume seems to think that more profound problems arises as a consequence of this basic problem. Hume seems to think that 'a belief is only reasonable if we can prove or justify or give a reason for it.' (Musgrave, 1993, p152).

that might be employed in the inference. And *some* such non–observational assumptions, or at least underlying presumptions, will be pragmatically necessary in most or all practical applications of induction.

No doubt, if Hume was wrong and it were possible, a good understanding of some justification that would help to identify, and rationally to account for, those non–observational *basic* beliefs that we ought to hold, would be most helpful in illuminating the further questions of justification which are our practical concern granted those beliefs. But whether or not Hume was right, what we need as a practical priority is an understanding of how in general to ensure, and to demonstrate, that the ampliative *extensions* we make to whatever initial beliefs we *presume* to be just, are justified *granted* the justness of those initial beliefs. In *Gambling with Truth* Levi (1967, p3–6) comments on 'a rather delicate division of labor' between philosophers and scientists, suggesting that although in science the issue of justification only arises 'in the context of specific enquiries', philosophers tend to be more concerned with the '"global" justification of the totality of beliefs held at a given time'. However he goes on to point out that scientists require appropriate *criteria* for the determination of their "local" justifications, and that it is the proper task of the philosopher of science to establish appropriate criteria for this purpose. In a similar vein, Arthur Burks (1980) has argued, following Pierce, that learning and induction involves a hierarchy of interrelated systems, in which case the 'locality' of an issue or justification may be a matter of degree, depending on the depth of the justification sought within the hierarchy. My primary concern in this analysis is with justifications relating to questions that we would ordinarily take to be straightforward matters of fact under conditions of uncertainty – as is generally the situation in the context of relatively high order or 'local' enquiries such as judgement of fact in criminal trials – rather than, for example, in the epistemological context of scientific theory choice.

Of course there is no sharp dividing line here; some beliefs are more common and more basic than others. But it should nevertheless be clear enough to most of us that we should not allow Hume's problem to significantly shake our confidence about the common predictive inferences that govern our everyday interaction with the world. Of course that does not solve the problem of the inductive inferences and assumptions that we disagree on. Moreover, as we have seen, there is an outstanding question to be raised about the precise content of the presumptions we employ when we make predictions. We have noted that the ontic representation of the principle of continuity that appears to be implicit in Hume's account in view of its overt ontic form does not seem to be quite right. I have suggested an epistemic modification would appear to remove the most obvious problems with it. But even if we are relatively untroubled by Hume's problem of unprovability, Goodman's more modern 'new riddle' of induction can appear to threaten the viability of even the most superficially innocuous accounts of the basic principle of induction. I shall turn to such more troublesome issues in the final part of the thesis.

But for the moment I shall focus in Part Two on the central issue of the logical structure of (logically ampliative) inductive argument. Here I shall examine in more detail the justificatory function and structure of argument, and illustrate how logically inductive arguments, properly construed are in fact deductively valid in form. We shall also see how this analysis seems to clarify the applicability of Hume's problem in the general case of logically ampliative induction. This will also help us to settle the question of the appropriate conditions for premise–adequacy.

PART TWO

THE DEMONSTRATIVE FUNCTION OF ARGUMENT, AND THE CASE FOR EPISTEMIC DEDUCTIVISM

CHAPTER FIVE
THE DEMONSTRATIVE FUNCTION OF ARGUMENT

The natural concept of argument

In order to approach the issue of the basic conditions for the cogency or goodness of an argument, we need to first to make some preliminary points on the nature and function of arguments. In this chapter I shall discuss a natural concept of argument, the basic or primary function of which is the propositional *clarification* (or, in the natural sense, 'demonstration') of justification for (typically) belief or a degree of belief that a hypothesis is true on the basis of some assumptions. My concern in this thesis falls within the subject of informal logic, or what might be called the philosophy of argument, where the term 'argument' is interpreted in a natural rather than a formal sense. As Terence Parsons notes in the leader to his essay 'What is an argument?' the frequent comment

> "I see what your premises are," says the philosopher, "and I see your conclusion. But I just don't see how you get there. I don't see *the argument*." (Parsons 1996, p164)

Parsons suggests that such comments indicate the fact that 'there is a notion of argument in philosophy which does not consist just of premises and conclusion; it has additional structure' and that this to be distinguished from the technical notion commonly employed in logic texts, where the term 'argument' refers more minimally just to 'an ordered pair consisting of the premises and the conclusion' – or more formally (c.f. Sinnott–Armstrong 1999) – consisting of a set of propositions and a proposition. I shall have more to say about the general structure of natural arguments in Chapter six. But for the moment we may note that Parson's elaboration on the more natural concept (which he calls the 'philosophical' concept) indicates certain features the significance of which I shall consider in more detail. Parson's suggests that

> The philosopher's argument is something ... more akin to the logician's notion of *derivation*: a series of statements with intermediate steps providing the transition from premises to conclusion. (Parsons, 1996, p164)

Of course it is not always the case that there are *intermediate steps* in an argument. In relatively simple cases the conclusion may be immediately drawn following a statement of the premise or premises.[85] But Parson's characterisation here (regardless of the legitimacy of the analogy in

[85] And in any case, it is generally acknowledged that intermediate steps of inference within a larger argument may themselves be regarded as small sub–arguments.

the pertinent respects with the logician's notion of derivation) reveals certain other points of significance. Notably natural arguments consist in a series of *statements*, as distinct from abstract entities such as propositions, and some of these statements in some sense 'provide' – or we might alternatively say 'explain' or 'clarify' – the 'transition' from premises to conclusion – the latter presumably referring to the inference, or the justification for it. I shall discuss the significance of these points in more detail in what follows.

The broader context of our concern in these matters is with those particular speech and thought acts that people perform when they are discussing and thinking about the justification for their beliefs and judgements, which we naturally refer to respectively as 'arguments' and 'inferences'. But there are a number of reasons why we should want to focus on the more tangible case of the *speech* acts that we refer to as 'arguments': One reason for this is ease of identification. The underlying assumptions of an *inference* in the psychological sense, whereby an inference is a process of thought, may be difficult to pin down. For example it might be clear that a certain proposition P I believe is part of the support for the conclusion of an inference I make, but it might nevertheless be unclear whether other propositions that perhaps form part of my justification for believing P are also to be regarded as premises of the inference. In the case of an *expressed* argument this problem need not arise, since of course so long as an argument is clearly expressed it should at least be clear what the premises are. Thus, on my intended sense of the term, an *actual* argument is (basically) a series of assertions of suitably related statements (along with any of their intended and relevant conversational implications) asserted with the intent of making it clear to an audience, that some particular proposition, the hypothesis, ought to, or may reasonably, be believed, or regarded as likely, to be true. (This basic account will receive some qualification in what follows, although I take it that this initial account is nevertheless fairly representative of the paradigm case of argument in the natural sense of the term.)

I do not of course deny that there are different conceptions of argument, or that a different concept will be more appropriate in the theoretical context of *formal* logic. It is simply that our particular concern here is with the more natural concept under which an argument is a series of assertions, made in a context with the purpose of clarifying justification for regarding a hypothesis with some epistemic attitude. But how does this concept relate to the theoretical or hypothetical arguments that no–one necessarily *asserts* which are commonly the subject matter of examples in the study of even informal logic? A *hypothetical* or *possible* argument is a *possible* (as distinct from actual) assertion of such a sequence of propositions with such intent. Such hypothetical cases provide a convenient resource for the theoretical logician who neither wishes nor needs to concern himself with the question of whether the 'examples' he discusses have been, or ever will be, actually *asserted* as an argument, as distinct from being merely *mentioned* or described and analysed as they often are in logical texts. However, since the distinction is irrelevant to much of our concerns about general principles of argument appraisal, I shall, where unproblematic, follow the standard practice[86] of employing the unqualified term 'argument' to refer to anything that is either an actual assertion of such a sequence of propositions, or a possible assertion of such.

Clarity – a primary condition of cogency

Generosity requires that when interpreting an argument we take into account any pertinent points that are somehow clearly implicit or clearly taken to go without saying. But generosity only requires us to go so far in taking responsibility for identification of what is intended by the arguer to be understood by an argument he or she presents to us. And the bounds of our responsibility in this respect are defined basically by what *is* clear from what is said in the context of presentation. If it is not clear from what is said in the context what is intended to be understood by an argument, then the argument as it stands will *fail* to set out clearly any purported justification for a certain epistemic attitude toward the hypothesis, and will thereby fail the first test of cogency. On this approach then the proper interpretation or construal of an argument may be taken to include any (clear) conversational implications of what is expressed[87] that are pertinent to the supposed justification, as well as (or in some cases possibly instead of)[88] the proper logical implications of what is actually said. Thus on this approach an argument will include *at least* what is logically or conversationally *implied* in what the arguer actually says, or otherwise *clearly and unequivocally* intended given what the arguer actually says, in the context within which it is uttered. In this case we might be inclined to say that an argument consists of a *core* of actual statements, plus certain supplementary propositions, that are one way or another implicit in what is actually said in the context. For the sake of simplicity though we may

[86] If the reader does not agree that this is a fair representation of standard practice, they may ignore the word 'standard' here, since that implication is peripheral to my concern.

[87] For example, if one of my expressed premises is 'Mega–route X is complete much more often than The Shroud', that does not *logically* imply but nevertheless usually *conversationally* implies that *I believe* Mega–route X is complete much more often than The Shroud. If I make that assertion without believing it there is something amiss or at least unusual about the context of the assertion, for example it might be the case that it is not intended as a genuine piece of information at all, but I am acting out a fictional episode of communication in the context of a play intended for your entertainment.

[88] I have in mind here cases where, in accord with perceived convention, a conclusion might take the form of a categorical statement, when in fact the argument is actually understood merely to justify a relatively small degree of belief or plausibility for the supposition that the stated proposition might be true. Skyrms (1975) for example seems to employ the policy of presenting an argument as a list of generally categorical premises followed by a categorical conclusion, presumably even in the case where the 'inductive strength' of the argument is very low, and *understood to be so* by the presenter, who might in fact be intending merely to put the case that the hypothesis is not entirely implausible. In such a case the

nevertheless regard the intended argument as, in principle, a series of *statements*, such as *would* be expressed by the arguer were he or she to fully explicate both what is explicit *and* what is implicit in the initial overt presentation of the argument. Of course, in practice, the best way to establish exactly what *is* intended by any initial presentation of an argument is precisely to elicit from the arguer an *explicit* account of exactly what is intended. The proper construal of an argument then may in principle be identified with the content of what is expressed in such a fully explicated version of it.

In any case an argument will not be a good argument if it is *not clear* what the arguer *intends* to be included in or understood by it. The onus after all is on the arguer to *clarify* whatever justification his argument is designed to clarify. So most of the rest of what we need to say about argument cogency may be related to arguments where it *is* clear what the arguer intends to be included in the argument. In practice we do not need to be overly dogmatic in this regard about a need for *absolute* clarity in the *overt* representation of an argument as it stands on initial presentation, since it will often be reasonably clear that there are certain implicit suppositions, and of course when certain pertinent points aren't particularly clear from the initial presentation of an argument, we will generally be able to *ask* the arguer for the respective clarifications.

Live argument v textual analysis, and the question of missing premises

In this regard there is a significant difference between the case of an old argument presented by a thinker now dead, and arguments presented by the living. The only chance I will get to make clear the arguments I want to propound is while I am alive, and if a first stab I make at presenting an argument is not satisfactorily clear to you, then you have the opportunity to enlighten me about what aspect of it is unclear to you. Perhaps you may be unsure whether I am taking for granted a certain presumption, for example that the evidence I cite is all the relevant evidence I have, or whether one of the conditionals I include among my premises is supposed to be a claim of logical implication. An important factor in the pragmatics of argument is that in such cases you are free to ask me to clarify the respective points of uncertainty. As far as everyday argument is concerned where we are generally engaged in interactive exchanges on an issue, this point is of considerable importance in for example settling the concerns often raised about the identifiably of 'missing premises', or concerning what kind or degree of support the premises are supposed to provide for the conclusion. Govier for example raises a number of points on this issue of missing premises – much of which may

proper logical implication of the stated conclusion h is that h is true, while the arguer would not in fact intend any such assertion in presenting his argument.

be quite relevant when we are dealing with an issue of *textual* analysis of an argument of a *dead* philosopher, which leaves room for a question to be raised, and concludes that

> All of these considerations suggest that the problem of missing premises is much more complicated than it might seem at first. … Confident statements to the effect that such and such argument clearly has some statement as a missing premise are inappropriate. Whether a statement is a missing premise in some argument depends on our theory of argument, our purpose in analyzing the argument, and much else. (Govier 1987, 102–3)

However, my concern is with *live* argument and the interactive processes of reasoning in which it arises. And in this scenario all you need to do to *find out* whether a statement is a missing premise in argument – *if* that is not entirely clear from the context – is to *ask* the arguer. Of course in some cases the response might be *uncertainty* on the part of the arguer, possibly leading to a retraction of the original argument, and possibly followed by a modified version.[89] But in such cases, at least we have established that the original argument was *not* satisfactorily clear, and may thereby conclude the appraisal with result that that initial version of it was an unsatisfactory argument. Those who want to engage in *textual analysis* of old arguments will naturally have much greater problems to face, as indicated by Govier. But as far as the interpretation and appraisal of *live* argument is concerned, within the everyday process of interactive reasoning, our situation is greatly simplified. And it is *this* kind of argument analysis which is my concern. This is not a thesis about textual analysis.

Aspects of epistemic justification, and the variety of epistemic aims of argument

We noted earlier that in introducing the concept of inductive inference, as they understand it, both Swinburne and Skyrms presented an initial account in terms of an epistemic relation – 'providing good evidence for' and 'making it reasonable to accept'. But what is also worth noting is that in each case this is immediately qualified in terms of *probability*. Skyrms suggests that

[89] I am not suggesting here that it will always be a straightforward matter to ask the arguer, particularly when the argument is encountered in print, or when the audience is large and only a few will have the opportunity to respond. However, with the benefit of electronic communication, it is generally not particularly difficult to put an interpretive query to an arguer. But the point I am making is also one of principle. The primary issues I am concerned with are how to make arguments (particularly inductive arguments) clear, and what are the criteria for appraisal of clear arguments – not how to go about interpreting an unclear argument when, for whatever reason, you are unable to communicate with the arguer in order to do so.

> An argument is *inductively strong* if and only if it is *improbable* that its conclusion is false while its premises are true, and it is not deductively valid (Skyrms 1975, p7)

Similarly, although he favours a more complex elaboration (also in terms of probability), Swinburne, in 1974, acknowledges as one possible interpretation of 'inductive inference':

> that an inductive inference is one for which, it is claimed, the premises make the conclusion probable (Swinburne 1974, p5)[91]

and more recently has offered a basic definition of 'inductive probability' which appears to be equivalent to the relation that is supposed to hold between premises and conclusion in those earlier accounts of 'inductive inference' or 'inductively strong argument':[92]

> Inductive probability is a measure of the extent to which one proposition *r* makes another one *q* likely to be true. (Swinburne 2001, p62, my emphasis)

Since in each case the probabilistic account is intended as an elaboration on an initial epistemic account of inductive inference, it would seem that a corresponding *epistemic* interpretation of the concept of probability would be appropriate in this context. Of course some theorists will prefer to present arguments in terms of physical rather than epistemic 'probabilities', such as long run relative frequencies, or 'propensities'. But even in such cases where an argument is presented in which it is supposed that the premises establish some physical probability on (or as) a hypothesis – we will still need to understand what *epistemic attitude* towards the hypotheses is, or is supposed to be, established by the argument.

Thus, while the epistemic aim in general of an inductive inference will be to establish the appropriateness of *some* kind of epistemic attitude towards the hypothesis (granted the pertinent assumptions), different *types* of epistemic attitude may be appropriate (and aimed for) in different cases. An inductive argument may for example be designed to establish the appropriateness of a categorical belief that the hypothesis is true, a pragmatic 'acceptance' of

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[91] Susan Haack offers a similar probabilistic account of the inductive inferability relation in 'Philosophy of Logics': 'An argument is *inductively strong* if the truth of its premises makes the truth of its conclusion probable.' (Haack, 1978, p247)

[92] Swinburne makes it explicit that 'r and q may be complicated conjunctions or disjunctions of other propositions' and thus this will mirror Haack's, and Swinburne's earlier acknowledged alternative, account of inductive strength in the case where r is the premise or conjunction of the premises of an inductive argument and q is the conclusion.

the hypothesis within a particular context or for a particular purpose, or merely some (quantified or qualified) degree of belief that the hypothesis is true. Further complications may also arise with regard to arguments that might arise in the context of scientific or philosophical justification. For example a constructive empiricist may be less concerned with the truth of a hypothesis than with the reasons for believing it to be empirically adequate (within the context of an associated theory). Even a realist who places particular emphasis on the revisionary nature of scientific progress, or on the unspecified provisos that will in practice apply to the theoretical generalisations that are commonly employed in scientific judgements, may *believe* an expressed general hypothesis to be strictly *false*, but nevertheless wish to argue that it has a satisfactory degree of 'verisimilitude' or *closeness*–to–truth for practical purposes. Similarly a Popperian anti–inductivist might wish to argue that a general hypothesis has a high degree of *corroboration*, even though he denies that that provides any justification for *believing* the hypothesis – but of course in each case the respective argument, if it is to be successful, will nevertheless need to set out the purported justification for the claim that the theorist *does* wish to make, whether it be that the hypothesis is well corroborated, empirically adequate, or in some sense 'close to the truth'.

Furthermore, it is not only the details of the supposedly justified epistemic status of the *hypothesis* that is relevant to the cogency of an argument, but also the presumed epistemic status of the information cited in the premises. Even if an argument is clearly deductively valid, it will not be a good or *cogent*[93] argument if no party to the argument has any reason to regard the *evidence statements* as true, to some degree likely, well corroborated, or close to the truth, or, in general, as having whatever epistemic status (theoretical or otherwise) they are supposed to be accorded in the context of the supposed justification.

Problems with Govier's account of the possibilities

With these considerations in mind we may note a number of problems with Govier's anaysis of the various views on argument cogency. On the basis of a literature survey, Govier (1992, p393) presents six different views as to the conditions for the two aspects of argument cogency (i.e. premise–adequacy and inferential or logical adequacy) as set out, with some modifications, below.[94]

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[93] By 'cogent' I mean an argument that is suitably constructed so as to be successful in legitimate achievement of its basic aim of justification–clarification, in the context of its occurrence.

[94] I have reproduced Govier's list of the six different views, the descriptions of which are more or less word for word, with clarifications and simplifications indicated by parentheses and footnotes, albeit with headings that I think better reflect the content of the respective views, and Govier's headings in parenthesis.

An argument is ***cogent*** if and only if

Explicit alethic deductivism: (which Govier calls 'Classical' deductivism)

(1) its explicit[95] premises are *true*; and

(2) its explicit premises deductively entail its conclusion.

Enthymematic epistemic deductivism: ('Methodological' deductivism)

(1) its premises (explicit and implicit) are *acceptable* (to the audience to whom the argument is addressed); and

(2) its premises, explicit *and implicit*, together deductively entail its conclusion.

Strict alethic inductivism: ('Classical positivism')

(1) its premises[96] are *true*; and

(2) *Either* its premises deductively entail its conclusion,
or its premises lend *strong* inductive support to its conclusion.

Strict epistemic inductivism: ('Pragmatic positivism')

(1) its premises are *acceptable* (to the audience to whom the argument is addressed); and

(2) *Either* its premises deductively entail its conclusion,
or its premises lend *strong* inductive support to its conclusion.

Liberal epistemic inductivism: ('Qualified spectrum view')

(1) its premises are *acceptable* (to the audience to whom the argument is addressed); and

(2) its premises are connected to its conclusion by an inferential link that is (at least) *as strong as the argument claims* it to be.

Standard epistemic inductivism: ('Pluralist view')

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[95] I add the term 'explicit' here partly because I take Govier's insertion of the qualification 'explicit and implicit' in the specification of 'methodological deductivism' to conversationally imply that that qualification does not apply in the case of what she intends by 'Classical' deductivism, and is thereby intended to mark one point of difference in this respect. This view is confirmed by Govier's subsequent complaint against Classical deductivism that 'few arguments are deductively valid *as stated*' (Govier, 1992, p396, my emphasis).

(1) its premises are *acceptable* (to the audience to whom the argument is addressed); and

(2) its premises, considered together, offer *sufficient or adequate* grounds for (believing) its conclusion.[97]

One of the problems with Govier's partition of the different views, as illustrated by my choice of headings, is that in some cases a number of logically independent differences are taken to be *jointly* essential to the discriminated views. For example, many philosophers think that it is the *epistemic* status of the premises, such as *knowledge* or *reasonable belief* that the premises are true, that matters with regard to the propriety of the premises. On Govier's account of the alternatives, "Classical deductivism' would need to be rejected in favour of clause (1) of 'Methodological deductivism' by a deductivist who recognises this. Unfortunately though, Govier's division of the alternatives suggests that deductivists who *do* take this view *also* take the view that propositions that are *not explicitly* included (either directly or by logical implication) in the content of what is actually *expressed* in an argument can make a difference to its cogency. Thus, on Govier's account of the alternatives, a deductivist who insists that no presumptions that are not expressed in an argument can contribute toward the cogency of *that* argument[98], but who believes that it is the *epistemic* status of the premises, and not simply their truth that is required, does not fall into any of the positions set out by Govier. Admittedly Govier does claim that pertinent texts reveal *at least* six alternatives. Nevertheless, such a clumsy approach to what is purported to be *differentiation* between the various views hardly seems conducive to analytical clarity.

Another problem with Govier's account of the alternatives is that *all* of the (four) 'views' that present *epistemic* conditions for premises adequacy share *exactly the same account* of the epistemic conditions required. On Govier's survey of the various views, it would seem that

[96] Govier does not make it clear at this point whether form here on, where inductive inference is concerned, 'premises' is supposed to refer to both explicit and implicit premises or only explicit premises.

[97] On Govier's account of the pluralist view, there is an 'additional' condition that the premises are relevant to the conclusion. I take it that it is generally regarded as a norm of argument construction that no irrelevant premises should be included, although accidental inclusion of superfluous premises need not be a significant problem for the adequacy or cogency of an argument provided the intended support for the conclusion is apparent from the relevant premises. Since no argument theorist would want to deny that the premises of an argument should be relevant to its conclusion, to be fair we should regard this as a condition that is common to all views, and that the inferential connection required on each view is required to hold between the *relevant* premises and the conclusion, although we may take these qualifications for granted for the sake of simplicity of expression.

[98] as distinct from an inference, since this is the context in which Govier frames the issue. It is reasonable to suppose that Govier intends 'argument' to be interpreted in the sense in which this is commonly distinguished form 'inference' – as something *expressed* (albeit perhaps in conjunction with associated unexpressed connotations or presumptions) – since otherwise no sense could be made of her distinction between the *explicit* and implicit 'premises' of the argument.

everyone who takes an epistemic view of premise–adequacy thinks that this can simply be spelled out in terms of the *acceptability* of the premises (to the audience). Furthermore, it would seem, from Govier's subsequent treatment of this concept, that she intends 'acceptability' as 'a *normative* concept' (p407) and is related to a concept of acceptance that is synonymous with belief.

> What counts is whether the audience has *good reasons* to believe [the argument's premises] true (Govier, 1992, p396, my emphasis)

This would appear to conflict with subjectivist views on which it is simply what we *believe* that matters for the appraisal of premises, and that the normativity in an argument simply applies with respect to the conclusion granted belief of the premises. On this alternative view, disregarded by Govier, a good argument will show that we ought to accept the conclusion granted what we *actually* believe, and any question that *might* arise with regard to *justification* for the beliefs on which the argument is based will be quite independent of the appraisal of the argument in hand. Furthermore, it is sometimes suggested that neither truth nor belief nor justification for belief is necessary for premise–adequacy. Entirely unexceptionable generalities are hard to come by, and as we have noted it is often supposed that we reasonably 'accept' general scientific hypotheses, in the everyday context of practical inference and decision making, even though we might most reasonably believe that they are strictly *false*.[99] Whatever one might think of that particular approach, it is clearly an unsatisfactory feature of Govier's account of the alternative's that no disagreement whatsoever about the epistemic conditions required for premise–adequacy is recognised between any of the acknowledged alternatives that interpret premise–adequacy in *epistemic* terms.

The deontological aspect of epistemic justification

In what I have said thus far I have taken it for granted that 'justification' may take either the weak form of mere rational *permissibility*, whereby the proposition *may reasonably* be believed (accepted, regarded as likely or whatever) or the stronger form of rational *obligation* to believe or accept the proposition. It is unfortunate that the common terminology of justification in epistemology tends to reflect the weak interpretation. To say that I am 'justified' or 'warranted' in believing a proposition P on the *natural* interpretation of these terms would appear to strictly imply nothing more than that it is rationally *permissible* for me to believe that P. An action or

[99] Maher (1993, p137) for example argues that 'anyone reflecting on the history of science ought to give a low probability (less than ½) to any given significant current theory being literally correct. Yet scientists continue to sincerely assert significant scientific theories.'.

attitude may be said to be 'justified' if it is permissible, and the claim that one *ought* to adopt the respective attitude, or act as prescribed, is naturally taken to *imply* that it would be just to do so. But the mere claim that an attitude is 'just' or 'justified' is not, on the natural interpretation of that claim, generally taken to imply that it is obligatory.

However, despite the traditional phrasing of philosophical discussions of the epistemology of credence in terms of 'justification' and 'warrant', in the normal dynamics of belief appraisal and debate in which arguments arise, there is not in general a presumption that the audience (typically the subject) to whom an argument is addressed *wants* or is naturally inclined to believe the hypothesis in question, and simply needs to acknowledge the *permissibility* of doing so. After all, even an ardent subjectivist might be inclined to insist that despite the subjectivity of his beliefs he has every *right* to hold the opinions he does. But, in the relatively simple *paradigm* case,of argument there is typically a *conviction* on the part of the arguer that the hypothesis is true, and that the evidence on which his or her conviction is based, and which is presented to the audience, is sufficiently compelling to render an attitude of indifference or agnosticism unreasonable. The basic aim of the argument in this paradigm case, in contrast to the natural associations of the concept of justification, is to make it clear to the audience that on rational consideration of the evidence they *ought* to believe the hypothesis – *not* merely that the evidence clarifies the *option* of their believing it. This strong justificatory objective is perhaps best exemplified by the criterion for conviction in criminal law, under which the hypothesis that the defendant is guilty as charged needs to be shown (to the satisfaction of the jury) to be established 'beyond reasonable doubt'. Thus, while it will sometimes be the aim of a (defensive) inductive argument merely to establish that the hypothesis *may* reasonably be believed (or accepted, or regarded with some degree of belief) granted the premises – rather than that rationality requires that it *should* be, it is the more challenging and pressing aim of endeavouring to show, when (in the view of the arguer) it *is* important, that the subject *should* adopt a certain epistemic attitude towards a proposition (in the evidential situation) that will demand the main focus of our attention.

Justification v. Rational persuasion

It is commonly supposed that *persuasion* to believe the hypothesis, albeit on the basis of rational considerations, is the basic function of an argument in the paradigm case. Dummet (1973, p296) for example (even in the context of the *justification* of deduction) simply contrasts 'suasive' arguments – whose role is to 'persuade' the audience 'of the truth of the concluison' with explanatory arguments. Others, for example Fogelin and Sinnott–Armstrong (2004) are in my view closer to the mark when they regard justification as the primary function. Persuasion to reasonably believe might well be a *common* or even typical objective of the arguer when presenting an argument. But even in such a case – insofar as it is intended as essentially an attempt to convince the audience of the *rationality* of the respective epistemic attitude – in the hope that they might adopt the appropriate attitude *on that basis* – the *primary* function of the argument is to make it clear to the audience that indeed that *is* the rational attitude to take. While this aim of clarification of rationality or justification is essential to all cases of rational argument as such. The common albeit supplementary aim of persuasion by production of such argument is not essential. Ti illustrate the point, we can imagine a situation in which someone employs an argument with the intent of making it clear to his audience *that rationality demands* that they *ought* to believe a hypothesis h, say the atheistic hypothesis that there is no God – even if the arguer knows full well that his audience, let us suppose a committed fundamentalist, will *not* conform with the demands of rationality but will most certainly continue along their chosen path of blind faith. The further aim of presenting such an argument in this case might be simply to fulfil what the arguer perceives to be his moral duty at least to *inform* his audience of what rationally demands they ought to believe.

My intention here is not to dispute the importance of the persuasive power of rational argument, or the frequency of its employment as a means of persuasion, but simply to note that evaluation in respect of such application of logical argument *as* a means of persuasion is a matter for the psychologist rather than the logician. Our concern is with identification of the basic criteria of logical *cogency* rather than persuasive power. If the presentation of the argument is suitably constructed so as to make it clear to the audience, or at least to make it satisfactorily easy for the audience to see (should they be willing to seriously consider the question) that rationally they *ought* to believe the hypothesis, but nevertheless they somehow fail to believe it, that is not a failing of the argument in respect of achievement of its essential function *as* a rational argument. That is merely a failure or unwillingness on the part of the audience to accept the clear implications of logical reasoning. But the fact remains that in such a case the original argument nevertheless provides clear propositional access for the audience to a satisfactory clarification of the fact that rationally they ought to believe the hypothesis. And as far as the merits of the *argument* is concerned – that is all we require for the argument to be a good or cogent argument. As a basic characterisation then, the essential function of an argument, at least in the paradigm case, is to make it clear to the audience that the subject (most commonly the audience themselves) rationally *ought* to believe the hypothesis, on the basis of the cited (or offered) assumptions. Thus an argument is cogent, in the sense of importance to us if it is suitably constructed so as to make it clear to the audience that the subject ought to (or may) adopt the epistemic attitude towards the hypothesis endorsed by the arguer (typically, belief, or some degree of belief).

The need to *demonstrate* justification

The initial account of argument I have set out is related to what I perceive to be the basic or primary *function* of arguments. The intended function of an actual argument, on this account, is to *make it clear* to an audience (typically an external audience, sometimes the arguer) by propositional reasoning, that some party (the 'subject' – usually the audience) ought to (or may) believe a hypothesis or regard it as to some extent likely. This might be described as the 'clarificatory' or 'demonstrative' function of argument, although the latter needs to be interpreted in a natural sense of the term associated with 'showing' or making it clear that something is the case, rather than in the unfortunate and unnecessary interpretation of the term whereby it is has came to be commonly utilised basically as a synonym of 'deductive'[100]. Whatever our initial agreements might be, the crucial thing that we need to be able to do, in order to be able to formulate, effectively evaluate, and persuasively employ a cogent inductive argument is to judge correctly and indeed to *demonstrate* – for one's own confirmation, or for that of anyone else who needs to know – when those initial agreements really *do* make it rationally obligatory (or permissible) to believe, or accept, the truth of, some proposition of concern not entailed by the content of those initial beliefs.[101] This concept of demonstration is to be distinguished from the traditional technical sense, or the simplified variant of it exemplified in Salmon's definition:

> A *demonstrative* inference is one whose premises *necessitate* its conclusion; the conclusion *cannot* be false if the premises are true … A demonstrative inference is *necessarily* truth–preserving; a nondemonstrative inference is not. (Salmon 1966, p8. My emphases.)

To use the term 'demonstrative' in such an unnatural technical sense in this way, when there is a perfectly adequate technical term for the concept defined, namely 'deductively valid', and particularly when there is significantly different and important condition for good arguments that would *naturally* warrant application of the term 'demonstrative' is, in my view, an unnecessary and regrettable distortion of the natural use of the term. I intend a more natural interpretation of 'demonstrative', whereby an argument is successfully demonstrative of the acceptability of a proposition simply if it satisfactorily *shows* us that the proposition of concern

[100] Or even sometimes more narrowly as a deductive argument with analytic or logically true premises.

should (or may) *be accepted* if the premises are true (depending of course on what it is intended to show us). This is after all what the essential function of rational argument is supposed to be. In a situation where we cannot determine the truth of a proposition of concern for certain, and its acceptability is not immediately obvious from what is known, argument is required in order to *enable us to see*, or *make evident*, whether the proposition ought nevertheless to be accepted on the basis of the relevant information we have.

In order to do this it is not sufficient simply to present a list of accepted premises which together, in the case of deduction, *happen* to entail the proposition in question or, in the case of induction, which are in some unspecified way *supposed* to 'make' the proposition in question acceptable (except of course in simple and unproblematic cases where there is no reason for anyone to question the suggested entailment or acceptability–making). Even in the case of deduction, it is not sufficient for a cogent deductive argument *merely* that the premises are clearly true and the conclusion *cannot* be false if the premises are true. If that were the case then *any* clearly true premise followed by a conclusion consisting of any analytical conjecture (i.e. a proposition that is necessarily false if false or necessarily true if true) which happens (without our yet having figured it out) to be necessarily true would constitute a cogent deductive argument for that conjecture.

Clearly then, even in the case of a deductively valid argument, whose premises are clearly true, there is a further condition that needs to be satisfied in order for the the argument to a satisfactory or cogent deductive argument – and that is that the argument must satisfactorily *demonstrate* that the conclusion must be true if the premises are. Similarly in the case of inductive argument, a satisfactorily demonstrative inductive argument needs to demonstrate or to *show* us (for example) that the subject ought to accept the hypothesis in view of the evidence. Often we are concerned with issues in situations where neither the truth of the hypothesis nor its inferability from our common assumptions are satisfactorily obvious to all concerned, In such situations, just as my mere intuition that a proposition is true is not sufficient to justify your acceptance of the proposition at issue (or indeed *my* acceptance of it in the latter case), my mere *intuition* that the proposition *follows* from certain agreed premises, or is *made acceptable* by them is not sufficient either. The fundamental purpose of argument, whether deductive or inductive, in such situations is to enable us to *demonstrate* (when it is the case) that a hypothesis ought to (or may) be accepted on the grounds provided.

[101] If our earlier analysis of Hume's core argument for unprovability is correct then in the context of everyday reasoning where we take it for granted that some non–observational assumptions, even if unproven, are just – then Hume's problem of unprovability will not be a problem for this.

We have sufficient technical terminology at our disposal for application to the concept of deductive validity without employing as an alternative a term of much wider and significantly distinct application, in particular when it's more natural interpretation has such direct relevance to the object of our analysis. Moreover, as we have seen, this basic interpretation of the term 'demonstrative' is also important for the appraisal of *deductive* argument, and so needs to be clearly distinguished from the concept specified in Salmon's proposed use of the term. Despite this conceptual distinction between demonstrativity and deductive validity however, we shall see that, as it turns out, even in the context of inductive reasoning, deductive logic is the *only* logic we need in order to be able to demonstrate to an audience that a certain epistemic attitude ought to be adopted toward a hypothesis.

We may note that this condition of demonstrativity (with respect to justification) may be distinguished from the less demanding condition of mere clarity of *intention* discussed above. I may present quite *clearly* an argument that is on reflection quite easily seen to be fallacious. In such a case, the argument *clearly presented* will *fail* to render satisfactorily clear the (mistakenly) supposed *justification* for the respective epistemic attitude toward the hypothesis, and thereby fail to satisfy the demostrativity condition for cogency. In this regard, we may distinguish two aspects of demonstrativity. In order for an argument to be satisfactorily demonstrative it must satisfy two conditions, as follows.

An argument is *demonstrative* iff

(1) The argument makes it quite clear exactly what the purported justification for the hypothesis is *supposed to be*, i.e. what epistemic attitude towards the hypothesis is supposed to shown to be obligatory or permissible, and exactly how this is supposed to be shown.

and

(2) The argument is suitably constructed so as to make it clear to the satisfaction of the audience that the hypothesis *is* justified in the above respect.

Interpersonal roles in the context of argument

One respect in which the aims of different arguments may vary is with respect to the relations between the arguer, the audience, and the subject whose epistemic attitude towards the hypothesis is purported to be justified by the argument. Moreover, since the epistemic status of a proposition is a relative matter, and is dependent on the epistemic position of the particular subject or subjects with respect to whom the attribution of the respective justification for believing the proposition is associated, this question of the epistemic status of the evidential information needs to be related to the question of the epistemic position of the *subject* of the argument (i.e. the party whose justification for a certain epistemic attitude towards he hypothesis is at issue) and to the epistemic position of the intended *audience* of the argument (whom the argument is intended to convince of the justness of the subject's recommended epistemic attitude towards the hypothesis). For example, even if I believe am justified in accepting certain evidence claims, and make an inference that is clearly logically adequate to me, on the basis of that evidence, if *you don't think* I am justified in accepting the evidence claims, I (as arguer) will be unable to employ a corresponding argument to convince *you* (as my audience) that *my* acceptance (as the subject) of the hypothesis that I infer on the basis of that evidence is justified.

Parsons (1996, p174, fn. 13) commends Hamblin's (1970) survey of the traditional literature on fallacies, but admits that he ignores the literature inspired by Hamblin investigating fallacies defined within two–person dialogues 'because of my focus on argument'. This seems to be an odd reason to offer for ignoring a particular kind of fallacy, since of course what is meant by the term 'fallacy' in this general context *is* fallacious *argument*. Nevertheless, Parsons attempts to explain himself by insisting that he does not regard argument as a form of dialogue. The problem with this explanation though is that this literature does not presuppose that argument in general *is* a form of two person dialogue, only that arguments are sometimes *presented within* a context of two–person dialogue (and that the distinction between the two persons can be relevant to the function or purpose of the argument). Moreover, as we have noted even when this is not the case, the basic function of an argument that is prepared and constructed by myself for my own benefit may nevertheless be understood with reference to (at least) two personal *roles* being involved in the justificatory process.[102] On examining Parsons' introductory account of the nature of argument however, certain claims he makes there would seem to be inconsistent with a denial that there are two personal roles involved in arguments. Parsons claims that 'Arguments originate in *texts*, written or spoken.', which are then subjected to a process of *interpretation* yielding a 'refined argument' (Parsons 1996 p165–6). But surely this commits Parsons to the implication that an argument has both an *author*, i.e. the author of the argumentative text or speech, generally termed the 'arguer') and an *audience* or receiver, i.e. the reader or listener, who *interprets* the argument. Moreover, some of the points Parsons wants to

[102] In such a case for example I may be the *subject* whose justification is sought for a certain epistemic attitude towards the hypothesis, and I may also be the *arguer* or in other words the author or presenter of the justification.

make, particularly on the structure of certain forms of argument (Parsons 1996, p168, fn. 5) seem to rely on the platitude that these two roles may (and often will) be taken by *different* people, with potentially differing normative perspectives, since these points make reference to both arguer (expressed in the second person) and an '*opponent*' who might have a different view about what is acceptable.

The significance of the multiple roles involved in the making of an argument is more clearly appreciated when we consider more closely than Parsons does, what is involved in the 'setting' and 'target' of an argument. Parsons (1996, p167) suggests that a 'refined argument' (which is an interpretation of a spoken or written 'source' argument) consists of a reasoning structure propounded within a setting as a means of reaching a target. With regard to the notion of a 'target' of an argument, Parsons is quite brief and has this to say:

> It is part of our notion of argument that it has a *goal*, which is to establish some particular proposition. That is all that I mean by a *target* – it is a proposition to be validated. (Parsons 1996, p168, Parsons' italics, my underline).

But the need for more detailed analysis here is evident when we consider (a) what exactly is meant by 'establishing' (or 'validating'– which Parsons seems to regard as a synonym) a proposition, and (b) whether it is indeed always the goal of an argument to *establish* a proposition. As we have seen, the various combinations of variables that may be involved in the different possible justificatory functions of an argument, above and beyond the differences in the content of the 'object' propositions (which consist of the ontic 'evidential' propositions and the hypothesis) is considerable – and may be illustrated by the examples of various possible combinations of variables set out in the tables below:

ROLES IN AN ARGUMENT			
	ARGUER	AUDIENCE	SUBJECT
ROLE	Clarifies (by production of the argument) that the subject has justification for holding some epistemic attitude towards the hypothesis	Party to whom it is to be made clear (that the subject has said justification)	Party whose justification is to be clarified
POSSIBLE RELATIONS TO ARGUER	Self	Self / Other	Self External Audience Third party

ASPECTS OF EPISTEMIC STATUS OF OBJECT PROPOSITIONS			
OBJECT PROPOSITIONS[103]: EVIDENTIAL ($E_1 - E_n$) & HYPOTHESIS (H)	ALETHIC STATUS of proposition	EPISTEMIC ATTITUDE with which the proposition is (or ought to or may be) regarded (by the subject) as having the respective alethic status.	DEONTOLOGICAL STATUS of epistemic attitude
E_1 E_2 . . E_n H	Truth Verisimilitude Empirical adequacy	*Regard with*: • certainty • acceptance • belief • qualified/quantified point or interval degree of belief (expectation or 'credence')	Rational/epistemic *obligation* e.g. the subject *ought* rationally to regard E_1 as certainly true *or* Rational/epistemic *Permissibility* e.g. the subject *may* reasonably believe that the hypothesis is empirically adequate

This table is not intended to provide a comprehensive account of all aspects of the epistemic status of the propositions that may be relevant to the understanding and appraisal of an argument. Nor do I intend to suggest that each of the headings included will always be applicable[104], or that they will always permit clear and distinct categorisations of the different possible aspects of the epistemic status of propositions. Rather the table is merely intended to provide an illustration of the different factors that may be involved in the interpretation of claims to epistemic justification, and to reinforce the inadequacy of a traditional purely ontic presentation of an inductive argument by mere specification of the supposed 'premises' and 'conclusion'. The sheer variety of the possible epistemic relations that may be understood to be involved in the justification for an inductive inference mean that we cannot, in any particular case, possibly (propositionally) clarify the supposed justification for the inference (or for that

[103] Some of the object propositions might be statements of *objective* (or 'ontic') probability, such as 'The proportion of Fs among Gs is r' – as distinct from the epistemic probabilities, i.e. degrees of credence, with which such propositions might be regarded.

[104] For instance in the case of subjectivist argument it may be the case that there is no (or at least no intended, or intendedly relevant) presumption of any deontological status of the evidence statements – the presumption might be simply that they are believed, or accorded some degree of belief (although it would nevertheless seem that even here an argument will typically be intended to show that since the premises are believed the hypothesis ought to be too).

matter the nature of the *inference* that is supposed to be justified) *unless* we spell out clearly just what the supposed inference and justification is supposed to be.

Epistemic justification, and other uses of argument

Thus far I have said quite a lot about the importance of what I regard as the primary function of argument – basically demonstration of justification – but very little about other possible functions. I have suggested that an important function of argument is to enable us to demonstrate, for our personal confirmation or for the enlightenment of others, that a hypothesis ought rationally to be, or (alternatively) may reasonably be, accepted, or regarded with some degree of belief, on the basis of certain assumptions. However I do not of course wish to imply that this demonstrative function is the *only* function of argument. Let us now consider the relation of this to other possible functions of arguments. In *Reason and Argument* Peter Geach points out that

> Drawing conclusions from accepted premises in order to reach conclusions that you can accept and propound for acceptance is only *one* use of inference. (Geach 1976, p26, my emphasis)

and he goes on to list various other uses of valid arguments. These include working out the logical consequences of theoretical or fictional scenarios for recreational or exercise purposes, demonstrating inconsistencies in certain sets of beliefs, in order to show that some revision is required, and determining the logical consequences of suppositions under consideration, in order to establish whether these are compatible with currently known or determinable facts. However, it may be regarded as implicit in Geach's analysis that the clarification of logical implication which is essential to the fundamental demonstrative function of deductive argument *also* enables these additional functions. Clearly if the principles of deductive reasoning developed (primarily) for application to the inferential function of clarifying valid inferences from accepted premises did *not* also enable those additional applications, then they would simply not be regarded as additional functions of *deduction*.

Analogous points equally apply in the case of *inductive* inference. The primary function of, for example, a relatively simple from of inductive argument – say to clarify that a hypothesis would be rationally acceptable or credible albeit, without being logically implied, on the basis of certain premises – may similarly be applied for example when we wish to investigate the inductive or probabilistic implications of certain suppositions, to assess the viability of possible credence states, to ensure credibility in our story lines, or for exercise of our reasoning powers.

As with the case of deduction, the conditions of logical adequacy for induction could clearly also be applicable in the context of such further functions in which we might not *actually* accept the premises, as well as to the particularly important cases in which we do and actually wish to establish the appropriate epistemic attitude toward the hypothesis. Likewise also, if such additional functions were *not* facilitated by the same principles developed for the fundamental inferential function, then such functions would not naturally be regarded as additional functions of *induction*.

In any case, *whatever* additional functions may be facilitated by the principles required for satisfactory demonstration of justification for logically ampliative inference, our aim here is essentially to establish principles that will enable us to satisfactorily determine and to demonstrate when our initial beliefs and expectations do rationally oblige us to extend our belief or credence to certain propositions not entailed by the propositions initially believed or expected to be true. This function is of crucial importance *in its own right* so as to enable us to produce and appraise the inferences that we need to make and to make well, in the many circumstances where a hypothesis of importance cannot be simply established deductively from our prior beliefs.

Horwich's objection to the justificatory function of argument

It is possible to lose sight of, or even to dismiss, the significance of the basic justificatory function of argument in focusing on other aspects of the relationship that needs to hold between premises and conclusion, in a logically adequate argument. For example, in discussing the epistemic implications of a deductive principle of inference, Horwich argues as follows:

> (1) Modus Ponens does not require of someone who accepts p and $p \rightarrow q$ that he should set about believing q *as well*. For (2) he might reasonably elect to *abandon* p. Rather, (3) its role is to legislate upon the rationality of certain combinations of beliefs. (4) It demands that, *ideally*, if his beliefs include p and $p \rightarrow q$, then q should *already* be included. (Horwich 1982, p74–75, my numbering and my emphasis)

However (2), as it stands, does not support (1), as Horwich suggests it does, because if someone *did* elect to abandon p then the condition of the requirement denied in (1), i.e. that he 'accepts p and $p \rightarrow q$', would not (or at least certainly should not) hold. The relation of logical implication between premises and conclusion in a deductive argument is *synchronic*. That is to say, that although it is possible for all the premises of a deductively valid argument to be true at t_1, and yet the conclusion to be false at t_2 (provided of course that it is no longer the case that all the premises are true) what is asserted in the claim of the deductive validity of the argument is merely that it is not possible for all the premises to be true and yet the conclusion to be false *at the same time*. This condition of synchronicity for deductive validity is likewise carried over into the *epistemic* implications of deduction. If we follow Horwich in setting the point into a scenario where an agent is considering the implications of an instance of modus ponens for his *future* set of beliefs, the epistemic implication of the argument, considered at t_1, is then that if he *does*, at t_2, accept that all the premises are true then he *should* (on the basis of deductive consistency) at t_2, *also* accept that the conclusion is true. Consideration, at t_1, of this requirement might *disincline him to accept the premises* at t_2, if he has stronger reasons for believing the conclusion is false than he has for believing the premises. But the requirement itself is undeniable.

The only potentially defensible reading of (1) would require us to attach different time references to the period over which it is suggested that the subject accepts the premises and the point at which it is proposed that that he should set about believing the conclusion. The point of importance is that, while a proponent of the relevant epistemic implication of modus ponens would want to deny (1) as it stands, such a differentially time–referenced reading is *not* the reading of (1) which any such proponent would want or intend to deny. Nor is it even a reasonably accessible reading of (1) *as expressed.* The force of the extension '*as well*' in the phrase 'he should set about believing q *as well*' can be brought out by making explicit what would take the appropriate place in the expansion '*as well as ....*'. To do this of course we simply need to look back in the sentence to find that this refers to '*believing (or accepting) p and p $\rightarrow$ q*'. But the suggestion that the agent should set about believing q *as well as* believing p and p $\rightarrow$ q, is hardly compatible with an interpretation which assigns different time indexes to the believing of q and the believing of p and p $\rightarrow$ q.

In contrast to Horwich's suggestion in (1), the role of modus ponens mentioned (but not specified) in (3) is normally construed as not merely *forbidding* the combination of p, p $\rightarrow$ q, and ~q, but indeed, as the very structure of the rule suggests, as positively *requiring* the addition of q (should one have failed to include q) to the set of beliefs of any agent whose beliefs (either newly, or continue to) include the combination of p and p $\rightarrow$ q.

The *ideal* demand expressed in (4) is not in itself objectionable as far as it goes, but it does not cover the requirement which is of real practical importance with respect to the common function of argument as a means of justifying belief extensions. Consider what the practical implications of an analogue of (4) might be in the case of a complex deductive calculation from a substantial but reliable set of data, in conjunction with a set of uncontroversial mechanical and geometric principles, where naturally the conclusion is not apparent to you from the assumptions *before* you process the calculation. What of significance does the calculation, once followed, demand of you? In contrast to Horwich's suggestion, it does not *merely* demand that you accept that your initial belief–set was not ideal and that that *past* belief–set should *already* have included the conclusion. If we seriously thought that that was all there were to the demands of logic then our arguments would never get us anywhere. The demand of importance is of course that if you intend to *continue* believing that all the assumptions are true (as presumably you will unless you have some reason to believe that the facts might have altered since your observations were taken) then you *should* ensure that your new belief–set *does* include the conclusion – the analogue of which is exactly what (1) denies.

Even in the case of simple examples like modus ponens, when indeed our initial belief sets would, not merely ideally, but actually be *expected* to already include the conclusion if it included the premises, essentially the same point holds true. The crucial function of logic is not simply to let us know when we have made mistakes like such logical omissions. Although it is important to realise when we *have* made mistakes, the fundamental *purpose* of argument, in general, is for us to *learn* from our realised errors or omissions with regard to the justificatory implications of our prior assumptions.

Such epistemic considerations are just as relevant to induction as they are to deduction. As with the case of deduction, having followed a satisfactorily demonstrative inductive argument for a rational obligation to accept a proposition p granted acceptance of the relevant set of premises, if a rational agent A were to accept the truth of all the premises, then it should be apparent to A that he *ought* to accept p. Similarly in the weaker case of an inductive argument for the rational permissibility of acceptance of p: if A follows a satisfactorily demonstrative inductive argument for the acceptability of p granted acceptance of certain premises, and accepts the truth of all the premises, then A should realise that he *may* rationally accept p.

Having discussed the essential function of argument, and the variable factors involved, in the following chapters I shall argue that the standard formal account of the *structure* of argument fails to account for certain crucial components – components that are in fact essential to the identity of certain sequences of statements *as* arguments, oversight of which makes an unnecessary mystery of the criteria for the logical adequacy of inductive arguments.

CHAPTER SIX

THE STRUCTURE OF ARGUMENTS

In the previous chapter we noted that that there does seem to be a distinction to be drawn between the natural concept of argument, and the formal concept. However some theorists, notably recently Sinnot–Armstrong (1999) and Sorensen (1999) insist on employing the formal conception even in the context of philosophical discussions of fallacies that are commonly supposed to be fallacies of informal reasoning. Since my concern is with natural argument, and moreover I believe that certain central features of natural arguments are crucial to a proper understanding of the relatively simple logic of inductive argument (properly construed) I aim to reinforce in this section that the formal account of argument does not provide a satisfactory account of argument in the natural sense of the term.

As a preliminary to his analysis of the fallacy of begging the question Walter Sinnott–Armstrong (1999) notes that we need to get clear about arguments and their uses. However his account of arguments is somewhat ambiguous. On one possible and indeed quite natural reading (the formal interpretation) it is obviously wrong. And on a less formal interpretation, I shall argue, his account would still be quite mistaken.

Sinnott–Armstrong opens his account of what constitutes an argument with the following pair of statements

> An *argument* consists in an ordered pair of a set of propositions (the premises) and a proposition (the conclusion). That is all there is to an *argument* ... (Sinnott–Armstrong, 1999, p174).

There are two possible ways to read this claim. The ambiguity here turns on the interpretation of the propositional content that is supposed to be read into the parentheses, and is basically as follows.

The formal interpretation

On one interpretation, the parentheses are not to be interpreted as qualifiers of the surrounding statement, and the definition could equally stand alone without the parentheses. On this reading, Sinnott–Armstrong appears to suggest that an argument consists simply of an ordered pair of a set of propositions and a proposition, [105] and insists that *this* is all there is to an

[105] This interpretation is explicitly endorsed by e.g. Sorensen (1999).

argument. On this interpretation, the parentheses would be taken simply to elaborate the point by adding that the premises and conclusion of an argument are nothing more than the respective elements of such an ordered pair. This would appear to be a natural reading of the opening pair of statements.

The main problem with this interpretation is that, understood in this way, Sinnott–Armstrong's claim is obviously false. For one of countless possible counterexamples, the ordered pair of the set (P) of propositions expressed by a sequence of mile stones leading to London – {'The distance from here to London is ten miles', 'the distance from here to London is five miles', ...} and the proposition (C) expressed by the subsequent arrival sign – 'Here is London', is an ordered pair of a set of propositions and a proposition.[106] But *this* ordered pair of a set of propositions and a proposition does not constitute an argument. The problem is not merely that it is a *bad* argument, which is the line of objection that Sinnott–Armstrong considers. The problem is rather that it is not an *argument* at all. This particular ordering of a pair of propositions and a proposition does not make up an argument, in the natural sense of the term – it just makes up a system of information about distances from London. The elements of *this* ordered pair are *not*, in virtue of this ordering relation, a set of premises and a conclusion. For another example, consider the case of a set of news reports followed by a weather forecast. We can define an ordering relation Rxy such that x is the set of propositions expressed by the news report immediately preceding the proposition y expressed by the first weather statement. Likewise, it is patently obvious that such an ordered pair of a set of propositions and a proposition is not an argument, and that the set of news statements do not constitute a set of premises in support of the meteorological statement.

The less formal interpretation

In view of this extreme implausibility on the natural and strong interpretation of Sinnott–Armstrong's claim as described above, we might then suspect that he actually intends some other interpretation by these opening statements. On a less formal interpretation, Sinnott–Armstrong would *not* wish to commit himself to the extra–parenthetical statement pair but the parentheses may be taken to indicate that he would however go along with the statement formed by substituting the terms in parenthesis for their counterpart descriptions in the main body of the sentence.

[106] Ordered by the relation 'Each member (P_n) of {P} *specifies the distance of the milestone expressing P_n from the place* that C claims to be here'.

An argument consists of an ordered pair of a set of premises and a conclusion.[107]

On this less formal and less implausible interpretation,[108] the objections noted above against the formal account would no longer apply. But the fact that Sinnott–Armstrong did *not* choose to express his claim in some such relatively simple and more favourable terms, and instead chose rather to express the claim in a way that clearly facilitates the *stronger* interpretation, seems to suggest that he did in fact *intend* the stronger interpretation. Moreover, his subsequent concluding statement to this brief section appears to confirm his commitment to the formal reading of the initial statement pair, since here there is *no* qualificatory reference to premises and conclusion:

> there is *nothing more* to an argument than an ordered set of propositions ... (Sinnott–Armstrong, 1991, p174, my emphasis)

This then would appear to suggest that no significant work is being done by the parentheses in his opening statement pair (as we noted with regard to the formal interpretation) and thereby rule out any suspicion that the relatively informal interpretation is what he really intended here. However, things are not quite that simple. Immediately following the latter quoted statement – which appears in the context of his earlier statements to commit him to the view that something's being an ordered pair of a set of propositions and a proposition is *sufficient* for its being an argument – Sinnott–Armstrong adds the much weaker claim that

> every such ordered set is *potentially* an argument (Sinnott–Armstrong, 1991, p174, my emphasis)

It is difficult to see how any significance could be attached to this weaker claim if he intended to stand by the apparent implication of his opening claim (above) that an argument *consists in* such an ordered pair. One way that the apparent tension here might be eliminated is by supposing that actually Sinnott–Armstrong did intend the less formal interpretation after all, and that all he means by this latter claim is rather that every such ordered set (of propositions) may potentially be *used* as (or perhaps more plausibly 'in') an argument. However this latter statement is to be

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[107] Note that this is Parsons' account of 'the technical notion most commonly found in logic texts' (Parsons 1996, p164)

[108] I mean less implausible as a candidate account of argument in the natural sense. I acknowledge that this is not a particularly plausible account of Sinnott–Armstrong's intended interpretation. As I suggested above the natural reading of the full account is that it is intended in the formal sense. I am only suggesting here that there is another way that an interpreter *might* take the significance of the parentheses (and that it gets us closer to the right account of the natural concept of argument – but as I shall argue, not close enough).

interpreted, it would seem that Sinnott–Armstrong's account of what constitutes an argument appears to be at best misleading and at worst quite implausible. On the most likely interpretation (the formal interpretation) of his initial claims there are clear and obvious counterexamples. But on an alternative interpretation of his initial claim, and more overtly in his concluding statements, he appears to invite a less formal interpretation when he suggests rather that what he initially claims to *constitute* an argument is only *potentially* an argument.

Since we have seen that the formal interpretation of Sinnott–Armstrong's claim is a non–starter (as an authentic account of the natural concept of argument)[109] let us consider the merits of the less formal interpretation of his claim – on which it is suggested rather that an argument consists of an ordered pair of a set of premises and a conclusion (on, we may suppose,[110] a natural interpretation of these latter terms). In what follows, we shall see that even this more conservative list of constituents is fundamentally flawed – something more needs to be added to make an ordered pair of a set of premises and a conclusion into the larger structure that is an argument.

The inferential connective

Even on this less formal and less objectionable interpretation of Sinnott–Armstrong's account of what constitutes an argument, the account is still not quite right. The common informal account of an argument as consisting *solely* in a set of premises and a conclusion is somewhat overly simplistic. An essential part of what makes up an argument – and indeed that part which endows the other parts of the argument with their respective status *as* premises and conclusion, is an *inferential connection claim.*

In informal representations of arguments the inferential connective generally occurs within the same sentence that expresses the conclusion (or the premises, or both) in the form of a word such as 'Therefore ...', 'Thus ...', or 'so ...', or 'Since'. But of course in an analysis of the composition of an argument, the inferential *connective* needs to be clearly distinguished from the *content* of either. But if we wish to make a distinction between the inferential connective and the conclusion (and indeed the premises) then it would seem, at least on the face of it, that we need to acknowledge that an argument is composed not *merely* of an ordered pair of a set of premises and a conclusion, but of an ordered triplet of a set of premises, something that is

[109] which admittedly it might not be intended to be.
[110] In order to maintain a distinction from the formal concept, and to facilitate authenticity as an account of the natural concept.

represented by an inferential connective, and a conclusion. In this case, even on the more favourable interpretation of his claim, Sinnott–Armstrong would still appear to be wrong to suggest that an argument consists *only* of an ordered pair of a set of premises and a conclusion.[112]

What then is the significance of the additional component – the inferential connective? In order to clarify this we may consider what exactly it is that an arguer is intending to express when he or she presents an argument 'P, *therefore* C'. Whether or not the arguer intends his or her series of statements to entail both P and C,[113] it is clear enough that such an argument is not *just* a statement of P *plus* a statement C. There is something more that an arguer intends to convey in expressing the *argument* 'P, *therefore* C', and that is the implicit claim that there is an *inferential connection* between P and C – as signified by the inferential connective – in other words that C (in some either general or more specific sense) 'follows' or 'may be inferred', from P.

In fact in natural presentations of arguments it is not uncommon for the respective inferential connection claim to be *spelled out* in some such terms with phrases such as 'From this it follows that ...', or 'We may infer from this that ...', or 'In view of this evidence it is highly likely that ...', and the commitment to the conclusion itself (when the connection claim is strong enough to imply that there is such a commitment) is often simply taken for granted once the premises and the statement of the inferential connection have been given. This kind of example then provides a natural illustration of the fact that the common inferential connective 'Therefore ...' should be understood to signify both a claim that there is an inferential connection between the foregoing premises and the conclusion *and* a (perhaps respectively qualified)[115] claim of the conclusion.

Inferential connection claims

It is notable that Govier (1992) appears to acknowledge the presence of an inferential connection claim in her initial account of what it is to present an argument.

[112] Certainly as these are standardly understood: I say 'on the face of it', in the above because as we shall see, the inferential connective may be regarded as signifying an additional claim that is relevant to the supposed justification for the hypothesis, in which case this claim could be regarded as an additional (obscured if not strictly 'missing') premise of a special kind.
[113] We have seen in the previous chapter that it is not, or at least by no means always, quite as simple as that.
[115] I shall discuss the interpretation of probability as a qualifier in Chapter 8.

In effect, an arguer putting forward an argument does these three things:

(1) She asserts the premises.

(2) She asserts that *if* the premises are true (or acceptable) the conclusion is true (or acceptable).

(3) She asserts the conclusion.

... What we call the challenge of argument is to construct and respond to arguments in ways that are appropriate to this basic structure. We must think through the premises and reasoning given and base our acceptance or rejection of the conclusion on this reflection. (Govier, Trudy, 1992, p61–2)

It is clear enough that Govier is not claiming that these three things are *explicitly* asserted whenever an argument is put forward. She accepts that 'Either premises or conclusions may, on some occasions, be unstated.' (1992 p58). She also notes that 'not all arguments contain indicator words' (1992 p6) – whose significance is that of (2) in the above, since an indicator word (or phrase) 'indicates that the conclusion is being inferred from the premises supporting it' (1992 p4).[116] It would appear to be in respect of these occasional omissions from the surface structure of an argument that Govier merely claims that these 'assertions' are made *in effect* when an argument is put forward. What Govier seems to be claiming here then is that these assertions are at least conversationally *implicit* when an argument is put forward. Unfortunately however Govier does not appear to make anything of this apparent acknowledgement of a commonly overlooked component of argument in her discussion of the issues of argument cogency, deductivism, and the logic of inductive inference. In contrast I shall argue that acknowledgement of this central component of argument provides the key to a proper understanding of the structure and *logic* of inductive inference, as well as the issue of deductivism

[116] Govier lists a number of indicator words and phrases (1992 p5). What is notable is that some of these appear to indicate stronger inference claims than others (contrast '*proves* that' and 'may be *deduced* from', with 'as *indicated* by'). However it seems to be a significant oversight on Govier's part that despite this, and despite listing *twenty eight* alternative indicator (or 'inference') words and phrases, Govier includes exactly *none* that are overtly *probabilistic* in character. Just in case Govier intends us to think otherwise, we should clarify that it would be quite implausible to suppose that when someone completes a detailed analysis of a body of evidence in defence of their opinion that P with a connecting phrase which says that P is 'highly likely' or 'most probable' on the evidence, they aren't putting forward any argument at all.

[120] i.e. internal to the passage. We may allow that some sense can be made of reference failure in respect of external references, e.g. 'The present king of France is bald.', when a simple difference in external facts could repair the failure – in other words the failure is not essentially a failure of the statement or passage in itself. But when an (explicit or implicit) reference in a passage depends for its success on there being some other linking term or statement in the same passage, and there is no such linking term or statement then we cannot possibly make sense of the passage as it stands.

The plurality of inferential connections

Govier lists a number of indicator words and phrases (1992 p5). What is notable is that some of these appear to indicate stronger inference claims than others (contrast '*proves* that' and 'may be *deduced* from', with 'as *indicated* by'). However it seems to be a significant oversight on Govier's part that despite this, and despite listing *twenty eight* alternative indicator (or 'inference') words and phrases, Govier includes exactly *none* that are overtly *probabilistic* in character. But of course it would be quite implausible to suppose that when someone completes a detailed analysis of a body of evidence in defence of their opinion that P with a connecting phrase which says that P is 'highly likely' or 'most probable' on the evidence, they aren't putting forward any argument at all. Moreover, it would seem most reasonable to suppose that such variations in the inferential connection phrases do actually signify related distinctions with regard to the associated inferential connection *claims*.

Because the word 'Therefore' (or even simply a premise/conclusion demarcation line) is commonly employed as an all–purpose inferential connective for informal or semi–formal representations of arguments, it is easy to make the mistake of thinking that in *any* argument the inferential connective carries a single *common*, and relatively vague, interpretation – tantamount to something like the following proposition

> From the preceding proposition the following proposition may be somehow inferred

But while the connection claim resulting from the application of such a broadly interpreted connective to any particular pair of premises and conclusion would presumably be *entailed* by the connection claim of *any* argument employing the same premises and conclusion, it could not seriously be maintained that this broad connection claim is the *only* connection claim that is ever entailed by the inferential connection claim of any argument – or indeed that it is the only important one. Certainly in natural presentations of arguments we are often much more specific in our inferential connection claims. In some circumstances we may be concerned to stress that the conclusion follows *deductively*, employing appropriate terminology to make the deductive connection quite explicit. Similarly, in the context of an inductive argument we may for example employ a phrase such as 'In view of this it is highly likely that …'. And similarly the point that this is *specifically* a claim of probabilistic connection might well be significant in the context.

In view of this plurality of inferential connectives, the acknowledgement of this additional component of an argument can play an important role in the individuation of significantly distinct arguments that might, on superficial representations (and in particular on the formal account), otherwise appear to be identical. For example, distinctions in the interpretations of the inferential connective, rather than between the respective ordered pairs of the premise set and the conclusion, are crucial to the distinction between a natural inductive version and the possible if forced (and patently invalid) deductive version of the following argument structure.

(E) All of the many emeralds that have been observed are green
If there were any non–green emeralds, it would be highly likely that some of them would have been observed by now.
Therefore, All emeralds are green

Although the deductive argument that results from a deductive interpretation of the inferential connective in (E) would be unlikely to be seriously employed, and would clearly be an invalid deductive argument, it is nevertheless an argument which could be *expressed.* It could quite easily be expressed in natural english simply by replacing the remainder of the argument following the premises with the words 'From these premises it follows deductively that all emeralds are green'. A natural account of one possible inductive interpretation of (E) on the other hand might read 'In view of this it is reasonable to believe that all emeralds are green'. On the former deductive interpretation the argument *clearly implies a falsehood* (namely the deductive inferential connection claim) and *fails* to set out clearly in whatever respect in the hypothesis might reasonably be supposed to be justified. On such an intended interpretation of the inferential connective the argument would clearly *not* be a cogent argument. In contrast, on an inductive interpretation, as exemplified above, the argument clearly does *not* entail that same falsehood, and does not so obviously (at least not in the view of an inductivist) fail to set out the respect (and sense) in which the inference may reasonably be supposed to be justified. Since the ordered pair of the premise–set and the conclusion in either case is the same, the formal account of argument composition and individuation fails to distinguish between these significantly different arguments. In view of the complex variables involved in inductive justifications as illustrated in the previous chapter, the sheer variety of inferential connections that might be implicitly (or explicitly) appealed upon in the context of a justification for an inductive inference makes it imperative as a condition of argument cogency that the content of the supposed inferential connections are, one way or another, rendered quite clear.

The significance of the sequence of propositions in an argument basis

In a footnote to his account of the constitution of an argument Sinnott–Armstrong considers the possibility that the *order* of the premises in an argument matters to its identity. On our account it may be noted that the inferential connections linking basic assumptions to intermediate conclusions and ultimately to the final conclusion impose a logical order wherever it *is* required within the sequence of propositions in an argument. Of course in informal representations of arguments, some of the crucial elements are commonly taken for granted. These elements are often referred to as 'suppressed' or 'hidden' premises, although on our account we may note that these may also include suppressed or hidden *inferential connection claims*. The most basic representations of arguments merely list the premises, in an order such that it is reasonably easy to see the logical connections implicit in the line of reasoning, and present an explicit inferential connective of some kind only prior to the final conclusion. And we have already seen that in plain English representations of arguments it is in fact quite common to make the final inferential connection claim explicit and to take the *conclusion* for granted.

To illustrate how the line of implication implicit in an argument may impose a partial ordering on the propositions composing the argument basis we may consider a simple example.

(SW) 1. Socrates is a man
2. Socrates is old
3. All old men are wise
Therefore, Socrates is wise

In the line of reasoning that we would naturally take to be implicit in the argument represented by (SW) a number of key elements are suppressed in the representation presented. In this line of reasoning the suppressed intermediate conclusion that Socrates is an old man may be taken to logically follow premises 1 and 2, and this logical ordering is imposed by the suppressed inferential connection claim that 'Socrates is an old man' may be (deductively) inferred from the pair of premises 'Socrates is a man' and 'Socrates is old'. The order within that pair of premises is unimportant to that intermediate inference, but the latter inferential connection claim makes that pair of premises logically prior to the respective intermediate conclusion. Similarly the pair composed of this suppressed intermediate conclusion and the explicit premise 3 is rendered logically prior to the final conclusion by virtue of the explicit final inferential connection, but the order within this pair is also unimportant to the inference.

Inferential connections and rules of inference

In formal and semi–formal presentations of arguments it is often explicitly claimed that the connection between certain premises and an inferred conclusion is established by a particular rule of inference. Normative, as distinct from purely prescriptive forms of rules or principles of inference are generalised forms of inferential connection propositions. By a prescriptive rule of inference I mean a rule of inference of the form 'Given premises (of some schematic form) A *infer* the conclusion (of some schematic form) B'. This is to be distinguished from a normative principle of inference such as (for example) 'Given premises of form A the conclusion B *may (or should) be drawn.*' It is clearly the latter normative interpretation that is pertinent to the primary *justificatory* function of argument which is our concern.

A normative propositional *principle* of inference then is a *general* claim, such as that expressed by the following form of modus ponens:

> For all values of the propositional variables P and Q: from the premises 'P' and 'If P then Q', the conclusion 'Q' may be inferred.

And a specific inferential connection claim such as

> From the premises 'Holly is a springer' and 'If Holly is a springer then Holly will be loopy' the conclusion 'Holly will be loopy' may be inferred.

is an *instance* of such a general inferential connection claim. The implicit line of reasoning then, when such a *general* principle of inference is employed in an instance of an argument such as, for example

(H) Holly is a springer.
If Holly is a springer then Holly will be loopy.
Therefore, by modus ponens, Holly will be loopy.

is that the implicit specific inferential claim that this *particular* conclusion follows from these *particular* premises *via* the deductive principle of inference modus ponens, since it is an instance of, and thereby implied by, that general inferential connection claim.

Must an argument have premises? – A response to Sorensen

Sorensen (1999) denies that an argument must have premises. He offers as a purported example

(MT) Therefore, there are arguments without premises. (Sorensen 1999, p498).

But this certainly does not appear to be an argument in any *natural* sense of the term. In fact by the standards of ordinary English syntax a passage consisting of just an expression of (MT) without any immediately prior statements would not appear to make any sense whatsoever: Spelling out in longhand the significance of 'Therefore' we have a reference to some 'proposition(s) immediately prior in this passage'. And unless there *are* some, to discharge that implicit internal[120] reference of the term 'Therefore', the passage is syntactically ill–formed, and in that respect nonsense.

Nor does this appear to be compatible with our basic characterisation of the natural concept of argument – on which an (actual) argument presents a *propositional clarification* of some justification for regarding a hypothesis with a certain epistemic attitude (typically believing or accepting that it is true). In other words, an (natural) argument *sets out* certain propositional considerations, which we call 'premises', *in support* of a certain view of the hypothesis.

Sorensen's argument for his denial relates to the role that the formal notion of a premiseless argument plays in enabling various other formal (meta–logical) concepts and distinctions.

> Premiseless arguments should be classified as arguments for the excellent reasons logicians cite: The empty premises set helps us distinguish proofs from derivations, it helps us define the concept of logical truth ..., and lets us crisply contrast inference rules that require assumptions .. with those that require no assumptions .... In short, our best theory of argument is best served by this well–entrenched definition. (Sorensen 1999,499)

In order to make sense of this argument we need to be sure we properly interpret the crucial term 'argument' in Sorensen's initial clause 'Premiseless arguments should be classified as arguments' (his conclusion). Now Sorensen has made it clear from the outset that he is employing the standard formal concept of 'argument' here. Paraphrasing his definition in a form amenable to a suitable abbreviation, an 'argument' in this formal sense may be defined as an Ordered Pair of a Proposition and a SET of propositions – and in order to avoid any misinterpretation or confusion with the *natural* concept of argument, as we have characterised it, we might refer to such a *formal* 'argument' accordingly as an 'OPPSET'.

Now the reasons cited by Sorensen are no doubt good reasons for acknowledging the usefulness of acknowledging the existence of OPPSETS *whose proposition sets are empty*, in respect of the role they play in enabling the various other formal concepts and distinctions he mentions. And of course, since the word 'argument' has traditionally been employed in formal logic as the term of choice for reference to OPPSETs, we might be inclined to concede that this may equally be regarded as a good reason for classifying such 'premiseless' OPPSETs as 'arguments' in that *equivalent* sense of the word (i.e. as OPPSETS). But of course *that* is presumably not a claim that an opponent of Sorensen in this matter would want to deny. When Sorensen says in his opening statement that 'Many sensible people think that an argument must have premises', he is presumably *not* talking about people operating in the context of formal (meta)–logic who are saying something about 'arguments' in the technical sense associated with formal logic (i.e. OPPSETs). He is talking about people who, operating in the context of informal logic, are expressing a view about arguments in the *natural* sense of term. But when he immediately goes on to say, in the context of his meta–logical proof of the conclusion of the conclusion of (MT), that

> MT has all the parts listed in the standard definition of 'argument' (Sorensen, 1999, p498)

he has switched, within two lines of dialogue, to talk about a quite *different* hypothesis, concerning something that is only an 'argument' in the *technical* sense. The definition he presents is *not* a standard definition of 'argument' in the *natural* sense of the term – which is the sense that is relevant to the initial point of contention.

The substantive issue here then is whether OPPSETs whose proposition sets are empty should be classified as 'arguments' in the *natural* sense of the term. But as we have seen, natural arguments have important features that corresponding OPPSETS lack, and so the above–mentioned considerations do *not* constitute good reasons for classifying OPPSETs whose proposition set is empty as 'arguments' in the *natural* sense of the term.

Anticipating an objection along these lines, Sorensen adds that

> If you want to say that MT only qualifies as an argument under a 'technical' definition of 'argument', then be prepared to say that the empty set only qualifies as a set under a technical definition of 'set'. (Sorensen 1999,499)

But Sorensen does not offer any explanation as to why we should think that the latter should in any case be *objectionable*. In fact it seems to be a platitude, since the notion of the 'empty set' *is* a technical concept – specifically associated with a technical concept of 'set'. If this is meant to be some kind of reductio ad absurdum of our response to Sorensen's initial argument, then it clearly won't do, since there is nothing whatsoever absurd about the supposedly associated commitment.

Nor does our disallowing premiseless natural arguments, prevent our making good use in *informal* logic, or practical reasoning, of the applicatory links with the formal concepts and distinctions mentioned by Sorensen. We can for example argue, in the natural sense of the term, that a certain statement expresses a proposition that is a logical truth – as *formally* defined – by pointing out that the proposition expressed can be derived in formal logic from no assumptions. Similarly we can *argue* that a statement expresses a proposition that is a logical falsehood by pointing out that the negation of the proposition expressed can be derived *in formal logic* from no assumptions. In either case, if our audience were disinclined to take such a premise on trust, then in preference to actually arguing the point in detail we might opt most simply to *refer* to some textbook derivation. Alternatively we could actually go to the other extreme and take the laborious line of *arguing* the point, for example by providing (or referring to) an appropriate truth tree diagram, and step by step, propositionally *setting out* exactly what should become clear from consideration of the pertinent features of it. A compromisory alternative, to either expressly arguing the point or merely referring, would be just to supply an appropriate diagram, *without* any associated explanation or argument, in the hope (or reasonable expectation) that the audience will be able to interpret it appropriately and thereby, on consideration of the pertinent points, *figure out for themselves* that the proposition (or, respectively, its negation) may be formally derived from no assumptions.

As if to clinch the case in his favour, having presented the technical details of the proof in his opening paragraph, Sorensen notes that

> the conclusion of MT can be rigorously (though trivially) proved within meta–theory.
> (Sorensen, 1999, p499)

But of course that merely emphasises the point that the conclusion of MT *is* to be interpreted in a technical meta–logical sense. And as noted above, we have no substantive objection to make against that trivial technical point. Having said that, it is worth noting that there is nevertheless a significant terminological objection that may be raised against (MT) *as stated*. The problem here is that by employing the common word 'argument' – a word whose *natural* usage in the context of everyday reasoning as distinct from formal logic is even more deeply entrenched – in application to such an (in *some* respects) significantly disanalogous technical concept, we are inviting exactly the kind of confusion and potential for misunderstanding as we have seen exemplified in Sorensen's arguments above. Since we often want to talk about the *application* of formal logic to the analysis and appraisal of natural arguments, and so *both* of these importantly different senses of the word 'argument' will often crop up in the same context, it would be preferable, at least when there is a danger of such conflation, to employ a more clearly technical term of reference for application to the technical concept, such as the respective definition abbreviation I have suggested.

The logical status of inferential connection claims: A special kind of premise?

To summarise thus far, we have observed that the standard formal and semi–formal accounts of what constitutes an argument, respectively as an ordered pair of a set of propositions and a proposition, or as an ordered pair of a set of premises and a conclusion, appear to be quite unsatisfactory – so long as they are interpreted in such a way as to ignore the inferential connection claim that is implicit in the inferential connective, and which *makes* a series of assertions (consisting of an inferential connection claim along with the surrounding statements it claims to be so connected) an *argument*. These implicit or explicit inferential connection claims also impose whatever relevant logical orderings there may be within the sequence of statements that compose the argument, and can determine the type of inference that is operative within the argument. Not only do the inferential connection claims comprise an essential part of the argument in itself, but they also play an important role in distinguishing between different types of argument, and even between distinct individual arguments that share the same ontic evidential propositions and hypothesis (traditionally identified as the 'premises' and 'conclusion'). Moreover, as we have seen, it is clear that the particular interpretation of the inferential connective in an argument can be highly relevant to its appraisal.

However, once the inferential connection claim is recognised as an essential component of the argument, and it is also recognised that, at least in the case of inductive arguments, the claim itself may be at least as open to question as the evidential premises it connects with the hypothesis – in contrast to the straightforward case of traditional ontic deduction when it will typically take the form of a logical truth[121] – then on reflection it would seem most appropriate

[121] In the case of an inference that is not logically ampliative, and where the hypothesis is directly asserted with the intent of unqualified commitment, i.e. an alethic deductive inference, we may be inclined to regard the pertinent inferential connection claim in the corresponding argument simply as an implicit appeal to a logical truth (basically of the form 'If P (the conjunction of the premises), then C (the conclusion)' where C is a logical implication of P) – in virtue of which the statement series is an argument, but which, for the sake of simplicity, need not be counted as a premise that requires explication – at least not in the normal context where the standard principles of deduction are generally taken to go without saying. However, in situations where certain details of the logic of deduction are in dispute, such an exemption will not be admissible (or at least with respect to inferential connection claims to which the points of disagreement are relevant). Graham Priest (1987) presents an example of how someone who thinks that some contradictions are true will not necessarily be convinced of the falsity of a proposition by

to regard the inferential connection claim itself, at least in the case of induction, as a substantive *presumption* – and moreover a presumption on which the supposed justification for the hypothesis crucially *depends*. In view of this we may be inclined to regard the appropriate construal of the logical status of the inferential claim in the context of the argument as that of a *premise* along with the others (the evidential premises) – albeit of course a particularly important premise of a distinctive kind. In this case it would appear that the *semi*–formal account of an argument as consisting of an ordered pair of a set of premises and a conclusion might seem to be compatible with our ultimate analysis after all – at least in application to the case of inductive (or 'logically ampliative') inferences – and so long as it is understood that at least one member of the premise–set is an inferential connection claim, and that it is the main (final) inferential connection claim within the premises that imposes the respective ordering on the pair, and in virtue of which the pair constitutes an argument. However I see this as more of a matter of terminological choice rather than a substantive issue. What matters is that the hitherto traditionally unrecognised inferential connection claim be acknowledged as an essential part of an argument. In the following chapter I shall examine the implications of acknowledgement of the inferential connection claim as an essential part of the argument basis for the question of the *logic* of induction, and the issue of deductivism.

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provision of a reduction ad absurdum. We may suppose that the audience in such a case will not agree the associated inferential connection claim.

[123] Salmon also employs the more expressive phrase 'deductive chauvanism' for 'deductivism', coined by J. Alberto Coffa – in whose memory the volume is dedicated.

CHAPTER SEVEN
INDUCTIVE VALIDITY, AND THE CASE FOR EPISTEMIC DEDUCTIVISM

A broad concept of validity

In his introduction to *The Limitations of Deductivism* (Grünbaum and Salmon, 1988) Wesley Salmon suggests that

> *Deductivism* ... is the view that the *only* logical devices required in the empirical sciences are deductive. (Grünbaum and Salmon, 1988, p2)

Salmon goes on to distinguish two qualified versions of deductivism – one being the view 'that the only admissible scientific explanations are deductive–nomological' – which he calls *explanatory deductivism*[123]'. Salmon distinguishes this from deductivism with respect to *inference*, which he calls *inferential deductivism*, as

> the view that the only legitimate *arguments* are valid deductions (Grünbaum and Salmon, 1988, p2, my emphasis)

In the context of *non*–deductive arguments, Salmon refers to the corresponding inferential relation that is supposed to hold between premises and conclusion (by those who think there *are* 'legitimate' non–deductive arguments) as 'logical correctness' (Salmon, 1966, p6).

However, bearing in mind that *deductive* validity is in any case commonly referred to specifically *as* such (in order to eliminate any potential confusion with alternative informal or contextual concepts of 'validity', I see no particular difficulty in employing the term 'valid' in a general or broad sense, that covers the corresponding relations for non–deductive arguments (if there are any) *in addition* to the particular *type* of validity that is supposed to hold in the special case of *deductive* argument. In this case we may simply draw a distinction between *deductive* validity, which is generally understood to consist in the logical or semantic impossibility of the conjunction of the premises being true while the conclusion is false, and 'validity' in general – as any relation whereby a conclusion is in some respect inferable from certain premises. This provides us with appropriately related terminology, while maintaining a simple and natural means of distinction between the two concepts. If we then interpret Salmon's term 'legitimate' or 'logically correct' in terms of *general* validity as I have described it, i.e. in the *broad* sense applicable to both deductive *and* inductive arguments, then (inferential) deductivism on

Salmon's account, amounts to the view that there *are* no valid arguments other than those that are *deductively* valid.

An apparently inferentially sound inductive argument

Consider the following argument

(RW) (1) Urn A contains exactly 999,999 white balls and one black ball (and this will continue to be the case at the time of the next draw).
(2) The next ball to be drawn from urn A will be drawn at random.
Therefore:
(3) There is only a one in a million chance that the next ball drawn will not be white:
Therefore:
(4) The next ball to be drawn from urn A will be white.

In particular, for the sake of simplicity, let us focus on the step of inference from (3) to (4). This step of inference is certainly not deductively valid. Moreover, the premise provided appears to constitute a perfectly good *reason* for believing the conclusion. It seems undeniable that this inference is broadly speaking 'valid' or 'inferentially adequate' in the sense that matters to us. And it certainly appears, at least on the face of it, to be deductively *in*valid. But is it really so?

I have said that an argument is *demonstrative* iff it clearly *sets out* the justification purported for the target proposition or hypothesis, granted whatever epistemic status (e.g.degree of belief) is supposed to be justly accorded to the premises. Although, following on our analysis thus far, it seems clear enough that argument (RW) is 'valid' in the sense that matters to us, there are number of reasons for saying that this argument *as it stands* nevertheless fails to *set out clearly* the justification that the premises are supposed to provide for the conclusion. As we noted, there are a variety of possible epistemic attitudes that might be taken towards the target proposition of an argument. One might, for examples, regard the target proposition as absolutely certain, virtually certain, simply 'believed', contextually 'accepted', more likely than not, with a 'high' degree of belief, or even with a specific degree of belief. In a case of (RW) the arguer *could* intend to establish by this argument that the conclusion (in this case identical to the target proposition) is *acceptable* (i.e. *may* reasonably be accepted) – which is the kind of justification associated with the particular concept of epistemic validity on which we have focused.

However, as we also noted earlier, not all arguments are intended to be interpreted defensively or permissively in this way. In fact it is more common for arguments to be employed *persuasively.* Thus in another instance of (RW) the arguer might intend to (rationally) persuade the audience to believe the target proposition, by using the argument to show them that the target proposition *ought to be* believed (or regarded with a high degree of belief). Of course in many cases it might be quite obvious from the context that an argument is being used persuasively rather than defensively. But the point that needs to be noted is simply that as far as the *demonstrative* quality or *clarity* of an argument is concerned, it is in general better if (at least wherever there is any room for doubt) it is made explicit *within* the argument exactly what kind of justification is intended to be understood as being claimed for the target proposition: whether the intended claim is for example that the target proposition *ought to be virtually fully believed* or, for one alternative, that it *may reasonably be regarded as more likely than not.* Once this justificatory *claim* is clear we may proceed to consider the cogency condition of demonstrativity: does the argument clearly set out in virtue of exactly what considerations the purported epistemic status of the hypothesis is supposed to be justified?

Following on our earlier discussion of the structure of arguments, this point about the different types of justification that may be purported to be established for the target proposition may be related to the appropriate interpretation of the inferential connective 'therefore'. As I have argued, arguments entail truth–valuable claims other than those explicitly specified either side of the premise/conclusion separator. An individual step of inference is typically represented as consisting in a set of premises P and a conclusion C which may or may not be accompanied by reference to a rule or principle of inference by virtue of which the conclusion is supposed to follow or to be inferable from the premises. But regardless of whether any such rule or principle is mentioned, what distinguishes such an *inferential* sequence of statements from other ordered pairs of the same (sets of) assertions – and indeed what makes it an *inference* as such – is that besides *asserting* both sets of propositions, when someone presents the respective pair *as* an argument or step of inference, they implicitly (if not explicitly) claim that the one follows, or *is inferable*, from the other.

For a clear argument it will need to be clear in what *sense* the conclusion is supposed to be inferable from the premises, for example whether the implicit claim is that the conclusion is deductively inferable from (a semantically necessary truth condition for) the premises or that it is (in some sense) inductively inferable. This of course relates to the type of validity that is supposed to hold between the basic premise set and the conclusion. Where a deductive inferability relation does not hold between these, the implicit claim might instead be that something along the lines of the type of epistemic validity we have described above will hold between them. In other words, one possible interpretation of the inferability claim in a non–deductive case might be the claim that if one were justified in fully believing that all the propositions in the premise set P are true, then one would be justified in believing or accepting that the conclusion C is true.

However it is notable that in natural arguments the premises of an inference are often presented in epistemic terms. For example:

(A_0) You know that John is an honest man.
So you ought to accept that what he says is generally true.

In this case the target proposition – i.e. the proposition whose epistemic status is intended to be established by this particular piece of argument – is 'What John says is generally true'. And in the process of informal or semi–formal argument analysis the target proposition is traditionally identified as the conclusion. What are taken to be the premises are likewise typically detached from the epistemic operators within which they might have been presented in the context of their natural expression. Thus a standard construal of the above natural argument would take the relatively simple non–epistemic form:

(A_1) Premise: (P_1) John is an honest man.
Conclusion: (C_1) What John says is generally true.

However, even if we were to accept that this simplified construal of the argument provides a satisfactory account of the respective contents of the premises and conclusion, it would still fail to provide a clear account of the nature of the inferential connection that is implicitly claimed to hold with respect to the premises and conclusion so construed. In contrast, the original expression of the argument (A_0) provides a natural indication of the inferential connection claimed by the proponent of the argument to hold between these propositions. By appealing to the audience's *knowledge* that John is an honest man, it would seem that knowledge P_1 is taken to be the relevant condition for the purported justification for C_1. And since the epistemic operator within which C_1 is enclosed in A_0 is one of *obligation* for acceptance, it would seem that the underlying inferential connection claim is the claim that if the premise *is* known then the conclusion *ought* to be accepted.

This is subtly different from the permissive acceptance validity that we, following the line of Fox, have discussed above – on which certainty of the premises is merely taken to be sufficient for rational *permissibility* of acceptance of the conclusion. This distinction characterises the distinction between an argument that is merely defensive – an argument which imposes no rational obligations relating to belief of the target proposition on the audience – and an argument that is persuasive or binding – rationally demanding some degree of belief or acceptance of the target proposition. Details such as this are essential for determination of the type of epistemic validity that is implicitly purported to hold by virtue of the appropriate interpretation of the premise–conclusion connective – and ultimately for the success or otherwise of the argument in demonstrating the purported justification for the conclusion, but can be lost in the traditional simplistic mode of representation that takes an argument to consist in nothing more than a set of premises and a conclusion, and commonly interprets epistemic terminology in the natural expression of an argument as merely providing an indication of their status in the context *as* a premise or a conclusion.

Such simplistic representation is *relatively* unproblematic in the case of deductive arguments since the relation between semantic or logical implication that is fundamental to deductive validity and the basic type of epistemic validity generally associated with it is relatively straightforward and taken for granted. Consider a semantically valid argument, employing what is appropriately interpreted to be a semantic inferability connective, such as an argument of the form

(D_1) (1) If P then R
(2) If Q then R
(3) Either P or Q
Therefore [i.e. a semantically (or logically) necessary condition for the truth of (all) the preceding assertions is the truth of the following assertion]
(4) R

Now it is *conceivable* that a sceptical reader of this argument, who was quite certain that all of the premises were true, and who was also quite certain that together they imply R (the inferential connection claim), might nevertheless fail to be certain that R, and even fail to realise that he *ought* to be certain that R. He might even offer in defence of his position the argument that personal certainty is not a *guarantee* of truth, in which case one of the premises *could* (logically possibly) be false *despite* his certainty (justified full belief) that it is not – in which case the *conclusion* might be false. However, a natural response to his position would be that considerations of probabilistic coherence demand that if one is certain that a set of propositions are all true, and that they semantically imply another, then one *ought to be certain* that the implied proposition is also true. Of course this epistemic principle is something that we ordinarily take for granted.

A speaker has an obligation to be honest and responsible in his communications. Thus, while the proposition that the speaker believes what he says and that he is justified in so doing are not semantic implications of his assertions, they are nevertheless 'conversational' or communicational implications of his assertions. In other words if the speaker is to be trusted then it may be taken from his assertion of a proposition that he believes what he says and that he has satisfactory evidential warrant for believing it. Similarly, a speaker has a responsibility to qualify his statements appropriately when he is significantly less than certain of their truth, i.e. when his evidence warrants only *partial* belief. So an unqualified statement conversationally implies that the speaker is certain of it (or at least virtually certain).

Furthermore, unqualified statements also carry communicational implications with regard to the appropriate epistemic attitude for the audience: Unqualified statements are made with the understood intent that they should be believed. The basic idea is that an audience who *trusts* me *will* believe what I say – on account of my word that it is so, and in trust of my honesty and responsibility – if they did not already believe it on account of prior evidence. It is of course possible that my audience might have conflicting information from another (initially) trusted source. But if that is the case then their initial trust in me, in this instance, will be undermined, diminished[124], or put on hold pending further investigation.

One of the responsibilities on the speaker associated with this situation of trust, is to deliver information in such a way as to respect the freedom of the listener to *decide for herself* whether to believe something that she has no rational *obligation* to believe. In this case, if I wish to express something that I *choose* or *happen* to believe, albeit justly, i.e. something that it is *permissible* to believe on the evidence, and which I *do* believe – but which I don't regard as something that my audience (or for that matter myself) necessarily *ought* to believe, then in view of the context of trust with respect to the communication of information, it would be (at least potentially) misleading to express such a belief as an *unqualified* statement of fact. Rather, I ought to qualify the communication of my premissible but non–obligatory belief that P with something along the lines of 'I believe that P', or 'It is reasonable to believe that P'. This is so

[124] In an ideal communicative situation, free from risk of deception, irresponsibility, or innocent error, I can (and should) *fully* believe the information I am given. In the normal situation where there is a degree of mistrust in most situations (even if the informer is entirly honest and responsible he or she may nevertheless mistaken) I will attach a degree of belief to what I am told. Even in an instance of receipt of information from a normally highly trusted source, if the information conflicts with information from

despite my genuine and justified belief that P is true, in virtue of my understanding that the evidence I know of does not obligate belief in P.[125] In view of this, when I responsibly assert a proposition P *without* qualification, that conversationally implies that my audience (may and indeed) *ought* also to be sure that P.

This may then be related to the common understanding that if someone is (or at least ought to be) certain that the premises of a deductively valid argument are true, then they ought to be certain that the conclusion is true. This is the strong form of *epistemic* validity that is naturally and rightly associated with the *semantic* validity of a deductively valid argument. Just as the relevant normative epistemic qualifications of the premises of D_1 (such as 'You ought to be certain that if P then R') are conversationally implied by the assertion of the unqualified premises, so the associated epistemic inferability claim is conversationally implied by the more basic semantic implication claim of D_1. In this case the following epistemic implicatory variant on D_1 may be taken to be understood in the context of the employment of D_1.

(D_2) (1) If P then R
(2) If Q then R
(3) Either P or Q
Therefore [i.e. if you ought to be certain that the preceding assertions (1 – 3) are true, then you ought to be certain that the following assertion (R) is true]
(4) R

Moreover, as we have seen, on the basis of the normative conversational implications of the unqualified assertion of the premises (1 – 3) the audience may also take it that

(5) You ought to be certain that assertions (1 – 3) are true

on the basis of which, taking the epistemic inferential claim of D_1 as a second premise

(6) If you ought to be certain that the assertions (1 – 3) are true, then you ought to be certain (R) is true

another normally highly trusted source, the usual high degree of belief will not, in that instance, be automatically accorded to the respective proposition.

[125] Jackson (1987 Ch.6) discusses some of the ways in 'The hearer may learn about the nature of the evidence which is up for borrowing ... from which premises are selected for presentation in the first place.'(p107). This might be regarded as one example of such responsibility on the part of the arguer.

a *semantic* argument (D_3) may be made, via the semantic implication claim

> *Therefore* [i.e. a semantic implication of the preceding propositions (5 and 6) is the following proposition]

to the *epistemic* conclusion

> (7) You ought to be certain that R

In a situation where the epistemic aim of the argument is to rationally persuade the audience to be certain that R, and in the process to clarify that they *ought* to be certain of it, this implication ought to be perfectly clear and, in respect of that, ought ideally to be explicit. Likewise, since the key factors in the logical burden of support for this *epistemic* conclusion are the conversationally implicit deontic epistemic qualifications of the initial premises ('You ought to be certain that if P then R' etc) and the *epistemic* inferential connection claim (6) conversationally implicit in the semantic implication connective of the initial version of the argument, these too need to be quite clear if the epistemic function of the argument – including crucially clarification of the *justification* for the hypothesis (the initial conclusion) – is to be carried through satisfactorily.

Of course, as we have noted, the communicational epistemic implications associated with semantic inference, such as those that we have drawn out here, are usually taken for granted, being too obvious and trivial to mention. For that reason, along with the relative simplicity of the basic semantic version of the argument, they are not generally made explicit in the context of straightforward deduction (where the target proposition is a logical consequence of prior knowledge). But that does not alter their crucial role in the all–important epistemic dynamics associated with the basic assertions and semantic implications expressed.

Moreover, and more importantly for our purpose, in arguments where the target proposition is *not* deducible from the set of propositions whose epistemic status is purported to supply justification for the target proposition – the 'evidential' or 'grounding' propositions (the premise–related counterparts of the hypothesis) – the presumed epistemic status of the grounding propositions and the associated epistemic implications that are supposed to relate the epistemic status of the grounding propositions to the justification for the target proposition will not always be obvious. In such a case, if the argument is to satisfactorily clarify the purported justification for the target proposition – in other words if it is to be satisfactorily demonstrative

– these (the presumed epistemic status of the grounding propositions and the associated inferential connection claims) will need to be made explicit.

However, once these *are* made explicit, the result is a series of epistemic propositions – i.e. the specifications of the purported justificatory status of the grounding propositions, e.g.

> You ought to know that there is only one in a million chance that the next ball drawn will not be white.

the inferential connection claim, connecting the latter with the justificatory claim for the target proposition, e.g.

> If you ought to know that there is only one in a million chance that the next ball drawn will not be white, then you should be virtually certain that the next ball to be drawn will be white.

and the justificatory claim for the target proposition, e.g.

> You should be virtually certain that the next ball to be drawn will be white.

But once the argument is reconstructed in this way, the resulting series of epistemic claims will form a satisfactorily demonstrative and moreover *deductively valid* argument for the justificatory claim associated with the initial demonstratively inadequate version of the argument (in this case the step of inference from (3) – (4) in (RW)).[126] Regarding the issue of premise–adequacy, it is clear enough that the *truth* of these premises is sufficient for the subject's being *justified* (in the sense described in the conclusion) in regarding the hypothesis with the respective attitude. But we have seen that what is required for argument cogency is that the argument *makes it clear* to the audience that the subject is so justified (the requirement of demonstrativity) – in which case the argument will nevertheless fail in this respect unless it is *clear* to the satisfaction of the audience that all the premises are true.

[126] Musgrave (1993, p170–5) presents a somewhat dubious argument for a view of inductive inference that he presents as Popperian, and which yields an example argument of the supposedly Popperian line of inference that has the logical structure of an epistemic deductivist construal as I have described it. Fox (1999, p455–6) presents some criticism of Musgrave's example, but (with some qualifications related to points that I have discussed above) approves of the basic strategy.

Is epistemic deductivism *relevant* to argument appraisal?

Having noted the inferential connection claim as a key if commonly overlooked assumption (at least in the case of induction) on which the conclusion inferentially depends, and the fact that once it is taken into account among the premises, the logical structure of *any* argument thus properly explicated is revealed to be deductive, it would seem that this clinches the case for inferential deductivism. The only arguments that are valid (in the broad sense) are those which are deductively valid simply because *all* arguments properly explicated are deductively valid. But if deductive validity is thus trivially satisfied in *all* cases of argument doesn't this then make a nonsense of the notion of validity as a criterion for argument cogency? No. 'Validity' in the broad sense does not depend on the trivial deductive validity that applies to whole arguments, fully explicated. Recall that an argument is 'valid' in the broad sense, if and only if its inferential connection claim is *true*. In this case for example a fallacious (supposedly) deductive argument that would be *traditionally* counted as deductively invalid, will still be 'invalid' in the broad (and important sense) on account of the *falsity* of its inferential connection claim. Furthermore, what *is* actually *deductively invalid* in such a case is not (as has been traditionally supposed) the actual *argument* (in the natural sense of the term) but the OPPSET (i.e. the ordered pair of a proposition and a set of propositions) composed respectively of the proposition expressed by the conclusion and the *evidential* premises (which don't include the traditionally overlooked inferential connection claim of the argument). Of course the deductive validity or otherwise of the *latter* (the traditional OPPSET) is what is of particular concern to us in evaluation of the validity of straightforward (non–inductive) deductive arguments – not, as has been traditionally supposed, because that *is* (all there is to) the argument, but because the deductive validity or otherwise of that OPPSET is *what determines* the truth or otherwise of the deductive *inferential claim* of the argument in such *non–inductive* cases. The general deductive validity of arguments in whole may indeed be regarded as relatively insignificant, in itself, to their evaluation. But the crucial point of epistemic deductivism, along with its essential acknowledgement of the larger (as it happens ultimately deductive) *structure* of arguments – is particularly significant in the analysis of induction, as it forces us to analyse the essential (in this case generally epistemic) *content* of the pertinent inferential connection claim – *evaluation* of which is the *crucial* determinant of the validity (in the broad sense of inferential adequacy) of indictive arguments. Moreover, as we have seen, even in the case of an argument that is presented in a traditional deductively valid form, the epistemic status of the premises may in some cases *not* be presumed to be certainty, and in such cases the presumed *epistemic* significance of the stated premises for the stated conclusion (*granted* their supposed epistemic status) may be of considerable significance for appraisal of the argument – in which case the presumed *epistemic inferential connection* will still need to be explicated and appraised (and the truth or otherwise of *that* implicit inferential connection claim may well be underdetermined by the deductive validity of the associated OPPSET).

Quick counterexamples to deductivism?

Fox (1999) has recently claimed to have refuted deductivism, by appealing to very simple apparent counterexamples which he calls 'epistemic syllogisms'– constructed simply by taking *as a premise* a statement of the acceptability of the conclusion. Examples of these are

> (E_1) One should accept that P. Therefore P.

and

> (E_2) It is reasonable for me to believe that P. Therefore P.

are discussed by John Fox in a recent comment on deductivism (Fox, 1999, p450–1. Fox goes along with the customary restriction of the term 'valid' to the context of deduction and talks instead about the question of the 'soundness' of non–deductive arguments:

> Arguments (argument forms) such that given [the truth of] their premises, it is (always) reasonable to accept their conclusions, I call '*sound*'. (Fox, 1999, p448)

One problem with this as we have seen is that the aim of showing that *it is reasonable to accept* a hypothesis is just one of many possible aims for an argument.[127] But, even granting (for the sake of other objections) his account of the appropriate relation of soundness, is Fox is right in suggesting that the cases he cites do indeed provide counterexamples to the strict deductivist.

Do arguments from conclusion–justification beg the question?

As a general characterisation of the significant features of such forms of inference, we may call such arguments 'arguments from conclusion–justification'.[128] This description is indicative of the respect in which I would suggest such arguments may be regarded as 'question–begging'. Of course the conclusion is not specifically assumed outright. Most arguments that beg the question don't do so quite that overtly. Not all arguments are intended to provide an assurance

[127] Fox (1999, p448) does however *acknowledge* different kinds of purported merit for arguments, mentioning three and opting to 'deal with the third'.

of the impossibility of the falsity of the conclusion, granted the truth of the premises. When an argument is intended to establish some more modest epistemic objective (granted the premises) such as establishing or confirming that one ought to *accept* a conclusion P, then taking as an assumption the proposition that that epistemic attitude towards the conclusion *is* (as the argument is supposed to establish) an attitude with which one ought to regard the conclusion, effectively presupposes what the argument is supposed to establish, and in that respect such an argument may naturally be said to 'beg the question'. The practical problem with this is that it seems to disable such an argument from the performance of its intended clarificatory function (in any context where the argument is needed in order to perform such a function).

Having considered the fallacy of begging the question, as apparently exemplified in these cases, it would appear in retrospect that we may have interpreted Salmon's use of the term 'legitimate' too narrowly in suggesting that this might be simply equivalent to 'valid' in our broad sense of the term. It is of course common to regard even certain types of argument that are undeniably *deductively* valid as, in a significant sense, 'illegitimate' or 'improper'. The particular cases that naturally spring to mind in this regard are precisely those that are commonly dismissed as question–begging. If we then incorporate this restriction into our interpretation of Salmon's notion of the 'legitimacy' of arguments, so that 'legitimate' implies 'valid, *non–question–begging*' then the examples of what I have called 'arguments from conclusion–acceptability' that are taken by Fox to 'refute deductivism' would not after all appear to constitute counterexamples to (strict) inferential deductivism as described by Salmon, since they appear to beg the question.

Moreover, the objectionability of question–begging is intimately related to the fundamental *purpose* of the requirement of validity. The basic reason *why* we require arguments to be *valid* is because we want to be able to use them, in their basic and central function – to *establish* the acceptability of propositions (when indeed they should be accepted) *in situations of ignorance or doubt* about those propositions (or *at least* about the rationality of their acceptance). It is precisely in such situations of fundamental concern, where the rational acceptability of a proposition needs to be established, that arguments which simply *presume* either the truth *or the acceptability* of the proposition in question are of no avail. Arguments from conclusion–

[128] Fox called such arguments 'epistemic syllogisms'. I take it that my description, if less brief, affords a clearer indication of the pertinent features of the relevant kind of argument.
[131] Of course, in cases where a simplistic ontic argument *is* elliptical for something epistemic, as we have noted should often be the case in inductive arguments, the interpretation of the implicit inferential connective would of course need to be adjusted accordingly.

acceptability, like arguments from the conclusion, are inherently unfit to perform such a function.

In these kinds of argument the acceptability of the conclusion is simply *taken for granted*, either explicitly (in the case of arguments from conclusion acceptability) or conversationally implicitly (in the case of arguments from the conclusion). Thus, despite their apparent validity, by their nature, such arguments will be *unable* to establish the acceptability of their respective conclusions, in any situation where that is not already acknowledged, respectively either because, in such a scenario, the *premise* is not acknowledged, or because the *acceptability* of the premise is not acknowledged.

Problems for the appeal to question–begging

However there are a couple of related problems with this line of defence for the strict deductivist. First, although philosophers sometimes make reference to a purported type of 'question–begging' – typically in terms of 'rule–circularity' – that is somehow supposed to relate to *inferential* adequacy, as distinct form mere premise adequacy – it is seems quite obvious that the cases Fox invokes here are merely cases where we have question–begging *premises*. And since the key claim of the strict deductivist must surely be understood as a claim about the *logical* correctness of arguments – relating to *inferential* adequacy – this appeal to the question–begging nature of these arguments might not after all constitute a satisfactory defence for the strict deductivist. Furthermore, this point would appear to confirmed when we look more closely at Fox's suggestions about the *context* in which such mini–arguments might be applied.

With reference to an allegedly Popperian approach to 'Countering Hume on induction' Musgrave (1993, p170–175) presents a somewhat dubious argument that is supposed to provide a justification for the epistemic conclusion that it is 'reasonable to believe' the hypothesis of his chosen example inference. Fox approves Mugrave's basic strategy exemplified here of arguing (dediuctively) to such a *justification* claim – albeit as a fundamental *part* of a larger form of inductive inference that he (Fox) endorses. Representing this portion of the overall inference by D, Fox endorses a construal of inductive inference of the general form 'D+E', where 'E' represents the 'epistemic syllogism' – or what we have described as an argument *from* conclusion justification. In view of this, returning to the charge of question–begging against the epistemic syllogism, we can see that in such a context, where E only constitutes the *latter* part of a larger argument for the ultimate conclusion (i.e. the hypothesis itself) the charge of begging the question would appear to be defused, since there is an underlying justification (of whatever merit) provided for the justificatory proposition (the premise of E). In other words the premise of E – which the strict deductivist might like to dismiss as question begging – will not fall so easily, since it is not actually an *assumption* of the ultimate argument in which E is to be employed, but is merely an *intermediate* premise, *derived* from prior assumptions.

Are arguments from conclusion justification *valid*?

However, as we have already noted, what is in any case of particular concern to the deductivist as such is *inferential* adequacy or in our broad sense of the term *validity*. And in respect of this question, on the basis of our earlier considerations, we may nevertheless find fault with Fox's epistemic syllogism. We have noted that an essential and defining constituent of a justification–clarificatory argument as such is an at least implicit *inferential connection claim*. Musgrave (1988b) and Fox (p455) both note that any *cogent* deductively invalid argument 'P therefore C' can be transformed into a deductively valid argument by the addition of the (thereby acceptable) 'validating conditional' – 'If P then C' – but dismiss the point as 'trivial' – presumably since such a transformation would be justified only if the *original* argument were (understood to be) cogent in the first place. However, so long as the *stated* argument is *not* intended to be somehow elliptical for a more complex epistemic argument (in which case it should anyway be reformulated respectively for clarity) the implicit *inferential connection claim* of the argument as stated will simply *be* precisely the *validating* conditional. As we noted earlier, what the inferential connective (that *makes* the sequence of statements an argument) 'P *therefore* C' *means* is basically *since* P is the case so is C (or 'from P, C follows') – thereby *implying* '*if* P then C'.[131] Supposing then that the epistemic syllogism

(E_2) It is reasonable for me to believe that P. Therefore P.

is intended exactly *as stated* – and is not elliptical for something more complex.[132] If this *is* the case then the argument, via the connotation of 'Therefore' *entails* the inferential connection claim "P follows from 'It is reasonable to for me to believe that P'" which, at least in typical cases of P, will be plainly *false*. Fox himself notes (p455) that the *generalised* form of the corresponding conditional (in the case of our chosen example, 'For all A, *whenever* it is reasonable for me to believe that A, A' will be simply false, and appears to conclude from this that epistemic syllogisms cannot 'be reconstructed as valid' by (justifiably) *adding* such a

[132] As an argument to an overtly *ontic* conclusion – *in the context of argument where epistemic qualifications where applicable are explicitly being employed* (notably in the expressed premise) the *natural* construal of such an argument *is* of course that the unqualified ontic conclusion is *fully intended.*

premise. However, what he seems to have failed to notice[133] is that the validating conditional of the epistemic syllogism (E_2) – 'If it is reasonable to for me to believe that P, then P' – doesn't *need* to be added to the syllogism, since it is already *implied* by the inferential connective.[134]

This result yields two problems for Fox's approach to inductive inference and his purported refutation of deductivism. The first is simply that since the inferential connection claims of epistemic syllogisms will generally be false, they will not supply a satisfactory part of any general form of inductive inference – as Fox appears to hope they will. And more generally – if, as I have suggested, we regard inferential connection claims (at least in the case of inductive inference) as substantive albeit special *premises* – epistemic syllogisms will in any case fail to constitute counterexamples to deductivism simply because on deeper analysis (revealing the covert inferential connection claim – which in conjunction with the primary premise deductively *implies* the conclusion) they *like any other argument* are ultimately seen to be *deductive* in their underlying logical structure.

[133] as indeed Musgrave seems to have failed to notice the corresponding *general* point that arguments *imply* the inferential connective

[134] and therefore in the corresponding general form of argument the generalised conditional will likewise be implicit

CHAPTER EIGHT
EPISTEMIC PROBABILITY: QUALIFIER OR RELATION?

I have argued that the proper way to construe an inductive argument is as an epistemic argument to a conclusion that consists in an epistemic qualification of the hypothesis. Kyburg, with reference to Hempel has argued that this is the wrong way to construe such arguments. In this chapter I shall attempt to defend the position I have endorsed against their objections, and to further clarify the concept of epistemic probability that is appropriate to the construal of probabilistic inductive arguments.

Kyburg and Hempel on the proper construal of statistical inferences

Claiming to follow Carl Hempel (1965), Henry Kyburg sets out two alternative views of the proper schematic form for an inductive argument, as follows:

> The schema for an inductive argument can take either of two forms (as pointed out by Carl Hempel [1965]):
>
> Premises
> =======
> Conclusion with hedge R
>
> or
>
> Premises
> =======   R, characterising the *inference*
> Conclusion
>
> In other words, we may ask whether an inductive argument has the form: from this evidence, it follows that the hypothesis H is probable (plausible, supported, ...) or the form: from this evidence H follows with probability, plausibility, support ... so and so? Or perhaps sometimes one and sometimes the other?
> (Kyburg, 1990, p60)

Kyburg declares his own view on this matter, with reference to Hempel (1965), and offers an example in illustration of the point:

> Hempel argues persuasively that the latter is the form we should adhere to – that is, that our body of scientific knowledge consists of categorical empirical statements rather than modally modified ones. The evidence we have supports (to some degree or other) the conclusion that the relative frequency of male births among humans *is* close to 0.51, not that it is *probably* close to 0.51. (Kyburg, 1990, p60)

Hempel does indeed argue that the latter is the interpretation we should adopt, but he does not do so in the patently fallacious way that Kyburg suggests here, i.e. by effectively *changing the question*.[135] The question is *not* 'Does our body of scientific *knowledge* consist of categorical empirical statements rather than modally modified ones?'. Rather the question is 'How should we construe the form of *non-deductive statistical arguments*?' (c.f. Hempel 1965, 57–8). In contrast to Kyburg's above–noted 'reformulation' of the question in terms of scientific *knowledge*, Hempel relates this more specifically to the interpretation of the concept of (inductive) *probability*, yielding the question 'How should we understand the concept of probability: as a modal qualifier or relation?'. Furthermore, on closer examination of Hempel's understanding of the issue, it becomes apparent that he did not intend to maintain that *categorical assertion* of the hypothesis should even be one of the alternatives under consideration here.[136]

Why is categorical assertion of the hypothesis not one of Hempel's options?

Before moving on to Hempel's arguments though, we may first pause to consider *why* Hempel did not propose categorical assertion of the hypothesis as a feature of one of the options. The particular 'example' cited above by Kyburg may in fact be utilised in illustrating one of the basic reasons why such a form of construal would not generally be appropriate. First we may note that Kyburg's peculiar choice of *conclusion*, as essentially a statement of *statistical* 'probability' is hardly apt in this context, particularly in view of the specific *type* of argument discussed by Hempel in this regard. To appreciate just how inappropriate this example is, we may note that the issue addressed by Hempel is quite specifically set in the context of critical appraisal of statistical syllogisms or 'quasi–syllogisms' as Toulmin (1958, p109) describes the qualitative versions of them, i.e. arguments of the following kind of form:

a is F

[135] Or at least he does not do so by misrepresenting the question in *this* way, although he does appear to make the same *kind* of mistake, in another respect as we shall see.

[136] The only point in this passage where Hempel appears to condone the possibility of interpreting a certain form of argument in such a way 'so that we are entitled to assert [the hypothesis] categorically and

The proportion of Fs that are G is q
Hence, with probability q, a is G

a is F
The proportion of Fs that are G is less than 2 per cent
So, almost certainly (or: probably) a is not G

a is F
The statistical probability for an F to be a G [in other words, the relative frequency with which an occurrence of F yields an outcome G] is nearly 1
So, it is almost certain that a is G

(Hempel, 1965, p54–5, latter parenthesis mine)

Thus, in the type of argument discussed by Hempel with regard to this issue, a statement of statistical probability or relative frequency, such as that mentioned in the example presented by Kyburg, would generally take the place of the statistical *premise* of such a syllogism, rather than its conclusion. We may also note that it is quite clear, particularly from consideration of the numerical example given, that in the type of argument discussed by Hempel the epistemic probability of (the 'credence rationally to be given to'[137]) the hypothesis on the basis of the statistical evidence, as expressed in the conclusion of the statistical syllogism is appropriately related to the statistical frequency or proportion described in the statistical premise. If the former is not appropriately related to the latter, as for example in the following

a is a new–born human
The relative frequency of males among new–born humans is close to 0.51
Hence, it is almost certain that a is male.

then the argument will fail to conform to the general form endorsed by proponents of the statistical syllogism. Mismatches such as this between the statistic described in the respective premise and the degree of credibility claimed in the conclusion clearly result in a fallacious argument. Applying this point to the example in hand, one way in which the argument may be amended (or at least that particular problem may be eliminated) goes as follows.

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without qualifications' (Hempel 1965, p59) is with regard to a schematization of a *deductive* syllogism where the hypothesis is qualified in the conclusion by '*certainly*'.

[137] Hempel 1965 p64, c.f. also the discussion, over p61–2, of Toulmin's interpretation of the epistemic probability claim in the conclusion. Note also that Kyburg acknowledges that what is at issue is the question of whether the hypothesis needs (at least sometimes) to be qualified in terms of epistemic

a is a new–born human

The relative frequency of males among new–born humans is close to 0.51

Hence, it is marginally more likely than not that a is male.

In contrast to the categorical claim which it seems Kyburg would like us to agree on the basis of *his* partially specified example, it is patently clear that in the context of *this* argument we would *not* want to detach the hypothesis (in this case that a is male) from the probability qualification. Even if we have no doubt at all about the premises of this argument (and this is all the relevant information we have to go on with regard to the hypothesis – the 'total evidence' requirement as Hempel calls it) these premises would clearly not warrant a *categorical assertion* of that hypothesis. Thus it would seem, on consideration of examples like this, that we must admit that at least sometimes the hypothesis *needs* to qualified in terms of epistemic probability. More specifically, when the degree of credibility warranted by the statistical evidence (in conjunction with the singular premise) is not sufficient to support outright belief, acceptance, or categorical assertion, the hypothesis in the conclusion of an argument of this kind should *not* be detached from the respective epistemic probability qualification.

In order to further illustrate the implausibility of such a policy of rejecting probabilistic qualification in favour of categorical assertion, we may also consider its implications in the context of 'confidence' or 'credible' intervals, i.e. estimations of the probability, or degree of confidence, that a parameter under investigation falls within a specified interval. Neyman (1937), proposed just such a 'categorical–assertion' treatment of confidence intervals, whereby the statistician 'must *state*' that the true value of the parameter under investigation lies between the respective confidence bounds. However there are intuitive objections to this approach to estimative inference, particularly in cases where only a limited confidence coefficient can be established for any usefully narrow interval. As Howson and Urbach (1993, p237–8) object, if we consider narrower intervals, there will be a correspondingly smaller confidence coefficient that applies to the respective hypothesis that the parameter under investigation lies within the narrower interval (or more accurately the interval with the lower frequency of error). In principle these confidence coefficients will range from 1 to 0, but it would hardly be defensible to suggest that in cases where the interval is very narrow and the coefficient correspondingly low it would be reasonable to *categorically assert* that the parameter falls within that interval.

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probability, yielding 'statements involving "probably", "likely", and the like in their epistemic usage' (Kyburg 1990 p60).

In his defence, perhaps Neyman may have intended to suggest only that categorical assertion would be in order in application to intervals for which the confidence coefficient is quite high (traditionally levels of .95 or .99 are considered). But even then there are still dangers that may be associated with the detachment of even high confidence qualifications in the treatment of interval estimates as categorical assertions. For example, when such a statistical estimate constitutes only one factor in a range of consideration relevant to the judgment of guilt of some crime by a particular party, ignoring such a confidence qualification may decisively misdirect the judgment of guilt when the latter is subject to a high probability criterion. Aitken (2000, p9–14) discusses the importance of sampling estimates and associated confidence coefficients in the context of criminal law, noting for example how such estimates can be related to sentencing judgements. However he also observes that in many state courts in America the quantity of illicit material within what may be a very large consignment e.g. of pills, 'is an essential element of the possession charge and as such must be proved beyond reasonable doubt.' (Aitken 2000, p12). This then leads Aitken to examine different approaches to the probabilistic interpretation of 'beyond reasonable doubt'. Suppose the court proposes a 95% probability criterion for conviction, and that a 95% confidence interval for the relevant quantity is established with the lower bound on the incriminating threshold. According to Neyman's criterion, the right policy is then to *categorically assert* that the consignment contains the critical quantity. Suppose however that there is also an issue of *identity* of the party responsible for the illicit consignment, and that a judgement is made that there is a 95% probability that the defendant is the owner (or the party responsible for the content) of the illicit consignment. Ignoring the degree of doubt about the critical quantity in the assignment, having taking that as 'proven', on the proposed standard of 'reasonable doubt' the court must then find the defendant guilty. But if we were to take into account the (5%) degree of doubt about the illicit content consignment *and* the degree of doubt about the identity of the owner (also 5%), then clearly we cannot consistently maintain only a 5% degree of doubt that the consignment contains the critical quantity *and* the defendant is the owner, in which case the defendant should be acquitted.

Kyburg's own lottery paradox (1961, p197 and 1990, p64–5) conveniently illustrates the logical difficulties that we can get into if we adopt the categorical assertion approach even in cases of extremely high objective probability. But, whether we are concerned with singular or statistical hypotheses, in cases of only modest probability, the importance of clarifying and maintaining appropriate probability qualifications that may apply to the conclusions of our inductive inferences (whether these be interpreted entirely subjectively or more objectively) can hardly be overestimated, and has accordingly became common practice in statistical and predictive inference. In view of the overwhelming practical and theoretical problems associated with

Neyman's policy of categorical assertion, it has been largely superseded by an explicitly qualified confidence approach to statistical estimation whereby the proposition that a parameter lies within any given interval will typically be assigned a certain degree of confidence or credibility on the basis of the evidence. It is generally acknowledged that in most cases of statistical or probabilistic inference only a limited degree of confidence in the hypothesis will be warranted on the basis of the evidence, and explicit acknowledgement of the respective degree of confidence in the conclusion of the inference will often be of considerable practical importance for the purpose of associated decision–making, as well as for application in the context of further related judgements, as illustrated in the simplified example above.

Why Hempel's analysis is therefore not an option either

We have already noted that the options as to the construal of inductive/statistical inference as Hempel sees it do not appear to be intended to include interpreting the conclusion as an unqualified categorical statement of the hypothesis. However on reflection it is difficult to see how the position he *does* advocate on the initial question (regarding the construal of inductive *argument*)[138] can in fact be consistently separated from exactly this unacceptable option that he does not even consider entertaining. Let me explain. The trouble with Hempel's argument on this issue is that, like Kyburg (albeit in different respects) he also falls foul of the fallacy of changing the question over the course of his argument. Moreover a natural element of caution to this potential pitfall is conveniently invoked by the peculiarity that the *title* of the central section on his treatment of the question of the construal of inductive/statistical arguments is in fact a notably *different* question – namely the question of whether *probability* is a modal qualifier or a relation.[139] Now Hempel wants to argue that probability, at least in the context of inductive/statistical argument, is a relation.[140] Leaving aside for the moment whether we ought to agree with him on that, on the understanding that Hempel surely doesn't want to suggest that the conclusion of such an inference is properly to be construed as an unqualified categorical assertion of the hypothesis, we may then begin to wonder exactly how otherwise he *does* intend to construe the arguments in question, *wherein* he claims premises and hypothesis are connected by such a probability relation. If the conclusion is *not* to be construed as an unqualified statement of the hypothesis, as in Kyburg's misrepresentation of Hempel's intentions, illustrated above, what exactly is the conclusion of such arguments supposed to be? In order to answer

[138] Or at least on any tenable construal of the position he advocates.
[139] The actual title at this point is 'PROBABILITY: MODAL QUALIFIER OR RELATION?' (Hempel 1965, p57).
[140] In fact he concedes that there are uses of the term 'in ordinary discourse' where it *is* to be interpreted as qualifier, but doesn't think they are applicable to that employed in the arguments with which we are concerned. (Hempel, 1965, p61–2).

this question we may look for an explicit account of what Hempel ultimately takes to be the proper construal of these arguments. We find that Hempel *claims* to provide such an account at the top of p60:

> Thus, in analogy to (3.3), [141] the arguments which (2.2) was meant to represent might be schematized as follows:
>
> (3.4) 'a is not G' is almost certain (or: is highly probable) relative to the two statements 'a is F' and 'Less than 2 per cent of F's are G'. (Hempel, 1965, p60)

But the trouble with this is that (3.4) does not even remotely resemble a schematic construal of any form of *argument*. The sentence expressing (3.4) is a *single* (schematic) *statement*, and what it states is simply *that a certain probability relation holds* between certain other specified statements. How could Hempel possibly think that this is even a *candidate* schematisation for a particular type of argument? Clearly this *cannot* be the case. (3.4) is nothing more than a statement to the effect that a certain probability relation holds between two *mentioned* statements or propositions. That the respective propositions *are* merely mentioned and *not themselves stated* (or for that matter implied) is patently obvious from the logical form of the statement, and indeed is appropriately indicated by the standard practice of enclosure in quotation marks. Of course it is generally understood by those who want to interpret probability in the context of inductive argument as a relation that holds *between* premises and conclusion (as indeed it initially seemed that *Hempel* intended to argue) that the probability relation that thereby *connects* premises and conclusion is not itself the *entire* argument[142], since of course the premises and the conclusion are generally understood to be essential *components* of any argument, *regardless* of whether any purported probability relation does or does not hold between them. But, as we can see quite plainly, these respective propositions are not even *implicit* in (3.4).

Having apparently said, quite incredibly and without forewarning, that (3.4) *is* an (amended) schematization of the form of argument represented by (2.2), Hempel moves immediately back

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[141] In the analogous case, considered earlier by Hempel (1965, p58):
(3.3) 'a is G' is certain relative to (i.e. is logically implied by) 'a is F' and 'All F are G'
Hempel seems to acknowledge that this is an account of 'the logical relation between premises and conclusion of a deductive argument', expressing 'the logical force of the argument' (i.e. presumably the force of the implication connecting premises and conclusion) with the aid of a relativised interpretation of the term 'certain'. He does not at this point appear to suggest that (3.3) is or could be a proper construal of the corresponding deductive argument (as he implicitly suggests on p60). And of course it is in any case equally clear that to do so in this case would likewise be patently absurd.

[142] if indeed it is supposed to be any *part* of the argument at all, since of course the standard formal definition of an argument is that it consists in exactly the ordered pair of the premises and the conclusion.

to the less incredible talk of the surrounding context, again addressing 'The concept of probability here invoked' – as distinct from the target issue of the appropriate construal of the relevant type of *argument*. This might leave us wondering whether Hempel really meant to say what he just did. The trouble is of course that *if* in fact he only intended to say what he appears to be arguing for in the surrounding context – i.e. that (3.4) merely describes the probability relation that holds between premises and conclusion in a proper construal of argument (2.2), and that it would be inappropriate in this context to read the term 'probably' in the overt account of the conclusion as it seems to be represented in (2.2), i.e. as a *qualifier* of the hypothesis – then it would seem that this would imply that the proper construal of *the conclusion* in the kind of argument represented by (2.2) is that it is an *unqualified* categorical statement of the *hypothesis* (i.e. the unqualified statement that *a is not G*) – since it is difficult to see what alternative account of the conclusion there could possibly be. However, on moving ahead to his conclusion, we find (a) that Hempel is expressly resistant to such a suggestion (p.60–61), and (b) that in fact he seems to acknowledge, in contrast to what he appeared to claim above, that the schematisation (3.4) is 'concerned only with the logical connections between premises and conclusion'(p.60). In this case it would seem that we may after all conclude that Hempel did not *intend* what he said earlier, i.e. that he does *not* after all want to suggest that (3.4) is the proper construal of the schematic form of the argument represented by (2.2).[143] The pressing questions outstanding then are

(1) What exactly in the end does Hempel think *is* the proper construal of the schematic form of this kind of argument?, and

(2) How does he think this is compatible with the view that we should not interpret the conclusion of such arguments as an unqualified categorical statement of the hypothesis?

Regarding question (1) Hempel does not appear to be entirely clear or consistent on this matter. His concluding account of the situation is at best confused:

> In an inductive inference, ... even if the premises all belong to the class of statements previously accepted or possessed, the conclusion cannot be added to that class; it can only be qualified by a number representing its probability relative to the premises. In reference to inductive "inferences" or "arguments," therefore one can speak of a "conclusion" only *cum grano salis*: the conclusion cannot be detached from the premises and asserted on its own when the premises are true. (Hempel 1965 p61)

[143] However, the absurd interpretation seems to resurface again towards the end of Hempel's concluding passage, where he suggests that 'the various types of broadly statistical *syllogism* are accordingly

Thus it seems in the end that Hempel wants to talk about 'the conclusion' of an inductive inference, for example maintaining that it cannot be 'asserted on its own' – not at least 'when the premises are true', and yet seems at the same time to want to distance himself from the thought that we can indeed *properly* speak of the 'conclusion' at all (and consequently, it would seem, whether we can properly speak of the 'argument' and therefore the 'premises' either.) As a purported *clarification* of the proper logical construal of the *arguments* in question, Hempel's analysis, certainly as he ultimately presents it, would appear to be an abject failure. Nevertheless, let us look a little more closely at its apparent implications, and see what we can make of it.

It would *seem* that the proposition Hempel is *referring* to when he talks of 'the conclusion' here (even if he wants to question the legitimacy of its logical status in the context *as* a 'conclusion') is the hypothesis (or 'evidendum', e.g. in the case of (2.2) the proposition that 'a is G'), since it is apparent from the foregoing discussion that this is what is supposed to stand in the relevant probability relation to the premises. Now a proponent of the *original* account of the logical form of the argument as set out in (2.2) would of course want to agree with Hempel that the *unqualified* hypothesis cannot 'properly' be construed as having the logical status of the conclusion in this argument, but of course it is the question of what is the *proper* construal of the argument that is our concern – in which case the correct response to this is simply to acknowledge *unequivocally* that the unqualified hypothesis is *not* to be construed as the conclusion. The *wrong* response – which is the line that Hempel seems to find himself forced to take, is to try to say that this *is* the conclusion but *really* it isn't a conclusion at all. But of course once we take on board the correct response, that would only appear to support the proponent of the *original* schematization of (2.2), wherein the conclusion is a probabilistically *qualified* statement of the hypothesis, precisely because the premises don't warrant unqualified assertion of the hypothesis – *only* a probabilistic qualification of it – which latter moreover Hempel appears to *admit* in the above cited passage.[144]

To conclude this section then, it seems that Hempel has ultimately conflated the question 'What is the proper construal of *arguments* of the surface form of e.g. (2.2)?' with the relevant but quite distinct question 'What is the proper construal of the *probability relation* that holds between premises and hypothesis in (2.2)?', and this has ultimately led him to fail to be clear even about whether (2.2) is ultimately to be recognised as an argument at all, let alone about

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replaced by schemata of the kind suggested in (3.4)' (Hempel, 1965, p63. my emphases).
[144] albeit on the understanding that the qualification is *relative* to the premises.

what exactly is its proper logical form. However Hempel seems, at least at some points, to be reasonably clear that

(a) the unqualified hypothesis 'a is G' cannot properly be construed as the conclusion,

but that there is a probability relation which holds between premises and hypothesis so that

(b) the hypothesis *can* be *qualified* by its probability relative to the premises.

Regarding the issue of how this then relates to the proper construal of the type of *argument* in question, Hempel at best leaves a mystery, and at worst suggests, quite absurdly, that such arguments are in fact *not* arguments at all but merely (extremely) misleadingly formulated statements of the respective probability *relations* (such as (3.4)).[145]

The proper construal of the conclusion of statistical inferences

In what follows we may consider how, once we have acknowledged (a) and (b), we ought to construe the logical form of the *arguments* in question. We have no reason (and Hempel has given us no reason) to suppose that the premises explicitly stated in (2.2) are indeed genuine premises of this type of argument. Leaving aside for the moment the question of whether there might be any additional hidden premises that may rightly be taken to be involved in the inferences that are supposed to be explicated in such arguments, we may focus for the moment on the outstanding question of the proper logical form of the *conclusion*, and furthermore what place (whether that of the conclusion or any other) the probability *relation* to which Hempel appeals might actually take within the schematic structure of the *argument*. We have seen that both Hempel and the proponent of the original schematization (2.2) seem to agree that we *can* qualify the hypothesis by its *probability*, and it would seem that *some* such qualification would after all appear to be the most obvious, indeed the *only*, available candidate for the conclusion. The outstanding question we need to consider then is whether the probability qualification that attaches to the hypothesis in the conclusion should be interpreted as a simple (non–relative) qualification – basically of the form 'Probably (or with probability r) h' – as it is represented in the original schematization *or* as a more complex *relative* qualification, – basically of the form 'Relative to e, h is probable (or h has probability r)' – corresponding to the permissible probability qualification conceded by Hempel.

[145] This is absurd, for one reason, because it involves a denial that the sequence of statements that constitutes (2.2) 'properly interpreted' even implies the quite unproblematic and explicit statements that constitute its overt premises.

In order to approach this issue, the first point to note is that it is relatively easy to provide a plausible account of the generally intended *epistemic outcome* of my putting to you an argument of the form described in (2.2). The intended epistemic outcome in such a case is basically that having considered the bearing of the evidence on the hypothesis, you should realise that you (or, more generally 'we') ought rationally to regard the hypothesis with a high degree of belief (near certainty). Now it is clear from Hempel's discussion that he relies heavily on Carnap in his view that probability in the context of inductive inference should be understood as a relation determined purely by logic that holds between evidence statements and hypothesis. Of course Carnap's view of probability is now rarely regarded as viable,[146] but that need not concern us here. Taking for granted for the moment that we have in place a presumption of 'total evidence', as Hempel calls it (we shall turn to the significance of this shortly), so long as we think that there is *some* kind of probability relation that holds between evidence and hypothesis[147], and we think that when this is taken into account with our knowledge of the evidence there are implications for our epistemic attitude towards the hypothesis (as indeed Carnap does) then it is relatively easy to comprehend related concepts of both non–relative (unconditional) probability[148] and relative (conditional) probability, that allow us to make sense of the structure of such arguments as those represented by (2.2). In this regard, a comment made by Hempel himself on the significance of Carnap's requirement of total evidence is particularly helpful in elucidating the situation.

> Broadly speaking, we might say that according to this requirement, *the credence which it is rational to give to a statement* at a given time must be determined by the degree of conformation, or the logical probability, which the statement possesses on the total evidence available at the time.[149] (Hempel 1965 p64)

[146] It is notable that Swinburne (2001) has recently endorsed a concept of 'logical probability' that looks a lot like the logical conception advocated by Carnap, however while Swinburne thinks that there are objective a priori criteria for the determination of his 'logical probabilities', he does not insist that the respective criteria must be purely logical, and indeed in 2002 (p4, footnote 10) he admits that his choice of terminology in this regard is not 'fully satisfactory' and distances himself quite explicitly from any assumption that the value of every such probability is a 'truth of logic'.

[147] (whether in virtue of pure logic, or of some feature of the physical world, or of our own psychological make–up, or of some basic 'deontic laws' of epistemology, or as a matter of pragmatics).

[148] Even if Carnap himself was resistant to such a notion.

[149] Of course this ignores the complication of subject relativity, whereby the total evidence available to any particular *subject* at any given time will vary, but the common simplification is effectively to suppose that all parties to the argument share a common evidence base – or at least that all the relevant information is held by all parties and is as cited in the premises, in which case subject relativity may be ignored.

To put the point another way, with reference to the probability *relation* such as (3.4) earlier cited by Hempel,[150] and for simplicity dropping the time index (on the natural understanding that unless indicated otherwise the temporal context is simply determined by the present tense) we may say that *if we know that e* (and e is the total relevant information) *and* there is a probability relation such that *h is highly probable relative to e*, then *the credence which it is rational to give to h is high.* This clearly respects the central point made by Hempel that the latter must be *determined by* the respective logical probability, in the sense that it is *dependent* on it. But so long as Hempel insists on interpreting logical probability merely in terms of a logical *relation* as exemplified in (3.4) – a relation which does not for example imply e – that logical relation in itself does *not* make it rational to give a high degree of credence to h. It does so *only in conjunction with the supplementary information that e* (and that e is the only relevant information known). On this understanding of the situation it would seem that we should have no difficulty in making sense of a (present tense) concept of unconditional epistemic probability P(h) defined simply as the degree of credence it is rational to give to h,[151] as well as a related concept of conditional epistemic probability (functionally equivalent to Hempel's probability *relation*) P(h|e) defined as the degree of credence it *would* be rational to give to h *if* our total relevant information were e.

Regarding the principle of total evidence, Hempel goes along with Carnap in holding that this is a principle of the *methodology* of inductive reasoning, analogous to principles concerning the epistemological application of formal deductive logic (Hempel 1965 p65–6). That might seem to be a fair position to take if like Carnap you think that indeed there are formal purely logical relations that determine the probability of one proposition on a set of propositions. However it will not seem to be such a reasonable position to take if you think that *epistemological* rather than purely logical relations are fundamental to the determination of inductive probabilities. In any case our concern is with the *informal* logic of induction, i.e. principles concerning the appraisal of natural argument, understood basically as attempted explications of supposed justifications for holding certain epistemic attitudes towards hypotheses. And in this case epistemological beliefs, such as that 'This is all the relevant information we have', wherever operative – whether taken for granted or overtly expressed, will typically be essential to the proper explication of the supposed epistemic justification.

Thus it seems that we may salvage the following agreeable points from Carnap's ultimately unsatisfactory analysis of the proper construal of inductive statistical arguments

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[150] albeit without commitment as to whether the stated relation is determined, as Carnap claims, purely by logic.
[151] or perhaps, less ambiguously, the degree of credence that *ought* rationally to be given to h.

(1) There is a presumption of total evidence.

(2) The argument invokes a probability relation connecting evidence and hypothesis – a relation which determines *the degree of credence rationally to be given* to the hypothesis, and

(3) On the basis of the evidence, in virtue of the respective probability relation, the hypothesis can be qualified by its probability relative to the evidence.

And having noted a simple and natural distinction between conditional and unconditional epistemic probabilities, defined in accordance with the relation described above, it would seem that we can establish a satisfactory explication of the general form of the line of reasoning that will typically underlie the type of argument in question, in line with the thesis of epistemic deductivism, as follows:

An epistemic deductivist construal of statistical/inductive inference – first formulation

(1) a is F.

(2) The proportion of Fs that are G is less than 2 per cent.

(3) Our total evidence relevant to the hypothesis that a is not G is (1) and (2).

(4) If (3) then we ought to regard the hypothesis that a is not G with a high degree of credence.

Therefore;

(5) we ought to regard the hypothesis that a is not G with a high degree of credence.

Here (1) and (2) are the original explicit premises, and (3) is the presumption of total evidence. Strictly of course premises (1) and (2) are superfluous, in that all that is strictly necessary in this line of justification for the proposed credence for the hypothesis is that (1) and (2) are (at least for the sake of the argument) justly accorded credence 1 (which is clearly enough implicit in (3)). Moreover, depending on one's interpretation of the concept of evidence, one might even suppose that they are entailed by (3). However none of this particularly concerns me, since they are nevertheless typically explicit in actual arguments and they at least perform the practical function of providing a clear list of the points of evidence that need to be acknowledged in the (often implicit) presumption of total evidence (3), while their being perhaps more than is strictly essential to the key points of justification does not in any way detract form the capacity of the argument to perform its intended function. (4) may be regarded as representing the probability relation that Hempel rightly perceives to be central to the argument, but wrongly regards as a legitimate substitution for the argument. (5) clearly follows from (3) and (4) by a simple step of modus ponens, and since it coincides with what it would seem such arguments are typically intended to establish, as well as with the form of the original conclusion when the latter is interpreted as a statement of unconditional epistemic probability as described above, it seems most appropriate to regard this as the proper construal of the conclusion to arguments of this kind.

This accords with the standards of epistemic deductivism since (a) it properly sets out the key features of the line of epistemic justification that the argument is intended to elucidate, (b) its *logical* adequacy is beyond reproach in that the argument is clearly deductively valid, and (c) the success of the argument will rightly depend on the audience's view of the correctness of the *inferential* connection claim (4) explicitly or implicitly made by the arguer in putting the argument.[152] In our discussion of Goodman's new problem of induction, we will see further confirmation of the merits of the epistemic deductivist position in respect of interpreting what are traditionally regarded as inductive 'rules' of inference as premises, which need to be presented for evaluation – as indeed Hempel (1965, p78) suggests we might regard the purported probability relations such as (3.4), as represented by (4) in the above argument.

The trouble with the total evidence assumption

One of the benefits of the epistemic deductivist approach to argument construal, as we have noted, is that it forces us to make explicit (or at least satisfactorily clear) certain assumptions that are relevant to the justification that is supposed to be explicated, which might otherwise remain hidden (for example, as Hempel suggests with regard to the total evidence requirement, in the background of the 'methodology' within which the argument is framed.). One of the problems that can arise when we do this though is that on reflection we may sometimes come to realise that the respective assumptions are not quite so unproblematic as we might have initially supposed. This is a problem that is commonly noted with regard to the supposed presumption of total evidence.[153] Generally speaking any relatively concise specification of evidence in

[152] As I have explained earlier, when such a claim is not explicitly stated it will generally be implicit in the inferential connective e.g. 'Therefore' in the original (explicit) formulation of the argument, where it will need spelling out since it will not be the simple deductive connective, and cannot be assumed to be analytic (as can the deductive connective) so needs explication as a premise for evaluation.

[153] Pargetter and Bigelow (1997, p68) for example question whether we will in general be able to specify all the relevant evidence. Swinburne (2001, p63) also emphasises how our probability assignments must in principle be taken to be conditional a great deal of our background information, and the many philosophical discussions of the concept and criteria for relevance illustrate the considerable difficulty of being entirely specific about this.

support of a degree of belief for a hypothesis will strictly speaking *not* be all the relevant information we have.[154] Hempel suggests that

> credence may be determined by reference to any part of the total evidence that gives to the statement the same support or credence as the total evidence (Hempel 1965, p64)

But the problem is that it will be extremely difficult to specify any part of the evidence that actually *will* give the same support as the total evidence when detached from *all* the remaining background evidence.[155]

Fortunately though, the problem for the above account of the statistical inference is not particularly difficult to amend. For example we may replace the objectionable total information premise with a more agreeable *acknowledgment* (whether implicit or as here explicit, in parenthesis) of the bearing of our background information within the inferential connection claim, thus:

An epistemic deductivist construal of statistical/inductive inference – Revised formulation

(1) We know that a is F.
(2) We know that the proportion of Fs that are G is less than 2 per cent.
(3) If (1) and (2) then (in the light of our background information) we ought to regard the hypothesis that a is not G with a high degree of credence.

Therefore;

(4) we ought to regard the hypothesis that a is not G with a high degree of credence.

Hempel's and Kyburg's objections to a probabilistic construal of the conclusion.

Having illustrated how Hempel and Kyburg appear to have conflated two distinct questions while supposedly investigating the proper construal of inductive/statistical arguments, each ultimately arriving at a different but implausible opinion, and having defended an analysis in

[154] Moreover, Goodman's 'new riddle' of induction, which we shall examine in a later chapter, may be taken to indicate that background assumptions that commonly go unsaid, and which moreover, may be extraordinarily difficult to articulate, can be *very* relevant to the justification for the inductive inferences that we draw.

[155] If Lowe's account (1987 c.f. p331)of the *semantic* dependence of certain assumptions we make in inductive inference on our background evidence (or assumptions), or anything like it, is correct, then it might not even be possible to *make sense* of any significantly partial account of our evidence considered in detachment from the remaining background evidence (or assumptions).

line with the original schematisation in which the hypothesis is qualified by a probability operator in the conclusion, we may now turn our attention to their independent[156] objections to such a position. With reference to Cooley (1959) Hempel pointed out how different but related instances of arguments of the form (2.2), each with true premises can yield incompatible conclusions, for example:

> Peterson is a Swede.
> The proportion of Roman Catholic Swedes is less than 2 per cent.
> So, almost certainly, Peterson is not a Swede.
>
> Peterson made a pilgrimage to Lourdes.
> Less than 2 percent of those making a pilgrimage to Lourdes are not Roman Catholics.
> So, almost certainly, Peterson is a Roman Catholic.

Despite having noted in a footnote (fn.8, p.56) that Toulmin acknowledged the requirement of total evidence in advancing this form of argument, Hempel (1965, p57–8) then proceeds to conclude from these inconsistencies that construal of such arguments in this form is untenable. Of course, if we were to disregard the total evidence presumption, the situation would be no better were we to remove the probabilistic qualification from the hypothesis in the conclusion in each case, as Kyburg proposes, or replace the argument in each case by a corresponding probability relation as Hempel proposes. Whatever line we want to take, acknowledgement of the total evidence presumption (or something like it) is essential to the solution to this problem, and of course the crucial place of this presumption in the logic of the argument is rendered quite clear in the epistemic deductivist construal of the appropriate form of such arguments as set out above. On this construal the premise–sets of the corresponding instances, cannot both be all true. If the relevant information cited in each original version is indeed known, then the total information premises in the epistemic deductivist versions will be false in each case. In each of these cases the probabilistic qualification of the hypothesis in the conclusion is validly drawn, and in either case, if all the premises are true then the conclusion will be soundly drawn. Thus the problem cited does not constitute any objection against the probabilistic qualification of the hypothesis in the conclusion.

Moving on to Kyburg's objections (1990, p61–2), he presents four objections (supposedly) to the probabilistic model of inductive argument, but unfortunately the first three are entirely misdirected. Following his immediate misrepresentation of the original question (as described

[156] i.e. independent that is to their positive arguments in support of a different position, whose fallacies we

above) these objections are merely arguments in favour of the relatively innocuous (and quite compatible) thesis 'that our body of scientific *knowledge* consists of categorical empirical statements rather than modally modified ones' (Kyburg, 1990, p60). His fourth objection is however properly directed against 'the probabilistic treatment' and is expressed in two distinct ways. His first formulation of the objection is the relatively contentious claim that there is no *objective* source for probabilities. His second, less contentious, formulation is merely that that there is no *agreed–upon* source for the probabilities. Moreover, Kyburg then proceeds effectively to undermine his own objection by suggesting that, despite the lack of agreement so far on any 'compelling objective standards', nevertheless we should give the ideal of objectivity a run for its money and 'find out how far [it] can take us' (Kyburg, 1990, p62) – which is of course exactly what people (other than subjectivists) commonly do with regard to probability when they *argue* about probabilities. Perhaps there is no objective source for probabilities. Perhaps the probability connection claim (of the form of (4)) in every instance of the epistemic deductive expansion of a statistical syllogism is false. But in many cases people are naturally inclined to think it is true, and want to present an argument to the respective probabilistic conclusion. And even if Kyburg is right in suggesting that there are no objective probabilities, they have a right to argue their opinion. The epistemic deductivist construal of such arguments merely sets out the appropriate construal of their logical form, if they are to satisfactorily explicate the essential features of the represented line of reasoning. As far as the issue of objectivity is concerned, I shall discuss this matter further, in response to a recent formulation of the subjectivist objection to the frequentist approach to probability judgement presented by Philip Dawid, in a later chapter.

Justification for a degree of belief or degree of justification for belief?

In my analysis thus far I have interpreted the concept of (objective) epistemic probability in a way that would accord with the views of most who would want to call themselves objectivists with regard to epistemic probability, i.e. as a rational, or *justified, degree of belief* or credence. However Achinstein (2001) has recently proposed is an alternative approach to objective epistemic probability, and the difference between the two is analogous to the difference between the two approaches to probability discussed by Hempel in the context of the logical form of inductive arguments. In the latter, the issue was presented by Hempel as a contrast was between probability as a relation between a set of statements and an *unqualified* statement, and probability as a *qualified* statement. The contrast in this case is between probability as a

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have exposed.

justified *degree* of belief, and probability as a degree of justification for *belief* (unqualified). This latter position has been propounded recently by.

Achinstein proposes that probability be construed 'as a measure of *how reasonable* it is to believe a proposition' (2001, p95). However he also entertains a supplementary non–degree theoretic concept of reasonableness of belief:

> The degree to which it is reasonable to believe something may make it reasonable to believe that proposition. ... Accordingly, I would claim, reasonableness of belief is a threshold concept with respect to probability: some probability significantly greater than 0 is necessary for it to be reasonable to believe a proposition. But *degree* of reasonableness of belief is not a threshold concept with respect to probability (Achinstein, 2001, p98.)

For example, Achinstein suggests

> it is reasonable to degree ½ to believe that a fair coin will land heads, but it is not reasonable to believe that the coin will land heads since it is equally reasonable to believe it won't (Achinstein, 2001, p98.)

But if we know that it is *not* reasonable (either) to believe that the coin will land heads (or that it won't), the question naturally arises: what is the point of even entertaining the thought that it might nevertheless, in some sense, be *reasonable to degree ½* to believe the coin will land heads? When I want to decide whether or not to *believe* something, if I am rational, I will want to know (a) whether (granted the evidence) rationality demands that I *ought* to believe the proposition, and if that is not the case (b) whether rationally I *may* believe the proposition – in other words whether it is *reasonable to believe* the proposition. If it is *not reasonable* to believe the coin will land heads then presumably the implication of that is that I *shouldn't* believe it. That is exactly what I need to know – as far as the rational determination of my decision to *believe* or not to believe is concerned. Granted that this is the case, the supplementary information (whatever it might be taken to *mean*) that nevertheless it is *reasonable to degree ½* to believe it will land heads is entirely superfluous to my concerns, since as Achinstein appears to admit, it is nevertheless categorically *not* reasonable to *believe* the coin will land heads.

Let us continue with the point of agreement (conceded on our part for the sake of argument) that it is *not reasonable to believe* that the coin will land heads. In that case we may ask ourselves whether we ought (or may) hold some *lesser* degree of credence (than the threshold for categorical belief) that the coin will land heads. The answer we may suppose is that we may and indeed ought (on the supposition that the coin is fair and tossed randomly) to regard the proposition that it will land heads with *credence* of ½, where this is understood in (at least something like) the traditional (inter)subjectivist sense that ½ is the betting quotient we regard to be fair for that proposition. Similarly we may conclude that in this case it is *not reasonable* to hold a credence of 0.7 that the coin will land heads. Again, this kind of proposition of *categorical* rational permissibility or obligation is all we need to know, there is simply no theoretical or practical point in deliberating on some further question about whether it might nevertheless be, in some sense, *to some degree* rationally permissible to hold a credence of 0.7 that it will land heads. A statement of *partial* permission to do X – whatever it might be supposed to mean – simply fails to tell me what I want to know, which is whether or not I *may* do X. *Categorical* justification is what we need, and *all* that we need.[157]

Achinstein (2001, p101) poses himself the question 'What advantage accrues for characterizing degrees of reasonableness of belief as subject to the rules of probability?'.[158] But whether or not it is even *consistent* to suppose that degrees of reasonableness conform to the rules of probability cannot be determined without first providing some account of what degrees of reasonableness are supposed to *be*, along with some indication of what is supposed to determine their *values*. To emphasise this point, we may note that on a *natural* interpretation of 'degrees of reasonableness' if we were attempting to capture a natural relation between simple or categorical statements of reasonableness and everyday expressions that appear to pertain to degrees of variation in the reasonableness of belief, we might nevertheless (plausibly) suppose that, for any proposition P, *if* it is categorically *not* reasonable to believe that P (i.e. if the lower limit of the rationally permissible degrees of belief for P falls below the threshold for categorical belief) then the degree of reasonableness of believing that P is 0. We would assign non–zero degrees of reasonableness to belief that P whenever (and only when) the *rationally permissible* degrees of credence for P (on the evidence) *include* some value that falls above the threshold for categorical belief. The degree of reasonableness assigned would presumably be

[157] This is not meant to suggest that rationality cannot be *relative*. What it is rational for one to believe *might* vary with one's intellectual or even pragmatic values, while remaining categorical on whatever norms apply to the promotion of those values. For the sake of simplicity I am supposing that 'we' share at least near enough the same (and hopefully value–promoting) norms of rationality with respect to belief. The point in any case is that, whatever one's norms might be, belief of any given proposition, granted one's evidence, either will be permitted (either by direct implication or by default, i.e. by absence of any implication of prohibition) or will not be permitted on those norms. (In other words they will fall into one or other of these categories.)

[158] In fact he seems to think that it is an advantage simply if it is stipulated as a 'defining necessary condition' (Achinstein, 2001, p101). He also seems to think that it is not uncommon for a theory of probability simply to assert that the respective 'probabilities' are '*to be construed* as one's satisfying these rules', suggesting that the subjective and propensity theories also do this (p100–1). But in fact both types of theory typically provide basic definitions and offer more or less successful *arguments* in support of the *hypothesis* that they do, or at least should, conform to the probability calculus (as covered in most philosophical text books on probability theory).

some function of the short–fall (if any) of the upper limit of the permissible degrees of credence below absolute certainty (1) and the shortfall (if any) of the lower limit of the permissible degrees of credence below the threshold for categorical belief.

For example if the threshold for 'belief' is taken to be 0.90, and the permissible degrees of credence that P on the evidence range from 0.8 – 0.91, we may want to say that it is more reasonable to believe Q than it is to believe P, when the permissible degrees of credence for Q range from 0.89 – 0.99. This would of course constitute a much more complex view of reasonableness and it is debatable whether the advantages of such a theory would outweigh its complexity. The simpler point just that it *is* reasonable or permissible to believe either should suffice for most purposes, with specification of the respective permission ranges as potentially useful supplementary information. In any case though, there should be no suggestion that believing P is to some non–zero degree reasonable when the permissible degrees of belief for P do not include *any* that fall on or above the lower limit *necessary* for belief. For example if the rationally permissible degrees of credence for R, on the evidence, range from 0.70 – 0.80 (and 0.90 is taken to be the minimal threshold for belief) then the *degree of reasonableness* for *believing* R, on this criterion for belief, could not plausibly be regarded as anything other than zero.

Now it might well be that we choose to represent degrees of reasonableness, along the lines contemplated above, on the same *scale* as probabilities, i.e. on the unit interval 0 – 1. But not only would there be *no advantage* in characterising them as subject to the *rules* of probability, but such a characterisation would be inconsistent with the bulk of our intuitive judgements about rationally permissible degrees of credence. For example, there are many propositions which, in our evidential situation, we would normally regard as warranting *only* agnosticism – propositions for which it would simply *not* be reasonable to adopt an attitude of *categorical* belief either way. An ancient but still valid example is the proposition that the number of stars is even. In view of the absence of *any* relevant information, anyone who was genuinely convinced that the number of stars is even – anyone who really believed that with sufficient confidence to honestly assert the claim outright, without qualification, would be irresponsibly credulous. For such propositions your degree of credence *must*, for the sake of rationality, fall between the respective thresholds (or borders) for belief that P and belief that not–P.[159]

[159] That is not to say that the respective borders might not be vague. More basically the point is that for such propositions one's degree of credence must take some non–extreme intermediate value – i.e. a value that is not close to either 1 or 0.

The implication of this is that one's degree of *credence* for P, and for not–P, in such a case must *not* be zero. But, following the criteria suggested above, since *belief* for either of these is *not* (to any degree) permissible, the degree of reasonableness for *believing* P and for believing not–P in those circumstances *is* zero. By (a traditional subjectivist) definition your degree of credence that P is the betting quotient for P at which (if you had to take a chance one way or the other) you would be equally willing to bet either on or against P. In this case, if you *do* accept bets (either way) at quotients in accordance with your respective degrees of *reasonableness* for believing P and not–P (both valued at zero) it would not take a particularly crafty bookie to ensure that you are thoroughly fleeced. Thus, on this apparently plausible criterion for belief being *at all* reasonable (and granted a credence–threshold for categorical belief greater than zero) it would not even be *possible* for your degrees of reasonableness for belief to conform to the rules of probability.

This illustrates that we need not have any problem with entertaining the thought that there may be some relatively innocuous natural conception of 'degrees of reasonableness of belief', but that on such a *natural* conception they would not appear to be subject to the laws of probability. Of course it would be a mistake to suppose that Achinstein has such a natural conception in mind, particularly since he admits that

> In accordance with my epistemic view, it may be *very reasonable* to believe that [John's] symptoms will be relieved, ... *even if it is not particularly reasonable for you or anyone else* to believe this (Achinstein 2001, p99, my emphasis)

In the following I will argue that even despite Achinstein's stipulation that 'degrees of reasonableness of belief' obey the probability calculus, it would be a mistake to think that it could be a conception of *epistemic probability*, which may usefully be employed for example in betting situations, and yet not be equivalent to a concept of (rational) *degrees of credence*.

Achinstein notes that subjectivist probability theorists 'supply an a priori answer' to the question as to what advantage could be accrued from characterizing degrees of reasonableness of belief as subject to the rules of probability:

> If a system of bets is made in accordance with [betting quotients equal to] degrees of reasonableness of belief, *where these satisfy the probability rules*, then no system of bets based on these degrees is bound to lose (no "dutch book" is possible) (Achinstein, 2001, p101, my emphasis and clarificatory parenthesis.)

and agrees that indeed this is an advantage 'at least in typical betting situations'. The first problem with Achinstein's account of the subjectivist's claim here is that what he says *prior* to the explanatory parenthesis is not quite right – in other words the antecedent is not an accurate account of what it *is* to be invulnerable to a dutch book. And as we shall see, the respect in which it is inaccurate is important, and illustrates why simply characterising degrees of reasonableness of belief as *conforming to the probability calculus* is by no means sufficient for rationality – not even by the standards of the most ardent subjectivist.

The qualification omitted is simply that no system of bets based on these degrees is bound to lose – *whatever the outcome*. Of course to be fair to Achinstein he would no doubt intend this to be taken as read. But the fact is that there is a natural and more liberal sense of 'bound to lose', and a contrast with this illustrates the extreme minimality of the condition of mere invulnerability to a dutch book – even by subjectivist standards. For example, I may be quite certain that Lightning will not win the Derby, since I have just seen him shot following a bad fall at an early fence. In this scenario, if I risk anything on a late bet that Lightning *will* win, I am *bound to lose*. More importantly, from the point of view of the subjectivist – who defines probability, and judges the fairness of bets, on the basis of his *degrees of belief* – in this scenario, I may nevertheless, if I am stupid enough not to follow his prescriptions, accept just such a bet within a system of bets that *is in*vulnerable to a dutch book – i.e. which is not bound to lose *whatever* the outcome (i.e. even on the hypothesis that Lighting *does* win) and thereby accept a perfectly probabilistically coherent system of bets on which I am nevertheless *bound to lose*, and more significantly from the point of view of our concern for rationality, on which *I know* I am bound to lose. The subjectivist of course not only avoids vulnerability to a dutch book, but *also* ensures that he freely accepts a systems of bets only if he does not *believe* he is bound to lose – precisely because he *defines* the probabilities he associates with particular propositions, on which he decides his betting strategy, as exactly equal to his *degrees of belief* for the respective propositions, i.e. *the betting quotients that he perceives to be fair*. Only by *also* setting his 'degrees of reasonableness of belief' exactly equal to his *degrees of belief*, by the standard definition of the latter, can Achinstein match the advantage of the subjectivist approach to probability in meeting *both* of these minimal requirements of rationality.
The significant difference between the subjectivist and traditional objectivist about epistemic probability is that the latter believes that there are certain betting quotients that a subject with certain evidence *ought* (or ought not) to regard as fair, but of course he also accepts the subjectivist point that rationality dictates that these must satisfy the calculus, and that (if he is rational) his actual degrees of belief will in any case be equal to the degrees of belief he believes to be rational. In this case the traditional objectivist about epistemic probability can agree with

the subjectivist, that so long as he is rational, he will set his probabilities equal to his *degrees of belief* – in other words at the betting quotients he believes to be fair (i.e. to incur no advantage on either side). But if Achinstein wants to propose some novel variant on the concept of objective epistemic probability that *departs* from this venerable tradition, as we have seen, he will do so only at the cost of minimal standards of rationality. Of course Achinstein might be happy to concede that what he understands to be his 'degree of reasonableness for believing' a proposition is exactly what he perceives to be his fair betting quotient – in other words his (rational or otherwise) degree of belief – but then of course we would have little incentive to go along with his relatively peculiar and potentially misleading terminology.

PART THREE

OTHER PROBLEMS FOR INDUCTION–FRIENDLY DEDUCTIVISM

CHAPTER NINE

THE NEW RIDDLE OF INDUCTION (AND JACKSON'S SOLUTION OF IT)

The case of 'grue'

For the sake of our evaluations of inductive inferential connection claims, e.g. the particular projectibility claim 'If all of the (many) emeralds that have been observed have been green, then we may be sure that the next emerald that is observed will be green', we may hope to be able to appeal to acceptable principles of inductive inference, which (as with the examples we considered earlier in the case of deduction) may take the form of their respective generalisations – in this case – 'If all observed As have been B, then we may be sure that the next A that is observed will be B'. We have already noted the general line of Hume's problem for the *provability* of such claims, but noted that so long as it seems satisfactorily clear to us that such claims are true, we may nevertheless employ such them effectively in associated arguments. Unfortunately though things aren't quite so simple in this regard. As Goodman (1954) pointed out, such general principles of induction, as stated, turn out on reflection to appear quite *unacceptable*, citing for example the consequences of such principles of projectibility in the case of 'grue':

> x is grue iff *either* x is observed before t and x is green *or* (x is not observed before t and x is blue)

Goodman took the view that some candidate properties are unprojectible outright – i.e. irrespective of evidential context. But as Jackson (1975) has pointed out, it is possible to conceive of an evidential situation in which 'grue' in this sense would be projectible, while 'green' would not, so what we need to provide is an account of the relevant conditions on account of which 'grue' is not projectible *when* it is not. It is a merit of Jackson's solution that it accounts for such exceptional cases as well as those that arise with regard to our normal epistemic situation. However, for the sake of simplicity and practical relevance we may focus our attention on the normal situation where it seems obvious that 'grue' is not projectible. In order to clarify the situation we need first to consider carefully the precise implications of 'grue'.

From the surface structure of the definition it would appear that what is being defined is a *type* of predicate, namely predicates definable in the form specified above, admitting of various semantically distinct instances in each of which a particular date–value will be substituted for t. On this interpretation, as Jackson points out, any particular instance of the predicate type will either simply apply or not apply to any (enduring) object throughout the duration of its existence (unlike 'green' which may apply to an object at one time and not at another). Different instances of the grue type of predicate could be conveniently distinguished by indexing with the respective date, for example 'grue_{t1}' where t1 encodes a particular date that takes the place of t in the definition schema for that particular instance of the predicate type.

In the context it would appear that Goodman intends us to consider any hypothetical instance of the predicate type that we may construct by substitution of t by a date that is taken to represent the 'editorial present', or the time of reading for the reader, since he immediately invites us to consider the implications of grue 'at time t'. Subsequently 'grue' has more commonly been formulated by taking a value for t that, at the time of authorship (and anticipated readership), lies somewhere in the modestly distant future. However Goodman's approach of considering the present moment as the value of t has the advantage of posing an immediate problem for even the relatively modest next–case induction as well as for the more ambitious generalisation schema.

Let us consider for the sake of illustration of the problem the simplistic formula for next–case induction: 'If all As observed before t have been B, then we may reasonably predict that the next case of an A (observed at or after t) will be B'. We may of course note that various provisos need to borne in mind, for instance that we do not know that the next A to be observed at or after t will not be a B, that we have seen enough of them and so on. And, granted the understanding that such provisos are taken to be satisfied, we are generally happy to agree that under such circumstances it does seem reasonable (and moreover important to be able) to predict in line with the general schema. To highlight the point, and to satisfy the various other provisos on which we might for stringency wish to insist, we may consider the case of a huge urn containing millions of marbles, from which a sequence of several thousand marbles has been randomly selected – all of which turned out to be green, and where we have no prior or independent knowledge regarding the variety or proportions of the colours of the marbles in the urn.

In such circumstances we would generally agree that it will be reasonable to predict (with a high degree of confidence) that the next drawn marble to be observed at or after t will be green. The trouble is that even under such circumstances, we would also have to agree that all of the large random sample of marbles observed before t have been *grue*. In other words they have all satisfied the description '*either* observed before t and green *or* (not observed before t and blue)', by virtue of satisfying the first specified disjunct, in a context where sufficient supplementary provisos have been satisfied for us to endorse the qualified schema with considerable confidence. And yet the implication of even such a strongly qualified schema – granted its satisfaction with regard to 'grue' – yields the incompatible prediction that the next marble to be observed at or after t will be *blue*. Being observed *at or after* t, the first disjunct 'observed *before* t and green' will *not* apply to the next marble drawn, and so in order for 'grue' to apply, as predicted, the second specified disjunct will need to apply, i.e. the marble must be unobserved before t and blue.

If indeed, as we believe to be the case, the prediction that the next marble will be grue *cannot* reasonably be made with a high degree of confidence under these circumstances (which would appear to be a necessary condition for maintaining that the prediction that it will be green *can* reasonably be made) we should be able to offer some reason as to *why* it can't. Since our standard accounts of the usual provisos don't rule out the projectibility of grue under these circumstances – if our instincts about its unprojectibility are right – there must be some other proviso that is satisfied in cases such as this by the inference to the 'green' conclusion, but which fails to be satisfied by the inference for 'grue'.

Jackson's 'counterfactual condition' solution

Frank Jackson, in 1975, has identified just such a proviso, moreover one which is readily acknowledged with reference to everyday inferential situations. In order to appreciate the proviso one may first consider the point that there are *always* features common to all the members of sample that we know are *not* features of all (or sometimes any) members of the population outside the sample. This is a quite obvious and relatively trivial matter in many cases. For example, where we know that the sample of consists of all As *observed before t*, and the target population is *all* As (whether observed before t or not) and we know that the sample is a *proper* sample form the population of As (i.e. it doesn't contain all the As) it will clearly be the case that none of the population of As *outside* the sample will have been observed before t.

Of course, for such common features of the sample as this where it is *known* that not all the population have the particular sample property in question, that consideration in itself would defeat any inference to the conclusion that *all* the population have the respective feature. However, as Jackson points out, sometimes when it is not simply known that not all the population have the property in question, our knowledge that not all the population have some related sample property can nevertheless be defeatingly relevant to the projection of the property in question in a way that is not quite so immediately obvious from its surface structure. Jackson offers, among others, the following example:

> Every diamond I have observed has glinted in the light. Does this support the contention that the next diamond that I observe will glint in the light? Clearly yes. But suppose we add a detail to the story, namely that the next diamond that I observe is unpolished. Now all the diamonds I have observed so far have been polished, and, moreover, I know that they glint *because* they have been polished – that is, if the diamonds had not been polished, then they would not have glinted. It is clear that once we add this detail, it is no longer reasonable for me to regard it as likely that the next diamond I observe will glint in the light. (Jackson, 1975, p88)

The operative proviso in this type of case then may be generalised as follows:

> certain Fs which *are* H being G does not support other Fs which are *not* H being G if it is known that the Fs in the evidence class *would not have been G if they had not been H* (Jackson, 1975, p88, my emphases)

Expressed more positively, what is required[160] for certain Fs which are H being G to support other Fs which are *not* H being G, is the proviso that the Fs in the evidence class would *still* have been G if they had not been H. Jackson refers to this proviso as *the counterfactual condition* (for projectibility).

Consider the case of 'grue' in the context of our random drawings of marbles from the urn. First we may note that the next–case projectibility of 'grue' is unproblematic for the marbles observed prior to t. In these cases this is quite compatible with the fact that we also expect it to be green, since a marble's being observed prior to t and green is sufficient for its also being grue, in virtue of the first specified disjunct in our definition. The problem arises with regard to the question of projectibility across the temporal threshold (t) at which satisfaction of the second specified disjunct (unobserved prior to t and blue) becomes instead the necessary and sufficient condition for the applicability of 'grue' to marbles not observed earlier. This is a problem because the applicability of 'grue' in *these* cases is *not* compatible with the applicability of 'green', and so we cannot probabilistically consistently predict *both* outcomes each with a high degree of confidence.

[160] Note I do not say 'sufficient'.

So what *is* it that makes the projection of 'grue' in contrast to the projection of 'green' so utterly implausible in these circumstances? It would appear that Jackson has a simple and straightforward answer to this question. The reason why the projection of 'grue' is so utterly implausible is precisely because in these circumstances it contravenes the counterfactual condition for projectibility. As Jackson argues:

> We know that an emerald that is grue and examined [before t] would not have been grue if it had not been examined [before t]; for if it is grue and examined, it is green and examined ... [But] a green examined emerald would have been a green unexamined emerald [and so, not grue] if it had not been examined[161]
>
> (Jackson, 1975, p89.)

So, applying the general principle of the counterfactual condition or proviso on the projectibility of candidate properties, all the marbles observed before t being grue does not support other marbles *not* observed before t being grue, because we know that the marbles observed before t *would not have been* grue if they had *not* been observed before t.

As Jackson also argues (p89), the projection of 'green' in contrast does, at least initially, appear to survive the counterfactual condition. The prime candiadate for a potential problem is the worry that there might be an analogous defeating counterfactual with regard to green, i.e. 'If the marbles observed before t had not been observed before t, they would *not* have been *green*'. If *that* counterfactual were also known (or believed) to be true, under the evidential circumstances we are considering, then Jackson's counterfactual condition would also rule out the projectibility of 'green' in that evidential situation, and would thereby fail to provide us with a solution to the problem. But of course we *don't* know that that is true. On the contrary, our background belief about this counterfactual is that, in contrast to its 'grue' counterpart, it is false.

Jackson himself accepts and illustrates how we may conceive of *other* evidential situations where we do *not* believe this, and moreover where the projection of grue satisfies the counterfactual condition and is rational. In particular Jackson describes a possible situation where be believe that the greenness or otherwise of certain objects is affected by the observation methods we need to apply. But, the argument goes, in *our* evidential situation we

[161] As indicated in the first clause, Jackson is taking 'before t' for granted for simplicity of expression. In the unabridged account Jackson makes explicit the crucial condition implicit in the final clause, that the examined emeralds would still have been green if they had not been examined.

believe that the marbles in the evidence class, as with any other objects that actually are green before t, and are observed to be so, *would* still have been green even if they had not been observed. In this case, the counterfactual condition appears to provide an independently justified and effective criterion for distinguishing and explaining the typical situations in which 'grue' and predicates of similar semantic form are *not* projectible, from other imaginable situations in which, intuitively, they would be.

CHAPTER TEN
PROBLEMS FOR DEDUCTIVISM AND DEDUCTION

We have already dealt with Mill's extension of Hume's charge against inductive argument, that deductive argument in general begs the question. But there is another problem commonly associated with Hume's that is brought to bear against deductive principles of inference such as modus ponens (and which could likewise be raised even against specific inferential connection claims that are instances of modus ponens,) and that is the question of the justification for accepting such principles (or claims).

The justification of deduction

In his recent book *Hume's Problem*, Colin Howson addresses the issues raised by Hume's classical problem of induction, ultimately concluding that 'Hume was right'[162] – and that

> no theory of rationality that is not entirely question–begging can tell us what it is rational to believe about the future, whether based on what the past has displayed or not. (Howson, p239)

Howson argues that the principles of deduction are free from the circularity inherent in the use of inductive principles. With reference to Machover (1996, p121) Howson invokes a meta–logical theorem of deduction called 'Cut'. He then argues as follows:

> *Suppose* that some antecedent proof, using assumptions Σ, had established the soundness of modus ponens. ... 'Cut' tells us that there is a proof of the truth of B from Σ and S [the statement that A and 'If A then B' are true] alone. Moreover *this* proof need not use modus ponens. In other words there is a deductive justification for detaching the conclusion of a modus ponens inference which ... need not employ modus ponens at all. ... Thus there are *independent* arguments for the soundness of deductive rules; that is to say that there are rules which are not, as they are in the inductive case, circular.

But, if the thesis of epistemic deductivism is right, any well–formulated natural argument to the conclusion that modus ponens is deductively valid will itself be an instance of modus ponens.

[162] Howson does however defend a Bayesian position on the 'logic' of induction and argues positively that inductive inferences can be constructed in such a way that they are 'sound', in a sense *similar* to that of deductive validity. Fox (1999, p453) however conveniently illustrates how Bayesian inference can be reconstructed in line with the principles of epistemic deductivism in an argument to the pertinent epistemic conclusion that *is* deductively valid.

Of course Howson would presumably respond that that all we need do in that case is to appeal to formal arguments for our proof of modus ponens, as indeed he would seem to intend in the above defence of deduction. But the problem with such a suggestion is that purely formal proofs, which, certainly as far as Howson is concerned, may for example take the form of a diagram, don't *clarify* anything unless they are suitably *interpreted.* Once exactly what is supposed to be shown by the formal proof is fully set out *propositionally*, in the manner required for clarity, in accord with the theory of epistemic deductivism, we will end up with an argument whose final step of inference is an *instance* of modus ponens. Now I agree with Howson that circular arguments are epistemically impotent when it comes to supplying clarification of the justness of adopting a particular epistemic attitude towards a hypothesis when previously there was none, and so in view of the above I am inclined to take the view, in contrast to Howson, that there *is* an analogous problem for showing that modus ponens is valid to anyone who seriously doubts it. But, as with the case of induction, I don't think that this unprovability (in the natural sense of the term) is a particularly serious problem for the practice of logical reasoning in a world where few of us *have* any serious doubts about the validity of modus ponens.

Haack (1976)[163] agrees that there is also a problem in this regard for any attempt to justify deduction with the aid of deductive argument, but takes a different view to myself of the moral of the point, suggesting that:

> The moral of the paper might be put pessimistically, as that deduction is no less in need of justification than induction; or optimistically, as that induction is no more in need of justification than deduction. (Haack 1976, 118)

But I would disagree with the moral on both counts. I don't think deduction is in any serious need of justification (at least in the context of everyday reasoning, as distinct from the philosophy of logic) – its justness is pretty well obvious to most of us, even if it might be unprovable. Moreover I am nevertheless inclined to agree with Howson to *some* extent in his view that the situation is not *entirely* analogous with the case of induction, since there clearly are *more* serious questions to answer about the various inductive inferential connection claims that arguer's are inclined to invoke in the context of inductive inference. However as we have noted Jackson has shown us that there are answers to be had even with regard to what have seemed to many to be the most serious of these problems. (whether in an attempt to justify induction or otherwise) – not least the relative degree of disagreement. To conclude my defence

[163] For further discussion of this issue see Dummet's (1973, 290–318) and Haack's response (1982).

of epistemic deductivism though I shall briefly consider how problems of apparent inconsistency have even been raised against such a simple and apparently crucial principle of deduction as modus ponens, and how we might defend our theory of logical cogency in the face of such difficulties.

Illusory problems with deductivism: Catton and the significance of indefinable concepts.

Philip Catton (1999) claims to find certain novel problems with what he calls 'the deductivist image of scientific reasoning' – problems associated with our intuitive comprehension of theoretical concepts, and their resistance to formal definition. Catton claims that

> the deductivist image is challenged by cases of actual scientific reasoning, in which hard–to–state and thus discursively ill–defined elements of thought nonetheless significantly condition what practitioners accept as cogent argument. (Catton, 1999, p452).

He addresses his objections against the form of deductivism advocated by Musgrave[164], which Catton describes as the view that, when scientists reach conclusions that are not deductively valid implications of the premises they state, their arguments may be treated as enthymemes, i.e. arguments within which additional premises sufficient to secure logical validity have been left merely tacit. Catton claims to show that this position 'cannot be sustained' (p453). As it stands Catton's account is, to say the least, a misleading account of the deductivist position, and is certainly not representative of mine. While I may concur with Catton to the extent that a purely alethic form of deductivism would appear to be unrealistic, I do not accept that the purported problems to which Catton appeals here do indeed pose any significant challenge to deductivism.[165] On the contrary, I shall argue that the insights of deductivism provide the resources for overcoming the problems posed by intuitive and ill–defined concepts for determination of the logical adequacy of arguments involving them, and that the real challenge posed by such cases is in fact one for the anti–deductivist.

Preliminary doubts about Catton's claim

[164] Catton refers to Musgrave's 1981 (esp. 83–84), 1988 (esp. 237–239), and 1989.
[165] I do however accept that there are other, well–known, problems posed by intuitive and ill–defined concepts relating to the logical adequacy of deduction, and thereby relevant to the issue of deductivism, i.e. the problem of vagueness and sorites paradox. However Catton does not discuss this particular problem, and so I will limit my comments on this issue to a brief appendix on the logic of vagueness.

Supposing for the moment that there are indeed actual cases of the kind described by Catton, 'in which hard–to–state and thus discursively ill–defined elements of thought nonetheless significantly condition what practitioners accept as cogent argument', it is difficult to see *how* such cases would in any way pose a challenge to deductivism, since of course deductivism is a thesis about argument's that *are* cogent – not about arguments that might mistakenly be *believed* to be cogent as a result of ill–formulated lines of thought or inevitable shortfalls in our powers of comprehension or analysis. Furthermore it is a thesis about the cogency of properly identifiable arguments – not about the cogency of some elusive unspecifiable psychological entities – perhaps consisting in part of some *inarticulable intuitive elements of thought.* Psychologically we might make all sorts of useful (and harmful) inferences that, at least in practice, we are unable to explain or defend by the articulation of any actual *arguments* – cogent or otherwise. But the thesis of deductivism does not concern such inarticulable inferences. When a scientist is *unable* to satisfactorily or fully *state* his or her supposed rational justification[166] for the drawing of a conclusion, as Catton appears to maintain in the purported problem cases, then we simply do not have any complete or satisfactorily specified argument to *appraise*.

Having raised these preliminary doubts about the significance of Catton's central claim for the issue in question, let us then proceed to examine the details of Catton's objections to the deductivist position.

Catton's argument for the non–monotonicity of Euclid's Proofs

With regard to geometrical reasoning, Catton begins his argument with reference to Euclid's argument to Proposition One in Book I of the *Elements*, and claims that 'As a logical demonstration, Euclid's argument is not cogent.' (p460). Catton explains that this is so because counterexamples can be constructed to arguments of the same logical form as Euclid's. Of course deductivists need have no problem with that. Euclid made a bold and impressive attempt to *prove* certain geometrical propositions and, although his arguments for a long time *appeared* to be good, they were in fact fallacious, and (as Catton admits, p459–60) it was precisely developments in our understanding of *deductive* logic that helped to *enlighten* us on that. One up for deductivism one might think. But that is not the line that Catton wants us to take. Catton

[166] Note the distinction between *fully stating* the justification that is supposed to be conveyed by the argument, however deep that might go, and stating *the full justification* for the conclusion, in the sense

wants to argue that there is a 'latent *non–monotonicity*' in intuitive reasoning – which will apparently include any form of reasoning where we employ concepts that are *intuitively* understood, rather than formally defined. He attempts to justify this claim with reference to Euclid's intuitive geometrical arguments, and then to argue that this likewise applies in the case of contemporary mechanics and theoretical science in general since 'one cannot take for granted the meanings of theoretical terms in empirical sciences' (p466).

Now if, as Catton would have it, much or even most of the good reasoning in science is, in virtue of its reliance on intuitively understood thoughts and concepts, inherently *non–monotonic* it would seem that the deductivist must be mistaken, since deductively valid inferences must be monotonic. However, from the story thus far regarding the invalidity of Euclid's proofs, this would not appear to be the right analysis of the situation in respect of Euclid's arguments, since, in accord with Catton's own account of non–monotonicity (p461), an argument is non–monotonic if, given the premises it is *right* (i.e. rationally correct, or 'reasonable' in Catton's terminology) to infer the conclusion, but addition of further premises could make it *no longer* right to do so. However, this would not appear to be the case on the above account of Euclid's arguments, since the correct analysis in this case would appear to be that it was in fact a *mistake* to infer the conclusion on the basis of the considerations presented. Surely the fact of the matter is that it merely *seemed* to be a cogent argument in the first place – *not* that it *was* cogent in the first place – but a modern extension of it, with the addition of new information, is not.[167]

Nevertheless, we must give Catton's argument for his view a fair hearing. Let us then examine the argument he presents for his claim that

> there was a latent non–monotonicity in Euclid's reasonings which came to light only in the last two centuries or so and was fully disclosed only by Hilbert's work. (Catton, 1999, p462)

Catton's argument (abridged) goes as follows:

> Any reasoning that depends on discrimination of relevance relationships is non–monotonic. A prime example is reasoning which employs ceteris paribus or 'other things being equal' statements, and this depends on judgements concerning what is relevantly similar (equal) or

that implies there will be no further questions about the justification for the premises employed. It is the former that I intend here.

[167] On Catton' terminology ('reasonable' to infer) it might seem in a sense right to say that Euclid's inferences were eminently reasonable in his day, since he could not possibly be expected to appreciate the logical subtleties required to demonstrate that they were in fact fallacious – but that is a *psychological* sense of 'reasonable' that is not definitive of the concept of non–monotonicity. That is why I opt for the alternative term 'right', which has a less psychological ring to it.

> relevantly dissimilar (unequal) to what. Discerning relevance seems not a mechanical process: ... Our way of discerning relevance relationships seems to involve a generalised "feeling" we have for the kind of world we are set into. Such feelings involve intuitions which gather together into a unitary cognition certain features of ourselves and our world – features which resist discursive codification.
>
> For example, such undefined intuitive notions as "length," "breadth," and "evenly" get their content from gestures we make with our hands, things we do with blackboard diagrams, calculation we can make, and many other skills. ... So although we have an innate readiness to acquire the classical concept of length, the concept acquired nonetheless is open–textured and can be changed. ... With the addition of new things to think about – non–Euclidean geometries, more powerful conceptions of the continuum – the cogency of this reasoning was called into question. In effect, the addition of new premises undermined the inferences. Thus the reasoning was non–monotonic. (Catton 1999, p462–464)

Focusing on the pertinent points, this argument may be summarised thus:

(1) Discerning relevance is not a mechanical process: Perception of relevant similarities involves intuitive cognition of features of the world that resist discursive codification.
(2) Intuitive geometrical concepts are 'open–textured' and can be changed.
(3) Alternative geometrical theories have been developed in which the intuitive concepts employed in Euclid's arguments *have* been changed, and as result the cogency of Euclid's arguments has been called into question.
(4) In effect, Euclid's arguments have been undermined by the addition of new premises.
Therefore:
(5) Euclid's arguments were non–monotonic.

Now there is a fairly clear relation between (4) and Catton's conclusion (5). If we ignore for the moment the 'In effect' qualifier, from the supposition that Euclid's arguments *have* been undermined[168] *only* by the *addition* of new premises (not for example, even in part, by the rejection or modification of any of the original premises) it would indeed follow that Euclid's arguments were non–monotonic. But *is* this the effect of the situation described in (1) – (3). Clearly not. Catton spends considerable space developing and emphasising the point that the intuitive geometrical concepts employed in Euclid's proofs can and have been 'changed' and 'reshaped' with the effect that they are 'only in certain respects analogous' with the original

[168] in the sense relevant to Catton's account of non–monotonicity on p 461, an argument has been 'undermined' in this sense if the addition of more premises has made it unreasonable to infer the conclusion, while in the original case it *was* reasonable. For the sake of establishing additional objections to this argument of Catton's, we may ignore our earlier comment that it would seem more appropriate to conclude that Euclid's original arguments *weren't* inferentially correct in the first place.

intuitive concepts (p463–4). In this case the 'undermining' of the conclusions in the modern 'extensions' of the arguments is *not* achieved simply 'by the addition of new premises', but, on Catton's own account, by the *modification of the concepts* involved. In this case, since the original premises have been modified, premise (4) of Catton's argument is false, and consequently the argument does not go through.

Catton does not seem to realise that an argument's being 'undermined' by the addition of newly comprehended *logical* 'information', such as a clearer understanding of the logical implications of the concepts involved, as provided by Hilbert in the case of Euclid's arguments, does not make an argument non–monotonic. An initially apparently good argument being turned into a patently bad argument by the addition of purely *logical* information simply means that the argument was fallacious all along. Even by the standards the inductivist, it is generally understood that it is possible for a reasoner to misunderstand some aspect of the meanings, or logical implications, of the premises of an *inductive* argument, and *as a consequence* to misjudge their inductive implications, and thereby the cogency of the argument that draws some such erroneous inductive implication. Thus even for an inductivist, if we can be made to realise by the addition of purely *logical* considerations (clarifying the proper logical implications of the premises) that it would be a mistake to draw the original conclusion from the premises of the original apparently good inductive argument plus just the pertinent *logical* information, then the right conclusion to draw is that the original argument was fallacious all along. Misjudging the inferability of a conclusion from a stated set of assumptions on account of a failure to appreciate the bearing of pertinent *logical* matters, however complex, might well in retrospect be judged to be perfectly understandable. But it remains nonetheless a matter of *misjudgement* – not a matter of matter of difference in *inferability*.

Before proceeding to further considerations relating to the formalisation of theories, Catton briefly comments on one type of argument that is quite commonly regarded as inductive in nature. Despite the brevity of his comments on this case, it is worth attending to Catton's treatment of ceteris paribus reasoning, not only to highlight the problems with his general line of argument, but in order to illustrate the positive merits of deductivist analysis in this further test case of Catton's choosing.

The deductivist analysis of ceteris paribus reasoning

With regard to ceteris paribus reasoning, Catton sets up his argument against the deductivist approach with the following account of it:

> A proponent of the deductivist image of scientific inference will seek to reconstruct any reasonings whatsoever as deductively valid in form: For example, a deductivist will take the inference to 'B' from 'if A, then (ceteris paribus) B' and 'A', to involve the additional premise ceteris paribus – other things are equal. (Catton 1999, p464–5)

Before proceeding to consider Catton's objection to such an approach to the construal of ceteris paribus arguments, we should first pause to consider a couple of problems with Catton's initial claim here, and with his chosen representation of this type of inference. First it is altogether *false* to say that a deductivist 'will seek to reconstruct *any reasonings whatsoever* as deductively valid in form'. A deductivist has no reason whatsoever to construe inferences that are clearly inferentially unsound (on as broad a conception as you like) as deductively valid, or to insist on the inclusion of certain additional premises in an argument put forward by a scientist who insists that his conclusion does *not* depend an any such covert presumptions. A bad argument is as much a bad argument for a deductivist as it is for anyone else – even when a perfectly good argument can be readily constructed from it with a little modification.

What the deductivist *does* believe is that the underlying structure of any inference that is *cogent* may *authentically* be construed (i.e. without misrepresentation of the implicit or background beliefs of the inferer that are relevant to the inference) in a form that is deductively valid.[169] This is a bold claim. It is not immediately obvious, although I believe relatively simple considerations do establish that it is *true*. Whether or not it is *analytic*, as Catton rhetorically suggests (p465), is an interesting question, but it is not a crucial question. A basic methodological point of the deductivist approach is that where the cogency of an argument is not patently obvious to all parties from its *surface* structure (plus agreement about the stated premises) the question of its cogency can profitably be investigated by *probing* its deeper (and often, for the epistemic deductivist, its epistemic) structure. This involves in particular examination of whether there are any unexpressed additional assumptions (whether factual or epistemic) held by the arguer that, if *added* to the stated premises, would render clear (or at least overtly accessible) the purported justification for the conclusion in the argument.

Let us then consider how such an investigation might go in the type of case discussed here by Catton. First we may note that the typically *informal* type of argument mentioned here – the ceteris paribus argument – may naturally be expressed in various different formats, and Catton's

[169] My own elaboration on this as an *epistemic* deductivist, is that this will in many cases involve explication of whatever *epistemic* principles (or connections) are supposed by the arguer to facilitate the inference.

semi–formal parenthetic representation of it is just one possible (and, as we shall see, ill–formulated) construal. Before commenting on the detail of this construal, there is an important preliminary point that needs to be borne in mind when considering what might *appear* to be an argument of this kind. The case of ceteris paribus reasoning is particularly susceptible to the fallacy of argument construal whereby we can sometimes be inclined to 'read in' a supposedly silent (or enthymematic) *conclusion* where in fact none is intended. Sometimes we simply make ceteris paribus *claims* in the context of discussion of an issue, without thereby implying, or claiming further, that these considerations are *conclusive*. In such cases, having noted that B has been associated with A in certain standard or typical cases, and that A is so in the case in question, the intent of a *claim* that 'if A then *other things being equal* B' might for example simply be to make the case for an *investigation* as to whether there *are* any identifiable relevant differences in the case in question – and not all to propose (or exemplify) jumping to the *conclusion* that B is so. In such common cases, where no explicit conclusion (that *B*) is expressed (or intended) – merely a *ceteris paribus* claim, it would of course be a mistake to interpret such a sequence of connected claims as an inference to an implicit conclusion B.[170] Having clarified that, let us leave such non–conclusive cases aside, and consider the logical structure of cases where the categorical conclusion *is* (let us suppose explicitly) drawn, as indicated in Catton's representation of the inference.

First let us consider the significance of the fact that Catton places the ceteris paribus clause in the premises in which it occurs in brackets. Does Catton intend this to suggest that the ceteris paribus clause (or a synonym) might not be *explicit* in the premise, or perhaps that it is somehow not essential to its *significance*? Neither of these thoughts would seem to be plausible since of course the ceteris paribus clause is the *definitive* feature of a ceteris paribus inference, and indeed if it were *absent* from the surface structure of the inference – otherwise constructed as represented by Catton – we would in any case be left with an argument that is straightforwardly *deductively valid*, namely modus ponens (if A then *B*, A, therefore B) and there would be no need to appeal to *any* hidden assumptions, ceteris paribus or otherwise, in order to explicate its logical adequacy. Whatever Catton's intended significance (if any) for the brackets around the ceteris paribus clause then, we may regard this clause as an integral (and indeed generally explicit) part of the structure of the ceteris paribus inference, and so (as far as our analysis of its structure is concerned) ignore them.

Another preliminary point we need to note is the unclarity of the logical form of the ceteris paribus clause within Catton's representation of the relevant (stated) premise of the inference 'if

[170] although that is not to say that one might rightly regard the prior premises as the basis of a more

A, then *ceteris paribus* B'. By this I do not mean the difficulty in spelling out the precise *meaning* of 'other things being equal'. Rather my concern here is with the *logical* operator that applies directly to this clause. In the *detached* version (in the hypothetical additional premise) the ceteris paribus clause, according to Catton simply reads as a straightforward proposition 'Other things *are* equal' – no hidden conditionals or other funny business here. But what happens if we transpose the same simple interpretation back into the consequent of the relevant conditional, where Catton places the clause in the stated premise? To highlight the question symbolically, let us symbolise the straightforward ceteris paribus proposition 'Other things are equal' by 'E'. Then Catton's representation of the ceteris paribus premise comes out as 'if A, then *E* B' – which is nonsense. A simple way of restoring some sense to this would be to suppose that Catton intends an implicit *conjunction* to be operative here, thus 'if A, then E *and* B'. But then of course, with one or two further steps of straightforward deduction, we would again have another *valid* deduction to B from the *stated* premises. Furthermore this would not in any case appear to be a reasonable construal of the relevant premise in a typical ceteris paribus inference.

Clearly there is a significant question concerning the logical status of the ceteris paribus clause placed as it is in the stated conditional premise as depicted by Catton. Another problem for Catton's position on this issue, is that 'being' clauses within the antecedent of conditionals are commonly intended precisely to signal a relevant *assumption*. For example one might say 'If he has travel more than a few hundred yards, then *John being lazy* he will take the car.'. In cases such as this the 'being' clause within the surface consequent is sometimes most simply construed as a background assumption that grounds or explains the truncated conditional generated by it extraction, thus '*Because* John is lazy, if he has to travel more than a few hundred yards, he will take the car.' A subtly different, slightly more complicated, albeit surface–structure preserving interpretation would be to read the 'being' clause within the surface antecedent as reflecting a nested 'since', thus 'If he has to travel more than a few hundred yards, then *since* John is lazy, he will take the car', but in either case the 'because P, Q' or 'since P, Q' in the paraphrase reflects a *presumption* of the relevant proposition P, as well as an assumption of the implicit conditional '*if* P then Q'. Now in at least *some* cases I would contend, this *will* be the proper interpretation of an 'other things being equal' inference of the surface form depicted by Catton. For example imagine a pair of race–goers debating the outcome of a race. The proponent of the argument points out that in the 3.30 it just so happens that all the horses run best on the current going (firm), they are all in good health, their respective loads are roughly equal, and they have all had to travel similar distances to the

modest inference to the ceteris paribus claim.

racecourse. He then argues as follows: 'If Lightning has the superior form then, *other things being equal*, he will *win*. Lightning clearly *has* the superior form: He *will* win.'. In such cases, the ceteris paribus clause is in fact an *overt* expression of a *presumption* that other things are equal, and the inference is (relatively) straightforwardly valid – without any need for appeal to covert assumptions.

However we may reasonably suppose that not all cases of a ceteris paribus clause *expressed* in this form of conditional will be intended in this presumptive way (i.e. as partly indicative of a presumption that 'other things are equal'): the 'other things being equal' clause in an expression of the form 'if A, then other things being equal B' can of course, and often will be, intended in a purely *provisional* sense – as mentioned above with regard to the case where no conclusion is drawn. Furthermore, it may also be used in such a provisional sense in an inference where nevertheless the respective conclusion (B) *is* drawn. This is clearly the particular *type* of ceteris paribus inference that Catton has in mind. Bearing in mind the question we have raised regarding the logical construal of such a clause within such a conditional expression, let us then consider how this is to be spelled out on this *provisional* interpretation of the clause. Spelled out in natural English, the 'other things being equal' clause in such a conditional expression could be paraphrased as 'If A, then *providing* other things are equal B'. In this case it seems clear that in *this* type of ceteris paribus inference, the appropriate logical interpretation will be in terms – not of a nested 'since' or a prior 'because' – but in terms of a nested *conditional*, or more simply an additional *antecedent*, i.e. either as 'if A, then (*if* E then B)' or more simply as 'if A *and* E, then B'. But then the argument forms

> *If* A *and* other things are equal, then B
> A
> Therefore B.

> If A, then (*if* other things are equal, then B)
> A
> Therefore B.

as they stand are most obviously *not* cogent. As they stand, without any supplementary presumptions, such forms of argument would not rightly convince anyone in virtue of the rational merits of the *stated* line of reasoning. As far as the surface content of such arguments goes, they are clearly *unsatisfactory* lines of reasoning. Nor do you need to be a deductivist in order to appreciate this. To illustrate the latter point, it might be quite natural for someone to think (whether rightly or wrongly) that an argument of the following form is perfectly inferentially adequate as it stands, despite the fact that it is not deductively valid.

> All of the countless numbers of As that have ever been observed in the practically unlimited variety of circumstances in which they have been observed have been B.
> We have no reason to think that in this particular case it will be otherwise.
> *Therefore*, in this case, A will be B.

But more challenging cases for the deductivist such as this bear no comparison whatsoever with the natural and uncontroversial evaluation of the patently hopeless cases set out above. In fact such argument forms are so obviously unsatisfactory without supplementation that it is hard to imagine that anyone ever should seriously argue from either of *these* i.e. *expressly E–provisional* premises and A *alone* to an immediate and unqualified assertion of B. To help to illustrate the point if anyone should remain unconvinced, it is worth noting that simply by *expressing the provision 'providing* other things are equal' within the stated premise, you are effectively *expressing a belief* that the implication that B *is provisional* on other things being equal. To then proceed immediately to a categorical assertion of the conclusion that B, without any understanding that you are taking as granted, or at least for the sake of the argument supposing, that the *stated provision* is satisfied, would be patently absurd. This is nothing to do with some kind of deductivistic chauvinism, as Catton suggests. It is simply common sense.

In arguments of this surface structure then, where the categorical conclusion *is* drawn, it would indeed seem right to suppose *either* that there is an *unspoken* presumption or supposition that other things are equal, *or* that the 'other things being equal' clause within the relevant conditional is in fact intended – *not* in the provisional sense but in the *indicative* sense – i.e. as a paraphrase of '*since* other things are equal', thereby *overtly* implying that other things are equal, as discussed above. In *either* case the argument, rspectively construed, turns out to be deductively valid.

Catton's objection to the deductivist analysis of ceteris paribus reasoning

Having clarified and confirmed the deductivist analysis of the underlying structure of a ceteris paribus inference, let us now move on to consider Catton's *objection* to the view that this form of inference involves implicit appeal to a presumption that other things are equal. Catton's stated problem for such a construal of ceteris paribus arguments is brief and goes as follows:

> What it means to say that other things are equal, is for the present at least quite beyond us to say adequately. The whole question concerns how we reason not logically, but intuitively – geometrically, or analogically, or inductively, or ceteris paribus. It is not illuminating to answer this question with the suggestion that we simply build intuitive premises (like "other things are equal") in reasonings that are deductively valid. This is not illuminating *because it is quite unclear what these premises mean.* (Catton 1999, p465, my emphasis)

But that is hardly a satisfactory reason for claiming that there is no such implicit assumption, since the ceteris paribus clause in any case occurs in the *stated* premise 'If A then *ceteris paribus* B', and Catton could hardly deny that *this* premise exists! On this particular line of objection then we may rest our defence.

The problems of theory formalisation, and ill–defined concepts

However, Catton has another argument to offer against deductivism, and this concerns the axiomatisability of theories. In the case of mechanics for example Catton claims that 'In order for the deductivist image to fit mechanics, Hilbert's sixth problem (for mechanics) would need to be solved.' (Catton 1999, p452). Catton argues the point as follows:

> Hilbert's sixth problem challenged mathematicians to do for physics what had been done for the science of geometry. Success so far in answering this challenge has been *very* limited. As a consequence, it is not altogether clear what we are talking about in physics. It is difficult even to determine if we are saying the same thing a different way when we move from one formulation of a theory to an alternative one. ...
>
> A work such as Newton's *Principia* sets out a theory of mechanics much as Euclid set out his theory of geometry. The use of various key terms is regularized by means of definitions, which connect these key terms with one another and with further terms left intuitive and undefined. The meaning of these intuitive terms plays a significant role in shaping what reasoning is regarded as cogent. And so cogency (or what passes for such) does not amount simply to discursive deductive validity. To bring this about, I believe one would need to formalize, or discursively delimit, ideas left merely intuitive and informal in present formulations of the theory ... (which ... might not [prove possible] if, say, as in the case of arithmetic, mechanics is not first order axiomatizable) (Catton 1999, p467)

The first point to note in this regard, is that *argument deductivism* is by no means the same thing as *theory–formalism*, i.e. the view that all theories either are, or should in principle be, fully formalisable. Even the most ardent deductivist about *argument* might allow that theories may and, even perhaps should, permit a variety of interpretations of theoretical terms, some or even all of which bear some non–logical semantic content – so long as any specific *arguments* a scientist might offer are clearly deductively valid. In this case it would seem that Hilbert's problem for the formalisation of physics need *not* be solved, or even soluble, in order for argument deductivism to be right. Let us then examine Catton' argument to the contrary.

Catton's key premises in this argument is that 'The meaning of these [undefined or primitive] intuitive terms plays a significant role in shaping what reasoning is regarded as cogent.'. But there are a number of problems with this suggestion, particularly in the context of a debate about deductivism. For one thing anyone who is a formalist about inference (i.e. anyone who thinks that the only good arguments are those that are *formally* valid (relative to a specified system) would want to point out immediately that this will be a problem only for those misguided theorists who have failed to see the light in this regard. In any introductory text in logic, one of the first things we learn is that the formal validity of arguments does *not* depend on the meanings of any terms other than the *logical* operators. (And of course whether or not the *premises* of an argument, granted their intuitive meanings, are *true* or acceptable – which is another requirement of argument 'cogency', since Catton expresses his point with that term – is neither here nor there with regard to the debate between the deductivist and the anti–deductivist, since this is an issue about *logical*–adequacy, i.e. argument cogency *granted* the premises – not about premise–adequacy.) As regards Catton's perception of what is required for *solution* of the purported problem – even a deductivist who is a formalist about deduction would not accept that *formal definition of primitive terms* in a theory is necessary to secure determinability of the logical adequacy of arguments constructed in the context of that theory. Even from the viewpoint of such an extreme form of deductivism, Catton's perception of the demands of deductivism is way over the mark.

Semantic v formal deductivism

But what about those deductivists who don't take such an extreme position? Do we have any reason to regard Catton's claim as true? And if so is it a problem for us in the way that Catton suggests? The first question can clearly be answered in the affirmative: for a *semantic* deductivist the meanings of terms other than the logical operators may indeed, in some cases, have a bearing on the cogency of an argument. On this view, potential disagreement about the meanings of terms, or even plain misunderstanding, as well as elements of indeterminacy of meaning, may in fact result in disagreement or error or even indeterminacy about the logical adequacy of arguments. And of course the formalist about deduction will leap on these problems as indications of precisely the reason why we ought to be formalists. However, the situation in this regard is certainly no better for the *anti–deductivist* – and in particular for any of them who allow that the meanings of non–logical terms can be relevant to the cogency of *non–deductive* arguments. They will not only *share* the problems associated with determination of the cogency of any given *deductive* argument, but they will have an *additional* problem with regard to the bearing of disagreement, misunderstanding and indeterminacy with regard to the meanings of terms on the supposed *non–deductive implications* of the premises of an argument. This is not a problem for deductivism over against anti–deductivism; it is simply the shared problem of the difficulty of identifying the (non–formal) semantic implications of a set of premises when the meanings of non–logical terms in the premises are not entirely clear.

Fortunately, with regard to (semantic) *deductive* implications at least, there is a well–established method for constructing perfectly logically adequate arguments, even in such problem cases. Moreover it is a method that still does *not* demand that we provide full formal definitions for all the terms we employ in our arguments, as Catton suggests we would need to do. In order to illustrate the method in question we need only consider the simplest of text–book examples. Consider for instance the argument

> John is a bachelor.
> *Therefore*, John is male.

Most of us would accept that this argument is *semantically* valid, even though it is not formally valid. In other words, it is not possible for the conclusion to be false while the premise is true – and this is so in virtue of the *meaning* of the premise, but *not* in virtue of any pertinent features of the *logical form* of the argument – as it is stated. However a formally valid argument can easily be constructed that more clearly illustrates why the conclusion may be drawn on the basis of the material premise, and which does so without invoking anything other than trivial semantic truths which no–one would want to deny and whose relevance to the derivability of the conclusion no–one would want to deny. Thus:

> John is a bachelor.
> 'John is a bachelor' semantically implies 'John is male'.
> *Therefore*, John is male.

So how does this relate to the problem cases, where the problem is precisely that the semantic implications *aren't* so clear? In order to illustrate this let us consider a more contentious conclusion.[171]

> John is a bachelor.
>
> *Therefore*, John does not have a long–term, exclusive, live–in, female partner.

might not seem quite so clear cut. Some readers might think that this argument *is* semantically valid – that for any named individual 'x', 'x does not have a long–term, exclusive, live–in, female partner' is a semantic implication of 'x is a bachelor'. Others might dispute this. One might suppose that the current natural meaning of the term 'bachelor' is not entirely uncontroversial in this regard. In a 'discursively ill–defined' context such as this, is there any way that I, as a semantic deductivist, can satisfactorily express *my* argument to the stated conclusion in such a way as leave no question about the logical adequacy of my argument *and* satisfactorily set out what are the pertinent *assumptions* on the basis of which I believe this conclusion may be cogently drawn. In particular, granted, as we have already noted, that the issue at stake is about the bearing of meanings on *inferential* adequacy – can I do all this effectively if *I* think that the conclusion *does* follow in virtue of the meaning of the premise (rather than on account of some further material presumptions, such as say that there is a law banning bachelors from having live–in partners, and that John invariably obeys the law)? The answer is of course yes I can – and indeed I can do so quite easily. All we require for this is another simple informal *modal* argument, thus:

(1) John is a bachelor.

(2) 'John is a bachelor' semantically implies 'John does not have a long–term, exclusive, live–in, female partner'

Therefore;

(3) John does not have a long–term, exclusive, live–in, female partner.

Now, *despite* the debatability of the pertinent question regarding the natural or intuitive meaning of 'bachelor', and my admitted incapability of providing a full formal (and uncontroversial) *definition* for this term, you have a simple and straightforward argument that *is clearly semantically valid*, and which clearly sets out why (granted that we have initially previously established that we share the belief that John is a bachelor) *I* infer that John that does not have a long–term female partner. Now if you have an equally firm opinion otherwise on

[171] If you don't think this conclusion is contentious, you may wish to think of an argument whose

premise (2), and we cannot find any satisfactorily objective authority who might be able to adjudicate on the matter, it might well be the case that we are doomed to genuine disagreement about this – but that is a question about the premise–adequacy of the argument – not about its logical adequacy. The important thing, from the point of view of our concerns about the philosophy of *argument*, is that by formulating the argument in this uncontroversial deductive way I have managed to explain my line of *reasoning* to you perfectly clearly – without any distortion or misrepresentation. Moreover, I have managed to achieve this quite easily despite the debatability of the intuitive meaning of the crucial term whose meaning is central to the argument.

A point of further interest in this regard is that the simple (informal) modal argument presented could easily be expressed formally, by means of a logical implication operator (to be interpreted in the semantic sense) in the context of an appropriate modal logic. Moreover it is easy to see that *any* arguments based on such perceptions of logical or semantic implication might be simply and, as far as inferential adequacy is concerned, uncontroversially set out in such a way. So once again it might seem that perhaps the formalist has the upper hand in this matter. My own disagreement with the formalist in this regard is relatively trivial. I simply think that in cases of quite simple arguments that *are* clearly semantically valid, such as

> This is red.
> *Therefore*, this is coloured.

are (at least as far as inferential adequacy is concerned) *good enough* as they stand, in that they are, and are clearly, valid. The difference is trivial since in any case, as we have noted, such arguments *can* be reformulated, without injustice to the underlying structure of the inference, in such a way as to render them formally valid.[172]

The deductivist solution to the problem of conceptual unclarity

Of course the real debate between the deductivist and the anti–deductivist is centred on the treatment of *inductive* or *ampliative* inferences, rather than matters of theoretical *analysis*, such

semantic validity is questionable.

[172] Nor need this always involve appeal to modal operators. A formally valid reconstruction might be made simply by adding a perfectly unobjectionable non–modal premise that is effectively grounded by the respective semantic implication, such as 'Anything that is red is coloured.' or 'If this is red then it is coloured.'. Moreover, for anyone who was unfamiliar with the concept of semantic implication, such

as those on which Catton focuses, and in the case of ampliative inference, *whatever* the interpretation of the relevant evidence statements, the hypothesis will *not* be a semantically valid implication. As we have noted, even in cases where there is no question about the hypothesis being a semantic implication of the evidence statements the meaning of the evidence statements may still be relevant to the inferential adequacy of the argument. In cases where the precise meaning of the premises is unclear, there may also be cases where, even in the judgement of a committed inductivist, the cogency of an *inductive* argument is unclear *because* the meaning of the premise is imprecise or unclear. For example:

> All of the red mushrooms that have been eaten in the past have been poisonous.
> This mushroom is (slightly) reddish orange.
> *Therefore*, this mushroom will be poisonous.

This kind of unclarity (in this case the vagueness of the respective colour terms) is a problem for both the deductivist and the anti–deductivist, and may (at least in cases where the unclarity resides in the premises) be regarded as primarily a problem concerning *premise–adequacy*, rather than simply a problem of logical adequacy, since it is essentially a problematic feature of the *premises* – namely that *their meaning* is unclear or imprecise (in respects relevant to the inference) – that is the root of the problem. Clearly one of the basic criteria that we ought to demand for the adequacy of a set of premise statements when appraising an argument is whether their *meaning* is satisfactorily clear – at least in any respects that might be relevant, whether semantically or otherwise, to the inferability of the conclusion.

However, the same sort of interpretive problem might equally apply to unclarity in the conclusion. It might be the case that the meaning of the *conclusion* is imprecise or unclear in a respect that makes the cogency of the argument unclear (or inclines us to be mistaken about it). To illustrate the point suppose we have an ordered division of the spectrum into n relatively precise colour specifications $C_1 - C_n$, and that C_{117} designates a colour that lies somewhere between paradigm yellow and paradigm green, in the vicinity of the hazy borderline area of the extension of 'yellow'. Some people might be inclined to say that C_{117} *is* a shade of yellow, while others would be inclined to disagree, insisting that it would be quite misleading to describe it as yellow, and that it is at most a slightly yellowy shade of *green*. Nor there does not appear to be any official semantic legislation to which they might appeal in order to settle the matter. Consider then the inductive argument:

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non–modal connecting claims might be about as basic as the explanation gets as to the arguer's supposed

(CY) 999 balls have been randomly drawn (without replacement) from an urn initially containing 1000 balls, and all 999 of the sample balls are C_{117} in colour.
Therefore, the remaining ball in the urn is yellow.

For an inductivist who thinks C_{117} *is* (a shade of) yellow, (CY) as it stands might seem to be perfectly satisfactory argument. But even an inductivist who thinks it is *not* yellow will think that (CY) is *not* a satisfactory argument. In fact he or she might well think that the conclusion is rendered extremely *im*probable by the premises, since they appear to make it highly probable that the ball will be C_{117}. Perhaps an inductivist who regarded the question of whether C_{117} is yellow as indeterminate might regard it as indeterminate whether this argument is logically adequate as it stands, i.e. whether one would be right to infer the conclusion granted exactly the stated premises. In this case it would appear to be a problem, even from the point of view of inductivism, to determine whether the conclusion is a reasonable inductive inference, because the *conclusion* is vague in respects relevant to the judgement of whether it is inductively supported by the premise.[173]

Clearly the problem for the determination of argument cogency in the case of relevant points of unclarity or potential misunderstanding of key terms in the argument, is as much a problem for the anti–deductivist with regard to the inductive arguments he may want to construct with intuitive terminology, as Catton mistakenly thought it was in the cases where the deductivist wants to construct a sound deductive argument that employs intuitive or relatively ill–defined terminology. However, as we have seen, even in such difficult cases, it was well within the capability of the deductivist to express his *deductive* arguments in a form that is *clearly* logically adequate, *and* which constitutes an honest and authentic account of the pertinent points of his reasoning. To be fair, of course the anti–deductivist is equally free to employ the same deductive techniques in his analyses of *deductive* inferences involving ill–defined concepts. The *outstanding* problem with regard to the problem of determining the inferential adequacy of an argument when some of the terms employed are to some extent unclear or potentially misunderstood, in respects relevant to the inference, lie with arguments such as this that do not even appear to be intended to be deductive.

connection between the material premises of the argument and the conclusion.

[173] Depending on one's interpretation of the concept of vagueness, examples such as this might be taken to illustrate either of two types of problem of unclarity: For those who take the traditional view this is a matter of indeterminacy, sometimes paraphrased as that there is no fact of the matter. For those who take the epistemic position on vagueness (c.f. Williamson 1992 & 1994) there will be a fact of the matter but we simply have no way of knowing that fact.

The epistemic deductivist solution to the problem of conceptual unclarity in inductive inference

Catton does not discuss overtly inductive arguments in any detail – choosing to concentrate rather on failed or would–be mathematical proofs or axiomatisations, and logical formulations of theories. But while he may have been wrong about the enthymematic expandability of *ceteris paribus* inference, I think he is nevertheless right to a large extent in thinking that such simple (alethic) enthymematic analyses will not *always* be entirely plausible. For example it is my view, as an epistemist about (most) induction, that the traditional standard form of representation of inductive arguments in purely alethic terms,[174] is more often than not misleading – that inductive inferences may often more authentically be represented by *epistemic* arguments. We have seen in an earlier chapter how drawing out the pertinent epistemic features of an inference naturally leads us to some form of epistemic argument that is straightforwardly deductively valid, and which properly draws out all the pertinent beliefs of the arguer (whether factual or epistemic) for the scrutiny of his audience. It is the position of epistemic deductivism that when an inference is not appropriately represented simply as an alethic deduction, some such form of deductively valid epistemic elaboration will generally be appropriate to the proper interpretation of the argument. An interesting challenge for the epistemic deductivist then is whether this approach to argument analysis and evaluation can also provide a clearly logically adequate and authentic reconstruction even of such an interpretively problematic *inductive* inference as the one that poses the problems for the inductivist that we have outlined above. It is my view that this challenge may be met, and the logic of this particular case may be illustrated as follows.

A primary indication that an argument presented in the terms of (CY) might be an enthymematic argument expression is the absence of any explicit 'total evidence' assumption. On checking with the presenter of an argument expressed thus, we may expect to confirm that it is intended to be understood for example that it is not known prior to the draw that one of the balls placed in the urn is black, and more generally that there is a background epistemic assumption in operation that we have no further relevant information regarding the colour of the remaining ball. In this case, provided (contra Musgrave)[175] that there are no other material

[174] i.e. extracting the non–epistemic content of the account of the evidence and identifying these evidence statements with the *premises* of the argument and extracting the non–epistemic evidedndum from any associated epistemic packaging and identifying this with the *conclusion.*
[175] or rather, contra Musgrave according to Catton, whereby the deductivist claim is supposed to be that there are sufficient total assumptions to deductively valid infer the original conclusion, i.e. the evidedndum.

assumptions that have gone unsaid, we will have at least one enthymematic assumption, namely that we have no further (material) information relevant to the hypothesis or hypothesis that the remaining ball is yellow. However, as we have noted above, it is also more than likely that the presenter of this argument will take the view that C_{117} *is* yellow. And since this is a contentious semantic assumption – moreover one that is relevant to the inferability of the hypothesis – this is a semantic assumption that needs to be declared if the arguer is to succeed in clarifying to his audience the particular line of justification initially summarily expressed in the purely alethic form of (CY).

A further point of interpretation that needs to be addressed is the significance of the inferential connective 'Therefore' in the context of this particular argument. So long, as we are supposing, that we *don't* have a set of premises that deductively imply the hypothesis, it would seem that the interferential connection claim implicit here is not intended to be interpreted as a deductive implication connective. The implicit claim rather is that the hypothesis is somehow *inductively* infereable granted the premises. What *kind* of inductive support is supposed to be granted to the hypothesis in virtue of the epistemic assumption that the only relevant material information we have is as stated, plus the semantic assumption that C_{117} is a shade of yellow, is a question that needs to be put to the arguer if the argument is to be properly understood as intended. For the sake of the example we may suppose that the arguer in this case is implicitly claiming that *since* the only relevant material information we have is as stated, and C_{117} is a shade of yellow, we *ought* to regard the hypothesis with an extremely high *degree of belief*. Since the corresponding conditional implicit in this 'since' claim[176] is not (or at the very least it is contentious whether it is) a logical truth, it is an (inductivist) inferability *assumption* that ought not to go without explication in a proper elaboration of the underlying argument. The interesting thing to note then from a logical point of view – and the key point of epistemic deductivism – is that granted these epistemic, semantic, and inferential assumptions, the *epistemic* conclusion that the arguer would have us draw, namely that we *ought* to regard the hypothesis with *an extremely high degree of belief*, follows *deductively validly*. Thus we have an epistemic reformulation (CYE) of the original inductive argument (CY) which is clearly deductively valid:[177]

(CYE)

(Premise 1) The material information we have which is relevant to the hypothesis 'that the remaining ball in the urn is yellow' is exactly that 999 balls

[176] i.e. the conditional obtained by substituting 'if' for 'since'.

have been randomly drawn (without replacement) from an urn initially containing 1000 balls, and all 999 of the sample balls are C_{117} in colour.

(Premise 2) 'x is/are C_{117}' semantically implies 'x is/are yellow'.

(Premise 3) If (Premise 1) and (Premise 2) then we ought to regard the hypothesis 'that the remaining ball in the urn is yellow' with an extremely high degree of belief.

(Conclusion) We ought to regard the hypothesis 'that the remaining ball in the urn is yellow' with an extremely high degree of belief.

and so there need be no question arising with regard to the *logic* of the underlying epistemic argument thus properly elaborated.[178]

In conclusion then, the problems of interpretation for the determination of argument cogency to which Catton draws attention are *not*, as he suggests, a problem for the *deductivist* account of argument cogency. As we have observed, this problem of interpretation is fundamentally a problem of premise (and/or conclusion) adequacy – a problem which merely *obscures* the question of logical adequacy that is relevant to the issue at stake between deductivist and anti–deductivist. Furthermore, we have seen that this problem may be overcome with the aid of familiar tools of deduction, and, with the insights of epistemic deductivism, even in the case of overtly inductive inference, where simplistic alethic deductivism does not appear to be realistic. We have seen that the results yielded by the epistemic deductivist analysis are overt *clarity of logical adequacy* plus *authentic* representation of the assumptions (and conclusion) that constitute a satisfactory explication of the justification that the arguer aspires to satisfactorily present in the argument. The challenge is then turned against the *anti–deductivist*, and is the challenge of how he might propose to achieve the same results in the analysis of inductive (or any other non–deductive inferences) *without* resource to any such deductivist clarifications.

Vagueness, and the logical adequacy of classical deduction

[177] Since it is simply an instance of modus ponens.
[178] A further consequent merit of this formulation of the argument is that, despite the vagueness of key terminology and the debatability of premise 2, since it is deductively valid, it would seem at least to fulfil the condition of *premise and conclusion adequacy* noted above that the meanings of its constitutive statements are sufficiently clear in respects that are relevant to determination of its logical adequacy.

I do however acknowledge that there are other, well–known, and more difficult problems posed by intuitive and ill–defined concepts relating to the logical adequacy of deduction, and thereby relevant to the issue of deductivism, i.e. the problem of vagueness and sorites paradox, and I shall examine the significance of these problems below.

Intuitively understood, ill–defined, terms of the *vague* sort have long been understood to pose a challenge to the logical adequacy of deductive logic (and thereby one might suppose, to deductivism). The traditional problem in this regard is the sorites paradox, which in its classical formulation turns on the apparent implication of the vagueness of 'heap'[179] whereby the addition of one grain of sand does not make the difference between an accumulation of grains of sand that is not a heap and an accumulation that it is a heap. The argument then goes, from a set of premises all of which appear to be true, such as:

Sorites Paradox

(Premise 1)	One grain of sand is not a heap.
(Premise 2)	If one grain is not a heap then an accumulation of two grains is not a heap.
(Premise 3)	If an accumulation of two grains is not a heap then an accumulation of three grains is not a heap.
:	:
:	:
(Premise 10^8)	If an accumulation of $10^8 - 1$ grains is not a heap then an accumulation of 10^8 grains is not a heap.

by a simple sequence of steps of modus ponens, to the patently false conclusion that an accumulation of 10^8 grains of sand is not a heap. Thus we have a classically valid argument from a set of premises all of which appear to be true to a false conclusion.

Four of the main lines of approach to the logic of vagueness are the supervaluation approach, degrees of truth theory, the intuitionist approach[180], and the epistemic approach. On the supervaluation and epistemic approaches, it is claimed that the argument valid but one of the premises is false. The supervaluationist says that it is indeterminate *which* premise is false, while the claim on the epistemic approach is that there is a fact of the matter which premises is

[179] 'Sorites' is derived from 'soros' the Greek for 'heap'. Other popular formulations exploit such vague terms as 'bald', 'tall', and 'yellow'.

false, but we have no way of *knowing* which it is. The intuitionist also maintains that the argument is valid, but declines to suggest that one of the premises is false, and instead employs a deviant logic which permits him to say that it is false that all the premises are true, while nevertheless claiming that this does not imply that there is a false premise. On the degree theoretic view, when vague terminology is in operation the truth or falsity of the propositions so constructed is not an all or nothing affair. Truth values may vary, even by the slightest of degrees, between the limits of definite or whole truth (val 1) and definite or whole (or 'absolute') falsity (val 0). In this case, while the sorites argument remains valid on a sympathetic interpretation of the classical sense, insofar as it guarantees preservation of simple or *absolute* truth, the logic of degrees of truth explains how an argument of such form, when it contains many premises which are extremely highly, but nevertheless short of absolutely, true (as it does in the sorites case) may nevertheless lead to a conclusion whose truth value is (much) lower than that of the lowest valued premise.[181]

A useful introductory reading and bibliography can be found in Sainsbury and Williamson (1997), and Keefe and Smith (1996) is a good collection of readings. Williamson endeavours to defend the intuitively implausible epistemic position in his (1992 and 1994). For a comparison of the supervaluation approach with degree theory, see Sainsbury (1988, Ch.2), and for an exchange on the relative merits of degree theory against intuitionism see my Rea (1989) and the response of Schwartz (1990). I shall not discuss the relative merits of these alternative approaches in any detail here, except to make one or two comments in further defence of my own favoured view, which is the degree theory.

It is an important merit of the degree theory that this is not an ad hoc suggestion, but a view that is grounded on consideration of the pragmatics of vague language use. Whether or not a predicate 'applies' to a subject – and thereby whether or not the subject/predicate statement is true – depends fundamentally on the conventions relating to the *use* of the predicate – and a basic indicator of this is whether competent language users would acknowledge that the predicate applies to the subject. But of course where vague predicates are concerned there is significant *variation* as to the perception of their applicability in different cases. In some cases we might be quite certain about the applicability of the predicate, while in other cases we may be more hesitant or doubtful about their applicability. Such variation in inclination can range

[180] The approach I refer to here is Putnam's proposal for applying intuitionist logic to sorites paradoxes, Putnam (1983).

[181] Whether or not one might want to say that it is thereby degree–theoretically 'invalid' in respect of that will depend on one's intuitive understanding of the central significance of the concept of validity. Edgington for instance suggests we mark the distinction between validity and invalidity with respect to

between absolute certainty about the applicability of the predicate, and absolute certainty that the predicate does not apply. The degree theorist, unlike the epistemic theorist, sees this not as simple uncertainty about a perfectly determinate matter of fact, but as a symptom of normal variation in the ongoing grounding *use* of the predicate (and its negation) in application to gradually differing subjects, effectively yielding variation in *applicability*, and thereby of the *truth* of the respective subject/predicate statements, that is mirrored in a responsive attitude akin to (simple) uncertainty, and roughly measured in a similar manner, on a scale from 0, representing absolute falsity to 1, absolute truth).[182] Complex propositions are then evaluated on the basis of (intendedly) intuitive expansions of the traditional truth functions, which agree with the traditional truth functions in the limiting case where only absolute values are admitted.[183]

Edgington (1992, p 203) suggests that 'the Sorites paradox and the Lottery paradox are *structurally* the same phenomena'. But this claim, in the context of a discussion of logic, could be misleading.[184] The epistemic theorist, supervaluationist, and the intuitionist would certainly like to stress their structural similarity in certain logical respects: the epistemic theorist and supervaluationist both want to claim that one of the premises is false (as in the case of the lottery 'paradox' – one of the tickets *will* win) while the intuitionist wants to deny that all the premises are true (as one must do in the case of the lottery paradox). But in the view of the degree theorist it is key *differences* in their logical structure that are crucial to a proper diagnosis of the sorites problem. The degree theorist is surely right in insisting that while the above logical features clearly hold in the case of the lottery, they do *not* appear to hold in the case of the sorites. The particular and distinguishing problem in the case of the sorites is that it would appear that *all* the premises are true. And it is only the degree theory that does justice to this crucial feature of the sorites argument, since the degree theory explains how all of the premises are *extremely* (if not all absolutely) true, and yet the conclusion is quite false.

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whether the argument form makes possible a shortfall in the truth value of the conclusion that is not attributable to truth value deficits in the premises.

[182] I have said more about this in an early unpublished paper (Rea,1992) where I discussed the transferability of the concept of 'partial grounding', as developed by Devitt and Sterelney (1987, p62–3) in association with the process of reference change, to the pragmatics of predicate vagueness.

[183] Sainsbury and Williamson (1996, p476) cite the standard approach which I also endorsed in (1989). On this approach:

$$\begin{aligned}
[\sim P] &= 1 - P \\
[P\&Q] &= \min\{[P], [Q]\} \\
[PvQ] &= \max\{[P], [Q]\} \\
[P \rightarrow Q] &= 1 - ([P] - [Q]) \text{ if } [P] > [Q] \\
&= 1 \text{ otherwise.}
\end{aligned}$$

[184] It is reasonably clear that the similarity to which Edgington refers here relates to a comparison between the epistemic structure of the lottery argument and the logical structure of the sorites argument. Nevertheless the idea that they are the same in respect of their *logical* structure is a mistaken idea that opponents would like to exploit.

Degrees of truth versus intuitionism

The intuitionist tries to salvage some respectability in this regard by maintaining that although he denies that all the premises are true he does not thereby imply that one of them is not true.[185] In intuitionist logic (~P v ~Q) does not follow from ~(P&Q). I have argued (Rea, 1989), that such an approach to logic is hopelessly too broad[186] since it effectively renders the coercive power of logic impotent. Faced with a conclusion he wishes to avoid, in an argument which seems perfectly valid, and for which it seems that not one of the premises is untrue, the intuitionist need feel under no pressure at all to reconsider his prior opinion of the conclusion. As far as the intuitionist is concerned, he does not need to deny the apparent truth that none of the premises are untrue, or that the argument is valid. In his system, these logical features of an argument are perfectly compatible with the conclusion's being false, since none of the premises being untrue is intuitionistically compatible with not all of the premises being true.

Schwartz (1990, p44–5) in his response tries to argue that this apparent 'free for all escape route' from any such classically pressing arguments is in fact 'nothing peculiar to the intuitionist proposal' and that 'the classicist and the intuitionist are about on a par' in this respect. His first line of defence for this claim is that

> if we believe of an argument that it is valid and that its conclusion is false, we are rationally committed to disbelieving the conjunction of the premises. This is not an escape route, it is just logic. (Schwartz, 1990, p44)

But of course things are not quite that simple in the more detailed and problematic context we are considering i.e. in the case of an *apparently good* argument, where (in common with the paradoxical case of the sorites) 'it also seems to be the case that all the premises are true' (Rea, 1989, p31). Since he does not believe that one of the premises may be false, the classicist thereby feels unable to deny that all the premises are true. In such a scenario the classicist is under logical pressure to rethink his prior opinion of the conclusion. He may of course seek the explanation of the problem elsewhere,[187] but the crucial point is that if he can find no fault with

[185] See Schwartz (1987).
[186] Here I use Haack's terminology, (1978) p139 'Further requirements concern the scope of a solution; it should not be so broad as to cripple the reasoning we want to keep'.
[187] As of course one is forced to do when faced with a paradox such as the sorites where acceptance of the conclusion, like denial of the premises, and like denial of validity, does not even appear to be an option. But of course the general point about a 'free for all escape route' when faced with an 'apparently good' argument relates more broadly to the context of argument in general, rather than paradox, where

the validity of the argument and he cannot bring himself to believe that *one* of the premises is false, then he will be under considerable logical pressure to reconsider his evaluation of the conclusion. In the same situation however, the intuitionist will *not* be under any such logical pressure, since even if he cannot bring himself to believe that (at least) one of the premises is false, he remains logically free to deny the conjunction of the premises, and thereby free to deny the conclusion. *That* is the logical loop–hole that the intuitionist is free to exploit in order to get himself off the hook whenever he wishes to avoid acceptance of the conclusion of an apparently good argument.[188]

Schwartz's second line of defence for his claim that the classicist and the intuitionist are actually 'on a par' in respect of this apparent difference in logical constraints goes as follows:

> It may seem that the intuitionist is getting away with something when he says that the conjunction of the premisses is false, but is unwilling to deny any of the conjuncts. Classical logic, in effect allows the same thing however. ... if a classicist rejects a conjunction, he must hold that at least one conjunct is false but he need have no beliefs about *which* conjunct is false. So the classicist and the intuitionist are about on a par with respect to 'free for all escape routes'. The intuitionist can say that the conjunction is false but refuse to deny any particular conjunct. The classicist as well can say that the conjunction is false but refuse to deny any particular conjunct. Schwartz (1990, p45, my emphasis)

But of course it is quite obvious that this *common ground* between the classicist and the intuitionist is *not* the logical *difference* between the classicist and the intuitionist that Schwartz carefully sets out and invokes in his account of Putnam's proposed escape route from the sorites argument (Schwartz 1987). The key difference between the lottery 'paradox' and the sorites neatly illustrates the irrelevance of this point in the kind of case in question where we have an apparently good argument in which it seems that not one of the premises is false. In the case of the lottery paradox it is patently obvious that *one* of the premises is false, although we cannot possibly know in advance *which* premise is false. The lottery argument does not even appear to meet the basic requirements of a cogent argument.[189] The problem when we have an apparently cogent argument is of course that it does *not* seem to be the case that one of the premises may be false. In such a case this *poses a logical barrier* to the classicist who might have *liked* to

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acceptance of the conclusion is at least a *conceivable* option for the disputants, even in the face of initial conviction that it is false.

[188] It does of course seem somewhat disingenuous of Schwartz to refuse to acknowledge this crucial *distinction* between the respective logical constraints faced by the classicist and the intuitionist in his response, when the primary point of his initial article was precisely to *clarify* that distinction in order to illustrate how it is *exploited* in the case of the sorites.

have been able to say that the conjunction of the premises is false (in order to maintain a denial of the conclusion), since for the classicist the latter implies the apparent falsehood that one of the premises *is* false. Schwartz's key point, as set out in his (1987), was precisely that the intuitionist in contrast *can* simply deny the conjunction of the premises of an argument *without* thereby committing himself to the apparent falsehood that one of the premises is false. What is more, in the same paper, Schwartz was perfectly clear in explicitly *distinguishing* this crucial *difference* between the classicist and the intuitionist from the *common* ground between them whereby neither the classicist nor the intuitionist needs to be able to say *which* premise is false:

> Here if anywhere is a significant difference between the intuitionist and the classicist. If the classicist denies a conjunction then he is committed to asserting that at least one of the conjuncts is false. .... *Of course he need not* be able to say which of the premises is false, *but he must hold that one is false.* (Schwartz, 1987, p182, my emphasis)

Nor did he appear to have any doubts that this diminishment of the requirements for the denial of the conclusion of a valid argument supplies the intuitionist with a *relatively easy* escape route from arguments with conclusions we have previously judged false, where nevertheless we cannot plausibly commit ourselves to the apparent falsehood that there is a false premise (such as in the case the sorites).

> Thus when the classicist denies the conclusion ... he is committed to denying one of the premises, in the face of all the difficulties of such a denial. .... The intuitionist seems to be able to avoid this unwanted consequence because he refuses to deny any premise (he just denies the conjunction of the premises) (Schwartz, 1987, p182)

Whatever lengths Swartz might go to in his attempt to deny that the intuitionist *has* a peculiar logical escape route, not open to the classicist, from unwanted conclusions (and he spends an entire page pushing the arguments cited above in support of such a denial) there is ultimately no escaping his earlier detailed *clarification* of the precise nature of such an escape route, and of Putnam's proposal for its *deployment* in the case of the sorites.

It would be a less absurd retort (if not a defence) to say (as Schwartz does not) that some versions of *degree theory* also allow that a conjunction may be false without implying that one of the conjuncts is false,[190] (although as I indicated in Rea 1989, p32, it is not such a version that I endorse). But even if one were to take that line, as I was careful to emphasise in Rea (1989,

[189] The sense in which it is a 'paradox' may be regarded as relating, for one thing, to the challenge it poses to intuitive probabilistic criteria for acceptability.

p32), the degree theorist is able to make more fine–tuned evaluations of the truth values of the premises of arguments employing vague terminology than are available to non–degree theorists, and thereby judge *when* these threaten to undermine the truth value of the conclusion. The same point would hold even if one took the view that the latter takes place via disvaluation of the conjunction of the premises. So even then such a measured response to the sorites argument in the context of the degree theory would not open up a 'free for all escape route' as the intuitionist proposal seems to do, since the latter does not appear to be accompanied by any such crucial discriminatory constraints. On the intuitionist proposal whether or not it seems implausible to suppose that one of the premises is false appears to be *logically irrelevant* to your right to deny the *conjunction* of the premises (and thence to decline to draw the conclusion) of an apparently valid argument. If your *prior or independent* opinion of the conclusion of a valid argument is that it is false, then even if (as in the case of the sorites) it also seems *equally or even more* implausible to suppose that even *one* of the premises may be untrue, you are in effect free to *ignore* the argument, since the latter poses *no logical barrier* to your nevertheless denying the *conjunction* of the premises (on the basis of your independent opinion that the validly drawn conclusion is false) and thereby disposing of the argument.

Of course an intuitionist might like to try to find some basis for rejecting my suggestion that this apparent logical loop–hole in the intuitionist's system might be exploited *whenever* he wants to maintain a denial of the conclusion of an apparently valid argument for which it seems implausible to suppose that one of the premises is false – in other words to counter my suggestion that this escape route is indeed a '*free for all*' escape route from *any* such classically pressing arguments. Unfortunately Schwartz in his response does not even purport to pursue such a line, opting instead to take the absurd line of denial that the intuitionist *has* any peculiar escape route at his disposal.

The excessive precision objection

Schwartz also objects that the degree theorist, in assigning specific degrees of truth to the component sentences of the sorites argument, fails to 'take vagueness at face value' complaining that 'this is the most refined and unbelievable precision'.[191]. He also suggests in a footnote that even if degree theorists allow vague degrees of truth 'it is hard to understand their definitions of the logical functions' since the arithmetic functions involved are 'not defined for

[190] As for example in the version advocated by Michael Clark in (1997).

vague quantities' (Schwartz 1990, p46). However degree theorists who discuss this issue commonly acknowledge that precise specifications of assignments will of course be somewhat arbitrary, and that theoretical precisification is simply a feature of the common practice of idealisation in the mathematical modelling of real variables. Theorists who take this view typically and rightly cite the analogue of mathematical treatments of the *probability* of propositions. Few probability theorists would want to insist that it is not to some extent an arbitrary decision when the probabilities of particular propositions are assigned an absolutely precise number in the real unit interval, or even for that matter, when perfectly precise values are assigned to common physical quantities. But no–one would want to suggest that the arithmetic functions are not applicable to such variables. [192]

In practice of course, as with the case of probabilities, it is generally the qualitative picture that matters. But for the purpose of mathematical application we typically represent relatively vague quantities by suitable precisifications. For most practical purposes it does not matter whether a proposition is *absolutely* certain or merely *extremely* certain. Likewise with degrees of truth. If I simply need to know whether the litmus paper has turned red, and on observing the paper my judgement of the truth value of the proposition that the litmus paper is now red is that it is true, and moreover that it is *extremely* true, I am unlikely to need to worry about the theoretical question of whether it is *absolutely* true. Similarly, if a Bayesian weather forecaster tells me that his subjective degree of belief that it will rain tomorrow is .7, I am unlikely to need to want to contact him to ask whether it is *exactly* .7, or if perhaps it might really be .713. Moreover the analogy holds just the same with respect to the question of an objective reality underlying the theoretical dividing line between absolute certainty and less than absolute certainty. To illustrate the point, suppose I step from the pouring rain into a perfectly soundproof building without any windows. The prediction on the lunchtime forecast was that the rain would persist until lat in the evening, and it is now just 2 pm in the afternoon. One tenth of a second after the door is closed I am *absolutely* certain that it is raining outside. Another tenth of a second after I am still absolutely certain. Ten seconds after I may be inclined to admit that while I am *virtually* completely certain, I am perhaps no longer *absolutely* certain that it is raining outside. But where do we draw the line? To draw the line between absolute certainty and anything however vanishingly less, as with the corresponding dividing line in the case of degrees of truth, is almost always arbitrary. The division is vague, and is only idealistically represented as precise. But this is unimportant, in both cases, because it is the significance of the *variability* between the extremes, and the approximate values of quantities of interest, that is the important

[191] Sainsbury and Williamson (1997, p477) among others also raise the same complaint, although their evaluation of its significance is more restrained, suggesting simply that the degree theory 'does not do justice to higher order vagueness'.

matter of the theory, not the identification of any precise and *objective* dividing lines between the extremes and the intermediate quantities. Thus, not only does the inauthenticity of precise quantification apply equally in the common case of probabilities, but so does the inauthenticity of the dividing line between cases that fall on the absolute limits of the variable and those that take intermediate values.[193] And in each case this is not a significant problem for the theoretical value or the workability of the theory.

The "paradoxes" of degree theoretical truth functions

Another allegation that has been raised against the degree theory is that in some cases the degree–functional assignments of truth values to complex propositions do not seem to concur with what intuitively they ought to be.[194] Sainsbury and Williamson (1996, p477) take this line. The examples they note are:

(1) On the degree theory, 'P & ~P' (a traditional logical falsehood) will be as true as 'P v ~P' (a traditional logical truth) when P, and therefore each of them, has a value of 0.5.

(2) If Eve is definitely female but a borderline adult, so that [Eve is an adult] and [Eve is a woman] are both 0.5, degree theory assigns value 1 (whole truth) to *both* 'Eve is a woman if and only if Eve is an adult' and 'Eve is a woman if and only if Eve is *not* an adult'.

(3) For a vague predicate 'φ' (where x and x' indicate adjacent members in a sorites series) degree theory assigns '$(\forall x) \sim(\varphi x \,\&\, \sim\varphi x')$' a value 0.5, whereas its *classically* logically equivalent $[(\forall x)(\varphi x \rightarrow \sim\varphi x')] \approx 1$.

Sainsbury and Williamson suggest that on account of these (admittedly thought–provoking) implications of the degree theory, it is open to the objection that 'Its logic is unintuitive and unmotivated'. But on reflection, to draw such a far–reaching negative conclusion from the

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[192] Edgington makes the same point in 1992, p203.

[193] Edgington presents an interesting discussion of the analogies and disanalogies between probability and degrees of truth in her (1996).

[194] Sainsbury and Williamson (1996, p476), as I did in (1989), cite the standard approach advocated by Forbes in 1985. On this approach:

$$
\begin{aligned}
[\sim P] &= 1 - P \\
[P \& Q] &= \min\{[P],[Q]\} \\
[P v Q] &= \max\{[P],[Q]\} \\
[P \rightarrow Q] &= 1 - ([P]-[Q]) \text{ if } [P] > [Q] \\
&= 1 \text{ otherwise.}
\end{aligned}
$$

initial oddity of these results seems to be quite unfair. Given a little consideration of the relevant context, these results are not particularly surprising or objectionable at all (comparative that is, in respect of example (2), to closely related pre–existing problems with the truth–functional definitions in classical logic).

In the case of example (1) we need only bear in mind that these formulae *will* take a value of 0.5 *only* when we are dealing with profoundly *indeterminate* cases, for example when a coloured patch is no more clearly *red* than it is clearly *not* red – not on account of any problem with the lighting under which it is viewed, but because the limited semantic conventions that govern our application of the predicate 'red' *fail to determine* decisively either that it *does* or that it *does not* fall within its extension. Usage of the predicate in similar cases displays more or less equally mixed tolerance and intolerance for its application, as well as more explicitly qualified attitudes indicative of *partial* applicability and equally partial applicability for its negation.[195] It is in just such instances that the law of excluded middle yielding the classical logical truth 'P v ~P' most clearly seems to *fail*, and where there *is* a positive motivation to regard the degree theory of truth as a more authentic account of the situation.

In the case of (2) the natural inclination is to think of the situation as logically analogous to one where we *simply don't know* whether Eve is an adult (but where there is a definite fact of the matter). In such a logically straightforward situation we should naturally want 'Eve is a woman if and only if Eve is an adult' to come out true, but 'Eve is a woman if and only if Eve is *not* an adult' to come out *false*. But of course the situation is not that straightforward, since the situation we are set in the example dictates that there *is no* definite fact of the matter as to whether Eve is an adult. Eve is a 'borderline' or middling case, more or less equidistant from definite adulthood and definite non–adulthood. Moreover, similarly odd results arise even in the case of classical logic where the standard truth functional definition is given for the material conditional. The 'paradoxes' of the material conditional are well known. In the present example it is particularly worth noting that essentially the *same* odd result arises, even in the context of classical logical logic applied to the above determinate case, when we suppose instead that (unbeknown to us) Eve is *not* an adult. A moment's examination of the classical truth table for 'Eve is a woman if and only if Eve is *not* an adult' reveals that in this case it *also* comes out true. For any classical logician who has came to terms with the oddities of the material conditional, the discovery of an extension of these oddities into a more broadly applicable logic that employs degree theoretic counterparts of the classical truth–functions should come as neither a surprise nor as anything particularly objectionable.

Regarding (3) Sainsbury and Williamson seem to think that a degree theorist ought intuitively to expect formulae that are classically logically equivalent to *remain* equivalent in degree theoretic logic. But with a little reflection it should be clear that such a pre–theoretical idea is no more plausible than the thought that the conclusion of a classically valid argument should preserve the value of the premises in a degree–theoretic analysis of it. In fact of course the theory was designed specifically to explain *how*, where vague propositions are concerned and intermediate values are acknowledged, *classically valid* implications of (very) true complexes of propositions can *fail* to retain the values of the complexes. And in view of *that*, in contrast to what Sainsbury and Williamson suggest, it is patently obvious that we *should expect* classical logical equivalences to fail (since of course classical logical equivalence is just two way classical logical implication).

The basic fact of the matter is that, once the semantic case is made for acknowledgement of degrees of truth, the degree–theoretic functions are defined in a way that *is* intuitively plausible. Intuitively, the value of a conjunction *should* be just that of its lowest valued conjunct, and the value of a 'material' conditional *should not* fall any further below 1 than the value of the consequent falls below that of the antecedent. But once this intuitive interpretation of the complexes is adopted, results such as those mentioned above are just a matter of truth–functional calculation (and of course are only relevant where in any case the classicist is *unable* to assign any definite truth value to the relevant atomic propositions). Why should it then be a surprise that '$\varphi x \rightarrow \sim\varphi x'$' is never more than marginally less than 1, while '$\sim(\varphi x \ \& \sim\varphi x')$' can fall as low as 0.5, just because when we consider *only* the limited cases where the respective atomic propositions can be assigned definite values of 1 or 0 they will always come out equal?

An interesting, albeit logically radical, probabilistic version of the degree theory which neatly eliminates some of the above oddities, and in particular the 'paradoxes' relating to the material conditional, has been advocated recently by Edgington (1992 and 1996). Edgington's proposal builds on ideas raised by Ernest Adams (1975), in particular that an argument is (deductively) valid iff it is impossible (on the probability calculus) for the uncertainty of the conclusion to exceed the sum of the uncertainties of the premises, and that the 'real' conditional 'If A, B' should be interpreted, not in classical truth–functional terms, but on the basis of a formula for its probability – which should simply be the *conditional probability* of B granted A, i.e. $p(B|A)$, rather than the classical $1 - p(A \ \& \sim B)$. Edgington proposes extending the same probabilistic structure to degree theoretic logic, whereby

[195] It is also in such cases that people are sometimes naturally inclined, albeit with less than full

$[\sim A] = 1 - [A]$

$[A \rightarrow B] = [B|A]$ i.e. the value that should be given to B on the supposition that A is true.

$[A \& B] = [A] . [B|A]$

$[AvB] = [A] + [B] - [A \& B]$

thereby avoiding the worst of such oddities as those underlying the latter examples noted above, as well as solving related problems that arise for the standard degree theoretical approach when we are dealing with complexes involving independent variables (Edgington 1992, p197 & 200–2).

Notwithstanding the above–noted mitigations regarding the odd results of the standard formulation, the positive merits of Edgington's proposal seem to me to be very appealing. However there is quite a profound problem with this proposal. The problem is Lewis's (1976) proof that *there is no proposition* the probability of whose truth matches the conditional probability of B given A.[196]

Edgington seems to think that this is not too serious a problem for the proposal, and suggests that the message of the proof is that

> conditional judgements are *irreducibly hypothetical*. They are judgements about what is the case *under a supposition*. And the proof shows that such judgements are not equivalent to judgements about what is the case *full stop*. (Edgington 1992, p196)

While admitting (in a parenthesis) that 'Embedded conditionals are still a headache', and referring to a range of literature on that issue including her own (1994), Edgington does not seem to appreciate that simply regarding conditionals as 'hypothetical judgements' rather than 'propositions' does not appear to do anything to defuse the broader implications of the proof. Edgington clearly wants us to be able to 'do logic with conditionals', but in order to be able to do this we must surely be able to allow that it might *not* be the case that A→B, and that A→B may or may not be *compatible* with certain other statements or judgements. And if this is the case then we can just as well run a hypothetical–friendly variant of Lewis's proof, where Edgington's proposition name 'FAB' is simply replaced by the hypothetical judgement name 'A→B', and ~(A→B) is interpreted accordingly (or simply replaced by 'not–(A→B)'). The

commitment, to make statements of the form 'P & ~P'.

[196] Edgington outlines and responds to this proof in (1992, p196).

result is a proof[197] that *there is no hypothetical judgement* the probability of whose truth matches the conditional probability of B given A. In view of this, despite the otherwise beautiful results of Edgington's proposal, I am inclined to remain of the opinion that the best account of the logic of vagueness that we have for the time being is the somewhat uglier but more robust standard version of the degree theory.

Conclusion

In conclusion on this issue then, paradoxes such as the sorites, which arise when we are dealing with vague concepts, do not need to be regarded as a serious threat to the logical adequacy of deduction. The broader degree–theoretic logic that may be invoked to deal with such cases may be regarded as preserving the validity of classical deduction, at least in the context of arguments restricted to precise concepts.[198] However, as we have seen, the problem of vagueness does pose some significant difficulties and challenges for a broader analysis of argument cogency. And we may be inclined to regard these problems as a reinforcement of our earlier considerations relating to the demonstrative function of argument, whereby *clarity* of premises (and conclusion) is understood to be a basic condition of argument cogency, as of course is generally appreciated in the tradition of scientific analysis and argumentation.

[197] Call it an 'informal' proof if you like. In any case the logic of the proof may be regarded as analogous to that of deontic logic or legal analysis, which Edgington refers to as examples of non–truth–value–friendly logics.

[198] Edgington's suggestion that we hold on to the idea that validity does not permit additional falsehood (beyond the sum of the degrees of false hood it assigns to the premises) can be applied equally well to classical deduction and to traditional degree theory – it is not dependent on her probabilistic account of the degree–functions.

SUMMARY AND CONCLUSION

In Part One of this thesis, I have approached an analysis of the concept of induction via a detailed interpretation of Hume's classical discussion of inductive inference and the problems he associated with it. This has led us to reject the common view that induction (in general) is inherently non–deductive, and allowed us to develop a more tolerant appreciation of various related concepts of induction. I have focused on a particularly important concept of 'logically ampliative' inductive inference that avoids conflation of the concepts of 'premise' and 'conclusion' – which relate to arguments, with the associated but significantly distinct concepts of the 'evidential propositions' and 'hypothesis' – which relate to inferences.

Following a detailed analysis of the function and structure of argument, in Part Two I have shown how an argument that is suitably designed to fully and clearly explicate an inductive (logically ampliative) inference may – and indeed will generally – take the form of a deductively valid argument. The inferential adequacy (or 'validity' in a suitably broad sense of the term) of such an argument will depend on the truth of its associated 'inferential connection claim'. I have called this view of argument 'epistemic deductivism', since on this view all arguments will have an overall form that is deductive, and inductive arguments will generally need to be explicated as essentially epistemic arguments. I have defended the latter claim against the traditional objections raised by Hempel and recently endorsed by Kyburg.

In Part Three I have defended this induction–friendly version of deductivism against various further problems that may be raised against it, in particular with regard to Goodman's problem for inductive principles of projectibility and Jackson's solution to it, the Hume–like problem for the justification of deduction, and the sorites problem for the principles of deduction themselves.

SUPPLEMENTARY MATERIALS

This basic idea underlying this thesis began with a paper, predating my work on the thesis,[199] comprising a response to a recent paper by Patrick Maher (1996), claiming that certain considerations relating to a priori probabilities pose a problem for the assumptions of a statistical inference as defended by Williams (1947), and proposing a different approach to the validation of induction (in view of the inadequacy of a recent proposal by Pargetter and Bigelow, 1997) based on what I have called an 'epistemic frequentist' analysis of probability (c.f. Rea, 2003). In view of limited space, I have chosen to focus on the main problems of induction in the thesis (primarily Hume's) rather than discussing the relatively obscure problem raised by Maher, particularly since, if I am right in my central thesis, such debates are in any case more relevant to appraisal of the assumptions we make in inductive inference, while my central concern is with establishing the appropriate logical construal of inductive arguments.

In particular I have been concerned to emphasis that the approach I propose to the construal of inductive inference and argument should be *generally* applicable, *whatever* one's views on the content or prior justification for the details of the inferential relations that have a bearing on the inferences we make. For this reason, in view of limited space, I have also excluded a discussion of the epistemic frequentist approach to probability (Rea 2003) that I have produced in the course of my study, and an associated discussion of a problem raised by Dawid (1982) with regard to the associated objective of making judgments of probability that are 'well–calibrated'. I have also excluded a response to problems raised by Cohen (1989) for the applicability of the (traditional) Pascalian calculus of probability to certain applications, notably in the context of Law, and a discussion of the concept of question–begging. Since Quine's classical objection (1951) to the notion of analyticity has resurfaced in recent discussions (e.g. Boghossian 1997) and has been alleged to entail similar problems for the concept of deductive validity (e.g. by Govier, 1987), I have also produced a detailed critique of Quine's argument in that paper, along with comments on recent discussions of the issue. Again however space does not permit me to include such relatively peripheral material.

[199] This paper was accepted for publication in the *Australasian Journal of Philosophy* (initially due to appear March 2001) but I requested withdrawal as I anticipated substantial development of my views on the main topic (the validation of induction) in the course of my study for this PhD.

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