Hume, Induction, and Probability

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Abstract

The overall aim of this thesis is to understand Hume's famous argument concerning induction, and to appraise its success in establishing its conclusion. The thesis accordingly falls into two main parts, the first being concerned with analysis and interpretation of the argument itself, and the second with investigation of possible responses to it. Naturally the argument's interpretation strongly constrains the range of possible replies, and indeed the results of Part I indicate that the only kind of strategy which stands much prospect of defeating Hume's argument is one based on *a priori* probabilistic reasoning – hence the overwhelming majority of Part II is devoted to a thorough investigation of this approach.

Leaving aside the many incidental discussions of others' work, the principal novel ideas in Part I of the thesis concern: (a) Clarification of Hume's distinction between "demonstrative" and "probable" reasoning (§1.4); (b) Identification of *arbitrariness* as a key notion in the argument (§2.2); (c) Substantiation of the argument's normative significance (§2.4); (d) Coherent interpretation of Humean "presupposition" (§2.5, §3.2); (e) Elucidation of Hume's conclusion and its lack of dependence on his analysis of causation (§§4.1-2); (f) Construction of a complete structure diagram for the argument (§4.3); (g) Refutation of Stove's well-known alternative diagram (§5.1); (h) Likewise of Stove's analysis of Hume's conclusion and his allegation of deductivism (§5.2); and finally (i) Exposition of a three-way ambiguity in Hume's notion of "reason", which both clarifies its context in relation to predecessors such as Locke, and

also explains how the argument can be genuinely sceptical while at the same time making sense of Hume's unambiguously positive attitude towards science (§§6.1-2).

The main discussion of Part II is prepared by (j) A demonstration of the impotence of the traditional non-probabilistic counters to Hume's argument (§§7.2-5) and (k) A clarification of the available probabilistic perspectives from which a potential justification of induction could begin. The principles of probability theory (based on Jeffreys' system) are briefly presented, and the presuppositions for what follows set out. Then the heart of Part II consists of four chapters providing a theoretical framework within which are situated and discussed attempted justifications by: (l) De Finetti (§§10.3-4); (m) Williams and Stove (§10.5); (n) Mackie (§§11.2-6); and (n) Harrod and Blackburn (§§12.1-7). All of these fail, but reflection on the "continuing uniformity" strategy of Mackie combined with the scale-invariance hinted at by Harrod surprisingly suggests: (o) A new method of argument based on Jaynes' solution to the "paradoxes of geometrical probability" (§§13.4-6), which appears to represent the "last best hope" of the probabilistic approach to justifying induction. The thesis ends by asking whether the derivation of this method should be considered as an anti-Humean *modus ponens* or on the contrary as a sceptical *modus tollens*. The issues involved here enter formal terrain beyond the scope of the thesis, but nevertheless there are clear indications that the presuppositions of the new argument are themselves insecure and subject to Humean doubt. On the whole, then, and despite a small ray of hope for the inductive probabilist, Hume appears to be vindicated.

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Preface

This thesis falls into two main parts. In the first, I have attempted to provide a detailed and thorough analysis of Hume's famous argument concerning induction, focusing in particular on the presentation of that argument in Section IV of his *Enquiry Concerning Human Understanding* (though Appendix 2 provides a corresponding outline analysis of the argument from Section I iii 6 of the *Treatise of Human Nature*). In the second part, I look at possible responses to Hume's argument so interpreted, and in particular, examine the question of whether a consideration of mathematical probability theory could in principle yield an answer to it, by providing a route through the one major logical gap that Hume's discussion leaves unclosed.

My keen interest in Hume goes back to my undergraduate days at Oxford, where I found his clear no-nonsense style a breath of fresh air after an initially exciting but ultimately disillusioning flirtation with Kant. Hume's incisive and penetrating arguments, particularly on the subjects of causal and religious knowledge, were in many respects a liberation, cutting a commonsense swathe through a jungle of wish-fulfilling natural theology and obscurantist *a priori* metaphysics. However these same arguments seemed to leave in their wake an unsettling epistemological instability: their sceptical thrust did not strike merely at the ambitious sophistry attacked so wittily in the *Dialogues Concerning Natural Religion*, but also threatened to undermine the very foundations of the solidly empirical science that Hume had sought to put in its place.

This concern became heightened when as a B.Phil. postgraduate, and eventually a lecturer in Leeds, I taught the *Enquiry* to undergraduates – the traditional Oxford way had been to treat Hume's philosophy as a sequence of more or less independent "topics", and typically to ignore those not represented also in the *Treatise*; whereas I firmly believed in the importance of appreciating the overall structure of a philosopher's thought, and felt sure that Hume's strong post-*Treatise* emphasis on the contrast in legitimacy between empirical science and natural theology was a crucially significant factor in his world-view. In this context, therefore, the major problem was in making sense of his notorious argument concerning induction, which seemed on the standard interpretation to be equally destructive of both.

Meanwhile my teaching had also motivated me to start putting together a collection of accessible papers on the topics of the *Enquiry*, but despite a comprehensive trawl through the literature, including several days spent buried in the Bodleian libraries, and despite the universally acknowledged centrality of the issue, I could find absolutely nothing on Hume on induction (either the *Treatise* or the *Enquiry*) that impressed me as giving a thorough, reliable and balanced treatment. The only authors whose interpretation of Hume's argument seemed at all persuasive were those who had merely examined a part of it, or sketched it from a distance, while those (such as Stove, and Beauchamp *et al*) who had attempted a more comprehensive analysis appeared to be able to make sense of it only by concentrating on one aspect of Hume's position to the exclusion of everything else. It was in the effort to sort out this muddle, and to understand the argument for myself, that I began the work that led through various intermediate stages to the long paper "Hume's Argument Concerning

Induction: Structure and Interpretation" (Millican 1995), which formed the basis for most of Part I of this thesis. Here I have tried not only to present my own interpretation of Hume's argument, but also to explain the objections to each of the significant rival interpretations of which I am aware. Given the context of its initial publication as an article, however, many of these responses to other writers had to be condensed into terse footnotes, which though arguably less appropriate in a thesis, have here been left in that form for the sake of preserving the focus and flow of the main discussion, which might otherwise have become excessively convoluted.

In explaining the background to Part II of this thesis I must acknowledge an important second line of influence, and an intellectual debt. Soon after starting on the B.Phil., I asked John Mackie to become my supervisor even though the topic of my thesis, in Philosophical Logic, was by no means amongst his central concerns. The breadth of his interests and his straightforward style impressed me greatly, and over the year before his early death I had the benefit of stimulating supervisions that sometimes ranged over Logic, Epistemology, Ethics and Theism all in a single session. Given my mathematical background and our common interest in Hume and the demarcation of legitimate empirical reasoning, it is not surprising that I soon became interested in his probabilistic defence of induction (Mackie 1979), which I set out independently to analyse. Despite my concluding that his argument failed (the details are given in Chapter 11), he generously encouraged me to send the discussion to *Analysis*, and this resulted in my first ever publication (Millican 1982). Since that time I have naturally retained a keen interest in probabilistic attempts to refute Hume, and in the second part of this thesis have tried to pull together my various strands of thought and to place them within a unified framework. This framework is not by any means comprehensive – for example I have made no attempt to provide any deep analysis of the various competing theories of probability, or to examine the many developments in the Carnapian tradition of inductive logic. Instead, bearing in mind the context and purpose of the thesis, I have dealt with issues only in so far as they have seemed relevant to probabilistic attempts to refute Hume, and in particular to those attempts that have hitherto themselves gone unrefuted. Unlike Part I, most of which has been published previously and discussed over the years by numerous colleagues (notably at two Hume Society Conferences), Part II is nearly all relatively untested and accordingly somewhat more speculative. In attempting to provide a systematic treatment of the various probabilistic methods of addressing Hume's argument, it is also I believe more novel. Realistically, therefore, it should probably be seen not as an attempt to conclude a debate, but rather to bring some order and focus to one that has hitherto been largely piecemeal and unsystematic. If it has succeeded at least in this more modest task, then I shall be more than satisfied.

Although it may, perhaps, be unconventional in the case of a doctoral dissertation as opposed to a book, I would like to dedicate this thesis, with love and admiration, to my wife Pauline for her companionship, understanding and support over what has been an extremely busy, and in some ways extremely stressful, period for both of us.

Peter Millican

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I am extremely grateful to the Department of Philosophy and the School of Computer Studies at the University of Leeds for generously granting me study leave to complete this work. Without the freedom from teaching duties provided by that leave, finishing this thesis would have been far more difficult and my self-education in probability and statistics, in particular, would have either taken a great deal longer or been relatively superficial. I am also very grateful to Professor David Holdcroft for encouraging me in the project and acting as my supervisor, and to all of my colleagues in the Philosophy Department who contributed to providing such a pleasant and stimulating environment for philosophical study and conversation.

Many people from other universities have also given helpful comments on previous incarnations of the first half of this work, notably at the Lancaster and Utah Hume Society Conferences, and at talks that I have given to the British Society for the Philosophy of Science and at a fair number of Philosophy Departments from Dundee and Glasgow in the north, to Kent and Sussex in the south. I have also benefited from independent conversations in other contexts with numerous individuals – apart from those mentioned elsewhere in the Acknowledgements, these include Michael Ayers, Simon Blackburn, George Botterill, Justin Broackes, Paul Castell, Edward Craig, Antony Hatzimoysis, Christopher Hookway, Peter Milne, and Galen Strawson, and I apologise to anyone that I may have overlooked. However I trust that all my specific intellectual debts have been properly acknowledged in the thesis itself.

I am especially grateful to those who have taken the trouble to give detailed comments on aspects of my work (Hume Society members all): Tom Beauchamp, Martin Bell, Don Garrett, David Norton, David Owen, Wade Robison, and Sandy Stewart. I would also like to thank those who have sent me copies of their own work, and in particular Don Garrett, who generously gave me a computer disk of his forthcoming book, currently in press with Oxford and in my view destined to be a major contribution to Hume scholarship, which has enabled me recently to add an entire section (§2.4) on a central issue that has elsewhere been little discussed.

Regarding the technical work in Part II, I am very grateful to Professor John Kent of the Department of Statistics in Leeds for guidance on reading and for a number of conversations which I hope helped to keep me on the mathematical strait and narrow.

Any work like this depends upon access to a good library, and I consider myself very fortunate to have been able to take advantage of the British Library Lending Division, the National Library of Scotland, and especially the Brotherton and Edward Boyle libraries at Leeds University (including the Brotherton Special Collections). I am indebted to the Leeds librarians for agreeing to double my already generous borrowing limit, and for coping with the regular coming and going of book mountains that this brought in its wake. Particular thanks are also due here to Richard Davies.

I would like to end by expressing gratitude to my family for their moral support, including my mother and my late father who first introduced me to philosophical thinking. My children David, Katie and Jonathan have been a constant source of happy distraction, but my principal debt is expressed in the dedication to my wife Pauline.

PART I

Hume’s Argument Concerning Induction: Structure and Interpretation

Chapter 1 The Context and Topic of Hume's Argument

1.1 Introduction

Hume's argument concerning induction is the foundation stone of his philosophical system, and one of the most celebrated and influential arguments in the entire literature of western philosophy. It is therefore rather surprising that the enormous attention which has been devoted to it over the years has not resulted in any general consensus as to how it should be interpreted, or, in consequence, how Hume himself should be seen. At one extreme is the traditional view, which takes the argument to be thoroughly sceptical, leading to the sweeping conclusion that all "probable reasoning" or "reasoning concerning matter of fact and existence" is utterly worthless, so that Hume is portrayed as a negative Pyrrhonian intent on undermining the credentials of all our would-be knowledge of the world. But at the other extreme a number of very prominent commentators, particularly in recent years, have put forward a strikingly contrasting view, that Hume's intentions here are entirely non-sceptical, and that so far from advancing a negative thesis himself, he is merely intent on showing the implausible consequences of the "rationalist" position taken by some of his philosophical opponents.

Different conceptions of what Hume is aiming to achieve in his famous argument have, understandably, been associated with different views about its structure, its validity, and the nature and acceptability of its premises. Thus for example those who have interpreted it as only a *reductio ad absurdum* of a certain variety of extreme "rationalism" have usually been relatively happy to accept its apparently reasonable conclusion, whereas most of those who have followed the traditional line have considered Hume's conclusion to be wildly paradoxical, and have accordingly sought to find fault either with the argument's supposed premises or with its reasoning. Allegedly faulty premises have included some which are explicit (e.g. that there are only two types of argument, "demonstrative" and "probable"), and others which are claimed to be implicit (e.g. Hume's analysis of causation or even his entire "atomistic" epistemology). But the most common and serious accusation against the argument has been that Hume, in Flew's words (1961 p. 82), "presupposes an exclusively deductive ideal of reason", which allegedly leads him to judge any argument as worthless unless it is deductively valid, and hence to draw sceptical conclusions about induction far beyond anything which his premises would justify. This claim, that Hume is a "deductivist", has been particularly associated with David Stove, whose analysis of Hume's argument has been more influential and more widely discussed than that of any other recent commentator.[1] Stove has vigorously attacked Hume many times, most

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[1] Amongst many who accept Stove's interpretation more or less uncritically are Mackie (1974) pp. 9, 15 and Penelhum (1975) pp. 50-53. The excellent bibliography Hall (1978) says that "On the

notably in his 1965 article "Hume, Probability, and Induction" and his 1973 book *Probability and Hume's Inductive Scepticism*. Here and elsewhere Stove has argued that what he identifies as Hume's deductivism is not only an error but a pernicious canker which continues to infect much contemporary philosophical thought, especially in the philosophy of science, where he sees Hume's deductivist legacy as the primary inspiration for the massively influential but dangerously misguided "irrationalist" tradition of Popper, Kuhn, Lakatos, and Feyerabend.[2]

This first part of the thesis will attempt to show that Hume's argument on the one hand falls far short of the all-encompassing deductivist scepticism of which he has been so vehemently accused, and has no such disastrous implications for the rationality of science, while on the other hand being significantly more than a mere *reductio* of extreme rationalism. It will also maintain that the argument is far better than most of its critics such as Stove suppose, and that many of the most popular objections to it arise from crude oversimplifications, anachronistic misinterpretations, or simply a failure to read Hume's texts carefully and sympathetically. The interpretation presented here is intended to be based squarely on those texts, and on the logic of the reasonings which they contain. And so a major part of our task will be to spell out

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specific subject of Hume's argument for scepticism about induction, Stove's monograph is indispensable" (p. 2).

[2] Stove (1970) contains a slightly modified version of the analysis that he first gave in "Hume, Probability and Induction", and this is further expanded in *Probability and Hume's Inductive Scepticism*. Both that book and Stove (1986) attempt, unsuccessfully in my view, to provide a probabilistic refutation of Hume, while Stove (1982) is his most sustained attack on Popper *et alia*.

clearly and explicitly the arguments which Hume uses to establish his position, with far closer attention to his own words than has customarily been given.[3] As we proceed, we shall see that this precision brings considerable benefits, since it will enable us to dismiss on local textual grounds alone a variety of rival interpretations. And once we have established a reliable overall picture of the structure of Hume's argument, we shall then be able to deploy this to dismiss yet more misinterpretations of his position. Before embarking on this detailed interpretation of Hume's argument, however, we must first make a choice between the three different versions of it which his writings contain.

1.2 The *Treatise* and the *Enquiry*

Hume's argument concerning induction was first presented in his *Treatise of Human Nature* (1739), where it occupies most of Book I, Part iii, Section 6, entitled "Of the

[3] It has become almost standard for commentators on Hume's argument to criticise their predecessors, usually with justification, for paying insufficient attention to his texts. But even those who have made such criticisms most forcibly have typically themselves discussed the argument only in the most general terms, or by means of highly selective quotation, and have failed to show convincingly how their account corresponds to the actual content of Hume's writings. In what follows, I have therefore sought to avoid this danger by providing a thoroughly explicit and detailed analysis, giving in passing a clear interpretation of every individual paragraph which occurs in Hume's most extensive presentation. In view of the long-running controversy which has surrounded even the identification, let alone the assessment, of Hume's most famous argument, I hope that this careful attention to the texts will be welcomed for its accuracy rather than regretted for its relative lack of speculative excitement.

Inference from the Impression to the Idea" (T86-92). An extended summary of the argument (which apparently considers it "The CHIEF ARGUMENT" of the *Treatise* – A651) appeared the following year in his anonymously published *Abstract* (A649-52), but by far its fullest and, in my view, clearest statement is in the *Enquiry Concerning Human Understanding* (1748), where it appears as Section IV, "Sceptical Doubts Concerning the Operations of the Understanding" (E25-39).[4] Here we shall focus mainly on the *Enquiry* version of the argument, though reference will also be made to the earlier works, particularly where this helps to clarify the significance of Hume's terms and his general philosophical position. But although it is surely natural thus to take Hume's final and most complete statement of the argument as authoritative, the majority of previous commentators have surprisingly concentrated instead on the version in the *Treatise* (and some even on that in the *Abstract*).[5] It may therefore be worth briefly outlining my other reasons for preferring the *Enquiry*.

To start with historical considerations, there is the fact that the *Enquiry* not only appeared nearly a decade after the *Treatise*, but was subject to a number of revisions between 1750 (the second edition) and 1777 (the first posthumous edition, which

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[4] Hume (1739), (1740) and (1777). In what follows I shall refer to these as simply *Treatise*, *Abstract*, and *Enquiry* respectively, and use the letters "T", "A", and "E" to signify page references to these editions (similarly using "ME" for Hume's *Moral Enquiry*, and "D" for his *Dialogues Concerning Natural Religion*).

[5] Those who have focused mainly on the *Treatise* include Arnold, Baier, Broughton, Fogelin and Stroud, whereas Beauchamp *et al*, Flew and Jacobson have instead concentrated more on the *Enquiry*. Stove follows the *Abstract* version, stating somewhat implausibly (and without explanation) that it is "the best in every respect" (1965 p. 192).

included Hume's last corrections – see Appendix 1 below). By contrast, and to his frustration, Hume had no opportunity for a revised edition of the *Treatise* owing to its meagre sales. In addition, we have Hume's own request, expressed in the "Advertisement" which he wrote in 1775 for the volume containing the *Enquiries*, which states that the *Treatise* is a "juvenile work" and that "Henceforth, the Author desires, that the following Pieces may alone be regarded as containing his philosophical sentiments and principles".[6] This request has not been taken very seriously by Hume's critics, who have tended to regard the *Enquiry* as merely a watered down and popularised version of the unsuccessful *Treatise*. But as we shall see there are good philosophical reasons for respecting Hume's judgement here, at least as regards the presentation of his argument concerning induction.

Perhaps the most fundamental of these is that the argument in I iii 6 of the *Treatise* is much less free-standing than that in the *Enquiry*, since it is deeply embedded in the context of Hume's investigation into our ideas of the seven "philosophical relations", and in particular, his search for the origin of the idea of causation (which extends from I iii 2 to I iii 14). In the *Enquiry*, by contrast, this search is postponed until long after the argument concerning induction, which is introduced not in relation to the idea of causation, but rather as a response to the

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[6] The *Advertisement* is reproduced on page 2 of Hume (1777). On 26th October 1775 Hume wrote to his printer William Strahan, asking that it should be prefixed to all remaining copies of the second volume of his *Essays and Treatises*, the volume which contained both of the *Enquiries*, the *Dissertation on the Passions* and the *Natural History of Religion* (Hume 1932 Volume II p. 301).

fundamental logical distinction between "relations of ideas" and "matters of fact" (E25) and a naturally arising epistemological question: "what is the nature of that evidence, which assures us of any ... matter of fact, beyond the present testimony of our senses, or the records of our memory" (E26). It is true that both versions of the argument begin with a discussion of causation, but in the *Enquiry* this is brought in not as a subject of (quasi-psychological) study in its own right, but just because it is the only relation which can take us beyond the evidence of our memory and senses (E26). The difference of orientation is clearly reflected in the language of the two presentations: the *Treatise* often talks in psychological terms (involving "impressions", "ideas" and mental processes), whereas the argument of the *Enquiry* is relatively independent of psychological considerations (and accordingly speaks more of "propositions" and their logical relations such as entailment and consistency). But it also affects their structure: the *Treatise* argument is more convoluted than it need be because it begins as an argument specifically about our mechanisms of causal inference, and only mentions "probable reasoning" in general when discussing the justification of what is commonly called his "Uniformity Principle", at which point it appeals to Hume's thesis that all probable reasoning is causal.[7] The *Enquiry* argument, on the other hand, starts out as an investigation into the foundation of probable

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[7] See the two *Treatise* structure diagrams in Appendix 2, noting in particular the subtle distinction between propositions (8a) and (8), the former of which (concerning causal inferences "from the impression to the idea") acts as the argument's main pivot, while the latter (concerning "probable" reasoning) is inferred from (8a) together with (1a) and (1), and is invoked only for the purpose of deriving (16) as a means of proving that the Uniformity Principle cannot be supported by probable reasoning.

reasoning, and therefore introduces this thesis immediately, after which the remainder of the argument can be more streamlined and better focused, since no mention need then be made of specifically causal reasoning as such. There are in addition certain other respects in which the argument of the *Enquiry* is smoother than that of the *Treatise*: its structure is more explicit; it spells out a number of important stages which in the *Treatise* are omitted; and its appeal to the Uniformity Principle is less misleading. All of these points will be illustrated in what follows.

1.3 The Topic of the Argument

The argument in the *Enquiry* starts from the famous distinction known as "Hume's Fork" (a nickname first coined, I believe, by Flew 1961, p. 53):

> All the objects of human reason or enquiry may naturally be divided into two kinds, to wit, *Relations of Ideas*, and *Matters of Fact*. Of the first kind are the sciences of Geometry, Algebra, and Arithmetic; and in short, every affirmation, which is either intuitively or demonstratively certain. ... Propositions of this kind are discoverable by the mere operation of thought, without dependence on what is any where existent in the universe. ...
>
> Matters of fact ... are not ascertained in the same manner; nor is our evidence of their truth ... of a like nature with the foregoing. The contrary of every matter of fact is still possible; because it can never imply a contradiction ... We should in vain, therefore, attempt to demonstrate its falsehood. ...

> It may, therefore, be a subject worthy of curiosity, to enquire what is the nature of that evidence, which assures us of any real existence and matter of fact, beyond the present testimony of our senses, or the records of our memory. (E25-6)

The point of Hume's investigation, then, is to examine the foundation of all our beliefs about "absent" matters of fact, that is, matters of fact which are not immediately "present" to our senses or memory (he sometimes speaks simply of "matters of fact", but clearly means to refer only to those which are *absent* despite his omission of the explicit restriction).[8] Hume will argue that such beliefs are founded on inferences from things which we have observed to those which we have not, these inferences operating on the assumption that the latter will resemble the former. Such inferences, which Hume himself refers to using phrases such as "probable arguments", "moral reasonings", or "reasonings concerning matter of fact [and existence]", are now commonly known as "inductive" inferences, and hence Hume's argument is generally referred to as his argument concerning *induction*.

There is a potential source of confusion here, because the term "induction" has a rather different traditional Aristotelian sense, according to which it denotes not reasoning from observed to unobserved, but rather reasoning from particular cases to

[8] It has sometimes been suggested (cf. Bennett 1971, p. 245) that Hume counts something as a "matter of fact" only if it is "absent". But this seems too strong a conclusion to draw from Hume's admittedly sometimes careless omission of the restriction (e.g. T92, E75), given that such a usage would make its common inclusion (e.g. E26, 45, 159) pleonastic; would not conform to his principal criterion for "matter of factness" (conceivability of the contrary); and would conflict outright with some of his explicit uses of the phrase (e.g. T143: "any matter of fact we remember", T469: "Here is a matter of fact; but 'tis the object of feeling, not of reason.").

general principles or from effects to causes (with "deduction", correspondingly, denoting reasoning from general to particular or from causes to effects). Moreover the word continues to retain some of this connotation, and perhaps for this reason various commentators on Hume, notably Antony Flew (but also more excusably many writers of introductory books on the philosophy of science), have presented his argument about "induction" as primarily focused on inductive inferences to *universal* conclusions, that is, inferences of the form "All observed *A*'s have been *B*'s, therefore all *A*'s whatever are *B*'s". It should, however, be clear from the extended quotation above that Hume's concern is with *all* inferences from observed to unobserved, including singular inferences of the form "All observed *A*'s have been *B*'s, therefore this *A* (of which I now have an impression) is a *B*". In the *Treatise*, indeed, such inferences about particulars are taken as the paradigm, as indicated by the title of the section in which the famous argument occurs: "Of the inference from the impression to the idea". And in the *Enquiry*, too, most of the examples which Hume gives are of singular inferences. We should avoid, therefore, drawing any conclusions about Hume's intentions from the historical accident that the topic of his famous argument is now generally referred to as "induction".[9] For he himself never uses the term in this context, and anyway seems to understand it not in any technical sense but merely as a synonym for "inference" (T27, 628, ME170).

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[9] Flew himself may have been misled in this way. In his (1979) he defines induction as "A method of reasoning by which a general law or principle is inferred from observed particular instances" (p. 159), and in his (1961) pp. 71-2 and (1986) p. 53, he apparently interprets Hume's argument as applying only to "inductive" arguments thus understood.

Although it seems best to avoid the anachronistic word "induction" when discussing Hume's argument, we should also guard against being misled by his own terms (viz. "moral", "probable", "reasoning concerning matter of fact") for the form of inference that he is investigating . The word "moral", for example, is now used almost exclusively to mean "ethical", whereas when philosophers such as Hume and Berkeley (in *Alciphron*) speak of "moral evidence", their meaning is instead (to quote the *Oxford English Dictionary*) "evidence which is merely probable and not demonstrative". But even this *OED* definition is potentially misleading, because "probable" here has no mathematical connotation – Hume's "probable reasoning" is not a matter of calculating odds but of extrapolating from observed to unobserved, and is so named not because it makes use of numerical probabilities, but simply because it is *a posteriori* and less than certain, unlike "demonstrative" reasoning (and "intuition", which Hume classes together with "demonstration" when drawing this contrast – e.g. E25). In what follows phrases such as "probable reasoning" will be used exclusively in Hume's now slightly archaic sense of "non-demonstrative (but nevertheless plausible) reasoning", reserving the word "probabilistic" for reasoning of the mathematical kind, and thus providing us with a simple and unambiguous method of referring to the topic of his famous argument.

1.4 "Demonstrative" and "Probable"

Where exactly the topic of Hume's argument should be delimited, however, is still perhaps not quite clear, because his third characterisation of "inductive" reasoning, as that "concerning matter of fact [and existence]", might appear to imply a distinction (between this type of reasoning and that which is "demonstrative") somewhat different from that suggested by the related terms "moral" and "probable". Consider, for example, the following inference (contemplated by the present author in 1995 whilst writing this thesis):

(I) Hume's First *Enquiry* was published 247 years ago

∴ In 3 years, it will be 250 years since the publication of Hume's First *Enquiry*

Does this count as "reasoning concerning matter of fact"? It might at first seem to do so, for both its premise and its conclusion assert straightforward "matters of fact" (contingent propositions knowable only *a posteriori*), and the inference moves from past to future, as is typical of inductive reasoning. But on the other hand the argument (I) itself (as opposed to its premise or conclusion) is more than merely "moral" or "probable" – the conclusion follows from the premise with absolute deductive certainty, as sure as the arithmetical truth that 247 plus 3 equals 250.[10] The inference

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[10] The certainty might not seem absolute, because the universe could end before 1998. But to circumvent this complication, we can read the conclusion as a conditional ("If 3 years elapse, it will then be 250 years ..."). Deductively valid arguments "concerning matter of fact" can of course be simpler if no reference is made to the future, for instance "This is an orange flame, therefore this is a flame"

also lacks another crucial feature which Hume takes fundamentally to characterise "probable" reasoning, a feature to which he appeals repeatedly both in his argument concerning induction and elsewhere: namely, that of being founded on the relation of cause and effect and hence on experience (he makes the explicit claim that all "reasonings concerning matter of fact" are so founded numerous times in the *Enquiry*, and not only in Section IV: see for example E42, 76, and 104). Taking these points together I am sure that Hume, had he considered the matter, would have classed (I) as a "demonstrative" rather than as a "probable" argument (and would no doubt have recognised accordingly the infelicity of his expression "reasoning concerning matter of fact"). For to have instead classed (I) as "probable" would have undermined not only his argument concerning induction but his entire theory of knowledge and belief.

Despite the points just made, my claim that (I) should be classified on the "demonstrative" rather than the "probable" side of Hume's distinction is controversial, because it has commonly been assumed to be definitive of Hume's "demonstrative" reasoning that it should be entirely *a priori* (Stove 1965 pp. 197-8 goes so far as to claim, very implausibly, that the distinction is strictly to be understood *solely* in terms of the "epistemological character of [an argument's] premises", quite independently of the argument's "degree of conclusiveness"). However the only clear evidence given to support this assumption (Passmore 1952, p. 20; Stove 1973, p. 35) has been Hume's comments regarding the limited province of "demonstration", and these are far from

(an example noticed by Stove 1973, p. 37, though he fails to follow through its implications for Hume's distinction).

decisive.[11] Stove gives five quotations (from A650, A651, E26, E35, and E163) which he interprets as stating "that there can be no demonstrative arguments for any conclusion concerning matter of fact". But this gloss does not correspond precisely to any of the five, for Hume's actual words refer not to the possible conclusions of a "demonstrative argument", but rather to those propositions that can, or cannot, be "demonstrated", which is something quite different. It is one thing to say, as Hume certainly does, that no "matter of fact" can be demonstrated, or proved demonstratively, or be the object of demonstration; it is quite another to say that a demonstrative argument cannot even be used to deduce one matter of fact from others. Hume never makes this stronger claim, which is just as well since it seems to be inconsistent with at least two passages in Section IV of the *Enquiry* alone (each of which we shall look at again in due course). The first of these is the discussion of "mixed mathematics" at E31, which combines the assertion that physical laws are

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[11] No evidence at all is presented by Beauchamp & Mappes (1975) p. 123, Beauchamp & Rosenberg (1981) p. 43, or Gaskin (1988) p. 77, while Stove (1965) pp. 197-8 just says that "no other interpretation can survive familiarity with the texts", and invites the reader to check for himself "with the aid of Selby-Bigge's analytical indexes" – this from an author who acknowledges that the question is "of fundamental importance" (1973, p. 36) and who castigates others for failing to begin "from a really close examination of Hume's own words" (1965, p. 211)! Two things actually revealed by a close examination of Hume's texts are first, an alternative explanation for the limited province of demonstration (T70-71, E60-61, 163), which has nothing to do with *a prioricity* but is instead based on the absence of precise relationships between non-mathematical ideas, and secondly, his penchant for argument by *reductio ad absurdum*, a form of reasoning that would obviously be quite impossible if demonstrative argument had always to start from *a priori* true premises.

matters of fact with the observation that "abstract [demonstrative][12] reasonings are employed ... to determine their influence in particular instances". The second passage is at the heart of the argument concerning induction, where Hume discusses the inference from a past to a future conjunction of cause and effect: both conjunctions are clearly matters of fact, but this does not prevent his canvassing the possibility of a demonstrative argument from one to the other (E34, cf. also Hume's contemplation of a would-be demonstrative causal inference at T161-2).

Summing up, we have seen good grounds for interpreting Hume's distinction between "demonstrative" and "probable" reasoning as that, roughly, between what we now call "deductive" and "inductive" reasoning, with the latter being understood in the broad sense which encompasses inference of singular facts as well as of general laws (for additional grounds, see the discussion of Locke in §2.5 below, note 23 in §3.1, and §6.1). Initially in both the *Treatise* and the *Enquiry* Hume explicitly treats his two categories as exhaustive (though he later modifies this policy at T124 and E56n by dividing non-demonstrative reasoning into "proofs" and "probabilities"), and this simple dichotomy implies that the category of "probable" reasoning should theoretically include all non-deductive reasoning, or at least any that is worthy of the name. But in practice Hume takes this category to include just one particular class of reasonings, and these form the topic of his famous argument. Probable reasoning, thus

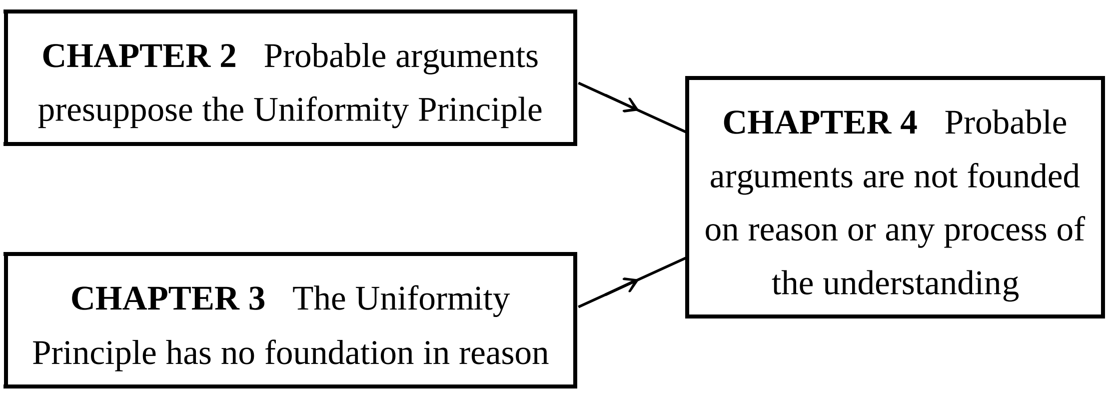
[12] The ancestor of this discussion in the *Treatise* makes the same point and apparently equates "abstract" and "demonstrative", in stating that "Abstract or demonstrative reasoning [sometimes] directs our judgement concerning causes and effects" (T414).

understood, is the kind of reasoning that we employ when we attempt to extend our knowledge by discovering new matters of fact which we have not directly perceived (so it is *ampliative* unlike the merely *analytical* (I)). And according to Hume, as we shall see, such reasoning always relies on extrapolation from observed to unobserved, based on the assumption that the two will resemble.

Chapter 2 Probable Arguments and the Uniformity Principle

2.1 The Overall Strategy of Hume's Argument

Hume's argument in Section IV of the *Enquiry* can be thought of as falling into two halves, the first of which is devoted to proving that probable reasoning presupposes in some sense what I shall call the "Uniformity Principle", and the second to showing that this Uniformity Principle has no foundation in reason. From these two results, Hume draws his ultimate conclusion, that probable arguments are "not founded on reason, or any process of the understanding". Diagrammatically, we can represent this overall argument strategy, and its order of treatment in the next three chapters, as follows:

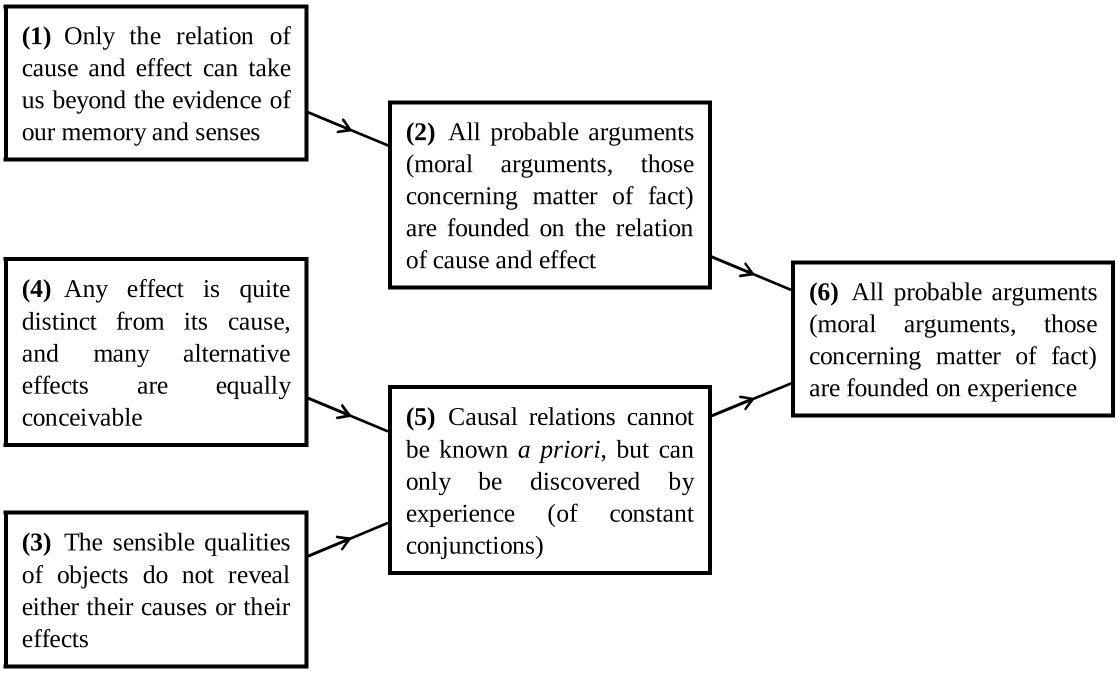


The present chapter will deal with the analysis and elucidation of the first half of Hume's argument, in which he claims that probable arguments "are founded on" an assumption of uniformity. Chapter 3 will then go on to consider the second half, in which he sets himself to undermine any pretensions that such an assumption might have to any basis in reason or the understanding. Chapter 4 will consider the nature of Hume's conclusion, and tie up some loose ends in his presentation of the argument.

2.2 "All Probable Arguments are Founded on Experience"

Part i of Section IV of the *Enquiry* is devoted to establishing one fundamental result, that all probable arguments ("moral arguments", "reasonings concerning matter of fact") must, if they are to have any force, be based on experience. So part of the answer to Hume's original query: "what is the nature of that evidence, which assures us of any [absent] matter of fact" (E26) is that such evidence cannot be purely *a priori*.

The essential structure of Hume's argument for this result can be represented as follows, with the numbering of the propositions reflecting their textual (and/or logical) order, and the set of arrows to any particular proposition indicating the grounds given by Hume for inferring that proposition (whether or not these grounds are, in fact, adequate – the aim here is to represent the structure of Hume's argument, not necessarily to endorse it).



This "structure diagram" provides, of course, no more than an idealised outline, since Hume himself does not present his arguments as having any such explicit structure. Indeed it is not easy in Part i to find even a straightforward statement of its conclusion, though proposition (6) is evidently implicit both in Hume's argumentative procedure and in the summing-up which he gives in the first paragraph of Part ii (E32).[13] Moreover his oft-repeated explicit statements of (2) and (5) are clearly intended to be read together, and Hume apparently sees (6) as such an obvious consequence of these that it does not even need to be stated, except perhaps in passing: "nor can our reason,

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[13] For the summing up, see the beginning of §2.3 below. Hume's procedure of arguing for (6) via (2) and (5) is also made clear at E27: "If we would satisfy ourselves, therefore, concerning the nature of that evidence, which assures us of matters of fact, we must enquire how we arrive at the knowledge of cause and effect."

unassisted by experience, ever draw any inference concerning real existence and matter of fact" (E27); "In vain, therefore, should we pretend to determine any single event ... without the assistance of observation and experience." (E30).

Hume's argument from (1) to (2) is presented very briefly at E26: "All reasonings concerning matter of fact seem to be founded on the relation of *Cause and Effect*. By means of that relation alone we can go beyond the evidence of our memory and senses." He then proceeds to give some illustrations to substantiate his claim that a "just inference from [facts about] one object to [facts about] another" (T89) can only be based on causation: this relation alone can provide the requisite "connexion between the present fact and that which is inferred from it", without which any such inference "would be entirely precarious" (E27).[14]

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[14] Jacobson (1987) claims that this quoted passage from E27 is crucial to understanding Hume's argument, which she takes to be founded primarily on the two premises that inferences about matter of fact are precarious unless they are mediated by *strongly objective* connexions, and that such connexions cannot in fact be perceived (or legitimately inferred). However it seems dubious to base so much on this passage, where Hume seems merely to be expanding on his familiar claim that only the relation of cause and effect can ground an inference to absent matters of fact, a claim made elsewhere (e.g. T73-4, 89) without any mention of connexions in this sense. Her interpretation also seems dubious because such connexions (i.e. powers) make an appearance in the *Treatise* version of Hume's argument only in what I below call its "coda" (T90-1), where they are clearly introduced as an entirely new consideration which has hitherto played no part. Even in the *Enquiry* Hume's talk of "connexions" is very non-committal, since both at E27 and at E29 (another passage on which Jacobson relies) he is in fact speaking very guardedly of "supposed" connexions. Any residual temptation to interpret Hume's arguments in *Enquiry* IV as turning on a strongly objective notion of "connexion" should be removed by the significant footnote at E33n, which refers the reader to Section VII for a "more accurate explication" of "power".

Having concluded that all probable reasoning is causal, Hume now sets himself to prove that all knowledge of causal relations is *a posteriori*: "I shall venture to affirm, as a general proposition, which admits of no exception, that the knowledge of this relation is not, in any instance, attained by reasonings *à priori*; but arises entirely from experience" (E27). The argument for this proposition, (5) in the structure diagram above, occupies the remainder of Part i. Hume provides two lines of argument for it, the first of which is initially presented using a thought-experiment. Suppose that Adam, just after his creation, and with no previous experience to call on, had been confronted with water and fire – simply from examining their "sensible [i.e. sensory] qualities", Adam could not possibly have inferred what effects they would have. This illustrates the general proposition (3): "No object ever discovers, by the qualities which appear to the senses, either the causes which produced it, or the effects which will arise from it" (E27). Hume thinks that this proposition, and what he takes to be its immediate consequence (5), appear unsurprising "with regard to such objects, as we remember to have once been altogether unknown to us", but when an object has been very familiar to us since our birth, "We are apt to imagine, that we could discover [its] effects by the mere operation of our reason, without experience." (E28).[15]

[15] When Hume denies that causal knowledge can be *a priori*, he seems to mean not only that it requires experience, but that it requires experience *beyond mere perception* of the sensible qualities of the objects concerned. The explanation of this may be that he here individuates kinds of object in terms of their sensible qualities, so that the appearance of any kind of object is already taken for granted when we enquire (even *a priori*) into its causes or effects.

To show that this natural assumption is mistaken, Hume employs a second line of argument, summarised in the diagram as proposition (4), which starts with a characteristic challenge: “Were any object presented to us, and were we required to pronounce concerning the effect, which will result from it, without consulting past observation; after what manner, I beseech you, must the mind proceed in this operation?” (E29). He then goes on to claim that the challenge cannot be met: that there is no way in which pure (*a priori*) reason alone can discover causal connexions. For any cause and its effect are logically quite distinct; *a priori* there is nothing in the one to suggest the idea of the other; so that in advance of experience any imagined pairing between causes and effects will appear entirely arbitrary. And even if by luck we happen to guess the correct pairing, so that we succeed in ascribing to some particular cause its actual future effect, nevertheless the conjunction of the two will still appear arbitrary from an *a priori* point of view, “since there are always many other effects, which, to reason, must seem fully as consistent and natural.” (E30).

It is very important to notice that this second line of argument is somewhat different from that with which Hume is commonly attributed, most notably by Stove:[16]

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[16] Stove (1973 p. 31, cf. Stove 1965 p. 194). I have slightly adjusted Stove’s wording to conform to the *Enquiry* presentation of Hume’s argument rather than to those in the *Treatise* and *Abstract* (which speak of “inferring an idea from an impression” etc.).

	(a)	Whatever is intelligible, is possible.
	(b)	That the inference from some cause to some effect, *prior to* experience of the appropriate constant conjunction, should have its premise true and conclusion false, is an intelligible supposition.
Therefore	(c)	That supposition is possible.
And hence	(d)	The inference from cause to effect, prior to experience, is not one which reason engages us to make.

For Hume, especially in the *Enquiry*,[17] is not stating merely that cause and effect are logically distinct – that the one is conceivable without the other – and concluding that for this reason alone there cannot be a legitimate inference from one to the other. He is starting from a much stronger premise, namely, that *a priori* there is *no discernible connexion whatever* between cause and supposed effect: in advance of experience the conjunction of the two appears "entirely arbitrary", and the supposed effect is therefore no more "consistent and natural" than any number of alternatives. So Hume's argument here need not rely, as Stove supposes, on the deductivist assumption that an inference from cause to effect is unreasonable unless the occurrence of the cause without the effect is logically inconceivable. It requires only the far more

[17] There is an interesting progression in Hume's thought here. In the *Treatise* his argument does turn largely on mere conceivability, and the suggestion of arbitrariness is relatively muted: "we might ... have substituted any other idea" (T87, cf. T111-2). In the *Abstract* this suggestion is expanded: "The mind can always *conceive* any effect to follow from any cause, and indeed any event to follow upon another" (A650). By the time of the *Enquiry* arbitrariness has clearly become Hume's principal emphasis, as it remains when he repeats the argument in the *Dialogues*: "every chimera of his fancy would be upon an equal footing" (D145); "he would never, of himself, give a satisfactory account for his preferring one of them to the rest" (D146).

modest principle that if the inference from cause to effect is to be justifiable *a priori*, then the connection between cause and effect must be at least to some extent non-arbitrary, and an examination of the cause must be able to yield some ground, however slight, for expecting that particular effect in preference to others. In adopting this compelling principle, Hume is not in any way committing himself to the deductivist view, that the only arguments which have any force are those which are logically conclusive.[18]

Having completed the principal arguments of Part i, Hume briefly states its conclusion: "In vain, therefore, should we pretend to determine any single event, or infer any cause or effect, without the assistance of observation and experience." (E30). He then adds two paragraphs spelling out some implications for science in general and for applied mathematics in particular. First, science has absolute limits, in that it cannot possibly uncover the "ultimate springs and principles" of nature: in other words it cannot provide pure rational insight into why things behave as they do. Such insight

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[18] This part of Stove's case is criticised by Morris on the ground that Hume is here "exclusively and explicitly concerned with deductive forms of argument" (1988, p. 56). In fact Hume is *explicit* about this only in the *Abstract* version, but Morris' point still has force because Hume clearly takes for granted in the *Treatise* too that *a priori* evidence must yield demonstrative certainty, and such an assumption could explain why Hume might be content to argue from mere conceivability at this stage in his argument, without implying any corresponding deductivism about *a posteriori* evidence. The same assumption plays a role in the *Enquiry* version later on (see §3.2 below), and its importance to Hume's philosophy is also apparent elsewhere, for example in his well-known search for the impression of necessary connexion (it can explain why he assumes that any would-be *a priori* impression of necessity must yield certain knowledge of causal connexions, but does not make the same demand of an *a posteriori* impression – a puzzle that vexes Craig 1987, pp. 93-100).

would require an *a priori* grasp of causal relations, which Hume's arguments have ruled out, and so the most we can hope for is "to reduce the principles, productive of natural phaenomena, to a greater simplicity, and to resolve the many particular effects into a few general causes" (E30). Scientists can continue to search for systematic order in the operations of nature, but they cannot aspire to an ultimate explanation of why things are ordered in the way that they are.

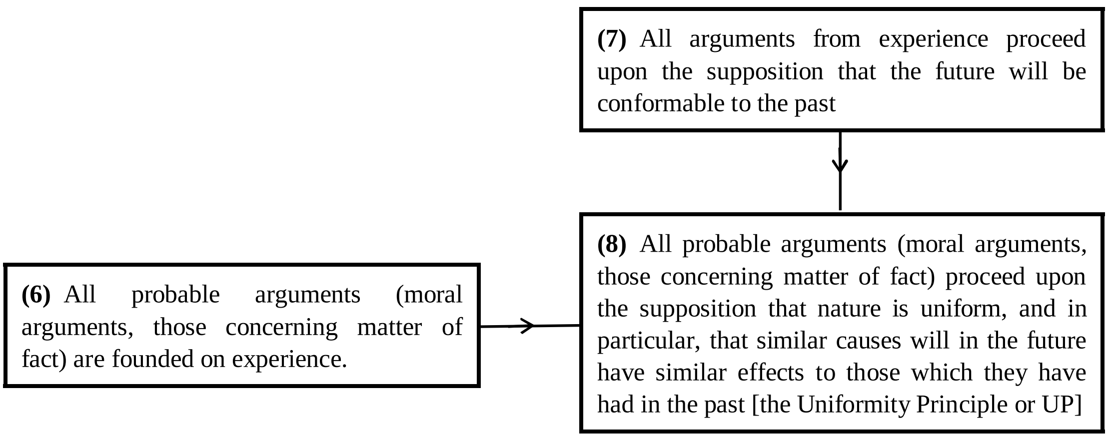
Applied ("mixed") mathematics might seem to provide an exception to this rule, since it appears to consist of rational deductions from the *a priori* principles of geometry and arithmetic. But Hume points out that any piece of applied mathematics also presupposes certain physical laws, for example the conservation of momentum, and any such law is incurably *a posteriori*. So although *a priori* mathematical reasoning certainly has a part to play in the application of such laws, "to determine their influence in particular instances", it remains true that "the discovery of the law itself is owing merely to experience, and all the abstract reasonings in the world could never lead us one step towards the knowledge of it" (E31).

2.3 "All Probable Arguments Presuppose that Nature is Uniform"

The first paragraph of Part ii provides a summary of what Hume takes his argument to have established so far, and the second announces his intentions for what follows:

> ... When it is asked, *What is the nature of all our reasonings concerning matter of fact?* the proper answer seems to be, that they are founded on the relation of cause and effect. When again it is asked, *What is the foundation of all our reasonings and conclusions concerning that relation?* it may be replied in one word, EXPERIENCE. But if we still carry on our sifting humour, and ask, *What is the foundation of all conclusions from experience?* this implies a new question...
>
> I shall content myself, in this section, with an easy task, and shall pretend [i.e. aspire] only to give a negative answer to the question here proposed. I say then, that, even after we have experience of the operations of cause and effect, our conclusions from that experience are *not* founded on reasoning, or any process of the understanding. (E32)

Hume then embarks, in the very long third paragraph, on a slightly unfocused discussion that combines two distinguishable lines of thought, the first of which can be represented as follows:



This part of Hume's argument is perhaps the least explicit of any, but as we shall see it can nevertheless be "reconstructed" with reasonable confidence on the basis of what he says both before and after it.

The quotation above from the first paragraph of Part ii makes clear that Hume's motive for investigating arguments from experience is to shed light on the general nature of probable reasoning (reasoning concerning matter of fact and existence) – this will explain the inference from (6) to (8) in the structure diagram. His investigation begins negatively, with a reminder that our experiential reasonings cannot possibly be based on any sensory perception of objects' "secret powers". But the positive account soon follows:

> notwithstanding this ignorance of natural powers and principles, we always presume, when we see like sensible qualities, that they have like secret powers, and expect, that effects, similar to those which we have experienced, will follow from them. (E33)

That this is indeed Hume's positive account is made clear by an otherwise puzzling back-reference two pages later, which he makes while summarising this part of his argument, and which cannot sensibly be interpreted as referring to anything else: "We have said, ... that all our experimental conclusions proceed upon the supposition, that the future will be conformable to the past" (E35). So Hume clearly takes himself to have stated that (7) all arguments from experience, and hence (8) all probable reasonings (since these are all based on experience), "proceed upon the supposition" that nature is uniform: that similar causes will, in the future, have similar effects to those which they have had in the past. Let us call this supposition the Uniformity Principle, or UP for short.

We have here reached the pivot of Hume's argument. For most of what he has said so far has been devoted to establishing proposition (8) – that all probable reasonings "proceed upon the supposition" of the Uniformity Principle – while most of what follows will be devoted to showing that the Uniformity Principle has no possible foundation in reason or the understanding. And it is from these two results that Hume draws his famous conclusion that our beliefs in [absent] matter of fact and real existence are "*not* founded on reasoning, or any process of the understanding" (E32).

2.4 "Foundation in the Understanding" and "Rational Justification"

Before proceeding further with the detail of Hume's argument, it is worth drawing attention here to an important terminological point which might otherwise prompt objections later on. This concerns the apparently somewhat different connotations of, on the one hand, Hume's phrase "foundation in the understanding" and its cognates, which he commonly uses to express his conclusions, and on the other hand, the word "justification" and its cognates, which for the sake of brevity and convenience I shall be using a great deal in my own articulation of his position.

The distinction here centres on the notion of *warrant* – for to say that a belief is "unjustified" is to say that it is, in some respect or from some perspective, *unwarranted*, whereas an increasingly fashionable view in recent years (at least amongst historically sensitive scholars) avers that when Hume describes a belief as having "no foundation in the understanding" he means something quite different. The most effective spokesman for this view is, in my opinion, Don Garrett:

> The conclusion of Hume's famous argument is widely regarded as an extremely negative evaluation of the evidentiary value of inductive inferences, and is often paraphrased as the claim that inductive arguments never provide any real "evidence" or "grounds" for their conclusions; that inductive inferences are "unreasonable", "irrational", and/or "unwarranted"; or that the premises of inductive arguments do not render their conclusions "more probable". ...
>
> More recently, however, a number of commentators have maintained that Hume's argument is not a radical attack on induction at all, but is to be understood more restrictedly, as an attack only on narrow "deductivistic" or "rationalistic" *conceptions* of inductive inference. ... I propose and defend a different interpretation of Hume's conclusion, according to which it is not a direct denial of the evidential value of inductive inferences on *any* conception of them, but is instead a straightforward negative conclusion, within cognitive psychology, about the causes of inductive inference.
>
> (manuscript of Garrett, forthcoming, beginning of Chapter 4)

As Garrett hints, the issue here is clouded by the influence of another fashionable view which he rejects and I shall later endorse (in §§6.2-3), namely, that Hume's notion of "reason" is fundamentally ambiguous. Both views, in their different ways, attempt to make sense of the fact that Hume's attitude to induction, even after his supposedly devastating critique of it, is fundamentally positive: he continues to use, and to recommend, "the experimental method". My own preferred way of resolving the apparent conflict is to posit an ambiguity in his notion of reason so that, in his assessment of inductive inference, a denial of reasonableness in the sense he opposes can be combined with an affirmation of reasonableness in the sense he accepts. Garrett's way is more radical, and involves denying that Hume's argument is judgemental of inductive inference in any sense whatever.

Garrett's interpretation of Hume's view of reason is accordingly relatively straightforward:

> Few interpretive remarks about Hume meet with more widespread agreement than the common claim that Hume uses the term "reason" in several different senses in his writings. If I am right, however, few interpretive claims could be further from the truth. ... Hume uses the term "reason" quite unequivocally to refer to the inferential faculty – a faculty that produces two kinds of arguments, *demonstrative* and *probable*. In arguing that inductive inferences are not "determin'd by reason", Hume is neither expressing an *evaluation* of the epistemic worth of inductive inferences, nor making a claim restricted to an arbitrarily *narrowed* sense of "reason". Nor is he denying that inductive inferences are a *species* of reasoning. He is denying only that we come to *engage* in this species of reasoning as a result of any piece of reasoning *about* it.
>
> (manuscript of Garrett, forthcoming, end of Chapter 4)

As a bold, simple and elegant hypothesis about what is going on in Hume's argument, this obviously deserves to be taken very seriously.

But however bold and elegant Garrett's approach may be, and however widely applicable to other aspects of Hume's philosophy that have traditionally (and perhaps indeed wrongly) been thought of as judgemental rather than descriptive, I do not believe that it can ultimately provide a plausible account of the logic of Hume's reasoning, nor of some of the statements of his conclusion. To take the latter first, and to anticipate slightly the discussion to come in §4.1 and §6.1, Hume does not merely deny that we *engage* in induction on the basis of "reasoning"; he also explicitly denies that *individual causal inferences* are "determin'd by reason" (e.g. T92, T97) and even that *particular inductive conclusions* are "founded on reasoning" (e.g. E32, E162) or on “the understanding” (e.g. E42). Such language seems clearly to be inconsistent with Garrett’s account, or at best an indication of careless expression on Hume’s part.

Harder to discount is the major role that considerations of *rational warrant* play in the logic of Hume’s reasoning. Again we must to some extent anticipate later discussion (in this case Chapter 3, and especially §3.1), where it will become clear that Hume’s entire argumentative strategy is to undermine the pretensions of the Uniformity Principle to any rational foundation, and hence to deny that probable inferences are founded on reason. And the first point to make here is that the possible species of rational foundation that Hume considers are not confined to *argument*, as they ought to be if “reason” is, as Garrett says, just “the inferential faculty – a faculty that produces arguments”. For twice in the *Enquiry* (if not, admittedly, in the *Treatise*) Hume explicitly denies that the Uniformity Principle can be founded on *intuition* (E34, E37), a capacity which involves immediate perception rather than inference. Quite apart from its role here in Hume’s argument, it is surely implausible to claim, as Garrett apparently must, that intuition falls outside the province of reason.

But a second and more significant point is that when Hume does turn to consider whether the Uniformity Principle can be founded on *inference* proper (see §3.2), his argument seems clearly to revolve around the denial that there is any *legitimate* method of inferring the Uniformity Principle, whereas if his concern were only to deny that *ratiocination* plays a role in our belief in uniformity, then it is difficult to see why questions of legitimacy should have any relevance. There may be all sorts of irregular bogus arguments for the Uniformity Principle, any one of which could perhaps causally underlie a person’s cognitive psychology and incline him to believe in uniformity, but

Hume consistently treats an argument's defectiveness as providing an absolutely conclusive ground for denying that the argument could either have reason as its source, or constitute the basis in reason for our making probable inferences.[19]

In short, the fundamental objection to Garrett's account is that it pays insufficient regard to the *normative* aspects of Hume's conception of reason (or rather, as I shall argue in §6.2, *two* such conceptions). Hume does not investigate the limits of reason – notably what argument can and cannot do – by the predominantly empirical methods that would be appropriate to cognitive psychology; instead he draws his conclusions on the basis of what would, and would not, provide a legitimate reason or a rational warrant. Indeed his ability to appeal to a genuinely normative notion of reason is, as we shall see later, crucial to the coherence of his entire philosophical enterprise.

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[19] The main bogus argument that Hume discusses is, of course, the circular probable inference in favour of uniformity, which he rules out of account not on the empirical psychological ground *that no such argument ever in fact occurs to us*, but rather the logical ground *that such an argument, even if it did occur to us, would be rationally unwarranted because circular* (E35-6). To be fair to Garrett, in the *Treatise* Hume sees the offensive circularity as being not purely *logical*, but at least in part *causal*: "The same principle cannot be both the cause and effect of another" (T90). And this impossibility of a causal circle can admittedly provide some basis, quite independently of considerations of rational warrant, for denying that the Uniformity Principle is founded on such circular "reasoning". This point, together with the absence from the *Treatise* of the line of thought explained in §3.1 below, makes Garrett's account far stronger as an interpretation of Hume's argument in that work than in the *Enquiry* (unsurprisingly in the light of §1.2). However I do not see how he can explain away the clear language of rational warrant that Hume uses, even in the *Treatise*, when presenting what I shall later call (in §4.2) his argument's "coda". There, at T90-1, he speaks for example of "solid" and "weak" reasoning, discusses whether one proposition "can" (or "can never") "prove" another, and gives the fact that "the foregoing reasoning had no just foundation" as a decisive ground for concluding that the reasoning in question cannot constitute a basis in reason for our inductive inferences.

In view of all these points, I shall in what follows take "justification" and cognate expressions to be generally appropriate abbreviations of such Humean phrases as "foundation in reason/the understanding". They have the considerable merit of brevity, even if they admittedly lack some of the flavour of the original.

2.5 The Uniformity Principle and its Presupposition

To return now to our analysis of Hume's argument, in the *Treatise* the Uniformity Principle and its pivotal role are made far more explicit than in the *Enquiry*:

> If reason determin'd us [to make probable inferences], it wou'd proceed upon that 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, let us consider all the arguments, upon which such a proposition may be suppos'd to be founded ... (T89)

But this very explicitness has, I suspect, seduced many commentators into misunderstanding both the nature of Hume's principle and its supposed function within probable reasoning. Indeed it may be that the misinterpretation of this particular passage in the *Treatise* is primarily responsible for the common assumption that Hume is a deductivist. Here Hume claims that probable inferences rationally depend in some sense upon the principle *that unobserved instances must resemble observed instances*. One natural interpretation of this claim is that the Uniformity Principle is (or ought to be) an implicit "middle term" of any such inference, without which the inference would be unjustified. So a probable inference, when fully spelled out, might take the form:

(M) All observed *A*’s have been *B*’s

Unobserved instances must resemble observed instances

∴ Any unobserved *A* is a *B*

Hume’s complaint against the rationality of induction then appears as the simple point that the second premise here, the Uniformity Principle, which is needed to provide (in Humean language) a “medium” between the first premise and the conclusion, is unavailable: the principle can itself be given no rational justification, and hence probable inference, which depends on it, is unwarranted. This “unavailable medium” interpretation is not incontestable (cf. T96n, 104), but it apparently derives strong support from what Hume says in the *Enquiry* when discussing the problem of inferring the powers of unobserved objects from those of observed objects: “There is required a medium, which may enable the mind to draw such an inference, if indeed it be drawn by reasoning and argument” (E34); “Where is the medium, the interposing ideas, which join propositions so very wide of each other?” (E37).

However the question now arises what it is about inferences of form (M) which according to Hume renders them justified, in contrast to their allegedly enthymematic and therefore unjustified cousins of the form:

(E) All observed *A*’s have been *B*’s

∴ Any unobserved *A* is a *B*

Many commentators on Hume have apparently thought the answer to this question too obvious even to require discussion: Hume must believe that (M) is rational and that (E) is not because he takes (M), but not (E), to be deductively valid. So when Hume states that probable arguments are "founded on" (T90), or "proceed upon" (T89, E35), or "suppose, as their foundation" (E37) an assumption of uniformity, what he means is, in Stove's words, that "Inductive arguments are all invalid as they stand, and it would be necessary, in order to turn them into valid arguments, to add to their premises [the Uniformity Principle]" (1973 p. 44 cf. 1965 pp. 203-4). And this of course would strongly suggest that Hume is, as Stove claims, a deductivist, since he is apparently taking for granted that an argument is reasonable only if it has deductive force, so that anyone who advances an argument which is not deductively valid must either be arguing unreasonably, or else must be presupposing some implicit "middle" premise (or premises) which, when added to the argument, would render it valid. On this account, therefore, Hume is both a deductivist and what we might call a "quasi-deductivist", in that he takes a probable argument to be in effect a disguised deduction, with the Uniformity Principle as a suppressed premise.[20]

It is simplistic to assume, however, that the "unavailable medium" interpretation necessarily implies that Hume views the Uniformity Principle as the missing link in an

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[20] A quasi-deductivist interpretation of Hume's argument need not imply, however, that he is himself a deductivist, for it might be thought that he uses it to show the limits of some variety of "rationalism" rather than as an argument *in propria persona*. This approach, which is similar in spirit to that of Beauchamp *et alia* (to be considered in §6.3), is for example taken by Baier (1991, p. 68).

otherwise enthymematic *deduction*. Many have indeed assumed this, but since they have done so without comment this may have been for no better reason than that they associated the notion of a "middle term" (or suppressed premise) with Aristotelian (or modern) formal logic, and hence with deductive systems.[21] In the interpretation of Hume, however, such considerations should carry very little weight, because Hume inherited his "logic" more from Locke than from Aristotle, and inherited with it a highly dismissive attitude towards deductive syllogistic theory (see for example E163). When Locke and Hume talk about a "medium", therefore, we should certainly not assume that they have in mind an exclusively deductive paradigm of inference. Here is Locke defining demonstration and probability (1690, IV xv 1):

> As Demonstration is the shewing the Agreement, or Disagreement of two *Ideas*, by the intervention of one or more Proofs, which have a constant, immutable, and visible connexion one with another: so *Probability* is nothing but the appearance of such an Agreement, or Disagreement, by the intervention of Proofs, whose connexion is not constant and immutable, or at least is not perceived to be so, but is, or appears for the most part to be so, and is enough to induce the Mind to *judge* the Proposition to be true, or false, rather than the contrary.

"Proof" is Locke's official term (IV ii 3) for what he elsewhere calls an intermediate idea or a medium (e.g. IV iv 7, IV xvii 15). But there is no suggestion here that these

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[21] Flew (1961) pp. 70-1 certainly seems to be misled by the Aristotelian model. His own exclusive focus on universal inductive arguments (criticised in §1.3 above), together with Hume's talk of a "medium", apparently suggest to him the idea of a syllogism, after which he repeatedly alleges, without any supporting argument, that Hume takes a probable argument to be a "[broken-backed] syllogism" or a "failed deduction" (pp. 71-89; cf. Flew 1986, pp. 52-5).

"middle terms" are restricted to demonstrative reasoning: on the contrary, Locke clearly states that the difference between demonstrative and probable reasoning is entirely non-formal, residing not in the presence or absence of "proofs", but solely in the strength of connexion between the "proofs" concerned. He re-emphasises this point many times, particularly at IV xvii 16:

> There are other *Ideas*, whose Agreement, or Disagreement, can no otherwise be judged of, but by the intervention of others, which have not a certain Agreement with the Extremes, but an usual or likely one: And in these it is, that the *Judgment* is properly exercised, which is the acquiescing of the Mind, that any *Ideas* do agree, by comparing them with such probable *Mediums*.

So Locke is clearly happy with the notion of a "probable medium", a "middle term" whose connexions with the premise and the conclusion of the argument in which it occurs are less than deductively certain. And given Locke's manifest influence on Hume (evident for example at T124 and the footnote to E56, where Locke is apparently acknowledged as the primary source of Hume's initial dichotomy between demonstrative and probable reasoning), this is already sufficient to undermine any case for Hume's being a quasi-deductivist which is based solely on his demanding a "medium" for inductive inferences.[22] More direct evidence to the same effect,

[22] Owen (1992) draws a far more radical lesson from Locke's influence, questioning even whether Hume employed the now standard propositional model of arguments as opposed to the "series of ideas" model suggested by Locke's words. I have doubts whether the latter is independently significant or even coherent (e.g. with inferences involving quantification), but in any case Hume's language in the *Enquiry* (e.g. E34) makes clear that he is very comfortable with the propositional framework, even if he still sometimes lapses into Lockean talk of "interposing ideas" (E37).

moreover, is furnished by Hume's own texts, most notably a passage in his *Dialogues* (D143, cf. D176), where the *a priori* theist Demea refers to the "mediums" by which the empirical theist Cleanthes endeavours to establish the existence of God, and complains precisely because these mediums are merely probable:

> ... still less can I approve of the mediums, by which you endeavour to establish [God's existence and nature]. What! No demonstration of the being of a God! No abstract arguments! No proofs *a priori*! ... Can we reach no farther in this subject than experience and probability?

This passage illustrates that Hume, like Locke, is quite untainted by the now apparently common but always gratuitous assumption that only a demonstrative argument can contain a "middle term". One might mischievously suggest, therefore, that those commentators who make such an assumption the basis of their interpretation of his argument concerning induction reveal not so much Hume's deductivist prejudices, but rather their own!

Although we have seen enough to indicate that the quasi-deductivist interpretation is textually and historically unwarranted, it is not yet clear whether there is a coherent alternative account of the supposed logical role of Hume's Uniformity Principle. Thus, in particular, we have as yet no explanation of why Hume should take for granted that inferences of form (E) presuppose ("are founded on", "suppose, as their foundation" etc.) such a principle, unless he is after all operating within a deductive paradigm. Two connected issues arise here: first, the kind of presupposition that Hume has in mind; and secondly, the nature of the principle which is according to him thus presupposed. In addressing the first of these, we should be careful at least initially to interpret the notion of “Humean presupposition” as broadly as possible, so as not to prejudge the issue in favour of a deductivist or other narrow interpretation. We can, however, give the notion some specific content by observing its function within Hume’s argument, where he is explicitly concerned with establishing whether or not inductive inferences are founded on reason, and ultimately gives a negative answer on the ground that an alleged presupposition of such inferences is not itself so founded. This suggests that an appropriate sense of presupposition might be spelt out roughly as follows: *an inference (or, in an extended sense, a person who makes that inference) presupposes some proposition if in order for that inference to be rationally well-founded, the proposition in question must also itself be rationally well-founded.* Such an account is indeed desirably broad, for it is consistent with a wide range of different views – including deductivist views – regarding what counts as rational well-foundedness (i.e. having an adequate foundation in reason or “rational justification”). But this provides a sufficient understanding of Humean presupposition to enable us now to enquire into the nature of Hume’s Uniformity Principle by asking the very question to which that principle is intended to provide an answer: what exactly *is* being presupposed in this sense by someone who employs an inference of form (E)?

One modest, and perhaps at first apparently trivial, answer to this question is as follows: when we use an inference of form (E) to extrapolate from observed to unobserved (for brevity, from “past” to “future”), we are presupposing that past instances are *evidentially relevant* to future instances, or in other words, that the nature of past instances gives some evidence concerning the nature of future instances. This is not all, however, for when we make a “probable” inference we suppose that the evidence provided by past instances is *positively* relevant, in that future instances are likely to *resemble* past instances rather than, for example, constrasting with them. Hence any probable inference presupposes that past instances have a *positive evidential relevance* to future instances: this presupposition can very naturally, though loosely, be expressed by saying that *future instances will resemble past instances* (cf. T89), or that *the future will be conformable to the past* (E35), or that *the past is a rule for the future* (cf. E38), and so on. I suggest, therefore, the following interpretation: when Hume claims that probable reasoning presupposes the Uniformity Principle, he is simply making the straightforward point that any probable argument, by its very nature, treats past instances as *positively evidentially relevant* to future instances. This interpretation makes good sense of Hume's claim and the various ways in which he expresses it, explains why he thought it too obvious to require further elaboration, and is also entirely consistent with his subsequent procedure. For if it is true that any probable argument treats past instances as evidentially relevant to future instances, and if it can be shown that there is no good reason for so treating them, then (at least on the broadly "foundationalist" conception of reason which Hume's treatment of presupposition shows him to be adopting here – cf. Stroud 1977, pp. 60-2) the reasonableness of all probable inference will indeed be undermined.

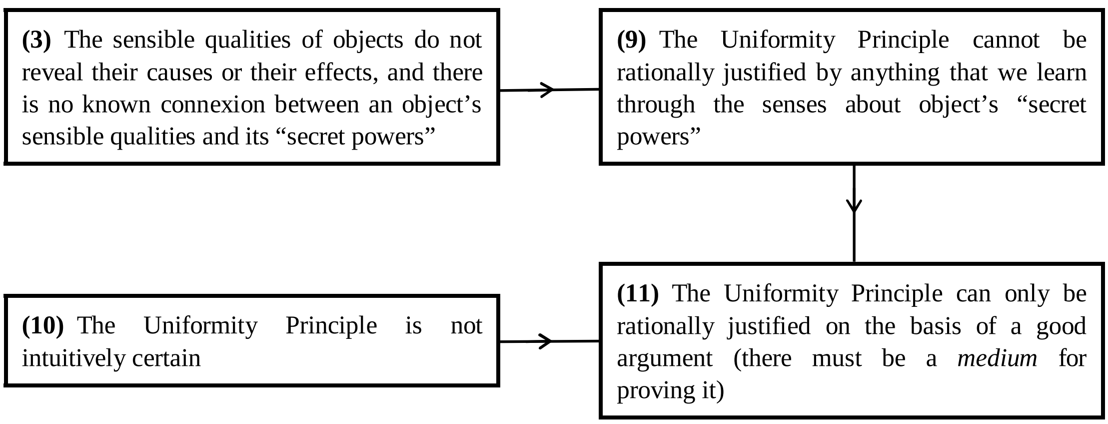
It is a significant virtue of Hume's argument, interpreted in this way, that it does not commit him to any very specific view regarding either what counts as rational well-foundedness (as we saw above), or what counts as evidential relevance (it is, for example, consistent with the deductivist assumption that the only kind of evidential connexion that carries any force is deductive implication). This means that Hume need give no such hostage to fortune – if his argument succeeds in showing that probable reasoning is not rationally well-founded, then this conclusion can stand even if his own personal theory of evidential relevance (whatever that may turn out to be) is rejected. All this might revive the suspicion that Hume is himself, after all, a quasi-deductivist (and hence very probably a deductivist too), even if he has ingeniously succeeded in insulating his famous argument from these personal weaknesses. But fortunately such a suspicion can be decisively refuted, ironically by turning on its head a criticism that has often been made of him.

Those who take Hume to be a quasi-deductivist are fond of accusing him of inconsistency or oversight, on the grounds that his proposed Uniformity Principle is actually quite inadequate to fulfil the role that he supposedly requires of it (e.g. Flew 1961, p. 74; Stove 1965 p. 204; Ayer 1972, pp. 20-1). This role demands that the Uniformity Principle be sufficiently strong to transform (at least many) "correct" *inductions* into *deductions* – to enable a conclusion about future instances to be drawn deductively from premises which describe only past instances. And it is clearly impossible to satisfy this requirement with the vague formulations of the Uniformity Principle that appear in the *Enquiry*. The more explicit principle of the *Treatise* is perhaps more powerful, at least if interpreted as stating literally *that future instances will resemble past instances in any considered respect*. But then such a principle is far too strong to play the role of legitimating inductive inference, for it is utterly implausible and has been falsified innumerable times – as for example when the first black swan was observed. The moral is evident: any principle strong enough to transform "correct" inductions into deductions will also be strong enough similarly to transform many "incorrect" inductions, and will certainly therefore be false. Evident this may be, and damning for quasi-deductivism, but it would come as no surprise to Hume, who recognises explicitly that even "good" probable arguments can fail despite what he takes to be the fact that nature is uniform. In Section I iii 15 of the *Treatise*, for example, "Rules by which to judge of causes and effects", he makes it abundantly clear that inductions are incurably fallible, *even on the supposition that nature is uniform*, since we can never be certain that our past observations have taken all relevant causal factors into account (T175 cf. E86-7). So if he is even minimally consistent in this sort of inductive fallibilism, Hume *cannot be* a quasi-deductivist. (Note however that this does not yet quite settle the question of whether he may be some other kind of deductivist – a task to be completed in §5.1 below).

Chapter 3 Seeking a Foundation for the Uniformity Principle

3.1 "The Uniformity Principle can only be Justified by Argument"

In the previous chapter (§2.3) we examined the first distinguishable line of thought in the long third paragraph of Section IV Part ii. It is now time to move on to the second line of thought, which can be represented as follows:



As in Part i Hume emphasises our inability to discern an object's causes or effects by mere observation of its "sensible qualities", but here the point of doing so becomes clear only after he has sketched his positive account of experimental reasoning based on the Uniformity Principle:

> ... there is no known connexion between the sensible qualities and the secret powers; and consequently ... the mind is not led to form such a conclusion concerning their constant and regular conjunction, by any thing which it knows of their nature. (E33)

This passage spells out clearly the implication from (3) to (9) as represented in the structure diagram above (though (9) as stated in the diagram makes explicit the contrast which Hume apparently intends between *sensory* knowledge of object's secret powers, which he here denies, and *inferential* knowledge, which he has not yet ruled out). At this point Hume treats the Uniformity Principle as a straightforward *proposition* "concerning [the] constant and regular conjunction ... between [an object's] sensible qualities and [that object's] secret powers", but he soon goes on to pose the question of its justification in a different way, treating the principle as something more like a *rule of inference*:

> These two propositions are far from being the same, *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 allow, if you please, that the one proposition may justly be inferred from the other ... But if you insist, that the inference is made by a chain of reasoning, I desire you to produce that reasoning. (E34)

Hume betrays no awareness of any shift in his procedure here, so we must assume that he would draw no distinction between the justification of an inference from *P* (a proposition about past instances) to *Q* (a proposition about future instances), and the justification of the proposition that *P implies Q* (in effect the Uniformity Principle).[23] Thus when he immediately continues to say 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." (E34), it indeed seems legitimate to interpret this as a comment about the justification of the Uniformity Principle, as represented by the inference from (10) to (11) in the structure diagram above.

3.2 "There is No Good Argument for the Uniformity Principle"

The stage is now set for the climax of Hume's argument concerning induction, in which he denies the possibility of any good reasoning at all which could justify the Uniformity Principle and hence provide a rational ground for probable inference. Many commentators have treated this part as though it were virtually the whole of Hume's argument,[24] so it is worth recalling that in the *Enquiry* it is not only preceded by Part i, but is also introduced by the line of thought outlined in §3.1 above, in which

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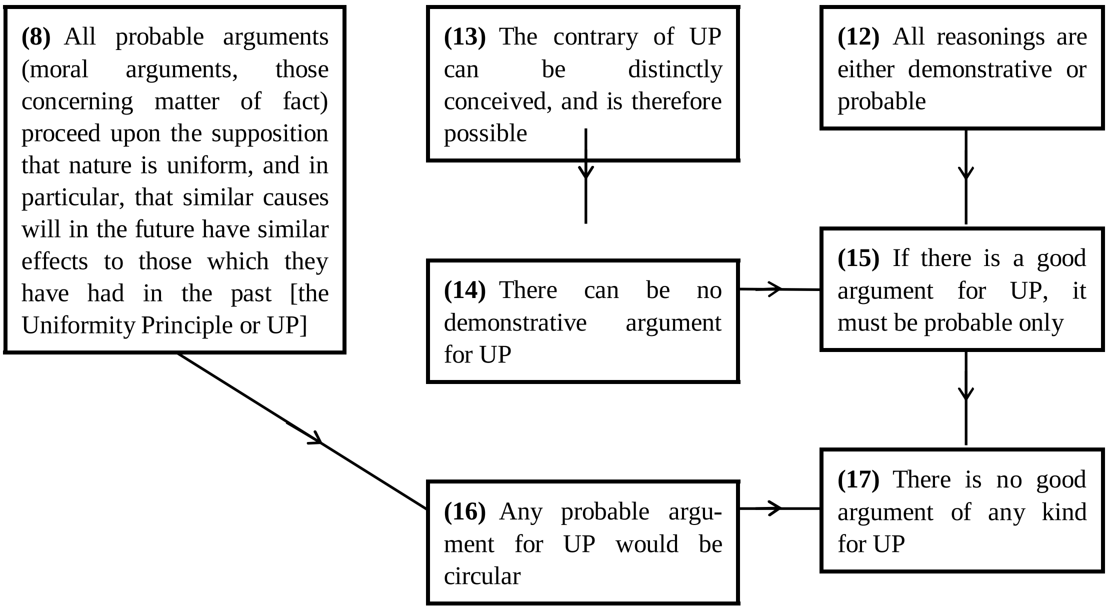
[23] It is worth noting in passing the significance of this. If Hume (correctly) draws no fundamental distinction between a proof of Q from P (i.e. $P \quad Q$) and a proof that P implies Q (i.e. $P \to Q$) then he cannot consistently distinguish between types of argument on the basis merely of the modality of their premises (which as we saw in §1.4 above is alleged by Stove).

[24] Fogelin (1985), for example, calls the entire argument concerning induction Hume's "no-argument argument" (p. 46).

Hume takes the trouble to argue that some reasoning is necessary if the Uniformity Principle is to be justified, a point which he apparently takes more or less for granted in the *Treatise* and *Abstract*.[25]

The structure of this most famous part of Hume's argument is admirably clear:



[25] Jacobson (1987) argues that it is Hume's preliminary line of thought regarding our ignorance of powers which carries his main sceptical thrust, and she accordingly sees his "circularity" argument as a merely secondary move designed to show that the sceptical gap cannot be plugged by justifying the Uniformity Principle. We saw earlier (in §2.2 note 14) that her interpretation is hard to square with the *Treatise*, but it also seems to misrepresent Hume's own view of his primary argument in the *Enquiry*, as revealed at E39: "it is not reasoning which engages us to suppose the past resembling the future, and to expect similar effects from causes, which are, to appearance, similar. This is the proposition which I intended to enforce in the present section."

It starts with the general claim (12) that "All reasonings may be divided into two kinds, namely demonstrative reasoning ... and moral reasoning, or that concerning matter of fact and existence [i.e. probable reasoning]" (E35). The inference from (13) to (14) is then quickly drawn:[26] "That there are no demonstrative arguments in the case, seems evident; since it implies no contradiction, that the course of nature may change ... Now whatever is intelligible, and can be distinctly conceived, implies no contradiction, and can never be proved false by any demonstrative argument or abstract reasoning *à priori*". Propositions (12) and (14) together imply (15): "If we be, therefore, engaged by arguments to put trust in past experience, and make it the standard of our future judgment, these arguments must be probable only". But now the previous conclusion (8) can be appealed to in order to show (16) "that there is no argument of this kind" (E35). For (8) states that all probable arguments presuppose the Uniformity Principle. "To endeavour, therefore the proof of [UP] by probable arguments ... must be evidently going in a circle, and taking that for granted, which is the very point in question." (E35-6).

Though superficially very straightforward, there is a lot going on here beneath the surface. For example, Hume is certainly not being entirely explicit when he states that "all reasonings" are either demonstrative or probable and goes on to rule out the possibility of either type of argument for the Uniformity Principle. For he was surely

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[26] This is the only essential use that Hume makes of his "argument from distinct conceivability" in Section IV of the *Enquiry*, since as we saw in §2.2, Stove is mistaken in seeing it as the sole basis of the inference from (3) to (4).

well aware that philosophers could, and would, concoct various *defective* arguments to support this principle – indeed he considers such an argument himself, at E36-8. What he is denying, therefore, is that any *good* argument is available for the purpose, on the grounds: first, that all *good* arguments are either demonstrative or probable; secondly, that there cannot be a *good* demonstrative argument for the falsity of what is distinctly conceivable; and thirdly, that a *good* probable argument cannot be circular.[27] This passage is, in fact, an illustration of a general rule of Hume interpretation, that when he speaks of "all [or no] arguments [reasonings/inferences]", the qualification "good" is usually implicit (see also for example T142, E150, D205). This is perhaps not surprising given the normativity of his notion of "reason" (as discussed in §2.4 above and to be considered further in §6.2 below).

Hume's grounds for ruling out the possibility of a good demonstrative or probable argument to sanction the inference from past to future also merit some discussion. Here are two candidate arguments, one of each form:

(D) *C*'s have always been attended with effect *E*

All *C*'s have similar effects, no matter when they are observed

∴ Future *C*'s will be attended with effect *E*

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[27] The first of these three points will suffice if the terms "demonstrative" and "probable" are themselves interpreted normatively, so that an argument only *counts as* being of the appropriate type if it is a *good* instance. But Hume himself does not consistently interpret them in this way, and in the *Treatise* especially seems perfectly content to talk of "fallacious" demonstrations (e.g. T53, 80) or "unphilosophical" probable reasonings (Section I iii 13).

(P) *C*'s have always been attended with effect *E*

In the past, similar objects have always turned out to have similar effects

∴ Future *C*'s will be attended with effect *E*

(D) indeed seems to be a deductively valid argument, and might thus appear to contradict Hume's assertion "that there are no demonstrative arguments in the case". But the contradiction is only superficial, for Hume's interest here is clearly epistemological rather than pedantically logical:[28] what he really means to say is that *the inference from past to future cannot be accomplished by means of a demonstrative argument whose additional premises (if any) are already known to be true* – (D) does not qualify as such an argument, because its second "middle" premise cannot be known unless we have already found some way of gaining access to knowledge of future *C*'s. As for (P), Hume would, I believe, acknowledge that this argument is in some sense a "good" probable inference, and would accordingly countenance it in other contexts *as a means of establishing the properties of future C's* (cf. T173-4). The problem is that (P) is useless as an attempt to establish the rational credentials of the Uniformity Principle [UP], for as a probable argument it must itself presuppose this principle, and therefore cannot be used to support it. This circularity can be spelt out on the basis of our earlier analysis of Humean presupposition: an inference such as (P) is rationally

[28] Hume's disregard for logical precision verges on carelessness when he specifies at E34 the premise and conclusion of the would-be inference discussed here. For he is clearly interested in an inference from the effects of past objects to the effects of future objects, not as his words strictly imply from past personal observations ("I have found ...") to a personal prediction ("I foresee ...").

well-founded only if UP is rationally well-founded, and it follows that the use of (P) to establish the rational well-foundedness of UP must inevitably “take that for granted, which is the very point in question”.[29]

Given Hume’s understanding of “demonstrative” and “probable” reasoning, then, he seems to be quite right to conclude that neither of these can furnish an argument capable of rationally justifying the Uniformity Principle in a non-circular and well-founded manner. But his “no-argument argument” can nevertheless be challenged by resisting his initial assumption (12 in the structure diagram) that “demonstrative” and “probable” arguments, thus understood, are the only two kinds of respectable reasoning available. Hume apparently makes this claim not out of dogmatism, but simply because he cannot imagine any other legitimate type of reasoning (cf. T90, E36). And anxious as he is to consider all possibilities, he seems to realise that this might be a weak point in his argument: “there may still remain a suspicion, that the enumeration is not complete” (E39).

To see how a third “species of reasoning” might escape, so to speak, between the horns of Hume’s dichotomy, let us focus on the particular characteristics of demonstrative and probable reasoning that enable him to rule out each of them in turn as a possible source of justification for the Uniformity Principle. First, then, the reason

[29] This “presuppositional circularity” differs somewhat from the more familiar “deductive circularity” of an argument whose conclusion is also one of its premises. In this sense, *contra* Stove (1965, p. 205), a circular argument need not be deductively valid.

he is confident that no demonstrative argument can do the job is that such an argument always yields total certainty relative to its premises, so the mere distinct conceivability of a change in the course of nature (13) is sufficient to show that the Uniformity Principle cannot be established by demonstration (14) no matter what our premises about the past might be. By contrast, his reason for denying that any probable argument can establish the Uniformity Principle is that all such arguments are founded on experience (7) and that they therefore themselves rely (8) on precisely the kind of extrapolation from past to future that the Uniformity Principle itself is needed to justify; hence any such argument for this principle will be viciously circular (16). Putting these together, it follows that if there were a third form of reasoning which yielded merely probable conclusions (rather than certainties), but did so on *a priori* grounds (rather than by extrapolation from past experience), then this form of reasoning would be completely immune to Hume's objections: he could not rule out the possibility of such a justification of the Uniformity Principle either on the basis of his argument from distinct conceivability or on the ground of circularity.

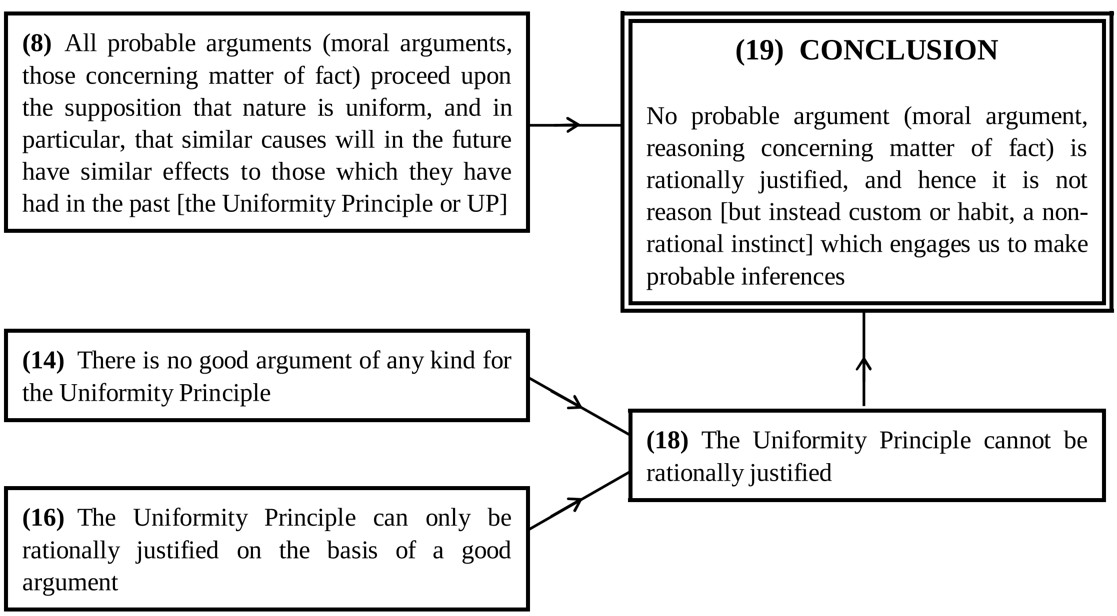
It is highly debatable whether *a priori* probabilistic reasoning (based for example on the Principle of Indifference, "logical probability" measures, considerations of invariance or other supposedly non-empirical principles) is a genuine possibility or, if it is, whether such reasoning could conceivably provide a justification for the Uniformity Principle. But those who claim that Hume himself showed this particular route to be a dead end are certainly mistaken, for as we have seen, when he denies that "probable" reasoning could perform such a role, Hume has in mind only inductive reasoning from experience, not mathematical probabilistic reasoning that is *a priori*.[30] There is, then, a definite gap in Hume's argument – it will be the principal task of Part II of this thesis to examine the question of whether this gap can be exploited by his opponents.

[30] Indeed it is far from clear whether such reasoning, if it exists, should count as "probable" at all, given that Hume considers *a prioricity* to be almost a defining characteristic of "demonstrative" reasoning (T124, A650, E25).

Chapter 4 The Conclusion of Hume's Argument, and a Coda

4.1 Hume's Conclusion: "Probable Inferences are Not Founded on Reason"

As on many other occasions, Hume leaves his reader to slot into place the final pieces of the philosophical jigsaw which he has created. But if the account given in the previous chapters is correct, the way in which they are intended to fit together is evident from the structure and flow of his argument:



The precise nature of Hume's conclusion may seem unclear from his own words. We have already seen that he anticipates it when stating his intentions at E32:

> I say then, that, even after we have experience of the operations of cause and effect, our conclusions from that experience are *not* founded on reasoning, or any process of the understanding.

But when later summing up the section at E39, he expresses his conclusion somewhat differently:

> ... it is not reasoning which engages us to suppose the past resembling the future, and to expect similar effects from causes, which are, to appearance, similar. This is the proposition which I intended to enforce in the present section.

There is a subtle difference here: at E32 he is saying that *our particular experiential conclusions* are not "founded on reasoning, or any process of the understanding", whereas at E39 he is saying that our supposition of *the Uniformity Principle* is not so founded. If we move forward to the beginning of Section V, however, we can find at E41 a passage which helps to reconcile these two readings:

> ... we ... conclude ... in the foregoing section, that, in all reasonings from experience, there is a step taken by the mind, which is not supported by any argument or process of the understanding.

So all reasonings from experience involve a step, namely the assumption of uniformity, which is not supported by "any process of the understanding" – which, indeed, *cannot* be so supported if Hume's argument is correct. And Hume goes on in Section V to provide an alternative explanation of why we make this step:[31] it is entirely non-rational, and is the product not of reason but merely of a particular one of our brute "natural instincts, which no reasoning or process of the thought and understanding is able, either to produce or to prevent" (E46-7). This instinct is what "makes us expect, for the future, a similar train of events with those which have appeared in the past" (E44), and Hume accordingly calls it "custom", or "habit". Here, then, is the answer to his original enquiry at E26 regarding "the nature of that evidence, which assures us of any [absent] matter of fact":

> All inferences from experience, therefore, are effects of custom, not of reasoning. ... Without the influence of custom, we should be entirely ignorant of every matter of fact, beyond what is immediately present to our memory and senses. (E43-5)

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[31] Unless Hume's view of the psychological processes involved has changed dramatically since writing the *Treatise*, he does not mean to say that the assumption of uniformity is an *explicit* step in all our inferences from experience. For T104 is unequivocal on the matter: "the understanding or imagination can draw inferences from past experience, without reflecting on [the Uniformity Principle]; much more without forming any principle concerning it, or reasoning upon that principle."

4.2 Coda: the Irrelevance of Causal Powers, and a Parting Shot

In both the *Treatise* and the *Enquiry*, Hume's main argument finishes with his circularity charge against any would-be "probable" justification of the Uniformity Principle. But in both he goes on to refute one natural attempt that might be made to justify induction by appeal to objects' "powers".[32] In the *Treatise* the way in which Hume introduces this discussion makes very clear its status as an illustration of the scope and force of his argument rather than as an essential part of it:

> Shou'd any one think to elude this argument; and without determining whether our reasoning on this subject be deriv'd from demonstration or probability, 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 expos'd to our examination. It may, perhaps, be said, that after experience of the constant conjunction of certain objects, we reason in the following manner. Such an object is always found to produce another. 'Tis impossible it cou'd have this effect, if it was not endow'd with a power of production. The power necessarily implies the effect; and therefore there is a just foundation for drawing a conclusion from the existence of one object to that of its usual attendant.

[32] In the *Enquiry* Hume first presents an additional new argument (but one highly reminiscent of T88 and T163-5) designed to strengthen his claim that the uniformity of causal relations cannot be established *a posteriori* by reason, on the ground that if it could be so established it would be knowable "upon one instance" and not (as we find) only "after a long course of uniform experiments" (E36 cf. E43). The argument is a weak one, and Hume's inability to imagine any kind of reasoning to which numbers of instances would be relevant illustrates his (historically unsurprising) poor grasp of probability theory.

> The past production implies a power: The power implies a new production: And the new production is what we infer from the power and the past production. (T90)

The *Enquiry* version of this attempted justification of induction is subtly different, in that instead of apparently using the mere existence of a cause and effect relationship to infer the existence of a power, it takes for granted from the start that objects have powers and appeals to the *constancy* of causal relations to infer a continuing "connexion between the sensible qualities and the secret powers" (E36).[33] But the forceful refutation that follows is equally decisive against either version:

> When a man says, *I have found, in all past instances, such sensible qualities conjoined with such secret powers:* And when he says, *similar sensible qualities will always be conjoined with similar secret powers*; he is not guilty of a tautology, nor are these propositions in any respect the same. You say that the one proposition is an inference from the other. But you must 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. ... 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. (E37-8 cf. T91)

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[33] This difference is indeed minor, because Hume considers a similar move later in the *Treatise* version at T91 ("Shou'd it be said, that we have experience, that the same power continues united with the same object ..."), and he has anyway already explained at T87 that the very notion of causation involves constancy.

Here we clearly have a straightforward application of Hume's central argument, rather than a significant independent addition to it. This elegant refutation does, however, help to settle an important issue concerning the relationship between Hume's reasoning about induction and his theory of causation.

In the *Treatise*, as was mentioned in §1.2 above, Hume's argument concerning induction is presented in the context of his analysis of causation. This can give the impression that the one relies heavily on the other, and many books on Hume have tended to confirm this impression by treating the two together, often within the confines of a single chapter. But the quotation just given shows clearly that Hume's case against the rationality of induction is quite independent of his "regularity" analysis of causation, for even if causation is instead a matter of "secret powers" or "natural necessities", and even if all observed *A*'s have in fact been endowed with the secret power to produce *B*, this in itself can give us no reason for supposing that some hitherto *unobserved A* has been or will be similarly endowed. The point is that because the connection between *A* and that power is not *a priori*, we can only justifiably infer a continued conjunction between them if we already have some justification for extrapolating from observed to unobserved. So an analysis of causation in terms of "secret powers" or "natural necessities" provides no answer whatever to the inductive sceptic.[34]

[34] Millican (1986) pp. 401-3 extends this line of thought by arguing that since the notion of natural necessity cannot even in principle legitimise induction, we can conclude that the notion itself is fundamentally incoherent. For if *A necessitates B*, then this should supply some explanatory account of

Since Hume's views about induction do not depend on his own analysis of the notion of causation, this naturally raises the question of why that notion should nevertheless feature so prominently in his famous argument, and whether it plays any essential role there. This question can only be straightforwardly addressed, however, in respect of the *Enquiry*, for in the *Treatise* and *Abstract* both the argument and its conclusion are explicitly presented as being fundamentally concerned with *causal* reasoning rather than with "probable reasoning" or "reasoning concerning matter of fact" (as observed in §1.2 above). Focusing on the *Enquiry*, then, and appealing to the structural analysis developed above, we can see that causation features importantly in Hume's argument at only two points: first, in Part i, where he uses it as a "middle term" for deducing that all probable reasoning is based on experience (propositions (1) to (6)); and secondly, at the beginning of Part ii (E33), where he appeals again to his earlier claim about our inability to perceive any connexion between objects' powers and their sensible qualities (proposition (3)), and goes on to draw the corollary that the Uniformity Principle cannot be justified on the basis of such perception (proposition (9)). Taking these two together, it seems that causation plays a role in Hume's argument only to the extent of enabling him to conclude *that inferences beyond the present testimony of our memory and senses (including inferences about the*

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why *B* follows *A*, but a "necessity" which cannot ground an inference to *B* provides no such account, because even if such a necessity is operative the conjunction between *A* and *B* remains at bottom an unexplained coincidence (the coincidence that *A* is conjoined with *the power to produce B*). Hume himself famously maintains that the idea of necessity is incoherent as applied to external objects, since it is an idea derived from an internal impression (T160-6). But the argument just given might provide him with a more reliable route to the same conclusion, because it does not in any way depend upon his dubious theory of ideas.

Uniformity Principle) cannot be drawn a priori from our immediate perceptions and hence must be based on past experience. However this proposition seems just as plausible in its own right without any mention of causation, and it can moreover be supported directly by most of the examples, and much of the argumentation, that he provides in Part i.

Hume's argument, therefore, can apparently be reconstructed without any essential mention of causation. And Hume himself might have welcomed such a reconstruction, for it would rid him of any dependence on his initial premise (1), about which he seems to have some doubts later in the *Enquiry* when in Section X he turns his attention to inferences based on human testimony. When these doubts arise, it is interesting and perhaps significant that he deals with them in exactly the way that would be required in order to permit such a reconstruction of his Section IV argument, for he makes no attempt to defend this premise, but instead simply remarks that it can be by-passed for his current purposes, on the grounds that any testimonial inference to the unobserved, even if it is admitted to be non-causal, must nevertheless be based on experience:

> This species of reasoning, perhaps, one may deny to be founded on the relation of cause and effect. I shall not dispute about a word. It will be sufficient to observe, that our assurance in any argument of this kind is derived from no other principle than our observation of the veracity of human testimony, and of the usual conformity of facts to the reports of witnesses. (E111)

This remark is tantalising, but unfortunately we shall probably never know whether Hume ever noticed its relevance to his argument concerning induction.

Having completed his “coda”, and with it his abstract philosophical arguments for the thesis that probable inferences are not founded on reason, Hume ends Section IV with a relatively down-to-earth parting shot:

> It is certain, that the most ignorant and stupid peasants, nay infants, nay even brute beasts, improve by experience, and learn the qualities of natural objects, by observing the effects, which result from them. ... If you assert, therefore, that the understanding of [a] child is led [to draw conclusions about the future] by any process of argument or rationcination, I may justly require you to produce that argument ... You cannot say, that the argument is abstruse, and may possibly escape your enquiry; since you confess, that it is obvious to the capacity of a mere infant. If you hesitate, therefore, a moment, or if, after reflection, you produce any intricate or profound argument, you, in a manner, give up the question, and confess, that it is not reasoning which engages us to suppose the past resembling the future, and to expect similar effects from causes, which are, to appearance, similar. This is the proposition which I intended to enforce in the present section. (E39)

This is effective rhetoric, but its philosophical significance is less clear, for of course the inductive rationalist is unlikely to claim that infants base their expectations on reason. Rather, he will concede that infants are supplied (by God, perhaps) with appropriate instincts which initially govern their thinking, but he will maintain that these instincts are, or can be, supplanted by reason as that faculty develops. Hume’s parting shot, then, has little force unless it is supplemented by other considerations such as the desirability of a simple and uniform theory of all human and animal reasoning. It is therefore worth noting that precisely this point is emphasised by Hume later in the *Enquiry*, in the important but relatively neglected Section IX, “Of the Reason of Animals” (itself a descendant of the similarly titled sixteenth and final section of Book I Part iii of the *Treatise*).

4.3 The Complete Structure of Hume's Argument

We can now at last put together a complete structure diagram of Hume's argument as it is presented in Section IV of the *Enquiry (Figure1)*. The argument is quite complex, and so to assist in appreciating its large-scale structure, the same diagram is then shown shaded in such a way as to identify the main "logical blocks" of the argument and the sections of this thesis in which they have been discussed *(Figure 1a)*. Finally, to make entirely transparent how the analysis is intended to correspond faithfully to Hume's actual words, there is a table setting out, for each numbered proposition in the structure diagram, those precise passages of the *Enquiry* text in which I have interpreted Hume as stating, more or less explicitly, that very proposition.

The next three pages are repeated in Appendix 2 at the end of the thesis for easy reference, together with corresponding diagrams and table for the significantly different version of the argument as Hume initially presented it in the *Treatise of Human Nature*. A brief comparison of the relevant diagrams should be sufficient to make clear some of the respects in which the *Enquiry* version of the argument indeed represents a very considerable improvement, justifying both Hume's revisions and our own predominant focus on the later work.

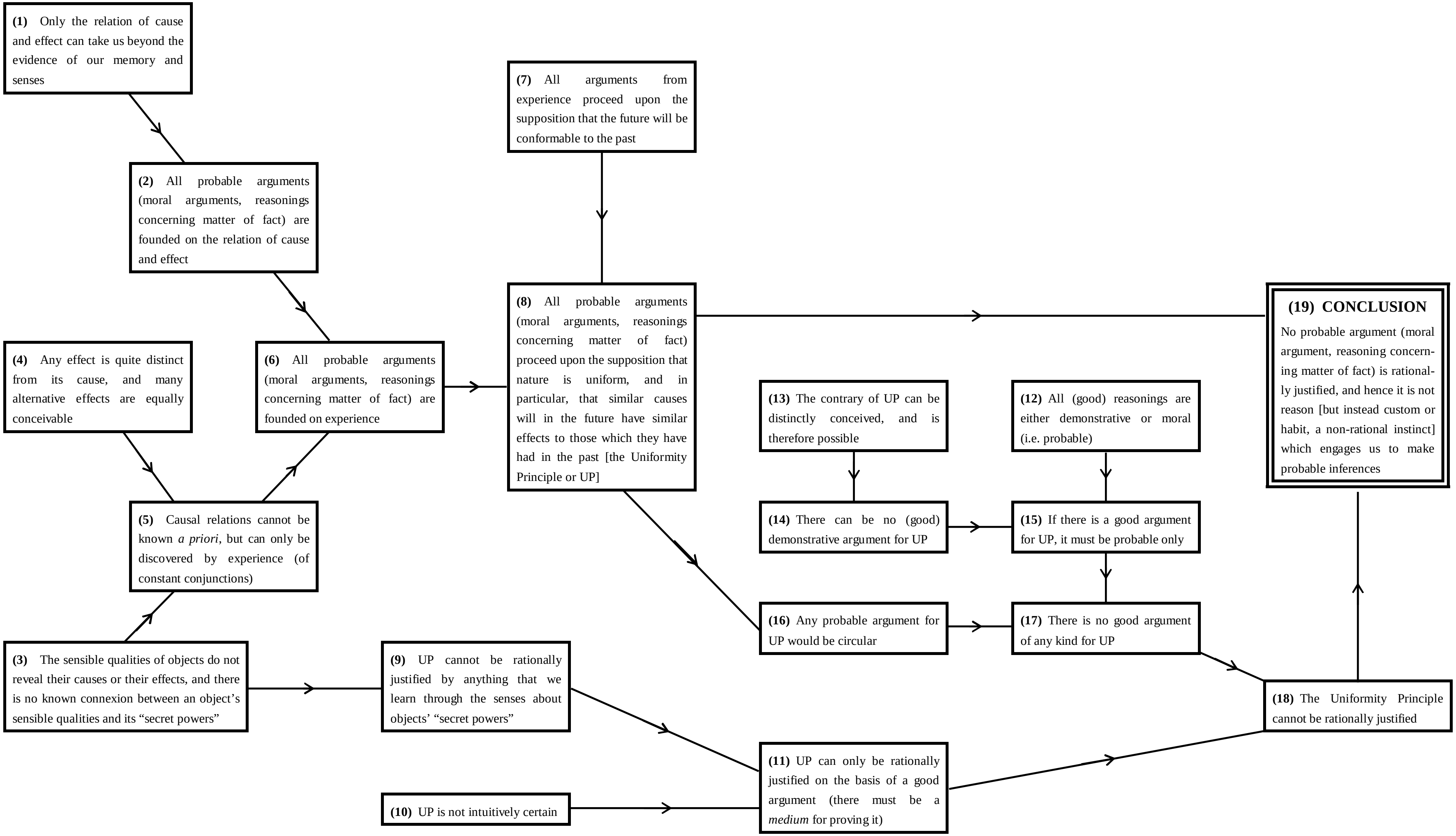

Figure 1 **Hume's Argument Concerning Induction (from the *Enquiry Concerning Human Understanding*)**

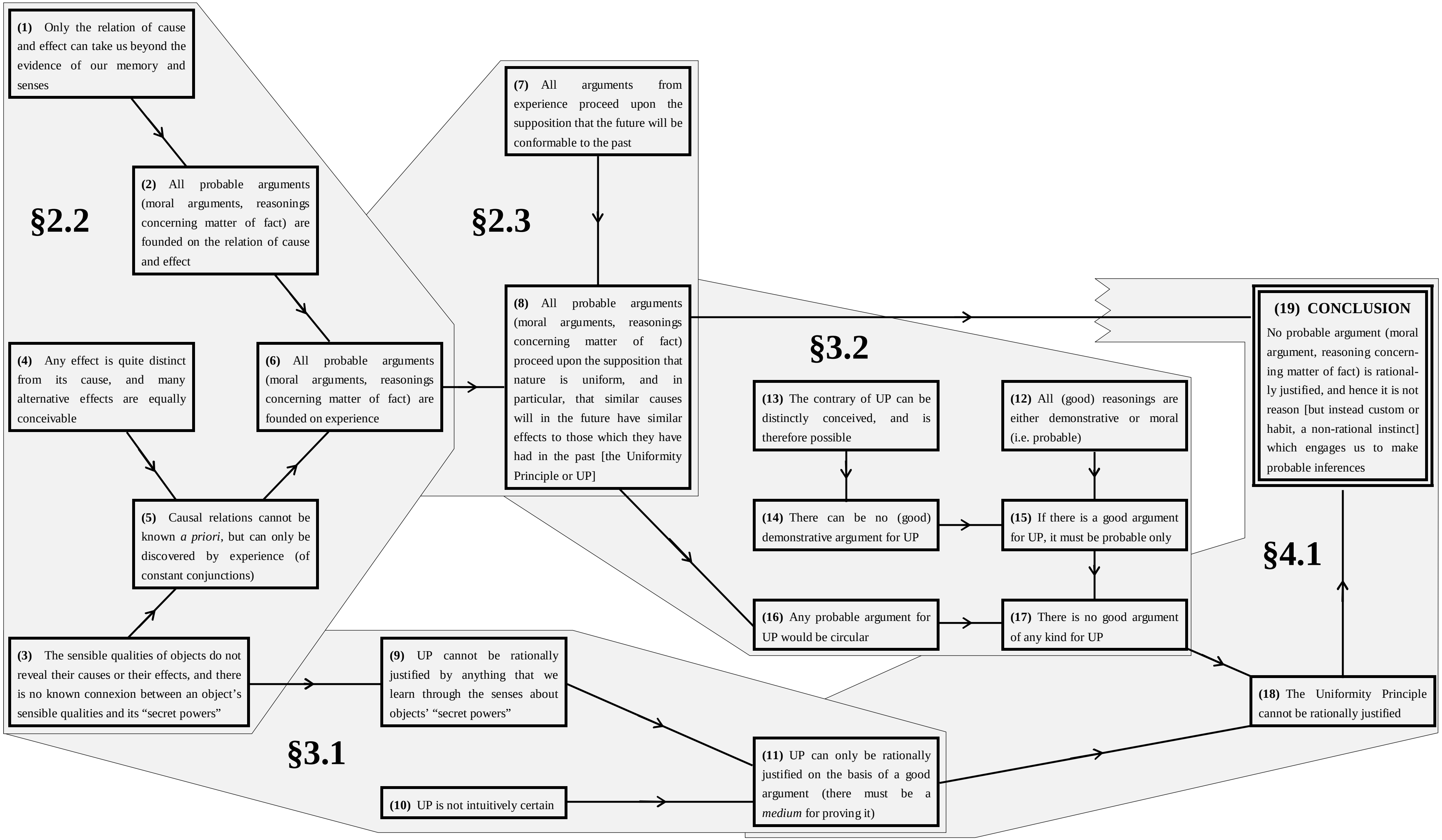

***Figure 1a* The Main Logical Blocks of the *Enquiry* Argument (as analysed in Sections 2.2 to 4.1)**

(1) E26: "By means of [*Cause and Effect*] alone can we go beyond the evidence of our memory and senses."

(2) E26: "All reasonings concerning matter of fact seem to be founded on the relation of *Cause and Effect*."

E35: "all arguments concerning existence are founded on the relation of cause and effect"

E159: "all our evidence for any matter of fact, which lies beyond the testimony of sense or memory, is derived entirely from the relation of cause and effect"

(3) E27: "No object ever discovers, by the qualities which appear to the senses, either the causes which produced it, or the effects which will arise from it"

E33: "It is allowed on all hands, that there is no known connexion between the sensible qualities and the secret powers"

(4) E30: "every effect is a distinct event from its cause. It could not, therefore, be discovered in the cause, and ... the conjunction of it with the cause must appear ... arbitrary; since there are always many other effects, which, to reason, must seem fully as consistent and natural."

(5) E27: "the knowledge of [cause and effect] is not, in any instance, attained by reasonings *à priori*; but arises entirely from experience"

E28: "*causes and effects are discoverable, not by reason, but by experience*"

E30: "In vain, therefore, should we pretend to ... infer any cause or effect, without the assistance of observation and experience."

(6) E27: "nor can our reason, unassisted by experience, ever draw any inference concerning real existence and matter of fact"

E30: "In vain, therefore, should we pretend to determine any single event ... without the assistance of observation and experience."

(7) E33: "we always presume, when we see like sensible qualities, that they have like secret powers, and expect, that effects, similar to those which we have experienced, will follow from them"

E35: "We have said, that ... all our experimental conclusions proceed upon the supposition, that the future will be conformable to the past"

E37: "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"

(8) E35: "We have said, that all arguments concerning existence are founded on the relation of cause and effect; that our knowledge of that relation is derived entirely from experience; and that all our experimental conclusions proceed upon the supposition, that the future will be conformable to the past."

(9) E33: "the mind is not led to form such a conclusion concerning [sensible qualities and secret powers'] constant and regular conjunction, by any thing which it knows of their nature"

(10) E34: "The connexion between these propositions [*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*] is not intuitive."

(11) E34: "There is required a medium, which may enable the mind to draw such an inference, if indeed it be drawn by reasoning and argument."

(12) E35: "All reasonings may be divided into two kinds, namely demonstrative reasoning, or that concerning relations of ideas, and moral reasoning, or that concerning matter of fact and existence."

(13) E35: "it implies no contradiction, that the course of nature may change ... May I not clearly and distinctly conceive [such a thing]?"

(14) E35: "That there are no demonstrative arguments in the case, seems evident"

E35: "whatever is intelligible, and can be distinctly conceived, implies no contradiction, and can never be proved false by any demonstrative argument or abstract reasoning *à priori*"

(15) E35: "If we be, therefore, engaged by arguments to put trust in past experience, and make it the standard of our future judgment, these arguments must be probable only, or such as regard matter of fact and real existence"

(16) E35-6: "To endeavour, therefore, the proof [that the future will be conformable to the past] by probable arguments, or arguments regarding existence, must be evidently going in a circle, and taking that for granted, which is the very point in question."

(17) E35: "it may be requisite ... to shew, that none of [the branches of human knowledge] can afford such an argument"

E159: "we have no argument to convince us, that objects, which have, in our experience, been frequently conjoined, will likewise, in other instances, be conjoined in the same manner"

(18) E39: "it is not reasoning which engages us to suppose the past resembling the future, and to expect similar effects from causes, which are, to appearance, similar"

E159: "nothing leads us to [expect constant conjunctions to continue] but custom or a certain instinct of our nature"

(19) E32: "I say then, that, even after we have experience of the operations of cause and effect, our conclusions from that experience are *not* founded on reasoning, or any process of the understanding."

E43: "All inferences from experience, therefore, are effects of custom, not of reasoning"

E46-7: "All belief of matter of fact or real existence [is due merely to] a species of natural instincts, which no reasoning or process of the thought and understanding is able, either to produce, or to prevent."

Hume's Statement of the Stages of the *Enquiry* Argument

Chapter 5 Stove's Analysis and his Interpretation of Hume's Conclusion

5.1 Stove's Structural Analysis and Hume's Alleged Deductivism

Now that we have a complete structure diagram of Hume's argument, an important task is to highlight and justify its differences from the well-known rival structure diagram devised by Stove (1973), which serves as the foundation of his influential analysis and which according to him provides powerful evidence that Hume is a deductivist. The two diagrams are in some respects hard to compare, because Stove focuses mainly on Hume's argument as it appears in the *Treatise* and *Abstract* (but see Appendix 2 for my own diagram of the former), and he intends his diagram to be "a composite photograph" (p. 30) of all three versions. So I shall here confine myself to relatively straightforward structural points whose relevance to both diagrams is obvious (using references such as "(8/e)" to signify a Humean proposition which in "translated" form is recognisably common to both). Having criticised Stove's diagram, I shall then go on to show how the structure of Hume's argument, so far from indicating that Hume is a deductivist, in fact suggests quite the reverse.

Stove's diagram is shown as *Figure 2* on the following page, in a form which is intended to display clearly not only his initial analysis of Hume's explicit argument structure, but also some important elaborations and interpretations of that structure which he develops in the course of his discussion.

Stove presents his structure diagram as a framework of letters, annotated by a separately listed "dictionary" identifying which proposition each letter represents. He gives two such dictionaries, the first (p. 31) in Humean language, and the second (p. 45) "translated" into modern philosophical terminology. To capture the spirit of both versions, whilst also endeavouring to making his diagram easier to follow, I have instead set out the propositions in boxes within the framework, generally preferring to follow his earlier Humean "dictionary" (in some cases slightly abbreviated) but adding his modernising "translations" in square brackets where these are particularly significant. I have also added to the diagram the two alleged suppressed premises which Stove takes to be implicit in Hume's argument, and the "fallibilist consequence" which is according to him the proper conclusion to be drawn from it – these three "unexpressed" propositions are distinguished here by shading. Note that Stove's own presentation of his diagram precedes this part of his analysis (pp. 46-50), and accordingly omits all three of these propositions – hence it does not contain what I have labelled (k), (l), (m) and their associated arrows, and instead represents proposition (j) as being inferred from (g) and (i) directly.

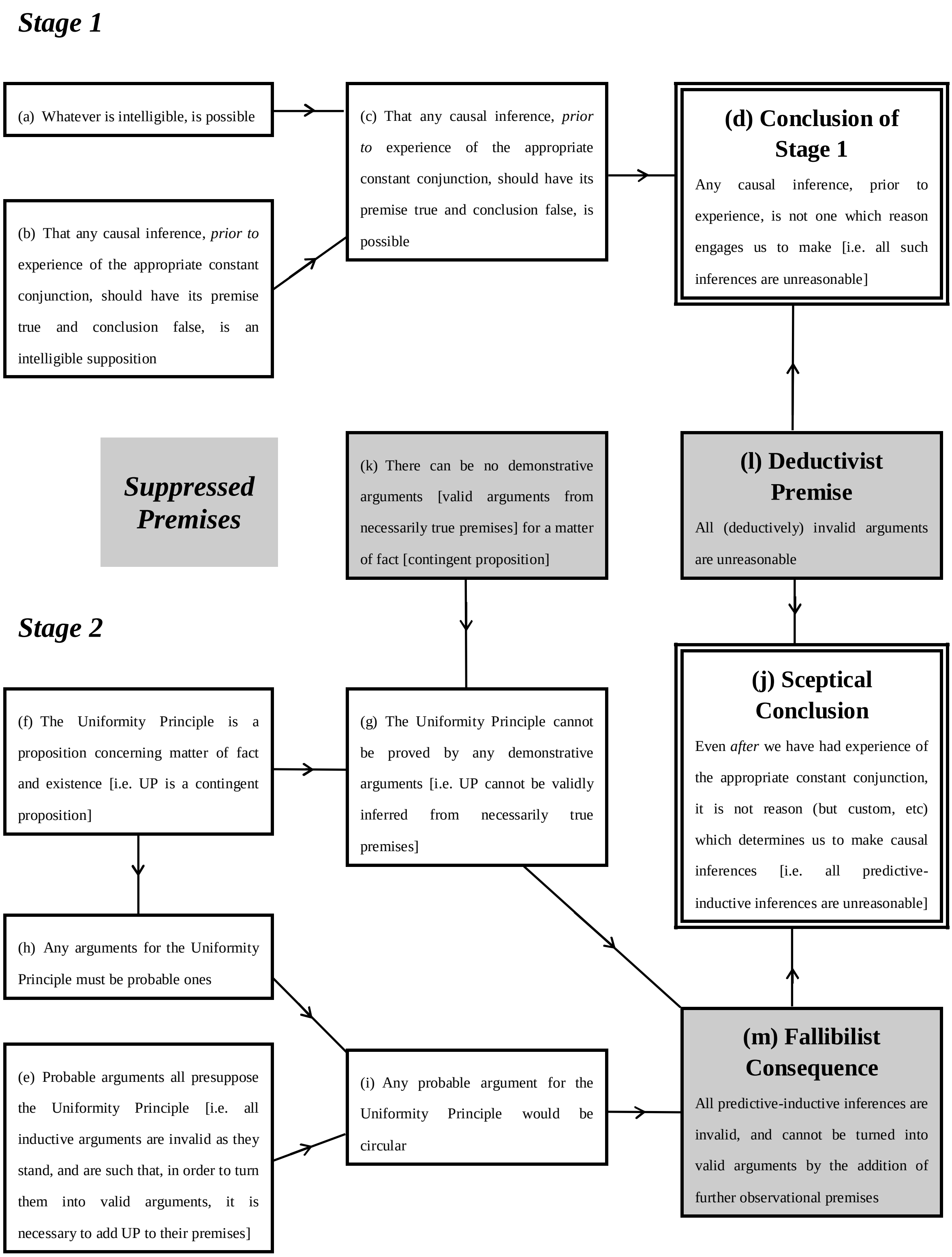

Figure 2 **Hume's argument according to Stove (1973)**

Some of the objections to Stove's analysis are relatively pedantic – for example, that he fails to distinguish consistently between Hume's talk in the *Treatise* and *Abstract* of inferences "from the impression to the idea" (shortened to "causal inferences" in the diagram) and his subtly different talk of "probable reasoning" (which in those two versions is confined to the discussion of arguments for the Uniformity Principle). Slightly more significant, perhaps, is that Stove completely misrepresents the logical role of proposition (h) as lying on the inferential route from (f) to (i), when in fact (i) clearly follows from (e) alone, and (h) should instead presumably lie between (g) and (m). But the main objection to Stove's diagram is his wholesale distortion of the overall structure of the argument in his effort to portray it as consisting of two independent stages: in fact, in all three versions *Hume argues for (5/d) only as a means of establishing that causal/probable inferences must be based on experience, and he then goes on to use this result as means of arguing for (8/e) and hence for (19/j), his ultimate conclusion*.[35] So Stove's (d), which he represents as the conclusion of "Stage 1", and his (e), which he represents as a premise of "Stage 2", really both correspond to intermediate steps in Hume's single and thoroughly integrated argument (whose integrated structure is further obscured in Stove's diagram by his failure to indicate the pivotal role of (e) in inferring (j)/(m) from (g)/(h) and (i)).

[35] Passages making relatively explicit the inference from (5/d) to (8/e) are T87 "'Tis therefore ... past experience" and T88-9 "Since it appears ... uniformly the same"; A651 "It would have been ... uniformly the same"; E35-6 "We have said ... point in question".

If Hume's argument does not in fact consist of two independent stages, then Stove's interpretative procedure, of appealing to a supposed parallel between Hume's reasoning for the alleged Stage 1 conclusion and his reasoning for the argument's overall conclusion, is largely undermined (quite apart from the doubts expressed in §2.2 above concerning Stove's interpretation of "Stage 1" itself). And this is significant, because despite the length at which Stove has written on Hume's argument, this two-stage analysis represents his only major item of evidence for Hume's alleged deductivism which can purport to be grounded independently of other controversial aspects of his interpretation. The other pieces of evidence on which he lays greatest weight (1965 pp. 205, 209-10; 1973 pp. 43-4) are his deductivist explication of Humean presupposition; his assertion that any circular argument must be valid and hence that at (16/i) Hume must have in mind only valid probable arguments; his explanation of why a deductivist Hume would reject an argument whose presupposition fails; and his general claim to have made sense of the structure of Hume's reasoning. But it is clear that all of these points are highly mutually dependent, and are therefore relatively easy to dismiss all together if a coherent alternative account is available. If the arguments above (especially in §2.5 and §3.2) are correct, then there is indeed such an alternative account, and one that is very much more faithful to Hume. Let us now therefore turn back to this account, and put to it the question which Stove puts to his own: does the analysis of Hume's argument which it yields indicate that he is, or is not, a deductivist?

There are at least two reasons why the argument structure represented in *Figure 1* is hard to square with the supposition that Hume was a deductivist. First, it is obviously a fairly complicated argument, which proceeds by carefully identifying a general presupposition of probable reasoning (8), and then systematically eliminating all the potential sources of rational support for this presupposition ((9), (10), (14) and (16)). But if Hume were indeed a deductivist, then such a complicated structure would be entirely unnecessary, for a deductivist recognises only one degree of rational support, namely deductive certainty, and Hume is well aware that such certainty can be ruled out directly with his argument from distinct conceivability – he uses it himself for just this purpose at stage (13). Stove, as we have seen (§2.2), interprets Hume as deploying another version of this argument earlier on, at stage (4). But he completely fails to observe that a small adaptation of this version would have been quite sufficient to give a deductivist Hume a "hole in one", reaching his final conclusion (19) with a minimum of effort:

	(a)	Whatever is intelligible, is possible.
	(b)	That any inference from observed to unobserved should have all its premises true and conclusion false, is an intelligible supposition.
Therefore	(c)	That supposition is possible.
And hence	(d)	No inference from observed to unobserved is one which reason engages us to make.

It is hard to believe that an argument so straightforward, obvious, and Humean in spirit (cf. E164, D189) would have evaded the great sceptic's scrutiny if it had been sufficient for his purpose.

The second reason for rejecting a deductivist interpretation of the argument in *Figure 1* is related to the first, and concerns Hume's procedure when examining the potential sources of inferential justification for the Uniformity Principle. With proposition (12) he states that all reasonings are either demonstrative or probable, and he goes on to rule out each of these in turn as providing a possible rational basis for the Uniformity Principle. But the crucial question is this: if Hume is a deductivist, and accordingly assumes from the start that only demonstrative reasonings are rational, then why does he even consider the possibility that probable reasoning might justify the Uniformity Principle? From the point of view of a deductivist, a probable justification is no justification at all. The fact that Hume is prepared to canvass such a justification, even though he of course ultimately rules it out on the grounds of circularity, very strongly suggests that he was no deductivist.

5.2 Stove's Probabilistic Interpretation of Hume's Conclusion

We have by now seen ample reason to reject Stove's analysis of Hume's argument, but this in itself does not imply that we can reject his formal interpretation of Hume's conclusion, which he works out (1973 pp. 53-62) somewhat independently of his earlier analysis. This interpretation has, moreover, provided the basis of a number of would-be "refutations of inductive scepticism" (e.g. Stove 1973 chapter 5, Stove 1986 chapter V, Gemes 1983), and is therefore worth considering in its own right. Stove starts from the assumption (argued earlier in his book at pp. 33-4) that Hume's conclusion can be paraphrased as "All predictive-inductive inferences are unreasonable", and he takes this to mean that in any predictive-inductive inference the premises (which concern observed objects) provide no support whatever for the conclusion (which concerns unobserved objects). This claim Stove interprets as a probabilistic "judgement of irrelevance" (p. 59) – a judgement that the existence of "past" (i.e. observed) objects which satisfy some condition in no way affects the probabilility that "future" (i.e. unobserved) objects will also satisfy that condition. If we restrict our attention to a particular condition, *F*, one past object, *a*, and one future object, *b*, then such a judgement of irrelevance of the past to the future ("IPF") can be expressed formally as follows:[36]

$$P(Fb/Fa) = P(Fb) \qquad \text{IPF}$$

That is, *the probability that Fb is true given that Fa is true* is exactly the same as *the initial probability that Fb is true* (the latter signifying the probability that *Fb* is true in advance of observing that *Fa* is true). This indeed seems a reasonable way of expressing Hume's "judgement of irrelevance" in probabilistic terms, if that is one's aim.[37]

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[36] I have simplified Stove's exposition here by omitting all references to "tautological" or other background evidence, so that for example the probability of *Fb* on tautological evidence (i.e. on the basis of no empirical evidence whatever) is shown as simply P(*Fb*).

[37] Particularly in view of the discussion in §3.2 above, which highlighted how Hume sees probability as essentially *a posteriori*, it must remain highly debatable whether he would have accepted the controversial notion of "logical probability" which Stove here presupposes. Nevertheless it is, I believe, worth seeing how far Hume's position can be explicated within such a framework.

Stove however extends this interpretation by stating that Hume (although he "never discusses explicitly ... *universal*-inductive inferences", p. 28) obviously "intended the sceptical conclusion which he drew about predictive-inductive inferences to be drawn also about universal-inductive ones" (p. 61). Stove accordingly takes Hume's conclusion about universal-inductive inferences to be also a simple judgement of irrelevance, and if we again restrict ourselves to our two-object universe, he takes this to have the implication that according to Hume the probability of the "universal" proposition (Fa & Fb) is in no way affected by the observation of Fa. Let us call this particular judgement of irrelevance "Hume's Inductive Scepticism according to Stove", or "HISS" for short.

$$P(Fa \mathbin{\&} Fb/Fa) = P(Fa \mathbin{\&} Fb) \qquad\qquad \text{HISS}$$

Stove's "refutation" of Hume involves a formal proof that where F is an empirical predicate which might or might not apply to each of the objects a and b individually, HISS has implications which are clearly unacceptable both to the inductivist and to the inductive sceptic. This proof he dignifies with the name of "von Thun's argument" (p. 68).

Von Thun's argument is unnecessarily complicated, extending over an entire page and using four "principles of logical probability" together with two inequality statements each of which asserts that some contingent proposition has initial probability less than 1. Two of the principles used in the argument are the Equivalence Principle, which states "that logically equivalent propositions can be substituted for one another *salva probabilitate* in statements and in principles of logical probability", and the Conjunction Principle, which states that for any propositions q and r,

$P(q \,\&\, r) = P(q) \times P(r/q)$. There is in fact a much simpler way of showing that HISS is unacceptable to both inductivist and sceptic, in a mere three steps and using only these two principles:

	$P(Fa \,\&\, Fb/Fa) = P(Fa \,\&\, Fb)$	HISS
	$= P(Fa \,\&\, (Fa \,\&\, Fb))$	Equivalence Principle
	$= P(Fa) \times P(Fa \,\&\, Fb/Fa)$	Conjunction Principle
$\therefore$	*Either* $P(Fa \,\&\, Fb/Fa) = 0$ *or* $P(Fa) = 1$	Arithmetic

To accept this conclusion would be to embrace either a wildly implausible counter-inductivism, or an equally absurd claim to *a priori* knowledge of the supposedly empirical proposition *Fa*. So HISS is indeed refuted.

The fact that HISS can be so easily refuted might prompt the suspicion that it is not after all a legitimate extension of the relatively innocuous IPF. And this suspicion would be well-founded, for whereas IPF represents a judgement of irrelevance of a past proposition *Fa* to a purely future proposition *Fb*, HISS represents a far less plausible judgement of irrelevance, of a past proposition *Fa* to a past-and-future proposition (*Fa* & *Fb*) which, so to speak, *overlaps with Fa* (i.e. the truth of the "premise" *Fa* already implies that the "conclusion" (*Fa* & *Fb*) is partially fulfilled). Someone who accepts HISS is, for example, committed to saying that if I am playing backgammon and throw one of my two dice before the other, the proposition that *the first die lands showing a "6"* has no impact whatever on the probability of the proposition that *after the second die has landed the two dice together will have shown two "6"s*. This is manifestly absurd on any account, and is certainly not a logical extension of IPF as Stove appears to believe. His *only* ground for viewing it in this way, as we have seen, is his claim that Hume "intended the sceptical conclusion which he drew about predictive-inductive inferences to be drawn also about universal-inductive ones", but as an argument for a particular formal interpretation of Hume this is quite feeble. For the judgement of irrelevance IPF instantiates more than one probabilistic schema, and it can therefore be generalised to other instances in more than one way. Stove (p. 61) chooses to see it as an instance of the schema:

(S1) *If E and H are such that the inference from E to H is inductive, then P(H/E) = P(H)*

But he gives no reason at all for preferring this very simplistic schema (which would however be significantly less simple when supplemented with a definition of "inductive") to other possibilities, such as the only slightly more complicated:

(S2) *If O and E logically imply no unobserved matter of fact, and H logically implies no observed matter of fact, then P(O & H/O & E) = P(H)*

This separates out the "observed" (*O*) and "unobserved" (*H*) components in the conclusion of any inductive argument, and reduces to Stove's (S1) where *O* is merely tautologous: it thus indeed subsumes IPF (with *O* tautologous, *E* substituted by *Fa* and *H* substituted by *Fb*). But in providing an assessment of the probability P(*Fa* & *Fb*/*Fa*), instead of yielding the preposterous result HISS it generates instead (with O substituted by Fa, E tautologous and H substituted by Fb) the far more Humean:

$$P(Fa \ \& \ Fb/Fa) = P(Fb)$$

It likewise generalises quite easily to all “predictive-inductive” and “universal-inductive” arguments, enabling us to express “the same sceptical conclusion about all of them” without running into any obvious absurdities. As a probabilistic schema of inductive scepticism it is vastly more plausible than Stove’s, for not only does it express quite straightforwardly the irrelevance of the observed to the unobserved – it is also, I believe, just as difficult to refute as inductive scepticism itself.

Chapter 6 Hume, Scepticism, and Reason

6.1 Is Hume an Inductive Sceptic?

Having now seen in §5.2 a relatively precise statement of what extreme inductive scepticism amounts to – namely the total evidential irrelevance of observed to unobserved – it is appropriate to ask whether this can properly be seen as the substance of Hume's own conclusion, which he certainly expresses in apparently sceptical language: "... 'tis impossible for us to satisfy ourselves by our reason, why we shou'd extend ... experience beyond those particular instances, which have fallen under our observation." (T91); "our conclusions from ... experience are *not* founded on reasoning, or any process of the understanding" (E32). Clearly too my above analysis of Hume's famous argument suggests a sceptical reading, for it treats that argument as pivoting around the Uniformity Principle, which it takes to be essentially a statement of evidential relevance whose rational credentials are then examined and apparently found wanting. Further seemingly strong textual support for the traditional sceptical interpretation extends well beyond the immediate context of the argument:

> Thus all probable reasoning is nothing but a species of sensation ... When I give the preference to one set of arguments above another, I do nothing but decide from my feeling concerning the superiority of their influence. (T103)

> *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* (T139)

> nothing leads us to this inference but custom or a certain instinct of our nature; which it is indeed difficult to resist, but which, like other instincts, may be fallacious and deceitful. (E159)

> we cannot give a satisfactory reason, why we believe, after a thousand experiments, that a stone will fall, or fire burn (E162)

The problem is that this extreme scepticism seems incompatible not only with a significant number of statements in favour of experimental reasoning that Hume makes in many different places, but also with the structure and logic of several important arguments in the *Treatise* and *Enquiry*, and even more fundamentally, with the general thrust of his empiricist philosophical project. Here first are some quotations that are at least problematic for the traditional interpretation:

> We infer a cause immediately from its effect; and this inference is not only a true species of reasoning, but the strongest of all others (T96n)

> One who concludes somebody to be near him, when he hears an articulate voice in the dark, reasons justly and naturally; tho' that conclusion be deriv'd from nothing but custom (T225)

> One, who in our climate, should expect better weather in any week of JUNE than in one of DECEMBER, would reason justly, and conformably to experience ... A wise man ... proportions his belief to the evidence. (E110)

The last of these is from Section X of the *Enquiry*, an essay whose entire argument centres on the principle that probable reasonings, and in particular those from testimony, can vary in force, implying that they are not all worthless: "the evidence, resulting from ... testimony, admits of a diminution, greater or less, in proportion as the fact is more or less unusual." (E113). Likewise in the *Treatise* another of Hume's most notorious arguments, concerning "scepticism with regard to reason" (I iv 1), depends completely on the idea that the force of a probable argument can diminish by gradual degrees (T181-3), while in both works Hume's discussions of "the probability of chances" and "the probability of causes" (*Treatise* I iii 11-12, *Enquiry* VI) apparently presuppose that probable arguments can be less than certain though still in some sense respectable. The evidence of these special discussions of probability is admittedly somewhat equivocal, for it is arguable (cf. Stove 1973 p. 120) that Hume's primary purpose in these sections is simply to explain psychologically why "philosophers" judge probable arguments as they do, rather than himself to endorse those judgements.[38] And perhaps a similar dismissal could even be given of *Treatise* I iii 15, where Hume presents his "rules by which to judge of causes and effects" which purport to distinguish between good and bad probable reasonings. However those who take Hume himself to believe that probable arguments cannot *genuinely* vary in force will have more difficulty explaining away the contrast between demonstrations and

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[38] Hume's detached language at the beginning of *Treatise* I iii 13, "Of unphilosophical probability", provides at least a hint that he is here explaining the opinions of others rather than expounding his own: "All these kinds of probability are receiv'd by philosophers, and allow'd to be reasonable foundations of belief and opinion. But there are others, that are deriv'd from the same principles, tho' they have not had the good fortune to obtain the same sanction." (T143)

probabilities which he draws quite explicitly at T31, clearly speaking *in propria persona*:

> ’Tis not in demonstrations as in probabilities, that difficulties can take place, and one argument counter-ballance another, and diminish its authority. A demonstration, if just, admits of no opposite difficulty; and if not just, ’tis a mere sophism, and consequently can never be a difficulty. ’Tis either irresistible, or has no manner of force.

It is likewise hard to deny that Hume is in earnest when he advocates in the very subtitle of the *Treatise* the introduction of “the experimental method of reasoning into moral subjects” (Txi), or when he proceeds to apply this method himself not only in the *Treatise* but also in particular in *Enquiry* X and XI and in the *Dialogues*, where he vigorously attacks natural theologians for failing to “proportion their belief” to the *empirical* evidence.

We are thus faced with a major interpretative puzzle. For on the one hand Hume argues, forcefully and repeatedly, that “we have no reason” whatever to make inductive inferences; while on the other he continues to make such inferences himself, treats them as varying in force, presents rules for assessing them, describes some as “just” and “true”, and criticises natural theologians and others (including “the vulgar”) for failing to conform to the appropriate standards. This inconsistency, it should be noted, is far more problematic than the superficially similar inconsistencies that Hume famously admits to when he acknowledges his psychological inability to relinquish various other rationally unwarranted beliefs (e.g. T187, 218, 265-70). If a simple conflict between reason and belief were the only problem, then his complete answer would be clear and straightforward:

> Nature, by an absolute and uncontroulable necessity has determin'd us to judge as well as to breathe and feel; nor can we any more forbear viewing certain objects in a stronger and fuller light, upon account of their customary connexion with a present impression, than we can hinder ourselves from thinking as long as we are awake, or seeing the surrounding bodies, when we turn our eyes towards them in bright sunshine. (T183 cf. E46-7)

But although such an appeal to natural weakness can indeed explain, and even pragmatically justify, our continuing to make irrational inferences and to infer unwarranted beliefs, it is hard to see how it can provide any basis for the sort of *normative* claim that Hume makes about these supposedly irrational inferences: that we *ought* to adhere to a particular set of rules when making them, or that those which are made in accordance with these rules are somehow *better* than others. Surely such normative claims require that the "superior" inferences have more than mere psychological compulsion to recommend them – that they be, at least to a minimal extent, and in some sense, *rationally well-founded*. The only way that Hume can maintain a consistent position, therefore, is to acknowledge a species of rationality distinct from that which he denies to inductive inferences. Fortunately, he does just this; but regrettably, the account which he provides of it is extremely sketchy and unclear.

6.2 Three Senses of "Reason"

Hume's use of the faculty term "reason", and of what seems (judging by his use of it) to be a synonym, "the understanding", can seem frustratingly inconsistent. Sometimes, as in the title of the *Enquiry* and of Book I of the *Treatise*, and of the section in each work which discusses "the reason of animals", he uses them to speak neutrally and naturalistically of "the reasoning faculty of brutes, as well as that of human creatures" (T176) – in this sense reason is the faculty whose role is to ascertain truth and falsehood (T415-6, 458), but it can nevertheless be "deceitful" (T180), "fallacious" (E55), "weak" (T182, E72, 76, 158), "infirm" (E161), and even "blundering" (T587).

At other times, most notably in the argument concerning induction itself but also elsewhere (e.g. T212, E138), Hume uses the two terms in a significantly different way (or ways), for here he takes them to refer to a faculty whose operations are guaranteed to be in some sense reliable, so that beliefs or inferences whose credentials are defective can confidently be ascribed to a different source: "This sentiment, then, as it is entirely unreasonable, must proceed from some other faculty than the understanding" (T193); "'tis a false opinion ... and consequently ... can never arise from reason" (T209). Typically in these cases, it is the imagination ("the fancy") which is attributed with the questionable beliefs and inferences – indeed Hume here seems to treat the attribution of a belief or inference to the imagination (or to "custom", which acts on the imagination – E48, 106) as virtually equivalent to the statement that it is in some way rationally defective.

Given this explicit contrast between reason and the imagination, it is somewhat surprising to find Hume apparently identifying the two faculties as one and the same: “the understanding or imagination can draw inferences from past experience” (T104). Nor is this an isolated anomaly, for Hume repeatedly attributes inferences from experience to “reason” or “the understanding” even though he has previously, and emphatically, established in his famous argument that any such inference is drawn by the imagination and “proceeds not from reason” (E54)! The following quotation, for example, seems dubiously consistent with the conclusion of the argument concerning induction:

> It has been observ’d, that reason, in a strict and philosophical sense, can have an influence on our conduct after only two ways: Either when it excites a passion by informing us of the existence of something which is a proper object of it; or when it discovers the connexion of causes and effects, so as to afford us means ... (T459, cf. T414, ME285)

It might be supposed that Hume (despite his use of the word “strict”) has here reverted from what we can call the “rigorous” sense of “reason” employed in his famous argument to the neutral sense in which it encompasses all “reasonings” no matter what their rational credentials. But in fact he appears to be using the term in a sense intermediate between his neutral and his rigorous senses:[39]

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[39] By suggesting a three-way ambiguity I here go further than Barbara Winters (1979), who importantly argued that Hume uses the term “reason” in (at least) two main senses (a view that later became very fashionable, as alluded to in §2.4 above). Note that in addition to the three senses discussed here, Hume also points out (or uses himself) various “improper” senses, which add to the potential for confusion: e.g. T414-8, 437-8, 536, 546, 583, E43n, ME239.

> In general we may observe, that as our assent to all probable reasonings is founded on the vivacity of ideas, it resembles many of those whimsies and prejudices, which are rejected under the opprobrious character of being the offspring of the imagination. By this expression it appears that the word, imagination, is commonly us'd in two different senses; and tho' nothing be more contrary to true philosophy, than this inaccuracy, yet in the following reasonings I have often been oblig'd to fall into it. When I oppose the imagination to the memory, I mean the faculty, by which we form our fainter ideas. When I oppose it to reason, I mean the same faculty, excluding only our demonstrative and probable reasonings. (T117n)

This footnote, which expands on the note at T371n, was specially inserted by Hume while the *Treatise* was going through the press – hence some copies of the first edition of Book I include it, while others do not. That it carries such emphatic authority is fortunate, because it indeed seems to provide an important clue as to what is going on. Although Hume has argued that probable inferences are ultimately founded on the imagination rather than on reason (in the rigorous sense), he is reluctant to see such inferences tarred with the same brush as the "whimsies and prejudices" that are considered to be the imagination's more typical offspring. He accordingly defines a *restricted* sense of "the imagination", according to which that faculty includes only the epistemologically less respectable mental operations, and this restriction implies that probable reasoning must be relocated instead into the province of the understanding, whose scope is thereby *extended*. Hume does not quite explicitly acknowledge this implication from a restriction of the one faculty to an extension of the other, but he nevertheless plainly endorses it, for the very same sentence which spells out the restricted sense of "imagination" begins by making clear that its whole point is to "oppose [the imagination] to reason". The upshot of all this is that Hume has implicitly specified a third sense of "reason" intermediate between the neutral and rigorous senses, which includes within its domain both demonstrative and probable inference, but excludes the less respectable operations of the mind that he elsewhere describes as "deceitful", "fallacious", and "blundering". In this new intermediate sense, therefore, "the understanding" overlaps with what he had previously called "the imagination", and might even be totally contained within it given that in the *Treatise* at least, all reasonings whatever are found to depend ultimately on the vivacity of ideas (T96n, 140, 184, 265-8). Hence having inserted his clarificatory footnote Hume can speak somewhat misleadingly, but now without blatant self-contradiction, of "the understanding, that is, ... the general and more establish'd properties of the imagination" (T267).

This new intermediate sense of "reason" provides the resolution of our puzzle regarding Hume's apparently ambivalent attitude towards induction, for it is in this sense that he unequivocally takes (suitably disciplined) probable inferences to be entirely rational. But this superficially non-sceptical position seems completely insubstantial if its only basis is a stipulative extension of the concept of reason: it is hard to see how such a redefinition can provide the normativity that we have seen Hume requires for his critical enterprise of distinguishing between good and bad reasoning, and between science and superstition. His apparent attempt at a persuasive definition seems, so far, to be entirely arbitrary – having discovered that probable (and maybe even demonstrative) reasoning is in fact the product of the imagination, he has chosen to dignify this particular operation of the imagination rather than others with the honorific term “reason”, but has not apparently provided any good grounds for drawing such a distinction amongst those operations.

Hume addresses this difficulty most directly in the *Treatise*, immediately after the section in which he has criticised “the antient philosophy” for being founded on “every trivial propensity of the imagination” (T224).

> But here it may be objected, that the imagination, according to my own confession, being the ultimate judge of all systems of philosophy, I am unjust in blaming the antient philosophers for makeing use of that faculty, and allowing themselves to be entirely guided by it in their reasonings. In order to justify myself, I must distinguish in the imagination betwixt the principles which are permanent, irresistable, and universal; such as the customary transition from causes to effects, and from effects to causes: And the principles, which are changeable, weak, and irregular; such as those I have just now taken notice of. The former are the foundation of all our thoughts and actions, so that upon their removal human nature must immediately perish and go to ruin. The latter are neither unavoidable to mankind, nor necessary, or so much as useful in the conduct of life; but on the contrary are observ’d only to take place in weak minds, and being opposite to the other principles of custom and reasoning, may easily be subverted by a due contrast and opposition. For this reason the former are received by philosophy, and the latter rejected. (T225)

Assuming that the ancient philosopher’s theory is not actually self-contradictory, it cannot be refuted purely by an appeal to reason in the rigorous sense. But this does not mean that any judgement of it must be arbitrary, since even the principles of the ancient philosopher’s own imagination will include causal reasoning and induction. These are unavoidable, universal and irresistable, so if his theory of “*sympathies, antipathies, and horrors of a vacuum*” (T224) or whatever yields conclusions that conflict with the results of causal reasoning, then the principles on which he founds this theory can be "subverted by a due contrast and opposition". Even the ancient philosopher ought to be consistent, so the universality of causal reasoning enables us to condemn him out of his own mouth.

We should note that it is not primarily the ancient philosopher's *theory* which is subverted by its conflict with natural causal reasoning, but rather the *principles* on which that theory is founded. It is this that grounds Hume's demarcation amongst the operations of the imagination, and hence gives him a weapon not only against those theories which directly contradict the results of causal reasoning, but also against any other theories which may be founded on similar principles. We can see the sketch of a systematic investigation into the various belief-forming operations of the imagination in Sections 9 to 13 of the *Treatise* Book I Part iii, although Hume's central concern in these sections seems to be with psychological explanation and corroboration of his theory of belief rather than with normative demarcation. In Section 9, "Of the effects of other relations and other habits", he discusses operations that are dependent on resemblance and contiguity, the non-causal associative principles, and he criticises caprice, credulity, "education" (indoctrination) and so on as means of forming beliefs because they so often lead us astray. He then turns his attention to causal reasoning, first outlining in Sections 11 ("Of the probability of chances") and 12 ("Of the probability of causes") those reliable methods of causal reasoning which "are receiv'd by philosophers" (T143), and then going on in Section 13 ("Of unphilosophical probability") to describe the various unreliable methods which are not "receiv'd". Again his objection to the "unphilosophical" principles (judging only be the recent and near, prejudice, self-deception) is that they regularly lead us astray, and so to avoid inconsistencies in our causal reasonings he recommends that we make systematic and critical use of *general rules* (T149), not in a way that encourages prejudice, but instead in a way that enables them to be modified, to take account of exceptions as they arise, in a logical and methodical manner. The appropriate "logic" (T175) is given by his "rules by which to judge of causes and effects", which he spells out later in Section 15.

Hume thus has the basis for a naturalistic account of his intermediate sense of "reason", according to which beliefs and methods of inference count as reasonable if they have a place within a consistent and systematic rule-governed framework dominated by the "permanent, irresistable, and universal" principles of the imagination, and in particular by the fundamental belief in inductive uniformity and the rules by which to judge of causes and effects which systematise its implications. Hume can, of course, give no independent justification for this fundamental belief itself, but fortunately its inevitability entails that none is needed. The difference between "the wise" and "the vulgar", therefore, or between "philosophers" and the superstitious, lies not in the reasonableness of their belief in uniformity, but only in how systematically they pursue its consequences: "philosophical decisions are nothing but the reflections of common life, methodized and corrected." (E162). Crucially, however, this seems to be just enough for Hume's critical purposes, because systematic consistency can be assessed by reason in the rigorous sense, and thus provides a non-circular and non-arbitrary norm of "rationality" in this looser sense. But why should anyone care about consistency with an unfounded belief? Hume's answer seems to be that we are naturally motivated by curiosity or "the love of truth" (T448), which achieves some satisfaction from the working out of a systematic theory, and which is understandably focused by our inevitable assumption that the world is, truly, uniform.

In the *Treatise*, this tidy and relatively comfortable position is unfortunately not maintained, because Hume finds to his dismay that his distinction between the "permanent, irresistable, and universal" properties of the imagination and those that are "changeable, weak, and irregular" breaks down under examination, as only the latter can provide an escape from his otherwise all-consuming "scepticism with regard to reason":

> [If we] take a resolution to reject all the trivial suggestions of the fancy, and adhere to the understanding, that is, to the general and more establish'd properties of the imagination; even this resolution, if steadily executed, wou'd be dangerous, and attended with the most fatal consequences. For I have already shewn, that the understanding, when it acts alone, and according to its most general principles, entirely subverts itself, and leaves not the lowest degree of evidence in any proposition, either in philosophy or common life. (T267-8)

In the *Enquiry* and the *Dialogues*, by contrast, this extreme scepticism makes no significant appearance,[40] leaving Hume with a far more satisfactory position in which he can combine the result of his argument concerning induction with a healthy respect for good, systematic, scientific inductive reasoning:

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[40] In the *Enquiry* Hume dismisses extreme "antecedent" scepticism (E149-50) on the straightforward ground that such thoroughgoing Cartesian doubt about one's own faculties would be paralysingly incurable. He also somewhat downplays his sceptical arguments regarding the external world (E151-5), which in the *Treatise* (T187-218, 226-31, 265-6) raise additional serious difficulties about the consistency of the "general and more establish'd properties of the imagination".

> To whatever length any one may push his speculative principles of scepticism, he must act, I own, and live, and converse like other men; and for this conduct he is not obliged to give any other reason than the absolute necessity he lies under of so doing. If he ever carries his speculations farther than this necessity constrains him, and philosophises, either on natural or moral subjects, he is allured by a certain pleasure and satisfaction, which he finds in employing himself after that manner. He considers besides, that every one, even in common life, is constrained to have more or less of this philosophy; that from our earliest infancy we make continual advances in forming more general principles of conduct and reasoning; that the larger experience we acquire, and the stronger reason we are endowed with, we always render our principles the more general and comprehensive; and that what we call *philosophy* is nothing but a more regular and methodical operation of the same kind. To philosophise upon such subjects is nothing essentially different from reasoning on common life; and we may only expect greater stability, if not greater truth, from our philosophy, on account of its exacter and more scrupulous method of proceeding. (D134)

It is a great shame that Hume said little, in his later works, on the demarcation between good and bad reasoning. But these hints (and others) in Part I of the *Dialogues*, and in Part XII of the *Enquiry* (e.g. E162), are enough to indicate the outlines of a mitigated scepticism that is well worth taking seriously, and whose power in distinguishing "science" from "superstition" is elegantly illustrated by his own deployment of it in his attacks on natural theology. After the *Treatise*, apparently, Hume preferred using his tools to sharpening them.

6.3 "Reason" in the Argument Concerning Induction

Although we are now relatively clear on the sense in which Hume believes induction to be "reasonable", a little more needs to be said on the sense in which he does not. In particular, we must ask whether the rigorous sense of reason which he employs in his famous argument is of wider significance, or whether it is simply a straw-man "rationalist" sense of reason, invoked only for the purpose of showing how utterly impotent it is, so that such rationalism can thereby be reduced to absurdity. The latter position has been most prominently maintained by Beauchamp, Mappes and Rosenberg (1975, 1981), who can perhaps largely be credited with having brought about the now widespread recognition that Hume's attitude to "reason" elsewhere in his writings is by no means obviously sceptical. Since they wrote, several others (Arnold 1983, Broughton 1983, Baier 1991) have taken a broadly similar approach to Hume's argument, and the points made below would apply with small variations to all of these.

According to Beauchamp *et al*, Hume in his argument concerning induction has no intention of drawing a sceptical conclusion, but is "merely concerned to show that inductive reasoning cannot provide the logical necessity which uniquely characterizes demonstrative reasoning (*a priori* reasoning) and that demonstrative reasoning cannot from its own resources alone prove matters of fact". The argument is thus "a frontal attack on rationalist assumptions which encourage the view that at least some inductive arguments are *demonstrative*" (1975 pp. 119, 121 cf. 1981 pp. 37, 41). On this interpretation, therefore, Hume's rigorous sense of reason is a purely *a priori* demonstrative sense, and is adopted only in order to be dismissed.

There are two main difficulties for this sort of interpretation, both of which we have encountered before. First, we saw in §5.1 that the argument concerning induction is hard to make sense of in deductivist terms, and this point applies just as much whether or not the sense of "reason" to which it appeals is supposed to be genuinely Humean. To repeat: if evidence must be demonstrative to count here as legitimate, then it is incomprehensible that Hume should use such a complicated argument structure to prove the lack of such evidence for the Uniformity Principle, and particularly odd that he should canvass the possibility of a "probable" justification for it. This difficulty can only be increased if the appropriate sense of "reason" is supposed to be *a priori* as well as demonstrative, because before canvassing his probable justification, Hume has already argued quite explicitly (stage (6) of the argument) that probable reasoning cannot be *a priori*.

The second main difficulty for any "anti-rationalist" interpretation of Hume's argument concerns the strong language which he uses to express its conclusion. Hume consistently says that we have no good (e.g. non-circular) reason *whatever* to believe the Uniformity Principle, and he never qualifies this denial by suggesting that it relates only to some limited notion of "reason". We have seen in §6.1 some of the most explicit statements of his position, but the quotation from T139 is worth repeating in context:

> Let men be once fully perswaded of these two principles, *That there is nothing in any object, consider'd in itself, which can afford us a reason for drawing a conclusion beyond it;* and, *That 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;* I say, let men be once fully convinc'd of these two 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.

If Hume's ambitions had been limited to refuting *a priori* demonstrativist rationalism, then he could not have spoken in these terms. For the denial of such rationalism amounts only to inductive fallibilism, and the Lockean orthodoxy was already unambiguously fallibilist: "most of the Propositions we think, reason, discourse, nay act upon, are such, as we cannot have undoubted Knowledge of their Truth" (Locke 1690 IV xv 2, cf. IV iii 9-17, 21-29; IV vi 7-16).

So what exactly is it about Hume's conclusion that is supposed to "throw men so loose from all common systems"? As we saw in §4.1 above, this conclusion can be elucidated as the claim that in all probable reasonings, "there is a step taken by the mind, which is not supported by any argument or process of the understanding" (E41). So what we seek is an interpretation of "the understanding" which would make this at the same time a legitimate implication of Hume's argument, and also a sufficiently radical result to upset the fallibilist Lockean orthodoxy. The obvious place to look is Locke's own discussion of reason and its role in probable inference.

We have already seen (in §2.5 above) the definition with which Locke begins the chapter "Of Probability" in Book IV of his *Essay*. At the end of this chapter he makes clear that he views probability so defined as an objective matter, for depending on the evidence that we have for it "so is any Proposition in it self, more or less probable" (IV xv 6 cf. IV xx 5). Forming a "right Judgment" about such propositions is therefore "to proportion the *Assent* to the different Evidence and Probability of the thing" (IV xvi 9 cf. IV xvii 16). The faculty whose role is to "discover" probability "is that which we call Reason. For as Reason perceives the necessary, and indubitable connexion of all the *Ideas* or Proofs one to another, in each step of any Demonstration ... so it likewise perceives the probable connexion of all the *Ideas* or Proofs one to another, in every step of a Discourse, to which it will think Assent due" (IV xvii 2). Locke is no narrow Cartesian rationalist – he sees fallible probable inference as falling within the province of reason just as legitimately as does demonstration, and for him, as for Hume, the belief resulting from such inference is typically quite involuntary: "we cannot hinder ... our Assent, where the Probability manifestly appears upon due Consideration of all the Measures of it ... a Man can no more avoid assenting, or taking it to be true, where he perceives the greater Probability" (IV xx 16).

Despite Locke's fallibilism and his involuntarism, however, there is clearly an enormous gulf between his view of probable inference and Hume's. For Locke takes probabilities to be "perceived" by reason in a manner analogous to its perception of demonstrative relations, whereas Hume's conclusion is that any probable argument involves a crucial step which even if it is admitted to be "reasonable" in some sense, cannot possibly be founded on rational perception but must be supplied instead by instinct. This, then, is where Hume parts company not only with the extreme Cartesian rationalists, and with the modern "probabilists" such as Locke (and Leibniz – cf.

A646-7), but with an entire philosophical tradition stretching right back to the ancients. According to this tradition, reason is a special cognitive faculty separating man from the brutes, seen by many as a manifestation of the divine image (cf. Craig 1987 ch 1). Its role is to facilitate belief not merely causally but intellectually, by yielding rational insight and real understanding either of the nature of things, or of the objective evidential relationships that hold between different states of affairs or propositions.

The significance of Hume's argument concerning induction, therefore, lies in its undermining of this broad and pervasive view of reason as a faculty of intellectual insight, by showing that any such faculty would be quite unable to "assure us of any ... matter of fact, beyond the present testimony of our senses, or the records of our memory" (E26). The notion of reason whose inadequacy is thus revealed is not merely a narrow deductivist notion, but is wide enough to include any form of intellectual perception, whether that be derived purely from intuition and demonstration (as an extreme rationalist might insist), or from the senses and probability (as Locke and Leibniz would allow). Hume proceeds by considering each of these four potential sources in turn (at stages (10), (14), (9), and (16) respectively), maintaining that none of them can provide any intellectual ground for that extrapolation from observed to unobserved which is necessary to draw warranted conclusions about "absent" matters of fact. Thus by default, the true foundation of such extrapolation is revealed to be an animal instinct, showing that in this crucial respect man is closer to the beasts than to the angels. But Hume does not end on a purely sceptical note – instead he extends the notion of reason (most clearly in that remarkable footnote at T117n) to accommodate inferences drawn from this essential and fundamental instinct, and indeed makes consistency with it the very criterion of scientific reasonableness. In this paradoxical manner, an argument with a truly disturbing sceptical conclusion – *that we can perceive no reason whatever to justify any inference from observed to unobserved* – becomes the bridge to a critical but optimistic empiricism, which while committing to the flames the “sophistry and illusion” of “divinity and school metaphysics” (E165) is able to leave unscathed “the proper subjects of science and enquiry” (E163).

PART II

Probabilistic Reasoning

as an Answer to Hume

Chapter 7 Introduction: Strategies for Refuting Hume

7.1 The Shape of Part II

Having established in Part I of this thesis (as in Millican 1995) what I hope is a reliable interpretation of Hume's famous argument concerning induction, the aim of this second part is to investigate the prospects for a successful refutation or circumvention of that argument. In this chapter, accordingly, I shall start by briefly reviewing the three major non-probabilistic approaches that have traditionally been seen as possible means of countering so-called "Humean scepticism" by providing some sort of justifi-cation of induction. However we shall soon discover as a result of this examination that none of these has any real prospect of refuting what has been identified in Part I as the genuine Humean position. This being so, the *a priori* probabilistic strategy, anticipated in §3.2 as a means of steering through the one clear gap that I identified in Hume's argument, remains as the only serious contender still standing.

In the following chapter I shall go on to discuss this probabilistic strategy in more depth, and in particular will consider how it might be differently developed in the light of the various possible interpretations of the concept of probability. It is no part of this project to address the long-running debate between these interpretations, and since the point of our investigation is to examine the possibilities for a refutation of Hume, it would obviously be inappropriate here to take for granted that the debate might be settled in favour of an interpretation that would render any such refutation a non-starter. For this and analogous reasons, that chapter ends by spelling out some assumptions that will inform the remainder of Part II – here the general intention is to give the opponent of Hume everything that he could reasonably ask for in his quest for a probabilistic refutation: a favourable interpretation of probability; a plausible form of the Principle of Indifference as an *a priori* method of assessing initial probabilities in cases where this does not lead to inconsistency; even a solution to Goodman's notorious paradox. Though personally uncertain whether *any* of these concessions are genuinely deserved, they are given to the anti-Humean for the sake of the argument, in order to render the conclusion of Part II as strong as possible. If the path to a possible justification of induction looks exceedingly narrow even after all these concessions have been permitted (and it does), then this will display very clearly the magnitude of the task that faces the would-be refuter of Hume.

Chapter 9 moves on to discuss the fundamental principles of the theory of probability, based on Harold Jeffreys' axiomatic presentation from the "Logical Relation" perspective. In Chapter 10, armed with this technical background, the main business of Part II begins, with an examination and ultimate refutation of various would-be justifications of induction (notably those of De Finetti and Williams) that are based on the idea of predicting the individual probability of future events. Chapters 11 to 13 then explore an alternative and perhaps more promising avenue, based on the prediction of continuing general uniformity – here I discuss the arguments of Mackie, Harrod, and Blackburn, before ending by proposing one of my own which, though I have no great confidence in its success, may provide the last remaining forlorn hope of a probabilistic justification of induction.

It is now time to begin our discussion by sketching the three main non-probabilistic approaches to the justification of induction that have dominated the contemporary debate:[41] the "inductive", the "analytic", and the "pragmatic". Having reviewed their main features and some standard objections from a conventional perspective, we shall then briefly re-examine them in the light of our previous analysis of Hume's own position, and will find that they all resoundingly fail to refute him.

7.2 The Inductive Justification

The *inductive* defence of induction is based on the obvious (if apparently naive) idea that induction can be shown to be a reliable method of inference by appeal to its past success. Of course the instant retort of the Humean will be that such a procedure is viciously circular – induction *just is* the method of making inferences about the future based on the supposition that it will conform to the past, so to assume that it is

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[41] So we shall not discuss, for example, Kantian attempts to justify induction on the basis of supposedly synthetic *a priori* principles. Quite apart from any other problems, the Kantian transcend-ental perspective is essentially backward-looking: we know that the world is conditioned in such and such a way because we have (i.e. *have had*) experience of such and such a type. Even if this sort of argument could be epistemologically useful on its own terms, which I doubt, still nothing (apart perhaps from God or a dogmatic slumber) protects the Kantian from the prospect of an imminent descent into chaos in which such experience suddenly and unexpectedly becomes impossible.

legitimate to justify the future employment of this same method by appeal to its past success "must be evidently going in a circle, and taking that granted, which is the very point in question" (E36). Max Black, the most tenacious proponent of this defence,[42] has famously argued that the circle is not vicious, by appealing to the distinction between a *premise* of an inference and a statement that the *rule* which governs the inference is reliable – according to Black, although the former cannot properly serve also as that same inference's conclusion, on pain of deductive circularity, the latter can. Hence he sees the following argument as providing a legitimate (albeit limited) defence of induction:

> Let rule *R* be: To argue from *Most instances of A's examined in a wide variety of conditions have been B* to (probably) *The next A to be encountered will be B.*
>
> In most instances of the use of *R* in arguments with true premises examined in a wide variety of conditions, *R* has been successful.
>
> *Hence (probably):*
>
> In the next instance to be encountered of the use of *R* in an argument with a true premise, *R* will be successful.
>
> (Black 1958, p. 128; 1963, p. 138)

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[42] Here my imagination is inevitably enlivened by the witty definition, in *The Philosophical Lexicon* (Dennett and Lambert, 1978) of "The Black Max" as a "coveted decoration, annually awarded to the philosopher who stays aloft the longest by flying in circles" (p. 2). Another memorable definition relevant to our topic: to "hume" a philosophical position is to commit it to the flames, bury it or otherwise destroy it – hence the wonderful verb "exhume" which, appropriately, means "to revive a position generally believed to be humed" (p. 6)!

However most commentators (e.g. Achinstein 1962, 1963; Kyburg 1964 pp. 260-1) seem to be unconvinced of Black's claim to have avoided "reprehensible" circularity here, and we shall see later that their scepticism is amply warranted.

7.3 The Analytic Justification

The *analytic* justification of induction can be viewed as a natural response to the perceived circularity of the inductive defence. For Black's argument does seem to invoke a supremely rational method of assessing any non-deductive rule of inference, namely on the basis of its past success, and if such a compelling method cannot itself be justified by this sort of argument without circularity, then perhaps this is indicative not that the method is in any way suspect, but on the contrary, that it is irreducibly *constitutive* of rationality. From this perspective, therefore, it is *analytic* that induction is rational: to say that a non-deductive rule or generalisation is rationally well-founded *just is* to say, in part, that it is founded (directly or indirectly) on induction.

The analytic defence has had a fair number of advocates,[43] but the two best known are probably Paul Edwards (1949) and P.F. Strawson (1952, chapter 9). Strawson's position is nicely summed up by the following two passages, one from the beginning of his presentation and one from near its end:

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[43] For a few references to the argument's heyday see Kyburg (1964), especially pp. 258-9 but also pp. 254-5. Black himself presents such a defence in his *Encyclopedia* article (1967), pp. 178-9.

> [I]f a man asked what grounds there were for thinking it reasonable to hold beliefs arrived at inductively, one might at first answer that there were good and bad inductive arguments, that sometimes it was reasonable to hold a belief arrived at inductively and sometimes it was not. If he ... said that his question had been misunderstood, that he wanted to know whether induction in general was a reasonable method of inference, then we might well think his question senseless in the same way as the question whether deduction is in general valid; for to call a particular belief reasonable or unreasonable is to apply inductive standards .... (p. 249)

> So every successful method or recipe for finding out about the unobserved must be one which has inductive support; for to say that a recipe is successful is to say that it has been repeatedly applied with success; .... [This] must not be confused with saying that "the inductive method" is justified by its success ... I am not seeking to "justify the inductive method", for no meaning has been given to this phrase. ... I am saying, rather, that any successful method of finding out about the unobserved is necessarily justified by induction. This is an analytic proposition. (p. 259)

Though Strawson is surely right to point out that we in fact treat inductive success as a criterion of reasonableness for methods of factual inference, the obvious objection to this line of thought (cf. Urmson 1953) is that to call a method of inference "rational" is not merely to *describe* the method or its past performance – to say that these match up to certain more or less standard criteria – but is also to *evaluate* it – to say that it is a *good* method of reasoning. And such an evaluation is at least in part a matter of judging that the method we speak of has *genuine evidential force*, making it such as to lead from true premises to true conclusions with some measure of reliability. So while the question of the justification of induction may indeed be hard to pose distinctly because of this mixture of descriptive and evaluative content in the words that we use, it does not follow that the question is, as Strawson claims, entirely senseless. For we can still meaningfully ask whether what we call “reasonable” really is so – whether the standard inductive methods that we treat as paradigms of reasonableness *actually* carry the real evidential force that our language implicitly ascribes to them.

7.4 The Pragmatic Justification

The *pragmatic* approach to the question of justification accepts that the question can indeed properly be asked, but then goes on to stress, in a somewhat Strawsonian spirit, that it cannot directly be answered. To those impressed by Hume’s sceptical assault this attitude can seem almost inevitable: “How could we realistically hope to justify our *most fundamental* method of factual inference, when Hume has shown that it cannot be proved reliable by *a priori* reasoning, and we have no other method of reasoning to which we can appeal for the purpose even in principle, since no other method has half as much authority as the one we are attempting to validate?”

“Validation”, then, is impossible, but the aim of the pragmatic justification is to provide instead a “vindication”, to use a term and a distinction made famous by Herbert Feigl (1950). On this account the *validation* of rules or methods is the process of showing that they are warranted by reference to more fundamental rules or methods – and clearly this cannot be done in the case of those (such as induction) that are the most fundamental of all. Here, however, *vindication* is still a possibility, whereby the fundamental rule or method is shown to be worthy of that status not by reference to others, but through a demonstration that it is appropriately suited to accomplish the purpose that is expected of it.

Given the agreed impossibility of proving induction to be reliable, any attempted vindication must obviously take some other form. And here Hans Reichenbach and his former pupil Wesley Salmon have pursued the aim of showing that although induction admittedly cannot be guaranteed to work, at least it can be proved to be our "best bet", in the sense that *if any method of prediction at all will work, then certainly induction will*. Thus their justification is "pragmatic" in the same sense as Pascal's Wager – it does not purport to show that a particular theoretical claim is rationally well-founded, but recommends that in practice we rely on the truth of that claim on the principle that if it is indeed true, we prosper, and if it is not, then our cause is lost whatever we do. The outcome may be uncertain, but given this "nothing to lose" situation, at least it is obvious where the rational punter must lay his stake.

Reichenbach (1949, p. 429) saw the fundamental task of induction as that of acquiring information about empirical probabilities, and given his adherence to the "Frequentist" view of probability (cf. §8.1), this amounts to the discovery of limiting frequencies of attributes within classes of objects (or, strictly, *sequences* of objects of a given class). Accordingly, the aim of his pragmatic reasoning (1949, pp. 469-82) is to vindicate the so-called "straight rule" of induction by enumeration: the rule whereby an observed relative frequency of an attribute in a sample (for example the hitherto observed proportion of ravens that have been black) is taken to be representative of the class/sequence from which the sample comes (thus it is inferred that ravens in the future will continue to exhibit a similar relative frequency of blackness to that which obtains in our observed sample). Salmon, refining Reichenbach's approach, presents the Pascalian options very starkly (1963b, p. 87):

	Sequence has a limit	**Sequence has no limit**
Rule of induction by enumeration adopted	(A) Value of limit established	(B) Value of limit not established
Another rule adopted	(C) Value of limit may or may not be established	(D) Value of limit not established

The controversial boxes are obviously the two on the left-hand side. Thus the proponent of this sort of pragmatic justification has to demonstrate: (A) That the straight rule will indeed succeed in establishing the value of the relevant limit if there is one; and (C) That no other rule carries a similar guarantee.[44]

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[44] Moreover although to demonstrate these things would no doubt be a major achievement in itself, it is very debatable how far a mere "vindication" of the straight rule would take us towards a justification of more sophisticated and widely applicable forms of inductive practice. If in real life we are constantly using a host of less abstract criteria to adjudicate between competing predictive hypotheses, then there may seem little point to a *pragmatic* justification in such limited terms unless it can be extended to other more realistic methods without undermining its own basis. (And this indeed seems doubtful, precisely because its foundation is so limited – e.g. Salmon's argument crucially depends on the fact that his rival rules are restricted to inference on the minimal basis of observed frequencies only, to the exclusion of considerations of order etc.) At any rate, the task of inductive vindication is by no means complete even once (A) and (C) are successfully established.

To summarise a complex debate very briefly, the enduring difficulty with (A) has been that of "the short run" (how can we in any finite time have any assurance that observed frequencies will correspond to the eventual limiting frequency even if there is one?); while most discussion of (C) has centred on the prospects of excluding rival "asymptotic" rules, which by definition converge eventually to the same limit as the straight rule but may do so only after a long succession of wildly different short run predictions. The main tools Salmon (1963a, 1963b, 1968a) develops to effect this exclusion are his criteria of "regularity" (mutual consistency of predictions about different attributes) and of "linguistic invariance" (roughly, mutual consistency of predictions independently of the vocabulary used), but despite his tenacious endeavours it cannot be said that these have stood up particularly well under detailed scrutiny. Perhaps there is a future in the general idea of a pragmatic vindication of induction, but the incisive criticisms made for example by Skyrms (1965) and Hacking (1968) cast what I suspect will be an enduring shadow over Reichenbach and Salmon's particular version of that enterprise, as Salmon (1968b) seems in part to acknowledge.

7.5 The Impotence of the Non-Probabilistic Attempts at Justification

Our review of the three non-probabilistic strategies for justifying induction has admittedly been very brief and cursory. However even this has been sufficient to reveal reasons why, whatever their arguable merits, each one of them is quite unable to make any dent in Hume's position as that has been interpreted in Part I of the thesis.

Against the *inductive* justification we can invoke the understanding of Humean presupposition that was developed in §2.5 and used in §3.2 to clarify what I take to be another Humean notion, that of "presuppositional circularity". It was argued there that trivial deductive circularity, the only form which Stove recognises, is quite insufficient to do justice to Hume's reasoning. It now appears that Black may have been subject to a similar deductivist myopia, which prevented him from noticing that non-deductive arguments too can be viciously circular, a fact that Hume himself was well aware of. In short, then, Black's self-supporting inductive inference fails for precisely the reason that Hume spells out in stage (16) of his argument: inductive inference can confer rational well-foundedness on its conclusion only if the presupposition of such inference is itself rationally well-founded; hence any inductive inference which purports to establish the rational well-foundedness of that presupposition will inevitably fail to do so. This circularity is not indeed of the crude deductive kind, but it is just as vicious.

Turning now to the *analytic* justification, here too we have good grounds for ascribing more philosophical sophistication to Hume than to some of his modern would-be refuters. For as we saw in §§6.2-3, his view of reason is complex and subtle: he is very ready to acknowledge that induction is paradigmatically reasonable in an important and genuinely normative sense, while in a somewhat more rigorous normative sense, his argument shows that it is not. Those who "defend" induction by appealing to purely descriptive criteria of reasonableness are therefore in one respect being *more* sceptical than Hume himself (for they thus undermine the normativity of reason), while at the same time failing to recognise the legitimate sceptical force of his famous argument (that induction cannot be justified by any form of reason that is broadly analogous to intellectual *perception*). In so far as the analytic justification has any merit, then, Hume can comfortably go along with it, for he himself takes induction to be paradigmatically reasonable in his intermediate, "empirical" sense. But as a would-be rebuttal of Hume, the analytic justification is utterly ineffective.[45]

Finally, we come to the *pragmatic* justification, which on its own admission seems to pose little threat to the Humean position. For the very fact that it aims only at a Pascalian vindication indicates that its intended role is more that of a Humean "sceptical solution" than that of a refutation of "sceptical doubts". And this naturally invites comparison with Hume's own sceptical solution, against which the narrow Reichenbach/Salmon approach begins to look weak and artificial. They aim to show that we have purely pragmatic grounds for employing the "straight rule" in statistical inference – if limiting frequencies are what we seek, then that is how we should find them. But even if this conditional can be substantiated (and we have seen some reason to doubt this), still a fundamental objection remains. The question must be asked: *why* should we seek limiting frequencies at all? The Reichenbach/Salmon response, of course, is that only thus can we make successful predictions – if we cannot discover

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[45] Cohen (1970, p. 188) recognises this fact, despite his attribution to Hume of thoroughgoing scepticism: "the premiss of the argument from ordinary language ... can readily be granted. Very much the same thesis would have been accepted by the arch-sceptic himself, David Hume."

such frequencies, or if there are none to be discovered, then no method of prediction whatever can work. But this argument presupposes at a deeper level that the world is inductively uniform in some way, for if it were not, then perhaps an equally good route to true predictions would be to follow a random sequence of different methods (or even no *method* at all). The claim that our inferences can be successful only if the world is uniform through time takes for granted, in other words, that we are already committed to inference on the basis of such uniformities. We are indeed thus committed, of course, as Hume repeatedly points out: we do in fact believe firmly in uniformity, at least from day to day even if not in the infinite Reichenbachian "long run". Moreover we cannot stop believing this and judging accordingly, however hard we try – even the impact of one of the most powerful philosophical arguments ever devised has absolutely no effect on our animal tendency to believe and to infer. This, surely, is as strong a pragmatic justification as one could possibly wish for, and by comparison with it, Salmon's Pascalian wager and Reichenbach's long run frequencies seem both farfetched and futile.

7.6 Conclusion: Probability or Bust!

The task of this chapter is now complete. Of all the possible strategies for refuting Hume that we have considered, the only one still in the field is, appropriately, the very candidate which the analysis in Part I itself identified (in §3.2) by finding the weak spot in Hume's own argument – namely, the *a priori* probabilistic strategy. Of course apart from the discovery of that weak spot, we do not as yet have any particular grounds for optimism here – perhaps the probabilistic approach will eventually turn out to be a complete dead end, just like the others. But we have seen good reason to think that if this approach *cannot* succeed in refuting Hume's inductive scepticism, then *nothing* can. In the following chapter I shall set the scene for a detailed investigation of this strategy, by briefly surveying its context within the philosophy of probability.

Chapter 8 Probability Theory
(a) Interpretations and Options

8.1 Four Interpretations of Probability

The interpretation of the notion of probability is an extremely contentious issue, with a large and ever-growing literature, but it is no part of the aim of this thesis to take sides in that debate, nor even to contribute to it – the issues are many and complex, and to do them justice would require far more space than is available here. However if we are properly to address the question of whether probability theory can provide any assistance in countering Humean inductive scepticism, it is clearly essential at least to consider whether the interpretation of probability could have any influence on our prospects of returning an affirmative answer. So at least some cursory discussion is called for.

Leaving aside uninterpreted axiomatic treatments of the subject, there are broadly four main schools of probability interpretation that need to be considered.[46] These are:

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[46] Fine (1973) provides an excellent though dated survey of the various theories, while Howson (1995) fills in some recent gaps. Weatherford (1982) pp. 11-17 discusses the variant classifications that have been applied to such theories by Carnap, Nagel, Kyburg, Good, Von Wright, Black and Fine – the classification used here broadly follows, in relevant respects, those of Kyburg, Black and Fine.

(a) The *Classical* tradition, so called because it continues in the spirit of Pascal, Laplace and the other founding fathers of probability. The defining characteristic of this tradition is its emphasis on the derivation of assessments of probability from supposedly *a priori* principles, notably the Principle of Indifference. This principle is associated with the classical definition of probability in terms of proportions of equiprobable alternatives – a definition that is often seen to restrict the application of the notion to narrow and highly structured domains (e.g. games of chance).

(b) The *Logical Relation* tradition, which sees probability as an evidential relation between propositions, such relations extending beyond the limited range of the classical theory. Since these evidential relations are taken to be logical rather than empirically based, theorists in this tradition (of whom Carnap is an obvious paradigm) have attempted to establish broad *a priori* measures of probability, often defined relative to a formal language, to augment or replace the classical principles. Some, such as Keynes, Jeffreys and Jaynes, can be seen as bridging the classical and the logical traditions – treating probability as a logical relation, but seeking to build on classical-style rather than language-relative principles.

(c) The *Subjective* or *Personalist* tradition, which treats probability as a measure of purely subjective degrees of belief or, more rigorously, degrees of idealised belief (where much of the point of probabilistic analysis is precisely to render one's actual degrees of belief more consistent and systematic than they otherwise would have been). This tradition, of which notable adherents include De Finetti, Good, (R.C.) Jeffrey, Ramsey, and Savage, can largely be seen as a reaction to the failure of classical and logical theorists to demonstrate convincing *a priori* principles of probability assessment – in default of such principles, perhaps the most that can realistically be hoped for is internal coherence rather than *a priori* rationality.

(d) The *Frequentist* or *Objective* tradition, which identifies probabilities with long-run relative frequencies of outcomes in concrete situations where a single type of event is being repeated (though some objectivists, such as Popper, are also willing to countenance "chance" or "propensities" to extend the theory to individual events).[47] Those within this tradition, such as Reichenbach and Venn but most influentially Von Mises, see probabilities as contingent objective properties open to empirical discovery (as opposed to being *a priori*, logical or merely subjective). The frequentist tradition stands out from the others that we have discussed in making relatively little use of the notion of prior probabilities and hence of Bayes' Theorem (to be discussed in §9.4). For example the "Neyman-Pearson" statistical methodology, which has long been orthodoxy for the testing of scientific hypotheses, is based purely upon the comparison of experimental results against expectations drawn from the hypotheses being compared, and takes no account of prior assessments of the relative probability of the hypotheses themselves.[48] The entrenchment of this orthodoxy can be gauged from the common tendency of

[47] See for example Popper (1959), Hacking (1965) chapters 1 and 2, Mellor (1971), Gillies (1973) chapter 7.

[48] The same is true also of Fisher's "fiducial" treatment of statistical inference based on likelihoods (see §9.4 for the definition of this term). Two useful philosophical discussions of the Neyman-Pearson and Fisher theories are Hacking (1965) and Seidenfeld (1979a).

> statisticians to class all non-frequentists as "Bayesians", even though "Bayesianism" so defined encompasses at least three different traditions.

We shall soon see that for the purposes of this thesis, our required perspective on the interpretation of probability can be narrowed down significantly from these four apparently quite distinct options.

8.2 Clarifying the Options

First, there is the obvious point that any *empirical* theory of probability is inevitably going to be quite useless for the purpose of yielding an *a priori* probabilistic defence of induction, which as we have seen is the only type of defence that can even in principle withstand the Humean sceptical assault. If probabilities are essentially a matter of empirical limiting frequencies, or of physical propensities that generate those frequencies, then Hume will have an easy answer to any argument that purports to justify induction by appeal to such probabilities:

> All inferences from experience suppose, as their foundation, that the future will resemble the past, and that similar *frequencies and propensities* will be conjoined with similar sensible qualities. ... It is impossible, therefore, that any arguments from experience *of past frequencies and propensities* can prove this resemblance of the past to the future; since all these arguments are founded on the supposition of that resemblance.
>
> (adapted from E37-8)

So henceforth the Frequentist or Objective tradition of interpretation need not concern us – if it were to turn out that this tradition provides the only legitimate understanding of probability, then we could immediately infer a negative answer to the question we are posing, regarding the possibility of a probabilistic defence of induction.

It might seem also, at first sight, that the Subjective or Personalist tradition is equally irrelevant to our purpose, precisely because of its overt "subjectivism", and that therefore the Classical and Logical interpretations are the only ones worth considering in the quest for a rational defence of induction. However this issue is not quite so straightforward. And here it is helpful first to clarify some ambiguity in the terms "Objective" and "Subjective", which might otherwise confuse our discussion. The crux of the ambiguity is that the "Objective"/"Subjective" dichotomy can be, and commonly is, drawn across at least two largely independent dimensions. One of these is what we might call the "world/person" dimension, and another the "impartial/biased" dimension (for want of better terms – the words "objective" and "subjective" are hard to replace here). Because these dimensions are distinct, the two dichotomies combine to generate four possible options, which we can represent diagrammatically as follows:[49]

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[49] This discussion has to be extremely superficial to keep it down to a manageable size and focus. Arguably the objective/subjective dichotomy can be drawn across no fewer than *five* more or less distinct dimensions, namely: world/person, impartial/biased, non-mental/mental, intersubjective/ private, measurable/non-measurable, which however are interrelated in complex ways so that some combinations are impossible, while others may conflate together a number of conceptually distinct possibilities. The four-option diagram is therefore very much a simplification, in which I have deliberately skewed the analysis towards combinations that are directly relevant to the task at hand.

	"Objective" = in the world	"Subjective" = in the person
"Objective" = from an impartial or impersonal point of view	**FREQUENTISM** *Facts* about the *world*, which are there whether or not anyone knows or believes anything about them, and which are typically open to impartial scientific investigation.	**LOGICAL BAYESIANISM** *Facts* about the state of *a person's knowledge* and *reasonable beliefs*, which are to be considered as rationally answerable to impartial logical investigation and analysis.
"Subjective" = from a biased or personal point of view	**PERSONALIST BAYESIANISM** *Opinions* about the world as held by a particular individual, which are to be considered more or less as they stand, and not as answerable to external "impartial" criteria.	**BAYESIAN DECISION THEORY** *Opinions and desires* as held by a particular individual, which are to be considered more or less as they stand, and not as answerable to external "impartial" criteria

Though no more than a crude caricature, this diagram enables a useful distinction to be drawn between two different types of "Subjective" probability interpretation, namely those I have called "Logical" and "Personalist" Bayesianism. Both start from the subjective situation of the individual attempting to draw conclusions about the world on the basis of his existing knowledge and beliefs, and both are "Bayesian" in that amongst those potential subjective data are included prior expectations (or, more technically, prior probabilities) concerning the range of possible conclusions that are contemplated.[50] Where the two differ, however, is in the extent to which the subjective data, and these prior expectations in particular, are answerable to advance rational scrutiny. For the extreme Logical Bayesian, on the one hand, mere opinions are of no

[50] The broad use of "Bayesianism" and the ambiguity of the "Objective"/"Subjective" dichotomy create enormous scope for confusion here. For example it is not uncommon for what I have called Logical Bayesianism to be referred to as "Objective" [= impartial] Bayesianism (e.g. Howson 1995).

account whatever, and only rationally corroborated prior probabilities should figure at all in statistical inference. For the extreme Personalist, on the other hand, absolutely any prior expectation should be taken into account no matter how flimsy its foundation, and rational adjustment comes to play a role only through the operation of Bayes' theorem on those prior expectations within statistical inference.

These sketches of Logical Bayesianism and of Personalism are indeed caricatures. No doubt most "Subjective" probability theorists would fall at intermediate points along the spectrum from the one extreme to the other, and perhaps few, if any, would consistently adhere to either of the endpoints. Nevertheless we can now make use of this admittedly crude analysis to clarify the possibilities for a Bayesian justification of induction.

To deal first with the Personalist end of the spectrum, it is clear that any justification of induction from this angle must in no way depend on the original "prior probability" assignments which, for the Personalist, are representations of an individual's possibly quite irrational and arbitrary initial beliefs. However this does not, in itself, render the Personalist perspective a completely hopeless basis for a defence of induction, because one of the cardinal virtues which its supporters ascribe to it is precisely its ability to generate inferences which, *on iteration*, become less and less sensitive to the particular choice of original prior probability function. The Bayesian approach to statistical inference, as we shall see in detail later, involves taking the prior probabilities for the hypotheses being assessed, together with relevant probabilistic data concerning any new empirical evidence and its relation to those hypotheses, and then applying Bayes' theorem to yield new, "posterior" probabilities for the given hypotheses, which then in their turn can come to play the role of "prior" probabilities in future inferences. Thus the prior probabilities get replaced, and as this process is repeated, it often happens that the posterior probability function which eventually results is only minimally dependent on those original prior probabilities from which the series of inferences ultimately started. This is the lifeline for the Personalist attempt to provide a fully rational basis for induction. To quote from a highly respected "subjectivist" manifesto (Edwards, Lindman and Savage 1963, p. 540):

> If observations are precise, in a certain sense, relative to the prior distribution on which they bear, then the form and properties of the prior distribution have negligible influence on the posterior distribution. From a practical point of view, then, the untrammeled subjectivity of opinion about a parameter ceases to apply as soon as much data become available. More generally, two people with widely divergent prior opinions but reasonably open minds will be forced into arbitrarily close agreement about future observations by a sufficient amount of data.

Hence the Personalist can after all aspire to build a rational defence of induction, by showing that anyone who is not from the start dogmatically sceptical and closed minded can be brought, by experience, to share the inductivist's expectations. As we shall see in Chapter 9, this is precisely the strategy which is followed by De Finetti.

Turning to the extreme "Logical Bayesian" end of the spectrum, we can now see that this merges completely into the "Logical Relation" approach, for both are characterised by, first, the search for *a priori* principles of "original" prior probability (their crucial difference from Personalism), and secondly, a reliance on Bayes' Theorem for statistical inference, using a combination of the prior probabilities for any relevant hypotheses together with, as usual (and quoting *verbatim* from our earlier discussion of Personalism) "probabilistic data concerning any new empirical evidence *and its relation to* those hypotheses". Since the strict Logical Bayesian aims to use *a priori* principles to derive original prior probabilities for both hypotheses and evidence, his only other data will thus indeed be relational – in fact it will take the form of "conditional probabilities" expressing the "likelihood" of the evidence relative to each hypothesis. Hence extreme Logical Bayesianism *just is* Logical Relationism.

Clearly the Logical Relationist has more options than the Personalist in his quest for an *a priori* justification of induction – he can use all the resources that are open to the Personalist, as well as any principles of "original probability" that he is able to establish *a priori*.[51] However to cut any ice with the sceptic these principles must be *a priori* in the literal sense (rather than just being taken for granted as part of some foundational framework or theory) and this makes the Carnapian language-based approach highly unsuitable. So-called *a priori* probability measures that are defined

[51] I have deliberately coined the term "original probability" here to avoid a serious risk of confusion with the terms "prior" and "*a priori*". "Prior probabilities" are simply the relevant probabilities as "fed in" to a particular application of Bayes' Theorem – they need not be "prior" in any more fundamental sense (e.g. they will often themselves have been derived from a previous application of Bayes' Theorem), and certainly they need not be justifiable *a priori*. By "original probabilities" I mean the *very first prior probabilities* that are used in a sequence of probabilistic calculations – the initial assignments of probability before *any* applications of Bayes' Theorem have been made – and this term is intended to be neutral between the different interpretations of probability. For the Logical probabilist such original probabilities may indeed only be appropriate if they are justifiable *a priori*, but obviously the extreme Personalist will be perfectly content with original probabilities that express merely personal prejudices with no *a priori* warrant whatever.

relative to a particular language, and which can therefore generate different predictions depending on which language is chosen, are clearly no basis for an anti-sceptical defence even if they are (arguably) legitimate as tools in the construction of a formal system of inductive probability. Even if there were no significant internal difficulties in the Carnapian project,[52] therefore, the Logical Relationist aspiring to justify induction would do much better to follow the path trod by Keynes, Jeffreys and Jaynes: seeking his *a priori* probabilistic relations not in language, but in the only other place where tradition has plausibly claimed to find them – in logical symmetries.

Symmetries are, of course, the implicit basis of the Classical view of probability, for it is the notion of symmetry that underlies the Principle of Indifference on which that tradition is very largely founded. The symmetries involved in applications of the principle in its crudest form are purely epistemological: *if we are not aware* of any relevant difference between event *A* and event *B*, then this in itself supposedly provides sufficient grounds for ascribing to them equal probability. Obviously then if our knowledge were different, our assessment of their probability might *quite rightly* be different too, and this seems to show that the Principle of Indifference presupposes what is essentially a *relational* view of probability – probability as a relation between the knowledge that we have and the events that we anticipate.[53] So it should in the end

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[52] Which, of course, there are, though this is not the place to discuss them because Carnap's system is too sophisticated to summarise very briefly. Two useful overviews that consider it in the context of the justification of induction are Ayer (1972) pp. 33-40 and Salmon (1966) pp. 70-79.

[53] Benenson (1984) chapter VI provides a detailed argument for this conclusion.

be no surprise that the symmetry-seeking Logical Relation theorists such as Keynes, Jeffreys and Jaynes indeed seem to bridge the Classical and Logical traditions, as was remarked earlier. In their hands these traditions merge, adding more sophisticated symmetries and thoroughgoing Bayesian inference to the former, whilst preserving intact its *a prioristic* and language-independent perspective.

8.3 Conceivable Probabilistic Perspectives for the Justification of Induction

The foregoing discussion has substantially clarified the available options for a refutation of Hume. It must be founded on *a priori* reasoning about probabilities, but it can in principle be presented from either of two "Bayesian" perspectives – either the Personalist, or the Classical "flavour" of the Logical Relationist.

A justification of induction within the Personalist tradition must aim to show that the rational individual's beliefs will tend, through Bayesian updating, towards those that are inductively favourable, more or less independently of that individual's original starting point. The strength of such a justification will be its lack of any reliance on controversial *a priori* original probabilities; its corresponding difficulty will be the extremely meagre materials with which it has to build.

A justification of induction in the Classical/Logical Relationist tradition (which I shall henceforth often refer to simply as the "Logical" tradition) has the prospect of building with more impressive materials – the "Logicist" can aspire to establish *a priori* probabilistic principles that will rationally impel the impartial reasoner towards inductive conformity right from the start, and not merely over time like the Personalist's relatively feeble iterative pressure. On the other hand the credentials of these materials can be questioned: allegedly *a priori* probabilistic principles do not have a particularly reassuring track record, and the Principle of Indifference with its host of accompanying paradoxes has been a popular victim of philosophical target practice for over a century. So the main problem for the Logicist is likely to be not the invention of a suitable argument, but rather the justification of its premises.

Nevertheless in what follows I shall try to be as generous as possible to the Logicist, and give the benefit of the doubt to any alleged *a priori* principle of probability which is based on moderately plausible logical symmetries, at least until it is proved guilty by leading to manifest contradiction: hence for example Keynes' version of the Principle of Indifference (described in §9.5 below) will be considered perfectly acceptable. I shall also avoid raising irritating "quibbles" such as Goodman's paradox – perhaps an overly generous concession, because this can be particularly awkward to meet from the Logicist standpoint. If in subsequent chapters, therefore, my discussion occasionally appears to some readers to be giving the *a priori* probabilist altogether too much, the reason is only that I wish to test his theory as far as it will go. For having done so, even if it is not yet refuted, we shall at least have a better understanding of its limits and of what resources it requires if it is to do the job that the anti-Humean asks of it. We turn now to review the resources provided by the theory of probability itself.

Chapter 9 Probability Theory (b) Basic Principles

9.1 Introduction: Fundamental Principles from Two Bayesian Perspectives

The purpose of this chapter is to bring together, for convenient reference and review, the main principles of probability theory and some of its most relevant and fundamental results. Given the controversy which surrounds that theory's interpretation, it would obviously be impractical to attempt to present its central principles from a completely "unbiased" point of view that takes all the various perspectives equally into account. Fortunately, however, our discussion in §8.3 has made clear that only two broad categories of interpretation need concern us – the Logical and the Personalist – and since both of these are "Bayesian", nearly all of their principles are the same even if the foundation of those principles can be radically different. To deal with both as simply as possible, therefore, I shall initially present the "fundamental notions" of the theory from a single perspective only, that of the "Logicist" Harold Jeffreys in his *Theory of Probability* (Jeffreys, 1961, chapter I), before going on in §9.3 to comment briefly on the respects in which Personalism is, or can be, differently founded. As the last chapter indicated and we shall see later, there are indeed excellent reasons, given the topic of this thesis, for taking Jeffreys' presentation rather than any other in the Logical tradition as our point of reference and departure.[54]

9.2 A Summary of Jeffreys' Axiom System

There follows a statement of Jeffreys' fundamental notions, definitions, axioms, theorems and conventions, nearly all of which are either followed by, or introduced by, my own brief comments. As far as possible I have given Jeffreys' principles *verbatim*, except for standardisation of the symbols and formal notation in conformity with common modern usage and the other parts of this thesis. Comprehension may be eased by bearing in mind the following general conventions: e is used to signify a proposition that would typically state an item of *evidence*, whereas h, k and l are more likely to state *hypotheses*. A pair of mutually inconsistent hypotheses may be referred to as h and h' (or k and k'), and a *series* of hypotheses (in some cases mutually related by being exclusive and/or exhaustive) are signified by $h_1, h_2,..., h_n$ (or $k_1, k_2,..., k_n$). Upper case letters are used to refer to *sets* of propositions, for example background knowledge (B) or subsets of the h and k series (H, K, L etc as required).

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[54] Notably, that he is clear but theoretically rigorous; takes probabilities to be comparable (unlike Keynes, for example); and very explicitly leaves room for improper prior distributions to represent ignorance. Typifying the regard in which Jeffreys is held, even by those within rival traditions, is the following remark from Hacking (1965, p. 201): "Jeffreys' *Theory of Probability* is the most profound treatise on statistics from a logical point of view which has been published this century".

PRIMITIVE NOTION *"Given e, h is more probable than k"*

Jeffreys' theory is relational, in the sense that it takes probability to involve a relation between two propositions rather than a property of one proposition in itself. To minimise any commitment to one particular scale on which such relational probabilities are to be measured, however, he constructs his system of axioms on the primitive notion of *comparisons*, rather than *absolute values*, of such relational probabilities. This primitive notion is therefore a three-place relation.

AXIOM 1 *Given e, h is either more, equally, or less probable than k, and no two of these alternatives can be true.*

This will turn out to be a crucial assumption of the conventional theory of probability, for it implies both that relations of probabilification exist between any two propositions whatever (relative to any third proposition), and also that the strength of such relations must be comparable on a single linear scale (so that probabilities cannot, for example, be interval-valued or in other ways only partially ordered). Such an assumption, or its non-relational counterpart, is of course by no means peculiar to Jeffreys.

AXIOM 2 *If e, h, k, l are four propositions, and, given e, h is more probable than k and k is more probable than l, then, given e, h is more probable than l.*

In other words, the probability relation (with fixed "given") is transitive.

AXIOM 3 *All propositions deducible from a proposition e have the same probability on data e; and all propositions inconsistent with e have the same probability on data e.*

This establishes fixed points at the ends of the scale of probabilities, representing certainty on the one hand and impossibility on the other. Jeffreys refrains from giving these specific values (e.g. the conventional 1 and 0) in order to be able to emphasise later that the values to be given *are* purely conventional, and are no part of the essential axiomatic structure.

AXIOM 4 *If, given e, h and h′ cannot both be true, and if, given e, k and k′ cannot both be true, and if, given e, h and k are equally probable and h′ and k′ are equally probable, then, given e, (h $\vee$ h′) and (k $\vee$ k′) are equally probable.*

This provides the basis, within Jeffreys' relational and comparative theory, for his counterpart of the familiar addition rule for calculating the probability of disjunctions of mutually exclusive propositions. In order to make this correspondence more explicit, however, he next states a theorem which immediately follows from successive applications of the axiom, and then introduces by convention (he is careful later, on pp. 29-30, to show that this is just a matter of convention) the standard method of representing probabilities, namely by additive numbers (though not yet specifically in the usual [0,1] interval):

THEOREM 1 *If $h_1, h_2,\dots, h_n$ are exclusive, and $k_1, k_2,\dots, k_n$ are exclusive, on data e, and if, given e, the propositions h_1 and k_1, h_2 and k_2,..., h_n and k_n are equally probable in pairs, then given e, $(h_1 \vee h_2 \dots \vee h_n)$ and $(k_1 \vee k_2 \dots \vee k_n)$ are equally probable.*

CONVENTION 1 *We assign the larger number on given data to the more probable proposition (and therefore equal numbers to equally probable propositions).*

Convention 2 *If, given e, h and h′ are exclusive, then the number assigned on data e to (h ∨ h′) is the sum of those assigned to h and to h′.*

The transitivity of the probability relation (Axiom 2) makes numeric representation appropriate, while Conventions 1 and 2 ensure that increasing numeric values will be made to correspond to increasing "degrees of belief", combined additively (rather than, say, multiplicatively – see Jeffreys pp. 29-30 for a brief explanation of this alternative).

Axiom 5 *The set of possible probabilities on given data, ordered in terms of the relation "more probable than", can be put into one-one correspondence with a set of real numbers in increasing order.*

This axiom is needed, in Jeffreys' words, "to ensure that there are enough numbers for our purpose" – it rules out certain complex patterns of ordering which would resist reduction to a single continuum (e.g. reflecting the impossibility of a bijective mapping from $\mathbb{R}^2$ to $\mathbb{R}$). This done, Jeffreys is now able to introduce symbolism for the fundamental notion in terms of which his theory is to be formalised:

Notation *P(h | e) is henceforth to be used to signify the number associated with the probability of the proposition h on data e.*

To make any further progress, however, it is necessary to come to a decision on the question of *which* interval of real numbers is to be available for the representation of probabilities – in other words, following on from Axiom 3, which particular number is to represent certainty and which impossibility.

Theorem 2 *If e is consistent with the general rules, and e entails ¬h, then P(h | e) = 0.*

In fact the choice of number for impossibility has already been implicitly determined for us by Axiom 3 and Convention 2 (so the last result is a theorem rather than an axiom). Axiom 3 requires that where *e* is inconsistent with *h* and with *k* (and hence, obviously, with $(h \vee k)$ too) , then on data *e*, all three of the propositions *h*, *k*, and $(h \vee k)$ should have the same probability. Convention 2 requires that these probabilities be additive, that (on data *e*) the probability of $(h \vee k)$ must be equal to the probability of *h* plus the probability of *k*. And these two conditions together imply that impossibility, the very bottom of the probability scale, must be represented by zero.

CONVENTION 3 *If e entails h, then* $P(h \mid e) = 1$.

The top of the scale, however, does have to be determined by convention, and it is obviously convenient to fix the representation of certainty so that it is numerically independent of the particular data *e* (given that no previous axiom has ensured this). To emphasise that the choice of the number 1 for certainty, though standard, is not the only option worth keeping open, Jeffreys here explicitly makes the point that "there are cases where we wish to express ignorance over an infinite range of values of a quantity, and it may be convenient to express certainty that the quantity lies in that range by ∞, in order to keep ratios for finite ranges determinate" (p. 21). From now on, he is very careful to draw attention to those parts of his system which depend on this convention, and I shall follow his lead.

AXIOM 6 *If e & h entails k, then* $P(h \,\&\, k \mid e) = P(h \mid e)$.

Axiom 3 ensured that, on given data, any pair of propositions which are certain will be ascribed the same probability, but this new axiom is needed to extend the equiprobability of equivalent propositions to pairs that are less than certain. Several theorems quickly follow:

THEOREM 3 *If h and k are equivalent in the sense that each entails the other, then each entails (h & k), and the probabilities of h and k on any data must be equal. Similarly, if (e & h) entails k, and (e & k) entails h,* $P(h \mid e) = P(k \mid e)$*, since both are equal to* $P(h \,\&\, k \mid e)$*.*

THEOREM 4 $P(h \mid e) = P(h \,\&\, k \mid e) + P(h \,\&\, \neg k \mid e)$.

THEOREM 5 *If h and k are two propositions, not necessarily exclusive on data e,* $P(h \mid e) + P(k \mid e) = P(h \vee k \mid e) + P(h \,\&\, k \mid e)$*.*

THEOREM 6 *If* $h_1, h_2, \ldots$ *are a set of equally probable and exclusive alternatives on data e, and if* H_m *and* H_n *are disjunctions of two subsets of these alternatives, of numbers m and n, then* $P(H_m \mid e) / P(H_n \mid e) = m/n$*.*

Theorem 6, like those which preceded it, is expressed in a manner that is independent of Convention 3. But we can now take advantage of that convention to give precise numerical expression to a special case of this last result:

THEOREM 7 *In the conditions of Theorem 6, if* $h_1, h_2, \ldots, h_n$ *are exhaustive on data e, and* H_n *denotes their disjunction, then* H_n *is entailed by e and* $P(H_n \mid e) = 1$*. It follows that* $P(H_m \mid e) = m/n$*.*

(presupposes Convention 3)

Here H_n itself is a possible value of *e*, and hence $P(H_m \mid H_n) = m/n$, which is virtually the same as a famous rule of Laplace. As Jeffreys puts it: "given that a set of alternatives are equally probable, exclusive, and exhaustive, the probability that some one of any subset is true is the ratio of the number in that subset to the whole number of possible cases" (p. 23).

<u>THEOREM 8</u> *Any probability can be expressed by a real number.*

(presupposes Convention 3)

Theorem 7 enables a probability of any non-negative rational value m/n to be specified, and by Axioms 1 and 2 (comparability and transitivity), any probability that is irrational will divide all such rationals into two disjoint but exhaustive subsets: those that are greater than it, and those that are less. This partition will constitute a Dedekind section, and thus define a unique real number for the given irrational probability. Note however that this theorem presupposes Convention 3, in that it applies only if infinite probability values are ruled out.

<u>DEFINITION</u> *If x is a variable capable of a continuous set of values, we may consider the probability on data e that x is less than x_0, say*

$$P(x < x_0 \mid e) = f(x_0).$$

If $f(x_0)$ is differentiable we shall then be able to write

$$P(x_0 < x < x_0 + dx_0 \mid e) = f'(x_0)\, dx_0 + \mathrm{O}(dx_0)$$

We shall usually write this briefly $P(dx \mid e) = f'(x)\, dx$ ("dx" on the left meaning the proposition that x lies in a particular range dx). $f'(x)$ is called the probability density.

(presupposes Convention 3)

Thus Jeffreys introduces the familiar notions of a cumulative distribution function $f(x)$ and a probability density function $f'(x)$, the latter of which importantly presupposes that the former is differentiable. This being the case, the probability (on data e) that the variable x will lie between the limits x_1 and x_2 (where $x_1 < x_2$) can be expressed in either of two ways, namely as $f(x_2) - f(x_1)$ or as the equivalent $\int_{x_1}^{x_2} f'(x)\ dx$.

<u>THEOREM 9</u> *If L is the disjunction of a set of exclusive alternatives on data e, and if H and K are subsets of L (possibly overlapping) and if the alternatives in L are all equally probable on data e and also on data H & e, then*

$$P(H \,\&\, K \mid e) = P(H \mid e).P(K \mid H \,\&\, e) \,/\, P(H \mid H \,\&\, e).$$

Though not a particularly useful theorem, this is the first to relate probabilities assessed on different data (e for some formulae, and (H & e) for others). Jeffreys points out that this style of result is provable only on the assumption that the alternatives in L are equiprobable both with respect to data e and with respect to data (H & e), and so the need to be able to relate probabilities on different data more widely requires the introduction of a further axiom, for which he takes the form of Theorem 9 as a model:

<u>AXIOM 7</u> *For any propositions e, h, k,*

$$P(h \,\&\, k \mid e) = P(h \mid e).P(k \mid h \,\&\, e) \,/\, P(h \mid h \,\&\, e)$$

This is the familiar product rule, except that in order to maximise the independence of his system from Convention 3, it includes the expression $P(h \mid h \,\&\, e)$ as a denominator. If Convention 3 is presupposed, this denominator becomes 1 and can therefore be omitted:

<u>AXIOM 7a</u> *For any propositions e, h, k,*

$$P(h \,\&\, k \mid e) = P(h \mid e).P(k \mid h \,\&\, e)$$

(presupposes Convention 3)

Jeffreys remarks on the importance of noticing that the second factor here, $P(k \mid h \,\&\, e)$, is not in general equal to $P(k \mid e)$. Where it is equal, however, we have a case of what Jeffreys call *irrelevance*, though I shall prefer the now standard term *independence*:

DEFINITION *If P(k | h & e) = P(k | e), then h is said to be independent of k, given e.*

We now come, finally, to the part of the theory that has been most significant in the philosophy of induction, namely, the formulae for so-called *inverse probability* inference. In order to minimise his reliance on Convention 3, Jeffreys first states the relevant theorem in a way that does not presuppose a particular numerical probability value for certainty:

THEOREM 10 *If.h_1, h_2,..., h_n are a set of alternatives, B the information already available, and e some additional information, then the ratio*

$$\frac{P(h_r|e\ \& B).\,P(h_r|h_r \& B)}{P(h_r|B).\,P(e\ |h_r \& B)}$$ *is the same for all the h_r.*

Axiom 7 (the product rule) yields expressions for P(e & h_r | B) and P(h_r & e | B), the evident equality of which determines an equation from which the ratio specified in the theorem is derivable as P(e | e & B) / P(e | B), and this clearly does not vary for the h_r. Needless to say this theorem can be simplified significantly if the second factor in the numerator of the ratio, P(h_r | h_r & B), is removed in accordance with Convention 3:

THEOREM 10a *In the conditions of Theorem 10, the ratio*

$$\frac{P(h_r|\,p\ \& B)}{P(h_r|B).\,P(p\ |h_r \& B)}$$ *is the same for all the h_r.*

(presupposes Convention 3)

In this case the expression for the value of the given ratio also simplifies, to 1/ P(e | B). So we now have a formula for P(h_r | e & B), namely P(h_r | B).P(e | h_r & B) / P(e | B).

9.3 The Basis of Personalism

At this point we can leave Jeffreys' presentation, having seen the establishment of his main principles as far as Bayes Theorem, which is the last formula derived above. Before moving on to discuss this important theorem in more depth, it will be useful briefly to contrast how probability theory may be founded from a Personalistic perspective. In fact Personalistic approaches can be in detail very similar to Jeffreys', except that they are unlikely to sanction the use of "improper" probability distributions in which certainty is represented by infinity rather than by 1. It is hard to make sense of such a distribution in personalistic terms (except as an approximation), and this indeed arguably casts a shadow over the coherence of the notion even in Jeffreys' system, given that he intends probabilities to represent "degrees of reasonable confidence" (p. 15) or "reasonable degree[s] of belief" (p. 34).

As explained in §8.1, the Personalist sees probability as a measure of strength of *actual* belief or confidence, and given the obvious subjectivity of this notion, it might seem hard to justify the claim that such degrees of belief can meaningfully be assigned numerical values, let alone the requirement that these values should conform to any formal axioms. The most popular way of justifying the assignment of numerical values is in terms of betting quotients (e.g. if I consider it fair to bet £1 on *P*'s being true for a possible win of £3, then I implicitly assign *P* a probability of ¼), but it is also possible to do so simply by appeal to the notion of equal degree of belief, following the sort of construction that is given by Jeffreys (see especially his Theorems 7 and 8 in §9.2).

Once numerical degrees of confidence have been established there are, as Hacking puts it, “overwhelming reasons for making [them] satisfy the probability calculus” (1968, p. 53). He lists six different arguments for this conclusion (with references, corrected here, giving “good authorities” rather than original sources):

1. Bayes’ arguments (Bayes 1763).
2. Metrization argument: if a metric is induced on comparisons of confidence, it must, subject to very feeble conditions, satisfy the probability axioms (Savage 1972, pp. 33-40).
3. Joint development of utility and degrees of confidence (Ramsey 1926).
4. Dutch book or coherence argument (Kemeny 1955).
5. Subjective version of arguments in Jeffrey’s *Theory of Probability* (Jeffreys 1961).
6. Differentiation argument: if you want degrees of confidence to be mathematically tractable, and to admit of some sort of learning from experience, they have to satisfy the axioms (Cox 1961, part I).

If you admit numerical degrees of confidence at all, I do not think you can consistently and simultaneously argue against all these arguments.

This is not the place to look further into these arguments, but Hacking’s main point (elaborated also in his 1965 pp. 210-11 and 1966 p. 335) is well made – the Personalist has many resources to which he can appeal in establishing the principles of his theory once precise numerical degrees of belief have been accepted.[55]

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[55] It is the notion of *precision* which should, in my view, be the main sticking point. Typically the requirement of precision is justified in a very summary manner, for example by Savage (1962): “... attempts to say that the exact probabilities are ‘meaningless’ or ‘non-existent’ pose more severe

9.4 Bayes' Theorem

In the remainder of this chapter it will be useful to draw attention to a few of the particular concepts and results that are destined to play an especially significant role in what follows. The first of these is Bayes' Theorem, which was the last formula to be derived in the presentation of Jeffreys' system above. However to give it a more intuitive feel, I shall here discuss it in less formal terms (omitting any reference to "tautological" and to "background" evidence that is taken for granted in the context, just as in the discussion of Stove – see §5.2 note 36).

Bayes' Theorem is used, as discussed in the previous chapter, when we wish to "update" our prior assessments of the probability of some hypothesis *h*, in the light of our observation or acquisition of some relevant item of evidence *e*. The theorem then uses the following pieces of information:

$P(h)$	The prior probability of the hypothesis *h*, *before* the updating takes place
$P(e)$	The prior probability of the evidence *e*, *before* it was observed
$P(e\|h)$	The conditional probability of the evidence *e*, *given that h* is true. This is also called by Fisher's term the *likelihood*, meaning the likelihood that the evidence *e* would have come about given that *h* were true.

problems than they are intended to resolve, similarly for replacements of individual probabilities by intervals or by second-order probabilities. ... Sight should not be lost of the fact that a person may find himself in an economic situation that entails acting in accordance with a sharply defined probability, whether the person chooses his act with security or not." (p. 145).

These are combined together as follows, to yield the conditional probability of the hypothesis *h* given that the evidence *e* has been observed – this conditional probability is precisely the *updated* probability for *h*, in the light of evidence *e*, that is the goal of Bayesian inference:

$$P(h|e) = \frac{P(h) \times P(e|h)}{P(e)}$$

Some obvious points follow. First, the updated probability for *h* is directly proportional to its prior probability – so other things being equal, the more probable a hypothesis starts out the more probable it will remain, and in particular, if the prior probability of a hypothesis is zero then Bayes' Theorem is powerless to raise it whatever the evidence. Secondly, the updated probability for the hypothesis is also proportional to the likelihood – so, other things again being equal, this means that the more some evidence is made likely by a hypothesis, the more that evidence, if it is forthcoming, confirms the theory. Finally, the updated probability for the hypothesis is *inversely* proportional to the prior probability of the evidence – so an item of evidence which would, but for the hypothesis, be very unlikely, will do more to support the hypothesis than an item of evidence that would anyway have been unsurprising. This last point is relatively unimportant compared with the others, because uses of Bayes' are often more a matter of comparing hypotheses rather than assessing their probability by absolute standards. In that case, P(*e*) drops out as irrelevant, and the crucial matter is simply the relations of proportionality amongst the other three components. This helps to explain why Jeffreys presents the theorem in the way that he does.

9.5 Prior Probabilities and Indifference

We have already seen that prior probabilities play a crucial role in Bayesian inference, and the last chapter included some discussion of the different ways in which they are typically assessed by Personalists on the one hand, and Classical/Logical Relationists on the other. The latter's traditional reliance on the Principle of Indifference is controversial, but Jeffreys believes it to be to a matter of logic:

> If there is no reason to believe one hypothesis rather than another, the probabilities are equal. ... *to say that the probabilities are equal is a precise way of saying that we have no ground for choosing between the alternatives.* ... To take the prior probabilities different in the absence of observational reason for doing so would be an expression of sheer prejudice. The rule that we should take them equal is not a statement of any belief about the actual composition of the world, nor is it an inference from previous experience; it is merely the formal way of expressing ignorance. ... It is not a new rule in the present theory because it is an immediate application of Convention 1. ... When reasonable degree of belief is taken as the fundamental notion the rule is immediate. We begin by making no assumption that one alternative is more likely than another and use our data to compare them.
>
> (Jeffreys 1961 pp. 33-4)

Jeffreys here describes the principle as "an immediate application of Convention 1", namely, the convention that larger numbers signify greater probabilities. But I suspect that its basis lies even further back, right at the start with his Axiom 1, where he asserts that propositions are universally comparable with respect to probability. This thought clearly strongly informs the passage above, and indeed it is difficult to see how he could avoid some kind of Indifference Principle when once this axiom of comparability had been affirmed.

In this respect it is interesting to draw a contrast with another great figure in the Classical/Logical Relation tradition. Keynes is notable for having resisted the idea that probabilistic relations apply universally, or even that where they do apply, they are always comparable: unlike Jeffreys, therefore, he did not believe that all probabilistic relationships can be fully and faithfully represented by simple numbers (a thought with which I shall in my concluding Chapter 14 express some sympathy, though I shall have to ignore it meanwhile). But given these views, it is no coincidence, I believe, that Keynes was the far more astute and careful critic of the Principle of Indifference.

Chapter IV of Keynes' *Treatise on Probability* is still one of the very best discussions of the principle,[56] with many telling examples both of "discrete" and of "geometrical" paradoxes. Since we shall be discussing the latter in Chapter 13, we can confine ourselves here to one of the former (Keynes 1921, p. 50-1):

> Two cards, chosen from different packs, are placed face downwards on the table; one is taken up and found to be of a black suit: what is the chance that the other is black also? One would naturally reply that the chance is even. But this is based on the supposition ... that every "constitution" is equally probable, i.e. that each individual card is as likely to be black as red. ... The alternative theory assumes that there are three equal possibilities – one of each colour, both black, both red. ... The chance of the second's being black is therefore 2/3. The Principle of Indifference has nothing to say against either solution. Until some further criterion has been proposed ... a preference for either hypothesis is totally arbitrary.

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[56] Others that are particularly interesting include Lucas (1970) chapter VII, Blackburn (1973) chapter 6, Benenson (1984) chapters VI and VII, and Van Fraasen (1989) chapter 12.

Keynes himself develops a more rigorous version of the principle, designed to avoid this sort of problem:

> There must be no *relevant* evidence relating to one alternative, unless there is *corresponding* evidence relating to the other; our relevant evidence, that is to say, must be symmetrical with regard to the alternatives, and must be applicable to each in the same manner. ...
>
> This rule can be expressed more precisely in symbolic language. ... The Principle of Indifference is applicable to the alternatives $\phi(a)$ and $\phi(b)$, when the evidence h is so constituted that, if $f(a)$ is an independent part of h which is relevant to $\phi(a)$, and does not contain any independent parts which are irrelevant to $\phi(a)$, then h includes $f(b)$ also.

Given this careful and narrow formulation of the Principle of Indifference, it is hard to see how anyone in the Classical/Logical Relation tradition could object to it and still have anything left to enable the theory to get off the ground. In line with the "benefit of the doubt" policy which informs my attitude to the logical theory throughout Part II (as explained in §8.3), I too shall therefore raise no objection to Keynes' principle.[57]

Our preliminaries are now completed, and it is time to move on to the main business of Part II of the thesis – refuting the would-be refuters of Hume!

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[57] Nor, for the same reason, shall I raise other general objections to the logical theory, despite their historical prominence in discussions of induction and probability. Aside from inconsistencies involving the Principle of Indifference, and Goodman's Paradox, perhaps the best-known such general objection is the one repeatedly urged by Ayer (e.g. 1957, 1979), who argues that the relational nature of logical probability makes a mystery of why beliefs should be based on complete rather than partial evidence when both are supposedly capable of yielding a probabilistic relation to the hypothesis in question. This objection has anyway been answered by Good (1967), who neatly explains how what Carnap called the "Principle of Total Evidence" can be shown to maximise expected utility.

Chapter 10 Seeking the Individual Probability of Future Events

10.1 Introduction: Future Events versus Continuing Uniformity

Attempts to justify induction probabilistically naturally fall into two main types. The first pursues a strategy of attempting to infer, with probabilistic support, that future *individual events* will be of a similar kind to those that have gone before. The second is more global, and instead proceeds by trying to show that the *general uniformities* we have hitherto experienced are likely to continue, at least for the immediate future. In this chapter I shall deal with the first type of justification, including arguments by De Finetti, D.C. Williams and Stove. Apart from points that arise in connection with De Finetti's theorem (which has been very little discussed in the philosophical literature), this discussion will be fairly brief, because many of the main points are generally familiar and quite straightforward.[58] In the next chapter, however, I shall begin a three-part examination of the alternative "continuing uniformity" approaches, which seem to open far more varied and more promising avenues of justification, but

[58] Arguments of this type, going back to Laplace, have long been a staple of critical discussions of "inverse probability". See the references in the last footnote to §10.2 below for examples.

which have hitherto been relatively little discussed (and never, as far as I am aware, within any sort of systematic framework).

10.1 Bernoullian Trials and the Law of Large Numbers

Most of the attempts to justify induction with which we are concerned in this chapter – those that attempt to assess directly the probability of individual future events – rely on some form of "Law of Large Numbers". There are several such large number laws in probability and statistical theory, some of which constitute highly specific and powerful results (the Central Limit Theorem, for example, shows that combinations of arbitrary distributions will tend to produce an overall "Normal" distribution when superimposed – a remarkable discovery). It is enough for our purposes, however, to have available a relatively modest and more familiar large number law, which tells us not about limiting *distributions*, but only about much simpler limiting *frequencies*.

In order to explain this law, we must use the notion of a *Bernoullian Trial*, (named in honour of James Bernoulli, who proved so much about them including this law). A Bernoullian Trial is simply *a repeated random experiment with two possible outcomes* (often called "success" and "failure") in which the outcome on each occasion is *independent* of the outcome on every other, *and in which the probability of success* (often called "p", making the probability of failure "$(1 - p)$") *is the same on every* *occasion*. The most common examples that are given of Bernoullian Trials are, of course, tosses of coins and rolls of dice.

The Law of Large Numbers states the following important result about repeated Bernoullian Trials:

> Let S be the number of successes throughout n independent repetitions of a random experiment with probability p of success (i.e. n Bernoullian trials with probability p). Then:
>
> For every $\varepsilon > 0$, $$\lim_{n\to\infty} \mathrm{P}\left(\left|\frac{S}{n} - p\right| \geq \varepsilon\right) = 0$$
>
> and $$\lim_{n\to\infty} \mathrm{P}\left(\left|\frac{S}{n} - p\right| < \varepsilon\right) = 1$$

Put loosely, the probability that the frequency of success S/n will approach "as close as you like" to the probability p can be made arbitrarily close to 1 by making n large enough. The more experiments are performed, the more certain it gets that the frequency will match the probability to any required degree of precision.

This valuable law can be proved very quickly by making use of the important Chebyshev Inequality, which is proved in Appendix 3. Since the proof is so short, and the result so fundamental, I shall give it here (adapted from proof given in Hogg and Craig 1965, p. 81).

First we establish the mean μ and variance σ^2 of the experimental setup, taking a success to be result 1 and failure 0. Clearly for individual trials $\mu = 1.p + 0.(1 - p) = p$, while σ^2, the expectation of the square of the difference from the mean, will therefore be equal to $(1 - p)^2.p + p^2.(1 - p) = p.(1 - p).(1 - p + p) = p(1 - p)$. If we then consider sequences of n trials, the mean will clearly be proportional to the number of trials, and because the individual trials are mutually independent, the variance will likewise be proportional to n. Hence we have $\mu = np$ and $\sigma^2 = np(1 - p)$ for a sequence of n trials. Now we can proceed as follows:

$$\mathrm{P}\left(\left|\frac{S}{n} - p\right| \geq \varepsilon\right) \quad = \quad \mathrm{P}\left(\left|S - np\right| \geq \varepsilon n\right) \quad = \quad \mathrm{P}\left(\left|S - \mu\right| \geq \varepsilon\sqrt{\frac{n}{p(1-p)}}\sigma\right) \ ,$$

where as above $\mu = np$ and $\sigma^2 = np(1 - p)$. Applying Chebyshev's Inequality (alternative form) with $k = \varepsilon\sqrt{n/p(1-p)}\sigma$, we derive:

$$\mathrm{P}\left(\left|S - \mu\right| \geq \varepsilon\sqrt{\frac{n}{p(1-p)}}\sigma\right) \leq \frac{p(1-p)}{n\varepsilon^2}$$

and hence, by combining this with our earlier result:

$$\mathrm{P}\left(\left|\frac{S}{n} - p\right| \geq \varepsilon\right) \leq \frac{p(1-p)}{n\varepsilon^2}$$

Clearly the right-hand side of this inequality can be made arbitrarily close to zero by increasing n sufficiently, so the result is proved.

10.2 An Inverse Law of Large Numbers?

The Law of Large Numbers is an impressive result: in any Bernoullian sequence the *frequency* of success will almost certainly approach the *probability* of success arbitrarily closely if the sequence goes on long enough. So from probabilities, frequencies can be inferred. Such an inference is not, of course, inductive: the frequency is inferred from the given probability rather than from the past behaviour.[59] But this would clearly have immediate relevance to induction if only the probability could be inferred from that past behaviour. For then we could argue as follows:

Past behaviour $\Rightarrow$ Probability $\Rightarrow$ Future Behaviour

And since we already have in our hands a result which seems to tell us that "frequencies will almost inevitably match probabilities arbitrarily closely if the sequence is long enough", it is not at all surprising that many philosophers over the years have been very attracted by the idea of reversing the Law of Large Numbers.

Unfortunately the reversal is far from simple. For the only inverse inference method available, namely Bayes' Theorem (and its relatives), requires for its operation prior probabilities which mere past frequencies, in the absence of further information, cannot provide. To spell this out a little, let us suppose that we are faced with an irregular coin of unknown weight distribution, texture etc. The coin is rigid and

[59] Obviously we can here ignore quibbles from any Frequency theorists who wish to claim a definitional connection between probability and past behaviour, for the reasons explained in §8.2.

durable, so we make the assumption that tosses of it are Bernoullian (i.e. independent and with constant “bias”) with unknown probability *h* of landing heads. Our aim is then to discover the approximate value of *h* (for simplicity let us say to the nearest 0·1) by repeated tossing. Suppose that we perform a fair number of tosses and find a frequency of heads of around 0·66 – how do we proceed and what can we infer?

We have 11 hypotheses for the approximate value of the unknown *h* (from 0·0 to 1·0 in intervals of 0·1). And what certainly is true, based on combinatorial considerations (or to be specific, the resulting relative magnitudes of the coefficients in the binomial distribution), is that of these hypotheses the one most likely to result in a frequency of 0·66 is the hypothesis that *h* is approximately 0·7. The value of *h* next most likely to result in that frequency is 0·6, and so on. But these relationships are merely *likelihoods* of our evidence given the various hypotheses, and we know from Bayes’ Theorem that such likelihoods on their own are insufficient to generate “updated” probabilities for the hypotheses in the light of the evidence. We must have prior probabilities to work with, or no such inference can be drawn. This point can be emphasised in the following way – suppose that the experiment above were carried out, say, 50 times with a very slightly bent coin: would we conclude after 33 heads that the probability of heads with that coin is 0·66? I do not think so, because we would consider it most improbable that a coin which is only slightly bent could skew the usually balanced probabilities so much. We might well conclude that the coin was biased to some degree, and we would no doubt grow to favour this hypothesis more and more if after 100 tosses, 200 tosses (etc) the frequency of heads remained high. Perhaps it would not be so *very* long before we indeed came to believe that the probability of heads was really as high as the evidence initially suggested, but if so, this would merely indicate that we did not from the start consider the hypothesis of such a bias to be *so very* improbable.

The general rule, then, is that we can infer probabilities from frequencies only if we are able to put into the Bayesian equations prior probabilities for the hypotheses that we are attempting to assess (i.e. the hypotheses of bias, which are in a sense "probabilities of probabilities" though that way of putting it is liable to raise metaphysical hackles[60]). Laplace notoriously attempted to make up for the lack of prior probabilities here by appealing to the Principle of Indifference across the entire possible range of bias from 0·0 to 1·0, and thus derived his famous "Rule of Succession". Since this is very familiar in the literature,[61] and such uses of the Principle of Indifference seem highly dubious (they do not get close to satisfying Keynes' criterion in §9.5, for example), I shall not discuss this further here but will instead move on to what has recently been considered by those few who have alluded to it to be a far more promising attempt to fill the gap.

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[60] For example from Von Wright (1957) p. 114: "The unknown of the problem ... is not a probability. It is the presence of certain conditions, relative to which the event has a probability. The 'inverse problem' in all its variations can be described as a problem of re-identification of the conditions under which an event has occurred, these conditions constituting a 'field of measurement' (data, information) of the event's probability."

[61] See for example Broad (1928), Keynes (1921) chapter 30, Kneale (1949a) pp. 201-7 and Von Wright (1957) pp. 102-17.

10.3 De Finetti's Theorem on Exchangeable Events

We have seen that "inverse inference" of probabilities from frequencies is problematic unless we are able to provide appropriate prior probabilities for the various relevant "hypotheses of bias". From the Classical/Logical Relation perspective on probability, "appropriate" here must mean having some secure rational foundation, and we have seen that this renders the inverse inference somewhat dubious. However from the Personalist perspective the provision of prior probabilities is no difficulty at all – they can simply be drawn from introspection. Hence if it were possible to derive some equivalent to the Law of Large Numbers from the Personalist point of view, this might open the way to a legitimate method of inverse inference from frequencies to probabilities and, of course, ultimately back again: induction might after all be given some foundation based on an inversion of the Law of Large Numbers.

Before proceeding with this line of thought, however, some further preparation is necessary, because the notion of a Bernoullian sequence of independent events with a fixed but unknown probability is clearly problematic from a Personalist viewpoint. As far as the Personalist in concerned, each event in the sequence has its own particular personal probability, no doubt in each case the same, but if the Personalist indeed considers the events to be parts of a repeated and inductively connected sequence, then these probabilities will certainly not be *independent* for him in the classical sense – otherwise all learning from experience would become impossible.

De Finetti accordingly defines the important notion of *exchangeability*, to serve as a Personalist substitute for independence. Briefly, a sequence of events is considered to be exchangeable if the person concerned views success in any one of them as just as likely as in any other, and any n-fold combination of results to be just as likely as any other n-fold combination that contains a similar number of successes and failures (let us call this "*n-exchangeability*"). Based on these apparently meagre resources, De Finetti proves a remarkable theorem which provides the Personalist with his own "law of large numbers" – a theorem that shows how, on pain of inconsistency, personalist probabilities about sequences of events viewed as exchangeable must inevitably conform to the sort of inverse inference which Laplace aspired to vindicate.

Since I am not aware of any existing presentation of De Finetti's mathematical reasoning which is accessible to the non-specialist, it is perhaps worth spelling out fully his proof of his "law of large numbers".[62] Suppose we have a sequence of random quantities X_i that are 1-exchangeable and 2-exchangeable, with the following expectations (and where $m_2 \neq \mu_2$):

$E(X_i) = m_1$ (i.e. the expected value of X_i is m_1 irrespective of the value of i)

$E(X_i^2) = \mu_2$ (i.e. the expected value of X_i^2 is μ_2 irrespective of the value of i)

$E(X_iX_j) = m_2$ (i.e. the expected value of X_iX_j is m_2 for any i, j where $i \neq j$)

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[62] De Finetti (1937) pp. 124-5. Braithwaite (1957) and Jeffrey (1983) chapter 12 give nice (and quite different) explanations of the significance of the theorem on exchangeable events, but neither contains any of the relevant mathematics. De Finetti himself is comprehensible with effort, but combines a somewhat roundabout prose style with an extremely terse mathematical style.

Note that it is unnecessary to assume any more than this 1-exchangeability and 2-exchangeability in order to prove the theorem (e.g. we need not stipulate that $E(X_iX_jX_k)$ also has a constant value, which would be required for 3-exchangeability).

Now suppose we have two sets of these random quantities, which may or may not overlap. One of the sets contains h such quantities (which we can refer to for convenience as H_1 to H_h) and the other contains k of them (K_1 to K_k). Suppose that r of the quantities are common to both sets (so that each of these r individual quantities has *three* possible designations – i.e. X_a, H_b and K_c for suitable values of a, b and c – but the ambiguity does not matter).

Let us refer to the mean of these sets as H and K respectively. Then we have

$$H = \frac{1}{h}\sum_{i=1}^{h} H_i \qquad\qquad K = \frac{1}{k}\sum_{i=1}^{k} K_i$$

However it will be convenient to be able to write such "Σ" sums without inserting limits, so in what follows, an expression such as $\sum H_i$ will be taken to denote the sum of all the H_i (i.e. from $i = 1$ to h), $\sum H_i H_j$ the sum of all the H_iH_j where $i \neq j$, and likewise the corresponding formulae for K (with k replacing h). $\sum H_i K_j$ will denote simply the sum of all the $H_i K_j$ for $i = 1$ to h and $j = 1$ to k (for obvious reasons there is no requirement in this case that $i \neq j$).

In these terms, De Finetti's statement (1937, p. 124) of what we are attempting to prove is as follows:

The "law of large numbers" consists of the following property: *if* H *and* K *are respectively the averages of h and of k random quantities* X_i *(the two averages may or may not contain some terms in common), the probability that* $|H - K| > \varepsilon$ *(where* $\varepsilon > 0$*) may be made as small as we wish by taking h and k sufficiently large*; this follows immediately from the calculation of the mathematical expectation of $(H - K)^2$.

We now proceed accordingly, and using the notation explained above, to calculate the expectation of $(H - K)^2$, which we will express as "$E((H - K)^2)$". We start by deriving a formula for $(H - K)^2$, and then calculate the expectation of each of that formula's components before combining these together.

$$(H - K)^2 \quad = \quad \frac{\left(\sum H_i\right)^2}{h^2} + \frac{\left(\sum K_i\right)^2}{k^2} - 2\frac{\sum H_i K_j}{hk}$$

$$= \quad \frac{\sum H_i^2 + 2\sum H_i H_j}{h^2} + \frac{\sum K_i^2 + 2\sum K_i K_j}{k^2} - 2\frac{\sum H_i K_j}{hk}$$

To find the expectation of each of the "Σ" sums, we note that $\sum H_i^2$ has h terms and $\sum K_i^2$ has k terms, each term having expectation μ_2; $\sum H_i H_j$ has $h(h - 1)/2$ terms and $\sum K_i K_j$ has $k(k - 1)/2$ terms, each term having expectation m_2; and $\sum H_i K_j$ has hk terms, of which r (those where the H-quantity and the K-quantity are one and the same) have expectation μ_2, while the remaining $(hk - r)$ have expectation m_2.

Thus we have:

$$\mathrm{E}\left((\mathrm{H}-\mathrm{K})^2\right) = \frac{h\mu_2 + h(h-1)m_2}{h^2} + \frac{k\mu_2 + k(k-1)m_2}{k^2} - 2\frac{r\mu_2 + (hk-r)m_2}{hk}$$

$$= \frac{\mu_2 + hm_2 - m_2}{h} + \frac{\mu_2 + km_2 - m_2}{k} - 2\frac{hkm_2 + r(\mu_2 - m_2)}{hk}$$

$$= \frac{k\mu_2 + hkm_2 - km_2 + h\mu_2 + hkm_2 - hm_2 - 2hkm_2 - 2r(\mu_2 - m_2)}{hk}$$

$$= \frac{(h+k)\mu_2 - (h+k)m_2 - 2r(\mu_2 - m_2)}{hk} = \frac{h+k-2r}{hk}(\mu_2 - m_2)$$

$$= \left(\frac{1}{h} + \frac{1}{k} - \frac{2r}{hk}\right)(\mu_2 - m_2) \leq \left(\frac{1}{h} + \frac{1}{k}\right)(\mu_2 - m_2)$$

and as required, we see that the expectation of $(\mathrm{H}-\mathrm{K})^2$ can be made as small as we please by taking sufficiently large values of *h* and *k*. How exactly does his law of large numbers "follow immediately" from this result? De Finetti leaves this part of the proof for his reader to fill in, but it is a consequence of the famous *Chebyshev's Inequality*, which can be expressed as follows:[63]

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[63] To remind the reader who wishes to go all the way back to first principles, a proof of Chebyshev's Inequality, which is itself extremely neat, is given in Appendix 3.

Let X *be any random variable with mean* μ *and variance* σ^2*. Then for any* $t > 0$*,*

$$P(|X - \mu| \geq t) \leq \frac{\sigma^2}{t^2}$$

If we consider (H – K) as our "random variable" here, then its mean will be 0 (obviously the expectation of H and K will be the same, namely m_1, since they are both averages of exchangeable X_i which themselves have expectation m_1) and its variance – the expectation of the square of the difference from the mean – will therefore be $E([(H - K) - 0]^2) = E((H - K)^2)$ as calculated above. Hence combining Chebyshev's Inequality with that result, together with the obvious inequality $P(\alpha > \varepsilon) \leq P(\alpha \geq \varepsilon)$, it follows immediately that:

$$P(|H - K| > \varepsilon) \quad \leq \quad \left(\frac{1}{h} + \frac{1}{k}\right)(\mu_2 - m_2) \Big/ \varepsilon^2 \text{ for any } \varepsilon > 0$$

and as De Finetti states, this probability can be made as small as one pleases by giving h and k sufficiently high values. Hence his "law of large numbers" is indeed proved.

10.4 What Has De Finetti Proved?

There is no doubt that De Finetti's law of large numbers is an important result. Ian Hacking, in his review of the volume in which the relevant essay ("La Prévision: ses Lois Logiques, ses Sources Subjectives", 1937) first became conveniently available in English, speaks of it in glowing terms:

> De Finetti proves that when a man [a florist, in Hacking's example] thinks of events as exchangeable and conducts experiments he will form the same expectations about next year's crop as if he had postulated statistical independence, and long run frequencies, and used the Bayesian theory of inference. One had thought of statistical independence as a physical property of some part of the world, and as an essential core to statistical inference. De Finetti has proved it *de trop*. This is the first successful philosophical reduction in the whole turbid history of reductionism.
>
> (Hacking 1966, p. 339)

Thus De Finetti has shown that Personalism can combine the standard resources of traditional statistics with its own great strength: the simple and uncontroversial provision of prior probabilities. Or so it appears. On this basis De Finetti himself has claimed to have solved the problem of induction, and others have claimed it on his behalf.[64] We must now examine how such a solution might work.

De Finetti's theorem, as applied to the case of inverse inference regarding exchangeable events, is a paradigm of the Personalist inferential strategy that I described in §8.2. The general idea is that in the case of a sequence of events judged to be exchangeable, the Personalist can start with *almost any* prior distribution of "subjective" probabilities and conditional probabilities, as long as these satisfy the constraints of exchangeability. Then as the experimental results come in, the Bayesian

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[64] See for example De Finetti (1937) p. 147, De Finetti (1970) vol. 2 p. 201, Zabell (1985) pp. 157-9, Hill (1988) p. 211. See also Jeffrey (1986) for a related treatment of induction, though his discussion is more balanced and his claims less optimistic than De Finetti's.

inferences they provoke, and the constant constraints of exchangeability, will together gradually mould that distribution into one that reflects the observed frequency of the events in the sequence. Eventually, if he is consistent, the Personalist will end up making predictions that conform precisely to what would be expected from the Objectivist who all along has taken the events to be indicative of a genuine underlying physical propensity.

There are, however, some problems. First, it should be noted that De Finetti's argument goes through only if μ_2 (i.e. the expected value of X_i^2) and m_2 (i.e. the expected value of X_iX_j where $i \neq j$) are distinct. If the two are the same, then we have not mere exchangeability but genuine full-blooded independence, and in that case no amount of Bayesian updating can make any difference to the relevant expectations. So for De Finetti's argument to get a grip, the person concerned must already be favourably disposed, albeit perhaps only marginally, towards the idea that the various X's are evidentially connected together. The radical Humean who feels confident that no such connection exists will be left quite unaffected by De Finetti's reasoning.

Secondly, and more generally, it does not seem to be inevitable that even the non-sceptical Personalist must end up adjusting his beliefs in the way that De Finetti describes. Certainly what he says is true regarding the logical structure of the synchronic system of exchangeable prior probabilities and conditional probabilities with which the consistent Personalist starts his reasoning. But as Hacking famously pointed out in 1967 (pp. 314-6), there is in fact no obvious inconsistency in the Personalist's updating his beliefs, when he does so, by some method other than Bayesian conditioning:

> The idea of the [Personalist Bayesian] model of learning is that *Prob(h/e)* represents one's personal probability [for *h*] after one learns *e*. But formally the conditional probability represents no such thing. If, as in all of Savage's work, conditional probability is a defined notion, then *Prob(h/e)* stands merely for the quotient of two probabilities. It in no way represents what I have learned after I take *e* as a new datum point. It is only when we make the dynamic assumption that we can conclude anything about learning from experience. To state the dynamic assumption we use probability given data, as opposed to conditional probability.
>
> (p. 315)

Hacking's "dynamic" assumption is precisely the assumption that if I start out with a (personal) conditional probability $P(h|e)$ – shown behaviourally by my willingness to lay appropriate conditional bets – and I then learn *e*, I must inevitably end up ascribing that same value to my updated (personal) probability for *h*. Not only is this not logically required, as Hacking emphasises, but indeed instances have been produced in subsequent discussions that show how such updating can even be irrational.[65] One relevant point here, commonly made against Personalist Bayesianism, is that it is in principle no more rational for the Personalist to apply Bayes' Theorem "forwards" than it is to apply it "backwards" – to work out an appropriate prior probability function after the event, so as to deliver the updated probability which he finds most plausible. Even many Personalists are happy to accept this view. Good, for example, strongly recommends the imaginary contemplation of possible outcomes, *and of what one's*

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[65] Earman (1992) pp.46-51 and Howson (1995) pp. 7-10 briefly sketch the debate on Bayesian conditioning, while Bacchus, Kyburg and Thalos (1990) provide a strong anti-Bayesian contribution to it. Two useful discussions that recently appeared together are Howson (1996) and Castell (1996).

beliefs would then become, as an excellent method of rendering one's prior probabilities sharper and more appropriate – he calls this the "device of imaginary results" (e.g. Good 1950, p. 35).

In a different context, Good has produced a nice example against De Finetti (in a discussion of De Finetti 1969). He imagines our watching a coin-tossing machine, producing a sequence of tosses that we, in accordance with De Finetti, initially consider to be exchangeable:

> ... if by chance we happened to get the sequence
>
> 010101010101010101010101010101010101010101010101,
>
> we would, if we were rational and had not done a very long preliminary sequence of trials, judge the (subjective) probability that the next digit would be a zero as well over 1/2. But this would be an admission that we would not strictly accept the postulate of exchangeability or permutability even before we did any experimentation.
>
> (Good 1969, p. 21)

If the dynamic assumption is dubious it is not clear that Good's final sentence here is correct. But whether it is or not, he seems to be on target with De Finetti. In the context of our discussion, moreover, his point can be developed. Suppose that the coin tossing machine were to produce as output:

0000000000000000000000000000000000000 11111111111

Surely we would strongly suspect that things had changed – that they were not now going on as they did to start with. Moreover in any inductive situation at all, this sort of change can easily be imagined (as Hume himself was fond of emphasising). The very fact that we would in such a case see ordering as a relevant consideration again casts serious doubt on whether we would ever in practice consider a sequence of events to be literally exchangeable, or even if we did so initially, that we would then persist in Bayesian conditioning on that basis in the teeth of such evidence.

In accounting for why the “dynamic assumption” of Personalism had previously been overlooked, Hacking indicates how the older theories of probability might have made it hard to see: “This assumption is not a tautology for personalism. It is a tautology for theories like Harold Jeffreys’, where a unique probability is associated with any pair *h*, *e*.” (1967, p. 314). So the Personalist can be seen as trying to have his cake and eat it – to take advantage of the objectivity conferred on conditional probabilities within the Classical/Logical Relationist tradition, while importing in his own entirely subjective (and to that extent “unaccountable”) prior probabilities. De Finetti cannot have it both ways, and therefore he has not succeeded in establishing as strong a result, in its relevance to induction, as he had imagined.

Another reason for doubting whether De Finetti has progressed very far beyond the old impasse of inverse inference is implicitly suggested by Good, when he points out a moral to be drawn from his earlier example of the coin-tossing machine:

> It seems to me that you would not accept the [exchangeability] postulate unless you already had the notion of physical probability and statistical independence at the back of your mind.
>
> ... I do not see how De Finetti would, without undue complexity, express the statement that [in the case of the coin-tossing machine] ‘*really* the trials are physically independent although we do not *know* that they are’.
>
> (Good 1969, pp. 21-2)

If De Finetti depends upon background assumptions of real physical propensities and independence to explain the exchangeable patterns of beliefs from which his theory takes off, then as far as the justification of induction is concerned, we are no further forward. No matter what my initial exchangeable beliefs may be, if assumptions of physical independence lie behind them then I am in exactly the same boat as the traditional Objectivist. And in so far as the evidence leads him to change his beliefs about the underlying physical basis of what happens, so I shall modify my beliefs for the same reason, not in accordance with my original assumption of exchangeability, but on the basis of how things now, in the light of what has happened, seem to me. Consistency *at a time* is an important virtue for any reasoner – rigid consistency *over time* can be simply a sign of obstinacy.

10.5 Williams and Stove

Before concluding this chapter, we must briefly consider one other attempt to justify induction by predicting individual future probabilities, but which attempts to do so without relying on any sort of problematic "inverse inference", whether Classical or Personalist. Instead Donald Williams (1947 chapter 4, 1953), followed by Stove (1986 chapter VI), aims to defend induction using a quite different form of "large number law" – one based on purely combinatorial considerations rather than probabilistic. Williams is emphatic that his argument relies only on "direct" probabilistic inference, typified by the "proportional syllogism":

m/n of P's are Q's

a is a P

$\therefore$ there is a probability of m/n that a is a Q

which "directly" derives a population probability from a population frequency (evidently something he and Stove find unexceptionable), rather than attempting an "inverse" argument using Bayes' Theorem to infer a population probability from the frequency in a mere sample. To justify induction, it is of course necessary in the end to vindicate some such form of inference from sample to population, but Williams finds an ingenious way to bridge this gap using only "direct" methods, by formulating a large number law based on *populations of samples* rather than populations of individuals.

The Williams/Stove large number law can be explained as follows. Suppose that we wish to discover what proportion of the individuals, within some large population of P's (say, one million or more in total), are also Q's, and let us suppose that this unknown proportion q% lies somewhere between 1% and 99%. Now imagine that we are about to select a sample of 10,000 P's from that population (Williams calls such a sample "myriadic"), and consider all the different ways in which this could be done (i.e. all the possible constitutions that such a sample could have). Clearly there is a vast number of such possible samples, some of which will contain all Q's and some of which will contain none (since our stipulations imply that the population must contain at least 10,000 of both Q's and non-Q's), but it is a combinatorial truism that in the overwhelming majority of these possible samples (at least 95% of them), the proportion s% of Q's in the sample will match the unknown population proportion q%, whatever than may be, to an accuracy of plus or minus 1%. Suppose now that we select our myriadic sample. Williams and Stove seek to justify induction from our sample to the population by the following sort of "direct" inference:

> In respect of the proportion of Q's, at least 95% of myriadic samples match the population from which they come to within 1%.
>
> The proportion of Q's in this myriadic sample is s%.
>
> $\therefore$ It is 95% probable that the proportion of Q's in the sample is within 1% of that in the population, i.e. that $s\% = q\% \pm 1\%$.
>
> $\therefore$ It is 95% probable that the proportion of Q's in the population is within 1% of that in the sample, i.e. that $q\% = s\% \pm 1\%$.

Williams is at great pains in his 1953 article to emphasise, against Kneale (1949b) and Black (1948), that the last stage of this argument is merely arithmetical rather than being any sort of inverse inference. And this is certainly true. The serious work is done at the previous stage, where a direct probability judgement is made that the sample probably matches the population, on the grounds that most samples do. The argument is thus essentially a proportional syllogism, performed in respect of a population of samples.

Williams' argument has been criticised by a number of authors (e.g. Nelson (1948), Will (1948), Strawson (1952) pp. 252-6, as well as Black and Kneale), and, when they have not been sidetracked by the spectre of alleged "inverse inference", criticised quite effectively. The essential problem is that the sort of inference from sample to population that Williams hopes to vindicate, even if considered as a proportional syllogism, can be legitimate only if the sample concerned is selected randomly or in some other way “fairly”. Nelson (1948 pp. 141-2) puts the point best:

> For sake of argument I shall allow ... that the law of compositions, derived from the law of large numbers, is applicable to all populations (e.g. the class of past, present, and future events). This law implies that the number of approximately representative subclasses is greater than the number of all other subclasses. But it does *not* imply or make probable that most of the *samples we select* will match the population. ... *Williams fails to connect these two propositions.* To do so is the purpose of randomness or uniformity. Thus in intrapolation, in which any elements of a population may be selected as members of the sample, the usual requirement is that the sample be randomly selected, in order to insure ... against a method which would make the composition of the sample a function not only of the population but also of the method. In extrapolations, in which some elements cannot be selected, a principle of uniformity is commonly believed necessary in order that a sample may be indicative of more than itself or in order that the course of experience may be presumed to supply representative samples.

As Nelson goes on to point out, Williams attempts to reply to this sort of objection on the grounds that if we have no evidence that our sample is unrepresentative, then we have no evidence to justify revising the probabilities given by the argument. Nelson’s response is to argue that such ignorance is not enough, and a positive assurance of randomness is needed: “the very notion of evidence has indispensable conditions not satisfied by ignorance”. But I think the fallacy of Williams’ argument can be highlighted more clearly by challenging the presupposition of his reply – for *we do* have evidence relevant to the question of whether the sample is unrepresentative, and we can prove that we do by considering the logic of his own combinatorial reasoning.

The point is that Williams' argument does not fairly represent the situation when we make an inductive inference: in such cases the "sample" from which we infer is entirely confined to *past* instances, whereas the inferences we draw typically concern *future* instances. The relevance of this is that Williams' large number law is derived as a result concerning the set of all the possible myriadic samples *taken from the population as a whole*. As soon as the population is partitioned, and samples taken from a limited subset to draw inferences beyond that subset, the combinatory argument is wrecked *even if the principle of partition is relatively arbitrary*. In the case of past and future, of course, we might well consider besides that the principle of partition is very far from arbitrary – we are used to the passage of time making a difference, and Williams has given us no reason to suppose that it will not do so in this case. The most that can be claimed for his argument, therefore, is that it successfully licenses an inference from sample to population when the sample is taken randomly from the *entire* population. This it can indeed do, but this is not to justify induction.

10.6 A Humean Punch Line: Why These Attempts Must Fail

The point just made against Williams is a very Humean one: for all we know things might change, and if all our information is garnered from the past, we seem unable in principle to obtain any evidence that such a change will not occur. Before moving on from this chapter, it is worth pointing out how powerful this thought is, and of how wide an application.

Suppose, then, that we had been happy to accept, at the beginning of this chapter, that the Laplacian "inverse" inference could yield knowledge of the real probability underlying the observed frequency in a Bernoullian sequence. Suppose still more, that such an inference, in the case of a totally *uniform* sequence, could yield knowledge of a genuine natural necessity underlying the observed uniformity (such an argument was indeed proposed by Clark (1983) and subsequently criticised by Millican (1986)). What then would follow about future trials? The answer is, surprisingly – nothing:

> When a man says, *I have found, in all past instances, such sensible qualities conjoined with such secret powers:* And when he says, *similar sensible qualities will always be conjoined with similar secret powers*; he is not guilty of a tautology, nor are these propositions in any respect the same. You say that the one proposition is an inference from the other. But you must 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. ... 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. (E37-8 cf. T91)

Exactly the same refutation that we saw in §4.2 works with "probability" substituted for "power". So even if the methods explored in this chapter were able to give knowledge, or probable knowledge, of past objects, we would still be unable to extend this to future objects without appealing to induction. If we are to justify induction probabilistically, therefore, we must turn to methods which instead of focusing on individual objects or events, explicitly try to bridge the gap between past and future by considering ongoing uniformity as a hypothesis to be confirmed in its own right.

Chapter 11 Continuing Uniformity (a) Mackie

11.1 The Probability of Continuing Uniformity: Introduction

We have now seen enough to confirm the impossibility of justifying induction by consideration of the probability of individual events: here the fundamental Humean objection – that induction must be presupposed if the behaviour of past events is to be taken as any indication regarding future events – seems to hold up extremely well even against the subtleties of De Finetti and Williams. We must now move on to consider another range of would-be justifications, which focus not on individual events but instead on the continuing (real or apparent) uniformities which those events have manifested in the past. The central theme of arguments of this type is the intuitively plausible idea that if a uniformity has been persisting for a long time hitherto, then this period of uniformity is unlikely to terminate in the immediate future.

The most straightforward way of pursuing this "continuing uniformity" justification strategy would be: first, to establish some original prior distribution $U(x)$ for the probability that a uniformity will last altogether for a total temporal extent x; secondly, to assume that the uniformity in question has already been observed to persist for a length of time *t*; and finally to combine these two data using an inverse argument based on Bayes' Theorem to yield a posterior distribution *D(t,p)* specifying *the conditional probability of the uniformity's continuing for at least a further time p, given that it has already been observed for time t.* As might be expected in the light of our discussion in §§8.2-3, however, there are two possible perspectives on this Bayesian procedure, corresponding to the two available interpretations of probability.

From the *Personalist Bayesian* perspective, the procedure must be such as to justify induction more or less independently of the choice of original prior *U(x)*. In other words it must be possible to show that virtually any "reasonable" original prior, meaning any prior which is not fixed from the start to be outlandishly sceptical (by, for example, assigning probability 0 to extended uniformity) can yield a posterior distribution *D(t,p)* that is such as to be "inductively favourable". There is, however, an obvious limitation, in that the usual Personalist trick – of relying on *repeated iterations* of Bayes' theorem to dominate the prior distribution step-by-step and gradually force it in the desired direction – is simply not available here. By its very nature, the "continuing uniformity" strategy appeals to just one special item of empirical evidence: the observed duration of past uniformity. So only one application of Bayes' Theorem can be made, substantially depressing the prospects of success. It should therefore come as little surprise that Mackie's defence of induction, which is precisely in this spirit, indeed fails to achieve its goal, as we shall see later in this chapter.

The *Classical/Logical Relation* perspective provides more flexibility. On the one hand it can make use of the straightforward Bayesian procedure discussed above, but this time founded on an "objective" (i.e. genuinely *a priori*) original prior distribution function *U(x)*. On the other hand, it can aim to appeal to independent logical symmetries in the inductive setup, and try by some alternative form of *a priori* reasoning to justify the claim that uniformity is likely to continue. To the best of my knowledge the former option has never been attempted, presumably because of scepticism over the possibility of establishing an appropriate "objective" prior – I shall accordingly defer consideration of this until Chapter 13. The latter option famously has been attempted, by Roy Harrod, and his ingenious argument (together with its later development by Simon Blackburn) will the topic of Chapter 12.

To anticipate the conclusions of these next two chapters, both Mackie and Harrod, in their different ways, attempt to justify induction using the "continuing uniformity" strategy but without having to rely on any specific prior probability function *U(x)* for the *a priori* likely extent of uniformity. Both are motivated by scepticism about the possibility of establishing any such *a priori* function,[66] scepticism which is entirely understandable in the light of the well-known "paradoxes of geometrical probability". However both their arguments fail, and this failure indicates that if the "continuing uniformity" strategy is to succeed at all, an *a priori* "objective" prior is indispensable. Hence in Chapter 13 we shall have no alternative but to take the bull by the horns and tackle the notorious paradoxes.

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[66] Harrod is emphatic in his rejection of any such prior: "... it has been claimed that [to show the validity of induction] we have to assume initial prior probabilities for certain beliefs. All such prior postulates have been totally dispensed with here." (Harrod 1956, p. vi).

11.2 A Criterion of Success

Before embarking on our three-part examination of the "continuing uniformity" strategy, however, we must consider what would be a suitable criterion of success for arguments of this type. The matter is non-trivial for at least two, complementary, reasons. First, we shall see that unlike the arguments considered in the previous chapter (those purporting to give a means of deriving the probability of at least some individual future events), the "continuing uniformity" defences of induction do not necessarily attempt to yield more or less determinate probabilities – they may only aspire to demonstrate a general pattern of probabilification, whereby the prolonged observation of uniformity in the past makes more likely an extended continuation of uniformity into the future. Thus if we insist that a justification of induction can count as successful only if it is able to yield determinate quantitative predictions, we shall inevitably fail to engage with these arguments.

But secondly, if in recognition of this we adopt a more liberal approach without at the same time erecting some explicit alternative criterion of success, there may be a risk of accepting arguments which superficially appear to justify induction on the basis of extrapolating uniformity, and yet in fact derive any strength they possess not from such extrapolation, but instead from would-be *a priori* and "past-independent" premises of very questionable legitimacy and scope. To illustrate this point, suppose that someone were to propose a justification of induction in the following manner:

> Let *P* be any contingent proposition which at this moment happens to be true. Then let *CP(x)* be the *a priori* probability density function for the extent of time for which *P* can be expected to remain true into the future. Clearly *CP(x)* can be non-zero only for $x \geq 0$, and although the detailed form of this function is unknown (for example it might well depend on the precise form of *P*), it would in general be extreme and arbitrary to assume that *CP(x)* is discontinuously infinite at $x=0$. But given that *CP(x)* is not discontinuously infinite at $x=0$, it follows that *P* is virtually certain to remain true for at least some further time.

Could this argument, even if reasonable as far as it goes, count as a justification of induction? I do not think so, because it fails to provide any substantial basis for inductive prediction. Simply knowing that *P* is likely to remain true for *some* further time is quite insufficient to warrant any significant use of induction – it would be consistent, for example, with the function *CP(x)* being concentrated almost entirely near $x=0$, so that even if a non-zero prediction of *P*'s continuing truth were to be warranted initially, nevertheless an instant later the already-growing temporal extent of *P*'s truth would begin to count *against* any further continuation rather than in its favour. We surely cannot consider an argument to provide a justification of induction if it does nothing more than to justify an *instantaneous* prediction of continuing uniformity, and thereafter provides no force in favour of induction rather than counter-induction (or any other anti-inductive policy).

So I would propose the following criterion for any would-be "continuing uniformity" justification of induction. Such a justification need not yield any quantitative probabilistic prediction about the future, but if it does not, it should at least give a basis for concluding in general that *the longer the uniformity in question lasts, the longer it is likely to continue*. This would most naturally be demonstrated in one of two ways, by showing that as the uniformity continues, either a constant prediction achieves a greater probability, or else that a constant probability attaches to a greater prediction. Both these forms of the criterion will arise in what follows.

11.3 Mackie's Defence of Induction

Mackie sets himself the task of justifying a prediction of general uniformity for a limited period, a prediction which can then itself be used to support other, particular inductions. He considers first the simple pair of rival hypotheses, that the world's ways of working are completely uniform throughout, and that they are completely random. "If we had just these alternatives to choose between, it would be reasonable to prefer the former in the light of our observations, unless it was antecedently almost infinitely less probable than the second." (Mackie 1979, p. 124). And to assume that it was so vastly improbable would, Mackie says, be question-begging.

Unfortunately, the matter is complicated by the profusion of other possible hypotheses, even if we accept it to be overwhelmingly probable that the world has in fact been completely uniform during the period of our past observations. For it could be that this uniformity has a limited temporal (or spatial) range, beyond which it terminates or gradually fades out. These hypotheses of extensive but limited order have yet to be dealt with, and Mackie's central argument is directed to this purpose:

> First let us consider the indefinitely large set of hypotheses all of which assign the same spatio-temporal extent to uniformity, but locate it differently .... It seems a legitimate application of the

> principle of indifference to assign equal antecedent probabilities to the various hypotheses of this set. Some of them, however, will have been ruled out by the already-observed spread of uniformity. ... Then, of the hypotheses not so ruled out, if the extent which is common to the hypotheses of the set ... is considerably greater than the observed spread of uniformity, relatively few will say that uniformity will terminate either at once or very soon .... So, taking the non-yet ruled-out hypotheses of the set as equally probable, we can conclude that, even if uniformity lasts only for this limited extent, it is not likely to end very soon. But, secondly, let us consider the set of sets of such hypotheses .... Clearly, the shorter the extent characteristic of each set of hypotheses, the smaller the proportion of the hypotheses of that set that will cover the observed spread of uniformity .... Consequently, by an inverse probability argument the observation of ... a certain spread of uniformity raises the probability that the extent of uniformity is considerably greater than that spread much more than it raises the probability that that extent is equal to or only a little greater than that spread. Now, ... we can appeal to a ... principle of tolerance to justify our not giving a zero antecedent probability to all hypotheses assigning more than a certain extent to uniformity. So long as the greater-extent hypotheses are not ruled out by such an unfair initial assignment, they can come out more probable in the end, their probability being raised more by the observation of some considerable spread of uniformity. And, as we have seen, once we have confirmed such a greater-extent hypothesis, we can go on by a direct probability argument to infer that the uniformity is not likely to end very soon.
>
> (Mackie 1979, pp. 125-6)

Mackie's informal presentation of his argument seems very persuasive and plausible, and to appreciate its difficulties we shall have to examine it mathematically.

11.4 The Mathematics of Mackie's Argument

To commence our analysis, let us assume that one of Mackie's hypotheses of limited order is correct, and that we have already observed uniformity for a length of time t, predicting that it will continue for a further time p. Now if $U(x)$ is the probability density function representing the initial probability that uniformity will extend over a total time x, then since, by the Principle of Indifference (which Mackie endorses), all hypotheses assigning an equal extent to uniformity are equally probable, it follows that the probability that uniformity is of duration x and begins at some point within a certain interval of time i is proportional both to $U(x)$ and to i, say $kiU(x)$, where k is some constant.[67] With this information, we wish to find the probability that uniformity will continue for at least a further time p, fulfilling our prediction to that effect.

Consider the set of hypotheses whose characteristic extent $(x + t)$ exceeds the observed spread of uniformity t by a time x (where $x > p$):

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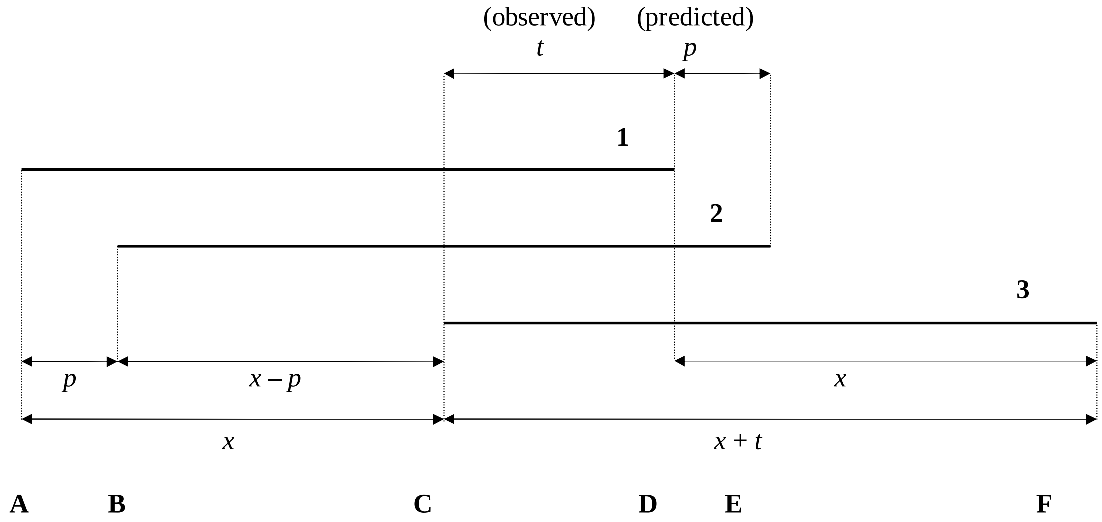
[67] For the sake of representing Mackie's argument as faithfully as possible, we must here put to one side certain complications arising from questions about the finitude or infinitude of time itself. For if time is finite, then the extent of uniformity must presumably "fit inside" that finite period, which would complicate our simple $kiU(x)$ formula owing to edge effects. But on the other hand, if time is infinite, then on a standard "proper" interpretation of probability functions, the value of the constant k must inevitably be zero, making nonsense of the cancellation by k in the mathematics that follows. In brief defence of Mackie's form of argument we can note: first, that in such an *a priori* argument the assumption of an infinite space of possibilities for temporal extent seems the more appropriate; and secondly, that precisely because k does cancel out in the subsequent mathematics, we have here a typical situation in which the use of an "improper" initial probability density function is considered by many theoretical statisticians to be quite acceptable (see also §§13.2 and 13.5 below).



Lines 1, 2 and 3 represent three possible uniformities of duration ($x + t$), all of which account for the observed uniformity CD of length t. Uniformity 1 ends immediately at D, 2 continues for a further time p, while 3, which began at C, the time of our first observation, finishes at F after a hitherto unobserved time x. There is, then, a range of possible uniformities of the given duration ($x + t$) that are consistent with our observations: they all start at different points within the period AC, and of these, the uniformities starting within the period BC will yield successful predictions. As Mackie says, the proportion $(x – p)/x$ (i.e. BC/AC) of successful predictions will grow considerably as x is increased. In addition – Mackie's second point – a greater value of x will bring a greater range x (i.e. AC) of possible (starting points for) uniformities, and we can therefore use an inverse probability argument to confirm those hypotheses involving a large value of x, at the expense of those involving a smaller value.

For any *particular* value of x, the initial probability of a uniformity of extent $(x + t)$ which will cover the observed spread of uniformity t is $kx \cdot U(x + t)$. So the initial probability of a uniformity of *any* extent which will cover this spread is equal to the sum of the function $kx \cdot U(x + t)$ for all possible values of x from zero to infinity – that is, $k\int_0^\infty x \cdot U(x + t)\,dx$. In this sum, the multiplication by x will give greater weight to $U(x + t)$ for larger values of x, and this is the basis of Mackie's inverse probability argument. But it should be noted that this weighting is quite independent of t – no matter how much uniformity is observed, the probability that total uniformity will last for a period which exceeds observed uniformity by an excess time x is raised, on account of that observation, in proportion to x alone.

Let us now move on from Mackie's inverse probability argument to his direct probability argument. As we have seen, if total uniformity exceeds observed uniformity by a time x, then the probability of a successful prediction to the effect that uniformity will continue for a time p (where $x > p$) is $(x - p)/x$. But again, this value is completely independent of t – the probability in no way improves as our observation of uniformity continues. So neither part of Mackie's argument can justify at all taking the past as a rule for the future, for if the past is to be a rule for the future, then in accordance with our criterion developed in §11.2 above, at the very least a greater observed regularity must give greater strength to a constant prediction. And Mackie's reasoning here makes no mention of the extent of the observed regularity: "t" appears only in the argument-place of the function $U(x)$, which he does not discuss.

11.5 Satisfying the Criterion

If induction is to be justified by some method such as Mackie's, then this will depend crucially on the behaviour of the function *U(x)*. We can establish the condition for such a justification using Bayes' theorem in its simplest form:

$$P(h/e) = P(e \,\&\, h) / P(e)$$

Our "evidence" *e* is the observed uniformity *t*, while the predicted "hypothesis" *h* is a spread of uniformity covering *both* the observed time *t* *and* the predicted time *p*. In this case, then, the initial probability of *both* "evidence" and "hypothesis" – P(*e* & *h*) – is equal to the initial probability of the "hypothesis" alone, since any example of the latter must be an example of the former as well.

As we worked out above, the initial probability of the observed uniformity *t* is $k\int_0^\infty x \cdot U(x + t)\, dx$. So by exactly parallel reasoning, the initial probability of the observed and predicted uniformity ($t + p$) is $k\int_0^\infty x \cdot U(x + t + p)\, dx$. Applying Bayes' theorem, we therefore reach the following result:

Probability of a successful prediction

$$= \quad k\int_0^\infty x \cdot U(x + t + p)\, dx \Big/ k\int_0^\infty x \cdot U(x + t)\, dx$$

$$= \quad \int_0^\infty x \cdot U(x + t + p)\, dx \Big/ \int_0^\infty x \cdot U(x + t)\, dx$$

To satisfy our criterion for showing the past to be a rule for the future, this probability must grow as t increases. This condition will be fulfilled if $U(x + p)/U(x)$ is an increasing function of x;[68] but this is hardly surprising, since that is essentially the same condition required by Bayes' theorem if it is to be the case that the longer a regularity goes on, the greater the chance of its continuing for a further time p.[69] Clearly, to claim that this is so is to beg the very question at issue: to assume that the past *is* a rule for the future.

11.6 The Incompleteness of Mackie's Argument

Mackie's defence of induction is incomplete because he considers only the two possibilities, that the past is *some* positive rule for the future in respect of the maintenance of uniformity, and that the world is "totally random". He can then claim, apparently quite reasonably, that a dogmatic assumption of randomness would be extreme and arbitrary, and that we must accordingly "allow from the start that there is some better-than-zero probability of some not purely random pattern" (Mackie 1979,

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[68] Since in that case the functions $U(x + t + p)$ and $U(x + t)$ provide a more favourable ratio as t gets larger while at the same time the minimum arguments for which they are evaluated – i.e. when $x = 0$, giving minimum arguments of $(t + p)$ and t respectively – increase, thus excluding those lower valued arguments which would provide a less favourable ratio.

[69] Strictly, Bayes' theorem yields the condition that $\int_{t+p}^{\infty} U(x)\,dx \Big/ \int_{t}^{\infty} U(x)\,dx$ be an increasing function of t, but subject to appropriate regularity conditions this comes to the same thing.

pp. 126-7). In the terms of our discussion above, Mackie assumes that either $U(x+p)/U(x)$ is an increasing function (i.e. that the longer a regularity goes on, the longer it is likely to continue) or that it is constant (i.e. that the past history of a regularity has no effect whatever on its future – his talk here of "pure randomness" which makes it sound so extreme and singular is misleading, because any probability density function of the form $U(x) = ae^{-ax}$ will yield this result, and this sort of "null" hypothesis of independence between past and future can hardly be considered *arbitrarily* extreme). But nothing that Mackie has said has in any way ruled out the possibility that $U(x+p)/U(x)$ should be a decreasing (or even an oscillating) function – and if this cannot be done, then his argument cannot succeed.

To sum up, Mackie aspires to justify induction without being required to consider in detail the prior probability density function $U(x)$ that his reasoning implicitly presupposes – he presumes that it will be sufficient for his purposes merely to rule out extreme and dogmatically anti-inductive candidates for that function by appeal to a minimal "principle of tolerance". However this is far from being the case: Mackie's argument just translates the old problem of justifying inductive predictions into the new one of establishing an "inductively favourable" probability function, and mathematical analysis demonstrates that his principle of tolerance is far too weak to give any assistance in finding a solution. Any initial appearance to the contrary is perhaps due to the way in which his lucid informal presentation conceals its fundamental flaw under the veil of an equivocation on what counts as confirmation of hypotheses of "a greater extent of uniformity" – this phrase has a crucially different significance depending on whether it is interpreted as meaning a greater extent of *total* uniformity, or a greater extent of *unobserved* uniformity, and it is perfectly possible for the one to be confirmed while the other is not. Thus although it is indeed obviously true within Mackie's probabilistic framework that as *observed* uniformity continues ever longer, this will inevitably raise the probability of a longer *total* uniformity, it does *not* follow at all that the probability of a certain length p of *unobserved* uniformity will be likewise raised. For if the *unobserved* component of the prediction is held constant, then as the observed length t is increased, so also the total predicted length $(t + p)$ will also be increased – the *total* prediction is not constant, and any additional confirmation conferred on the new longer prediction by comparison with the previous shorter one has therefore to be derived from the asymptotic behaviour of the function *U(x)* rather than from the sort of general argument that Mackie provides. Hence Mackie's aspiration, to justify induction probabilistically using Bayes' theorem but without specific consideration of initial probabilities, cannot be fulfilled.[70]

[70] The argument of this chapter is based on Millican (1982), on the draft of which John Mackie kindly commented prior to publication. He agreed that the mathematical analysis provided a faithful representation of his reasoning, and generously conceded that he could see no answer to it apart from the possibility of appealing to some sort of logico-pragmatic justification for presupposing an inductively favourable prior probability function *U(x)*. He also suggested two possible approaches along these lines, which I accordingly discussed in Section II of the published paper – unfortunately they encounter analogous problems to those that beset the traditional "pragmatic" justification of induction, as explained in §7.5 above, and can be shown to fail for very similar reasons.

Chapter 12 Continuing Uniformity (b) Harrod and Blackburn

12.1 Harrod's Initial Argument, and an Objection

Harrod first presented his ingenious justification of induction in the article "Induction and Probability" (1951), and further developed it in his book *Foundations of Inductive Logic* (1956). However the central idea, on his own account, dates back to an article on memory that he published in *Mind* in 1942:[71]

> "The principle for which I argue can only be established by reference to the general nature of the universe. Of this in a certain sense we know nothing *a priori*. It might be a Heracleitean flux through and through, or it might be uniform through and through, or it might be any form of admixture. But suppose that it were possible to discover by experience that it was not Heracleitean through and through, would anything follow? Let us suppose that by experience it was discovered

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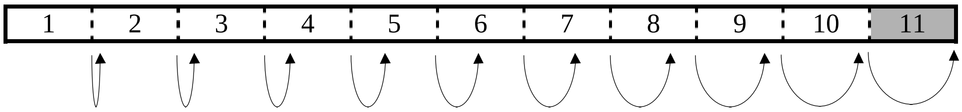
[71] It seems possible that Mackie's approach was also inspired by this thought of Harrod's, for he begins his discussion with a strikingly similar dichotomy between uniformity and randomness, and pursues the same objective. Moreover his method of argument can be seen as an attempt to follow through Harrod's central idea (that one is *a priori* unlikely to be on the extreme edge of any local uniformity), but in a way that enables the crucial probabilistic conclusion to be inferred using a direct "time-symmetrical" application of the Principle of Indifference, avoiding the asymmetries that as we shall see ultimately undermine Harrod's own thought experiment (notably the fact that one has experienced only the current uniformity's past, while making a prediction about its future) .

> to have certain stable elements in some part of it. ... Now, if contact has been made with certain stable fragments, it is improbable at any time that one is on the extreme edge of those fragments. Whatever their size, it is much more probable that one is at some distance from the edge." (quoted in Harrod 1956, p. xii)

His principal argument is essentially an attempt to flesh out mathematically this central idea, that if uniformity has characterised our experience hitherto, it is very unlikely that the current "fragment" of stability, whatever its absolute extent, is just about to end. In his book, he introduces the argument as follows:

> "We are starting, as we must in a fundamental analysis of induction, from a condition of total nescience. Before having made any inductions, man could know nothing whatever, except what is under his nose. Consider a journey by such a nescient man along a continuity. The continuity may consist of a uniform colour, texture or sound, or of a repeated pattern .... Note that at this stage we are concerned with one continuity only, not with a class of continuities. Some memory is required for this experience. We need, therefore, a vindication of the informative nature of memory; this is fully elaborated in a later chapter. ... Let a conclusion be proposed that this continuity will continue for a length constituting at least one-tenth of the length for which it has already proceeded. In more general terms we may suppose a belief in a continuance for at least $1/x$ of the length for which it has already proceeded. If we entertain the belief of at least one-tenth, which is the conclusion of our argument, continuously from the beginning to the end of the journey, it is quite certain that we shall be right ten times for every once that we are wrong. We shall be right during the first ten-elevenths of the journey and wrong during the last eleventh. If we entertain the belief of continuance for at least $1/x$, it is quite certain we shall be right x times for every once that we are wrong. This, in accordance with the traditional notation, gives a probability of being correct of $x/x+1$. The probability of the belief in continuance will be higher, the more modest the extrapolation. (Harrod 1956, pp. 52-3)

We can set this out diagrammatically, representing the continuity (which is of unknown length) as a rectangle divided into eleven equal segments:[72]



The man sets out along the continuity from left to right, and as he goes, he repeatedly makes predictions that it will continue for at least one tenth of the extent that he has already observed – in the diagram just ten such predictions are shown as arrows, but it is supposed that he makes them constantly and without interruption (let us call any such prediction a "one-tenth-extrapolation", or simply an "extrapolation" for short).[73] Obviously the man does not know which of his one-tenth-extrapolations will succeed and which will fail (he remains unaware of what proportion of the continuity he has traversed until he reaches its end), but we can see from the diagram that all of those that he makes during the unshaded segments of his journey (i.e. the first ten-elevenths) will succeed, whereas all of those made during the shaded eleventh segment will fail (the rightmost arrow in the diagram, starting from the boundary between the tenth and

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[72] For the sake of simplicity and concreteness, in what follows I shall generally deal only with the case of x=10, and silently adapt quotations from Harrod accordingly. Much of his own discussion seeks to be more general (though sometimes inconsistently, for example on p. 57 of his book), but the logic of his argument is completely unaffected by the particular value chosen, except that the relevant proportions and probabilities, notably $x/(x+1)$ and $x^2/(x+1)^2$, obviously increase for greater values of x.

[73] Again for simplicity I shall silently substitute this terminology of "making extrapolations" into quotations from Harrod, since his own terminology of "giving answers" is infelicitous and potentially confusing within short extracts.

eleventh segments, represents the very last one-tenth-extrapolation to succeed). This calculation is quite independent of the length of the continuity,[74] and Harrod accordingly suggests that it is tempting to conclude, as an *a priori* truth, that any one-tenth-extrapolation has, in the absence of special considerations, a probability of 10/11, or in other words approximately 0·91.

Although Harrod modestly admits that he had originally been inclined to draw this conclusion himself,[75] he goes on to point out a crucial objection:

> We now proceed to the next stage of our argument. It may be objected that this probability number [*viz.* 0·91 or whatever] would not have relevance to any actual situation where someone was striving to enlarge his knowledge .... The ground of the objection is that ... at any point of the journey ... some of the one-tenth-extrapolations have already been entertained and proved correct. But the traveller is interested in what lies ahead, and will therefore not deem this probability number to be relevant to his case. Even those one-tenth-extrapolations already made that have not yet been proved correct, viz. the last one-eleventh of them, must be assumed for his purposes to have been correct if he is to assign to a further continuance of one-tenth any positive probability at all; for if any of the one-tenth-extrapolations already made are false, the extrapolation that he gives now and all future extrapolations must be false also."
>
> (Harrod 1956, p. 54)

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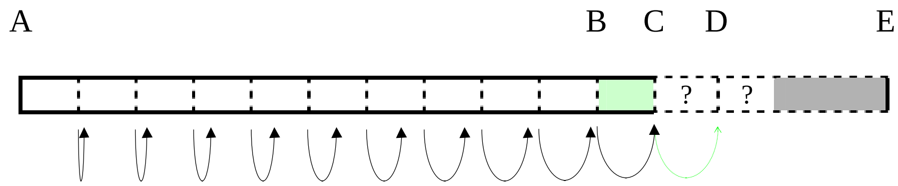
[74] As long as that length is finite – obviously in an infinite continuity *every* one-tenth-extrapolation will eventually turn out to be true. We shall ignore this complication while discussing Harrod's argument (as he largely does himself), because the only significance he sees in it (1956, p. 64) is to change the relevant probability judgements from equalities (e.g. "Probability = 10/11") to inequalities (e.g. "Probability ≥ 10/11").

[75] In his Preface (1956, p. xiv), Harrod credits J.O. Urmson with having noticed this difficulty.

Again it is helpful to illustrate the point diagrammatically:

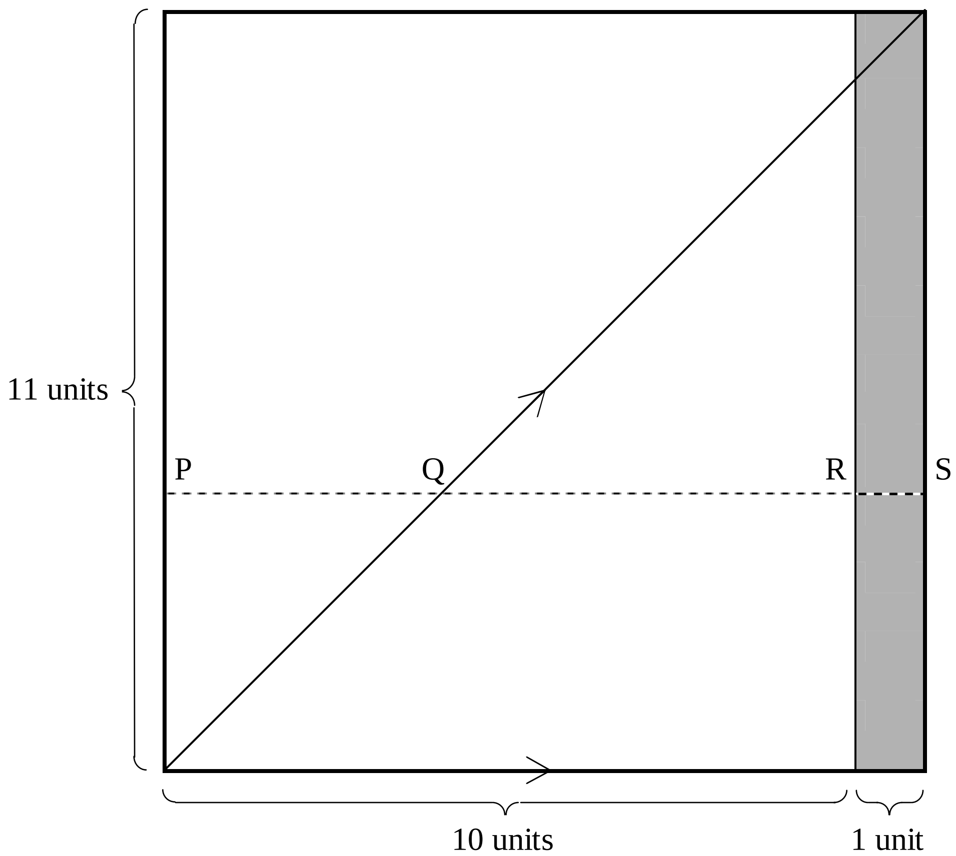


Here C represents the point reached by the traveller, and the ten unshaded segments (from A to B) the first ten-elevenths of the continuity *so far observed* (the traveller does not yet know how long the continuity will go on, so he cannot of course tell what proportion the distance travelled AC is of the *total* continuity AE). All of the one-tenth-extrapolations made during the journey AB (a few are shown, as solid arrows) have therefore already been verified, and those made during the lightly shaded segment BC – which are "pending" and not yet verified – will also have to turn out true if the currently contemplated extrapolation CD (shown as a dotted arrow) is itself to have any chance of being successful. So although the traveller indeed knows *a priori*, on the basis of the first stage of Harrod's argument, that ten-elevenths (roughly 91%) of the overall continuity AE will ultimately turn out to have yielded true extrapolations, he also knows that a particular section of that continuity, namely AB, has *already* yielded 100% true extrapolations, so that the proportion of true extrapolations made over the remainder BE must inevitably be less than the overall 91%. How much less he cannot tell, because as was pointed out earlier, he does not yet know the length of the total continuity AE, and so cannot assess what proportion AB/AE of the total set of true extrapolations have already been verified. Moreover of those extrapolations made during BE, which might in the worst case *all* turn out to be false (if the continuity in fact ends immediately at C), the traveller knows that those made within the lightly shaded interval BC are, so to speak, "ahead in the queue" of the extrapolation CD which he is now contemplating. All things considered, therefore, Harrod recognises that it is quite impossible, on the basis merely of the first stage of his argument, to justify the claim that the current extrapolation will probably succeed.

12.2 Harrod's Revised "Square Array" Argument

Harrod believes that he has a way of overcoming the difficulty that he himself has raised against his initial simple argument:

> The next step is to consider this series [of one-tenth-extrapolations] from every point of view, namely from the point of view of every position on the line. The total array of extrapolations, namely the series of extrapolations considered in turn from every point of view, may be represented by a square, each side of which has eleven units. The horizontal side of the square is simply the series of extrapolations; the vertical side of the square represents the number of times that this series has to be surveyed, if it is surveyed from every point of the journey.
>
> ... the whole area on the left-hand side of the diagonal represents, within the total array of extrapolations, all the extrapolations that have already been given considered from every point of view; the area of the right-hand side represents the extrapolations still to come. Taking the journey as a whole and surveying it successively from every point of view during the course of the journey, it is clear that half the total array of extrapolations belong to the past. In order to get a probability that will be relevant to a traveller in the course of the journey, it is necessary to subtract extrapolations already given. Therefore, in estimating this probability we should concern ourselves only with the extrapolations on the right-hand side of the diagonal.

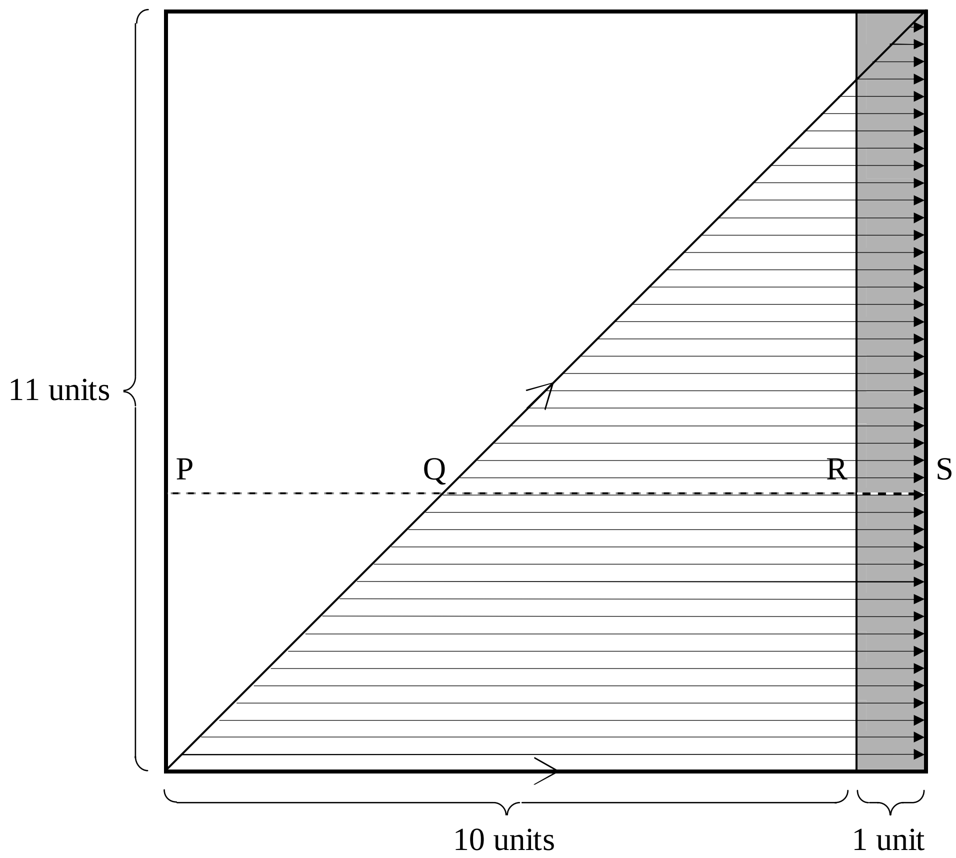


It is to be noted that in the total array of extrapolations, equal weight is given to each of the series as surveyed from each point of view. This implies that the traveller is equally likely to be at one point as at any other point of his journey. This is *not* an application of the Principle of Indifference. It is by definition quite certain that the traveller will be an equal time in each ... equal part of the line ....

In the figure an area on the right-hand side of the square has been shaded. This is a rectangle equal to one-eleventh part of the whole square. Within this rectangle all the answers are false; in the remainder of the square all the answers are true. ... To assess the probability for a traveller, who has already proceeded part of the way – he knows not what proportion – we must subtract extrapolations already made. ... That leaves the extrapolations to the right of the diagonal. The probability of being correct in a one-tenth-extrapolation is represented by the ratio of the unshaded area on the right of the diagonal to the whole area on the right of the diagonal. ... the ratio of true extrapolations to all extrapolations, after subtracting extrapolations already made as surveyed from every point of view, is seen to be $10^2/11^2$.

(Harrod 1956, pp. 55-7)

The logic of Harrod's reasoning here can perhaps be clarified if we add to his square diagram some additional arrows representing a number of possible series of repeated extrapolations. Each such series begins at some point within the continuity, and these points are evenly spaced from the continuity's beginning to its end:



Harrod's idea is that we can allow for the disregarding of past extrapolations by considering only *the series of those that are future* at each point along the continuity,[76] and by calculating the average proportion of such future extrapolations that will turn out to be true. For example point Q in the diagram is roughly 4/11 of the way along the continuity, and a traveller who starts from Q will make correct extrapolations from Q to R (i.e. roughly 6/11 of the continuity), and false ones from R to S (exactly 1/11). Hence his series of future extrapolations, which lasts altogether around 7/11 of the continuity, will contain roughly six true extrapolations for every one that is false. This ratio of true to false extrapolations within each series will obviously be more favourable the earlier in the continuity the series starts (e.g. the most favourable ratio, of ten true to one false, will be achieved by a traveller who starts his series of extrapolations immediately the continuity has begun, while a traveller whose series starts in the last eleventh of the continuity will make no true extrapolations whatever).

In the diagram we have represented the possible series of extrapolations from, so to speak, a "God's eye" perspective, but of course Harrod's "nescient" traveller cannot know which point on the square's diagonal corresponds to his own current position – though he knows how long the continuity has already lasted in *absolute* terms, he has no idea what its total length will be, and therefore no idea what *proportion* of it is past

[76] Strictly, Harrod usually discusses this more in terms of *an ongoing series of extrapolations from the beginning of the continuity to the end*, in which the traveller however focuses only on the proportions of true and false extrapolations within *the subseries of those that are future*. Obviously both methods of expression come to the same thing, but it is far simpler to discuss the issue in terms of a series that begins from the current position.

and what proportion future. Hence, also, if he now begins his own series of one-tenth-extrapolations, he will be unable to work out in advance how large a proportion of this series will turn out to be true and how large a proportion false. But what he can reasonably do, according to Harrod, is to take an average over all the possible series, giving equal weight to each point on the diagonal. If he does so, he will find that, on average, these series contain 100 true extrapolations for every 21 that are false, so given his total ignorance of his relative position along the diagonal, Harrod concludes that the nescient traveller is warranted in taking the probability of his own current extrapolation to be 100/121, or around 0·83.

Before proceeding to discuss the logic of this argument, it is worth noting that if successful it would clearly satisfy the criterion developed in §11.2 above. For each extrapolation that it credits with a probability of 100/121 extends precisely one tenth as far as the continuity already observed – so as the continuity lengthens, the "equiprobable" predictions lengthen too. Harrod explicitly points out the moral:

> We must now proceed to the next, and crucial, stage in the argument. ... Suppose that the traveller has already proceeded for ten yards. ... Therefore the probability of the continuity proceeding for at least a yard is, in these circumstances, 100/121 (= $10^2/(10+1)^2$). If, on the other hand, the continuity has already proceeded for twenty yards, the probability of its proceeding one yard more is equal to the probability of its proceeding for one-twentieth of its length to date; x is in this case 20 and the probability of one yard more is 400/441 (= 109·747/121). Thus the probability of continuance for at least one yard has substantially risen. ... Similarly ... we may infer an increasing probability of the journey lasting for at least one yard more as the journey proceeds.

> This is true for any other conventional unit of measurement. It may be called the law of increasing probability of continuance. *This is the Principle of Experience.* (Harrod 1956, pp. 62-3)

Harrod claims, then, to have provided an argument which can confer a determinate and substantial probability on a prediction, and which can moreover raise the probability of any given prediction arbitrarily close to 1 if the preceding continuity has already persisted for a sufficient extent.[77] We must now investigate whether his "square array" argument is indeed powerful enough to deliver such a momentous result.

12.3 Blackburn's Discussion of Harrod's Argument

Although Harrod's argument is perhaps the best known of the probabilistic attempts to justify induction, it has on the whole been very poorly discussed by critics. Its initial publication in 1951 seems to have gone unremarked (Kneebone 1951 barely mentions it), and despite the rapid appearance of two hostile responses (Bronowski 1957 and Popper 1958) in the *British Journal of Philosophy of Science* following its restatement and development in Harrod's book (1956), the objections offered by these eminent critics are unclear and insubstantial, and Popper's in particular was soon answered forcefully by Harrod himself (Harrod 1959). Kyburg (1964) gives the argument a brief

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[77] On these principles, for a prediction of one yard to achieve a probability of p, where p is a value arbitrarily close to 1, it must simply be the case that when the prediction is made the previous length of the continuity, in yards, has reached at least $(p+\sqrt{p})/(1-p)$ – slightly less than $2/(1-p)$.

mention in his review of "Recent work in Inductive Logic", but he only describes it in its initial unrevised form, and confines his criticism to briefly reiterating a dubious objection of Bronowski's,[78] and then raising a second difficulty which seems very much in the same spirit as the one that Harrod himself had urged against his initial argument, and which had indeed prompted him to develop the revised version.

Ayer's paper "Has Harrod Answered Hume", published in 1970 but more widely available after its inclusion in *Probability and Evidence* (1972), was therefore the first substantial published critique of the revised argument, but disappointingly Ayer focuses mainly on relatively peripheral issues such as the place of memory in Harrod's account and some complications in the notion of a "continuity". His only significant criticism of the *logic* of the argument is very cursory, and moreover somewhat gives the impression of ignoring the declared purpose of Harrod's averaging procedure:

> Does [Harrod's] second formula meet the difficulty? I do not think that it does. It does not discount the past in the way that is required. What the formula yields is the result of neglecting past successes from the point of view of every position on the line; what it does not yield is the result of neglecting past successes from any given point of view. But this makes it useless to the traveller. It is of no interest to him to know that the ratio of future successes to failures in general

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[78] Kyburg summarises Bronowski's objection thus: "the argument holds only for nice regular Euclidean-like spaces; it wouldn't hold for a space with more fence per pasture". But as Blackburn points out (1973, p. 138) this objection is quite incoherent, because Harrod uses the illustration of a spatial journey only as a metaphor for the temporal (and therefore continuous one-dimensional) experience of a continuity – hence any additional dimensions and properties of space or spaces, whether Euclidean or non-Euclidean, are entirely irrelevant to his reasoning.

> is $x^2/(x+1)^2$. He wants to know what the proportion of his future successes will be, from the position which he actually occupies, and this the formula does not tell him. Since it treats his position as an unknown factor, it is not to be expected that it should.
>
> It would appear, then, that the principle of indifference, in the form of the assumption that the traveller is as likely on any given occasion to be at any one point in the line as at any other, is required after all. (Ayer 1970, p. 107)

It is surprising that Ayer's critique is so unfocused, and that its fundamental objection culminates with the supposedly devastating accusation that Harrod must depend on the Principle of Indifference. For in 1969 Ayer had examined a PhD thesis by Simon Blackburn that was later to appear as a book (1973), in which Blackburn discusses Harrod's argument in depth, restates it in a manner that *explicitly* relies on a "defensible" version of the Principle of Indifference, and then, despite his evident sympathy for Harrod's project, provides the only clear statement so far of a genuinely powerful objection to the logic of the revised argument. We must now examine this objection, and having done so, will then turn to consider Blackburn's own suggested twist on the argument, by which he believes that it can be resuscitated to serve as the basis for a successful defence of induction against the sceptic.

Blackburn's objection to Harrod's revised argument "is the precise counterpart of the point which rendered revision of the argument necessary in the first place". And this objection is, that when we are about to make a one-tenth-extrapolation, we cannot assess its probability purely on the basis of the 100/121 proportion of successful extrapolations in the total aggregate of Harrod's series. For "we do know something further about our prediction: it is not in the chunk of the total aggregate which consists of all the regular predictions made by any successive starter from the beginning until [ten-elevenths of the continuity so far observed] had passed" (1973, pp. 146-7). This way of expressing the objection appropriately stresses its similarity with the difficulty that Harrod had recognised in his initial argument. But to emphasise that it does indeed hit home just as effectively in the case of the revised argument, it may be worth spelling out the point in a way that more directly reflects Harrod's method of presenting that second argument, focusing on the "vertical" progression "up the square array" through the series of extrapolations, rather than on the "horizontal" progress "from left to right" through the continuity itself.[79]

In these terms, then, the thrust of Blackburn's criticism is that when the traveller begins to make his series of one-tenth-extrapolations, he is not entitled to assume a 100/121 probability of successful prediction, because the 100/121 proportion is a uniform average over all the possible series (from the very first series which began at the start of the continuity, to the very last series which will begin just as the continuity ends), whereas the traveller's knowledge is not uniform in this respect: *he already knows that some of the continuity has passed, and hence that the series he is about to initiate is not one of the very earliest.* Hence the overall average must be modified to exclude these early series, and since they are the series for which the proportion of successful extrapolations is highest, this modification implies that the overall

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[79] Since the objection amounts to ruling out of consideration those parts of Harrod's square array that lie to the left of or below the traveller's current position on the diagonal, it can quite legitimately be expressed in either "horizontal" or "vertical" terms – both come to exactly the same thing.

probability of success (calculated as the average of the remainder) should be reduced from 100/121 to some lower figure. *How much* lower the traveller could only tell if he knew what proportion of the continuity he has already traversed. And because he does not know this, he is quite unable to attach a determinate probability to the one-tenth-extrapolation which he is currently contemplating.

Since this objection is indeed seriously damaging to Harrod's position, it is particularly interesting to note that he himself appears to anticipate it:

> Before proceeding it is necessary to give warning against a very strong temptation that must be resisted. It is a fact that, as the traveller proceeds with his journey, the ratio of all true future extrapolations that he will make (by predicting at least 1/10) to all future extrapolations declines. There is a strong temptation to seek some formula which would take this fact into account, and, in order to get greater refinement, make the probability fall as he progresses. Any such procedure would lead to a gross fallacy. In dealing with probability it is essential to abide by the principle that a probability is a relation between premises and a conclusion. But as the traveller proceeds with his journey he gets no new information of any kind that is relevant to the estimate of this probability, and it would, therefore, be fallacious for him to alter his estimate. The traveller is assumed to be in complete ignorance in regard to the total length of the continuity. Consequently he is in complete ignorance as to what relation the length he has already travelled bears to this total length. Therefore, he cannot bring the length of travel already accomplished into any relation to the formula. ... What we have to do ... is to decide on our formula at the outset, and then to adhere to it; we have to adhere to it because in the course of the journey we get no new relevant information.
>
> (Harrod 1956, pp. 60-61)

But Harrod's response is not convincing. If we "cannot bring the length of travel already accomplished into any relation to the formula" and hence we cannot revise the formula to take that length properly into account, then the natural conclusion to draw is not that the formula should be retained unchanged, but rather that it should be discarded on the grounds that it will now give an incorrect result. Nevertheless there is a more plausible way in which Harrod's point here can be further developed – this will soon become apparent if we return to our consideration of Blackburn's discussion.

Despite his effective criticism of Harrod's revised argument, Blackburn clearly does not regard this as robbing it of all value. For although it cannot yield what Harrod desires, namely a determinate judgement of probability for a certain class of predictions, nevertheless Blackburn sees it as providing a crucial demonstration that such predictions are at least guaranteed to have a strong connection with general success. And this connection, he believes, can be sufficient to confound the "Humean sceptic" by showing that the sceptic's own policy, of consistently refusing to acknowledge the likely success of such predictions, is itself bound to fail:

> The whole line of thought starts with the fact that over a period of time – when that time includes the start of some continuities – it must happen relatively rarely that we are in the last fraction of the duration of those continuities. The sceptic is not impressed with this; he puts weight on the existence of the duration of the continuity up till now as destroying any guarantee that a predictive policy started *now* will have a high success rate. We then point out, in revising the argument, that even taking this into account, still on the average the policy of predicting at least a one-tenth continuance time has a high success rate. The sceptic replies that this is true if we take the average of people starting to use that policy from beginning to end of the regularity; still, we are not at the beginning of the regularity, we are, in my example, ten years through its life, so averages of success rates from beginning to end of the continuity do not interest him.

> But we in turn can wonder how little interest a sceptic could be entitled to betray in such an average. Let us consider a Humean sceptic, one who thinks that ... a degree of confidence of 1/4 ... is as justified as $10^2/11^2$. ... We try to attack this claim by pointing out that 1/4 is the degree of confidence appropriate to use of a policy that goes wrong three times out of four; but, we say, it is a very rare event indeed for someone to start predicting a one-tenth continuance somewhere along a regularity, and be wrong three times out of four. The inevitable reply is that this is a rare event only in the class of possible users of that policy from beginning to end of the regularity, and we are not at the beginning, we are ten years through it. Yes, but what connection with success enables a pessimist to use *that* fact to justify, at least as far as any other prediction, the expectation that the regularity will finish?
>
> ... The idea is that the Humean sceptic's defence of confidence of 1/4 is an instance of a use of an argument which itself gives the wrong answer most of the time.
>
> (Blackburn 1973, pp. 147-8)

This is certainly a neat piece of *ju-jitsu*, and it may perhaps also provide a useful model for developing the line of thought expressed in the previous quotation from Harrod. There he pointed out that any particular duration of observed continuity (say, ten years) is a completely unknown proportion of the total continuity, and hence no specific weight can be given to it as a factor implying a reduction in the overall probability of a successful extrapolation. So on this basis Harrod might join Blackburn in responding to the latter's Humean sceptic: "I acknowledge that we are ten years through the continuity, but what enables you to use *that* fact to justify, at least as far as any other prediction, the expectation that the regularity will finish? If ten years of continuity is sufficient to undermine my expectation that this sort of extrapolation will generally be reliable, then why should not the same be true of ten days, ten hours, ten seconds, ten microseconds? And if *any* duration of continuity at all is, according to you, sufficient to undermine that expectation, then how can you reconcile this with the general high ratio of success apparently guaranteed by my revised argument?"

It seems, then, that the spirit of Harrod's argument is still alive and kicking, even if Blackburn's criticism has undermined its pretention to deliver precise probabilities for the relevant extrapolations. To assess whether it deserves this continued vitality, and whether it can bear even the reduced weight that Blackburn's anti-sceptical twist places on it, we must turn to consider some new, and more fundamental, objections.

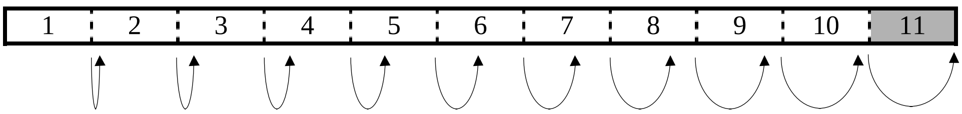
12.4 Two Decisive Objections to Harrod's Argument

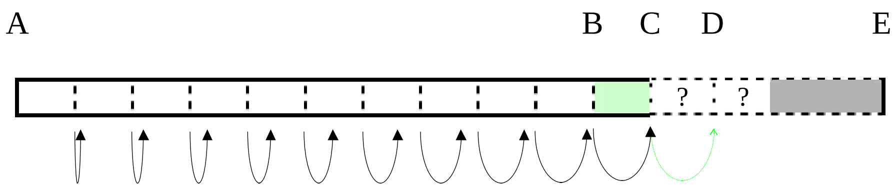
I shall now attempt to prove that, when analysed more deeply, Harrod's argument is much weaker than the previous discussion would suggest, and in fact ultimately provides not even the flimsiest basis on which to construct a probabilistic defence of induction. I shall start with two criticisms that focus on respects in which Harrod's presentation of his argument is subtly but seriously misleading, and has in fact seduced commentators into failing to notice the real underlying logic of the situation.

First, and most fundamentally, Harrod's square array completely misrepresents what is of interest to the traveller who is about to make a one-tenth-extrapolation. Harrod sees him as initiating a series of future extrapolations, and wishing to know *what proportion of the extrapolations in that series will turn out to be true*. Since the traveller does not know what fraction of the continuity he has already traversed, he cannot of course calculate this proportion of true future extrapolations directly, but he can supposedly allow for this ignorance by averaging over *all possible series* – those beginning from every position on the continuity – thus yielding Harrod's $10^2/11^2$ result. We can see from the quotation given earlier that Ayer (1970, p. 107) goes along with this perception of what the traveller wishes to know: "He wants to know what the proportion of his future successes will be, from the position which he actually occupies". And Blackburn's sympathetic treatment of the argument indicates that he too concurs in viewing it in this way.

However this is a complete illusion engendered by Harrod's clever presentation, and by his "square array" diagram in particular. For what the traveller wishes to know, regarding the series of future extrapolations that he is about to initiate, is not *what proportion of the extrapolations in that series will turn out to be true*, but simply *whether <u>any at all</u> of the extrapolations in that series will turn out to be true*. Just one solitary success will be quite enough to satisfy him in his role as a one-tenth-extrapolator, because the series of extrapolations is not random but *sequentially ordered*. The future extrapolations in that series form an orderly queue, and the particular one-tenth-extrapolation which the traveller is just about to make is firmly at the head of that queue – if *any* other extrapolation in the series ultimately turns out to be true, then this one certainly will too. Thus Harrod's square array diagram is entirely misleading – the only question that matters concerning any point on the diagonal is whether it is, or is not, within the shaded rightmost eleventh of the square, and beyond that, its particular distance from the right-hand side is utterly irrelevant. As far as the logic of the situation is concerned, therefore, Harrod would be just as well to stick with his initial unrevised one-dimensional argument.

Going back to that initial argument, then, we must recall the important objection which Harrod himself saw as decisive (discussed above in §12.1), but in doing so we can now take particular notice of a second important respect in which his presentation, even of that initial argument, lends it more credibility than it deserves. Here again are the two diagrams which I used to illustrate that discussion:





There is a fundamental difference of perspective between these two. The first, like Harrod's own square array diagram and like most of his analysis, presents the situation from a "God's eye" point of view; whereas the second represents things as they appear to the traveller: with knowledge of what lies behind him, but total uncertainty ahead. As far as the traveller is concerned, *unless he can appeal to induction* the past history of the continuity from A to C is entirely irrelevant to his situation. Seen in this light it is obvious that he gains nothing by examining the overall proportion of true extrapolations over the entire series from A to E, when he is well aware that the past extrapolations shown as solid arrows have already been confirmed. To proceed in such a manner would be like trying to improve his chance of winning a lottery by placing his ticket in a folder together with a number of winning tickets from previous weeks, on the grounds that the new ticket has thus become one of a series most of which have been successful! Viewed as a justification of induction, Harrod's argument seems ultimately no more powerful than this.

12.5 A Further Objection to Blackburn

It seems doubtful whether Blackburn's attempted rehabilitation of Harrod's argument can survive the criticisms that we have just discussed. At the very least the first of them shows that there is no advantage in moving from the initial one-dimensional argument to the two-dimensional array; while the second indicates that there is something very questionable about the traveller's drawing inferences regarding the probable success of his current extrapolation on the basis of a proportion which can only be guaranteed by including already-verified extrapolations within the relevant set, and by ignoring the fact that the members of that set are sequentially ordered. Nevertheless it might be thought that Blackburn's anti-sceptical twist on Harrod's strategy blunts the force of this latter point – perhaps the current extrapolation can indeed derive a presumption of success not on the basis of a would-be proportional syllogism that illegitimately conflates together past and future extrapolations within the present continuity, but instead by appeal to the general success of a one-tenth-extrapolation policy as it would apply more widely. In other words, perhaps Blackburn can defend Harrod's initial argument not by considering the current extrapolation as one member of the set of *one-tenth-extrapolations within the present continuity*, but instead by considering it as one member of the far larger set of *all one-tenth-extrapolations within any continuity whatever*.

The objection to this procedure, however, is that the 10/11 proportion of successful extrapolations (or $10^2/11^2$ according to the revised argument) is guaranteed for each continuity quite independently of every other continuity – 10/11 is not just an overall average for the set of extrapolations taken as a whole, but is an individual average for every distinct series of extrapolations within that overall set.[80] Hence an appeal to the 10/11 success rate of a one-tenth-extrapolation policy as it would apply within other continuities can be of no relevance whatever to the traveller making such an extrapolation on a particular occasion – everything that he knows is summed up in the final diagram of the previous section (§12.4), and as we have seen, his knowledge of the eventual 10/11 success rate for the entire series of extrapolations within his *present* continuity, which is the only relevant information that either Harrod's or Blackburn's argument yields, is of absolutely no help to him.

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[80] Perhaps this observation provides a grain of truth motivating Popper's otherwise very puzzling objection to Harrod's argument, which seems to be that because the proportion of successful extrapolations is fixed even if the traveller knows when the uniformity will end, this proportion "*cannot be a probability that may be interpreted as the degree of [the traveller's] imperfect knowledge*" (Popper 1958, p. 223). As both Harrod (1959) and Blackburn (1973, pp. 139-40) point out, this argument as it stands just seems to be a *non-sequitur*.

12.6 Scale Invariance and Infinities

Despite its logical flaws there may yet remain some attraction in the Harrod-Blackburn argument, because it does seem very appealing to base our inductive predictions on considerations that apparently apply *a priori* to any continuity quite independently of its scale. If we imagine ourselves mysteriously transported into an alien universe whose ways of working are completely unknown to us, we surely could not predict in advance what would be the typical extent of the continuities that we observe there, but it does seem plausible that having observed any particular continuity for a given time, we would thereby acquire some understanding of its overall scale and could thereafter predict with reasonable confidence that it would be likely to continue for at least a small fraction of its current extent. Such "scale-invariant" reasoning can also be appropriate in common life. Suppose that I send a letter to some organisation initially anticipating an immediate reply, but although I am assured that my letter has been received and will ultimately be acted on, no reply is forthcoming. For the first day or two I may be surprised when nothing arrives. After one week, I will adjust my expectation but may nevertheless retain a reasonable confidence that although no reply can be expected within the next day, at any rate one will probably appear within another week. After one month, I will revise my estimate again, and probably start measuring the expected further delay in terms of months rather than weeks, and so on.

We shall see in the following chapter that scale-invariance may indeed provide the best hope for a probabilistic justification of induction, though it raises a host of difficulties at very large and very small scales. Before leaving Harrod and Blackburn, however, it is worth noting that their argument, despite appearances, is not in fact thoroughly scale-invariant, because it treats the density of extrapolations made during the traveller's journey as being entirely uniform. The consequences of this are, first, that awkward questions can be raised regarding the alleged *a prioricity* of their model; and secondly, that even if their strategy were appropriate in principle, there is no reason for supposing that it would deliver the correct result.

The first of these points can be put like this. Harrod's traveller is supposed to have an *a priori* guarantee that ten-elevenths of his total series of extrapolations, from the beginning of the continuity to its end, will ultimately turn out to be verified. But of course such a claim can be strictly true only if the number of extrapolations within that series is an exact multiple of eleven. This is an irritating complication, but since the traveller is an inhabitant of a thought-experiment rather than a real human being, Harrod can apparently get round it simply by postulating that the extrapolations are made at a sufficiently high rate for any rounding error to be negligible, say one extrapolation per microsecond. All very well, but one per microsecond is not enough to ensure the literal *a prioricity* of even an approximate 10/11 ratio – suppose, for example, that we consider a world in which every uniformity lasts between 1·0 and 1·09 microseconds: time enough for one extrapolation to be made but insufficient for any at all to be verified. To ensure that his thought experiment is literally *a priori*, therefore, it seems that Harrod must postulate that the extrapolations are made continuously, thus implying that an infinite number of them occur within any finite interval. Given the obvious objection to any thought experiment which presupposes the achievement of an infinite task within a finite time, however, he would presumably be wise to portray the tale of his traveller as merely an expository metaphor, with the real logical force of his argument being conveyed by a mathematical model in which every point on the continuity is considered as the origin of an abstract and hypothetical one-tenth-extrapolation.

Once infinities enter the picture, however, the 10/11 ratio begins to be undermined in a quite different way. For now it is possible to generate other ratios by changing the mathematical model accordingly. And it is interesting to note that this can be done in a way that is more faithful than is Harrod's own argument to the scale-invariant intuitions that appear to underlie it. Suppose, for example, that we imagine an infinite series of nine-fold-extrapolations, each new such extrapolation being made just as the last is fulfilled. If the relevant continuity lasts as long as 1 second, then one of these extrapolations will predict the continuity's persistence for a further 9 seconds. If that extrapolation is fulfilled (after a total of 10 seconds), then another will immediately be made, this time predicting a further 90 seconds (to 100 seconds in all); and so on until the continuity terminates. This, however, is to look only at the very end of the series. By the time we reach 1 second we have already verified extrapolations made (in reverse order) at 0·1 seconds, 0·01 seconds, 0·001 seconds, and so on. So if an infinite density of extrapolations is to be permitted, then we can set up an infinite series of such extrapolations in which only one – the last – will be false and all the others true. If Blackburn's Principle of Indifference is applicable to this series (and it is not clear why it should not be if it is applicable to an infinite series of uniform density), then we can apparently draw the conclusion that any such nine-fold extrapolation is virtually certain to be fulfilled. And this, one might think, is rather too good to be true! Of course many other setups will also be possible, with varying density of extrapolations and correspondingly different proportions of true extrapolations within any series. In short, if we permit the mathematical tricks that Harrod and Blackburn need in order to make their form of argument genuinely *a priori*, then any number of alternative models become possible, delivering a wide variety of very different answers.

12.7 Conclusion

Our conclusion must be that Harrod's argument fails to justify induction, and moreover does so (because of its singular nature) in a way that makes it difficult for any of its logical machinery to be salvaged from the wreck. Only the hint of scale invariance as a clue to further progress seems worth preserving, though any development along these lines will clearly have to contend with potentially unruly infinities.

Blackburn's interesting twist on the argument is also suggestive, and his essential idea might well survive the demise of its original context. At any rate we have hitherto seen no reason why his method of turning the tables on the sceptic should not play a role in some future pro-inductive strategy, if only it can be harnessed to logical machinery that genuinely works.

Chapter 13 Continuing Uniformity (c) An Objective Prior?

13.1 Introduction: The Need for an "Objective Prior"

In Chapter 11 we examined Mackie's attempt to justify induction on the basis that general uniformity, having been evident in our experience for a very long time, is extremely likely to continue for a least a limited further period. Our examination concluded that his attempt failed, but that this failure need not be due to any incorrectness in the inverse probability argument he employed, but simply to the fact that such arguments must inevitably presuppose some prior probability distribution *U(x)* for the expected extent of uniformity. Put crudely, if the prior probability distribution is "inductively favourable" then an argument such as Mackie's can satisfy our criterion for justificatory success (§ 11.2), but otherwise it cannot. So this kind of argument seems to take us no closer to a probabilistic justification of induction – it simply demonstrates where the heart of the problem lies: that of establishing an "inductively favourable" prior probability distribution for the extent of uniformity.

It is important to re-emphasise that the prior probability distribution we seek, if it is to play its intended role, must be both literally *a priori* and thoroughly "objective" (a word that I shall henceforth release from the scare quotes imposed in §8.1, since no confusion with Frequentism is here at all likely). We saw in Sections 3.2 and 8.2 that Hume's argument can only be circumvented by a probabilistic justification *that makes no appeal whatever to experience*, and Section 11.1 made clear that the Personalist's method of building a justification by iteration on a non-specific "subjective" prior is in this context not an option. To provide any opposition to Hume, therefore, we must find some way of deriving an objective prior probability distribution $U(x)$ for the extent of uniformity, founded on total ignorance of matter of fact.

13.2 Finite and Infinite Applications of the Principle of Indifference

Attempts to establish objective "ignorance" priors have almost invariably been based on some form of the Principle of Indifference, or on analogous considerations of logical symmetry. But the time has come to distinguish two quite different types of application of this principle, depending on whether it is used on the one hand to apportion the "probability measure" between *a finite number of discrete possibilities*, or on the other, to assign a measure function over *an infinite range of possibilities* such as an interval of the real numbers or some geometrical space.

Even the discrete applications of the Principle of Indifference are controversial, as we saw in §9.5. But we there decided, in line with the policy announced in §7.1 and §8.3, to allow Hume's opponent the benefit of the doubt in this respect, given that

Keynes and others have demonstrated reasonably plausible ways in which the principle can be qualified so as to be confined to cases of genuine symmetry, and in circumstances where paradox can apparently be avoided.

We have also allowed to go unchallenged, within Mackie's argument (§§11.3-4), two applications of the Principle of Indifference to a continuous range: one infinite in extent (remarked on in the footnote to §11.4) and the other a finite time interval (yielding the proportion $(x - p)/x$, and in fact guaranteed to be harmless if the infinite case is legitimate). Again these seemed plausible applications, despite the fact that the infinite uniform prior is "improper" – having no finite integral – and therefore cannot represent a conventional probability distribution summing to 1. For Mackie's argument was intended to be in a sense "timeless" – the whole point of the prior probabilities was to avoid giving any privilege to one point in the universal time sequence above any other, and it was only the empirical evidence of observed uniformity, serving as an anchor for the hypothesised durations of uniformity, that broke the infinite symmetry. Once the symmetry was broken, and the limits of empirical possibility established, the improper prior could straightforwardly reduce to a uniform prior over a finite range, which could therefore be rendered "proper" by the simple expedient of rescaling.

If this sort of manoeuvre is indeed legitimate, then the question naturally arises: why we should not take the uniform improper prior over the infinite half-line, from 0 to infinity, as an appropriate function $U(x)$ for the *a priori* expected extent of uniformity? And if we are permitted to do so, then it does appear that induction can be justified by Mackie's method, because the uniform prior over the infinite half-line is, so to speak, infinitely weighted towards the upper end. To understand why we cannot complacently follow this line, it is necessary to examine the well-known paradoxes of "geometrical probability", which historically have been largely responsible for the opprobrium with which the Principle of Indifference is commonly viewed.

13.3 The Paradoxes of Geometrical Probability

These paradoxes can arise when the Principle of Indifference is applied to continuously varying quantities such as physical length, area, volume, weight, density, temperature or temporal duration. All that is required to generate paradox is that two or more such quantities be related non-linearly and known to vary within corresponding limits (for example, a cubic die known to have an edge between 1 cm and 3 cm, with a side area correspondingly between 1 cm^2 and 9 cm^2, and a volume between 1 cm^3 and 27 cm^3). Then precisely because the relation between them is non-linear, the Principle of Indifference as applied to the different quantities will inevitably yield contradictory results. Two extended examples will suffice to give the flavour of such paradoxes: the first is an adaptation of one given by Von Mises (1957, p. 77), and the second is the famous paradox of Bertrand (Bertrand 1889, p. 5).

Von Mises asks us to imagine that we are faced with a flask containing a mixture of water and wine. Now suppose we are told that the relative proportions of water and wine are somewhere between 1:3 and 3:1, but we are given no further information. The problem is then to use the Principle of Indifference to work out from what we have been told a prior probability distribution, which will provide an objective method of assessing *the probability that the mixture contains more water than wine*. We can proceed in at least three different ways:

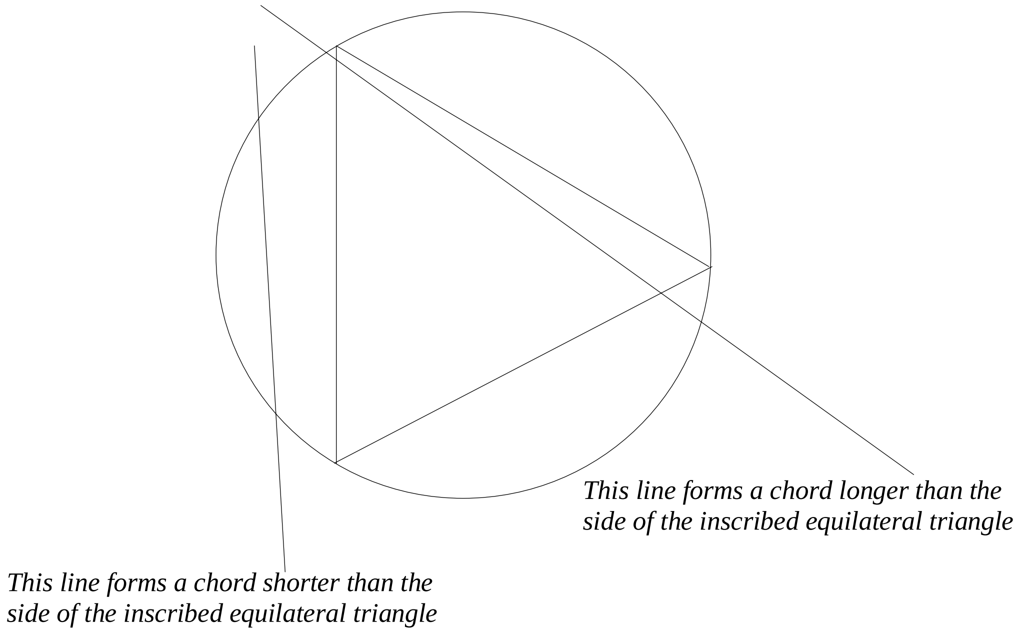
(a) On the face of it the answer is simple: our information gives no ground for distinguishing between water and wine, so the probability that there is more water than wine should presumably be the same as the probability that there is more wine than water. Assuming that the continuous nature of the distribution permits us to discount the possibility of there being exactly equal amounts of each, this reasoning yields a probability of $^{1}/_{2}$ that there is more water than wine. (Another way of reaching this conclusion is to apply the Principle of Indifference to the absolute quantity of water or wine: each constitutes between $^{1}/_{4}$ and $^{3}/_{4}$ of the liquid in the flask, a range centred on $^{1}/_{2}$ – the very amount that implies equal quantities of each.)

(b) Our information tells us that the ratio of water to wine lies between $^{1}/_{3}$ and 3 – and that is all. If we accordingly apply the Principle of Indifference to the interval between $^{1}/_{3}$ and 3 we will find that $^{3}/_{4}$ of that interval (between 1 and 3) corresponds to the situation of there being more water than wine, whereas only $^{1}/_{4}$ of it (between $^{1}/_{3}$ and 1) corresponds to the situation of there being more wine than water. Again discounting the possibility of exactly equal amounts, we are left with a probability of $^{3}/_{4}$ that the mixture contains more water than wine.

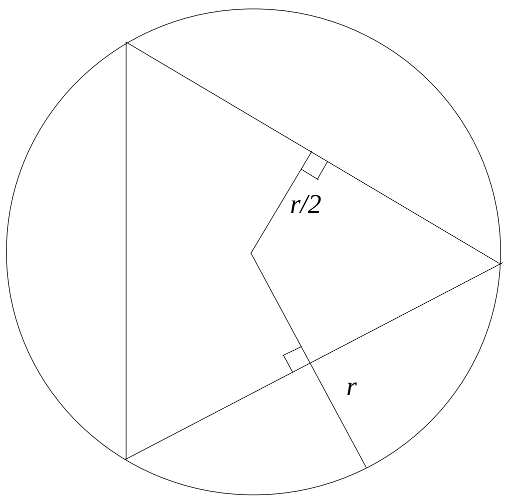
(c) Turning our attention now to the ratio of wine to water, this also lies between $^{1}/_{3}$ and 3 – so a parallel argument to that in (b) can be applied in the same way to this ratio. This yields a probability of $^3/_4$ that the mixture contains more wine than water, and hence a probability of only $^1/_4$ that it contains more water than wine.

So we are left with a paradox. Three different methods of argument using the Principle of Indifference, each based on exactly the same information, give three very different answers to exactly the same problem. And although in this particular case the overall symmetry in the situation, between water and wine, may incline us to see one of these answers (namely the first, giving an equal probability of each predominating), it is obvious that any number of variations can easily be constructed which would eliminate the symmetry whilst retaining the paradox (e.g. in Von Mises' asymmetrical original we are given only that the water to wine ratio lies between 1 and 2, so that the wine to water ratio correspondingly lies between $^1/_2$ and 1 – here the Principle of Indifference yields just two contradictory probability distributions, corresponding to our methods (b) and (c) above).

Turning now to Bertrand's paradox, we are asked to imagine a chord of a circle being drawn at random (Jaynes 1973, p. 134, suggests we think of long straws being tossed randomly into the vicinity of the circle, and confine our attention to those straws that intersect it). Any such chord must have some length between 0 (in the limiting case of a tangent to the circle) and $2r$, twice the circle's radius (in the other limiting case, of a diameter). The problem Bertrand poses is to work out, based on this information alone, the probability that any chord thus produced will have a length greater than the side of an equilateral triangle inscribed within the circle. And just as in the case of our water and wine paradox, there are at least three different methods of calculating the relevant probability using the Principle of Indifference.

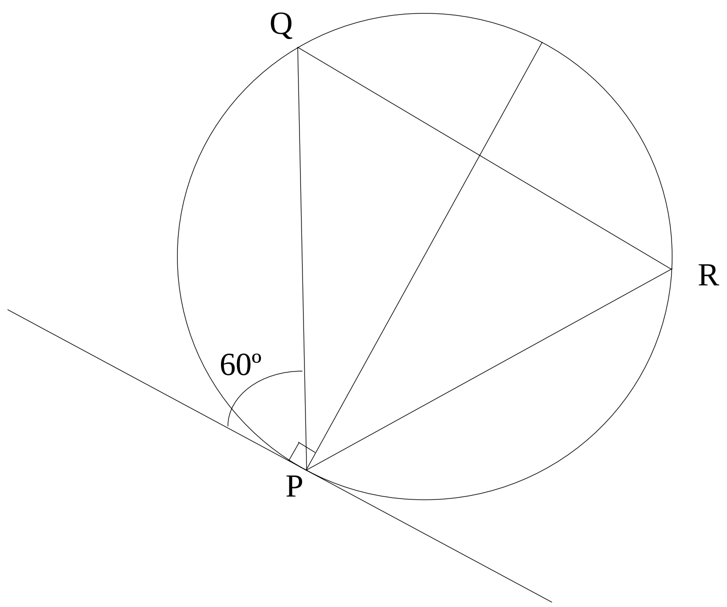


(a) The first method is based on the perpendicular distance between the chord and the centre of the circle:



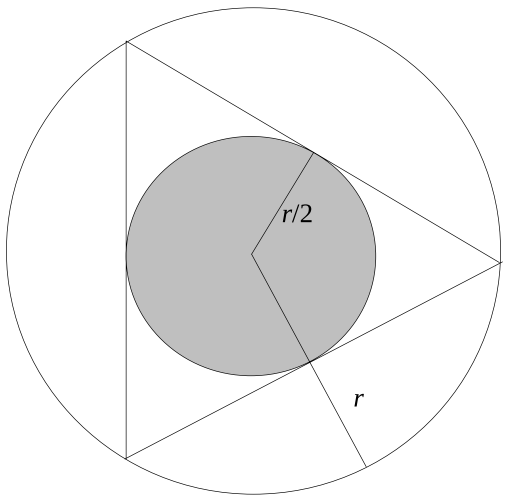
Clearly this distance can range between 0 (in the case of a diameter) and *r*, the circle's radius (in the case of a tangent). And as the diagram indicates, the chord will be longer than the side of the inscribed triangle if, and only if, its perpendicular distance from the centre lies between *r*/2 and *r* (i.e. within the top half of the total range). On this basis, therefore, the Principle of Indifference gives a result of $^1/_2$ for the required probability.

(b) The second method focuses on the acute angle between the chord in question and the tangent to the circle at the point of intersection P. As the following diagram illustrates, this angle can range from 0º (in the case of a tangent) to 90º (in the case of a diameter), and the chord will be longer than the side of the inscribed triangle if, and only if, it lies in the top third of this range, between 60º and 90º.



Applying the Principle of Indifference over the range from 0º to 90º, therefore, we obtain an answer of $^1/_3$ for the required probability. (Another way of getting this same answer is to consider the point at the other end of the chord from P as being randomly selected from the entire circumference – the relevant arc length PQ is directly proportional to the angle, so the two methods are equivalent. And it is obvious from the diagram that one third of the circumference, here between Q and R, corresponds to a chord length that exceeds the side of the inscribed triangle.)

(c) The third method is founded on the observation that any chord (except for the limiting case of an exact diameter, which we ignore as vanishingly improbable) can be uniquely specified in terms of its mid-point, which can lie anywhere within the circle. Any chord which is longer than the side of the inscribed equilateral triangle, however, will have a mid-point that lies within $r/2$ of the centre (as we saw in (a) above), and hence within the shaded circle in the following diagram:



The shaded circle has half the radius, and therefore one quarter the area, of the larger circle. Hence the Principle of Indifference, applied to the distribution over this area of the mid-point of a randomly drawn chord, yields a probability of $^1/_4$ that such a chord will be longer than the side of an inscribed equilateral triangle.

Once again we have a paradox, and of a similar kind to the water and wine. Three different methods of argument using the Principle of Indifference, each based on exactly the same information, have given us three very different answers to an identical problem. The conclusion almost universally drawn from such paradoxes is that the Principle of Indifference, even if it can be defended under certain conditions in its application to finite spaces of discrete possibilities, is ineradicably anomalous in any application to continuously varying quantities (and indeed to infinite ranges generally). And if this conclusion stands, we seem to be left with no plausible method of establishing an objective ignorance prior for the expected extent of temporal uniformity *U(x)*, and hence no possibility of justifying induction on that basis.

13.4 Indifference Amongst Problems: Jaynes' Answer to Bertrand's Paradox

Jaynes (1973), however, argues that things are not nearly so bad, at least with regard to the Bertrand Problem. For he believes that contradiction can be avoided by attending more carefully to the implicit symmetries of the problem as posed:

> Bertrand's problem has an obvious element of rotational symmetry, recognised in all the proposed solutions; however, this symmetry is irrelevant to the distribution of chord lengths. There are two other "symmetries" which are highly relevant: Neither Bertrand's original statement nor our restatement in terms of straws [being thrown onto the circle] specifies the exact size of the circle, or its exact location. If, therefore, the problem is to have any definite solution at all, it must be "indifferent" to these circumstances; i.e., it must be unchanged by a small change in the size or

> position of the circle. This seemingly trivial statement, as we shall see, fully determines the solution.
>
> (Jaynes 1973, p. 136)

Jaynes then goes on to show by detailed mathematics (too lengthy to repeat here, but similar in principle to what we shall be using in the following section) how the joint requirements of:

- *rotational invariance* (the distribution must be similar no matter how the circle – or the direction of the straw-thrower – is rotated);

- *scale invariance* (the distribution must be similar if the circle is shrunk or slightly expanded, or if its dimensions are measured in different units); and

- *translational invariance* (the distribution must be similar if the circle – or the straw-thrower – is slightly translated in any direction)

indeed determine a unique distribution for the random position of the straw or the drawing of the chord. In fact they determine the (locally) uniform and isotropic distribution, which implies answer (a) to Bertrand's problem. So the problem does after all have a unique solution, namely, that the probability of the randomly drawn chord exceeding in length the side of an inscribed equilateral triangle is one half.

The significant moral that Jaynes draws from all this is intended to be more widely applicable:

> ... the principle of indifference has been unjustly maligned in the past; what it needed was not blanket condemnation, but recognition of the proper way to apply it. We agree with most other writers on probability theory that it is dangerous to apply this principle at the level of indifference between *events*, because our intuition is a very unreliable guide in such matters, as Bertrand's paradox illustrates.
>
> However, the principle of indifference may, in our view, be applied legitimately at the more abstract level of indifference between *problems*; because that is a matter that is definitely determined by the statement of a problem, independently of our intuition. Every circumstance left unspecified in the statement of a problem defines an invariance property which the solution must have if there is to be any definite solution at all. The transformation group, which expresses these invariances mathematically, imposes definite restrictions on the form of the solution, and in many cases fully determines it.
>
> (Jaynes 1973, p. 144)

Irrespective of the merits of Jaynes' solution to the Bertrand case in particular,[81] this is an extremely interesting idea. It conjures up the prospect of rescuing the reviled Principle of Indifference in its application to at least some infinite domains, and doing so, moreover, in a principled way that is not motivated merely by ignorance or simplistic assumptions of uniformity. The Jaynesian manoeuvre even promises to reverse the traditional sceptic's taunt, that "absence of knowledge isn't knowledge of absence", for in the cases where Jaynes proposes to apply it he can plausibly claim that absence of knowledge *is indeed* knowledge of absence: what is part of a defined

[81] Jaynes' solution is discussed sympathetically by Rosenkrantz (1977) pp. 73-81, relatively neutrally by Székely (1986) pp. 43-8 and Van Fraasen (1989) pp. 306-317, and very sceptically by Nathan (1984).

problem, and what is not, can in principle be perfectly determinate even if the problem concerns partial knowledge. Particularly intriguing for our purposes, however, is that this approach has a direct application to the problem of defining our elusive temporal uniformity function *U(x)*.

13.5 Deriving an "Objective" Prior for the Extent of Temporal Uniformity

Our problem is to establish a unique prior probability density function *U(x)* for the extent of temporal uniformity, and to do so in the absence of any empirical knowledge whatever (so there is absolutely no indeterminacy about the *problem*). Following Jaynes, therefore, let us examine whether there are any fundamental symmetries in this problem that may enable us to transform it into another that is equivalent.

It might seem at first sight that *translational invariance* is a possibility, of the same kind that Mackie assumes in his own argument. However this was not translational invariance of the *extent* of temporal uniformity, merely of its *location*. Nevertheless Mackie's idea can indeed be put in Jaynesian terms, and it is instructive to illustrate the method by doing so. Suppose, then, that I am trying to work out, *a priori*, a probability distribution function *S(x)* for when uniformity can be expected to start. I now imagine an *alter ego* doing this 1000 years ago, and ask: *Would he reach the same answer?* Well yes, presumably he would, at least if there is any objective answer there to be reached. For if my distribution function is to be genuinely *a priori*, then the particular point in history at which I contemplate the problem should have no impact whatever on the distribution, and hence my historical *alter ego*'s function $S'(x)$ should be exactly the same as my $S(x)$. However now consider the *a priori* probability that uniformity starts *right now.*[82] Again, and for similar reasons, if the distribution is to have the objectivity we desire it must be the case that I and my *alter ego* reach the same answer. But if we each measure time in years from our instant of contemplation, then my *right now* – the origin of my distribution – is his 1000. Hence $S(0) = S'(1000)$, and by applying similar reasoning to instants t years before and after my present moment, we can conclude in general that $S(t) = S'(t + 1000)$. Putting this together with our earlier result that my distribution function and that of my *alter ego* are identical, i.e. $S(x) = S'(x)$, it clearly follows that both distributions are entirely uniform, justifying Mackie's informal intuition (that "It seems a legitimate application of the principle of indifference" to assign a uniform prior)[83] in an apparently rigorous manner.

Thus translational invariance seems warranted for the temporal *location* function $S(x)$, but the same does not apply to our temporal *extent* function $U(x)$, and this will highlight the considerable gulf between the traditional Principle of Indifference and Jaynes' version. On his principle we cannot simplistically argue here in the traditional

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[82] Or strictly (since $S(x)$ is a continuous function), that it starts in the interval from now onwards for a small period δx which can then be made to tend to zero in the usual way. To eliminate another possible quibble, note that this is all supposed to be entirely *a priori*, so of course the mere empirical detail that uniformity actually began fifteen billion years ago (or whatever) is irrelevant here.

[83] Mackie (1979) p. 125, as quoted above in §11.3.

fashion that *a priori* we have no knowledge of the extent of uniformity, and that therefore we might as well ascribe an equal value to $U(1000)$ as to $U(2000)$, $U(3000)$, and so on. For this would not be sanctioned by considerations of translation invariance, given that $U(x) = U(x+1000)$ cannot in general be warranted by transformations of the problem. Indeed that equation can be refuted, because the function $U(x)$ is undefined or zero for negative x – *there cannot be a negative extent of uniformity* – so translation invariance would likewise make $U(x)$ vanish everywhere.

Scale invariance, however, is altogether more promising, and here we can run through a thought-experiment exactly parallel to our earlier one (and similar in spirit to an example given in Jaynes 1968, p. 126). In this case, however, my *alter ego* must differ from me not in virtue of being *translated* in time, but instead in virtue of using a different *scale* of time – different units of measurement. Suppose, then, that he measures time in deciyears ($^1/_{10}$ year), and like me is trying to work out, *a priori*, his own probability distribution function $U'(x)$ for how long uniformity can be expected to last. We ask, as before: *Would he reach the same answer?* And again the reply is "yes", because nothing in our purely *a priori* knowledge of the universe gives us any ground whatever for expecting one unit of temporal measurement to be favoured over another. Hence, as before, my *alter ego*'s function $U'(x)$ should be exactly the same as my $U(x)$. However now consider the probability that our functions ascribe for uniformity's lasting *t years* – but here, because our scales differ and do not cancel out, we must be pedantic and say "*between t years and t years plus* δt". Just as my year is ten of his deciyears, however, so my δt is ten of his $\delta t'$, and this too must be taken into account. For the probability in question, therefore, whereas my function delivers the result $U(t)\delta t$, his delivers the result $10U'(10t)\delta t'$ (δt and $\delta t'$ are of course made sufficiently small that variations in $U(x)$ and $U'(x)$ over the range are negligible). Again, however, if our distributions are genuinely *a priori* and objective they should give the same answer to the same question. So just as in our previous reasoning on Mackie's behalf, we derive two equations connecting our target functions: namely

$$U(x) = U'(x)$$

and $$U(t)\delta t = 10U'(10t)\delta t'$$

By substituting and cancelling we can immediately conclude that $U(x) = 10U(10x)$, which is we find sufficient to specify at last the form of our long sought-after function: $U(x) = a/x$, where a is some constant. However any value of a is here as good as any other, since it cancels out in any Bayesian calculation. Hence it is simplest to take:

$$U(x) = \frac{1}{x}$$

as our probability density function for the *a priori* expected extent of temporal uniformity. This yields, for the probability of x's falling within the interval (a, b):

$$\int_a^b \frac{1}{x}\, dx = \ln(b) - \ln(a)$$

and hence it is commonly called the *log uniform* distribution (it is equivalent to a uniform distribution from $-\infty$ to $+\infty$ for the logarithm of x). Since $\ln(x)$ grows linearly as x grows exponentially, this does indeed seem to be the natural and correct result if scale invariance is to be taken seriously.

13.6 The Log Uniform Distribution

It is not only in the analysis of induction that the log uniform distribution can perform impressively, for it also seems able to dissolve many of the traditional paradoxes of geometrical probability. To illustrate, consider the example mentioned earlier of a cubic die known to have an edge between 1 cm and 3 cm (hence a side area correspondingly between 1 cm² and 9 cm² and a volume between 1 cm³ and 27 cm³), and imagine that we are asked the question: "On the basis of these data only, what is the probability that the die's edge is greater than 2 cm?". Since an edge of 2 cm corresponds to a side area of 4 cm² and a volume of 8 cm³, if we follow the traditional Principle of Indifference we shall not know whether to assign our uniform distribution to the edge ratios {1:2:3}, the side area ratios {1:4:9}, or the volume ratios {1:8:27}, yet clearly at most one of these can be assigned a uniform function. However given that ln(*x*) grows linearly as *x* grows exponentially, the three dimensions can easily be reconciled by assigning probabilities on the basis of the log uniform distribution – since ln(1) = 0 this gives a probability for the edge/side/volume being greater than 2/4/8 of:

$$\frac{\ln(3)-\ln(2)}{\ln(3)} \qquad \frac{\ln(9)-\ln(4)}{\ln(9)} \qquad \frac{\ln(27)-\ln(8)}{\ln(27)}$$

all of which work out to exactly the same (i.e. around 0·369). So the paradox completely disappears.

The log uniform distribution therefore seems to be a powerful tool for representing ignorance about a non-negative real parameter, and as such it might even raise hopes of a more general revival of the Principle of Indifference than would be sanctioned by Jaynes' "problem transformation" approach. But optimism here should be restrained, because for reasons obvious to those with memories of using logarithm tables for school arithmetic, the log uniform function can perform its magic only when dealing with relationships involving multiplication, division, and exponentiation. As soon as addition and subtraction enter the picture, as they do for example with the Von Mises' water and wine paradox (where the combined volume sums to a constant), the distribution loses its power, and we return to the situation of having several possible non-linearly related dimensions of ignorance, with none of them having any obvious claim to logical or epistemological privilege.[84]

Its failure to be a universal panacea does not, of course, impugn the credentials of the log uniform distribution as a suitable representation of prior ignorance concerning the value of a non-negative parameter along a single dimension. But to many the distribution might seem to have a devastating drawback when applied, as we have done with our function $U(x)$, to a parameter (in our case the extent of uniformity) which ranges over the *entire* non-negative half-line from 0 to $+\infty$, namely, that it lacks a finite integral. Indeed not only does $\ln(x)$ tend to $+\infty$ as x itself tends to $+\infty$, but also $\ln(x)$ tends to $-\infty$ as x tends to 0. So as a distribution function it is doubly divergent.

Fortunately we have already seen in Chapter 9 that the theory of probability can be formulated apparently perfectly well in a manner that makes room for such "improper"

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[84] For argument along these lines see Milne (1983) pp. 54-5 and Van Fraasen (1989) pp. 313-4.

distributions. And it happens that Harold Jeffreys, who devised that formulation, was famously enthusiastic about the log uniform distribution. So far from seeing its double divergence as a liability, he even argues forcefully that it is a considerable virtue:

> If we have to express previous ignorance of the value of a quantity over an infinite range, we have seen that ... we shall have to represent certainty by infinity instead of 1; thus the fact that $\int_0^\infty dv / v$ diverges at both limits is a satisfactory feature.
>
> (Jeffreys 1961, p. 120)

It turns out that here Jeffreys means a good deal more than just "satisfactory", because he goes on to make the point that if the function diverged at only the upper limit, then this would imply with virtual certainty that the supposedly "unknown" quantity for which it provides the distribution function must be greater than any given finite value. Likewise if it diverged only at the lower limit, then this would virtually imply that the unknown quantity must be less than any given positive value. It is only if the function diverges at both ends, therefore, that it can truly be used to represent complete ignorance on the entire non-negative half-line.

Jeffreys' arguments are powerful and interesting, but to follow them further would take us too far from our topic. But to sum up, I believe that together with my Jaynesian derivation in Section §13.5, they show as conclusively as one could reasonably expect that the log uniform distribution, as a representation of our *a priori* ignorance concerning the extent of temporal uniformity, is as good as any other distribution function that we are ever likely to find. If *it* cannot do the job properly, then I do not believe that any other conventional distribution function can do it either.

13.7 An Inductively Favourable Conclusion?

Having derived our "objective" prior, we should obviously ask whether it satisfies the criterion expounded in §11.2 – is it, in short, "inductively favourable"? To this crucial question there are three possible answers, and for reasons to be explained in the concluding chapter I am not sure which to prefer.

The first and most obvious answer is that the log uniform prior is inductively favourable to a supreme degree. Once uniformity has succeeded in persisting for any finite time at all, and "cleared" the divergence to infinity that takes place at the origin, then it immediately becomes overwhelmingly likely to last longer than any given finite period. The log uniform distribution on the interval from 1 (or any other finite value) to $+\infty$, is so to speak "infinitely weighted" towards the upper end. I find this answer, on behalf of the inductive probabilist, embarrassingly strong.

A second and perhaps more optimistic answer, therefore, is that the log uniform prior, because it is scale invariant, ought to be inductively favourable in the same way as the informal and intuitively attractive arguments that we considered in §12.6 when reflecting on Harrod's unfortunate defence. Although the infinite weight referred to in the previous answer is certainly a significant technical obstacle, it seems not totally unreasonable to hope that there might be some form of "normalising" analysis which would justify the natural idea that because the log uniform prior is scale invariant, the predictive confirmation that it bestows should also be a meaningfully scale invariant function rather than one that immediately shoots off to infinity. It is indeed easy to formulate such "modest" scale-invariant confirmation functions – the difficulty is to justify ignoring the absolute confirmation that is always there for the asking by pushing things all the way to the limit.[85]

The third, and pessimistic, answer is that the problems alluded to above are intrinsic to the use of an "improper" prior for inference of this radically *a priori* type. When infinities appear in the equations, it might plausibly be said, all hope of coherence disappears.[86] If this is indeed the correct answer to our question, then the force of the reasoning explained in this chapter, which I believe to be considerable, would tell not in *favour* of the inductive probabilist but *against* him. The argument in §13.5, in particular, seems to me to be incontrovertible, in so far as it succeeds in showing that *if* it is appropriate to expect complete ignorance about a "scale" parameter to be representable by a probability distribution function at all, *then* the log uniform distribution is the only possible choice. If this choice is in fact ultimately incoherent, then the inductivist *modus ponens* has become a sceptical *modus tollens*.

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[85] An obvious possibility here, taking a hint from Harrod's strategy, is to focus on a conditional probability, for example the probability that uniformity will continue for at least as long as it has done already, *on the "modest" assumption that it will last altogether at most ten times that long*. This indeed gives a scale-invariant result (namely ln(5)/ln(10) or roughly 0·7, independently of the length of the uniformity), but it seems paradoxical to fix an arbitrary limit at a tenfold extension of uniformity, when greater extension will always yield a stronger result.

[86] As no doubt would indeed be said by most statisticians, who will typically take the attitude advocated in the text Box and Tiao (1973, p. 21): "improper ... density functions ... are frequently employed to represent the *local* behavior of the prior distribution in the region where the likelihood is appreciable, but *not* over its entire range. By supposing that to a sufficient approximate the prior ... suitably tails to zero outside [that region] we ensure that the priors actually used are proper."

PART III

CONCLUSION

Chapter 14 Induction Defended or Hume Victorious?

Modus Ponens, or *Modus Tollens*?

I shall here try to come at least partly off the fence onto which I climbed at the end of the last chapter, and to answer the crucial question: Does the derivation in Section 13.5 of the log uniform distribution, as the "objective" prior for which we had been seeking since Chapter 11, constitute the basis of a probabilistic defence of induction?

My somewhat double-edged answer is as follows:

- *If* it is in principle possible to represent our *utter ignorance* of a parameter on the entire non-negative half-line by a conventional distribution function; and
- *if* it is then legitimate to use that distribution function to draw Bayesian inferences about that parameter;
- *then* the derivation in §13.5 of the log uniform function as a prior probability distribution for the extent of total uniformity can (probably) provide such a basis for making inductively favourable inferences.

This statement is deliberately very guarded, and I confess to being less than completely sure whether it is more appropriate to regard the last chapter as a *modus ponens* in favour of induction or as a *modus tollens* against one of the antecedents of the above conditional. However I shall very briefly recite my reasons for being significantly more inclined in the latter direction.

First, I continue to find it extremely implausible that substantial epistemological conclusions such as this should be drawn by pure reason.

Secondly, despite Jeffreys' advocacy of the log uniform distribution, and despite my conviction that it is almost certainly the inductive probabilist's best (and maybe only) hope, I continue to feel very uneasy about the use of improper prior distributions except as a convenient approximation to proper priors. Without considerably more investigation and understanding of the implications of their use, I cannot express any confidence that the results they yield are fully meaningful and coherent.[87]

Thirdly, I believe that there are strong theoretical grounds, which I have not had space to explore here, for refusing to accept that knowledge and ignorance can adequately be modelled by conventional probability distribution functions. In most cases, perhaps, they perform reasonably well, but when extreme situations of absolute

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[87] Relevant discussions of non-informative improper priors are provided by, for example, Berger (1985) pp. 82-90; Rosenkrantz (1977) pp. 62-83; Seidenfeld (1979b); Walley (1991) pp. 226-35 and Zellner (1971) pp. 41-53. The issues are too complex to address here, but I am not inclined to be particularly optimistic on the Logical Bayesian's behalf.

ignorance are in question, the cracks begin to show.[88] Interval-valued probabilities and other richer and more flexible models seem in such cases better motivated, but these provide an unlikely basis for a defence of induction, precisely because they put fewer constraints on coherent degrees of belief, ignorance and indecision.[89]

On the whole, therefore, and though I would be delighted to find some way of proving coherent and persuasive the positive conclusions of my previous chapter, if I had to bet, I would bet on Hume. It seems to me that the results of this thesis have given excellent *inductive* grounds for confidence that he will ultimately emerge victorious against every attempt to refute him. And just as Hume did, I trust induction.

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[88] A personal hunch here is that the development and incorporation into probability theory of Keynes' concept of "weight" (1921, chapter VI) might help to explain in a well-motivated manner why conventional probability models break down in situations of extreme ignorance (somewhat as arithmetical division breaks down when the denominator is zero). As far as I know the only significant recent discussions of Keynesian weight are those of Cohen (1985, and 1989 §14), who sees it as relating closely to the "method of relevant variables" developed in his (1970) and (1977).

[89] Interval-valued (and therefore only partially ordered) probabilities were pioneered by Keynes (1921, chapter XV), Koopman (1940) and Good (e.g. 1950 pp. 40, 82-3; 1962), but developed much further by Kyburg, whose work provides perhaps the most extensive philosophical discussion currently available (e.g. 1974, especially chapter 10). A more recent and monumentally thorough but predominantly technical treatment is provided by Walley (1991), who gives an excellent survey of such work and develops his own theory at great length. Howson (1995) pp. 13-14 is useful for a very brief guide to some recent developments.

APPENDICES

APPENDIX 1: An authoritative text of Section IV of Hume's First *Enquiry*

Though tolerably good, the familiar Selby-Bigge text of the *Enquiry Concerning Human Understanding*, based on the first posthumous edition of 1777, is marred in particular by editorial interference in capitalisation, spelling, and most seriously in punctuation. Nidditch's third edition of 1975 brought some improvement, but still left a text significantly deficient by modern scholarly standards. This appendix contains a corrected version of the Section IV text (annotated with the Selby-Bigge page numbers in square brackets), which has been checked in detail against the copy of the 1777 edition in the Brotherton Library of the University of Leeds, and which indicates in footnotes marked thus: "♦" all modifications from the text of the 1772 edition (the last to appear under Hume's direction), again checked personally against a copy in the Brotherton Library. My own preference is for the 1777 edition, on the grounds that differences between the two can generally be presumed to result from Hume's final editorial directions. However opinions differ as to which of the two texts should be taken as the more authoritative, and the forthcoming Oxford critical edition will be using the 1772 rather than the 1777 as its copytext. The text that follows has been compared against the preliminary electronic release of that copytext, which is indeed absolutely accurate (in respect of Section IV, though not entirely elsewhere) as checked against the Brotherton copy of the 1772 edition.

[25] SECTION IV.

SCEPTICAL DOUBTS concerning the OPERATIONS of the UNDERSTANDING.

PART I.

ALL the objects of human reason or enquiry may naturally be divided into two kinds, to wit, *Relations of Ideas*,♦ and *Matters of Fact*. Of the first kind are the sciences of Geometry, Algebra, and Arithmetic; and in short, every affirmation, which is either intuitively or demonstratively certain. *That the square of the hypothenuse is equal to the square of the two sides*, is a proposition, which expresses a relation between these figures. *That three times five is equal to the half of thirty*, expresses a relation between these numbers. Propositions of this kind are discoverable by the mere operation of thought, without dependence on what is any where existent in the universe. Though there never were a circle or triangle in nature, the truths, demonstrated by EUCLID, would for ever retain their certainty and evidence.

Matters of fact, which are the second objects of human reason, are not ascertained in the same manner; nor is our evidence of their truth, however great, of a like nature with the foregoing. The contrary of every matter of fact is still possible; because it can never imply a contradiction, and is conceived by the mind with the same facility and distinctness, as if ever so conformable to reality. *That the* [26] *sun will not rise to-morrow* is no less intelligible a proposition, and implies no more contradiction, than the affirmation, *that it will rise*. We should in vain, therefore, attempt to demonstrate its

♦ **77** to wit, *Relations of Ideas*, **72** *viz. Relations of Ideas* falsehood. Were it demonstratively false, it would imply a contradiction, and could never be distinctly conceived by the mind.

It may, therefore, be a subject worthy of curiosity, to enquire what is the nature of that evidence, which assures us of any real existence and matter of fact, beyond the present testimony of our senses, or the records of our memory. This part of philosophy, it is observable, has been little cultivated, either by the ancients or moderns; and therefore our doubts and errors, in the prosecution of so important an enquiry, may be the more excusable; while we march through such difficult paths, without any guide or direction. They may even prove useful, by exciting curiosity, and destroying that implicit faith and security, which is the bane of all reasoning and free enquiry. The discovery of defects in the common philosophy, if any such there be, will not, I presume, be a discouragement, but rather an incitement, as is usual, to attempt something more full and satisfactory, than has yet been proposed to the public.

All reasonings concerning matter of fact seem to be founded on the relation of *Cause and Effect*.♦ By means of that relation alone we can go beyond the evidence of our memory and senses. If you were to ask a man, why he believes any matter of fact, which is absent; for instance, that his friend is in the country, or in FRANCE; he would give you a reason; and this reason would be some other fact; as a letter received from him, or the knowledge of his former resolutions and promises. A man, finding a watch or any other machine in a desart island, would conclude, that there had once been men in that island. All our reasonings concerning fact are of the same nature. And [27] here it is constantly supposed, that there is a connexion between the present fact and that which is inferred from it. Were there nothing to bind them together, the inference would be entirely precarious. The hearing of an articulate voice and rational discourse in the dark assures us of the presence of some person: Why? because these are the effects of the human make and fabric, and closely connected with it. If we anatomize all the other reasonings of this nature, we shall find, that they are founded on the relation of cause and effect, and that this relation is either near or remote, direct or

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♦ **77** *Cause and Effect* **72** *Cause* and *Effect* collateral. Heat and light are collateral effects of fire, and the one effect may justly be inferred from the other.

If we would satisfy ourselves, therefore, concerning the nature of that evidence, which assures us of matters of fact, we must enquire how we arrive at the knowledge of cause and effect.

I shall venture to affirm, as a general proposition, which admits of no exception, that the knowledge of this relation is not, in any instance, attained by reasonings *à priori*; but arises entirely from experience, when we find, that any particular objects are constantly conjoined with each other. Let an object be presented to a man of ever so strong natural reason and abilities; if that object be entirely new to him, he will not be able, by the most accurate examination of its sensible qualities, to discover any of its causes or effects. ADAM, though his rational faculties be supposed, at the very first, entirely perfect, could not have inferred from the fluidity,♦ and transparency of water, that it would suffocate him, or from the light and warmth of fire, that it would consume him. No object ever discovers, by the qualities which appear to the senses, either the causes which produced it, or the effects♦ which will arise from it; nor can our reason, unassisted by experience, ever draw any inference concerning real existence and matter of fact.

[28] This proposition, *that causes and effects are discoverable, not by reason, but by experience,* will readily be admitted with regard to such objects, as we remember to have once been altogether unknown to us; since we must be conscious of the utter inability, which we then lay under, of foretelling, what would arise from them. Present two smooth pieces of marble to a man, who has no tincture of natural philosophy; he will never discover, that they will adhere together, in such a manner as to require great force to separate them in a direct line, while they make so small a resistance to a lateral pressure. Such events, as bear little analogy to the common course of nature, are also

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♦ **77** fluidity, **72** fluidity

♦ **77** causes which produced it, or the effects **72** causes, which produced it, or the effects, readily confessed to be known only by experience; nor does any man imagine that the explosion of gunpowder, or the attraction of a loadstone, could ever be discovered by arguments *à priori*. In like manner, when an effect is supposed to depend upon an intricate machinery or secret structure of parts, we make no difficulty in attributing all our knowledge of it to experience. Who will assert, that he can give the ultimate reason, why milk or bread is proper nourishment for a man, not for a lion or a tyger?

But the same truth may not appear, at first sight, to have the same evidence with regard to events, which have become familiar to us from our first appearance in the world, which bear a close analogy to the whole course of nature, and which are supposed to depend on the simple qualities of objects, without any secret structure of parts. We are apt to imagine, that we could discover these effects by the mere operation of our reason, without experience. We fancy, that were we brought, on a sudden, into this world, we could at first have inferred, that one Billiard-ball would communicate motion to another upon impulse; and that we needed not to have waited for the event, in order to pronounce with certainty concerning it. Such is the influence of custom, that, where it is strongest, it not only covers our natural ignorance, but [29] even conceals itself, and seems not to take place, merely because it is found in the highest degree.

But to convince us, that all the laws of nature, and all the operations of bodies without exception, are known only by experience, the following reflections may, perhaps, suffice. Were any object presented to us, and were we required to pronounce concerning the effect, which will result from it, without consulting past observation; after what manner, I beseech you, must the mind proceed in this operation? It must invent or imagine some event, which it ascribes to the object as its effect; and it is plain that this invention must be entirely arbitrary. The mind can never possibly find the effect in the supposed cause, by the most accurate scrutiny and examination. For the effect is totally different from the cause, and consequently can never be discovered in it. Motion in the second Billiard-ball is a quite distinct event from motion in the first; nor is there any thing in the one to suggest the smallest hint of the other. A stone or piece of metal raised into the air, and left without any support, immediately falls: But to consider the matter *à priori*, is there any thing we discover in this situation, which can beget the idea of a downward, rather than an upward, or any other motion, in the stone or metal?

And as the first imagination or invention of a particular effect, in all natural operations, is arbitrary, where we consult not experience; so must we also esteem the supposed tye or connexion between the cause and effect, which binds them together, and renders it impossible, that any other effect could result from the operation of that cause. When I see, for instance, a Billiard-ball moving in a straight line towards another; even suppose motion in the second ball should by accident be suggested to me, as the result of their contact or impulse; may I not conceive, that a hundred different events might as well follow from that cause? May not both these balls remain at absolute rest? May not the [30] first ball return in a straight line, or leap off from the second in any line or direction? All these suppositions are consistent and conceivable. Why then should we give the preference to one, which is no more consistent or conceivable than the rest? All our reasonings *à priori* will never be able to shew us any foundation for this preference.

In a word, then, every effect is a distinct event from its cause. It could not, therefore, be discovered in the cause, and the first invention or conception of it, *à priori*, must be entirely arbitrary. And even after it is suggested, the conjunction of it with the cause must appear equally arbitrary; since there are always many other effects, which, to reason, must seem fully as consistent and natural. In vain, therefore, should we pretend to determine any single event, or infer any cause or effect, without the assistance of observation and experience.

Hence we may discover the reason, why no philosopher, who is rational and modest, has ever pretended to assign the ultimate cause of any natural operation, or to show distinctly the action of that power, which produces any single effect in the universe. It is confessed, that the utmost effort of human reason is, to reduce the principles, productive of natural phaenomena, to a greater simplicity, and to resolve the many particular effects into a few general causes, by means of reasonings from analogy, experience, and observation. But as to the causes of these general causes, we should in vain attempt their discovery; nor shall we ever be able to satisfy ourselves, by any particular explication of them. These ultimate springs and principles are totally shut up from human curiosity and enquiry. Elasticity, gravity, cohesion of parts, communication of motion by impulse; these are probably the ultimate causes and principles which we shall ever discover in nature; and we may esteem ourselves sufficiently happy, if, by accurate enquiry and reasoning, we can trace up the particular phaenomena [31] to, or near to, these general principles. The most perfect philosophy of the natural kind only staves off our ignorance a little longer: As perhaps the most perfect philosophy of the moral or metaphysical kind serves only to discover larger portions of it.♦ Thus the observation of human blindness and weakness is the result of all philosophy, and meets us, at every turn, in spite of our endeavours to elude or avoid it.

Nor is geometry, when taken into the assistance of natural philosophy, ever able to remedy this defect, or lead us into the knowledge of ultimate causes, by all that accuracy of reasoning, for which it is so justly celebrated. Every part of mixed mathematics proceeds upon the supposition, that certain laws are established by nature in her operations; and abstract reasonings are employed, either to assist experience in the discovery of these laws, or to determine their influence in particular instances, where it depends upon any precise degree of distance and quantity. Thus, it is a law of motion, discovered by experience, that the moment or force of any body in motion is in the compound ratio or proportion of its solid contents and its velocity; and consequently, that a small force may remove the greatest obstacle or raise the greatest weight, if, by any contrivance or machinery, we can encrease the velocity of that force, so as to make it an overmatch for its antagonist. Geometry assists us in the application of this law, by giving us the just dimensions of all the parts and figures, which can enter into any species of machine; but still the discovery of the law itself is owing merely to experience, and all the abstract reasonings in the world could never lead us one step towards the knowledge of it. When we reason *à priori*, and consider merely any object or cause, as it appears to the mind, independent of all observation, it never

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♦ **77** it. **72** our ignorance.

could suggest to us the notion of any distinct object, such as its effect; much less, shew us the inseparable and inviolable connexion between them. A man must be very sagacious, who could discover by [32] reasoning, that crystal is the effect of heat, and ice of cold, without being previously acquainted with the operation of these qualities.

PART II.

But we have not, yet,♦ attained any tolerable satisfaction with regard to the question first proposed. Each solution still gives rise to a new question as difficult as the foregoing, and leads us on to farther enquiries. When it is asked, *What is the nature of all our reasonings concerning matter of fact?* the proper answer seems to be, that they are founded on the relation of cause and effect. When again it is asked, *What is the foundation of all our reasonings and conclusions concerning that relation?* it may be replied in one word, EXPERIENCE. But if we still carry on our sifting humour, and ask, *What is the foundation of all conclusions from experience?* this implies a new question, which may be of more difficult solution and explication. Philosophers, that give themselves airs of superior wisdom and sufficiency, have a hard task, when they encounter persons of inquisitive dispositions, who push them from every corner, to which they retreat, and who are sure at last to bring them to some dangerous dilemma. The best expedient to prevent this confusion, is to be modest in our pretensions; and even to discover the difficulty ourselves before it is objected to us. By this means, we may make a kind of merit of our very ignorance.

I shall content myself, in this section, with an easy task, and shall pretend only to give a negative answer to the question here proposed. I say then, that, even after we have experience of the operations of cause and effect, our conclusions from that

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♦ **77** yet, **72** as yet,

experience are *not* founded on reasoning, or any process of the understanding. This answer we must endeavour, both to explain and to defend.

It must certainly be allowed, that nature has kept us at a great distance from all her secrets, and has afforded [33] us only the knowledge of a few superficial qualities of objects; while she conceals from us those powers and principles, on which the influence of these objects entirely depends. Our senses inform us of the colour, weight, and consistence of bread; but neither sense nor reason can ever inform us of those qualities, which fit it for the nourishment and support of a human body. Sight or feeling conveys an idea of the actual motion of bodies; but as to that wonderful force or power, which would carry on a moving body for ever in a continued change of place, and which bodies never lose but by communicating it to others; of this we cannot form the most distant conception. But notwithstanding this ignorance of natural powers * and principles, we always presume, when♦ we see like sensible qualities, that they have like secret powers, and expect, that effects, similar to those♦ which we have experienced, will follow from them. If a body of like colour and consistence with that bread,♦ which we have formerly eat, be presented to us, we make no scruple of repeating the experiment, and foresee, with certainty, like nourishment and support. Now this is a process of the mind or thought, of which I would willingly know the foundation. It is allowed on all hands, that there is no known connexion between the sensible qualities and the secret powers; and consequently, that the mind is not led to form such a conclusion concerning their constant and regular conjunction, by any thing which it knows of their nature. As to past *Experience*, it can be allowed to give *direct*

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* The word, Power, is here used in a loose and popular sense. The more accurate explication of it would give additional evidence to this argument. See Sect. 7.

♦ **77** when **72** where

♦ **77** those **72** those,

♦ **77** that bread, **72** that of bread, and *certain* information of those precise objects only, and that precise period of time, which fell under its cognizance: But why this experience should be extended to future times, and to other objects, which♦ for aught we know, may be only in [34] appearance similar; this is the main question on which I would insist. The bread, which I formerly eat, nourished me; that is, a body of such sensible qualities, was, at that time, endued with such secret powers: But does it follow, that other bread must also nourish me at another time, and that like sensible qualities must always be attended with like secret powers? The consequence seems nowise necessary. At least, it must be acknowledged, that there is here a consequence drawn by the mind; that there is a certain step taken; a process of thought, and an inference, which wants to be explained. These two propositions are far from being the same, *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 allow, if you please, that the one proposition may justly be inferred from the other: I know in fact, that it always is inferred. But if you insist, that the inference is made by a chain of reasoning, I desire you to produce that reasoning. 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. What that medium is, I must confess, passes my comprehension; and it is incumbent on those to produce it, who assert, that it really exists, and is the origin of all our conclusions concerning matter of fact.

This negative argument must certainly, in process of time, become altogether convincing, if many penetrating and able philosophers shall turn their enquiries this way; and no one be ever able to discover any connecting proposition or intermediate step, which supports the understanding in this conclusion. But as the question is yet new, every reader may not trust so far to his own penetration, as to conclude, because an argument escapes his enquiry, that therefore it does not really exist. For this [35] reason it may be requisite to venture upon a more difficult task; and enumerating all the

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♦ **77** which **72** which,

branches of human knowledge, endeavour to shew, that none of them can afford such an argument.

All reasonings may be divided into two kinds, namely♦ demonstrative reasoning, or that concerning relations of ideas, and moral reasoning, or that concerning matter of fact and existence. That there are no demonstrative arguments in the case, seems evident; since it implies no contradiction, that the course of nature may change, and that an object, seemingly like those which we have experienced, may be attended with different or contrary effects. May I not clearly and distinctly conceive, that a body, falling from the clouds, and which, in all other respects, resembles snow, has yet the taste of salt or feeling of fire? Is there any more intelligible proposition than to affirm, that all the trees will flourish in DECEMBER and JANUARY, and decay in MAY and JUNE? Now whatever is intelligible, and can be distinctly conceived, implies no contradiction, and can never be proved false by any demonstrative argument or abstract reasoning *à priori*.

If we be, therefore, engaged by arguments to put trust in past experience, and make it the standard of our future judgment, these arguments must be probable only, or such as regard matter of fact and real existence, according to the division above mentioned. But that there is no argument of this kind, must appear, if our explication of that species of reasoning be admitted as solid and satisfactory. We have said, that all arguments concerning existence are founded on the relation of cause and effect; that our knowledge of that relation is derived entirely from experience; and that all our experimental conclusions proceed upon the supposition, that the future will be conformable to the past. To endeavour, therefore, the proof of this last supposition by probable arguments, or arguments regarding [36] existence, must be evidently going in a circle, and taking that for granted, which is the very point in question.

♦ **77** namely **72** *viz.*

In reality, all arguments from experience are founded on the similarity, which we discover among natural objects, and by which we are induced to expect effects similar to those, which we have found to follow from such objects. And though none but a fool or madman will ever pretend to dispute the authority of experience, or to reject that great guide of human life; it may surely be allowed a philosopher to have so much curiosity at least, as to examine the principle of human nature, which gives this mighty authority to experience, and makes us draw advantage from that similarity, which nature has placed among different objects. From causes, which appear *similar*, we expect similar effects. This is the sum of all our experimental conclusions. Now it seems evident, that, if this conclusion were formed by reason, it would be as perfect at first, and upon one instance, as after ever so long a course of experience. But the case is far otherwise. 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. Now where is that process of reasoning, which, from one instance, draws a conclusion, so different from that which it infers from a hundred♦ instances, that are nowise different from that single one? This question I propose as much for the sake of information, as with an intention of raising difficulties. I cannot find, I cannot imagine any such reasoning. But I keep my mind still open to instruction, if any one will vouchsafe to bestow it on me.

Should it be said, that, from a number of uniform experiments, we *infer* a connexion between the sensible qualities and the secret powers; this, I must confess, seems the [37] same difficulty, couched in different terms. The question still recurs, on♦ what process of argument this *inference* is founded? Where is the medium, the interposing ideas, which join propositions so very wide of each other? It is confessed,

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♦ **77** a hundred **72** an hundred

♦ **77** on **72** On

that the colour, consistence,♦ and other sensible qualities of bread appear not, of themselves, to have any connexion with the secret powers of nourishment and support. For otherwise we could infer these secret powers from the first appearance of these sensible qualities, without the aid of experience; contrary to the sentiment of all philosophers, and contrary to plain matter of fact. Here then is our natural state of ignorance with regard to the powers and influence of all objects. How is this remedied by experience? It only shews us a number of uniform effects, resulting from certain objects, and teaches us, that those particular objects, at that particular time, were endowed with such powers and forces. When a new object, endowed with similar sensible qualities, is produced, we expect similar powers and forces, and look for a like effect. From a body of like colour and consistence with bread, we expect like nourishment and support. But this surely is a step or progress of the mind, which wants to be explained. When a man says, *I have found, in all past instances, such sensible qualities conjoined with such secret powers:* And when he says, *similar sensible qualities will always be conjoined with similar secret powers*; he is not guilty of a tautology, nor are these propositions in any respect the same. You say that the one proposition is an inference from the other. But you must 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. If there be any suspicion, that the course of nature may [38] change, and that the past may be no rule for the future, all experience becomes useless, and can give rise to no inference or conclusion. 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. Let the course of things be allowed hitherto ever so regular; that alone, without some new argument or inference, proves not, that, for the future, it will

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♦ **77** consistence, **72** consistence

♦ **77** confess **72** confess,

continue so. In vain do you pretend to have learned the nature of bodies from your past experience. Their secret nature, and consequently, all their effects and influence, may change, without any change in their sensible qualities. This happens sometimes, and with regard to some objects: Why may it not happen always, and with regard to all objects? What logic, what process of argument secures you against this supposition? My practice, you say, refutes my doubts. But you mistake the purport of my question. As an agent, I am quite satisfied in the point; but as a philosopher, who has some share of curiosity, I will not say scepticism, 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, if we do not augment our knowledge.

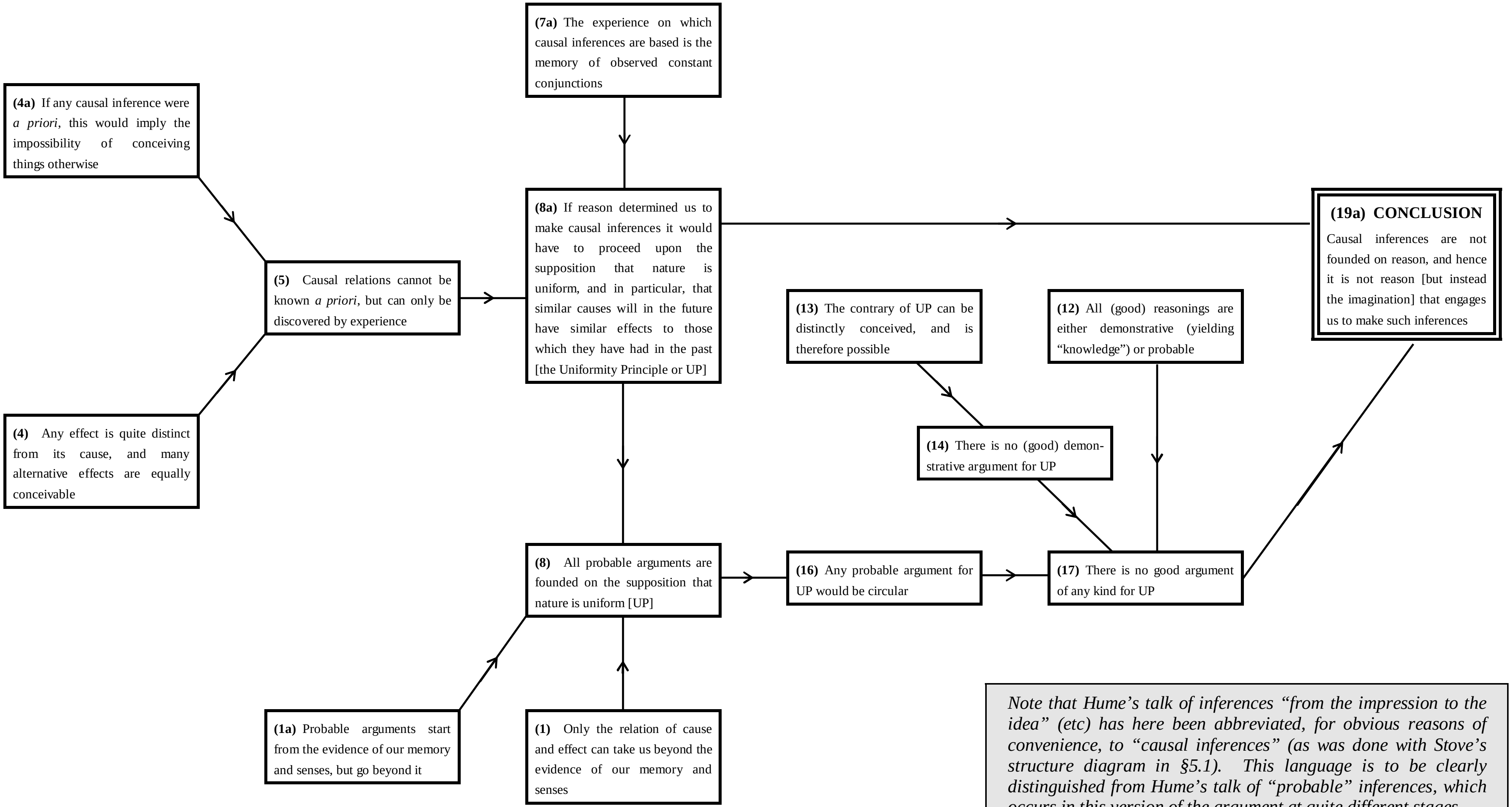
I must confess, that a man is guilty of unpardonable arrogance, who concludes, because an argument has escaped his own investigation, that therefore it does not really exist. I must also confess, that, though all the learned, for several ages, should have employed themselves in fruitless search upon any subject, it may still, perhaps, be rash to conclude positively, that the subject must, therefore, pass all human comprehension. Even though we examine [39] all the sources of our knowledge, and conclude them unfit for such a subject, there may still remain a suspicion, that the enumeration is not compleat, or the examination not accurate. But with regard to the present subject, there are some considerations, which seem to remove all this accusation of arrogance or suspicion of mistake.

It is certain, that the most ignorant and stupid peasants, nay infants, nay even brute beasts, improve by experience, and learn the qualities of natural objects, by observing the effects, which result from them. When a child has felt the sensation of pain from touching the flame of a candle, he will be careful not to put his hand near any candle; but will expect a similar effect from a cause, which is similar in its sensible qualities and appearance. If you assert, therefore, that the understanding of the child is led into this conclusion by any process of argument or ratiocination, I may justly require you to produce that argument; nor have you any pretence to refuse so equitable a demand.

You cannot say, that the argument is abstruse, and may possibly escape your enquiry; since you confess, that it is obvious to the capacity of a mere infant. If you hesitate, therefore, a moment, or if, after reflection, you produce any intricate or profound argument, you, in a manner, give up the question, and confess, that it is not reasoning which engages us to suppose the past resembling the future, and to expect similar effects from causes, which are, to appearance, similar. This is the proposition which I intended to enforce in the present section. If I be right, I pretend not to have made any mighty discovery. And if I be wrong, I must acknowledge myself to be indeed a very backward scholar; since I cannot now discover an argument, which, it seems, was perfectly familiar to me, long before I was out of my cradle.

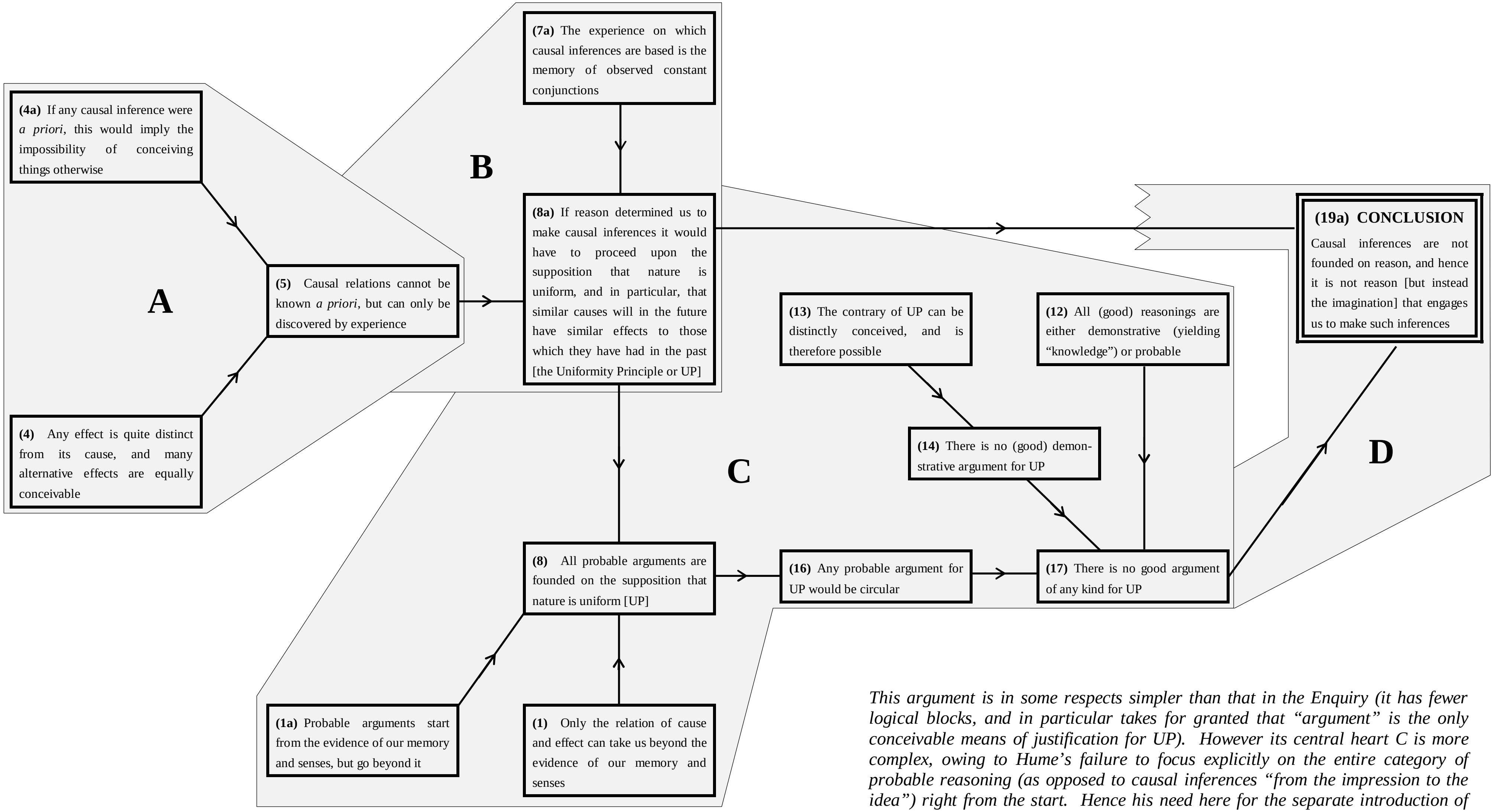
APPENDIX 2:
Structure diagrams of the *Treatise* and *Enquiry* arguments, with Hume's statement of their stages

The first part of this thesis was largely organised around the task of constructing a reliable structure diagram for Hume's argument concerning induction as it is presented in Section IV of the *Enquiry Concerning Human Understanding*. This appendix repeats that structure diagram, together with its analysis into "logical blocks" and the table of quotations from the *Enquiry* which correspond to each proposition that the diagram contains. First, however, are provided for comparison a similar pair of structure diagrams representing the argument as it appears in Section I iii 6 of the *Treatise of Human Nature*, again accompanied by a table containing the relevant quotations from Hume's own words. Since the purpose of this thesis has been to investigate the structure and philosophical significance of Hume's *mature* thought, it seemed inappropriate to devote within it the space that would have been necessary for a precise paragraph-by-paragraph account of the *Treatise* argument as well as that of the *Enquiry*. However the material in this appendix should enable the interested reader to work out in some detail how my interpretation of the *Treatise* argument would differ from that of the *Enquiry*, and to see in particular how the claims made in §1.2 regarding the inferiority of the *Treatise* presentation can be substantiated.



Note that Hume's talk of inferences "from the impression to the idea" (etc) has here been abbreviated, for obvious reasons of convenience, to "causal inferences" (as was done with Stove's structure diagram in §5.1). This language is to be clearly distinguished from Hume's talk of "probable" inferences, which occurs in this version of the argument at quite different stages.

Hume's Argument Concerning Induction (from the *Treatise of Human Nature*)



This argument is in some respects simpler than that in the Enquiry (it has fewer logical blocks, and in particular takes for granted that "argument" is the only conceivable means of justification for UP). However its central heart C is more complex, owing to Hume's failure to focus explicitly on the entire category of probable reasoning (as opposed to causal inferences "from the impression to the idea") right from the start. Hence his need here for the separate introduction of (1)/(1a) to establish (8) even after the first two stages have been completed.

The Main Logical Blocks of the *Treatise* Argument

The proposition numbers have been chosen so as to highlight as far as possible the logical parallels with the Enquiry version of the argument as analysed in the main body of the thesis

(1a) T89: "in all probable reasonings there [is] something present to the mind, and ... from this we infer something connected with it, which is not seen nor remember'd."

(1) T89: "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"

(4a) T87: "Such an inference wou'd amount to knowledge, and wou'd imply the absolute contradiction and impossibility of conceiving anything different."

(4) T87: "But as all distinct ideas are separable, 'tis evident there can be no impossibility of that kind. When we pass from a present impression to the idea of any object, we might possibly have separated the idea from the impression, and have substituted any other idea in its room."

(5) T86: "the inference we draw from cause to effect, is not deriv'd merely from a survey of these particular objects, and from such a penetration into their essences as may discover the dependence of the one upon the other."

T87: "'Tis therefore by EXPERIENCE only, that we can infer the existence of one object from that of another."

(7a) T87: "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. ... Without any farther ceremony, we call the one *cause* and the other *effect*, and infer the existence of the one from that of the other. ... But in all cases, wherein we reason concerning them, there is only one perceiv'd or remember'd, and the other is supply'd in conformity to our past experience."

T89-90 "The idea of cause and effect is deriv'd 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 suppos'd to be immediately present in its impression, we thence presume on the existence of one similar to its usual attendant."

(8a) T89: If reason determin'd us [to infer an idea from the impression of its usual attendant], it wou'd proceed upon that 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.*

(8) T90: "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"

(12) T89: "In order therefore to clear up this matter, let us consider all the arguments, upon which such a proposition 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."

(13) T89: "We can at least conceive a change in the course of nature; which sufficiently proves, that such a change is not absolutely impossible."

(14) T89: "there can be no *demonstrative* arguments to prove, *that those instances, of which we have had no experience, resemble those, of which we have had experience*."

(16) T90: "and therefore 'tis impossible this presumption can arise from probability. The same principle cannot be both the cause and effect of another"

(17) T90: "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"

T91-2 "We suppose, but are never able to prove, that there must be a resemblance between those objects, of which we have had experience, and those which lie beyond the reach of our discovery."

(19a) T91: "Thus not only our reason fails us in the discovery of the *ultimate connexion* of causes and effects, but 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."

T92: "When the mind, therefore, passes from the idea or impression of one object to the idea or belief of another, it is not determin'd by reason, but by certain principles, which associate together the ideas of these objects, and unite them in the imagination."

Hume's Statement of the Stages of the *Treatise* Argument

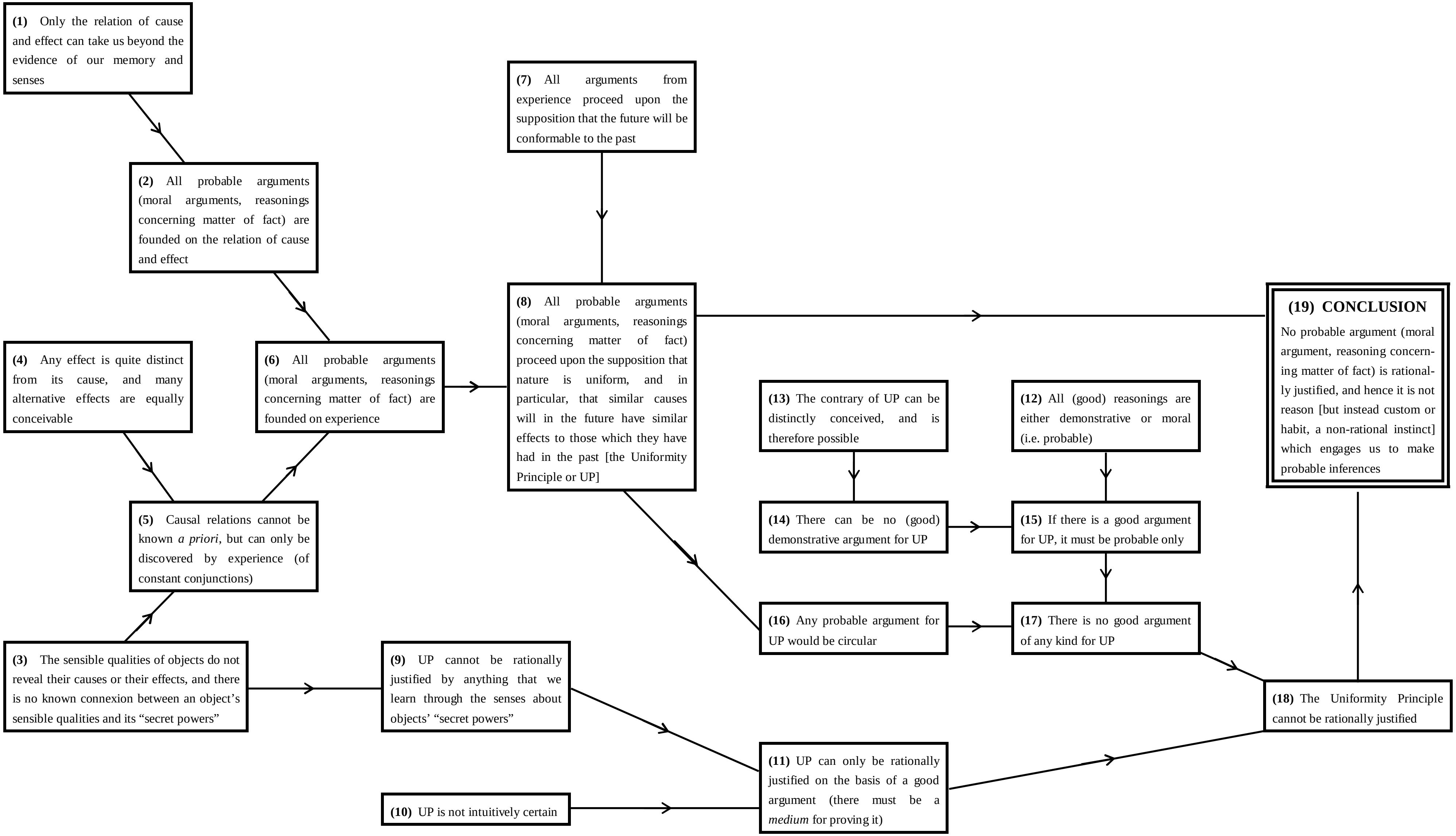


Figure 1 **Hume's Argument Concerning Induction (from the *Enquiry Concerning Human Understanding*)**

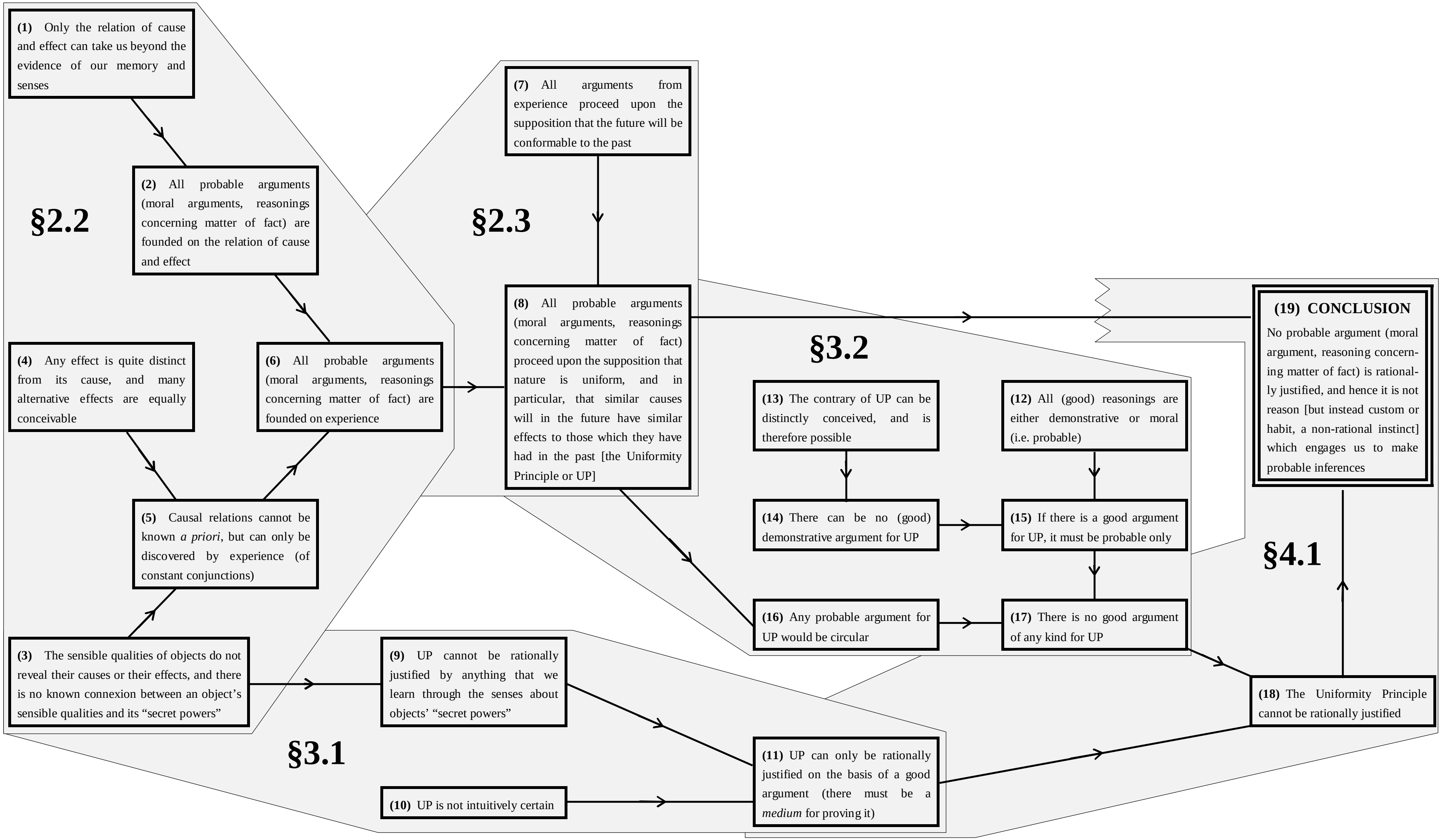


Figure 1a **The Main Logical Blocks of the *Enquiry* Argument (as analysed in Sections 2.2 to 4.1)**

(1) E26: "By means of [*Cause and Effect*] alone can we go beyond the evidence of our memory and senses."

(2) E26: "All reasonings concerning matter of fact seem to be founded on the relation of *Cause and Effect*."

E35: "all arguments concerning existence are founded on the relation of cause and effect"

E159: "all our evidence for any matter of fact, which lies beyond the testimony of sense or memory, is derived entirely from the relation of cause and effect"

(3) E27: "No object ever discovers, by the qualities which appear to the senses, either the causes which produced it, or the effects which will arise from it"

E33: "It is allowed on all hands, that there is no known connexion between the sensible qualities and the secret powers"

(4) E30: "every effect is a distinct event from its cause. It could not, therefore, be discovered in the cause, and ... the conjunction of it with the cause must appear ... arbitrary; since there are always many other effects, which, to reason, must seem fully as consistent and natural."

(5) E27: "the knowledge of [cause and effect] is not, in any instance, attained by reasonings *à priori*; but arises entirely from experience"

E28: "*causes and effects are discoverable, not by reason, but by experience*"

E30: "In vain, therefore, should we pretend to ... infer any cause or effect, without the assistance of observation and experience."

(6) E27: "nor can our reason, unassisted by experience, ever draw any inference concerning real existence and matter of fact"

E30: "In vain, therefore, should we pretend to determine any single event ... without the assistance of observation and experience."

(7) E33: "we always presume, when we see like sensible qualities, that they have like secret powers, and expect, that effects, similar to those which we have experienced, will follow from them"

E35: "We have said, that ... all our experimental conclusions proceed upon the supposition, that the future will be conformable to the past"

E37: "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"

(8) E35: "We have said, that all arguments concerning existence are founded on the relation of cause and effect; that our knowledge of that relation is derived entirely from experience; and that all our experimental conclusions proceed upon the supposition, that the future will be conformable to the past."

(9) E33: "the mind is not led to form such a conclusion concerning [sensible qualities and secret powers'] constant and regular conjunction, by any thing which it knows of their nature"

(10) E34: "The connexion between these propositions [*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*] is not intuitive."

(11) E34: "There is required a medium, which may enable the mind to draw such an inference, if indeed it be drawn by reasoning and argument."

(12) E35: "All reasonings may be divided into two kinds, namely demonstrative reasoning, or that concerning relations of ideas, and moral reasoning, or that concerning matter of fact and existence."

(13) E35: "it implies no contradiction, that the course of nature may change ... May I not clearly and distinctly conceive [such a thing]?"

(14) E35: "That there are no demonstrative arguments in the case, seems evident"

E35: "whatever is intelligible, and can be distinctly conceived, implies no contradiction, and can never be proved false by any demonstrative argument or abstract reasoning *à priori*"

(15) E35: "If we be, therefore, engaged by arguments to put trust in past experience, and make it the standard of our future judgment, these arguments must be probable only, or such as regard matter of fact and real existence"

(16) E35-6: "To endeavour, therefore, the proof [that the future will be conformable to the past] by probable arguments, or arguments regarding existence, must be evidently going in a circle, and taking that for granted, which is the very point in question."

(17) E35: "it may be requisite ... to shew, that none of [the branches of human knowledge] can afford such an argument"

E159: "we have no argument to convince us, that objects, which have, in our experience, been frequently conjoined, will likewise, in other instances, be conjoined in the same manner"

(18) E39: "it is not reasoning which engages us to suppose the past resembling the future, and to expect similar effects from causes, which are, to appearance, similar"

E159: "nothing leads us to [expect constant conjunctions to continue] but custom or a certain instinct of our nature"

(19) E32: "I say then, that, even after we have experience of the operations of cause and effect, our conclusions from that experience are *not* founded on reasoning, or any process of the understanding."

E43: "All inferences from experience, therefore, are effects of custom, not of reasoning"

E46-7: "All belief of matter of fact or real existence [is due merely to] a species of natural instincts, which no reasoning or process of the thought and understanding is able, either to produce, or to prevent."

Hume's Statement of the Stages of the *Enquiry* Argument

APPENDIX 3:
A Proof of Chebyshev's Inequality

To render this thesis mathematically self-contained, and to satisfy the curiosity of any reader who may wish to be assured of the full basis for De Finetti's (and indeed Bernoulli's more familiar) law of large numbers, there follows a simple proof of Chebyshev's Inequality, stated in the form most suitable for the derivation of De Finetti's law. The proof of the initial lemma is adapted and expanded from Hogg and Craig (1965) pp. 47-8.

We start by proving an important lemma, that if $u(V)$ is a non-negative function of the random variable V with expectation $\mathrm{E}[u(V)]$, then if c is any positive constant,

$$(*) \qquad \mathrm{P}\big(u(V) \geq c\big) \;\leq\; \mathrm{E}[u(V)]/c$$

We assume that the random variable V is continuous with density function $v(x)$, though if V is discrete the proof can easily be adapted by using sums rather than integrals.

Let A be the set of all values x for which $u(x) \geq c$. If $A = $Ò (that is, if $u(x) \geq c$ for *all* values of x) then the integral $\int_A u(x)v(x)dx$ will give the sum of $u(x).v(x)$ for all values of x from $-\infty$ to $+\infty$, which will therefore be equal to $\mathrm{E}[u(V)]$. If on the other hand $A \subset$ Ò, then that integral will exclude some values of $u(x).v(x)$, and since such values can only be positive or zero, the integral will be either less than or equal to E[$u(V)$]. In either case we have:

$$\int_A u(x)v(x)dx \leq \mathrm{E}[u(V)].$$

Now recall that A is the set of all values x for which $u(x) \geq c$. Hence the left-hand side of this inequality cannot be increased if we replace $u(x)$ by c. This gives us:

$$c\int_A v(x)dx \leq \mathrm{E}[u(V)].$$

But since $v(x)$ is the probability density function for the random variable V, we also have that

$$\int_A v(x)dx = \mathrm{P}(V \in A) = \mathrm{P}\big(u(V) \geq c\big).$$

And by combining these last two results, we reach the desired lemma:

$$(*) \qquad \mathrm{P}\big(u(V) \geq c\big) \leq \mathrm{E}[u(V)]/c$$

Moving on, let X be any random variable with mean μ and variance σ^2. Then the Chebyshev Inequality, which we are to prove, states that:

$$\text{for any } t > 0, \qquad \mathrm{P}\big(|X - \mu| \geq t\big) \leq \frac{\sigma^2}{t^2}$$

To prove the Inequality, we first use our lemma ($*$) with V substituted by X, $u(V)$ by $(X - \mu)^2$ and c by t^2 (where t is positive):

$$\mathrm{P}\big((X - \mu)^2 \geq t^2\big) \;\leq\; \mathrm{E}[(X - \mu)^2]/t^2$$

We can simplify both sides of this inequality, on the left by taking square roots, and on the right by noting that $\mathrm{E}[(X - \mu)^2]$ just is the variance σ^2:

$$\mathrm{P}\big(|X - \mu| \geq t\big) \leq \frac{\sigma^2}{t^2}$$

which is the Chebyshev Inequality, as desired.

A common alternative form of the Chebyshev Inequality can be derived from the lemma ($*$) with V as before substituted by X and $u(V)$ by $(X - \mu)^2$, but this time with c substituted by $k^2\sigma^2$ (where k is positive). This yields the result:

$$\mathrm{P}\big(|X - \mu| \geq k\sigma\big) \leq \frac{1}{k^2}$$

REFERENCES

Listed on the following pages are all the works referred to in this thesis, divided between those that are cited in Part I (mainly on the interpretation of Hume) and those that are cited in Parts II and III (mainly concerned with technical issues) – the few works that are cited in both parts accordingly appear twice. Within this framework entries appear in alphabetical order of author and then date order, with the relevant date being in most cases that of first publication (rather than, for example, date of translation). However in the case of books that have gone through several editions incorporating significant authorial changes, the date of the edition actually referred to is given, followed at the end of the reference by the dates of previous editions. Where reprints or translations are mentioned, this indicates that any quotations and page references have been taken from these rather than from the original publications. Following each reference, in brackets, is a list of the pages of the thesis at which that reference is mentioned (if a book is *never* itself mentioned, this indicates that it has been included in the bibliography in virtue of containing articles that are mentioned).

Since the major publishing houses now operate worldwide and are very well known, the place of publication has been omitted from references to books published by them, and the familiar university presses are referred to simply by the name of the university concerned – so “Oxford”, for example, refers to the Oxford University Press (or Clarendon Press).

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In the case of Hume himself, there are obviously far too many references for them all conveniently to be specified in either a bibliography or a simple index of names. Hence to provide a worthwhile resource for cross-referencing and other such purposes, all discussions of or quotations from specific sections or pages of Hume's relevant works have been listed in separate indexes, which deal in order with the *Treatise of Human Nature*, the *Enquiry Concerning Human Understanding*, the *Abstract of the Treatise*, the *Enquiry Concerning the Principles of Morals*, and the *Dialogues Concerning Natural Religion*.

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