Philosophy of Rational Belief

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Submitted for the degree of

Ph.D.

University of Edinburgh

1984

Declaration

I declare that this thesis is my own composition and is, except where specifically stated otherwise, the result of my own research.

Acknowledgements

References to primary and secondary literature are acknowledged in the notes at the end of the text. I am grateful to a number of people for answers in conversation or by letter to questions on specific points: especially to Dr P.Addison, Dr D.Bloor, Dr D.O.Edge, Mr J.E.Llewelyn, Dr A.Millar, Dr H.Montgomery, Dr P.Mott, Prof. R.Swinburne, Prof. N.Tennant, and Prof. K.Ward, and to any others whose comments or suggestions provoked reflection at the time which I cannot now recall. I am especially grateful to Prof R.W.Hepburn for many helpful suggestions, comments and recommendations, especially for his advice to read the work of L.J.Cohen and articles or books by H.H.Price, and K.Ward. Prof. D.W.D.Shaw supervised at a very early stage, and to him, and especially to Prof. J.McIntyre and Prof. J.P.Mackey the continuing supervisors I am very grateful for many occasions when I have been encouraged to rethink, or to approach from a fresh angle, the problems with which this work is concerned. Errors and confusions remain of course my own.

I also wish to thank my wife for checking some of the script, and Mrs. G. Ellis for turning an untidy manuscript into an ordered typescript.

Contents

Abstract of Thesis

The problems of characterizing rational belief are fewer than is the case with knowledge, though we presumably believe more things than we know. Knowledge is often defined as a special kind of justified true belief. Fallibilism urges that any of the conditions for knowledge may fail. In the case of rational belief we are dealing with fewer conditions, and withdrawal in the event of failure is less drastic. 2. Talk of beliefs involves problems of intensionality (referential opacity) and intentionality (allusion to believing subjects). But talk of beliefs is not clearly dispensable. Reference both to conscious mental states and to dispositions is needed to account for belief. If belief is to be rational it requires at least adequate evidential support. Belief should largely be determined by evidence but there is a voluntary element which we cannot exclude. Rational belief is not all or nothing acceptance. Paradox results if we do not proportion belief to evidence. 3. We need to consider the rationality of complexes of beliefs. Here two such sets of beliefs are selected for study in their own right, and for purpose of comparison. One comprises beliefs about the historical past, the other, metaphysical theistic beliefs. Mitchell argues for an analogy between them, and that in each case upholders of such beliefs construct a cumulative case in their defence. This raises complex issues of which the most important is that of the criteria to be used. 4. Beliefs about historical events use theory and observation to give an account of what is not directly accessible. Rival theories are assessed by criteria as in other disciplines. The extent to which the criteria used here differ from those used in science is considered in relation to two problems One is the special character of historical explanation. The other is

the debate between realists and anti-realists with regard to the historical past, and the role of implicit prediction here. Though there are differences from science, the rational assessment of beliefs about the historical past has identifiable similarities with science in its methods and criteria. 5. In the case of metaphysical beliefs examples are selected from recent writing on theistic belief by Swinburne. Some, both theists and non-theists, differentiate sharply between metaphysical and other beliefs. Criticism is here made of those who emphasize incommensurability and commitment as precluding rational scrutiny. Hick's view is more cautious, and we must admit that people do use rational criteria yet differ in their conclusions. Swinburne is perhaps over-confident in using Bayes' theorem here, with consequential over-emphasis on prior probabilities. 6. We need to consider several criteria and their appropriateness for assessing metaphysical beliefs. i) Internal consistency is one, as is consistency with other beliefs. ii) Swinburne places too much emphasis on simplicity, though it is one criterion amongst others. iii) Explanatory power is also relevant, though gains in explanatory power take different forms, and some gains are of an unusual type. iv) Fruitfulness in making successful prediction plays a large role in science, and is implicit in historical study. It should not be excluded here, though appeal to it requires careful assessment. v) Accuracy is also important, and should be ranked higher than simplicity, though assessing accuracy here has problems of its own. We can therefore use these criteria to assess the rationality of metaphysical as of other beliefs, but in some cases, here as elsewhere, clashes of criteria leave ground for continuing conflict.

Introduction

We all of us reveal by our actions and our words that we act on certain beliefs. Most of us claim that at least some of our beliefs are rational. But why do some of us, some of the time, claim that some beliefs are more rational than others? It is this question with which this work is concerned. The question can be stated simply and briefly, perhaps too briefly and a little carelessly. But it is a very searching question. It will take several chapters even to explore and to tease out some carefully selected issues raised by the question. To specify with complete accuracy what in any context it is rational to believe is almost certainly beyond human capacity. Yet to explore why in certain contexts we hold that it is more rational to believe one thing than another is of great importance. In order to consider this question of why it is held to be more rational to believe some things than others I wish to select a set of more specific issues which will help to focus the discussion, and help to specify those areas within the vast numbers of our beliefs with which I am especially concerned.

The nature of belief itself is integral to the discussion as a whole. It is not just a preliminary matter. Unless we have at least some notion of the distinction between belief and knowledge we are liable to great confusion when discussing particular sets of beliefs. My examples of particular sets of beliefs will chiefly be drawn from the areas of history and metaphysics. But even within discussions of the methodology of science one finds instances of this confusion

over the relation between knowledge and belief. Scientific theories and hypotheses are revised. This process is often spoken of in terms of the growth of scientific knowledge. But such a phrase is prone to mislead the unwary. It suggests progressive accumulation of knowledge. But if a theory or a hypothesis in any field is deemed to be false, and is replaced with another deemed to be more worthy of belief, it is not wholly accurate to speak of the growth of knowledge. What is happening is the replacement of one set of beliefs about reality with another set. The second is similar, to a greater or less extent, to the first. But the crucial point is that we consider the second to be more worthy of belief or more rational than the first. Discussion of the relation between belief and knowledge is therefore essential to our enterprise. It is not a mere preliminary. It is a vital part of the whole enterprise. This issue therefore will be given prominence in the opening chapters along with the equally central and necessary discussion of the character of belief itself.

I am <u>not</u> proposing, except perhaps in isolated hints and in occasional lapses, to say what it is that one should believe. I hold that many make such assertions too hastily, though it is a temptation to which I may be prone from time to time and I hope the reader will have enough natural curiosity to feel that an <u>occasional</u> hint of this kind is not too serious a blemish in a work officially devoted to studying methods of comparing the rationality of sets of beliefs.

I am proposing to focus on just why it is that we do

(and perhaps should) hold that certain beliefs or sets of beliefs are more rational than other beliefs or other sets of beliefs. I am aware that other people have interested themselves in the question and in print. I hope to allude to some of the works that have helped me most or provoked me most, though failure to mention a work does not mean that I do not value it or have not read it, (though it may mean that). An enormous discussion has focussed on the rationality of scientific theories. Works with titles such as 'The Rationality of Science', or 'The Methodology of Scientific Research Programmes' or 'The Logic of Scientific Discovery' all in one way or another are concerned with why it is deemed more rational to believe some scientific theories than others. But this work is not concerned with the natural sciences. What I am concerned with is the rationality of beliefs about other matters. The two chief areas of belief with which I am concerned are why it is rational to hold some specific beliefs about the past, and why it might be rational to hold (or not to hold) some metaphysical beliefs rather than other metaphysical beliefs.

These areas of belief about the historical past and belief in propositions of a metaphysical character are not unrelated. They share in common a tendency to 'deviate' to a greater or less extent from beliefs about those matters considered to fall within the province of natural science. Indeed one influential writer (Basil Mitchell) has argued at length that one can defend the rationality of metaphysical beliefs at least to some extent by an analogy which makes an appeal to the methods we use to argue that beliefs about

historical past are rational.

Some readers may be put off by the fact that I am even willing to consider that there might be ways of arguing that some metaphysical beliefs are more rational than others. I discuss this issue in more detail later and there defend my stance. Here I briefly assert that philosophers of science are now much less confident than they once were that one can rigidly exclude all metaphysical beliefs from either the set of scientific beliefs or the set of rational beliefs. If some metaphysical beliefs are necessary elements in either of these other sets, then we had better explore overtly the question of how to determine which of these it is most rational to hold.

Other quite different readers may hold that the only beliefs which it is really of ultimate importance to hold (or to choose rationally) are metaphysical beliefs. Such readers may feel impatient with the earlier chapters and argue that they are only a time consuming preliminary. To such I would wish to make two replies. The first is that it is not frivolous to maintain that patience in such matters is a virtue. The second reply is a little longer. It is widely held that some metaphysical beliefs and especially theistic beliefs are near to or on the far side of the limits of rational belief. It is therefore very important to say clearly and carefully why one holds that there are methods of arguing that some of these metaphysical beliefs are rational and/or more rational than other such beliefs or sets of beliefs. That is the end point of my discussion. But the rest is not merely preliminary. There is an

argument which runs through the whole and which must be assessed as a whole. Belief and knowledge must be compared and differentiated or confusion will ensue. The character of belief itself is a necessary element in considering the character of rational belief. The rationality of beliefs about the historical past offers an excellent area to test the permissibility of deviation from scientific method in assessing rational belief. And finally the question of metaphysical belief raises the question of whether some metaphysical beliefs are inescapable in our total set of beliefs, and whether we can give a good account of methods for setting limits to rational belief or at least for preferring to hold (or not to hold) some metaphysical beliefs rather than others. This is the programme of this work. This introduction has of course been written, as it should be, as the writing of the work as a whole comes towards its end. But I most strongly urge the reader to read and indeed reread this introduction. A seasoned traveller arriving in a city understands the wisdom of obtaining a map at the first opportunity.

Chapter One Rational Belief and the Question of Knowledge

Rational belief is our concern in the pages which follow. I propose to begin by making some comparisons between the problems of rational belief and the problems of knowledge. In current philosophical discussion there is much debate over the attempt to define knowledge in terms of belief. We shall see in due course that one such influential attempt specifies four conditions. According to this view knowledge is undefeated (or indefeasible) justified true belief. The greatest controversy at present surrounds the attempt to define the notion of a defeasibility condition. This is commonly referred to as 'the fourth condition', the others being that what is known must be justified, must be true, and must be believed. Because the debate about knowledge has been so prominent in philosophical writing it has tended to overshadow the topic of rational belief. But the latter is of great interest in its own right. A look at some aspects of the relation between the two areas will therefore be an appropriate place to begin. I intend to maintain that most of us would say that what we claim to know consists of a smaller set than the set of what we believe. (If knowledge can indeed be defined in terms of belief then what we know (if anything) is a subset of what we believe). But the problems of knowledge are more extensive than the problems of belief. By this I do not have in mind the problems raised by the great number of things that we believe. I mean that the debate over the problem of knowledge raises all the issues connected with rational belief and some additional ones. If this is correct

then to focus on the question of rational belief is, in this sense, more specific than focussing on the problems of knowledge. But it would also be relevant to, and a contribution towards, the discussion of the problem of knowledge. One way of putting this would be to say that a concern with rational belief is a concern with what it is to believe, and with what it is for us to claim that at least some of our beliefs are justified. There are however certain difficulties about the use of the term 'justified'. For reasons that will, I hope, become clear, attention will be focussed on the notion of rational belief, and an attempt made to tease out some of the problems of rational belief. Specific attention will be directed to the question of criteria for rational belief in the fields of history and of metaphysics. But more of that anon.

There are things which we think that we know, but which it is not certain that we know. We think that we know that we as human persons evolved by natural selection. But we do not have absolute guarantees that it is truly knowledge. It is not certain that we know it. But we do claim, or many of us claim that it is rational to believe that we evolved by natural selection. Similarly we <u>may</u> know that our present universe began between ten and twenty thousand million years ago. But it is not certain that we know this. Whether we do in fact know it will depend on various factors, of which one of the most important is whether it is in reality the case that our universe began when we think it did. But we do claim, or many of us claim, or at least I am not alone in claiming, that it is rational to believe that it began in the period

specified above. The problem of knowledge and the problem of rational belief are related, but they can be distinguished. My chief concern in this present work is with the philosophical problem of rational belief. But it is appropriate to approach that problem by means of a skirmish with the problem of knowledge.

I have begun with a series of assertions, and I now wish to provide some of the arguments which lead me to make these assertions. I propose to argue that our claims to have knowledge are vulnerable, and that this accentuates the importance of our claims to rational belief. I do not claim that we have no knowledge, merely that our claims to have knowledge are vulnerable. Nor do I claim that there is a single category of rational belief, merely that the questions of epistemic and doxastic probability are in need of elucidation. For example I would say that it is rational to believe that p if there is strong or adequate evidential support for p. But this proposal opens up further questions. What kind of evidential support is relevant to different classes of statement, and how do we assess the strength or weakness of such evidential support? The discussion of these questions opens the way to yet more questions, so an extensive discussion will be necessary. This discussion must eventually consider the criteria for rational belief. These may vary from one domain to another. The criteria for rational beliefs about electrons may differ from the criteria for rational beliefs about Caesar's conquests in Gaul, and the criteria for rational beliefs about what is just may differ yet again. In the case of metaphysics some might argue that there are beliefs,

but that these are incapable of rational justification. This view might be taken by upholders of metaphysical beliefs as well as by opponents. For example some theologians might maintain that one ought to believe in God even though one could not provide adequate evidential support for such a belief. Whether or not this domain is an appropriate field for the application of rational criteria of evidential support is a matter of vigorous debate, and that debate will be considered in this work in due course. But our initial starting point was a vague assertion that our claims to knowledge are vulnerable, and I must soon attempt to give greater precision to that assertion.

The problem of rational belief comes to the fore when we maintain that most claims to knowledge are fallible. We may think that we know that p, but our claim to know it can fail for a variety of reasons. Chief amongst these is simply the realization that p is false. If p does turn out to be false then our claim to know has to be withdrawn. We are obliged to eat our words. I said that I knew that p, and I thought I knew that p, but now I realize that I did not know that p, and that p is false. But though it may later turn out that my claim to know that p was false, this does not necessarily detract from it having been reasonable for me to believe that p. Of course if p turns out to be false then it is no longer reasonable to believe that p, but it does not necessarily cease to have been reasonable to believe that p on the evidence previously available. In this way claims to hold a belief rationally differ from claims to know. This difference means that the whole question of rational belief deserves to be treated as a topic related to,

but different from, the problem of knowledge. The question of rational belief needs to be looked at in its own right, and also in relation to the question of knowledge, and especially in relation to the fallibility of claims to knowledge.

The problems of knowledge are made most acute by the impact of sceptical questioning. It is the various forms of scepticism which reveal problematical features in epistemology. Against radical scepticism it has been argued[1] that a doubt which doubts everything is not a doubt. This counter argument against radical scepticism has a certain force. It notes that doubt depends for its expression on certain assumptions. This reply to scepticism maintains that one cannot formulate or articulate a doubt without making some assumptions. Some things have to be held steady so that others can be tested. But this counter argument fails to defeat one of the main points which a sceptic can put forward. The sceptic may not argue that we could be mistaken about everything. A more limited challenge may be presented. This can still be quite serious. It is the argument that any one of our beliefs, or claims to knowledge, may turn out to be mistaken. This challenge is quite sufficient to throw doubt on every tenet that we hold, as it leaves us uncertain which is, and which is not, in fact true. We realize that this weakened sceptical challenge does not suggest that every member of our belief set is false, but it does cast doubt on each individually and on very many subsets of our total set of beliefs.

Our confidence in what we claim to know or in what we merely believe is undermined by sceptical questioning. Our

tenets are open to correction. It may therefore turn out that though we think we know that p, yet p may be false after all and our claim to know that p may be undermined. One response to this weakened sceptical challenge is to attempt to find some beliefs which are incorrigible. Obviously if there is no way in which p can possibly be false then our claim to know that p cannot be undermined in this manner. If p is an incorrigible statement then there is no risk of p being false, but there may be other reasons why we do not after all know that p.

To err is human and the contention of fallibilism is that we often err, or that there is almost always a possibility that we are in error. It is therefore the view that we are almost always prone to error, rather than the view that we are almost always mistaken. If the fallibilist challenge is correct, then it has considerable consequences for any theory of knowledge. We only know that p, if several conditions for knowledge are met, including the prime condition that p be true. If we are always prone to error then any one of these conditions may fail and our supposed knowledge degenerate into mistaken belief or inadequately grounded belief.

Claims to knowledge can fail not only because p turns out to be false, but because some other condition for knowledge is not met. For example a fellow walker may say, after a quick look at the sky at the start of a ten mile walk, that he is going to take a chance on the weather, and leave his waterproof clothing behind. At the end of a sunny morning he says 'There, I knew it wouldn't rain'. It is clear that he hoped that it wouldn't rain, but by no means clear that he knew it. If he had at the

start said that he knew it wouldn't rain because he had listened to the local weather forecast, I might be less inclined to doubt his claim at the end of the morning (even though I fear that the forecast is not regularly updated, and suspect that sometimes the announcer will continue to predict a dry day when rain is beating upon his studio window). But a quick look at the sky really was an inadequate justification. I think that he may have (confidently) believed that it wouldn't rain, and had some evidence to support his belief, but I don't really think that he knew. Had he said that he knew that the walk across the hills was only seven miles and not ten because he had measured it on the map, I would accept his claim, as long as I had reason to believe that he was an experienced map reader. It is necessary for knowledge that p is true, that someone (confidently) believes that p and has good ground for believing it. A further condition may also be necessary, but for the moment three will suffice. If any of these three fail, then we may have an instance of confident but mistaken belief, or of a lucky guess that turns out to be true, or of confident true belief that was inadequately justified, but not of knowledge. My main concern is with those cases where the claim to know fails because p later turns out to be false, but these other factors must be noted.

The factors other than the truth of p are complex, and the failure of any one of them may undermine a claim to know. Suppose my companion says at the outset that the walk is only seven miles but is not confident about his map reading. We use a pedometer and at the end of the morning it reads seven miles

exactly. He may say 'I knew it', and it is quite in accordance with normal usage to say such a thing. But it is also in accordance with normal usage to have reservations about such a claim. Hesitant true belief is not really knowledge even if we tolerate the loose usage of the term 'know' in such instances. It has been claimed that we use the word 'know' in a strong and in a weak sense.[2] But this view is not without its difficulties.[3] It might be better to say that we use the word 'know' in several senses, some weaker and some stronger. This would capture the fact that in some instances we tolerate the claim 'I knew it', though on a more rigorous examination of the claim we would reject it. There is a discrepancy or apparent contradiction here in the way in which the word 'know' is allowed to slither in ordinary language. The context makes it clear to the speaker of ordinary language that the claim 'I knew it' is not to be subjected to the demands of questioning as to whether hesitant true belief really amounts to knowledge. This shift of sense or connotation can be disturbing to those who wish to capture the sense of natural languages in a formal system. But it need not be a sign of inconsistency. In the study of one's own language or other natural languages a good dictionary or lexicon will list and classify different senses of a word by citing them in context. Thus the difference between knowing a fact and being acquainted with a person will be marked out. There is no reason to think that formal systems cannot be adjusted to take account of this vital feature of natural languages. Just as the lexicon codes the different uses or senses of a word, so a formal system can distinguish between

k_1, k_2, k_3 and so on. Indeed unless a formal system has some means of coping with vagueness in natural language, it is likely to be the formalization which introduces confusions which a competent linguist would be able to untangle.

In the case in question 'I knew it' is equivalent to 'I was right' rather than to 'It was an instance of real knowledge from the outset'. But the example indicates an inconsistency in our usage of the term know, only if we admit the premiss that all uses of the same word must have the same sense. But this latter premiss is evidently false. We do use words in different senses, and this is also the case with the word 'know'. In saying this I am not committing myself as yet on further distinctions between different senses of the word 'know', nor on whether these create or resolve epistemological problems.

Except in the weaker sense just identified, we do not count hesitant true belief as knowledge. This fact may have a bearing on another issue, namely the awareness of whether one is or is not confident about a particular belief. It could be argued that if one knows as against merely believes that p, then one must at least be confident that p, and that each of us knows whether or not we are confident in a given case. Suppose that we grant that we know whether or not we are confident in any given case. This would entail that if we are not confident, then we know that we do not know that p, for we know that we only believe that p. But the converse would not be entailed. Though we might know that we were confident that p, it would not follow from this that we knew that p, and we might know that we were confident that we knew that p but not know that we knew that

p. So we might know that we were confident that we knew that p, but not know that we knew that p. In order to know that we knew that p other conditions would need to be satisfied, such as an adequate justification. So only limited inferences could be drawn from the assumption that we know whether or not we are confident in a given case. We can know that we don't know that p, if we know that we lack confidence. But even if we knew that we were confident, we would only know that we were confident, or we would only know that we were confident that p, not necessarily know that we knew that p.

It may be different however with incorrigible propositions. In the case of some mathematical equations there is no risk that the proposition will turn out to be false. In a very complex equation there is a risk of error. But in very simple ones such as 2 + 2 = 4, there might be a risk that we might misread one of the figures, but there is no risk that sane, healthy, and intelligent adult citizens will be liable to error as to its truth or falsity. In the case of such an incorrigible mathematical truth one might argue that merely to be confident that it is true is sufficient for one to know it, and to know that one knows it. If this view is correct then there would be at least some cases in which one not only knew that p, but also knew that one knew it. If this view is correct then this situation would obtain, but only with a very specialized group of indubitably true propositions. But is it correct? Malcolm in the second version of his article 'Knowledge and Belief' reformulated his claim on this subject. Originally he had written 'Reflection can teach me that I know something in this [the strong] sense'. Later

he corrected this to '... reflection can make us realize that we are using 'I know it' in the strong (or weak) sense in a particular case'. The second formulation is more restricted. Malcolm's examples included statements reporting direct perception as well as simple mathematical equations, so his correction involves at least two factors. These need separate discussion, and I propose to take the case of reports of perception first. Malcolm's examples envisage a situation like this. A says that he knows there is a sheet of paper in front of him. He realizes that he is using 'know' in a strong sense. There is no way he can imagine what he claims to be false, though others might imagine his claim to be false. This seems to me to overstate the case. Others may indeed imagine that A's claim is false, by, for example, suspecting that A is deluded. But so might A himself. Malcolm's 'strong sense' is too strong. I grant that the word 'know' is used in different senses, and I have already noted an example of a weaker sense of the word. But there are limits to the inferences that may be drawn from this linguistic feature, and Malcolm seems to have overstepped them. I may confidently believe with good ground that there is a sheet of paper in front of me, and be aware that this situation obtains. This may lead me to say emphatically that I know that there is a piece of paper here. But even this strong knowledge claim does not wholly exclude the possibility of error. Malcolm seems to think that the possibility of error is there, and that others may recognize this, but that I cannot both say 'I know' in the strong sense and suppose that anything could disprove p. There is a problem

here, but what is it? It is erroneous to think that I could justifiably say that there is no possibility of error. If someone else can conceive of my being mistaken about the paper, then I can. I cannot exclude the possibility of my being subject to illusion or delusion. If it is incompatible both to say 'I know that there is a sheet of paper here' and to admit under pressure that I cannot exclude error, then it is the first of these statements which is the source of the trouble. The strong use of 'know' is running into some of the same difficulties which afflict weaker uses of the term.

This problem must be explored further. If I do indeed know that p, then p is true. But it does not follow from my saying 'I know that p' that I do indeed know that p. It is possible that p is false. But I cannot coherently say 'I know that p and I believe that p may be false'. The problem is that if I say that I know there is a piece of paper here, then I cannot coherently also believe that I may be mistaken. Yet I may be mistaken. Clearly I believe that I am not mistaken, but I am also obliged if subjected to severe sceptical questioning to admit that I may be mistaken even about matters of direct perception. The upshot of this line of argument is that though we may in practice say with emphasis 'I know that p', this usage is problematical. Even the emphatic use of 'I know that p' now looks as though at least in some contexts it means something less than it appears to mean. It now seems in these cases to be equivalent to 'I confidently claim that p'. There may still be several senses of 'I know', but even the supposedly strong sense is not always as strong as was supposed.

The difficulty just noted concerns statements of direct perception. For the moment I propose to leave aside special cases such as statements saying 'I know that I exist' and 'I know that I believe something'. I propose next to consider the case of claims to know mathematical truths. On the face of it there is a difference between claims to know that 2 + 2 = 4 and claims to know that 99 x 99 = 9801. Both mathematical statements are true. On the customary analysis of knowledge if I have adequate justification for believing these statements and confidently believe them, then I know them. It is unlikely that I would be hesitant about the first, but I might be hesitant about the second, in which case I would not know it though I might correctly but hesitantly believe it. As both statements are necessarily true it is impossible that they could turn out to be false. So here there is no question of a claim to knowledge failing because p turns out to be false. But there could be a failure of justification in the second case. My mathematical skills may be very rusty. I believe that I have correctly calculated 99 x 99 as 9801, but I have in fact made two errors. The errors cancel each other out. My result is therefore correct but scrutiny of my method immediately reveals to you that I have made these two glaring errors. Though what I claim to know is indubitably true, and though I confidently believe it, and though I think I have adequate justification, in fact I do not know it.

But the example just given must be carefully distinguished from other situations in which a claim to knowledge has failed or may fail. In other cases hesitation about p, or the falsity

of p, lead to the collapse of a claim to know that p. In this case there is no possibility of p being false. Yet the claim to know that p is false. It has failed not because of the falsity of p, but because of error in the supposed justification for believing that p. It follows that even if p is necessarily true and we confidently assert that we know that p, we may still not in fact know p. The existence of necessary truths is no defence against the contention that any of our claims to knowledge may fail.[4]

We must also, however, bear in mind the case of privileged knowledge. Here at least we must note an exception. If I believe that I believe something,[4a] then I do believe something. If I believe that I exist, then my belief is true. In these very special cases a claim to knowledge could not fail. I do know that I believe something, and I do know that I exist. In neither of these cases can p turn out to be false, nor is my belief hesitant, nor is it inadequately justified. So there are some instances where claims to knowledge cannot fail. These instances are very few. They require, however, that any statement of epistemic fallibilism be qualified. The contention of epistemic fallibilism must be formulated not as the view that any of our claims to knowledge may fail, but that almost any of our claims to knowledge may fail. But this concession arises from the existence of privileged knowledge, rather than from the existence of necessary truths.

If we do in fact know p then it must be the case that p is true. But it does not follow that necessary truths or incorrigible propositions are necessary for knowledge. I may

indeed know p simply because I justifiably believe that p, and p is in fact true, though not necessarily true. Nor, as I have argued, does it follow from the fact that I believe a necessary truth that I know it. I may believe that p, when p is a necessary truth, but my reason for believing that p is quite erroneous. My erroneous route to the correct conclusion that 99 x 99 = 9801 provides another instance of a failure of claim to know. This failure is an instance of epistemic fallibilism. Epistemic fallibilism is different from doxastic fallibilism. There are ways in which a claim to knowledge may fail which differ from the ways in which a belief that p may fail. One version of doxastic fallibilism locates the possibilities of error either in the possible falsity of p, or in our capacity to believe not-p when p is necessarily true. Even if this is a correct description of doxastic fallibilism, we also need to note the further possibility of truly believing that p for erroneous reasons. Any one of the conditions for knowledge may fail. If we wish to characterize epistemic fallibilism, it is therefore necessary to set out the conditions for knowledge and to see how they may fail.

Much recent debate in epistemology focuses on the examples put forward by Gettier.[5] His examples pose the problem of a justified true belief which is dependent on a false statement. These examples point to the inadequacy of the view that knowledge is justified true belief. Gettier's examples have led to a rash of efforts to amplify the three classic conditions for knowledge by the addition of a fourth. This last is often a defeasibility condition. If one takes this line, one then

redefines knowledge as non-defectively justified true belief. Lehrer's 1974 solution to this difficulty was to conclude that someone knows that p only if completely justified in believing that p 'in the verific alternative to his corrected doxastic system'.[6] The verific alternative is then defined as a system in which erroneous beliefs are replaced with their contradictories. This attempts to ensure that S is completely justified in believing that p in a way that does not depend on any false statement. Lehrer's is one of several attempts to add a further condition which would ensure that non-defectively justified true belief was knowledge.

Vigorous debate still continues about the examples offered by Gettier. It would be possible to include further discussion at this point but it is not necessary to my argument. A further clause may well need to be added to the definition of knowledge as justified true belief. But if so that would strengthen and not weaken my case. We would then have further possibilities for the failure of claims to know. But three are already sufficient, even if the briefer definition of knowledge as justified true belief were adequate.

In order to characterize fallibilism I propose a more complex description than Haack's and one which I hope is less vulnerable to the objections brought against Haack's definition.[7] My strategy is to argue that knowledge claims are vulnerable because any one of the necessary conditions for knowledge may fail. Thus Kap may be false either because a does not (confidently) believe p, or because p is false, or because a is not justified in believing p. If that is not enough, one

might also add that a may truly and justifiably believe p but that the justification may depend on a false statement and so be defective. The version of fallibilism with which I am concerned is broad. It is not concerned only with cases where p turns out to be false, but with any case of the failure of a claim to know that p.

The version of fallibilism which I have outlined is not thoroughgoing scepticism. It does not maintain that we are always mistaken, or even that we <u>are</u> almost always mistaken. Nor does it maintain that we are <u>always</u> prone to error, only that we are <u>almost always</u> vulnerable to error. There are very few knowledge claims which are incapable of being false, and incapable of being disbelieved, and incapable of being believed on inadequate grounds.

This form of epistemic fallibilism is however not too distant from scepticism. In saying that almost any of our claims to know may be false, it leaves us vulnerable to doubt as to which of our purported claims to know is in fact true, and which is false. Kekes has argued that weak fallibilism will however lead to the more sceptical strong fallibilism, if the rationality of scientific methods is not defended. He accuses Popper and Kuhn of failure at just this point.[8] He claims that they assert rather than defend the view that the methods of science are rational, and that their failure at this point allows weak fallibilism to lead on to strong fallibilism. His claims are far reaching and their implications will be addressed at greater length as this work proceeds. For the moment it will suffice to say that our account of fallibilism is close to what Kekes

describes as weak fallibilism. Whether or not it eventually leads to more sceptical conclusions remains to be explored in due course.

If knowledge requires at least that we have justified true belief and if any of these conditions may fail, then a series of possible outcomes may ensue. There is hesitant true belief. I am presented with evidence which suggests the correct conclusion that a new particle has been discovered. But I am incapable of grasping the full force of the arguments, and only hesitantly accept a view which turns out to be utterly convincing. Or there is unjustified true belief. I arrive at the truth but I do not know it, because my reason for believing the true answer is erroneous. In another instance I may possess strong evidence that p, and confidently believe that p, but later discover that p is false. I suppose one calls this justified but false belief, though the description is not a happy one. The difficulty draws attention to the variation in meaning of the word 'justified' in different contexts.

If more than one condition for knowledge is deficient then further possibilities arise. I may believe something without justification and it may indeed be false. False belief for which there was scanty evidence, or perhaps none at all is not unknown. I have heard or read people who give every impression of believing that all inhabitants of a certain country (say Argentina) are to be despised, or all admired, on no other evidence than that they are citizens of that country. There is ample evidence that beliefs of this kind are neither justified nor true (no matter what the nationality may be).

Further examples begin to reveal complexities of belief not yet discussed. For example there may be some evidence for p, yet I may hesitantly believe that p, or even disbelieve p, and p indeed be false. Or again I may disbelieve p, as I lack evidence for p, but p may actually be true. And yet again there may be no evidence for p and I only hesitantly believe p, or refuse to believe p, and p is indeed false.

Even the examples given in the preceding paragraph may not exhaust the matter. If knowledge requires a fourth condition, then the list of examples could be extended. But it is already evident that my argument needs to look at the character and variety of beliefs and the evidential support for beliefs, and not solely concern itself with the special (though very complex) case of claims to knowledge.

There is another and more serious reason for turning to the question of rational belief rather than that of knowledge. I only genuinely know that p if (amongst other things) p is indeed true. But the concept of truth has its own difficulties in this connection. It is worth starting with a simple example. C is a secretive and autocratic prime minister and A and B are members of her cabinet. B firmly believes that there is a plan to hold an election next year, but that this is a secret not divulged to A. If A then comes to B and offers detailed information suggesting that C will 'go to the country in the spring', B suspects that the secret is out. He says to C 'A knows we are planning a snap spring election'. C has however no such firm intention. Relative to B's beliefs about C's plans, A 'knows' of the early election plan. B says that

A knows p, because amongst other things B firmly believes that p is true and the other conditions for A knowing p seem to be met. But p is false, though B does not realize it. If we suppose that B is garrulous and that D also believes that there is an early election plan, B will divulge A's information to D, and B and D will agree that A knows of the early election plan. The use of the word 'know' is not only tolerated but encouraged, as long as there is agreement between B and D that the belief in question is true. If it becomes evident later that there will be no such election, and that C had no such plan, the statement A knows that p or A knew that p has to be withdrawn.

In normal conversation[8a] the vocabulary of knowledge is used so long as the speaker and listener agree on the truth of what is at issue. In the example of the secretive prime minister, B and D agree that p is true (though it is not) and so agree in saying that 'A now knows that p'. Relative to the information available to B and D, p appears to be true, and so it seems that A knows p. But the subsequent withdrawal of the vocabulary of knowledge reveals that the language of knowledge incorporates realist assumptions. A claim to knowledge may be made, and accepted, so long as the speaker and hearers agree on the truth of p, but it is not perpetuated once p is shown to be false. (Of course other factors are also relevant to knowledge, but in this instance we may ignore them). There are some areas in which there is no danger of p being shown false, but many areas where it is in jeopardy. The difficulty posed by the need to revise claims to knowledge is a serious one. At a much higher level of generalization it

raises issues about the character of reality on which Kuhn and Popper take very different stances. Popper claims that the task of criticism is to produce theories of increasing verisimilitude. As successive conjectures are put forward in place of earlier refuted conjectures, it is hoped that a closer approach to reality is being made. Kuhn denied any such possibility, and argued that truth may be a term with only intratheoretic application.[9] But these heady heights of grand generalization require a longer approach. What is sufficient for our present purpose is to note that claims to knowledge are accepted on the basis of intersubjective agreement, but are withdrawn when it becomes evident that they are incompatible with an actual state of affairs. Of these two situations the withdrawal is the more significant. It points to the fact that, in many cases, knowledge is dependent on states of affairs beyond the control of those claiming to know.

In the case of the secretive Prime Minister had she said to B that A knew of her plan for an early spring election then the truth of p would not be in question (as long as she was not being Machiavellian). But in the case of statements claiming knowledge of, say, the age of the universe, then the ultimate truth of the matter is very much in question. In view of the need to withdraw claims to knowledge in such areas it is clearly wise to deal with direct statements rather than with propositional attitudes. Talk of 'revisable knowledge' introduces unnecessarily paradoxical features into epistemology. Claims to know are withdrawn, they are not revised. But it is simpler to deal with (and if necessary to revise) statements

such as 'The universe is between ten and twenty thousand million years old' rather than the more complex 'We know that ...' or 'our scientific knowledge includes the conclusion that...'.

There are, however, occasions when propositional attitudes are at issue, and it is with some of these occasions that I am concerned. It is appropriate to ask how we know, or whether we know that certain things are the case. It is also appropriate and sometimes preferable to ask whether it is rational to believe that such things are the case and why it should be rational to believe this. Where propositional attitudes are at stake there are advantages in focussing on rationality of belief rather than on knowledge. Chief amongst these advantages is that already outlined in the argument of the preceding pages. Claims to knowledge can and do fail for several kinds of reason, and consequently they need to be withdrawn. But in the case of rational belief the process of withdrawal is less complex. If I had good reason at time t_1 to believe that p, and at time t_2 am presented with a refutation of p, I may still correctly be able to maintain that it <u>was</u> then rational for me to believe that p, even though it no longer is so. Rationality of belief is a function of the available evidence, whereas knowledge depends on a wider range of factors. It is also worth considering whether the concept of knowledge is capable of being defined in terms of belief. If knowledge were correctly defined as undefectively justified true belief, then belief would be a more primitive category than knowledge. But the continuing debate over the

fourth condition of knowledge is only one reason for caution in pressing this claim. The question of the definition of belief is itself problematical and one must not too readily assume from the frequent efforts to define knowledge in terms of belief that the category of belief is itself clear and distinct.

In this chapter I have set out what I consider to be one important feature of the notion of rational belief. This is the discussion of the relation between the problems of rational belief and the problems of knowledge. I have argued that in focussing on the first of these we may be making some contribution to some of the problems of the second. The desire to know, and even the desire to know that we know is deep seated. But in many cases we may have to settle for something less. If we could at least in such cases defend the view that our beliefs about these matters are rational, we would have made some progress. So I propose to consider further what it is to believe, and what is involved in the claim that there are criteria for defending the rationality of some of our beliefs.

Chapter Two Belief and Rational Belief.

If we are to discuss the rationality of a specific set of our beliefs we need to have some idea of what belief is. A concern with rational belief involves an exploration of belief as well as an exploration of the rationality of particular sets of beliefs. This is not some distant preliminary matter but one which is central to the whole notion of rational belief.

In discussing the character of belief certain problems will quickly become apparent. One of these is a distaste in certain quarters for the whole notion of propositional attitudes. Could we not confine ourselves to the discussion of statements rather than beliefs? Or even better limit ourselves to the discussion of the truth or falsity of sentences? I have stated very bluntly an objection which is usually made more indirectly. What is more usually done is to point to the complications which the language of belief brings with it. I shall indeed very soon consider some of those complications. They are real and difficult. But I also propose to set out, perhaps with excessive brevity, some factors which make me wish to persevere with language of belief.

Discussing the truth or falsity of sentences is all very well, but it does not do justice to some of the other factors involved. Sentences are uttered; they are spoken by people. Those people may utter the sentences with varying degrees of conviction. Also if we cannot decide the truth or falsity of a sentence we might still wish to consider how probable it is. Such probability is an evidential probability. I shall argue that this is not to be confused with mathematical probability,

but involves epistemic or doxastic factors. Further, sentences cannot always be considered on their own. Discussion of one sentence about Caesar involves us in a whole complex of other statements, sentences, assertions and I would insist, beliefs about past history. So there is a proper place not only for the discussion of sentences but also of beliefs.

There are at least two objections to the free use of language including phrases such as 'a believes that'. One of these objections is the confusion which can arise as a result of referential opacity. Let us suppose that Sharp is a philosopher who knows something about Roman philosophy but has not studied Roman republican history. In this case Sharp believes that Cicero studied philosophy. But we will also suppose that Sharp does not believe that Tully studied philosophy. The identity of Cicero and Tully happens not to be known to Sharp. The problem has wider implications. We cannot (quite) say that Sharp believes p & -p. We have to allow that where propositional attitudes are concerned the following situation may obtain. Two individuals may be identical, but the same things cannot be said of them. Hintikka's proposal was to resolve this[10] by speaking of individuals in different possible worlds. But this leads us by another route to just the sort of difficulty that propositional attitudes have long been seen to produce. It leads to some kind of commitment to the existence of conceptual entities. Not only are there problems because of the obscurity of identity noted above, but also because the attempt to resolve such problems leads to a more complex and less economical ontology. Once we start attempting to analyse beliefs (or other aspects of intensionality)

we are involved with a wider range of entities. Extensions can be analysed in terms of truth values, classes, and relations. Intensions involve us with objects of thought, or with possible worlds.

So there are difficulties of formulating procedures for coping with referential opacity. But the attempt to resolve these difficulties leads to a more elaborate ontology. This in turn raises a further problem. It is not clear whether there is a separate class of intentional sentences which are capable of being marked off from other sentences. (If there are, then the existence of such a class of sentences might help to confirm psychophysical dualism. Philosophers such as Chisholm wish to maintain just such a view). Sentences of the form 'a believes that' might then not only be classed as intensional but also as intentional. Despite Cornman's objections,[11] it is inherently plausible that we should so classify cognitive sentences and especially belief sentences. The latter are sentences which refer to mental activities and therefore intentional.

Those who have reservations about belief statements have objected to them both on the grounds that they are intensional and on the grounds that they are intentional. In the former case they do not permit the substitution of extensionally equivalent phrases in the clause following 'a believes that'. They resist the desire of some philosophers to find an extensional translation for intensional sentences. This in turn means that such sentences require a more complex ontology. But if these sentences are also intentional as well, they raise more basic difficulties. They refer to mental activities and they cannot

readily be translated into sentences which speak of simple physical entities and processes. If my argument is to make positive use of belief sentences then I must at least indicate in outline why I resist the objections brought against them.

Both objections derive from the quest for a unified science. In the one case there is the desire to make language about mental process translatable into sentences about physical processes. In the other case there is the desire to translate all sentences into the simpler language of extensional sentences. Once these objections are starkly presented they can be entitled the physicalist thesis and the extensionality thesis. The two can be briefly considered in sequence.

The physicalist thesis put forward, for example, by Hempel is in any case not the only way to uphold the more important thesis of the unity of science. All that is necessary is for statements about mental events to be compatible with statements about physical events. It is not necessary for the former to be reducible to the latter. Further the argument could be reversed. If statements about the mental did in the end turn out to be irreducible to statements about the physical, this is not necessarily a ground for calling the former into question. If irreducibility were ever proved it might favour Chisholm's view as against that of Hempel.[12] It might favour psychophysical dualism. But the argument over this can at present be left to one side as there is no compelling reason at present to think that one of these views has prevailed over the others. Belief statements are an integral part of natural language, and a necessary part of psychology and this is a sufficient reason,

for the present, to continue to use them. I do not thereby commit myself to Chisholm's dualism, merely to the rejection of physicalist reductionism as a ground for calling statements about belief into question.

The objection arising from the extensionality thesis also requires discussion here, though it too will inevitably have to be considered briefly. Quine is one of the chief antagonists in this regard. He does not propose the abolition of propositional attitudes, indeed he admits that they are not clearly dispensible.[13] He says this despite the fact that such sentences involve intensions and his opinion that intensions are creatures of darkness in need of exorcism.[14] There are several problems. One is ambiguity. 'Ralph believes that someone is a spy' may be construed in a relational or in a notional sense. In the first case there is someone whom Ralph believes to be a spy. In the second case Ralph believes that there are spies. The problems grow when we allow quantification <u>into</u> a belief context. Such a procedure allows sentences to be true or false depending not on the individual to which reference is made, but on the description of that individual. Thus Ralph believes the man in the brown hat to be a spy. But the man in the brown hat is Ortcutt whom Ralph does not believe to be a spy. If we are not careful we say of Ortcutt that Ralph does and does not believe him to be a spy. Quine and others have discussed the consequent difficulties at great length. Central to the difficulties is this. In intensional contexts truth and falsity seem to depend not on reference to individuals but on the phrases used to describe individuals. But this leaves us with

an overpopulated mental universe in which there are as many entities as there are descriptions. Further we cannot satisfactorily resolve the question of when two intensions are or are not identical.[15] The objections which Quine has raised certainly reveal that there are obscurities and difficulties involved in dealing with intensions. But they do not exclude the use of sentences containing propositional attitudes. Indeed Quine admits that they are not clearly dispensable. We do need a better principle for the individuation of attributes but we should not for that reason reject sentences of the form a believes that p.

Though some intensions are also intentional some are not. Modal sentences are intensional and raise similar difficulties over identity and description, but are not intentional and do not obviously indicate mental discourse. Belief statements are both intensional and intentional and do belong to a realm of discourse which requires reference to mental activity. Chisholm has maintained that one can specify a set of belief sentences which have a distinct set of interrelationships. The entailments and non entailments between them mark off belief sentences as a distinct set of intentional sentences and these he says are a sufficient condition for identifying the sphere of mental activity.[16] My argument accepts some of Chisholm's bold contentions without requiring all of them. I agree that the relation between sets of belief sentences is one where attention to entailments and non entailments is essential. I do not necessarily claim that a unique set of such relationships obtains in belief contexts. I neither accept nor reject Chisholm's argument that intentionality

is a sufficient condition for identifying mental activity. But I do agree that belief sentences entail a reference to a believing subject. That admission is however open to a series of possible interpretations in terms of the relation of mental events to physical events. My argument simply requires that belief sentences speak of the behaviour of personal agents. That behaviour may be analyzed in psychological or physiological language, or by a combination of both, but that issue is one which goes beyond the scope of the justifiability of using sentences incorporating the verb 'to believe'. I happen to take the view that one cannot simply reduce language about persons and beliefs to language about physical events though one might coordinate the two sets of discourse. But that issue cannot be pursued in detail here.

Statements of belief (or of knowledge) require reference to a believing or knowing subject. Though we sometimes for brevity write Bp for someone believes that p, we should if more careful write Bap or Bcp, to say that a or c believes that p. But this reference to a knowing or believing subject is not to be overlooked. An interesting argument has been directed by Haack against Popper. She points out that Popper envisages knowledge without a knowing subject. But Popper also maintains fallibilism, and in order to characterize fallibilism one does need to refer to a subject who may be mistaken. The argument for a view of fallibilism similar but not identical to that of Haack is given earlier in this work, and it is worth noting here that this view of fallibilism also requires a reference to a knowing or believing subject. Fallibilism is quite widely

held, and it is worth noting, in passing, this argument that an objection to discussions of sentences with references to believing subjects would require either the abandonment or the reformulation of fallibilism as defined above.

In order to discuss rational belief it is necessary to discuss belief. In order to discuss belief one must first note difficulties and objections. The problem of extensionality is one such difficulty which has already been considered briefly. Whether belief is or is not dispositional is another issue which has been much debated, and must be considered next. It is natural to infer from some belief statements that these describe the actual mental state of an individual person at a particular time. Thus if I am questioning a colleague, his beliefs may become more clearly apparent as the discussion proceeds. I may reasonably infer from his replies that he is actually thinking about the topic under discussion at the time. So in these circumstances I may say to a third party, 'It is clear that N believes in Platonic abstract entities this afternoon.' If I am expressing a desire for a more limited ontology and N is directly mentioning that he believes in Platonic universals, then it is reasonable to hold that, at least in this instance, his belief is an active mental occurrence. But it is also notoriously the case that not all sets of sentences about belief can be construed in this manner. It is for example fairly evident from my behaviour that I believe that the gravitational force of this planet does not vary dramatically. I do not customarily act in such a way as to suggest that at any moment the muscular exertion normally necessary to step over a one metre interval might suddenly

deposit me six metres away. But except when writing this paragraph no such belief is consciously or at least verbally formulated in my mind. Today it is so formulated, but when I was walking to my office yesterday it was not. The language of belief is diverse. It may imply a present precisely formulated mental occurrence or it may imply a disposition to act in a certain way. There is complexity here and some attempt to explore it is necessary.

The attempt to say that belief is a mental act is open to objections. One of these objections is that we say that someone believes a certain proposition even in cases where we would also deny that he is currently entertaining that proposition.[18] The belief in the constant force of the gravity of our planet is one such example. But in at least some cases the belief in a certain proposition at least includes the mental act of entertaining that proposition. I say proposition because we do in practice assume that a single belief may be expressed in different sentences. Let us suppose that I saw a tall red haired man removing the one and only blue bicycle from the rack outside the Arts Faculty. One of my colleagues owns just such a bike, and a few minutes later he expresses dismay at the disappearance of his machine. I may say either that I have just seen a tall red haired man removing the blue bike or that the blue bike was removed a few minutes ago by a tall man with red hair. The sentences differ but the belief which gives rise to the similar utterances is more or less the same. The use of the term proposition is a convenient way of describing a situation where several slightly different

sentences express largely the same belief. I believe a proposition p, if I entertain one of several sentences which are closely related to one another in content. I am aware that some philosophers wish to manage without propositions at least for scientific purposes. But we need some device for coping with the fact that, having seen what I have just seen, I may utter any one of a number of similar sentences, each of which is likely to send my friend in hot pursuit of a red haired man with a blue bike. I use the term proposition merely to indicate what it is that I believe about the bike is capable of being expressed by one of a class of related sentences. I grant that in the longer run a more satisfactory analysis of, or replacement for, the notions of synonymity and propositions is needed, but the quest for such a replacement is likely to continue for some time.

The case of the stolen bike provides an example where my belief is a current mental state. But it is difficult to say just what it is that I am currently entertaining when I believe that a tall male red head stole the blue bike. I can express my belief by uttering one of a class of related sentences. In that case it might be preferable to use just that idiom for describing what it is that I believe. So I could propose the following as a description of my current state of entertaining the belief that p.

CB1 'I believe that p' is virtually equivalent to 'I am currently about to assert one of a class of related sentences q, r, and s...'

But this will not do for two reasons, one of which I propose

to pursue, and one of which I merely note for the present. The one which I will only note is that I have not specified just how sentences q, r, and s... are related. But more serious is my firm conviction that CB1 does not do justice to my belief. It begins to analyse my current actual belief in terms of a disposition, but the disposition as described is neither the whole story nor even essential to it. I would still believe that the bike has been taken even if its owner did not appear, and I had no reason to think it stolen, and so no pressing reason ever in the future to utter any sentence whatsoever about the bike. So CB1 might be amended to a counterfactual conditional.

CB2 'I believe that p' is in this case virtually equivalent to 'I would assert one of a class of related sentences q, r, and s ... if asked about what I have just seen.'

But even this does not do justice to the situation. In this instance there is not only a disposition, but also an actual entertainment of a proposition. I am actually entertaining some thought or other about the blue bike. My conviction is that I introspect, and am aware that I am entertaining a belief. It is not simply the case that someone else may say I have a disposition to act in a certain way. At least some beliefs are actual mental occurrences.

Now I grant that I can verbalize my belief about the bike. One could therefore argue that my belief insofar as it is verbalized requires the use of a language which is shared. But the belief about the bike may be expressed in terms of different but related sentences. So is the belief itself the

entertainment of one of these sentences, or the class of such sentences, or the proposition expressed by each of these sentences, if we are to tolerate the use of language about propositions? None of these options is without difficulty. The dispositional element in my belief can be expressed in terms of any of the sentences or by pursuit of the thief, but the actual occurrence of the belief is harder to pin down. We need to satisfy the conviction that introspection leads us to say that something more is happening than a disposition to utter some sentence or other (or to engage in some physical action of another kind).

Perhaps we could try the following strategy and invoke the mental entertaining of words or imagery. This would suggest

CB3 'I believe that p' is in this case virtually equivalent to 'I am entertaining words or images such that if asked about what I have just seen I would utter one of a class of related sentences q, r, s ...'

This does not resolve the continuing problem of specifying the relation between q, r, and s, but it does attempt to articulate the difference between a belief which is a conscious mental event and one which is a disposition to act without being a conscious occurrence. There is also the further question of whether the dispositional analysis might not itself be problematical. But at least we have made some progress.

It might also be possible to make some advance with our class of related sentences. We need to invoke such a class if we wish to say that any one of a series of similar utterances

is evidence for the same belief. Let us take a simpler example. I am now looking at a red pen and I would truthfully answer 'yes' to the question 'Did you just now consciously believe that there was a red pen on the desk?' But I might express my belief about the red pen by uttering one of several sentences. I would certainly assent to each of the following sentences: (q) 'There is a red pen on my desk', (r) 'This pen has red ink in it', (s) 'A red pen is in front of me'. (I could of course also truthfully be said to believe that there was a red pen here if I were disposed to assent to q v r v s, or just to use the pen for underlining, even if I had not consciously formulated the belief). But what is it about the set of sentences (q, r, s) which leads some people to say that if I assent to q or r or s then I believe the proposition p to the effect that there is a red pen here? Synonymity is problematical for several reasons, one of them being that the three sentences are not always substitutable salva veritate. Locutions such as 'utterers of q and r are samesayers' are open to a similar objection. Perhaps we could argue that q and r and s are substitutable salva veritate in answer to the question (c) 'Is there a red pen here?'. (They are so if the question is asked at the correct time and place). So we could call these sentences c-synonymous, or c-substitutable, if they are interchangeable as answers to a question c, (though not so to questions d or e).

The procedure outlined above is an attempt to maintain that at least some beliefs are mental occurrences, and not solely dispositions. I have tried to do so without undue reliance on the problematical supposition that there are propositions.

I merely wish to maintain that there are cases of belief as an occurrence, and that these are characterized by the entertainment of words and images. That the further specification of these words or images requires an appeal to a disposition to utter one or another of a class of c-substitutable sentences I do not see as a disadvantage. In wishing to draw attention to an element of mental activity in the holding of some beliefs, I do not aim to deny the role of dispositions in any attempt to characterize belief. I merely wish to argue that the dispositional account is not the whole story.

Those who emphasize the dispositional element in belief do so with good reason when they draw attention to certain uses of the vocabulary of belief. When we see a supposedly non-interventionist Conservative government intervening in industry, or in the affairs of Universities, we rightly infer that such a government does believe in intervention, even if we suspect that they have not fully considered the implications of their behaviour, and might even deny that they are doing what it is evident to others that they are doing. We say that they believe in intervention because we detect a disposition to intervene, at least in certain notorious instances. Indeed we could go further. We see large amounts of money made available to set up a new industry in Northern Ireland, and large amounts of money withdrawn in order to close down certain University departments. Here we have a disposition to intervene. Even if we were told by those acting in such a manner that they did not believe in Government intervention we would rightly be highly sceptical about such protestations. We do in

practice use the word 'believe' when we correctly note a disposition. If the disposition were not present we might withdraw such an inference, but we would not do so merely because those disposed to Government intervention say that they do not believe that Governments should intervene.

But other examples reveal the complexity of this issue. There are occasions when we say that a tendency to behave in a certain manner does more to reveal someone's real beliefs than their own professions of belief. Thus if I declare that I believe punctuality to be a virtue, but persistently arrive late for meetings without good reason, it becomes apparent that I do not genuinely believe that I ought to be punctual, and so presumably do not genuinely believe that punctuality is a virtue. On the other hand if someone else is persistently late despite declaring a belief in punctuality, and is evidently genuinely upset and apologetic about these persistent failures, then I would say that they believed punctuality to be a virtue. There is an interplay between profession of belief, consciousness of believing something, and acting as if one believed it. Of course one could say that a tendency to regret a failure to be punctual is a disposition which provides evidence for the persistent latecomer's genuine belief in the merits of punctuality. We are confronted with conflicting evidence because we detect contrary dispositions.

Another instance of conflicting dispositions results from the force of habit. Lee cites the example of a long standing belief that the local cobbler is dependable.[19] After a recent series of his failures I may come to believe that he is not dependable. But after an interval the old disposition may

resurface when I am questioned. When asked if he does a good job I reply at once 'Yes he's dependable'. The next day I may remember that I no longer believe this to be true. This example, adapted from Lee, may show that a disposition continues though the belief has changed. But that is not the only way to describe the new situation. A dispositionalist might reply that though the old disposition survives it has really been replaced by a new one. The belief is now a new disposition to say after due reflection that the cobbler is not dependable. But the clause 'after due reflection' concedes the case that there is more to belief than mere disposition. This seems as well established as the argument that belief is not a matter of conscious reflection alone. The problem which Lee poses is that we are obliged to say that the belief has changed even though the old disposition continues. Even if we say that the belief is now the new disposition, it is clear that we cannot simply equate beliefs and dispositions.

The issue of sincerity is also a factor. Let us suppose that there is a local politician called Slide who has a disposition to utter sentences declaring his local authority a 'nuclear free zone'. He vigorously opposes any proposal to place missiles there, he refuses to take part in civil defence exercises, and declares that his fellow citizens will be safe as long as their town is a nuclear free zone. But Slide has his own place reserved in a fall out shelter under the council chambers. Do we say that he believes that his town is safe, and believes that it is a nuclear free zone? Some of his dispositions favour such an inference. But other of his

dispositions suggest that he really does not believe that there is any such thing as a nuclear free zone, or at least that he does not really believe that his town is at all safe from nuclear attack. In this example it won't do to say that S believes p if S acts as if p is true. S acts as if p is true, and S acts as if p is not true, and we say, if we think that S is insincere in acting as if p is true, that he does not really believe p, but really believes not p. In order to decide which of a set of conflicting dispositions represents a person's beliefs we require some further specification. S believes p if S is sincerely disposed to act as if p is true.[20] But S is only sincerely disposed to act as if p is true if S believes that p is true. We have been obliged to invoke the explanandum in order to explain the explanans.

The language of belief is complex. We can certainly in some instances say that a belief may be inferred from someone's behaviour without our assuming that this belief is at that time consciously held. But at other times we need to assume that he consciously assents to p if we are to say that he really believes p. We must remember that this issue is complex when proceeding to a consideration of the rationality of belief which is the primary objective.

Belief and evidence.

In the pages which follow we will chiefly be concerned with the question of rational belief. In most cases we can proceed with an analysis which assumes that rational belief can be treated in terms of conscious assent to certain sentences. But I allow that there are many rational beliefs which do not

take the form of conscious assent to sentences or classes of sentences. I normally behave as one who believes that the force of gravity on this planet is not perceptibly variable to those walking to their offices. However in order to discuss the rationality of such a belief, it is fair to assume that a belief which is dispositional in character could become a consciously entertained sentence and the evidential support for that sentence be assessed. I propose, therefore, to discuss rational belief in terms of beliefs either in the form of sentences to which conscious assent is given, or sentences which could be, or would be, given such assent once debate or disputation arose.

The next task is to explore what is involved in saying that a certain sentence is rationally believed. In order to do that I wish to start with a proposed definition which may require subsequent alteration.

RB1 S rationally believes that p if S believes that p and S has adequate evidential support for p.

We could then, just for the purpose of recalling the development of the argument so far, compare this with the definition of knowledge which was debated earlier. There we discussed an analysis of knowledge which could be expressed as follows

K1 S knows that p if S has an undefeated justified true belief that p.

We have already noted that K1 is by no means unproblematic. It will however serve to point up one observation. In order to say that S rationally believes that p we do not need to say that p is true, nor do we need to say that the belief which S has is indefeasible (or whatever). Perhaps we might say that

rational belief is (roughly) equivalent to justified belief. I am, however, at present of the opinion that such a move would lead to greater difficulties. The problem is that the term 'justified' can be variously construed. In some contexts we say that S is justified in believing that p because S has adequate evidential support for p, but in other cases we mean that p is true, or that S has conclusive reasons for believing that p. Indeed this very shift in the meaning of the term 'justified' may well contribute to some of the confusion which arises in discussions of knowledge as justified true belief. I do not, however, wholly exclude a link between justification and evidential support. I simply maintain that it is preferable to use the language of evidential support in discussing the character of rational belief.

Another issue is the degree of adequacy which is required of the evidence. We could insist that one rationally believes that p only if there is greater support for p than for not p. This could be expressed as follows:

RB2 S rationally believes that p if S believes that p and S has greater evidential support for p than for -p.

But this gives rise to a problem about those cases where the evidence is evenly balanced. Are we then to say that it is not rational to believe that p, and not rational to believe that -p? A variety of options arises here. Perhaps the simplest is to say that in cases where the evidence is evenly balanced it is reasonable to suspend judgement. But other theorists might prefer a different option. One could say in

this case that it is equally rational to believe p or to believe not p. Or one could say that to suspend judgement is rational, but to accept either p or not p is not unreasonable. This option has the disadvantage that what is not unreasonable is differentiated from what is reasonable. Finally one might say that neither acceptance nor suspension of judgement is reasonable or unreasonable, and that either course is arational. The difficulty is set out clearly by Lehrer and others in an article[21] published in 1967.

One tactic would be to supplement RB1 with a cautious definition of adequacy. This would simply state that S had adequate evidential support for p at least when S had greater evidential support for p than for not p. This would then leave open the question of those cases where the evidence for and against p was evenly balanced. This would be my minimal position. But it would be better not to shirk the issue of evenly balanced evidence. I wish to propose that rational and reasonable be treated as equivalent and also that we regard what is not unreasonable as reasonable. I would then argue that in considering rational belief we are for the present to consider only evidential factors. In considering rational <u>behaviour</u> other issues may be relevant which do not affect the sense of the word rational in the contexts which are being considered here. I therefore favour the view that where the evidence is evenly matched the rational <u>doxastic</u> procedure is to suspend judgement.

This decision would mean that I would uphold RB2 and would wish to supplement it with RB3.

RB3 S rationally suspends judgement with regard to p if S has equal evidential support for p and for -p.

My concern with rational belief is a concern which is directed towards the relation between rational acceptance and the strength of the evidence. I readily grant that in practical affairs one may need to decide between two courses of action, but that is a different issue from the one under consideration here.

In the preceding paragraphs I have already anticipated a further point. This is that I have begun to treat belief and acceptance as roughly equivalent. Here is a further contentious matter. Lehrer in an article published in 1979 reviewed his earlier answer to the Gettier problem.[22] In the course of his reconsideration he recast his description of the conditions for knowledge and, amongst other changes, substituted the language of acceptance for the language of belief. In doing so he explicitly differentiated the two terms. His argument turns on a comparison of belief and acceptance with desire and choice. He claims that one 'may refuse to accept what one cannot help but believe, just as one may refuse to choose what one cannot help but desire.' The problem here is that we do, of course, in ordinary language use words like 'believe' and 'accept' in a variety of different ways. Even if we narrow the scope of both terms to the kind of examples considered in this chapter, there is still variation in the precise sense of the terms. But is it correct or necessary to suppose that there is such a distinction as Lehrer makes? Are there cases in which we correctly say that I cannot help believing something

but refuse to accept it? In the case of delusions I would say not. If someone psychotically believes that his phone is being personally monitored by Ronald Reagan he will not refuse to accept this. But suppose someone who is not psychotic cannot help believing that a certain former associate is not to be trusted. Is it really the case that we could also say that he refuses to accept that this person is not to be trusted? If we did come to say that, would we not then be obliged to say that he no longer maintains his former belief? Lehrer speaks of sorting through our beliefs to decide which ones receive our assent, but surely once we withhold assent from p we no longer believe that p. Of course there is the phenomenon of lingering suspicion, but that is not at all the same thing as continuing to believe that p while refusing to accept p.

Other examples include the case of believing that someone is well disposed but concluding from the evidence that he is not, yet so wishing it to be the case that he cannot help believing it. I would not call this belief but wishful thinking, or wishing to believe. One may indeed wish to believe something, but if one is confronted with evidence of its falsity, or with lack of support for its truth, one must either reject it or withhold assent. If I reject or withhold assent from a sentence then I do not believe it, even if I most passionately wish it were true. Conversely if I cannot bring myself to face the evidence[22a] that a cherished belief is false then I do continue to believe it, but I also continue to accept it, though irrationally. In these contexts I therefore reject Lehrer's distinction between belief and acceptance.

I grant that there is only a partial overlap between the language of belief and the language of acceptance. Each of the terms has a variety of uses. For instance I may believe that I am mortal, and rationally accept that I am mortal, but not emotionally accept that one day I will die. Also there are uses of the language of belief which are not directly translatable into the language of acceptance. If someone is persistently late, I may say that he does not believe that punctuality is a virtue; it is odd (though not perhaps impossible) to say that he does not accept that punctuality is a virtue. But in what follows I am chiefly concerned with those areas where the language of belief and the language of acceptance does overlap. I therefore propose to use the two terms interchangeably. I grant that the two are not always interchangeable, but I reject the view that in these contexts here they are not normally capable of being substituted for each other. In what follows I will be concerned with rational belief as rational acceptance and where the one phrase is used I would normally assent to the substitution of the other, at least for the present.

Up to this point I have deliberately oversimplified the issue of rational belief or rational acceptance. I have done so by considering only the relation between such belief and what I have loosely termed 'evidence'. But matters are of course much more complicated than that, and in due course the analysis must be taken much further. For example, what I have so far loosely classed as 'evidence' could be broken down into further factors. There is, for any hypothesis h, a class of factors which operate in its favour. Some of these are

theoretical, and some are observational, though the distinction between these does not permit a total separation between them. A higher level hypothesis will incorporate a certain amount of higher level theory, and also various degrees of lower level observation, which will themselves rely on lower level theory. The distinction between higher and lower is here purely formal and structural, and is related only to the analysis of this hypothesis in terms of supporting evidential factors. (There is further complexity here, especially if one were to uphold a non-foundationalist epistemology.) An example may clarify some of the points at issue.

Theoretical astronomers are divided between those who hold that the universe will eventually suffer 'heat death' and those who hold that there will be a 'big crunch'. On the first hypothesis there is insufficient matter in the universe for the gravitational force to prevent ever greater dispersion and loss of heat. On the second hypothesis there is sufficient matter for the currently receding galaxies and clusters of galaxies to cease their expansion and be pulled back by gravitational force in a process comparable to, but the reverse of, the earlier expansion. For the moment I will leave aside the further point that these are rival hypotheses. Each of these hypotheses is supported by theoretical and observational factors. For instance they each make certain assumptions about the amount of matter in the universe, the present distances between the galaxies and the momentum of their recession. They each try to exclude reliance on totally novel factors, but rather construct a hypothesis which draws inferences from available phenomena.

Here we have theoretical and observational factors. The phenomena are the results of observations. The estimate of the amount of matter relies on the observation of and calculation of the mass of the galaxies. But this in its turn involves further theoretical factors which make these observations possible. Some of the observations may not be direct but based on inferences from marks on a photographic plate, which are interpreted as the result of photons or other radiation striking the plate, and coming from a galaxy of a certain size and distance. Theory and observation are intertwined.

Our initial assessment was that it is rational to believe p iff there is more evidence for p than for -p. But it is now clear that the terms used here are too simple. The term 'evidence' requires much further elaboration. Some people might wish to restrict the term evidence to observed states of affairs. But this is not so simple a matter. I grant that observed states of affairs are relevant evidence. But there are other factors in evidential support. I propose therefore that we bear this distinction in mind in assessing whether or not there is greater evidential support for p than for -p. It is an issue to which we must return at a much later point in the argument.

A further factor requiring this recognition is that the same evidence may support rival hypotheses. For example evidence e_1 to e_5 may be observed. But hypotheses h and h' may both explain the evidence equally well, and be equally supported by the evidence. For example Rubens at certain

periods entrusted the execution of parts of his paintings to his assistants. Let us suppose that Philby is an art lover who comes across a previously unrecognized work in the style of Rubens. He knows enough about art to know that this painting is not a later forgery, nor by any stretch of the imagination could it be by Rembrandt or El Greco. But Philby, though he can tell the difference between a Rubens and most other painters, cannot tell the difference between a Rubens painted mostly by the master and a Rubens painted mostly by the workshop. No doubt to an expert Rubens scholar the evidence is there on the canvas. But the evidence is not accessible to Philby, or at least not recognizable by him. He has done well, as an amateur, to identify the right school. So, for Philby, the evidence he is capable of interpreting, favours two hypotheses equally well. One is h 'This is a Rubens', the other is h' 'This is from Rubens' workshop'. (We will interpret h and h' as if they exclude one another for reasons of convenience). The evidence that Philby can recognize, includes the sweeping gestures, the fine drapery, the vivid colours, the resemblance of the faces to models used by Rubens, and certain characteristic arrangements of the figures on the canvas. We will suppose that this is enough to render h more reasonable than any other hypothesis except h', and to render h' more reasonable than any other hypothesis except h. Given e_1 to e_5 we could then say it is rational for Philby to hold that h v h' is more probable than any other hypothesis. But between h and h' he cannot decide, though others might be able to do so. In this case we can adopt the procedure of considering the disjunction

h v h' as well as separately considering h and considering h'. Philby would be rational to say 'This is either a Rubens or from his workshop'. But Philby would also be rational to withhold judgement between h and h'.

I have introduced these two examples at the risk of straying from the current theme, and at the risk of partially but inadequately anticipating later steps in my argument. But I do so advisedly, as it is necessary at this point to give some indication that I am well aware that the matter of evidential support is much more complex than the initial and highly provisional analysis offered so far. But a complex analysis must proceed by stages and has to begin somewhere. My initial, if admittedly oversimplified starting point, is that rational belief is a function of degrees of evidential support. In other words that we assess the amount of support that the available evidence gives to a hypothesis and this determines whether or not it is rational to accept that hypothesis. The term evidence is here used in the wider sense.

My proposal has led from the need to explain the character of rational belief to the need to explain the notion of evidential support. Only if some adequate sense can be given to that notion will it provide any explication of the concept of rational belief. It is therefore imperative that later chapters explore further the murky notion of evidential support, or the explanans will offer no greater clarity than did the explanandum.

In these opening chapters I have argued that the elucidation of rational belief is an important part of wider epistemological

investigation. The theory of knowledge is a very broad area with certain notable problems contained within its boundaries. Not least of these is the relation between knowledge and belief. I have accepted many of the arguments of those who maintain that knowledge can be defined in terms of belief. Or rather I have argued that belief and rational belief raise a more limited set of problems than knowledge does, and that in studying them we are examining some of the components of a theory of knowledge. I have also argued that rational belief is worthy of investigation in its own right. Indeed if the argument of this chapter is correct then establishing that one rationally believes that p could often be either an acceptable substitute for, or else a first step towards, establishing that one knows that p. The problems of rational belief are a subclass of the problems of knowledge, but what is known is a subclass of what is rationally believed. By investigating rational belief we address ourselves to a crucial area within epistemology. In the next chapter I propose to focus on a particular claim about rational belief made by Basil Mitchell. This is the claim that there is a broad analogy between different classes of rational belief. Mitchell maintains that between scientific and historical and religious or metaphysical beliefs there are certain affinities, and that criteria for assessing the rationality of belief in one of these areas are related to criteria for making assessments in the others. This claim I wish to examine soon.

We may, however, use the term rational in more than one sense. So some clarification of the various usages of the term rational is required. Swinburne lists five different

levels of rationality and thus introduces a series of fine distinctions and qualifications into the assessment of rational acceptance. His rationality$_1$ is the lowest level he regards as worthy of the description rational. This merely regards someone as rational if he holds p, and his belief is probable given his inductive standards, and given his evidence. This is indeed a minimal definition of rationality. As Swinburne himself points out, a belief judged by these standards can only fail if the person who holds it has failed to recognize an inconsistency. But a more adequate definition of rationality would look at other factors. These include whether an adequate investigation of the evidence has been conducted. They also would include whether the inductive standards being used are adequate, or, if one preferred, whether the criteria for judging the evidence are adequate. As we shall see later some people use the term inductive in a narrower, some in a wider sense. Hence my qualification that what we are here concerned with is a matter of the criteria for judging the evidence not just inductive standards in the narrower sense. Swinburne's fifth, and highest, sense of rationality includes requirements similar to these more exacting ones. In pursuing the question of rational belief here, I am therefore much more concerned with whether a belief is rational in the more exacting sense, than whether it satisfies some easier test.

The various gradations which Swinburne proposes need not concern us greatly at this point. It is sufficient to note that his intermediate degrees of rationality represent cases where the believer has investigated the evidence or criticized

his criteria to an extent which seems adequate to him, but which is not to be deemed fully adequate. In other words it is a question of whether someone's belief is rational where judged by his subjective standards. I do not propose to linger over the details of this analysis. The subjective standards of an individual are clearly not as adequate a test of rationality as standards which are objectively correct, if it is indeed the case that we have access to the latter. But that is precisely one of the chief problems for any theory of rational belief. Thus, I do not propose to adopt in detail Swinburne's five-fold analysis of rationality. I do however accept that there is a distinction to be drawn between beliefs held on the basis of investigation and criteria which the believer holds to be adequate, and beliefs held on a basis which is indeed adequate. This issue is one to which we must return at a later point.

Two further related questions affect our notion of belief. One is whether there is an 'ethics of belief' the other is whether belief is or is not voluntary. These issues are related because if we hold that someone ought to believe a certain statement or ought not to believe it, then we must hold that in some sense they are able to believe or not to believe it. Now this issue is not as simple as it might seem. At one time Chisholm held that epistemic concepts are moral concepts. His views then were similar to, but not identical with, the earlier views of W.K.Clifford in his classic discussion of the ethics of belief. But Chisholm's views on the matter were subjected to severe criticism by Firth.[23] On the disputed view 'S has adequate evidence for h' actually means 'h is more worthy of

belief by S than not-h'. But this is open to the following objection. We need to be able to say that h is worthy of belief because S has adequate evidence for h. But if the two locutions are identical in meaning we are no longer able to say that the evidence is the reason for the hypothesis being worthy of belief. It would, as Firth points out, also be erroneous to say that 'this steak is tender' means 'this steak is worth eating because (amongst other things) it is tender'.

All this does not exclude there being an ethics of belief in some more qualified sense. One could, for example, debate whether one ought to hold a belief or refrain from holding it simply on evidential grounds. Let us suppose that a soldier has been lost in action. His equipment is found scattered on the battlefield, and there is no record of him having been taken prisoner by the other side. This and further evidence makes it reasonable to believe that he has been killed in action. His wife is told that he is missing presumed dead. Should she believe that he is dead? On purely evidential grounds let us suppose that there is good reason for her to believe it. But there is some slight reason for doubt. Though it is more likely that his was one of many unidentified bodies given urgent burial, and though a thorough check has been made amongst the prisoners of war, let us suppose that there is a slight chance that he escaped with injuries and is an unidentified casualty in a foreign hospital. In view of this slight chance we might well say that his wife ought on the evidence to accept the view that he is dead, but ought not to act in such a way as to exclude the possibility of his return. But in that case

we are saying that she ought not to give full assent to the belief that he is dead. There are at least two factors here, an evidential factor and a prudential factor. The weight of the evidence obliges her to take the view that he is dead. But it is also wise to recognize that the evidence is not absolutely conclusive and that to take irrevocable action on the assumption of his death would not be justifiable in the immediate future. This is not simply a case of making one's belief proportionate to the available evidence. It is a case where it seems, that at least on one description, one ought not to give full assent to p if there is a slight chance that p is false and if the chance of its being false would be a matter of considerable importance. The example seems to suggest that there is an ethic of belief and that it is not merely an ethic determined by evidential considerations.

A further example is the classic instance of the business partner who finds strong evidence that a colleague is dishonest. If he acts on the weight of the evidence he accepts that his colleague is not to be trusted. But let us suppose that this conclusion would lead to the liquidation of the firm. Given sufficient reasons of this character, we might well judge that the businessman should not hastily accept a conclusion about his partner, even if a great deal of evidence favoured it. The example can be made sharper if we suppose that Williams who made the discovery of the evidence against his colleague Evans, is a man who cannot easily disguise his feelings. If he accepts the conclusion to which the evidence points, he will display distrust and so precipitate a financial disaster. In these

circumstances we may well wish to say that he should not accept or believe the proposition that Evans is dishonest even if the evidence against Evans seems strong. But to say this entails the view that belief is not wholly involuntary, and that what one ought to believe is not decided solely by the currently available evidence.

The following lines of further investigation now present themselves. If there is a moral ground for sometimes failing to accept a proposition for which there is adequate evidence, might there not also be an epistemic ground for similar caution. This issue I wish to defer for the present but it may reappear much later in a discussion of the role of criticizability in relation to the acceptance of hypotheses. The second issue is whether belief can be to some extent under the control of the believer. The third is whether belief (or acceptance) is a matter of degree. If it is a matter of degree then it is misleading to speak simply of the alternative of accepting or not accepting a hypothesis.

In some of the cases cited above one might invoke the concept of tenacity. There are undoubtedly instances where we commend the tenacious maintenance of a hypothesis in the face of evidence which seems to refute it. Thus both religious believers and upholders of scientific theories exhort one to continue to maintain a belief which has sustained a measure of disconfirmation. In such a situation one might well put the matter thus. The new evidence obliges us to doubt a proposition which we previously believed more confidently, but other factors persuade us to maintain the belief and not to abandon

it prematurely. We are convinced that there may be further evidence which would vindicate our tenacity in maintaining our belief against the odds. Of course the two examples may not be wholly comparable. The scientist may only decide to continue to entertain an otherwise discredited hypothesis in order to carry out further tests. He may in fact suspend belief in its truth, or even doubt its truth, but judge that tenacity in entertaining and continuing to test the hypothesis is desirable so that it can be subjected to more rigorous testing. The case of the tenacious religious believer might be more closely comparable to the wife who continues to hope that her husband will return, or the businessman who continues to trust his partner in the hope that his integrity will after all be vindicated. Of course one might need to utter the warning that one needs to distinguish between those cases that are comparable to the tenacious loyalty of Penelope to Odysseus, and those that are instances of a refusal to come to terms with an unpalatable conclusion.

Nor need the issues only be discussed in negative terms. There are certain situations where one exhorts a person to accept a proposition which can only fully be vindicated after it has been accepted. The obvious example is that of the reluctant swimmer. The floundering pupil is confronted with conflicting evidence. Most of his contemporaries can swim, so it is reasonable to infer that he can learn to do so also. But all his previous attempts have ended in sudden submersion and ignominious rescue, so it is reasonable for him to believe that he cannot swim. Yet only if he can be persuaded to believe

that he will learn to swim has he much likelihood of being able to do so. In this case only when the belief is accepted can the evidence which supports the belief (and the swimmer) be procured. We tell the pupil that he ought to believe that he will be able to swim, in order to facilitate his achieving this result. But this implies that one can appropriately tell someone that they ought to believe something, and therefore that belief is at least to some extent under their control. But this is not an issue which can easily be settled.
It is of course beliefs about what is the case that we are here concerned with. It is not so difficult to say that we can tell someone that he ought not to believe in cannibalism or genocide. In matters of this kind there is an element of voluntary control. But I cannot choose whether or not to believe that today is Thursday. Given the evidence which usually alerts one to what day of the week it is, I would be acting in a perverse manner if I said that I chose to believe that today is Monday. To adapt an argument from Swinburne,[24] I believe that today is Thursday, that I am now in Edinburgh, and that David Hume lived in the eighteenth century. I cannot just decide to believe that today is Monday, that I am in California and that David Hume was a mediaeval poet. Hume himself argued[25] that belief arises from certain determinate causes and principles of which we are not masters. Swinburne takes this point further and maintains that this is a matter of logic rather than psychology. If I can change my beliefs at will then I would be aware of doing so. But I trust my beliefs and act on them because I am convinced that they are formed by factors

independent of my will. So, in outline, Swinburne argues that if I chose at will to believe that I now see a table, I would realize that this belief arose from my will and not from the presence of a table, and I would know that I had no reason to trust my belief and so would not really believe. I think that Swinburne's argument is largely correct but I do not wholly accept his claim that this is a logical matter. His argument seems to me to depend on a blend of introspection, definition of belief, and inference. It may involve inferential considerations, but is not purely a matter of logic. He is saying that we do in fact trust our beliefs, because we believe that they are formed by external factors and not by our wishes. As this is so, it would be the case that if we were to create a belief at will we would not really trust it.

But even if we accept the main point that belief is largely involuntary, or at least that it is chiefly determined by matters other than our wishes, what of the language of obligation? If we say that someone ought to believe, then we imply that they are able to do so.[26] The analogy is somewhat misleading. It is true that if we say someone ought to act justly, then we imply that it is within their control to choose to act justly. But the language of obligation in relation to belief functions somewhat differently. Here when I say that someone ought to believe something, I usually mean that there is evidence which would normally induce the belief in a reasonable person. Thus if a former colleague of mine in Bristol thinks that I can send a mutual friend by train from Edinburgh to Aberdeen in one hour, I present him with evidence, when urging him that

he ought to believe that the journey takes rather longer. I am not asking him to change his belief at will, I am drawing his attention to evidence which is highly likely to cause him to change his belief. It is therefore misleading to argue that the language of obligation necessarily implies that belief is voluntary.

It is, however, one thing to reject the idea that belief is largely voluntary, and another to conclude that it is wholly involuntary. People do differ in the conclusions they draw from evidence. If they did not there would be a singular dearth of casinos and bookmakers, and a singular uniformity of creeds. Faced with the same evidence, different scholars arrive at different conclusions. This is notoriously the case with textual criticism. Scholars are equally acquainted with the readings of the manuscripts, and the criteria for assessing them, but arrive at diametrically opposed conclusions. It is hard to think that this is the case because external factors wholly determined the result. This does not in itself prove that belief is in part voluntary, but it does lead one to conclude that it is not wholly determined by evidence.[27]

There are also the much discussed cases of attempts to induce belief or to maintain belief in the face of evidence which fails to support it adequately. This is usually discussed in terms of a would-be religious believer who wishes to induce belief, or a practising member of a religious faith who wishes to maintain belief. But an example could equally well be drawn from other creeds. A Marxist who in his student days believed fervently that the dictatorship of the proletariat

would soon lead to a socialist utopia, might gradually find this belief eroded by increasing recognition of the character of communist regimes in country after country. As instance after instance has to be dismissed as not really an example of true socialism, he begins to doubt whether the theory he once held so fervently can be sustained. Yet he wishes to believe it. He will therefore attempt to focus on those places where his hopes have not yet been completely dashed, or focus his attentions on the undeniable horrors of extreme right wing regimes in order to sustain his crumbling belief in the ultimate beneficence of communist rule. An acute observer might well say of him that he can't or won't accept that his ideological views are mistaken. Indeed she may be quite convinced he is unable to accept such an unpalatable conclusion. Moreover he himself might in a candid moment say 'I can't and won't accept that I was wrong.' So there are linguistic idioms which imply the existence of an emotional factor, as is noted by Price and Palmer.[28] The latter notes that an observer may say of the tenacious believer that he cannot afford to give up his belief, but it is very difficult to imagine the believer himself putting the matter thus. So there is a puzzle about examples in which the expression 'cannot and will not' appears.

The conjunction of 'I cannot and I will not' seems paradoxical, but is it? One can imagine a University suddenly deprived of sufficient funds to run its courses, deciding to sell some of its art treasures. The Professor of Fine Art replies to such a proposal that he cannot and he will not comply with the request to hand over items held in trust. He cannot,

because it would outrage his sense of honour, his obligations to those who made bequests, and his commitment to the value of teaching about art in the presence of original works of art. The usage 'I cannot and I will not' makes perfectly coherent sense here. It here means 'I am morally and otherwise obliged not to do this and therefore I choose not to do this.' But the case of the tenacious Marxist is different. He recognizes the force of the arguments against his ideology, yet the spell of the dream of a classless society makes him reluctant to accept the evidence that every attempt to realize the dream has failed. The evidence ought to make him modify his belief, but it does not. He is unwilling to abandon his belief and so unable to do so. In one sense his utterance is an admission that he continues to uphold an ideal despite disconfirmatory instances. The example shows both that belief is largely a function of external evidential factors but also that belief is not wholly involuntary. There is an element of will involved. One cannot sustain a belief against hopeless odds, but one can tenaciously cling to a belief despite contrary evidence.

The case of the tenacious ideologue has similarities to the case of the trusting business partner, or the case of Penelope believing against the odds that Odysseus will return. In some cases, and especially when the belief in question is after all ultimately vindicated we agree that the believer has managed to will the maintenance of the threatened belief. In other cases we tend to make comments to the effect that our ideological colleague does not really believe in his utopia any more, and that he really knows that the game is up. In

the face of these conflicting usages we must conclude that the thesis of a voluntary element in believing is at best true in certain marginal cases.

The case of Luther's famous 'I cannot and will not recant' is somewhat different from the examples given above. There was presumably the possibility of acting against his beliefs by doing what others had done before him and publicly recanting his dissent, while privately maintaining it. But Luther will not and cannot act against his conscience. He holds himself morally obliged to say publicly what he believes privately and is determined to do so. The public act is morally prescribed and personally willed. But what of the private belief? Presumably he might have said of that also, that he could not and he would not recant. Here the act of believing and the nature of the obligation differ from the case of public pronouncement. He would have maintained that given his epistemic assumptions, the evidence obliged him to the belief he then held, and that he could not change from it. He assumed that the utterances ofthe Bible took precedence over the pronouncements of the Church, and that the text of Paul's Epistles clearly enunciated the principles which he then accepted. Luther's public act was personally willed, and was morally prescribed by his beliefs. But his beliefs were the product of the evidence presented to him by the biblical texts in conjunction with his assumptions about those texts. Given the biblical evidence and Luther's assumptions, his beliefs followed.

But the case of Luther deserves further attention. A non-protestant critic would no doubt maintain that Luther's

assumptions about biblical authority were arbitrary, and that therefore there is after all a voluntary element in his belief. But this will not do. To the critic of any position it will seem that the holders of that position have made unwarranted assumptions. But this does not mean that the believers voluntarily chose to accept those assumptions in much the same way that one might choose a Cox's Orange Pippin and reject a Golden Delicious. In Luther's case the assumption about biblical authority was accepted and believed for reasons which undoubtedly seemed to him to be compelling. Such belief is not an instance of voluntarism.

I have argued that belief is largely an involuntary matter, although one must concede certain examples where a belief can be maintained or rejected despite the general tenor of the evidence. Is there then an ethic of belief? I reject the view that epistemic concepts are ethical concepts. But this does not dispose of the issue altogether. Even if, with Firth, we reject the actual identification of certain epistemic and ethical terms, there may still be an ethic of belief. The tradition exemplified by Locke and W.K.Clifford maintained that one ought to proportion one's belief to the evidence. One of the tenets of this tradition is that it is always wrong to believe anything upon insufficient evidence. Its counterpart is that one ought to believe whatever is supported by sufficient evidence. This view is now commonly termed evidentialism. (We are of course, continuing to assume that the beliefs under discussion here are beliefs about what is the case.)

I have already suggested above that there are cases where

one does deviate from strict evidentialism. If there is at least a marginal capacity to alter one's belief in special cases, then there is the possibility of believing something even though the evidence urges us to do otherwise. If there is the possibility of marginally resisting the evidence, ought one to use it or to urge others to do so? The strict evidentialist would say no. Once we introduce other considerations we are in grave danger of deserting the disinterested pursuit of truth for the sake of other values, such as, for example, loyalty to a person or an institution. This immediately conjures up dark visions of those who have been urged to deny or hide a plain but disturbing truth for the sake of the short term advantage to an institution. It is not that, however, which is at issue here. The question is rather whether there are instances in which we ought to act in such a way as to modify our own beliefs or urge others to do so. We do in practice urge that there are cases where we ought to give someone 'the benefit of the doubt'. The case of the suspect business colleague is one such instance.[29] Williams has evidence which makes it more probable than not that Evans has acted dishonestly. But the survival of the business depends on confidence, and that confidence would be severely shaken, and great harm done to many innocent parties, if Williams revealed that he suspected his colleague. But Williams is not someone who can disguise his feelings. If he does suspect his colleague, no matter how careful he is, someone will detect his change of attitude and the business be destroyed. The partnership is one of long standing, and one can therefore argue that, out of loyalty to his colleague and his employees,

Williams ought not to believe that Evans is dishonest merely on the balance of the evidence. He ought to give him the benefit of the doubt at least until the evidence became more decisive, or the grounds for suspicion turned out to be false.

I therefore grant that there are cases where non-evidential factors may urge one to demand a higher degree of evidence before accepting a conclusion than would normally be appropriate. One suggested way of modifying strict evidentialism is to say that there is a prima facie case for proportioning our belief to the evidence, rather than that we should in all circumstances do so. This seems to do justice to the example cited. On evidential grounds it is more likely that Evans is dishonest, but on utilitarian grounds the cost of coming to that conclusion at once might be considerably greater than the cost of resisting it for a while. But our main concern is with the epistemic rationality of belief rather than with estimating the utility of different courses of behaviour. We must therefore consider this particular aspect of the evidentialist argument that one ought to believe only what the evidence suggests. The case of the ability to modify belief here operates differently.

In this case the obligation is an epistemic obligation. But it still presumably implies the capacity to modify belief. A recent political appointee in a large and influential democracy is discovered to believe that Angola is in South America. A colleague first verifies that the politician has not simply made some dreadful slip of the tongue. He then urges him that he ought to and indeed must, amend his belief, as Angola is in Southern Africa. In this case the information

not only enables but obliges him to change his belief. To change one's belief in accordance with the evidence is normally possible. There is however scope for sentences urging people to amend their beliefs so as to proportion them to the evidence. Many people hold beliefs, sometimes passionate beliefs about matters of great moment, but have spent very little time evaluating the evidence already available to them, let alone gathering fresh evidence. It is easy to see that there is scope for urging people to collect additional information, and that this will enable and oblige them to change their beliefs. It is also appropriate to draw to their attention the significance of evidence already available to them and so make it possible, and indeed necessary, for them to change their beliefs. In this sense therefore there is an epistemic obligation, and there is usually the possibility of belief change, where that change is in line with the evidence. There is therefore in this more narrowly epistemic sense an ethic of belief. We may conclude our observations on this matter with the following summary. People are able to change their beliefs, but belief is only marginally under voluntary control. There is however an appropriate sense in which one may urge other people to modify their beliefs in line with the evidence adduced which one cites as providing sufficient reason for the change.

It is a characteristic both of strict, and of the qualified evidentialism advocated above, that belief should normally be proportionate to the evidence. This issue of proportionality now deserves a little attention. I argued earlier that one should believe or accept p iff p was more

probable than not-p. But this formulation is misleading. It suggests that we either believe that (accept) p or withhold p or deny p. There is compelling reason for thinking that this issue is more complex, and this can be seen even from a simplified version of the lottery paradox. I place three marbles in a bag. One is red, one green and one blue. One is drawn by a blindfolded assistant and kept out of my view. The probability that it is not the red marble is .666. The probability that it is not the green marble is the same. So is the probability that it is not the blue one. In theory then I should believe or accept each of the three propositions that the red marble is not in my assistant's hand, nor is the green one, nor is the blue one. But this is impossible. Three marbles were placed in the bag and one has been drawn. I cannot therefore accept the conjunction of three propositions each of which I have sufficient reason to accept. What is wrong?

One could readily formalize the lottery paradox as a reductio. All that is required is the principle that the lottery is fair, that statements with a given degree of probability above .5 are to be accepted, that the consequence of any accepted statement are to be accepted, and that the conjunction of accepted statements is also to be accepted. The result is that one is faced with the acceptance of a contradiction. If the lottery is fair one ticket must win, but if the conjunction principle is used, one accepts the conjunction of all the propositions that any given ticket will lose. Kyburg's lottery paradox has been much discussed and formalized in

article after article, so there is no point in merely reiterating the technicalities which are of less direct interest to the present discussion.[30]

Some lines of escape can quickly be shown to be implausible. For instance one may initially think that the problem can be reduced by raising the degree of probability required for acceptance. But even if one demanded a probability of .9 one could still be faced with a paradox in the case of a lottery with a large enough number of tickets. In the case of a fair lottery of a thousand tickets there is a probability of .999 that any ticket will lose. A threshold of .999 would in many cases be regarded as more than sufficient warrant for acceptance or rational belief. But the paradox requires a modification somewhere. One suggestion is the abandonment of the principle of conjunction in the context of rational belief. This would inhibit the move from 'I accept p' and 'I accept q' to 'I accept p and q'. But that is too drastic a solution and runs counter to our intuitions. A more satisfactory solution would prevent our accepting a conjunction of accepted statements just in those cases where that probability of the conjunction fell below our threshold of acceptance. This proposal would acknowledge that acceptance or rational belief is not an all or nothing affair. It is rather a matter of degree of confidence. The degree of confidence should match the likelihood of the statement in question being true.

In the case of lotteries and games of chance the probabilities are captured by the classical calculus. But it is much debated whether evidential probability can be assessed

by classical or even by Bayesian methods.[31] For the present we must be satisfied with a limited conclusion. Acceptance or rational belief is a matter of degrees of confidence. In the case of beliefs about matters of chance the calculation will follow classical lines, in other cases a different procedure may be appropriate. For example in the case of the conjunction of beliefs about the outcome of the lottery the conjunction will decrease in probability in accordance with the law of dependent probabilities. But in the case of evidential statements which corroborate one another we would expect the degree of confidence to rise in accordance with some increase in probability to be assigned to the conjunction of corroborative statements. But the debate over the appropriate formula for corroboration need not deflect us from the basic principle that it is degrees of confidence that are at stake.

Another example of the difficulties about conjunction is the case of the cautious professor of history. She has just completed her magnum opus on the social consequences of the Edict of Nantes. As she reads through the proofs of her book she cannot avoid reconsidering her assent to its content as well as looking for printer's errors. Statement after statement in the book receives her renewed assent as she considers the conclusions of fifteen years of research. She also renews her assent to the conclusions drawn from the arguments contained in each chapter. But if asked whether she believed that her work contained any false statements she would have to admit, like other cautious historians, that it is probable that it did. Only someone who had an unrealistically high estimate

of such a work would argue that it was unlikely to be wrong in some detail at some point or other. But to admit the likelihood of an error in these circumstances is to say that it is rational to believe each of n statements, where n is a large positive number, but not to believe the conjunction of all n statements. This may seem paradoxical but it is a conclusion that we do intuitively accept. Some might say that if each of seven inferences on matters of fact has a probability of .9 then it is reasonable to expect that one of those inferences is mistaken.[32] Of course matters will be much more complicated than this simple calculation suggests. Some of the probabilities will be dependent, some independent and some corroborative. The true calculation will therefore be somewhat different. But that reinforces rather than reduces the impact of the argument that conjunctions of accepted statements are not necessarily themselves to be accepted with the same degree of confidence as their conjuncts.

The question of rational belief is, we conclude, closely linked with the question of evidential support. But there are paradoxes which result from some of the attempts to define the relation between rational belief and evidential support. These paradoxes warn us that the matter is more complex than an initial analysis of the problem suggests. This complexity is especially located in the fact that combinations of different beliefs sometimes raise and sometimes lower the degree of confidence which is appropriate. This suggests strongly that we must pay closer attention to the differences between different types of belief. We must also pay careful

attention to the way in which conjunction and corroboration operate in this field. These will be topics to be pursued in the ensuing chapters. The first of them is the question of different classes of beliefs and the extent to which they are and are not comparable.

In this chapter we have considered problems relating to the concept of belief itself. At the outset we looked at a group of problems related to belief being a category which involves reference to mental states or events. Though there are difficulties about propositional attitudes, and although some reference to dispositions is needed, we also require a concept of belief which uses the language of conscious mental states. We then turned from analysis of the character of belief to considering the relation between belief and evidence. There I argued that the notion of evidential support is central and needs further exploration in the chapters which follow. But I also defended the view that there are factors which should dissuade us from subscribing to strict evidentialism. Evidential support is a central issue in our enquiry but not the entire story. Finally I argued that we cannot adequately discuss the question of rational belief by limiting our consideration to beliefs expressed in single sentences. Beliefs are interrelated and the evidential probability of a conjunction of beliefs has special properties which we need to consider. This last factor should encourage us to take seriously the issue of the rationality of large complexes of beliefs such as historical and metaphysical beliefs which will be considered in the chapters which follow.

Chapter 3. On the Rationality of Historical and Metaphysical Beliefs.

In the first chapter we were concerned with the relation between belief and knowledge. Then in the second chapter we moved on to the question of what belief is, and what makes it rational. That latter issue was initiated there but will continue to be discussed through the rest of this work. The work as a whole is concerned with rational belief, that is its theme. But in order to examine that topic certain more specific areas will be selected for closer attention. One of these is the area of beliefs about the historical past. Another is metaphysical belief.

I have chosen these two areas for special attention for a number of reasons. One is that much attention is currently paid to the rationality of beliefs about matters studied by the natural sciences. But these beliefs form only a section of our total set of beliefs. If we are concerned with rational belief we need, as I argued earlier, to take account of complexes of beliefs, and not just those beliefs which can be expressed in single sentences. Much attention is paid by specialists in the philosophy of science to the question of what makes beliefs in those areas rational. But the area of belief about the historical past is also an important field in which we wish our beliefs to be rational. Some writers claim with great vigour that rationality in this area functions differently and makes use of different principles and criteria. So to some of those points of difference we must direct our attention.

But metaphysical beliefs also form a distinct set. Here too there are claims and counter claims about rational procedures. Some even claim that historical and metaphysical beliefs are comparable in that the assessment of their rationality involves special principles.

I wish to examine each of these items in its own right and also the claim that there is an analogy between them.

Up to this point the discussion has largely been concerned with the rationality of beliefs expressed in single sentences. It has also chiefly been concerned with those sentences that form statements about matters chiefly determined by empirical observation. But there are also larger complexes of statements to which people claim to give rational assent. If we were to discuss the issue of rational belief exclusively in terms of single sentences, a large area of the problem of rational belief would never come into focus. This might be just tolerable if one held a strongly foundationalist view of the matter. Some people might be convinced, for example, that one only believes rationally when one is disposed to assent sincerely to a statement which either is incorrigible or which can be reliably inferred from one or more such incorrigible statements. But such a strongly foundationalist view is hotly contested. One need only point to the very different views of Chisholm and Lehrer to see that some place greater emphasis on propositions which are self justified, or justified by their relation to what is directly evident, while others emphasize the justification of propositions by the relations they bear to each other.[33] Foundationalists face the difficulty that there are very few incorrigible propositions, and that directly evident propositions are a limited class. It is also very difficult to provide a satisfactory account of how one can rationally defend many statements that we do in practice accept, by reliable inference from so limited a base. The issue is usually discussed in terms of knowledge, and whether knowledge can be

justified only by a chain of inferences leading back to what is evident or 'given'. It is worth reiterating a point made earlier that the requirements for rational belief are fewer and less stringent than the requirements for knowledge. But this does not resolve the question of whether rational belief ultimately requires an appeal to basic beliefs, or whether all that can be done is to test part of our framework of beliefs against the rest. For the moment, however, I propose to proceed on the assumption that we must at least do the latter. We must at least test various subsets of our belief system against other of our beliefs to see if there are inconsistencies. A minimal requirement of rational belief would be that subsets of our beliefs should be free of internal inconsistency. They should also be free of inconsistency with other subsets of our framework of belief.

The question of larger complexes of beliefs is central to this enquiry. This is not simply because I wish to examine such complexes, and the question of the nature of belief in simpler sentences is a necessary preliminary. It is also because the supposedly simpler sentences are only simpler in certain respects. They are grammatically brief and convenient, they use fewer words and concepts, and can be formalized with fewer variables. But if it is indeed the case that there are very few basic propositions, and that it is not at all easy to show how non-basic propositions are inferred from basic propositions, then we must recognize not just the desirability, but the necessity of considering the rationality of larger complexes of beliefs.

I propose therefore to take examples from two areas which raise very considerable difficulties of the character indicated

above. One of these is the rationality of beliefs about matters of history, the other is the question of the rationality of metaphysical beliefs. The term metaphysical has various uses but I propose to include within the scope of this argument those beliefs of a metaphysical character which are of maximal scope. But though I wish to widen the scope of the investigation in this way it will be necessary to control it in another. In order to do this I propose to take the arguments used by Basil Mitchell in his book The Justification of Religious Belief and those used by R.G.Swinburne in his book The Existence of God as recent examples of the defence of classical religious metaphysics. The argument put forward by Mitchell is especially interesting because of his claim that there is a comparison to be made between historical reasoning and what he calls 'the claims that are made for the rationality of large-scale metaphysical systems'.[34] The term 'large-scale metaphysical systems' is perhaps not the most elegant or precise of expressions, but I will use it as a convenient way of referring to what Mitchell wishes to justify, and in order to subject his argument to further analysis and critical scrutiny. The choice of Swinburne's work also deserves a brief comment. This work is of special interest not only because it examines metaphysical beliefs, but because it does so with the apparatus of a neo-Bayesian theory of rational belief. In considering his arguments it is possible both to examine closely his way of arguing that theistic belief is rational, and to scrutinize his assumption that a neo-Bayesian method is the appropriate way to assess rational belief.

I propose to discuss Mitchell's argument first and that of Swinburne later. Mitchell's work is of special interest because

it discusses metaphysical beliefs in the context of a general theory about the justification of other kinds of beliefs. He considers the sort of supporting arguments which are used to defend the rationality of beliefs of a historical character, -- and also draws on work in the philosophy of science concerning the rationality of scientific hypotheses. Indeed his discussion contains many valuable points of reference to the work of philosophers of science such as Kuhn and Lakatos. This means that Mitchell's book provides a useful test case for theories of rational belief. It is of course specifically addressed to the question of the justification of religious belief. But it also incorporates many arguments concerned with the nature of the rationality of beliefs of many classes. Those whose main interest in the study of rational belief is the search for a rationally articulated view of religious belief will not require much further reason for regarding Mitchell's book as a suitable choice for scrutiny and investigation. I would only add as additional ground for this choice that Mitchell attempts throughout his book to treat the discussion of religious belief as the investigation of a cumulative case. He recognizes that it cannot be a matter of demonstrable proof (or disproof), but he also opposes the irrationality of accepting or rejecting such belief without rational argument.[35] But it is just Mitchell's contention that the justification of religious belief can be seen as the construction of a cumulative case which should be of interest also to those whose approach to the philosophy of rational belief is not necessarily concerned with the viability of religious belief. Mitchell again provides a good subject for scrutiny because it is central to his argument that there is a basic similarity in the construction of a cumulative rational argument

in several different disciplines. His claim is that the cumulative case which the theist puts forward is comparable to the cumulative case made for their conclusions by historians, or literary critics or the upholders of scientific hypotheses. Thus his position ought to be of interest both to those specifically interested in the rationality of theistic belief, and also to those interested in whether or not there are common elements in the rational defence of theories in disciplines of such widely differing character as history, literary criticism, and the natural sciences. One might accept some of Mitchell's case where it relates to disciplines other than theology, or one might assent to all of his case, or one might argue that he has underestimated the differences between a rational case in historical study and a rational case in physics and that a fortiori the differences between these disciplines and the arguments of theistic metaphysics is even greater. But at this point it is too early to offer even a hint as to which of these options is to be preferred. It is enough to claim that if we wish to examine in some detail an argument that the rational defence of complex systems of beliefs is comparable in different disciplines then Basil Mitchell's book provides an excellent example on which to work.

For the reasons just given a consideration of Mitchell's argument should be of interest both to those concerned generally with the philosophy of rational belief, and to those more specifically concerned with the question as to whether there can be rational theistic belief. The crucial contention which he makes is contained in the following passage:

> 'I shall endeavour to show that in fields other than theology we commonly, and justifiably, make use of arguments other

than those of proof or strict probability; and that, typically, theological arguments are of this kind.' [36]

He goes on to say that what has been taken to be a series of failures when treated as attempts at 'purely deductive or inductive argument' could well be better understood as contributions to a cumulative case. He argues that on this view theists claim that their view makes 'better sense' of all the evidence available than any of the rival theories.

Now in this initial statement by Mitchell it is worth noting carefully his use of terminology. In some instances he is using words in a sense different from that which I shall myself be using and so some analysis of these differences is imperative at the outset. By proof he seems to mean the use of a deductively valid argument and his statement concedes that in his view both in theology and in other fields there are cumulative arguments of a different character from this. But Mitchell also asserts that these are not arguments involving 'strict probability' and that they are 'not inductive'. This terminology is more problematical. If he means that such arguments do not present precisely quantifiable probabilities, or that they are not the kind of argument leading to mathematical probabilities based on reliably computed frequencies, then I agree with him. But I would not wish to exclude the term probability altogether. Indeed I suspect that Mitchell would agree with this point as he merely contends that the arguments in question do not involve 'strict probablity'. I shall later argue that epistemic probablity is not necessarily to be identified with the mathematical probability of games of chance or of calculations of frequencies. I would claim that there are arguments of a

cumulative character which raise the epistemic probablity of a conclusion, or which increase the evidential support for a theory. Yet I would also claim that this increase (or decrease) of support is not necessarily to be calculated in terms of mathematical probability either neo-Bayesian or classical. But this raises complex issues which will reappear much later in the discussion.

There is one further area of disagreement over terminology before it is possible to proceed. This is that Mitchell disavows the term inductive for the kind of cumulative case which he has in mind. Once again if he means by inductive the kind of enumerative induction which sometimes figures prominently in discussions of induction then I agree with him. But the terms inductive and induction can be used more widely. There is also eliminative induction, and the term induction is used more loosely still in some contexts to include any argument which raises the epistemic probability of a theory or a conclusion.[37] I therefore agree with Mitchell that the arguments under consideration are not arguments exclusively involving deductive inference or mathematical probability or enumerative induction, but I would add that they may involve epistemic probability or inductive considerations of a more general kind.

Several issues are raised by the claim that in history and in literary criticism and in theology and in other disciplines there is argumentation of a cumulative kind. The first of these to deserve attention is the more detailed exploration of just how such arguments proceed in disciplines other than those involving metaphysical theology. In order to do this I propose to focus on the case of historical argumentation, though being ready to draw on supporting instances from law and from literary interpretation.

The relevant part of Mitchell's larger case is that there is an analogy between historical and metaphysical interpretation. I shall, for convenience, refer to this central section of his argument as 'Mitchell's analogy'.[38] In fact he argues for a series of analogies. He supplements his comparison of rational argument in history and in metaphysics with further analogies from literary interpretation and from natural science as viewed by Kuhn. It is however his argument from historical study that I wish to look at next.

Mitchell offers two examples. The first concerns explorers who presumably have some archaeological skills. They discover a large hole in the ground and a series of smaller holes nearby. This leads one of the explorers to infer the need for an explanation other than that they are natural depressions. Next in a neighbouring cave they find a papyrus containing fragments of the plan of a building. The first explorer now proposes a theory that the large hole took the centre post of a wooden building and that the smaller holes took the other posts. In this example involving archaeology and papyrology there is indeed an argument involving a simple form of a cumulative case. The evidence from the site raises questions as to whether the holes are due to natural causes or to human activity. Different theories will explain the evidence. A regularity about the holes in the ground seems to one explorer to raise the probability that the holes formed part of some human construction. (I am here amplifying Mitchell's argument). But widely differing interpretations of the data are still possible. The further discovery of the papyrus with a building plan in it adds to the now cumulative case for the place having been the site of a human habitation. Further confirmation

is derived from matching the details of the plan with the distribution of holes in the ground. But there is some discrepancy which provides a measure of disconfirmation, especially in the view of the second explorer. At this point the first explorer provides an additional explanation for the features mentioned in the plan which cannot be traced on the site.

Mitchell's basic point is that the argument is cumulative and that several features of the case cooperate in favour of the conclusion. I have already made some additional comments and will now add further ones. The final move by the first explorer is an example of an ad hoc supporting hypothesis. It does not depend on new evidence. It is a fresh piece of supposition or hypothesis in order to account for difficulties in a case which has elements favouring a suggested conclusion. This is certainly a common feature of such arguments but it is important to draw a clear distinction between such an ad hoc additional hypothesis and the inferences which had already been made on the basis of the evidence then available. The gratuitous character of the supplementary ad hoc hypothesis renders the whole case vulnerable. It is gratuitous in that this particular part of the case lacks evidential support. However the case as a whole is plausible and this additional element is needed to provide an adequate overall theory. However it is a weak point in the case. At least two defences can be made at this point. One can either counter attack by claiming that rival theories contain at least as large an element of ad hoc argumentation. This is an adequate though not an ideal defence. A better defence is to treat the supplementary hypothesis as a prediction requiring a further search for evidence. Let us suppose that the papyrus fragments

envisaged a building with more posts than can be traced on the site. Rather than just speculate that the extra posts would have been in a part of the building likely to disappear first and leave fewer traces, it would be better to bring in more refined techniques to examine the area where minute traces of the extra posts might be found. For example it might be possible with very careful chemical analysis to find traces of iron nails in the soil at just those points where the wood of the missing posts had completely vanished and the holes had gradually become filled in and overgrown. The ad hoc hypothesis would then be equivalent to a prediction which had been confirmed by the discovery of further evidence.

One could make some further observations on the character of the cumulative case. The existence of one or two holes in the ground is in itself patient of other very convincing explanations. But if the holes are themselves square, and if the distribution of holes is regular then each of these factors raises the probability that we are dealing with a human construction. This inference is warranted by the following considerations. On the one hand though holes in the ground can be due to natural causes or to the burrowing of animals these are rarely either square in themselves or arranged in geometrical patterns. Human constructions do regularly have such features, and it is otherwise observable that such features have elsewhere turned out to be associated with other evidence for human habitation and construction. There is therefore an informal appeal to frequency or regularity which underlies the claim that the data increase the evidential support for the theory. I hasten to add that neither I nor

Mitchell are here envisaging an analogy between the procedure of the archeologists and the argument for design. (Indeed the feature of frequency or regularity, to which I have drawn attention, is in Hume's discussion used against such an argument). In Mitchell's scheme he uses the illustration from historical archaeology as a parable (or allegory) in which the large hole represents the 'intellectual demand for ultimate explanation to which natural theology appeals' and the smaller holes represent private religious experiences of sin, grace etc. But I wish for the moment to remain with the contention that historical arguments often consist of a cumulative case which relies not on deductive inference nor on enumerative induction, but on separate pieces of evidence which when taken together raise the evidential support for a theory. In any case I think that Mitchell's use of the parable to defend theistic metaphysics is open to criticism. He says that the fragmentary plan represents the concepts of Christian revelation. But here the analogy is not so close. In the case of the archaeologists the plan is additional supporting evidence because it provides independent testimony to the existence of a human habitation on the site. But in the case of metaphysics the concepts of the biblical texts are not so much independent evidence freshly acquired, as existing texts already available, containing theories based on the same experiences of sin, grace etc. as are now being cited in favour of what are essentially similar theories. Indeed Mitchell's designation of the religious belief he is wishing to justify as 'traditional' Christian theism makes that point abundantly clear. But, in fairness, one must recognize that all parables have their limitations and Mitchell's case is in

general sound as examined so far, even if I would resist some details in it. I grant his general point that in each case a cumulative case is involved. But can Mitchell's analogy cope with further analysis?

Mitchell's second more specifically historical example is taken from the debate amongst historians over the events leading up to Caesar's crossing of the Rubicon in 49 B.C. He cites an article by P.J.Cuff (in Historia 1958) as an example of how a historian may make a fresh interpretation of a limited amount of documentary evidence. The issue was not so much the legality of Caesar's crossing of the Rubicon, or rather its illegality. The crucial dispute is over the duration of Caesar's command in Gaul. Caesar was in difficulties, because, if his command of his provinces and his legions expired before he could be elected consul, he was liable to face prosecution. Cuff argued that it is impossible to harmonize all the evidence as to the duration of Caesar's command. He then inferred that it is an error to try to make all the pieces of the jig-saw puzzle fit together. His solution is different. He maintains that the exact duration of Caesar's command was ambiguous at the time, and that this ambiguity over the terminal date gradually became apparent and led to two rival views one held by Caesar's party and the other by Pompey's.

In this instance there is an element of assessing the likelihood of different interpretations of the different pieces of evidence. But the more significant factor here is that we see a new solution being propounded which (in Mitchell's terms) 'puts the whole problem in a new perspective'. A new theory is proposed whose persuasive force consists in the capacity to

reintegrate all the existing pieces of evidence into a more convincing pattern. Now again I grant Mitchell's basic point. In historical study this is a common and appropriate way to proceed. It is indeed the case that historians come up with theories which commend themselves by providing a more satisfactory account of the existing data even though in such cases fresh data may neither be provided nor indeed be available.

Mitchell cautiously, and in my view wisely, refrains from making an immediate move to justify metaphysical beliefs by a direct appeal to this feature of historical method, but one can see the drift of his argument. For the moment I wish to remain with the proposed criterion for assessing the rationality of the historical conclusion. Here we are dealing with a complex set of beliefs. These include beliefs about the date, the text and the significance of Roman legislation and about the words and actions of people such as Cicero, Pompey and Caesar. The conclusion is that the Lex Vatinia was ambiguous but that its ambiguity only became apparent as the end of Caesar's command came nearer. This belief, or inference or conclusion then rests on a complex set of premises comprising other beliefs, and is arrived at by a series of inferential steps of a non-deductive kind. Of these the most interesting is the suggestion that the confusion over the terminal date of Caesar's command was inherent in the original situation and that this then explains why 'the jig-saw pieces did not fit'.

I now wish to make some more critical comments on Mitchell's use of the example from Cuff. I will for the sake of convenience accept that Mitchell's account of Cuff's article is adequate for

our purposes. One factor which commends a conclusion such as is here offered is that the new theory accounts for and explains the alleged 'lack of fit', and that it does so more economically than is the case with rival theories. Let us for the moment assume that this is the case. The other more critical factor is this. Our willingness to accept the theory is at least in part dependent on our experience or awareness of other similar events. We know from our own experience that a subsequent dispute may often be due to the presence of a latent ambiguity in a text. It happens for instance in legislation over overseas students. A series of governments raises the fees it charges to overseas students by an amount substantially in excess of the amount it charges home students. Only afterwards is it realized that until now it has counted as home students those who have been taking school courses in the country for three years prior to their application to go to University. Only later does the ambiguity become apparent and fresh legislation is introduced to stop what is now castigated as a 'loophole'. We are not concerned here with the ethics of government, but with the tendency for a latent ambiguity to be tolerated until it gives rise to subsequent controversy. Because we are aware of this tendency it operates as an additional factor commending the application of such an explanation to events in the more distant past. There is in such a procedure an implicit appeal to regularity or to frequency. What is believed to have happened in the case of Julius Caesar is in part supported by an implicit appeal to what is experienced in other situations, notably in one's own time. But if this analysis is correct then it renders Mitchell's analogy vulnerable to an objection similar

to one long since deployed against other defences of the rationality of metaphysical systems. Insofar as the latter are of universal scope and are offered as explanations of everything, they cannot be supported by an implicit (or indeed any) appeal to their being an instance of the kind of thing which happens regularly. They can only be defended in this manner by an argument which claims that what is true of parts of the whole may be true of the totality. It is at this point that Mitchell shifts his ground and moves from an analogy with historical argumentation to an analogy between metaphysics and other wide ranging theories. But here we must for the moment leave Mitchell's subsequent moves in order to give further consideration to the case of beliefs about history.

One interesting observation on the analogy between metaphysical and historical beliefs is the following. It can justifiably be claimed that there is some such analogy at least in part, if only for the reason that Mitchell's 'traditional Christian theism' is in fact a system which includes many historical statements. It can also be argued that many of the more grandiose historical systems either border upon, or actually are metaphysical systems. Let us consider each of these rather different points in turn.

The first is that contained within traditional Christian theistic metaphysics are a number of beliefs which would readily be classed as historical rather than metaphysical. If we are here arguing for a distinction between historical statements which are included in a metaphysical system, and those parts of that system which are clearly metaphysical some care is needed. I think that the distinction is best demonstrated by attempting to show that there are at least two different classes of belief

and that the two different sets of statements offered below are then approximately classified as 'historical' and 'metaphysical'. To begin with let us simply distinguish between beliefs of type A and beliefs of type C. Let us call them 'A' statements and 'C' statements.

With this simple, and as yet unexplored distinction in mind let us try classifying some statements which presumably form part of Mitchell's traditional Christian theism. I would suggest that we begin by classifying the following as 'A' statements.

A1. Four of the disciples of Jesus were Galilean fishermen.

A2. Jesus at one time lived in a town called Nazareth.

Now both of these statements A1 and A2 are comparable to other straight historical statements such as that some of Socrates' followers were Athenian citizens or that Plato at one time visited Sicily. Only if we were to take the name Jesus as including connotations of more than human status would there be ground for objecting to such a classification. I propose, however, for the purpose of argument to rule that the simple name Jesus be treated as the designation of the historical human figure and to insist that where statements are to be taken as alluding to those suprahuman characteristics which orthodox Christianity ascribes to its founder we will use one of the titles such as Christ or Lord to make this clear. If this point is accepted then A1 and A2 are as acceptable as historical statements as the comparable statements about Socrates and Plato. Their classification as statements of a historical character is without prejudice to their being held to be true or false historical statements though I happen to believe both that they are true

historical statements and that they can rationally be held to be such.

We could now proceed to compile a further list of such historical statements.

A3 Jesus taught in parables.

A4 Jesus taught non-retaliation.

A5 Jesus was executed by crucifixion on the orders of Pontius Pilate.

In each of these instances one of the component elements in Mitchell's traditional theistic system is not just like a historical statement, or capable of being supported by arguments analogous to those used in historical debate. It is a historical statement, and it can be provided with evidential support by exactly the same sort of arguments involving literary sources and archaeological data as is the case with other historical statements. The rationality of belief about such matters is therefore to be argued in the same way as is the rationality of the belief that Caesar's command in Gaul was due to end in 50B.C. or 49B.C. or that he crossed the Rubicon in 49 B.C.. But with other statements this is not so obviously the case and indeed it may not be the case.

The clearest examples of sentences which belong to a different category are those expressing beliefs of type C. I propose initially to mark out this category by providing examples which come from the tradition to which Mitchell belongs.

C1 God is the creator of the universe.

C2 God effected the redemption of humanity through the cross.

C3 God will judge every human being at the last judgement.

These are statements of traditional theistic metaphysical belief which are different in character from the set of statements of

type A listed above. Whereas the first set of statements make assertions about past historical events these C statements are concerned primarily with matters which are of larger scope. They may contain historical allusions but they are of greater scope than sentences of the first type. Thus C2 refers to the past historical event of the crucifixion of Jesus but it does so in a way which exceeds the bounds of the first category. This sentence speaks of God. Now I grant that there may be revisionist interpreters of Christian theism who might attempt to argue that sentences about God are not after all sentences which aim at reference to a supernatural being. But Mitchell at any rate is no such theistic revisionist. For him, if I understand him correctly, such sentences are indeed intended to refer to the activity of a supernatural being who is all powerful, beneficent and omniscient and who in other respects matches the descriptions of traditional theistic metaphysics.

Now if Mitchell's analogy is to hold it is essential that one show not just that statements of type A can be defended by arguments similar to those used in historical study, but that this is also the case with statements of type C. The reason for this insistence should be obvious. It is, I maintain, the case that sentences of type A are not only comparable to historical statements but in fact are such statements. In other words I am arguing, and I think it would be widely accepted (at least by those whose theism is traditional) that theistic metaphysics is a complex system which includes different types of belief. Some of these are beliefs of a historical character. Others are beliefs involving specifically metaphysical motifs. I am of course using the term 'metaphysical' here in a special but I

think widely used sense. At the very least I would argue that I am using the term as Mitchell does when he is describing what he calls metaphysical systems of maximal scope. I grant that the term 'metaphysical' may have other uses but these are not in play at this point where I need to make use of Mitchell's terminology if I am to discuss his argument.[39]

Once it is conceded that A statements and C statements can be distinguished, the next step in my argument can be taken. This is that only if there is an analogy between the rational defence of C statements and the rational defence of historical judgements is Mitchell's case of any real help to the theistic metaphysician. It is of course the case that A statements can be defended rationally by the normal processes of historical investigation. I grant this precisely because A statements are indeed historical statements. But the distinctive and controversial feature of belief systems such as traditional theistic metaphysics is that they contain statements whose character exceeds the limits of normal historical investigation. Now, of course, the A statements must be defended if the C statements are to be defended. But the A statements do not entail the C statements. Nor do they offer more than limited inferential support to the C statements. Why is this? I do not think anyone will seriously maintain that metaphysical conclusions can be obtained deductively from A statements. But can they be given evidential support from the A statements? This is a much more interesting question and one which is entirely in keeping, if I do him justice, with Mitchell's case. I would argue that the A statements may provide limited support of this kind.

As an example of the limited support which I think may justifiably be claimed let us take the case of certain of the A statements and their relation to the claim of the following C statement.

C4 Divine deliverance is effected through the cross of Christ.

Now such a C statement requires the truth of at least the following A statements.

A6 There was a historical person called Jesus.

A7 His character was such as to have impressed many of his contemporaries favourably.

A8 He was executed on a Roman cross on the orders of Pontius Pilate.

Unless A6 - 8 are true our C statement is difficult to sustain. The C statement entails certain historical statements, and unless those are shown to be true or at least shown to be historically probable, then the C statement suffers disconfirmation. But A6 - 8 can be satisfactorily defended. Readers of a mainly philosophical bent will have to accept on trust my statement that this is a view which would be held by almost all of the serious historical investigators of the subject.[40] But it is also not difficult to maintain that such statements only give limited evidential support to the much larger claim of C4 that divine deliverance is effected through the cross of Christ. But the matter cannot be disposed of quite so swiftly.

One could add to A6, 7 and 8 further statements which would also belong to category A. These might be more difficult to defend historically but I do not think them unduly controversial.

One could suggest the following:

A9 Many of the disciples of Jesus displayed considerable growth of moral character as a result of their acquaintance with him.

A10 After his death similar change of character was noticed amongst people who were told about him but had not met him.

A11 Such changes also took place in people several centuries later in similar circumstances.

Now again I do not claim that A9 - 11 prove the truth of C4 or even make it more probable than improbable. I do grant however that C4 can be supported by beliefs such as A9 - 11 and that C4 is more likely if A9 - 11 are true than if no such supporting evidence were available. In other words I do maintain that the relationship between the historical and the metaphysical elements in traditional theism is more complex than either some of its defenders or some of its opponents seem to think. This point is in agreement with Mitchell's claim that a cumulative case is involved here. It is also in harmony with his view that the case is like the kind of cumulative case involved in historical study. But the likeness extends only a certain distance. A9 - 11 are comparable to the kind of arguments one might use to defend the statement that Socrates continued to have a morally beneficial influence on people long after his death. But C4 is making a claim of much greater scope than the statement about Socrates. The problem is that the cumulative case we have outlined supports C4 only in so far as it is comparable to the statement about Socrates. In other words it only supports those elements in C4 which are of a A type,

and does not contribute to the support of just those elements in C4 which are of a C type.

This last judgement may perhaps be deemed as not wholly warranted. It should perhaps be reformulated. Only if one can show that the factors supporting C4 are of greater scope than those supporting the statement about Socrates, is the greater metaphysical content of C4 provided with evidential support. On that issue I suspect that it would be difficult to proceed further than we have done at this point. But before leaving this particular example it is important to note that Mitchell does not confine his defence to examples from history, he also claims that the defence of large-scale metaphysical systems is in other respects comparable to the debate over the merits of rival scientific paradigms.[41] But for the moment we must stay with the arguments about history.

Because our task is not simply to engage in debate with Mitchell, but rather to explore the character of rational belief in a series of different contexts, another related issue must now be considered. This is the question of a class of B statements which are neither as purely historical as the A set nor as unambiguously metaphysical as the C set. Again these are best illustrated by example.

B1 Paul believed that Jesus reconciled humanity to God.

B2 Paul's letters speak of Jesus as Son of God.

B3 Luther maintained that one is justified by faith alone.

B4 The Council of Trent upheld judgement by works.

These form a kind of hybrid group. One may recognize that B statements include allusions to metaphysical concepts. But the truth or falsity of B1 - 4 does not at first sight seem to depend on the truth or falsity of the metaphysical concepts.

The B statements express beliefs about beliefs. They are historical beliefs about the metaphysical opinions of certain historical personages. They are sometimes a source of great confusion to unwary students who will occasionally discuss whether or not Paul or Luther held a particular metaphysical view with all the fervour that one might expect from one discussing the merits of the substantive belief rather than the belief about the belief.

The category of B statements needs to be mentioned, but it should not deflect us. I would argue that the truth or falsity of B statements is determined by exactly the same kind of investigation that one devotes to A statements, and that B statements should not be confused with C statements. The nature and character of Paul's beliefs about metaphysical matters is decided by the same procedure as the nature and character of Paul's opinions about how many times he had visited Jerusalem or Galatia. The decision depends on the normal historical procedure for evaluating which texts were indeed written by Paul, and what view such texts do in fact contain. The procedure is entirely similar to the procedure one would adopt when asking whether Caesar believed that his command in Gaul expired in 50 B.C. or in 49 B.C..

But the B statements are not simply assimilable to the A statements. They do need to be differentiated. The difference is not so much one relating to the historical arguments one would adduce in their defence. The difference consists in the fact that they are historical statements about the metaphysical beliefs of particular people. They therefore include metaphysical

concepts. Is it possible to identify further what differentiates them? I think it is. Let us take B2. In one sense all that is required in order to confirm B2 is to find the appropriate form of words in a letter which can be held to be one of the genuine Pauline epistles. This can be done as the relevant phrase occurs in each of the four major undisputed letters. If B2 is construed as saying that Paul's letters used the form of words in question, then the procedure for confirming B2 is strongly analogous to the procedure for confirming the statement that Caesar believed that his command in Gaul extended to 49 B.C.. But the matter is not quite so simple. This becomes apparent if we ask a further question. Let us suppose that Basil Mitchell and, say, D.Z.Phillips differ over the meaning of the belief that Jesus is the Son of God. If we were now to ask whether Paul believed that Jesus was the Son of God in the sense upheld by Mitchell or whether he believed it in the sense upheld by D.Z.Phillips we would be driven towards the conclusion that B2 is more complex than our initial proposal suggested.

There is therefore a secondary complexity about beliefs about beliefs. In one sense my belief that A believes that p is simply to be confirmed or disconfirmed by arguments as to whether A is (or in the example was) sincerely disposed to assert that p. In another sense the different interpretations of what it means for someone to believe that p are relevant to the discussion of whether it is (or was) the case that A believes that p.

Although I think that this analysis of the class of B statements does point to further complexity, I do not think

that the discovery of this further complexity necessarily vitiates Mitchell's case. He argues for an analogy between metaphysical beliefs and historical beliefs, but also extends the analogy to include literary interpretation. The latter may well involve just the class of B statements to which I have drawn attention. If we are discussing the meaning of stanza from the metaphysical poetry of Donne, or if we are discussing the antimetaphysical polemic of Philo in Hume's Dialogues then similar issues arise. However I do think that the issue is more complex than Mitchell acknowledges. The resolution of certain controversies in the interpretation of literature may turn on the precise metaphysical connotation of a sentence, or a line, or a stanza of a poem. In that case the analogy between the defence of a metaphysical system, and the defence of a belief about the opinions of a historical or literary figure may be weakened. In at least some cases the point at issue in the discussion of a historical or literary matter may itself not just be like a metaphysical question it may be a metaphysical question. We must therefore be careful to note that while the interpretation of literature does indeed involve just the kind of cumulative argument which Mitchell claims, there are sometimes hidden complexities in the interpretation of lines such as the ones he cites

'Tis madness to resist or blame
The force of angry Heaven's flame.

But I do not wish to exaggerate this point. The more important distinction is between groups A and C.

The analysis of B statements poses the difficulty that there are statements which we might sometimes regard as if

they were A statements, but which when we ask a certain type of question about them turn out to have some of the characteristics of C statements. In considering an analogy which is attempting to justify metaphysical systems by arguments accepted as normal in historical study we must be careful to make sure that we are comparing C statements with A statements, and avoid the potential confusion which B statements might contribute. This is not to say that B statements are in themselves unduly problematical. It is merely the case that they are liable to introduce unnecessary confusion into the consideration of Mitchell's analogy.

But even when we limit ourselves to considering the claim that one can justify C statements by the sort of cumulative argument one uses to justify A statements we still find that the issues are complex. The problem I now wish to consider is this. I proposed the distinction when it became apparent that some of the statements contained in a metaphysical system either are or contain historical statements. Thus within the set of metaphysical beliefs which Mitchell is defending we find historical sentences about Jesus and Paul as well as metaphysical statements about salvation and the divinity of Christ. It is possible to extract pure A statements. Thus one can identify some of the historical elements in the set of theistic metaphysical beliefs. But it is very difficult, perhaps impossible to produce a complete set of pure C statements. Many of the C statements will continue to contain explicitly or implicitly some A element. Thus sentences which describe Christ as redeemer will almost always contain an explicit or implicit element which alludes to the historical fact of the crucifixion

of Jesus. We can formulate this element in terms of an A statement of purely historical character, but it is very difficult, perhaps impossible, to quote a C statement in which the Christian belief about salvation is expressed in purely metaphysical terms.

Indeed the very quest for pure C statements may do violence to the character of the system of beliefs which we are investigating. There may be some pure C statements but many statements which are classed in group C also contain historical elements. 'God is immortal and invisible' is presumably a statement which comes as near to being a pure C statement as any. But 'Christ died for the sins of the world' cannot readily, or perhaps in any way, be reformulated to yield a sentence which lacks a historical as well as a metaphysical element. Indeed it would do violence to the character of the belief we are investigating to make such an attempt.

This means that there is a further complexity to Mitchell's analogy. I am still disposed to persevere with it as a potentially illuminating approach, but I think that it needs to be further qualified. If his claim is to remain a significant claim then it must be saying that the justification of the C elements in the C statements of belief depends on the kind of cumulative argument used in the justification of purely historical A statements. We can then ask if this claim is to be accepted.

There is however another line of argument on the whole question which needs to be considered. Mitchell's analogy claims that systems of metaphysical belief can be justified by the kind of cumulative argument used in historical study. Now in order to set up such a comparison it is very important

that the examples do in fact conform to this model. This is certainly the case where the duration of Caesar's command in Gaul is taken as a model for the defence of a conclusion by cumulative argument. Indeed it also illustrates the further point that sometimes, in such discussions of past historical states of affairs, a new theory can be propounded which views the existing evidence in a different way. But we need to be careful in the choice of examples. This is because some sets of beliefs about history may themselves be 'large scale metaphysical systems' (to quote Mitchell's phrase). There have been in the past, and may well still be, those who put forward all embracing schemata for the interpretation of 'history'. Very often such grand schemes receive rather abrupt treatment from those whose view of the academic study of history persuades them that they should focus on specific events within the more limited period of a century, a generation, or even the span of a few days in the life of one of history's more notorious villains. But there are works of much larger scope. We must therefore be careful lest the analogy between metaphysics and history be expressed in terms that are vague or slippery.

Examples of beliefs about history which are themselves metaphysical in character would be Marxist views of history, or idealist or physicalist theories of history. In these cases there would certainly be sets of more purely historical sentences, but also sentences involving a good deal of high level theory, or concepts of maximal scope. It is not the presence of theory as such which marks off such metaphysical systems, it is rather the great generality of the schemes that

they exemplify. Theories which contain concepts of maximal scope certainly fall into this category. The view that all events can be described in terms which are reduced to the language of physics would be a prime example. But also any theory that the whole of history has an ultimate goal would be another. Now there may be a case for saying that theistic metaphysics can be defended by arguments comparable to those used to defend systems of this type. One might even claim that it is no more difficult to defend theistic metaphysics than systems of this type. And Mitchell does indeed seem to put forward a case somewhat along the first of these lines. But it is important to distinguish carefully between this claim and the analogy with the debate over Caesar's command in Gaul. In the one example theistic beliefs are said to be defended by a cumulative argument such as that used by historians dealing with the details of a particular era. In the other case theistic beliefs are being defended as comparable in character to other metaphysical systems of belief. Mitchell's case for the rationality of theistic belief uses both of these approaches. He first sketches the analogy with history, then admits that the analogy has limitations, and supplements it with the further line of defence. It is however the first of these that I wish to explore further at present.

It is a merit of the work we are considering that it does see that the question of rational belief cannot just be confined to the rationality of believing simple sentences. Our beliefs come in sets and we test the probable truth or falsity of a belief or set of beliefs by exploring its relationship to

other beliefs. This process of testing beliefs and sets of beliefs is of crucial importance in distinguishing between those beliefs which we hold to be rational and those which we class as irrational. The case of beliefs about past events is sufficiently complex to raise a whole series of issues which are of immediate concern to anyone interested in the rationality of belief, and this is the case both for those chiefly concerned with metaphysical belief and for those concerned with beliefs of a less general character. Without necessarily invoking a purely foundationalist theory of justification, I would however argue that we could propose that at least from one point of view we could construct a scale of increasing complexity. At the lower end of the scale is a belief about a matter of direct observation. I believe that there is a sheet of paper in front of me. More complex than this is the belief that Caesar's command in Gaul extended to 50 or 49 B.C.. This is more complex because it includes beliefs about matters of direct observation and more besides. It includes the belief that there are coins of Julius Caesar now observable. It also includes the belief that I once observed the text of a book which is claimed to be a copy of a copy of a copy (etc) of one written by Caesar. Then there is a further level of complexity. This is that I know people who believe that Christ died for the redemption of the world. Such a belief includes beliefs comparable to those about the existence of a figure of the past such as Caesar, but much more besides. For the sake of our present analysis we can speak of ~~those~~ three tiers of belief each of which includes elements to be found in the previous level.

I must of course make certain qualifications at this point. I am well aware that the three tiers of belief which I have here identified are only three amongst many widely differing classes of beliefs. I not only do not dispute that, I positively assent to and accept that. But these three tiers do represent an increasing order of complexity when viewed from the perspective I have presented. Each tier includes the problems of the previous one and also additional problems besides. I also accept that there are many types of belief in addition to those selected here, as well as there being many tiers of complexity. I am very far from presupposing that all types of belief are to be assessed by the same criteria. There may be certain common criteria, but that must be argued for and not presupposed. For the time being we must note that as well as there being beliefs about matters of direct observation, and historical beliefs and metaphysical beliefs there are also others. There are beliefs about the classification of animals and birds, of protons and electrons, of civil and criminal law, of ethical values and of astronomical and cosmological theory. Such widely differing classes of beliefs may have some features in common, but are so evidently different in subject matter that it would be wise to expect them each to raise special difficulties of their own when it comes to evaluating the grounds on which they are held and the degree of confidence with which we should hold them.

For the present however I wish to confine the discussion to the more limited but still sufficiently broad question of the comparison of historical and metaphysical beliefs. Another objection which might be made is that not all metaphysical

beliefs necessarily include a historical element. There may be C beliefs which lack an A element. Thus 'God is an immortal and invisible being' does not obviously or straightforwardly contain any reference to history. This belief therefore cannot be shown, on the grounds given above, to be more complex than a belief about Caesar. Whether it is so or not is a nice point. What is sufficient here I hope is to argue that this example of a C belief belongs to a class of C beliefs (held by Mitchell and many others) which we are investigating, and which taken as a whole do contain A elements. Indeed more than this can be argued. It is inherent in the character of the set of theistic metaphysical beliefs under discussion that the God about whom they speak is not only a being about whom metaphysical statements are made, but who is also held to have acted in certain ways in history. Whether one agrees with his beliefs or not it ought to be recognized that this is what Mitchell and many other traditional theists believe, and such beliefs cannot properly be discussed if one only looks at those sentences which speak of an omnipotent and benevolent deity, and not at the associated beliefs of a historical character. To abstract certain elements and look at these alone is an artificial procedure and undoubtedly contributes to the sense of unreality which can sometimes be detected in purely philosophical debates about theism. Even in the case of Jewish and Islamic theistic belief I would hold the same point to be valid. One cannot properly evaluate Judaism without recognizing that its beliefs about God are beliefs about a deity who is held to have acted in a certain way in the history of his people.

In the case of Islam also one cannot completely separate abstract belief about God from the belief that he has revealed himself through the teaching of the prophet. Thus even if we could identify single sentences within such systems of belief of a purely C type we would have to admit that these are in practice held in conjunction with other C beliefs which include A elements. This shows, I believe, that the issue is more complex than Mitchell's published argument admits. I do, however, think that Swinburne's defence of C type statements exemplifies one way of conducting such a defence and I propose at a later point to offer some criticisms of his presentation also.

The preceding paragraphs do, I think, show that the analogy which Mitchell put forward is a very interesting one. Points can be made for and against it, and it is especially important to note that the issue is much more complex than appears at first sight. Having, I hope, given sufficient preliminary indication of its complexity I now wish to take a somewhat longer route to examining its validity or invalidity. I propose to consider at some length the character of beliefs about historical matters. This will serve two purposes. It will advance our general study of the rationality of belief by considering that issue in relation to a specific class of utterances of belief namely those about past events. Secondly it will advance our consideration of Mitchell's analogy. When we return from the more extended discussion of historical beliefs I propose then to consider some arguments of Richard Swinburne as well as those of Mitchell. This is because I

think that Swinburne has taken up the question of cumulative argumentation and the confirmation of belief in a way which extends and amplifies the debate. If at that later point we are to consider whether a metaphysical system can successfully be defended by an argument which relies on a cumulative assessment of probabilities, then a critical appraisal of Swinburne's approach is particularly appropriate. Indeed not only has Swinburne developed his view in a series of three volumes, but his approach has given rise to contrary arguments from other philosophers such as J.L.Mackie.[42] The issue is therefore one giving rise to lively debate in philosophical circles at present. But our next step must be to look more closely at the kind of argumentation used in assessing beliefs about historical events.

Chapter 4. Historical Study and the Criteria of Rational Belief.

We began our study with a question about the character of rational belief. In order to examine this topic I first focussed on the relation between belief and knowledge to see where the concept of rational belief stood in relation to the more frequently discussed and often more highly prized category of knowledge. Then the argument turned to the nature of belief and to the factors which urge us to consider some beliefs rational. In the third chapter I pointed out that we also need to consider complexes of beliefs and sets of beliefs as well as beliefs expressed in single sentences. I there selected historical and metaphysical beliefs as special areas for further attention.

I chose historical and metaphysical beliefs because each of these areas has aroused controversy in relation to criteria of rationality. Each deserves attention in its own right. But the question of an analogy between them has also been raised and will be kept in mind. In this chapter the focus will be on factors affecting the rational appraisal of beliefs about the historical past. In any study of rational belief this deserves attention in its own right. It also has connections with problems of rational belief elsewhere. If current philosophy of science makes use of the history of science in order to assess the rationality of science, then it ought to be interested in the rationality of historical study. But in any case historical beliefs form an important subset of our total set of beliefs. So the question of their rationality is a central not just a preliminary matter. Then as further chapters follow I also propose to return to the question of an analogy betwen historical and metaphysical beliefs. So this chapter is more directly concerned with issues involved in assessing the rationality

of historical beliefs themselves in their own right. But a later chapter will also draw on this one for comparative purposes.

The rationality of beliefs about past events is an interesting test case for a theory of rational belief. This is so because beliefs about historical events form a distinct and problematical class. I wish to claim that there are certain similarities and affinities between different classes of belief. But I also wish to maintain that the different classes are different, and that they cannot be wholly assimilated to one another. The extent to which different classes of belief are evaluated by similar procedures is a vexed question. The issue is particularly acute in the case of metaphysical beliefs. Some philosophers might well argue that such beliefs cannot be treated as if they were comparable to beliefs about chairs and tables or even the events of the only partly accessible past. But to adopt this view in a full blooded manner leads to the consequence that there are very few or perhaps no common principles for assessing the rationality of different classes of belief. This would leave one with one of two choices. Either each class of beliefs has its own canons of rationality, or certain classes of belief are beyond rational evaluation. I personally do not find either of these extreme positions acceptable, although I admit that if one does not claim that all beliefs are assessed by the same criteria of rationality one must give at least <u>partial</u> assent to one or other of these views, (preferably the former). The relation between historical beliefs and beliefs about matters of more immediate perception provides an interesting test case for examining the extent to which common criteria of rationality

can or cannot be held to apply to different categories of belief.

The most evident difference between historical beliefs and beliefs about matters of present observation is the inaccessibility of the past. Past events are not directly accessible. But to what extent does this factor really distinguish history from other disciplines or historical beliefs from beliefs about the physics or chemistry of everyday objects? In the case of historical events we are dependent on the indirect evidence of present data from which we infer what took place thirty or three hundred or three thousand years ago. We possess artefacts or original documents or copies of documents from which we make our inferences. Statues and coins and inscriptions and papyri provide us with objects of direct perception about whose character and significance we construct theories and hypotheses. Such data can be very differently described. For example I am very familiar with photographs of a particular coin which can be described in the following ways:

D1 This is a round piece of metal.

D2 This coin bears the name Vespasian.

D3 This coin bears the legend Judaea capta.

D4 This is a coin from the time of the Roman Emperor Vespasian.

Each of these descriptive statements contains an element of observation and an element of theory. The interrelations between theory and observation are complex and I propose at this point only to draw attention to a few of them. D1 describes a currently observable object in terms of geometrical shape and chemical composition. D2 is different. It describes lettering found on the object, it describes the object as part

of a familiar monetary system, and it describes the combination of letters on the object as comprising a name. D3 is similar to D2 except that the inscription on the part of the coin mentioned in D3 is a slogan containing not only a name but also (part of) a verb. D4 not only identifies the object as a coin but also gives it a place in the chronological framework of Roman Imperial history. From many convergent pieces of evidence such as the existence of this coin, the arch of Titus, the writings of ancient historians, and recent archaeological evidence of buildings captured by Roman soldiers and burnt, we conclude that this coin relates to the crushing by the soldiers of Vespasian of a revolt in Judaea which had begun in A.D. 66.

Historical statements about the military successes of Vespasian and Titus, or the duration of Caesar's Gallic command, are statements about events which are only indirectly accessible. But that does not in itself mark off historical beliefs from the beliefs of other scientific disciplines. Many other statements are about matters which are only indirectly accessible. For instance some states of affairs are only observable by instruments because of their comparative inaccessibility. Very minute particles and very distant objects fall into this category. There is here however an important distinction. Such objects can be reinspected on successive occasions (and perhaps with different instruments). Now while one might maintain that the past events of the fall of Jerusalem and the fall of Masada can be reinspected, the process of reinspection is not precisely comparable. In the one case the object is present but very minute or very distant, in the other it is

the evidence which is present and reinspected, the past state of affairs is "arrived at" only by theorizing about the evidence. (Indeed it might be argued by some theorists that the past state of affairs only exists in the historian's theoretical reconstruction, though I would not wish to use such a way of describing it).

There are however whole sections of natural science which are much closer in character to historical study than is suggested by the usual examples taken from such disciplines. For instance one could cite beliefs about the origin of the species and beliefs about the early cosmological events which resulted in the rapid acceleration of galaxies in different directions. (I am of course here using 'cosmological' in an astronomical rather than a metaphysical sense.) Here we have examples from biology and cosmology which do have distinct affinities with examples from historical study in so far as they are all concerned with states of affairs which are held to have obtained in the distant past. For this purpose the phrase 'less recent past' must serve as a less than wholly precise way of referring to that period which antedates the memories of those now alive. The very recent past raises issues of a slightly different character again in so far as it is accessible via our own memory. Indeed a case may be made for using the recent past as a model for the understanding of the less recent past. But be that as it may we are obliged to note that certain assertions about cosmology and biology have distinct similarities to assertions about historical events of the more distant past. The issue with which we are here concerned cuts across the common distinction between the natural and the human sciences.

In certain respects at least, some parts of biology and cosmology share with historical study the methodological problem of drawing inferences about past states of affairs from present evidence. In other respects of course the common distinction remains. The study of the early history of the galaxies and of the origin of the species are very different disciplines from the study of human history. What especially marks off human history is the complex nature of human personhood and human motivation. (This last point stands whether or not one adopts a physicalist or a personalist view of human behaviour).

One of the tasks of the historian is to propound theories and hypotheses about past events. The theories are the present constructs of the community of historians, the evidence is present in the form of objects and reports, we might even say that the 'facts' about the past are present conclusions about past states of affairs when those conclusions are indeed true. But it is no novel conclusion to insist that the past states of affairs may be the subject of our theories or hypotheses but cannot themselves be recovered. This point has surfaced in recent philosophical controversy over the work of Leon Goldstein.[43] I wish to maintain however that in at least some sense of the term the events of the past are independent of our theories about them.[44] The events of the past have left surviving traces and when we come across traces of these events we can correct our theories. This process of judging whether in the light of new evidence our previous theories are true or false is an essential part of historical study. But it is not the only factor which leads us to infer the reality

of past events.

It is right to distinguish the recent past from the more distant past. But the very distinction itself tells us something about the character of history. The chief ground for differentiation is that many events in the recent past can be attested from the memories of those still alive today. Thus the argument from memory provides an important step in the discussion about the past even if it is only the events of the recent past which are remembered at first hand. This is not to say that memory is always correct. Indeed it is all too evident to us that our memories cannot always be relied upon as they provide us at times with conflicting evidence. But the unreliability of specific memories does not undermine the argument from memory altogether. On this Walsh writes

> 'Part of the evidence for the judgement that memory is liable to mislead consists of memories of occasions on which we have ourselves been misled by it, and unless these memories are treated as authentic the wider judgement could never be made.'[45]

Memory is of course not to be equated with immediate perception. But the relation between them is a subtle one. The sense perception of objects can be repeated. This is not the same as recalling a memory once again. In the case of sense perception the object is freshly sensed on each occasion. In the case of the successive recounting of one's memories we would be wiser not to speak of fresh access to the past event but to the reawakening of the memory of the past event. But even successive sense perception of objects is significant only if a memory of the previous sensing is accepted. Thus there is a role for

memory even in the observational procedures of the natural sciences, even if that role is not identical with the role of memory in recalling recent historical events. Further if memory is not always reliable, neither is sense perception always so.

In the case of remembered events (or remembered states of affairs) we are dealing with something which is at least partially comparable to sense-perception. But the events of the more distant past are not directly accessible to our memories. All that we can claim is the following. Memory provides us with ground for believing that there were events in the recent past. But the acknowledgement that there were events in the recent past, along with objects (and records) which lead us to make inferences about the more remote past, provides us with ground for believing that there were events in the distant past. This argument does not attempt to refute total scepticism. All it can do is to claim that if on the basis of sense-perception we accept the reality of present objects then there are comparable but not identical reasons for accepting that there were past events and past states of affairs.

Insofar as history and natural history are concerned with past events and past states of affairs there is as I argued earlier a partial analogy between them. But there are also significant differences. We cannot simply assimilate history to natural history. In both disciplines we find an assumption on the basis of present evidence that certain things took place or were in a certain state at an earlier period. Thus natural sciences also incorporate theories which postulate past happenings. But history is essentially though not exclusively

concerned with events involving human activity and with institutions and forces in which human behaviour plays a role. Thus history does not simply consist in events decided by single great individuals, but, even when concerned with the movement of prices in the later Roman Empire, it is dealing with past happenings in which human behaviour is a significant factor. I would therefore appeal to the analogy between history and natural history in arguing for the reality of past happenings, but wish to qualify it when considering the distinctive character of historical events.

Reasons for wishing to qualify the analogy between history and natural history would not only derive from the argument that history typically involves reference to the thoughts and intentions of human agents. That is so, and it does mean that history is for that reason to be distinguished from natural history. Even if someone were to argue that human thought is merely to be seen as the behaviour of a complex physical organism, one could still argue that human thought operates in ways which are markedly distinct from the behaviour of molecules, or sub-atomic particles, or algae or zebras. But it is not only the role of human thought which is a factor in marking off history from natural history. Many philosophers of history would maintain that the laws and generalizations used by historians differ at least in certain respects from the laws of natural science. The chief reason often given is that explanation in history rests on generalizations of a different kind from those used in natural science. The generalizations are more often limited in character to the traits of human behaviour in particular periods or amongst particular groups of people. This is not to say that there are not also universal

laws which are also exemplified in historical events. Human behaviour is of course subject to the constraints of nature. But many patterns of human behaviour themselves only seem to be capable of being described by laws of limited generality. Thus certain patterns of behaviour are more common amongst some groups and in some periods. The marriage customs implied in the play Romeo and Juliet, and those in Shakespeare's England, are not necessarily those of other peoples in other ages. Even when one is offered what looks like a real generalization it differs from scientific laws. Take for example

H1 Discontent amongst the peasantry always leads to revolution.

This is imprecise in its details, specifying neither the degree of discontent needed nor the time which might lapse before revolution comes.[46] Few historians would be willing to attempt to tighten such a generalization as they would not consider it possible to amass sufficient data to complete the task. But this argument does not exclude the possibility of social scientists attempting to assess, in the present, more precise details of such a correlation. Historical study lacks such precision. We cannot readily decide whether it is unobtainable in principle or not, but it is unobtainable in practice in many cases.

In considering the rationality of beliefs about historical events a series of issues can be specified for further debate. Some of these have already been introduced in a preliminary way but in each case some further discussion will be needed. There is the character of historical explanation insofar as the statements of historians purport not only to tell us what

happened but why it happened as it did. Then there is the question of historical objectivity. If historians describe a series of past events from different and apparently incompatible viewpoints, to what extent can we claim that the statements made by any one of them are rationally believed? A further major issue is the relation between historical study and other disciplines. Many philosophers write as if the natural sciences exemplify rationality. Indeed debates about rationality and epistemic justification are most commonly conducted with reference to the rationality of conclusions arrived at in the natural sciences. If in the study of history decisions are made in a somewhat different manner, what are the implications of that for the rationality of historical beliefs? And if other beliefs are defended because of their comparability to historical beliefs what are the consequences for those beliefs of our conclusions about historical beliefs? Finally and perhaps most interestingly what is the relationship between statements of belief that certain historical events occurred and the events themselves. Can we speak meaningfully of the reality of past events and should we reject those, like Goldstein, who speak of historians actually constructing historical events? In a work of this kind it is not possible to give full treatment to all of these issues and I propose to focus especially on the fourth and last of them as this aspect of the status of historical beliefs is undergoing the most vigorous recent discussion. But in order to approach this question I propose to look briefly at those which lead up to it.

Both when attempting to discover what it was that happened, and also when offering explanations as to why it happened,

historians engage in the construction of theories. In this respect history proceeds much as other sciences do. There is present evidence, and on the basis of the evidence rival theories may be offered as to what occurred and what led up to the occurrence. Of course there are differences in that, as we have noted above, the past itself is not available for our inspection, we can only inspect the present evidence. But in that historians deal with theories about evidence there is a comparison to be had with the rational pursuit of other disciplines. But the borderline between theory and observation is a notoriously problematical one. It is helpful to distinguish between different levels of theoreticity. The example of the Roman coin given above already indicates some of the issues. But I propose to pursue the point with a different example, that of the Coptic gnostic papyri from Egypt.

In the case of these papyri there are different levels of theory involved in the description of the evidence. Botanical theory is involved in identifying the material as papyrus, theory about ancient books in declaring that they were bound as codices, theory about language in identifying the script in them as Coptic, theory about the history of ideas in declaring the contents to be gnostic in character. Even at the simplest level in declaring the discovery to be an 'object' we are involved in some kind of very basic theory. But the texts in question serve as evidence for higher level historical theories also. Let us suppose that there is now widespread agreement amongst the relevant scholars that these texts are papyrus codices written in coptic and coming from Egypt between AD200 and AD 500. These conclusions now form an evidential base for

higher-level theories about the origins of Gnosticism. They do so even though it is admitted that theory is involved in the basic description of the evidence that we have given. Indeed the basic description may well not only contain theory but also imply that a change occurred or that something happened in the past. In this case the basic description implies that the texts were written in Coptic language in Egypt in the time of the Roman Empire. Our task is easier in this case as there is indeed general agreement amongst the relevant experts on what I have classed as the basic description of the evidence. Even if there were not I would still maintain that what we are investigating follows a more complex form of a basic pattern. The pattern is this. Observation + $theory_1$ provides evidence for $theory_2$. Perhaps closer to this example is the variant: observation + theories 1, 2, 3 provides evidence for $theory_4$.

Let us take this pattern as an outline of the situation described. Now $theory_4$ in this case represents what I have called a higher-level historical theory about the origins of Gnosticism. We have a contemporary object on whose description we have good measure of agreement, which it is claimed forms the basis for a historical theory or a set of rival theories about gnostic origins. Let one of these theories be that Gnosticism was independent and not a byproduct of Christianity. We will call this the thesis of an independent Gnosticism. (Its dependence on other Hellenistic and oriental beliefs is generally accepted). To what extent is there a difference between the rationality of believing that Gnosticism arose in an independent manner and the rationality of believing that this object is a papyrus codex or that that object is a wooden table? It is at this point that it becomes

apparent that the rational defence of theories is not entirely uniform in different fields for reasons additional to those already given. To some extent one can supply the usual answers. One theory is preferable to another on grounds of internal consistency, its consistency with other accepted beliefs, its simplicity, explanatory power, fruitfulness and the like.[47] This answer is certainly what one is given in many discussions of scientific method. Thus we are told that Newton's theory of gravity is preferable to its predecessors because of its greater simplicity and its greater explanatory scope,[48] or that Einstein's theories are acceptable because of their explanation of anomalous data and their offering predictions which were subsequently confirmed. The criteria are however by no means without problems. Estimates of simplicity may vary, and one criterion may have to be traded off against another. But the use of these criteria is a commonplace of the philosophy of science. The recourse to these criteria is certainly most evident in the case of high level scientific theories, and especially in those cases where rival theories eventually gave way to one dominant theory. But the criteria are not all equally applicable to all of these examples.

In the case of historical beliefs the choice between theories does also in practice rest on criteria such as those listed.[49] But here too the criteria are not all equally used in all cases. Fruitfulness in making predictions is a very important criterion, but one much more rarely available in historical study. Also it is not so easy to obtain theories of wide scope and greater explanatory power in a discipline which relies heavily on limited generalizations and is often concerned with the events

which occurred in particular places and particular periods. Yet both of these criteria do have a place and an important one in certain instances. Very often however it is the internal consistency of a theory, its consistency with other beliefs and its simplicity which commends it. In the case of the Nag Hammadi Gnostic texts it was observed that two of the texts were notably independent in the manner defined above. This observation was held to confirm one of two previously rival theories about Gnosticism, namely the independence theory. However confirmation is usually a matter of degree and it is so in this case. The new discovery in a sense acted like the verification of a previous prediction. One could cast it in the following form:

H2 If Gnosticism did arise independently we would expect one day to find Gnostic texts of an independent character.

But the acceptance of the independence theory also relies on other criteria notably that of simplicity. Its supporters have to maintain that it is also simpler (and therefore more rational) to hold that some of the recently discovered gnostic texts are independent and arose amongst Gnostics who had no close acquaintance with Christianity, rather than that such texts emerged amongst Gnostics who had been originally influenced by Christians and who then ensured that their texts left no trace of such influence.

In this example we see the operation of two criteria. A previous theory gains confirmation when an implicit prediction is fulfilled. But the evaluation of the fulfilment of this particular prediction itself depends on the judgement that one

view of the new texts is preferable to its rival or rivals on the ground of simplicity. Clearly much more analysis of these criteria is needed than has so far been provided by this example. But what is here being maintained is that, given that certain criteria are used to evaluate the rationality of scientific theories, we can examine the practice of historical science to see if similar criteria are used. If they are, then it would be possible to pursue the argument by maintaining that the criteria of historical study are at least as rational as those of the natural sciences. But the issue cannot be so easily resolved. We have already noted certain differences and distinctions between the two areas of investigation. Where historical methods and criteria differ from those of the natural sciences we have a test case for the argument that at least to some extent this academic discipline exhibits variations from the criteria and standards of rationality used in the natural sciences. This is therefore a crucial issue in the entire argument of this work. If someone is inclined to assume that the criteria of rationality in the natural sciences are a paradigm case of what it is to believe something rationally, then our evaluation of historical methodology may provide a test case of whether in fact other disciplines ought to be assimilated to the model of rationality which is held to obtain in the natural sciences. Two factors must be examined. The first is whether there are indeed differences in the criteria for rational belief in history and in the natural sciences. I have already argued that in at least some respects there are such differences, but the issue deserves further exploration.

The second factor is whether if we do concede the existence of such differences that lowers our estimate of the rational status of historical judgements or obliges us to admit at least some variation in the application of criteria of rationality.

A further point could well be noted at this juncture. When estimating the rationality of believing that all the marbles in a bag are red when a certain proportion are drawn at random and found to be red, we can use a mathematical formula which computes the probability involved. Intuitively we recognize that the more marbles that have been drawn and found to be red the more likely it is that all are red. Of course it is more complex, as one green marble instantly destroys the likelihood of all the marbles being red, though it may only marginally reduce the likelihood that most of the marbles are red. But in such cases one can argue that a conclusion is rationally acceptable or rationally believed if there is more evidence for it than against it (provided especially that there is no conclusive evidence which tells against it). This is largely the position adumbrated towards the end of chapter two. One must, however, remember an important distinction. The case of the coloured marbles differs in at least one significant respect from that of many inductive inferences. In the case of a bag containing a specified number of marbles we reason that the probability of the marbles in the sample matching the <u>given</u> population increases with the size of the sample. But in the case of many inductive inferences the population is not fixed. If we do not know the total population of ravens we are unable to calculate the proportion of observed black ravens to unobserved ravens. The inferred

generalization that all ravens are black has to rest on our observation of large numbers of black ravens in as many different places and circumstances as we can manage, and the absence of any observations of ravens of a different colour. This differs from the example of the marbles, and it also differs from the case where we have conflicting evidence i.e. strong circumstantial evidence of theft on the part of an employee who had previously been regarded as of unimpeachable honesty. In such a case we would, as argued earlier, be inclined to say that circumstantial evidence of guilt may well be insufficient to outweigh reliable evidence of previous good character. The examples show that non-deductive inference takes very varied forms. Even in the case of the ravens, however, we might allow that there was greater evidential support for the generalization that all ravens are black in the following circumstances. The supporting evidence is substantial and varied as assumed above, no firm counter instances are recorded, and any reports of multicoloured ravens come from untrained and untested observers. In such a case we would say that there is greater evidential support for the generalization than against it.

But we must now take account of a very different factor. This is that in evaluating higher level theories in science and in history a variety of criteria are involved. These cannot readily be reduced to the formula that more evidence favours theory A than theory B. We may need to amend our view of rational belief once again in order to make clear what in these circumstances might be a better description of the matter. While we might continue to tolerate statements

to the effect that the balance of evidence is in favour of theory A we might prefer to express the issue in terms of criteria.

To some extent this issue had already been anticipated at an earlier point by using the expression 'evidential support'. But we could now note a further factor. It is RB2 which is in question here and which needs to be reconsidered and supplemented:

RB2 S rationally believes that p if S believes that p and S has greater evidential support for p than for -p.

This was supplemented with RB3 which covered those cases where the evidential support is equal. But now a further point can be added

RB4 S rationally believes that p if p is a theory which satisfies the criteria of rationality better than -p.

Our procedure is to work towards an analysis of the problem of rational belief by gradually revising or expanding the formulations of what is involved in rationally believing that p. RB4 introduces into the formulae specific reference to the criteria. But it still leaves unresolved what it is for a theory to satisfy these criteria better than its negation does. This is an especially problematical area. Judgements of comparative simplicity can be attacked as subjective, and even if they were agreed, there might still be ground for disagreement elsewhere. One theory might be simpler and another display greater explanatory scope. How then do we decide between them? Or one theory might make spectacularly successful predictions, but be regarded with deep suspicion as it seems paradoxical in certain respects. The controversies over the logical status of quantum mechanics are held by some physicists to exemplify just that difficulty.

But for the present we must return to the question of the rationality of belief about matters of history. All the same we must continue to look at the criteria used, and the way historical judgements are held to satisfy them.

I have argued that historical statements are based upon present evidence such as texts, inscriptions, the remains of buildings. These pieces of evidence are described and the description of them incorporates lower level theory. The evidence as described is then set by historians into higher level theory about past events and past states of affairs. Where these theories are in conflict with one another, choices have to be made. Those choices are not arbitrary but guided by criteria such as consistency, simplicity, explanatory power, fruitfulness and the like. In this respect there is a broad similarity between the structure and evaluation of theories about the historical past and the structure and evaluation of scientific theories, certain differences being conceded. These differences can be detected especially in the relative inaccessibility of past events[50] and in the rarity of opportunity for actually testing predictions.

The next topic for discussion is the question of historical explanation and the extent to which the vigorous controversy over historical explanation throws light on the question of rational belief. One class of historical statements has attracted particular attention from philosophers and this is the class of explanatory statements. Some approaches to this question begin from a priori principles about the nature of explanation. From this standpoint it is argued that one can only explain why an event occurred if one can specify both a

general law and a set of initial conditions. Given the general law, and the satisfaction of the initial conditions specified in that law, one can then obtain deductively the conclusion that the event occurred. This outlines very briefly an approach which attempts to make historical explanation conform to a certain model. The model is that which sees explanation in terms of a deductive inference from a covering law and a set of initial conditions. The more precise description for this line of approach is 'deductive-nomological'. But this ugly hybrid of Greek and Latin terminology is clumsy and inelegant, and in practice an alternative but looser description has been used. This approach has therefore often been described as 'the covering law' approach. It should, however, be noted that the covering law approach only provides a complete explanation where the covering law in question is strictly universal. The crucial point can be simply made. There is a law that the rolling of four dice will almost always result in a total of more than four spots being uppermost. The statistical probability is 1295/1296 = 0.9992. But if we ask why in such a case more than four was scored, the law in question does not provide a complete explanation even if it tells us that such a result was extremely likely.

The controversy over the covering law approach to historical explanation is especially instructive. It provides an example of an attempt to assimilate one discipline to the standards, methods and criteria of another. The model of explanation by deduction from a universal law and a set of stated initial conditions is one which is much more common in the natural

sciences. Even there, however, it is not the only model for explanation, as the use of statistical generalizations shows. But it is with the different character of historical explanation that I am chiefly concerned and the diversity even within the natural sciences must be left to those more acquainted with those fields. In the case of historical explanation a notorious dilemma arises. On the one hand explanation by deduction from a universal law offers an impressive model of how on a priori reasoning complete explanation ought to operate. One could well argue that unless the event was certain, given the universal law and the stated initial conditions, one had not actually offered a convincing account of why the event did in fact occur rather than fail to occur. Thus the practice of the natural sciences and our strong intuitions about the nature of explanation point firmly in one direction. But the actual practice of historical study and a great many theorists about historical methodology point in a different direction. In practice historians are reluctant to provide, or to allow others to provide universal generalizations. There is wide agreement that most explanations offered by historians fall short of the covering-law model advocated by Hempel. There is also a claim by some philosophers that historical practice should not be forced into a theoretical mould provided by other disciplines.[51] The controversy which arises here does so as a result of the formulation of an issue in one discipline being proposed for use in regulating the methodology of another discipline. We must therefore examine the problem a little more closely.

One of the factors in the controversy is that terms like 'explanation' and 'complete explanation' are inadequately defined. Just as with statements about causes some further clarification is often needed. In ordinary language we say that a fire in a cinema was caused by an unextinguished cigarette. But someone who insisted on pedantic precision would demand that we name other necessary and sufficient conditions. So in the case of explanation what is often offered in ordinary language is an account of those features which lessen our puzzlement about why an event occurred. We are puzzled that Louis XIV reduced pressure on the Netherlands and thus allowed William of Orange to land in England. Trevelyan offers the explanation that Louis calculated that William would become involved in a civil war and long troubles in Britain and so allow France more scope in continental Europe. The fact that in that case Louis blundered badly is beside the point, the calculation would have seemed reasonable enough at the time. But the explanation offered would need to be amplified in order to satisfy those who uphold the covering-law model. The one view sees explanatory statements as statements which reduce puzzlement. The other view insists that explanations must give an adequate account of why things happened as they did and did not turn out differently. In order to do that the explanation needs to be recast so as to imply (or confer high inductive probability on) the event. Here view '<u>a</u>' and view '<u>b</u>' are operating with looser and tighter definitions of explanation. On the tighter view one requires a generalization in order to provide a full or complete explanation. (The terms full or complete do not here imply an ever receding series of further

explanations of facts cited in the original explanation). But one can also see that many of the required generalizations are at present either not available, or if available only cast in a form which incorporates a series of not very precise restrictions. Thus one could produce a generalization to the effect that rulers normally refrain from pressing their enemies if they think that the latter have embarked on a course of action which will keep them hors de combat. But a generalization cast in such a form does not allow one to infer with deductive certainty events such as the failure of Louis to keep up pressure on William of Orange. Nor is the support for such a generalization easy to produce.

Indeed some might argue that there is a paradox here if the generalization is needed to support the specific instance, and if the specific instances are the grounds on which the generalization is accepted. It is true that historical generalizations are rarely made explicit, and are often suspected of unreliability if they are set out in detail. So it is paradoxical to insist on a shaky generalization to add rigour to a specific explanation. But the paradox may be lessened if one argues as follows. The historian notices that R is often followed by S and infers inductively that R is a significant contributory cause of S. This general inference is strengthened if it can be shown that R is never followed by -S or that if R is followed by -S some further factor is implicated. (Here an auxiliary hypothesis is needed). The general inference even if only given qualified assent could then serve to support the specific explanation in a modified Hempelian manner. This suggestion does not resolve all

the difficulties affecting the theory of explanation. It does, however, provide a way of showing how individual explanations can be backed with limited generalizations which themselves derive support from the observation of specific sequences of events. The links are however non-deductive ones. Even the move from the generalization to the specific explanation is not deductive. The generalization is a hypothesis tested in certain ways, and propped up by auxiliary hypotheses. The hypothetical generalization only renders the individual explanation more likely, it is not so tight as to allow the explanation to be inferred with deductive certainty.

The above account helps to diminish our unease over the problems of historical explanation, but does not resolve all the difficulties. It invokes the kind of defence of hypothetical generalizations and their need of auxiliary hypotheses which is not unknown in other disciplines. The key point is however that the generalization may only confer probability on the explanation. A specialist in probability theory might wish to add further refinements in terms of the probability of the explanandum in a specified reference class being higher than its prior probability.[52] But the mathematical complexities cannot be further pursued here.

In the argument above and especially in the example concerning the policy of Louis XIV it is accepted that one of the differences between history and the physical sciences is that history is at least some of the time concerned with human agents and their thoughts. In explaining why a general acted as he did we may well suppose that it was because he thought that the disposition of enemy forces was different from what it later turned out to

have been. There may even be some record of a despatch, or a memoir claiming that to have been the case. Or it could be that only such an error of judgement on his part would satisfactorily explain that general's disastrous plan of advance, and so such an error is inferred by a historian. Now I grant it is a very important part of historical explanation to consider the thoughts as well as the actions of people of previous generations, as well also as the character of their institutions and social customs. But this does not mean that one has to assent to some of the more extravagant idealist views of history. There is no necessity for the acknowledgement that past thoughts play a role in history to lead us to the view that history is primarily or exclusively concerned with the thoughts of past agents, or that the historian has the same thoughts as past agents. One very direct objection to the second of these views is that the past thought led to a certain action whereas the historian's present consideration of that past thought does not lead to the same action. A historian may indeed imaginatively place himself in the situation of Caesar or Napoleon, and ask what he himself or what some modern commander might do in a similar situation, but that is all that we need to admit, without going so far as to say that the past thoughts are re-enacted. If what is required is an argument against the view that we cannot reliably infer what was then thought, we still do not need to fall back on the disputed formulations. We do not need to speak of re-enacting past thoughts. It is sufficient to argue along one of the following lines. We assume that people generally give us a truthful account of their mental calculations unless we have one of a number of reasons for suspecting it to be in their interest to lie. If we have an account of what someone's calculations were then we test whether it is reasonable

to believe that account in ways similar to the tests we would apply to a witness in a court. Not all of those tests are available, but we can ask about whether it was in someone's interest to lie, whether on previous occasions his evidence was trustworthy, whether his account is self consistent and so on. Or if we have evidence only for someone's actions, we might infer what his calculation was, even in the absence of direct evidence, by asking what calculation is usually made by people who act in that way. In other words we may admit that the consideration of what past agents were thinking may indeed be a part of what is involved in a historian constructing an account of what he has reason to believe happened. But this does not require us to admit the more extravagant claims of idealist theories of history.

Before leaving the question of historical explanation a number of remaining issues need to be noted. First there is the relevance of theories about historical explanation to other claims about rational belief. Arguments about causes and about explanation figure in Swinburne's discussion of the defence of metaphysical beliefs by inductive arguments. It is therefore relevant to consider not only how the problems of explanation affect the question of rational belief in historical contexts, but also how this has a bearing on the analogy between historical and metaphysical beliefs. While it is true that a fair measure of what might be called inductive argumentation is involved in historical explanation caution will be needed in assessing Swinburne's extension of this to metaphysical arguments. There are as we have seen real difficulties about the rational acceptance of historical

generalizations. We may find that these difficulties are even greater in the case of metaphysical explanation, and that the analogy with rational belief in a historical context is one that needs to be treated with considerable caution.

Certainly historians do offer explanations, and these implicitly involve generalizations, even if the latter are often limited in character and only partially provided with rational support. Such generalizations rest on the evidence of the regular conjunction of particular occurrences, and on the merits of rival hypotheses about such regularities. But these hypothetical generalizations are often limited to the way people behave in particular periods, or to the way particular classes of people behave. But such generalizations if implied rather than stated are less likely to be tested, and historians are notoriously reluctant to expand their explanatory remarks about particular incidents into testable generalizations. Further even if stated in a way that in principle might allow testing, restricted generalizations about past periods are inevitably difficult to confirm or disconfirm.[53] Even in the contemporary social sciences this is problematical due to the many factors involved. In the case of historical generalizations this difficulty is compounded by the paucity of the data. But the difficulties are not insuperable. Two examples must serve, one from the nineteenth century, one from a much earlier period.

i) 'The conditions were favourable to a revolution. The government had no military forces at hand. The working class was passing through an acute stage of unemployment...'[54]

Here one can see an implied argument that revolutions are more likely to succeed when governments are caught without troops at the ready, and also that the populace is more likely to rebel when unemployment is high than when it is not. Each of these generalizations probabilifies the explanation, and each could be supported by instances of positive correlations. But the cases where such factors failed to lead to successful revolution would also need to be considered and further auxiliary hypotheses offered to account for these. The rational acceptance of the explanation thus depends on the preferability of the theory incorporating the hypotheses in question over rival theories and rival hypotheses. For example one would need to provide an explanation for successful revolution in Iran when the government had very powerful military forces and had been encouraging rising economic expectations in the population. The credibility of explanations and their related generalizations rests on the evaluation of the relative merits of rival hypotheses. It is not a case of simple enumerative induction,

ii) 'The Ghassanid prince and 12,000 Christian Arabs went over to the enemy. They were Monophysites and hated Heraclius; and their pay was many months overdue.'[55]

On this Cohen notes that while each factor alone would hardly explain the desertion, the two together form a plausible explanation, as each blocks a hole in the other. Yet he adds that it would be rash to accept an unqualified generalization that leaders are deserted by troops when arrears of pay compound religious differences. One needs to exclude factors which might counteract

a tendency to desert. These might include, fear of a common enemy, desire for loot, military tradition, or admiration for a leader. The presence of the former factors and the relative absence of the latter, would increase the likelihood of desertion. But further factors again might be irrelevant to the question. So the adequacy of an explanation might be increased by specifying further factors provided that they are relevant. That would imply further linked generalizations raising the probability of the explanandum. But the relevance of additional factors would have to be subject to a test. Such a test could be carried out even in a discipline like history where it is often the case that all the data are already there. The data may be available already, but the hypothesis that desertion is more likely in certain combinations of circumstances than in others could be tested up to a point. The problem is that such tests tend to be carried out more by looking for counter instances than by adding measurable instances. The combined factors are so many that it would be very difficult to determine the extent of the increase in the probability of desertion when troops have been left unpaid for 9 months as against 6 months. It is, however, plausible that the risk increases with the delay. We would infer that from our own feelings on the issue, and from observation of normal human behaviour.

So the rational credibility of historical explanations does to some extent turn on the comparative credibility of a set of relevant generalizations. These in turn are supported by arguments which render them more or less likely. So even if deductive inferences are not available and even if we cannot readily quantify the increase in probability which arises,

such matters do involve the assessment of probabilities. But the probability in question here may well function differently from the mathematical probabilities involved in coin spinning and dice throwing. Cohen is making a valid point when he argues that in legal and historical examples we often work by accepting that what normally happens is probable, unless there is evidence for one of those events which upset our normal expectation. The relevant procedure is a series of tests. One of Cohen's examples illustrates this. One normally assumes that a witness on oath tells the truth. But a witness may tell the truth about strangers, but not where he is an interested party. Or a witness may have good vision in daytime, but poor vision at night.[56] If either factor is thought to operate then we lower our estimate of the probability. When a witness speaks without evasion and without self contradiction, and in agreement with other independent witnesses we raise our estimate of the probability of the truth of his evidence. That there is a series of tests we would readily agree, and also that these raise or lower the probability involved. But rival calculi of probability have been proposed as methods of estimating the likelihood. Upholders of these rival systems could not from the points made above find conclusive reason for claiming that one view must be correct and the other mistaken. Yet there is a difficulty for those who use a mathematical calculus to estimate epistemic probability. In the case of the dice and the coins mathematical values are available. In the case of theory choice in history or in science, except in those cases where statistical factors operate, such values are arbitrarily invented. This should make us cautious about mathematical

treatments of the probable truth or probable verisimilitude of theories.

At an earlier point in the argument of this chapter a certain selectivity was proposed, and one of the topics selected for attention was the relation between what it is reasonable to believe about past events and the events themselves. It is to this issue that I now wish to return. I have already accepted that historical conclusions about past events are inferred from present evidence. I accept that what historians are doing can be described as constructing an account of past events and past states of affairs from present evidence. But it is one thing to agree with constructionist methodology and quite another to advocate philosophical constructionism. The latter view is suggested when philosophers of history like Goldstein talk of historians constructing facts or constructing events. Thus Goldstein writes about A.J.P.Taylor:

> 'Taylor's conception of the origins of the Second World War involves historical facts which have no existence at all in the conception of his opponents.'[57]

This is either loose wording by Goldstein or the endorsement of a position with considerable difficulties. If we distinguish facts from states of affairs, then facts are what true propositions assert about a state of affairs. But that means that the word fact is incorrectly used in the quotation. We should not say that Taylor's account involves facts which don't exist for his opponents. We should say one of the following. 'Taylor's account establishes facts which his opponents do not accept as facts' or 'Taylor's account claims to have established facts but his opponents do not recognize them as such'.

It is certainly normal in popular usage to say that historians reconstruct the course of past events. I suppose, to be more precise, we should say that historian A constructs an account of what he judges to have taken place. But some writers go further and speak of the construction of events. Goldstein does this when he says that 'Hexter then constructs a course of events which is supposed to make sense of what he has and knows' (about the 17th century Presbyterian Independents).[58] Goldstein contrasts this with the view that historians discover what happened by reading descriptions in old documents. The italics are Goldstein's. Now there could be an acceptable reading of what Goldstein says. I would agree that historians do not discover, or do not always discover, accounts of past events; they have to construct such accounts from the evidence available to them. (I would prefer to allow that such construction might indeed be discovery of what actually happened, but let that pass). But reconstructing what happened by constructing an account of what happened is not the same as constructing events. It is because Goldstein uses the latter phrase combined with a running criticism of realism about the historical past that his views have drawn critical fire. He does not deny a real past, only that it can provide a check on historical conclusions. He writes for instance

> 'We have no access to historical events apart from their constitution in historical research and no way of investigating reconstructed past events until they have been reconstructed.'[59]

Now there is a sense in which what he says is undeniable. Our only access to past states of affairs is indeed by historical method. We have no independent access to past reality.

But Goldstein seems to contrast this situation with that of observation of present objects. Now I grant that there is a difference in that, as I argued above, present objects can be reinspected. But one could argue against Goldstein that we only know present objects insofar as we observe them, and we have no independent access to present reality except as it comes to us through observation. But Goldstein does not seem to admit this, if I understand him correctly. He comes near to it at times in his reply to Nowell-Smith.[60] Yet at crucial points he shies away. Thus he says that we can glance at a vase that is present, but take no such glance at the historical past, and he sees this as a key difference. I would argue that insofar as he is right in talking of the primacy of knowing, we know the vase and past events only insofar as they are given to us through perception in the one case and through historical method in the other. But this does not lead to anti-realism, or to a denial of any effective role for the real object or the real past. In each case further observation, or the fresh application of historical method may oblige us to change view of the matter, and this is how reality exerts pressure on our picture of it. Of course in each case there is no access to that reality (present or past) except through perception in the one case, or historical method in the other. But that point does not render the concept of reality vacuous and without effect on our claims.

In order to unravel this tangle it is perhaps best to stand back from what Goldstein said, and attempt to clarify the substantive issue. It is true to say that insofar as we

know, we only know by means of the methods available to us. We do not have independent access to reality in that sense. But that does not mean that there is no check on our use of the methods. The way we use the methods in test t_1 can be further checked by our using test t_2 on additional evidence as and when that becomes available. If on evidence e_1 we claim to know that Roman Imperial finances were scrutinized by civil servants, then evidence e_2 may in the future confirm or disconfirm this view. (Confirmation is of course here used in the usual sense of providing further support for a view). Now this means that our views are constrained by reality. In the case of disconfirmation fresh evidence causes us to accept a fresh belief which is inconsistent with our previous set of beliefs. We may not always be able to tell <u>which</u> of our theories must be altered when alteration is demanded by an inconsistency in our set of beliefs, but some alteration is demanded in the case of disconfirmation, and it is reality which tugs at the web of our beliefs.

But we can only truthfully speak of knowledge when our beliefs are indeed true ones. This makes our claims to know precarious. But we cannot somehow evade that precariousness of our claims to knowledge by tactical retreat. It won't do to say at least we do have knowledge when the object of our knowledge is our own construction. That is the case whether we are attempting to construct an account of past events or to give an account of what we now see. Of course statements about what we <u>seem</u> to see now, or what we <u>hold</u> to have happened in the past are less vulnerable than statements about present objects or past events. But it won't do to reduce historical

study to knowledge of constructs, or knowledge of 'facts' if by that all we mean is the facts as we construct them.

There are two reasons to resist such tactical retreat. In the first place it does not secure complete invulnerability. (When I say 'I know that I am thinking' my statement is incorrigible. But my claim to know that I believe that p is not incorrigible. I may make an error and believe that I believe that Caesar died in 43B.C. and then quickly correct myself. In such an instance I did not know that I believed that p, because in fact I really believed q and not p.) Secondly and more importantly, our conduct of historical investigations would be futile if it were aimed at producing constructs regardless of whether they were true accounts of past events and past states of affairs. It is the attempt to provide an account which is a true description of the real past which gives history its point. Now I do not think that Goldstein would assent to some of the views which I am attacking here. But I state them, and reject them, in order to show why I reject views which tend in this direction. He asserts p, and p seems to me to imply q. As I reject q, and believe q to be implied by p, I must reject p also. Perhaps Goldstein's views do not imply what I am rejecting. But in that case he would need to show a greater willingness to assent to the realist view that is being advocated here than he seems to display in his arguments so far.

In fact Goldstein seems to waver over the case of observation. At times he implies that what he says of the inaccessibility of historical reality is also true of physical reality, at times he seems to deny this. He certainly speaks of his position with regard to history as 'anti-realistic'.[61]

But he argues that in the case of science 'in the world of the natural, present nature seems to impose itself brutely upon our awareness of it'. The problem is that he claims that this does not obtain in history. I would argue that in each case reality does impinge on our beliefs though I grant that it does so through the medium of scientific or historical method. The crucial factor in the case of historical beliefs is the situation which arises when fresh evidence overturns accepted beliefs. This happens more rarely in history than it does in physics, but when it happens it has a comparably devastating effect. (Pompa also criticizes Goldstein's anti-realism.)

Archaeological excavation or the availability of previously inaccessible documents are normal means whereby fresh data become available and confirm or discredit existing theories. Thus the strong suspicion of collusion between Israel, France and Britain in 1956 was eventually confirmed by the publication of memoirs by Anthony Nutting.[62] Had the alleged Hitler diaries been genuine these might well have confirmed or discredited many interim conclusions about the policy of Nazi Germany under Hitler. In the latter case however, even if the diaries had not been exposed as forgeries, historians would have needed to test the new data against the likelihood that even genuine diaries might well contain false and misleading statements. But such a situation is not unknown in the natural sciences where new experimental data which upset existing theories may themselves be suspect till further tested.

A better instance of the need to check and recheck fresh data which overturn existing theories is the highly controversial

case of the excavations at the K^{e}phar Nahum synagogue. When this fine white limestone synagogue was first discovered it was assumed by some to confirm the mention of such a first century building in the text of Mark. The text mentions such a building, and the discovery seemed to confirm the evidence of the texts. Unfortunately however it gradually became apparent that on stylistic and other grounds this building could not be as early as the first century. This left the upholders of the theory that K^{e}phar Nahum had a synagogue in the first century with a problem. Either such a building was under the present one, or there was such a building elsewhere perhaps on a site now under the lake, or no such building existed. Let us call these theories T, T' and T''. At first T was the preferred theory. Further excavation was eventually conducted in 1953-4 below the steps at the southeast corner of the building. Below the white limestone foundations of the later building could be seen the black basalt blocks of an earlier construction. This new datum was taken as confirming theory T. But further confirmation was considered desirable. Fortunately the site had not been cleared by total excavation as sometimes happens in other contexts. After 1968 trenches were taken through the floor of the present building. Underneath it was not a first century synagogue such as that recently found at Gamla. Rather there was an <u>insula</u> of houses. Theory T could now only be sustained if supported by an <u>ad hoc</u> auxiliary hypothesis that one of these houses was used as a synagogue. But such an <u>ad hoc</u> revision would only be rationally credible if it lends itself to further testing. For example if a new trench uncovered a

house with some traces of a menorah, or a niche for a torah scroll, or some other trace of cultic use, then the rational credibility of the revision of theory T would be confirmed. But no such further tests have yet been successful. Instead a fresh controversy has broken out over the claim that under the limestone building late fourth century coins were found which cause the latter fine and elaborate construction to be dated by the Franciscan excavators to early Byzantine times. Israeli scholars have cast doubt on this as they find it improbable that so fine a Jewish construction in this style could have taken place after the time when the Roman Empire had passed under Christian rule. This last point further illustrates the way in which fresh data are not themselves beyond question, but may be impugned in various ways.[63]

Now I have deliberately cast the account of the excavations in the form of a rational reconstruction of the argument in a way that illustrates the similarity between historical and scientific argumentation. I do not think that I have done violence to the history of the excavations in the process. There is here a parallel to the kind of testing of theories, and amendment of theories by supplementation with ad hoc auxiliary hypotheses which happens in the natural sciences. The need to subject theories so amended to further testing is a crucial point in the methodology of scientific research programmes as adumbrated by Imre Lakatos.[64] But the fact that this was what the historians and archaeologists saw to be necessary, and eventually performed, is precisely what supports my claim that in this instance the methodology of historical research and the methodology of scientific research programmes can be shown to have a similar

rational structure. Nor do I think that the process of reasoning in the historical example is untypical of historical research. Indeed the objectors fasten on alternative theories which would explain the presence of fourth century coins, and on general historical and stylistic considerations, but do not reject the essential outline of the process of reasoning which led to the successive investigations.

Theories about past events and past states of affairs are assessed as constructions designed to explain present evidence. Theories differ in simplicity and explanatory power, in internal consistency and in the extent of their consistency with other theories. Rational belief is in these instances a matter of rational theory choice. We are often faced with a set of rival theories, and need to offer grounds for preferring one over the others. We have already seen how judgements of comparative simplicity counted in favour of one theory of the Egyptian gnostic texts. Here in the case of the rival theories about an ancient synagogue we see the role of auxiliary hypotheses and the need to predict fresh data. A theory may be amended by the addition of an ad hoc auxiliary hypothesis provided that such an amendment offers a fresh opportunity for testing in a crucial experiment. History may not always provide such chances of acquiring fresh data but it is in principle possible, and with the ever more sophisticated techniques somewhat more practicable in the case of archaeology.

Even where fresh data are not immediately forthcoming one could cast ones beliefs about past facts into the form of rationally reconstructed beliefs containing predictions. Thus one could

argue that Cuff's theory about the duration of Caesar's Gallic command contains an implicit prediction. This would be that if some long lost text of Cicero were to come to light which commented explicitly on the issue it would favour Cuff's interpretation rather than its negation, or any of its rivals. Even in natural science it is sometimes conceded that the eventual testing of such predictions may take an indefinite time.

I have alluded to the way in which historical argumentation can be shown to conform to criteria which are deployed in the philosophy of science. As an illustration of the latter I propose to cite two passages from Lakatos. These represent a refinement of the view of scientific rationality developed by Popper.

> 'For the naive falsificationist any theory which can be interpreted as experimentally falsifiable, is 'acceptable' or 'scientific'. For the sophisticated falsificationist a theory is 'acceptable' or 'scientific' only if it has corroborated excess empirical content over its predecessor (or rival), that is, only if it leads to the discovery of novel facts. This condition can be analysed into two clauses: that the new theory has excess empirical content ('$\underline{\text{acceptability}}_1$') and that some of this excess content is verified ('$\underline{\text{acceptability}}_2$'). The first clause can be checked instantly by <u>a priori</u> logical analysis; the second can be checked only empirically and this may take an indefinite time.'

This position is then slightly reformulated later by Lakatos in the following manner:

'Let us take a series of theories, T_1, T_2, T_3, ... where each subsequent theory results from adding auxiliary clauses to (or from semantical reinterpretations of) the previous theory in order to accommodate some anomaly, each theory having at least as much content as the unrefuted content of its predecessor. Let us say that such a series of theories is theoretically progressive (or 'constitutes a theoretically progressive problemshift') if each new theory has some excess empirical content over its predecessor, that is, if it predicts some novel, hitherto unexpected fact. Let us say that a theoretically progressive series of theories is also empirically progressive (or 'constitutes an empirically progressive problemshift') if some of this excess empirical content is also corroborated, that is, if each new theory leads us to the actual discovery of some new fact. Finally, let us call a problemshift progressive if it is both theoretically and empirically progressive, and degenerating if it is not. We 'accept' problemshifts as 'scientific' only if they are at least theoretically progressive; if they are not, we 'reject' them as 'pseudoscientific'. Progress is measured by the degree to which problemshift is progressive, by the degree to which the series of theories leads us to the discovery of novel facts. We regard a theory in the series 'falsified' when it is superseded by a theory with higher corroborated content.'[65]

This position is defended with a series of examples of actual scientific problems which cannot be discussed here. Nor can

I engage at this point in minor disagreements over incidental points in this account. I agree with the main theses of this position, and wish to use it to claim that if this is an acceptable account of the rationality of scientific theories, then it can be argued that historical beliefs are at least as rational as scientific beliefs, when they satisfy the criteria outlined above.

In practice of course one must admit that the prediction of novel data is infrequently rewarded with success and one often has to be content with the conclusion that a series of theories or series of revisions of a theory, in historical study is 'theoretically progressive'. In such cases it is important to show either that rival series are not progressive but degenerating, or that on the other criteria the preferred series has clear advantage. There is no clear decision procedure in such matters. No one has succeeded in ranking the criteria and constructing a fully articulated method for deciding between theories. But this does not mean that such decisions are arbitrary. Criteria do exist, and have been listed above, and theories can be preferred over their rivals in the light of these criteria. The fact that some theories seem equally balanced in that they can each claim the support of different criteria does not exclude the fact that in other cases one theory can be preferred to its rivals.

There is one interesting and important issue which arises from the debate over Goldstein's work still to be discussed. This is the question of the provisional nature of historical judgements. Are we to say (with Walsh and others[66]) that common usage and historical conviction rightly lead us to say that some

historical judgements provide us with knowledge. Walsh argues that it is odd to say that it is only a well supported belief that George Washington was the first President of the United States. But Goldstein argues that statements are never irrevocably established. The problem is that each of these points is correct. It is certainly odd (though not false) to say only that it is a well supported belief that George Washington was the first President of the USA, rather than that we know this. Yet even if the statement is beyond serious doubt, it is not beyond all doubt. Perhaps he had a double who stood in for him, that he was killed before becoming President and his double took over his identity and his (then) future Presidency, and perhaps documents of overwhelming plausibility lie in an attic awaiting discovery. In that case we would have to withdraw our claim to knowledge. But we would also have to withdraw the claim that we have a well supported belief (even if we had had a well supported belief until its support was undermined). Even if we had said 'I believe G.W. to have been the first President of the USA' we would in this case have to abandon the belief. And even the mere assertion 'G.W. was the ....' would have to be negated. In other words the problem of withdrawal may function slightly differently, in different contexts, but it is pervasive. Claims to know, to have well supported belief, or just to believe, and also simple assertions are all subject to withdrawal in one form or another, if fresh evidence upsets them. My tactic in focussing on rational belief in this work is not intended to bypass the fact that it is not only claims to know which may need to be withdrawn.

Walsh is surprisingly willing to envisage a radical approach to the problem. He writes:

> '... once absolute facts go, the way is open to a radically new conception of truth and fact generally. I agree that no such view has so far been worked out with any seriousness. But I suggest that it might be, and that Goldstein's arguments point towards it ...'[67]

Perhaps one should not make too much of what in context is a more guarded concession, but this so sharply conflicts with our normal usage of the terms 'truth' and 'fact' as to make it implausible that such a revision could be coherently carried through.

The most acute problem however to my mind is not this but rather the undecidability of certain statements especially in history. A realist will argue, as I do, that past states of affairs were as they were, and if we reason truly about them, we know the facts (assuming that our beliefs about them are undefeated justified true beliefs). I can argue, as I have, that past reality exerts pressure on present theory via fresh evidence. But what of those cases where fresh evidence is not there to become available? The realist is obliged to argue that historical statements are all either true or false, but has to admit that many may actually be undecidable.[68] If we have rival theories about some poorly evidenced period of pre-history it could be the case that no fresh evidence ever will throw light on the merits of the rival hypotheses. One could argue in such a case that it is rational either to believe whichever of the theories better satisfies our other criteria, or to suspend judgement if we deem them equally

plausible on such grounds. Can we in such cases eliminate the recourse to prediction? We could do so if we knew for certain that further evidence was not to be had, but this is not how things are. Even where further evidence is not in fact available it may not be certain that it is not available. But if we have good reason to believe that further evidence is not to be had, we might rationally decide between rival views on the other criteria without invoking the criterion of fruitfulness in making successful predictions. (This last concession may have a bearing on issues of a more metaphysical character to be discussed in the next chapter).

Summary and Conclusion

We must now take stock of the argument in this chapter. Certain crucial questions have been explored from different angles. Of these the most important is the extent to which the evaluation of the rationality of a belief varies in ways that are determined by the character of the belief in question. I have argued that in the case of beliefs about historical events as in the case of choice between rival scientific theories, certain criteria can be specified. While a limited class of beliefs about matters such as coin spinning and dice throwing can be assessed in a simpler manner and by the use of the classical calculus of probability, most other beliefs are evaluated in a more complex way. With these it is a question of assessing the relative merits of rival theories in terms of criteria. These criteria include internal consistency, consistency with other accepted beliefs, comparative simplicity, explanatory power, and fruitfulness in making predictions.

One can point to a broad similarity here between the use of these criteria in the natural sciences, and their use in various aspects of historical study. Historical explanation is one particular example where evaluation by these criteria can be shown to be relevant to assessing the rationality of historical beliefs.

Yet though one part of the argument of this chapter is to draw attention to certain similarities in the evaluation of the rationality of beliefs in history and in the natural sciences, certain important differences have also been noted. The inaccessibility of the past is a constant problem for historical study, but an issue which impinges much less on the natural sciences. The differences between memory and sense perception are a further factor. The role of evidence in relation to theories about past states of affairs differs in certain ways from the way evidence is used in relation to scientific theories about states of affairs that can be replicated, or that can be predicted even though they have not yet been observed. But though the role of prediction is much more dominant in the assessment of scientific theories and much rarer in historical study, the role of prediction is of great importance. One could rationally reconstruct many historical arguments so as to make it evident that they contain implicit predictions.

How then does Mitchell's analogy[68a] stand? It is important to note that Mitchell in his own way agrees that the difference between the rational defence of historical theories and that of scientific ones is not to be exaggerated. He himself appeals to the work of Kuhn and of Lakatos in the philosophy

of science. The philosophy of the natural sciences admits of a good deal of sophistication about the relationship between theory and the criteria by which theories are deemed rationally acceptable, and I note that, though it is beyond the scope of the argument of this chapter to pursue it further. In the case of historical theories and the rational credibility of statements about the past one can argue that many of the criteria deployed in the natural sciences are indeed applicable, once allowance is made for those differences that have been specified above. The most crucial of these is of course the infrequency of successful prediction and the fact that in certain cases historians rationally and wisely deem such a test not to be available. In the next chapter I propose to consider the arguments put forward by Swinburne that the criteria we have discussed can be used not only in assessing arguments in science and in history but also in assessing the rational credibility of metaphysical beliefs. That is a highly controversial issue but one which is hardly irrelevant to the study of rational belief. It is therefore important to give careful and critical attention to his arguments.

Chapter 5 Criticism of Swinburne's Case for Rational Metaphysical Belief.

The argument so far has considered several different aspects of rational belief.--Our starting point was the relationship between belief and knowledge, and then the question of what makes some beliefs rational. In considering rational belief it was noted that controversy exists over the status of historical and metaphysical beliefs and their relationship to the criteria of rationality employed in the natural sciences. In the case of historical beliefs I have argued that they can be held to be rational if they satisfy criteria comparable to, but only slightly different from, those used in the philosophy of science. This conclusion maintains two theses. The first is relatively non controversial, namely that at least some historical beliefs belong to the category of rational belief. The second is both more debatable and more interesting. It is that in accepting the rationality of historical beliefs we largely defended them by attempting to show that they satisfy the criteria currently accepted in the philosophy of science, but argued that some modification of these criteria was appropriate in the special circumstances pertaining to the assessment of historical beliefs. The chief modification was that though historical beliefs contain predictions, the possibility of testing these predictions arises less often than in many of the natural sciences. Even here, however, some comparability with certain areas of scientific investigation was noted. The study of past states of the universe and of past states of animal evolution provide examples.

The next step is to ask whether or not a further extension

of the sphere of rational belief should include metaphysical beliefs. This issue has already been raised in a preliminary way. At an earlier point it was noted that Mitchell argued for an analogy between historical and metaphysical beliefs. While some criticism of that analogy has already been offered, its real test was deferred to this chapter. In order to examine the analogy further, I wish to examine the rationality of the class of metaphysical beliefs which Mitchell had in mind, namely theistic metaphysical beliefs, and more specifically the type of arguments put forward by those upholders of Christian theism who maintain that they can defend their views by rational argument. In Mitchell's book on the justification of religious belief, he does more to establish the general character of a rational defence of theistic belief than to advance detailed arguments on specific points. For this reason I propose to take recent work of Richard Swinburne as a further example of one who undertakes the rational defence of theistic belief. I do not claim that Swinburne is at every point in agreement with Mitchell, merely that he provides a more detailed example of the type of programme which Mitchell proposed.

Swinburne maintains that theistic belief can be defended by inductive argumentation. By this he means that it can be defended by arguments which raise the probability of the desired conclusion. Indeed he goes further. He maintains that 'On our total evidence theism is more probable than not'.[69] In other words, in his terminology, he claims that his total argument is 'a good P-inductive argument'.

While maintaining the reserve I expressed earlier about the

use of the term 'inductive', I am, for the purposes of debate, willing to acquiesce in Swinburne's terminology that a P-inductive argument is an argument which renders its conclusion more probable than not. The question is whether Swinburne's arguments can indeed perform this task for theistic belief. I have chosen Swinburne's work for critical discussion because it represents a significant attempt to argue for the rationality of theistic metaphysics, and because it does so in an original manner by using confirmation theory. Though I think Swinburne's arguments may not in fact establish the conclusions that he desires, many writers have hailed this work as a highly significant contribution to natural theology. Thus Crombie says of it that it is 'an excellent book which, in conjunction with its predecessor, has done a vast amount to forward discussion in their subject'.[70] Penelhum also praises Swinburne's earlier volume with the judgement that no defence can compare with this in the quality of its arguments or the clarity of its thought.[71] Though the response to these volumes has by no means been uncritical, it is clear that, if we wish to consider the merits of arguments for the rationality of theistic metaphysics, Swinburne's trilogy provides what many consider to be a most significant recent contribution to the subject. The critical scrutiny of these types of argument is therefore of central significance for the current work, so they will provide the main (though not the only) focus for debate.

The issue here at stake in the discussion which follows is this. It is whether or not metaphysical beliefs can be rationally appraised. To this I give a positive answer. I maintain

that the beliefs under discussion are meaningful, and that they are either true or false, and that it is appropriate to evaluate whether it is rational to hold them. I also maintain that criteria comparable to those used in the philosophy of science and in the philosophical appraisal of historical writings are appropriate in this field also. We can assess the epistemic probability of such beliefs by comparing these beliefs with rival theories. In evaluating the rational superiority of one theory over its rivals we need to assess their accuracy, internal consistency, explanatory scope, simplicity, and fruitfulness. This is the main issue and I propose to argue that these metaphysical beliefs are a proper subject for rational debate.

But a second issue is beyond our scope. This is whether as a result of such debate it is (epistemically) rational to hold these beliefs. In order to give a positive answer here it would be necessary to provide an argument which showed that theistic metaphysical beliefs are more probable than rival views, or more probable than the negation of such beliefs. Swinburne claims that he has done this. His claim is therefore in effect a claim to have restored natural theology as a successful intellectual venture. This is a bold claim, but I am not sure that it is successful. I think that Swinburne has shown how a restoration of natural theology might be possible, but I do not think that he has in fact provided arguments which effect what he desires to effect. If a negative judgement on this aspect of Swinburne's work is correct then several possibilities remain and it will be relevant to outline them. One might

infer that if this defence fails then all attempts at natural theology must fail. But again though such a view is frequently ascribed to believers as well as to non believers it can be argued that it is premature and inadequately grounded. A further possibility is that a rational defence of theistic metaphysics might in principle be possible, but has not in practice been achieved. Our purpose however is rather different. It is to explore the question of the rational criteria to be used in evaluating metaphysical theism. It is with this issue of criteria for rational belief that we are concerned.

One immediate objection to the whole enterprise attempted in this chapter is that it does violence to the character of metaphysical beliefs to treat them as comparable to beliefs about matters which are empirically decidable, or at least in principle empirically decidable. Some philosophers might wish to object that there is need of a demarcation between empirical and metaphysical assertions and that the former can be tested by criteria for rational belief but that the latter cannot. Also some believers and some philosophers of religion might wish to object that theistic belief is not a matter of assessing probabilities but involves a total commitment which cannot be harmonized with the policy of assessing the probable truth of one's beliefs. I propose to consider these two lines of objection in sequence, though briefly.

The construction of a demarcation between scientific and non-scientific statements has been a constant motif in contemporary philosophy. Let us characterize one main aim of philosophy as the attempt to correct our intuitions and our judgements. Our intuitions and our judgements do not form a consistent set.

So some intuitions or judgements need to be revised or abandoned. This happens even within logic as can be seen from attempts to reduce or resolve logical paradoxes. (See for instance the attempts by Prof. N. Tennant to avoid the Lewis paradox).[72] In the field with which we are concerned, one possible judgement is that scientific statements are somehow more reliable than historical ones. But in the philosophy of science appeal to the history of science is itself necessary to the exploration of scientific statements. A further possible judgement is that the conclusions of progressive research programmes in the natural and human sciences are acceptable, but that metaphysical statements fall into a different and less acceptable category. But in order to maintain such a view several arguments need to be carried through successfully. First one would need to show that conclusions reached in the natural and human sciences do not depend on any metaphysical assumptions contained in the theory base from which those conclusions are derived. If there are metaphysical assumptions in that corpus of theory one would need to exempt at least some metaphysical statements from relegation beyond the pale of acceptability. If for example we class a realist theory about the existence of past states of affairs as necessary to our historical research, and so as necessary to our view of the extent and character of past scientific research then at least one metaphysical belief can be identified within our class of assumptions from which the conclusions of progressive research programmes are derived. This argument may not avail against views like those of Goldstein, but I have already offered criticisms of his position in the

previous chapter. Further even if one could show that the natural and human sciences operated without being dependent on any metaphysical assumptions one would still have to offer some reason for accepting findings in these fields but rejecting the entire class of metaphysical statements. Or if one admits that there are metaphysical assumptions used in the sciences one would have to say why these statements are acceptable and others not. If it is replied that sentences used in the sciences are acceptable because they or inferences from them satisfy criteria of rationality such as those outlined above, then it becomes necessary to show that other metaphysical statements neither satisfy these criteria nor criteria largely equivalent to them. In other words the objector has to engage in the argument proposed in this chapter and not dismiss it in advance. If there is a demarcation it is not between scientific and metaphysical statements as such, but between those statements which satisfy the criteria and those which do not.

The second line of objection comes from the opposite direction. Some believers might insist that certain metaphysical beliefs, such as theistic belief, involve a total commitment to a way of life, or to a way of seeing the world, and that one must either stand within it or outside it, and there is no way in which one can stand aside and assess the alternatives dispassionately. Another argument is that what is involved in becoming a religious believer (or in choosing between God and Mammon)[73] is not a matter of assessing epistemic probabilities. A further way of expressing the objection from this quarter is to insist on a distinction between belief in God and belief that God exists. These three forms of the second line of

objection are interrelated but not identical. I propose to discuss them in the reverse order.

The distinction between belief in God, and belief that God exists is undoubtedly important and must not be ignored but equally it should not be exaggerated. Its importance lies in the fact that most theistic believers would insist that it is inadequate to describe their position as one of holding the view that God exists, or that it is probable that God exists. An associated line of argument would reject the term hypothesis in this connection. Most believers would claim that their belief is held with far greater conviction than a mere hypothesis, that it entails not just recognizing the existence of God but a relationship of gratitude, faith, love, and obedience to his moral demand. Anyone who offers an account of theistic belief which would be rejected as a misleading description by the majority of those holding that belief should certainly be subjected to critical scrutiny. One essential point here is a distinction between faith and belief. In other contexts the two terms are often interchangeable. But in this context faith more often has connotations for believers of grateful acceptance of a redemption which no merely human agent can achieve. Indeed in biblical contexts even the words translated by 'believe' and 'belief' often have such connotations. But in Swinburne's writings belief in God almost always has its more philosophical sense of holding the view that God exists. This distinction is of great importance, but two further observations must be made. It is not possible to maintain the wider sense of the term faith without conceding that belief that God exists

forms an indispensible element in it. It is correct to say that in religious discourse faith means the acceptance of benefits from God. But those who believe that they receive benefits from God can only do so if they believe that God exists and that he has the character they attribute to him. Philosophical debate has focussed on these latter assertions, as it is these which hold a certain epistemic precedence. But that is not all that it is appropriate to say. Philosophical discussion is chiefly interested in the support or lack of support for these beliefs. It has therefore tended to focus, as Swinburne's book does, on cosmology and on questions about the existence of God. But if most Christian believers would in fact insist that beliefs about redemption and beliefs about the nature of Christ are more central to their position, then any philosophical discussion which focusses exclusively on other aspects fails to discuss with accuracy the beliefs that are actually held. It is this which gives an air of unreality to much philosophical discussion of theistic belief.

I grant wholeheartedly that if one is to discuss the beliefs of a community one must do so in a way which accurately describes the character of those beliefs. But I do not agree with attempts to resist rational enquiry into the grounds on which such beliefs are held. The greater the conviction with which a belief is held, the more important it is to test that belief against its rivals. The more a belief determines a whole pattern of behaviour the more important it is to ensure that that belief is a true belief. This point is implicitly conceded by dedicated believers who are usually amongst the first to criticize what they hold to

be false alternatives to their beliefs. In his review of Swinburne's conclusion to the trilogy D.Z.Phillips argues that the gain or loss of religious belief is not accounted for by reasoning, and that those who choose between God and Mammon do not do so 'according to common criteria in an assessment of probabilities'.[74] He is correct to say that more is involved in such decisions than pure calculation, but quite quite mistaken if he is asserting either that people do not investigate the probable truth of their beliefs, or that they should not do so.

The view that people do not, or do not need to attempt to examine the probable truth of their beliefs would follow if it were true that such an investigation is impossible. If it really were the case that there are different and incommensurable ways of viewing the world then no comparison between these rival views would be possible. Let us call this view the incommensurability thesis. It is similar to the argument over whether rival scientific paradigms are incommensurable, an issue which has been much debated in the wake of Thomas Kuhn's remarks on the subject. In the case of Kuhn the thesis is that rival scientific paradigms cannot be compared with one another as they each describe the universe in terms which are theory-laden. Mitchell has argued that there is indeed a similarity between controversies over metaphysical beliefs and controversies between upholders of rival scientific paradigms.[75] Mitchell is however too cautious to infer radical incommensurability or radical meaning variance from this. It is one thing to say that the upholders of rival paradigms often misunderstand one another, but quite a different thesis to say that these views cannot be compared with each other. The latter is a false

inference from the view that observations are theory-laden. I accept the view that observation or experience is theory-laden, but not the more radical inference.

That theories are totally incommensurable is in any case implausible if there are tests which, for instance, favour one paradigm against another. That there are such tests accounts for scientific preference for Einstein against Newton. But perhaps what is meant is only that theories are incapable of complete comparison owing to meaning variance. But even that is questionable. It is argued that one set of astronomers observed stars in different locations whereas another astronomer identified the successive positions of a planet. But while we could express this as one group of scientists experiencing stars where another experiences a planet this is not the only way of expressing the matter. We can and do modify the theory by which we describe experiences and observations. While there is no observation without some theory, observation is not only expressible in terms of one particular theory. Thus in this example we could say with greater accuracy that at t_1 A observed a bright object at l_1, and at t_2 B observed a bright object at l_2, and at t_3 C observed a bright object at l_3, and D inferred that there were stars at l_1 and l_2 and l_3. We can then compare this with the view that at t_4 E calculated that observation of a bright object at l_1 at t_1 and at l_2 at t_2 and at l_3 at t_3, would be better explained by the existence of a planet whose orbit included l_1 and l_2 and l_3. We <u>can</u> compare these rival views and they are not incommensurable.[76]

But perhaps it could be argued that while incommensurability

has rightly been rejected in discussions of Kuhn's views on science, there might be a legitimate case for it in metaphysics. Thus D.Z.Phillips argues

> 'Religious language is not an interpretation of how things are, but determines how things are for the believer. The saint and the atheist do not interpret the same world in different ways. They see different worlds.'[77]

It is notoriously difficult to pin down exactly what it is that Phillips is saying, especially as he combines language of determined commitment with approval of the view that a sentence such as 'God exists' is not a statement of fact.[78] The most straightforward way to understand this would be to say that the faith of such a believer that God cares for him means that he approaches everything in life with hope and confidence. It is argued that such a view means more than the reduction of belief about God to a determination to live in a loving (or hopeful) way. But if there is more, it is difficult to give content to this additional meaning without making statements in the indicative mood. But this Phillips refuses to do. Also such a position does not do justice to the conviction of the great majority of believers past and present that their belief in God does at least include some statements of fact.

Now I admit that there is a sense in which someone's attitude determines how things are. For instance the attitude of a community of believers determines whether a disaster is seen by them as an occasion for despair, or as an opportunity for remedy and hope. But this, though an element in traditional theistic faith, is only part of a larger whole and to treat

it as the whole is to engage in a content reducing stratagem. A better description of the matter would be that faith sees the world not as an arena for proving oneself but as a place where persons receive their value and their inspiration to goodness from a source outside and beyond themselves. But this account of faith requires statements in the indicative mood. I grant that much of the language of the mystics, and that of the Sufi tales, and possibly (on some interpretations) that of the parables of Jesus may be interpreted as differing from statements of fact, though I happen to hold that indirect statements of how things are do play a role in the latter example. But the doctrinal utterances found in the Apostle Paul and in the gospels, and in the classic creeds and confessions do contain or entail assertions, and this is how the great majority of believers, and their critics, see the matter.

Our concern is with the evaluation of rational belief. Within that class this chapter is concerned with two related questions. Are metaphysical beliefs open to rational criticism, and if so what are the rational criteria to be used ? The first of these questions would receive a negative answer if it could be shown that metaphysical beliefs are not amenable to rational scrutiny. I shall argue later that theistic metaphysics is open to rational scrutiny, but in order to do that a little more attention must be given to those other than Phillips who maintain that rational scrutiny is out of place in the case of these beliefs.

It could be argued that theistic belief is such that if true it requires total commitment whereas rational enquiry

could only result in a qualified assent which is inappropriate to religious faith. There is a point of substance here and it needs to be clarified and discussed. There is a place for passionate loyalty and commitment but to argue that such commitment should be made to a creed regardless of any attempt to discover the truth or falsity of that creed is gross irrationality and liable to justify adherence to creeds which have brought about great evils in the course of history. This objection might be avoided if a shift is made from emphasizing the objective character of what is believed, to stressing the element of passionate conviction in the believing relationship. (Some interpretations of Kierkegaard take this line)[79] Now if this view were amended to one which praised passionate commitment to the pursuit of truth and goodness it would have more to commend it. But there would then be no reason to condemn the rational pursuit of truth in the area in which we are concerned. Indeed the enquirer who commits himself utterly to the pursuit of truth could more appropriately be portrayed as the exemplar of subjective faith, rather than as a passionless researcher doomed to a futile pursuit. But the fact that one can use such emotive portrayal to justify opposing positions suggests that this is no way to conduct an argument.

The grain of sense which can be found in the contention we are considering is this. It might be the case that theism demands total loyalty while being such that it can neither be proven nor disproven, nor even shown to be probable or improbable. But against this several points can be made. When rival claims to such loyalty are defended in a similar

manner the edge of this line of argument is blunted. There are rival positions, a choice between them is needed, and so some attempt to assess at least the probability of the rival metaphysical views should be attempted. This point is strengthened when one remembers the evils to which blind loyalty has led people. It is further strengthened when one remembers that the great majority of traditional theists have held their beliefs to be rational, and at least probable if not more. Again the greater the demand for commitment the more important it is to seek out the truth of that to which commitment is demanded. If these principles are granted it would follow that those who urge loyalty to a deity who commands total commitment without providing evidence or ground for the truth of that to which one is to be loyal are urging loyalty to a being who falls short of human standards of goodness.

These objections would carry less force if theism were cast in the form of belief in an ideal of goodness. (Such a view is not wholly distant from that defended in Iris Murdoch's Gifford Lectures of 1982).[80] But that would entail a considerable reshaping of traditional theism. That in itself is not an insuperable objection. It could be the case that such a revision proved more rationally acceptable and open to fewer objections than its rivals and so be deemed closer to the truth. For the present I merely note this possibility while recording one line of objection to it. If such a view were cast in the form of a demand for moral striving[81] it would be inconsistent with one of the prime insights of the theistic tradition stemming from Paul, Augustine and the Reformers that faith is not so much a matter of moral striving as of

the recognition that one's value and one's inspiration come from a source other than the self. All the same the issue is one of considerable interest and importance. Whether a view of this kind could be developed in a way which can overcome objections from this quarter as well as from anti-Platonists is an issue which cannot be pursued at this moment but which may reappear later.

That there is a need for a commitment which exceeds the rational support for theism is also maintained in a more guarded form by Hick.[82] His argument is that God has created a world in which his presence is not unambiguously revealed. The reason for this is to enable human response to him to be free. If God were known to exist, his being is such that the knowledge of his existence would make a total difference to us and we would not be in a position of freedom to choose to respond to him in faith. Hick cites Irenaeus (Haer. 4.37.5) in favour of the traditional character of this view. Hick's argument is quite different from that of Kierkegaard, and is used to supplement rather than to denigrate rational enquiry. In this respect it does not offer an objection to the kind of rational enquiry undertaken by Swinburne and selected for criticism here. But Hick's position requires the view that the balance of probability is about even. In this he differs from Swinburne for the latter holds theism to be on balance more probable than not. Perhaps this difference is not too serious as for both writers the deciding factor is religious experience which is available only to the believer. Hick's argument therefore demands rather than rejects the kind of rational enquiry

envisaged here. It does however maintain that more than rational investigation is at stake, as well as requiring the rational scrutiny of arguments which is to follow.

Hick's view is that the ambiguity of the evidence can nonetheless permit belief in theism to be defensible, as a certain 'epistemic distance'[83] between God and humanity is a necessary condition of human choice in the matter being genuinely free. But this appeal to a necessary epistemic distance is not without its problems. One objection is that classical Christian theism held that angelic beings knew God and yet rebelled. So in terms of one biblical and patristic story rebellion and lack of epistemic distance are not inconsistent with one another. (Interestingly the writer of the Epistle of James says 'the devils believe' when describing this situation. Here is an example of a case of what the writer presumably might equally have called knowledge being described in terms of belief.[84]) But this objection does not destroy Hick's case, as his version of theism abandons this particular mythical element.[85] Hick sees that his defence is inconsistent with part of the tradition and disowns that element in the tradition.

More serious objections can be brought against Hick. If he holds that the evidence is ambiguous, but that nevertheless his version of theism is to be preferred to an alternative reading of the evidence, why is the benefit of the doubt to be given to the one view rather than to its alternative? Some additional non epistemic argumentation is here needed. If it is available then what is offered is a rational but non evidential ground rather than a purely voluntary choice. If it is not available then the voluntary element in faith is in danger

of being arbitrary rather than just voluntary. This situation is compounded if the ambiguity of the evidence is such that there is not just a choice to be made between one view and its alternative but between several rival views. If ambiguity (or as Hick calls it 'epistemic distance') means that after consideration of the evidence several metaphysical beliefs all seem equally probable then the difficulty of defending a choice of one of them against the others is intensified.

I have spent some time discussing the views of those who wish either to resist or to qualify the contention that metaphysical beliefs are open to rational scrutiny. My aim has been to give as large a place as possible to rational enquiry. But we shall see later that the ranking or weighting of rational criteria may be differently estimated. This would of course help to explain how rival views may remain in conflict even though those who hold them agree that they should assess their beliefs by rational criteria. But we must now turn to consider Swinburne's method of arguing for the rationality of theistic metaphysics.

Swinburne's Programme.

I will use the term Swinburne's programme to define the series of arguments which he uses chiefly in the second volume of his trilogy. This is the volume entitled The Existence of God. In it he proposes to use what he calls inductive arguments for the existence of God. By this he means a series of arguments of a non deductive kind. Within this series he distinguishes between those arguments which are confirmatory (and which raise the probability of his conclusion), and the cumulative effect of his arguments which he claims renders theism more probable than not. This distinction is expressed in his terminology as the use of C-inductive and P-inductive arguments.

A C-inductive argument merely makes a conclusion more probable than it was previously, whereas a P-inductive argument renders it more probable than not. I shall accept this terminology for the purpose of discussion.

Much more debatable is the fact that Swinburne uses a form of Bayesian confirmation theory to assess the weight of his arguments. He wisely refrains from attempting to give precise numerical values to his equations, and limits himself in the main to considering whether a particular probability is high or low or very low, or greater than the probability of another item. But the use of Bayes's theorem is itself controversial, and this is one of the main elements in his argument which cannot simply be taken for granted, even for the sake of discussion. I therefore propose to consider Swinburne's programme in two distinct ways. One is to draw attention to doubts about the proper applicability of Bayes's theorem. This is a complex matter and I am well aware that Swinburne has presented his reasons for taking his view of the matter at length in his book An Introduction to Confirmation Theory.[86] But the reasons for dissent in this area are sufficient for this issue not to be ignored. I therefore propose to indicate briefly why I think that the version of Bayesianism proposed does not do justice to the problem of theory choice and hypothesis evaluation in this area. I then propose to continue the argument by maintaining that even on Swinburne's Bayesian view the arguments are not as compelling as he claims.

This strategy is I believe strongly defensible. The major arguments need to be assessed whether one is using

Bayes's theorem or not. If it is possible to maintain that Swinburne's case cannot be carried either when evaluated within his formal system or when evaluated more informally then we have a significant conclusion. I believe this to be the case and will present arguments to that effect.

But first I must outline in a provisional way my reservations about the use of Bayes's theorem in this context. The theorem states:

$$P(h/e.k) = \frac{P(e/h.k)}{P(e/k)} \times P(h/k)$$

This is to be read as follows: the probability of h (the hypothesis) on e (the new evidence) and on k (the background knowledge) is equal to the probability of the evidence given h and k, divided by the probability of the evidence on k alone, and multiplied by the probability of the hypothesis on k alone. P(h/k) is also called the <u>prior</u> probability of h. The theorem itself is deducible from the axioms of the probability of chance. Naturally I do not reject the calculus of the mathematical probability of chance events when it is in its proper domain. Nor do I deny that Bayes's theorem is deducible from it. But it is far from agreed that the evidential probabilities of confirmation theory are to be treated as the mathematical probabilities of chance. In the case of games of dice and cards and marbles the two types of probability may well coincide. But even there there are problems. Even in the simple case of spinning coins, horrendously complex arguments divide the statisticians into rival schools. If the bias of a coin is given we can, by the calculus, estimate the probability of the next spin producing heads. Let us call the bias θ. Given θ the <u>direct</u> inference can be calculated. But the <u>inverse</u> inference is

problematical and notoriously so. If θ is unknown and we have the results of n spins, can we use the calculus to estimate the value of θ? If the theorem in question is used we are involved in assessing the prior probability that the bias of the coin has a certain value. Frequentists object that this admits an unwarranted element into the calculation. It is also argued that using the principle of indifference leads to incoherent results.[87] If there are problems even with examples so close to those where the mathematical calculus works well, then a fortiori we should expect worse problems in domains more remote. (Also in domains with infinite quantities the Dutch Book defence[88] is of no avail).

Swinburne defends the applicability of the theorem by arguing that it satisfactorily explains the rational choice between scientific theories. He also gives a brief example involving the inference of guilt of committing a crime from evidence and prior probabilities.[89] This latter example is particularly unfortunate. Swinburne does not mention that this issue is hotly contested. Swinburne's example runs as follows. The hypothesis h is that Jones robbed Barclays Bank, e is the evidence that he was near the bank at the time of the crime, and k the background knowledge that Jones robbed another bank, Lloyds bank, on another occasion. Swinburne maintains that the probability that Jones robbed Barclays Bank is the prior probability that he did it, multiplied by the extent to which the hypothesis makes e more expected than it was on the background knowledge. This example is open to two serious objections. It incorporates an estimate of the prior probability

of the guilt of the accused, and it incorporates as background evidence knowledge (based on a previous conviction?) that Jones robbed another bank. Both of these elements are utterly inadmissible in a court of law. The rules of assessment of legal evidence in Keele as in New York absolutely forbid the kind of procedure being recommended here by Swinburne. There are further difficulties in the details. How for instance would one calculate the prior probability of guilt even if the court allowed such a consideration? Paradoxes arise from the use of the axioms of mathematical probability at this point as Cohen has shown.[90] This is more serious.

Swinburne's appeal to decision between scientific theories is better presented but still controversial. He cites examples from Newton and Kepler and Einstein. From these he infers that the prior probability of a theory is determined by its simplicity, its fit with theories in a neighbouring field and its narrowness of scope.[91] He then says 'For large-scale theories the crucial determinant of prior probability is simplicity'.[92] Now many do agree that simplicity, scope and fit are values for theory appraisal, and that the history of science exemplifies these (and other) values. But it does not follow from this that these values are used to assess the <u>prior probability of a theory</u>. That we should be asking about the prior probability in this way is simply assumed and not shown by Swinburne. That simplicity is the crucial determinant of the prior probability of large scale theories assumes both that we should be assessing theories on prior probability and that simplicity is the way to do it. Now I grant as I have argued above that <u>one</u> of the criteria for rational theory choice is simplicity. Therefore it is

worth arguing against Swinburne later that his theory is not as simple as he claims it to be and this will be a main element in the argument which will follow later. But Swinburne too readily reduces the criteria to those that fit his Bayesian scheme. He is on better ground when he argues that the broad scope of Newton's theory is offset by its great explanatory power.

Swinburne's use of Bayes certainly brings the criterion of explanatory power into prominence. In his terms this is the capacity of a theory to entail or make probable 'the occurrence of many diverse phenomena which are all observed to occur, and the occurrence of which is not otherwise to be expected'.[93] For a Bayesian this means that explanatory power is a function of (i.e. is determined by) the $p(e/h \,\&\, k) \div p(e/k)$. But the formula itself does not tell us how the formula is to be used. Swinburne in practice starts with k as background information of a tautological[94] character. This is one standard procedure. For this method, k consists of the truths of logic and mathematics. Then e represents empirical evidence additional to the background knowledge (k). That means however that in the first stage one is assessing the prior probability of h, and of e, on k alone, where k is tautological. Swinburne does just this at the outset. But the calculation of such prior probabilities where k is tautological means that one is involved in assessing the intrinsic probability of h, or likelihood of e on no empirical evidence, and that we have already argued to be a source of great difficulties, and in at least some cases inconsistencies.[95]

The subsequent stages of Swinburne's argument gradually

feed in additional evidence as the new e, and incorporate the e from the earlier stages into the new k. Readers of Swinburne unfamiliar with Bayesian conditionalization may initially find this confusing, but it is standard procedure in some quarters. The merit of this procedure is that it tries to capture our judgement that cumulative evidence builds up a case. We start with k as minimal tautological knowledge and assess h on e_1 and k_1. Then we add e_2, and our new k_2 is e_1 & k_1 and so on. Though this is standard for Bayesians, there are however those like Cohen who argue that the corroboration of inductive probabilities generates paradoxes if the calculus of mathematical probability is used.[96] I grant that cumulation or corroboration occurs, even if the mechanism by which it is calculated is contested. I also grant that in assessing theories we need to know if the empirical evidence is more likely given the theory (or hypothesis) in question than on its rivals. I also grant that Swinburne is correct in saying that if several theories explain the evidence equally well, then we have to use some other criterion or criteria to decide between them. But here we strike a major point of conflict.

Swinburne insists that given equal explanatory power it is simplicity which decides between hypotheses. He therefore gives enormous weight to simplicity as a decisive factor and goes on to defend the probability of theism to a large extent on the grounds of its simplicity. On this issue it will be sufficient if I can later show that he is overestimating the simplicity of theism, but I also hold that he overestimates the role of simplicity as a criterion. He argues that there is an infinite number of theories which would make the evidence

equally likely.[97] It is therefore necessary to choose between them on grounds other than explanatory power, and Swinburne argues, it is simplicity which does this. Now I agree that simplicity is one of the criteria used, and that it may rule out many possible theories which are never even seriously considered. But when there are serious rival theories with equal explanatory power the decision between them is decided by comparative simplicity only when all else fails. The normal procedure for deciding between serious rivals would be to devise a 'crucial experiment'. Now I grant many qualifications need to be added about decisions between theories modified in the light of crucial experiments. It is of course almost always possible to modify the protective belt of auxiliary hypotheses in order to defend a theory which is at a disadvantage as a result of test by crucial experiment.[98] But Swinburne places enormous emphasis on simplicity and says extremely little about fruitfulness in making predictions, or about crucial experiments, or about the problems of the ad hoc adjustment of theories. Even when he does reject ad hoc addition of a hypothesis to a theory he does so in terms of loss of simplicity.[99] But the whole point of much discussion of scientific method is the regulation of ad hoc adjustment by requiring fresh testable predictions. This is also the point, if I understand it correctly, of Bayesian conditionalization. The device of amplifying e is intended to capture the importance of finding fresh evidence. But Swinburne plays down this element in Bayesian conditionalization. When Swinburne sets up his hypothesis h all the evidence is already to hand. He feeds in the evidence bit by bit, but none of it is novel evidence. This point is explicitly admitted

(on page 67) but its implications are not discussed in any detail.

It may be that in the case of large-scale metaphysical systems like traditional theism there is no prospect of fresh evidence. It could be that the idea of devising a crucial experiment, or looking for new evidence is out of place here. But that has to be argued, and would require defence. It would mean that there is a major difference between the criteria used to assess theism and those used in scientific method. Nor would the analogy from history provide a refuge from the problems at this point. I argued above that even though the discovery of fresh evidence is less frequent in historical research than in the natural sciences it still plays a crucial role when available. But in any case one could well argue that fresh evidence has impinged on metaphysical theories at least in an indirect manner as I shall now argue.

For several reasons I prefer the term theory to hypothesis for the categorization of the central core of a view under assessment. Let us call the metaphysical core of theism the theory and let us call its related hypotheses about historical and scientific matters auxiliary hypotheses. These hypotheses have been upset by the discovery of fresh evidence. For instance the geostatic hypothesis had to be abandoned under the pressure of fresh evidence. Also the hypothesis of the historical accuracy of the biblical texts has been constantly upset by archaeological and other findings. It can be argued that fresh evidence of this kind only affects the protective belt of auxiliary hypotheses and not the central core of metaphysical theistic theory. But the loss of auxiliary

hypotheses by ad hoc removal is as criticizable as the addition of ad hoc supplementary hypotheses without fresh warrant. It is a content-reducing strategem. This issue arises from a consideration of the criteria for assessing non-deductive inferences. It should not be confused with the older debate over falsification as a criterion of meaningfulness. The two issues are very different. The one arose from the discussion of what it is for a statement to be meaningful. The current issue arises from consideration of what makes a research programme degenerative or progressive.

It counts against Swinburne, though in this instance not against Bayesianism, that he drops this criterion. It is ironic that, just where Bayesian conditionalization does accord with a central desideratum of the methodology of scientific research programmes, Swinburne fails to use it. The failure to stress the role of fresh evidence and the resort to content decreasing strategems are condemned by neo-Bayesians and followers of Lakatos alike. Failure at this point is a serious matter even if it initially affects auxiliary hypotheses rather than the central core of theism.[100]

Swinburne's lack of emphasis on the role of fresh evidence is but one factor which leads him to overemphasize the use of simplicity as a criterion for theory choice. His particular way of using Bayes's theorem throws a great deal of weight on prior probabilities, and Swinburne's assessment of prior probabilities turns largely on simplicity. I have already objected that this does not do justice to the emphasis (recognized by Bayesian conditionalizers and by others) which should be placed on fresh evidence. I have also objected that some aspects of the Bayesian reliance on

prior probabilities create problems which have not as yet been solved. I do however grant that simplicity is one criterion of theory choice even if I maintain that Swinburne overrates it at the expense of other criteria. I also suspect that discussions of simplicity in recent literature suggest that there are more problems in providing a satisfactory account of what simplicity is and why it should be a criterion[101] than Swinburne seems to allow.

One of the reasons for being very cautious about Swinburne's use of Bayes's theorem is that different Bayesians vary considerably in the way they use the calculus. This strongly suggests that there is no very direct link between the theorem and the way the theorem is used to justify rational belief in scientific (or other) theories. What is at issue there, is whether a particular way of using the calculus does justice to the criteria for deciding between theories, and especially whether it provides a satisfactory account of examples drawn from the history of science. That issue cannot be further pursued here, but it is relevant and necessary to show how Bayesians do differ significantly. Swinburne relies very heavily on the assessment of prior probabilities and contends that simplicity is the main determinant of such prior judgements. But faced with problems in the evaluation of such judgements other Bayesians argue that the dubious prior probability judgements are eventually 'swamped' by the steady accumulation of evidence.[102] But Swinburne relies heavily on his assessment of intrinsic probabilities. In his concluding chapter this is admitted when he says

> 'The crucial factor with which we shall need to compare P(h/k) will be P(e/k) ...'[103]

At this point I must emphasize what it is that I do not claim as a result of the criticisms of Bayesian arguments I have put forward. I do not claim that these arguments show a Bayesian or a mathematical calculus to be impossible in this context. But I do hold that Swinburne's assumption of the correctness of his Bayesian approach is questionable. There are serious objections and difficulties in the way of such an approach. But I am neither attempting to resolve the mathematical problems of confirmation theory nor to construct a perfect method for the evaluation of scientific hypotheses. I am merely stating my reasons for my reserve about Swinburne's general views on confirmation before proceeding to criticize his specific criteria for the probability of theistic metaphysics. I propose to criticize the latter even on his assumptions about confirmation. My arguments against his views on confirmation are merely provided to make it clear that I have reservations about even those parts of his position which I shall tolerate for the purposes of discussion. I do not wish to be dubbed a Bayesian just because I argue that even if Swinburne's Bayesianism were correct his attempt to justify theistic metaphysics by it is open to severe criticism. But my reservations about Swinburne's use of Bayes's theorem do not necessarily mean that I am committed to the view that no revision of Bayesianism can escape the criticisms that have been offered.[104] My present stance is simply that Swinburne's use of Bayes's theorem coincides with <u>some</u> elements in the use of criteria for rational theory choice, but runs into difficulties at certain specified points, and neglects certain other important criteria for rational theory choice or rational belief.

So far I have given reasons for accepting one of Swinburne's main contentions that metaphysical beliefs can be the subject of rational enquiry. I have criticized those who deprecate the role of rational discussion in this area. But I do not thereby commit myself either to Swinburne's Bayesianism or to his optimism about providing inductive arguments to justify theistic metaphysics. His particular use of Bayes's theorem I have criticized, while not necessarily rejecting all versions of Bayesianism in this area. But it is now time to move on and to consider the more specific criteria which Swinburne uses to justify theistic metaphysical belief.

Chapter 6. Criteria for Rational Metaphysical Belief?

It is widely held that there is a great proliferation of potential theories.[105] One major aspect of the problem of rational belief can therefore be described as the problem of rational theory choice. This in turn can be considered as the problem of deciding that one theory is preferable on rational criteria to its actual (or potential) rivals. Even if we restrict such an operation to the consideration of serious rival theories such a procedure still seems somewhat generous when judged by the more parsimonious approach of an earlier generation, but we must let that pass. The issue with which we are concerned is that of criteria for rational metaphysical belief. The question of criteria is a central feature of our whole enquiry. For a metaphysical belief to be a candidate for being a rational belief it must at least look as though it is going to satisfy criteria for rational belief, and it is those criteria with which we are especially concerned. Naturally we would like to find some philosopher's stone which enabled us to tell instantly (or at least speedily) which metaphysical theories are or are not rational. But I hope that it will be clear from this study that such a procedure is certainly not instant. The present work aims to show how a task of that nature might be undertaken, and with that more modest aim we must for the present be satisfied.

We could defend the rationality of a set of metaphysical beliefs if we could show that the criteria used to assess it are comparable to (though not necessarily identical with) the criteria used in assessing other beliefs. This is the point of

the earlier chapters on the character of beliefs about the historical past. My contention is that in that area we are dealing with beliefs which are subject to revision.[106] We assess those beliefs by criteria, and if they satisfy those criteria better than other theories or beliefs then we deem them to be rational beliefs. (Perhaps they might also be things that we know but that is a yet more complex issue). I accepted the rational character of beliefs about matters of the historical past on the grounds that they satisfy certain criteria, in much the same way as theories or beliefs about the natural world satisfy criteria.[107] Indeed I went further and argued that many of the criteria used in the one field are comparable to the criteria used in the other. One of the chief differences lay in the frequency with which the criterion of fruitfulness in making predictions could be used, but I drew attention to the possibility of using even that criterion in historical study, and indeed its importance when it was possible to use it, and its potential importance at other times.

So the question this final chapter must address is this. Are the criteria used to defend metaphysical beliefs at least comparable to those used in assessing beliefs in other domains? If they are then this warrants the procedure of assessing the probability or rationality of such beliefs and the attempt to declare that they have evidential support. This does not of course mean that one could write a blank cheque to be used to the credit of any metaphysical belief whatsoever. What it does mean is that we are saying that one can assess the probability and not just the logical possibility of such beliefs.

I do not propose to discuss the issue in a purely abstract manner. I propose to select some examples of the methods and criteria used by Richard Swinburne in his important book The Existence of God. This is not because I agree with his conclusions, or even necessarily with all of his methods. It is because I maintain that this book raises very important issues about the methods and criteria we use to assess the rationality of metaphysical beliefs. By selecting certain specific arguments from Swinburne as examples and discussing them, I hope to explore the question of methods and criteria in this area. Because my aim is to focus on criteria and methods I must leave certain elements in Swinburne's arguments untouched. This does not mean that I always agree with them. Though I find his arguments clearly and forcefully presented, and a major contribution to methodology, I also find some of his points strangely presented and curiously argued. But I cannot deviate from the main task in order to register dissent on other matters. I may make use of occasional devices such as warning quotation marks or the word sic in parenthesis to remind the reader that like any author I may quote or summarize arguments which I will not always criticize at every conceivable point. The one focus of attention is on the contention that metaphysical beliefs can be appraised by methods comparable to those used in assessing theories or beliefs about history and about other matters. In each case we do or we ought to revise our beliefs when rational considerations oblige us to do so. In each case we should prefer those beliefs which best satisfy rational criteria, other things being equal.

There was a tendency a generation ago to link the distinction between rational and irrational beliefs with the distinction between scientific and metaphysical beliefs. Traces of this tendency sometimes still survive. But with changes in the philosophy of science came the end of attempts at a strong linkage of this kind. Two factors especially contributed to this. One was the recognition of the relevance of the history of science to the methodology of science and the accompanying awareness that the views of natural scientists did not simply develop by the accumulation of knowledge, but also by the overthrow and replacement of paradigms and theories. The other was the recognition that the tenets of science are theory-laden. These changes narrowed the gulf between scientific, historical and even metaphysical beliefs.

We seem to require at least two distinctions. One is a distinction between the beliefs which are assigned to different academic disciplines (historical, metaphysical, scientific). The other is the distinction between those beliefs in each discipline which are rational and those that are not. It is this latter distinction with which we are especially concerned. Without it any theory is as tenable as any other theory. In the case of metaphysical beliefs we must consider whether there are criteria of rationality which enable us to maintain the distinction in question. We need to ask whether the criteria here are comparable to those discussed earlier in relation to beliefs about the historical past. In earlier chapters we listed criteria such as consistency, accuracy, explanatory power, fruitfulness and simplicity. Now we must consider these, or criteria like them,

in relation to the rationality of metaphysical beliefs.

Criteria (1a) Coherence (or internal self consistency).

The first criterion is the one to which I shall give least attention as it is less controversial than the others. We are concerned here not so much with truth, or with mere possibility, as with (epistemic) probability. It is a necessary condition for the probability of a theory or set of beliefs that it be possible. If it contains incoherence or self contradiction then it is not logically possible. If it is not a logically possible theory or set of beliefs then it cannot be a probable one. In the case of theistic metaphysical beliefs much attention has been directed to the question of their logical possibility, and the extent to which such a system of belief is or is not free from incoherence. Indeed Swinburne himself devoted the whole of a previous volume to just this question.[108] In it he made certain modifications to traditional theism and then argues indirectly that the resulting 'modified traditional theism' is free of incoherence. This is one of those points I have no inclination to contest. My interest is in the means of determining the probability of such systems of belief. For that purpose it is of course necessary to note that freedom from self contradiction is a sine qua non.

I am aware that some writers claim that in science the requirement of coherence can be overridden. It is argued that if a theory rests on inconsistent foundations but is startlingly successful in making predictions then it is rational to prefer that theory to its more consistent but less fruitful rivals. This claim is however highly controversial[109] and in any case

hardly applicable to the evaluation of metaphysical beliefs where the paucity of prediction is notoriously problematical. For our purpose we may treat coherence as a necessary condition.

Of course matters are somewhat more complex. If we find that a theory is incoherent it is not automatically discredited as impossible, and therefore worse than improbable. An inconsistency may be marginal rather than central. It might be possible to remove one or two peripheral hypotheses from a theory (one or two beliefs from a set of beliefs) and restore the coherence of the whole, or rather of most of it. One description of such a process is the selection of the preferred maximally consistent subset of an inconsistent set. Indeed Swinburne himself aims to do just that when he makes modifications to his version of traditional theism. At this point however we may begin to see something of the complexity of the criteria we are investigating. We tend to be somewhat suspicious of a person who continues to maintain a theory while making one concession after another as parts of the theory have to be abandoned or modified. Of course in many theories it is the case that there are peripheral hypotheses. It is often <u>not</u> the case that every element entails every other. But constant reduction or constant modification rouses suspicion. This issue is that of the 'content-reducing-stratagem'.[110] One main suggested remedy is to look for an increase in testability to offset the loss of confidence engendered by <u>ad hoc</u> modifications of theories. But this is an issue which belongs more to the discussion of the criterion of fruitfulness in making predictions which must be discussed later.

The last issue has been discussed in rather an abstract

way. Perhaps an analogy may help some readers and illustrate the point at issue. Sets of beliefs are sometimes compared to laundry bags full of unconnected items. But suppose it is claimed that the bag contains clean laundry. It contains 50 items and we are able to inspect five. Two are found to be dirty. Someone might argue that these two items are not connected with the rest, and that all the essential items in the bag are clean laundry. But those of us with suspicious natures would tend to ask for some means of specifying which further items are to be classed as essential and which not. Only if a satisfactory answer is forthcoming would we then withdraw our suspicion.

Modification of a theory may be the result of finding an internal incoherence, or due to the failure to satisfy some other criterion. So we shall have to keep an eye open for this factor. It may recur. In the meantime I intend to rest content with the observation that self consistency is a necessary but insufficient condition for the probability of a set of beliefs.

(In this chapter I am chiefly concerned with criteria for rationally evaluating theistic metaphysical beliefs. I am well aware that many philosophers have focussed far more on questions like the nature of religious language and the coherence of theism. I am also well aware that these issues are relevant to evaluating the rationality or probability of such beliefs. My chief point is that these factors are most relevant to the question of the <u>possibility</u> of such beliefs and that what I am especially concerned with are those further criteria which are particularly relevant to the <u>probability</u> of such beliefs. On the question of religious language I

simply propose to state very briefly and without any extended discussion the type of approach presupposed here. I am interested in exploring the criteria for testing those versions of theistic belief which hold that some of the terms used to describe God are to some extent analogical. In other words I am considering criteria used in relation to beliefs interpreted as saying that divine wisdom and divine love and divine personhood are comparable to but not in every respect comparable to human wisdom and human love and human personhood. In accepting for the purpose of the argument that the version of theism under discussion is this one I am declining to discuss criteria for evaluating interpretations of theism such as those which take theistic language purely or mainly in an attitudinal sense. An example of the latter would be D.Z.Phillips interpretation of the phrase 'the love of God' to refer not to a divine being who acts in a loving way but solely to the importance of the believer so acting.[111]).

(1b) Consistency with other beliefs.

The theories or sets of beliefs which we hold are part of our total system of beliefs. Much of the giddiness which philosophy is blamed for inducing is in fact caused by the confusion which results from incoherences in our total system of beliefs. The problem is that it can be very difficult to identify the offending propositions, and even more difficult to determine which to discard or change. In the case of theistic metaphysics a bold adherent might argue that his theistic belief is an entire system which comprehends statements of every kind so that there are no 'other beliefs' with which

his theistic belief needs to be compatible. In such a case one could argue that all that has happened here is that a different classification has been adopted. The class of beliefs comprising this believer's set of metaphysical beliefs is expanded to include all his other beliefs. What on my model arises as an issue of coherence with other beliefs is, on this person's model, turned into an issue of internal consistency such as we have just discussed. The difference is one of terminology and classification rather than one of substance. In either case the test is whether there are beliefs which contradict the specific narrower set of beliefs we are examining.

Let us use the terminology of my model. If within someone's total set of beliefs their theistic beliefs are in contradiction to other beliefs, then something has to be abandoned or modified somewhere. So much is relatively straightforward. But faced with such a contradiction where is the adjustment to be made? This is the question which focusses attention on the issue with which I am most concerned. Some criterion (or some criteria) other than that of consistency is needed to provide a ground for restoring the consistency of a set of beliefs in one way rather than another. Faced with a failure of consistency it is usually the case that there are several or even a great number of rival remedies. A few very brief examples may illustrate the point.

Let us class belief in the omnipotence omniscience and benevolence of a personal deity as a set of theistic metaphysical beliefs T_1. Let us class the set of beliefs that the Khmer Rouge murdered millions of their fellow citizens

and that today scores of children will die of cancer or other diseases as a set of other beliefs T_2. The consistency of the total set of beliefs which includes T_1 and T_2 is under discussion. I cite the example solely to point up the methodological issue at stake. Some theists might well argue that any alleged inconsistency between T_1 and T_2 is more apparent than real and that the divine omnipotence and benevolence are not compromised by the admitted evidence of T_2. Others might contend that some adjustment needs to be made in T_1. Some people might argue that consistency can be restored if the nature of divine benevolence is clarified and qualified. True benevolence in this context is concerned with the true good of character formation and 'soul making' and to that end the real possibility of evil choices and real suffering is inescapable. In other words a modification is introduced into what benevolence might otherwise be taken to mean. Others might focus on omnipotence and proceed in various ways to aim at restoring the coherence of T_1 and T_2 by qualifying omnipotence. (The earlier term meant supremely powerful rather than capable of doing anything; it is incoherent to imagine that omnipotence includes the capacity to perform what is logically contradictory; God is a being of immensely great but not absolutely unlimited power). Various moves are here possible. Others again might aim to restore the consistency of their total set of beliefs by maintaining T_2 and by abandoning rather than modifying or defending T_1. Finally a purely fanciful option will illustrate the far reaching implications of Quine's law that any sentence

whatever may be held true provided we make sufficiently drastic changes elsewhere in the system. An enthusiastic Christian Marxist might argue that premature death due to disease is the result of imperfections in the distributions of resources in society and that the alleged atrocities of the Khmer Rouge are fabrications of the CIA and the Vietnamese. (Just in case any of the readers of this book is attempting to attribute all the views I cite to myself I should add that I regard the last view as quite indefensible. I cite it only to illustrate the point that beliefs about empirical matters are beliefs and can be contested and are contested, sometimes unreasonably (as here) and sometimes with reason). I can now say why there is a point at issue which concerns criteria and method. I am not intending to add to the innumerable discussions of the problem of evil; I am seriously raising the question of what criteria are relevant to the rational choice of theories or beliefs. The question at issue is this. Faced with rival proposals for defending or restoring the consistency of a set of beliefs why do we, or why should we, choose one of them rather than the others?[112]

Even if we were not faced with an alleged loss of consistency we might still arrive at a similar problem. Suppose we accept the view that the number of potential theories or sets of beliefs is very large. Suppose we also assume that there is a large class of consistent sets of such beliefs. Let us call our set of beliefs about chiefly empirical matters S_8 and select three sets of chiefly metaphysical beliefs as rivals to each other. These are S_1, S_2, and S_3. Let us

further suppose that S_1, S_2, and S_3 are each internally coherent. And let us also assume that each of S_1, S_2 and S_3 either is coherent with S_8 or can be made coherent with S_8 without disproportionate effort. This situation is contrived, but not wholly implausible. Faced with a choice of $S_1 + S_8$ or $S_2 + S_8$ or $S_3 + S_8$ which of the three rival options do we choose and why? In such a case criteria other than coherence need to be invoked.

(2) Simplicity

One of the criteria to be considered in this connection is that of simplicity. This is in fact a much more difficult and controversial matter than is sometimes thought. In some ways I would prefer to discuss it after considering most of the other criteria. I recognize the wide use of appeals to simplicity, as well as harbouring reservations about the problematical character of this criterion. If it were the last criterion to be invoked one could reduce its role somewhat by arguing that one exercized a preference for a simpler theory only when one had to choose between rivals after one had sifted out all the other rival theories on other criteria such as explanatory power, fruitfulness and accuracy. But there are at least two reasons for considering simplicity at an earlier point in this chapter. One is that simplicity is correctly invoked at an early as well as a late point in the rational scrutiny of beliefs. A wildly complex theory may be extremely difficult to test. If one values criticizibility then one wants criticizable theories to be presented for scrutiny. Popper argues that the appropriate concept of

simplicity is to be equated with degree of falsifiability (his italics).[113] I would prefer to speak of criticizability, but I think that Popper has drawn attention to an important element in the process of evaluation. Faced with rival theories we look for testable theories. The very simplest are quickly rejected. We then look for a theory which has not already been rejected (Popper would say falsified) and which is readily testable. In this sense simplicity is a criterion which we introduce at an early point (though we cannot wholly equate simplicity with criticizability.[114]). Though there may be some connection between simplicity and vulnerability to criticism the two notions cannot be equated. The following objection has been put. If all observed X s are Z s, then the theory that all X s and Y s are Z s is more ('falsifiable') vulnerable to criticism than the theory that all X s are Z s. Yet we would hardly hesitate to designate the latter as simpler (and the theory which we should test first).

My other reason for agreeing to discuss simplicity at this stage is more mechanical. I propose to consider the role of simplicity as a criterion in examples drawn from Swinburne's book. He introduces it as one of his first criteria. It is therefore convenient for me to consider it now. Swinburne is of course using a form of Bayesian confirmation theory. In his opinion simplicity is the main factor in determining prior probability so he invokes this at a very early point in his second volume when he moves on from the coherence of theism to the consideration of its probability. Though I have already indicated that I do not share Swinburne's

confidence about Bayesian confirmation theory, I do agree that simplicity is a relevant criterion so debate is possible on at least part of Swinburne's argument. Before starting on a specific example from his book, however, I wish to consider some specific problems with regard to the appeal to simplicity.[115]

It is somewhat surprising that wide assent is given to the view that simplicity is a virtue in a theory yet we lack an adequate account of what simplicity is and why we should prefer it. There is evident excitement amongst physicists at the thought that developments in small particle research may explain the proliferation of sub-atomic particles in terms of a limited and ordered set of yet smaller particles. The hoped for theory is neater and simpler. But can we specify what makes a theory simpler than its rivals? Some distinctions are needed. Value is placed on different types of economy. We can for instance distinguish economy of mathematical form, economy of theoretical premises, and ontological economy of postulated entities. But it is problematical that the logical form we use can affect apparent simplicity. Logical equivalents can look simpler or more complex than each other, as can mathematical formulae which are equivalent. Apparent simplicity can also depend on linguistic usage. 'Emeralds are grue' expresses simply in the language of grulers what looks less simple and so less plausible when expressed in the language of colours. (That emeralds are green before the year 2000 and blue thereafter). If we argue that colours are natural kinds, whereas grulers are not, we are told that the identification of natural kinds depends on scientific theories and the latter

on the use of simplicity in theory choice.[116] Our line of argument appeals to simplicity but does not do much to clarify the notion. It shows that our notion of simplicity may be more deeply embedded in our conceptual system than we sometimes realize. It is a more primitive notion in other words. It underlies many of the other judgements that we make. It is therefore hard to try to define simplicity, and unwise to try to define it in terms of less primitive notions. Yet it is not viciously circular to appeal to simplicity in this case. If Green can argue convincingly that Grue's defences always produces a new loss of simplicity Green's case is strengthened.

The simplest theory is certainly not always preferred and apparent simplicity may not be real simplicity. That planets travel in circles is a simple hypothesis. But it fails to meet the criterion of accuracy and so it falls. That every physical body exerts a gravitational pull on every other is a more satisfactory theory. This shows that some balance is needed between accuracy, explanatory power and simplicity. We cannot prefer the intuitively simpler theory at the expense of all other criteria. We can perhaps reformulate our rule to prefer the simplest of rival theories of equivalent accuracy, explanatory power and fruitfulness. (Which would still leave us with a problem of evaluating different performances on these criteria). The examples cited above help to illustrate some aspects of the appeal to simplicity but do not tell us how to define the criterion.

Sober attempted to define simplicity in terms of degree of informativeness in answering a question. But this leads

to unsatisfactory results noted below.[117] We seem to be left with the somewhat embarrassing position that we do prefer some theories to others on grounds of simplicity but we do not have a wholly adequate account of what simplicity is.[118] This means that we should treat appeals to simplicity with a certain amount of caution. It also means that for the time being the best way to proceed will be by means of the discussion of particular appeals to simplicity in the domain with which we are concerned. This in fact is how the matter is discussed in other domains so it should not distress us unduly. The problem is not that we are unable to provide examples of preferences for one theory over another on grounds of simplicity. It is that we lack a clear account of how these preferences help us to achieve an adequate definition of the criterion (or criteria) of simplicity. Our current task is however to argue that it is possible to make judgements about metaphysical theories on grounds of simplicity. In order to do that I propose to discuss some examples of judgements of simplicity made by Swinburne. If I sometimes argue in passing that considerations of simplicity require conclusions other than those which Swinburne favours, this does not conflict with my contention that simplicity is an appropriate criterion. On that point I agree with Swinburne even if I disagree at times with his use of the criterion.

By examining some examples of the appeal to simplicity in this one recent major work on theistic metaphysics we can explore aspects of the role of the criterion of simplicity in this area. In order to discuss Swinburne's appeal to simplicity I will have to give some indication of what he says. Some of what

he says is, in my view, very strangely expressed and strangely argued. I am therefore anxious that my readers should not attribute the views I am criticizing to myself. In order to avoid this hazard I propose to start by letting Swinburne speak for himself in a few selected quotations in which he makes an appeal to simplicity.

> 'In these respects the theist proposes a significant simplification of our world view. There are three tenable views as to the relation between scientific and personal explanation. One untenable view is the occasionalist view.... Two other untenable views are the view that scientific explanation is analysable in terms of personal explanation and the opposite view that personal explanation is analysable in terms of scientific explanation... Given that all these views are false, there really operate both scientific causality and personal causality and neither is analysable in terms of the other. There remain three tenable views of their relation to each other.'[119]

Swinburne entitles these theism, materialism and dualism and then argues that theism is simpler and more preferable. He argues:

> 'Now clearly both the theist's view and the materialist's view result in a significantly simpler world-view than the dualist world-view... The materialist programme has a certain initial plausibility... There are however... very considerable scientific and philosophical difficulties ... I have pointed out here that equal simplicity may be gained in a different way by supposing that in

> the stated respect scientific explanation is reducible to personal explanation.'[120]

Just in case it seems at first sight that Swinburne has crudely contradicted himself it should be noted that in the last clause he presumably means that in his view scientific explanation can be included within theistic personal explanation. He continues:

> 'So then theism has very considerable <u>simplicity</u>. Simplicity is the major determinant of intrinsic probability... The intrinsic probability of theism is, relative to other hypotheses about what there is, very high, because of the great <u>simplicity</u> of the hypothesis of theism.'[121]

In the above quotations I have italicized those areas which appeal to considerations of simplicity. The passages cited above form the conclusion to a line of argument stretching over a hundred pages in which Swinburne discusses what he means by inductive argument, explanation, scientific explanation and personal explanation. In the course of that longer argument for the simplicity of theism Swinburne makes another appeal to simplicity. He maintains that the deity in which theists believe is the simplest kind of person there could be. This is an issue related to but not to be confused with his general argument for the simplicity of theism. The general argument is the major one, the other appeal to simplicity is an argument within an argument. I propose to discuss the major argument first and the subsidiary argument later on. The two types of appeal to simplicity are to my mind not the same and should not be confused with one another. (I am not saying that

Swinburne makes such a confusion, but that we need to pay close attention to what he says).

Swinburne's main argument falls into two parts. He argues that theism and 'materialism' are simpler than and so preferable to 'dualism'. This is the first step. The second is his argument that 'materialism' as the surviving main rival to theism

> '...<u>seems</u> doomed to failure. For a detailed materialist theory could not be a simple enough theory for us to have reasonable confidence in its truth.' [122]

From these two steps follow, or so he claims, Swinburne's conclusion already cited above that theism has very considerable simplicity and so, relative to its rivals, very high intrinsic probability.

We have here then a very explicit set of appeals to simplicity to attempt to resolve a metaphysical debate. Swinburne does not regard simplicity as the only criterion by any means. But he does make it the major determinant of the <u>prior</u> probability of a theory, in this case of a metaphysical theory. I have already indicated my reservations about this Bayesian element in Swinburne's argument. I did that in Ch. 5 when I was considering approaches to the question of criteria and methods which differed radically in various ways from my own. I do not propose to repeat that discussion here. It is sufficient to note that here we are concerned with the narrower issue of the place of appeals to simplicity in relation to metaphysical beliefs. The point at issue here is whether the argument adds to, or detracts from, the contention that a metaphysical belief

is more rational than its rivals if (amongst other things) it is simpler than its rivals.

At this point we must consider the question of rivalry, which is a crucial element in the rational evaluation of competing beliefs. Current discussion is remarkably tolerant about the number of potential theories with which any given theory could be compared.[123] But tolerance has its limits and the limitation of potential rivals is usually effected by specifying certain serious rivals. Which rivals are to be regarded as serious depends on their success in meeting criteria such as simplicity, explanatory power and the like. Attentive thinkers will see that this is not a circular argument. Theories are sifted. Those which blatantly fail one or more of the tests are relegated to the list of less serious rivals. Those that promise to satisfy the tests rather more adequately are then more carefully scrutinized as serious rivals.

There is however a more taxing problem in comparing competing theories. This is that some theories are so different in character that it is hard to see how they can be compared.[124] There is however a device for coping with this which Hesse describes. She points out that

> 'Two theories h and h' can always be made to have notionally the same content by conjoining with h that part of the relevant content of h' with which it does not conflict, and similarly for h'.'[125]

Swinburne does not explicitly state that this tactic is being adopted, but to note this factor will avoid potential confusion. Swinburne contrasts theistic personal explanation

with what he variously describes as 'scientific explanation', 'the materialist programme' and the like. At first sight this may seem confusing, but I do not think that a close reading of Swinburne supports the view that he is confused. He is not envisaging some facile comparison of science and religion of a generation ago whereby science was deemed to offer explanation within the universe and religion explanation of the universe, or some such uneasy truce was proclaimed. He is working with more recent theories of theory. True he sometimes uses the term 'scientific explanation' in a broader sometimes a norrower sense. But this is usually clear enough in context. Let us attempt a 'reconstruction' of this element in his argument, focussing on his appeal to simplicity.

Swinburne claims that theism (P) is simpler than its rivals. The main rivals he considers are dualism (D) and (scientific) materialism (S). These are large scale theories aimed at explaining at least two sets of data. These sets of data are (s) events explained by reference to scientific theories (or laws) about physical objects and the like (scientific explanation in the narrower sense: hence the lower case s), and (p) events which require a reference to the intentions of persons in order to explain them (personal explanation). The choice of lettering is mine and the reasoning for it will I hope become plain.

Where Swinburne is confusing is that he describes both P (theism) and p (explanations of human actions) as 'personal explanation'. But the context almost always makes it clear whether he means P (the theory that everything is ultimately

due to the intentions of a personal deity) or p (the theory that certain events are caused by the intentions of human persons). In set theoretical terms the hypotheses comprising p are a subset of the hypotheses comprising P.

Similarly the term 'scientific explanation' is used in two ways. It is used of the large scale theory S that everything is to be explained by a programme of materialist reduction so that p is explained in terms of s. But the term 'scientific explanation' is also used in the narrower sense to refer to s alone. Again in set theoretical terms the hypotheses comprising s are a subset of the hypotheses comprising S. (Or, if you prefer it, the theories comprising s are a subset of the theories comprising the research programme S).

We can now examine the character of the appeal to simplicity which Swinburne makes in the passages cited above. He is saying in effect that there are three main rivals for comparison: P, S, and D. D comprises s and p and asserts that s is not reducible to p, and p is not reducible to s. S comprises s and p and asserts that p will eventually turn out to be reducible to s. P comprises s and p and asserts that both s and p are reducible to (theistic) personal explanation. There is a notational difficulty here but if I understand Swinburne correctly we cannot say that he is claiming s is reducible to p nor that s is reducible to P. We will need a further subset of P namely P' to which it is claimed s and p are reducible. P then asserts, amongst other things that s and p are reducible to P'. The need to distinguish P and P' is I suspect purely notational[126] and I do not propose to argue that P loses simplicity on that

score. This I believe exemplifies the point made earlier that simplicity or complexity of description may not necessarily signify that a theory is really simpler or more complex. In fact I hold that the apparent complexity of distinguishing P from P' masks a real loss of simplicity. But the issue on which we are to focus is this. Can we explicate and defend the appeal to simplicity in cases such as these?

One objection needs to be considered right away. This is that P, S, and D are not proper competitors. It could be argued that theism, materialism and (explanatory) dualism are not real rivals as explanations. There is force to this objection but I think that it is misplaced and premature. Swinburne proposes that we consider the prior probability of these theories by estimating their relative simplicity. He then considers the relative explanatory power of theism at the next stage. To object <u>at this stage</u> that the three theories are not equal in explanatory power is to miss the point. I am proposing to argue later that a loss of simplicity may be compensated for by a gain in explanatory power. I am also proposing to argue that while one may make an initial provisional estimate of the simplicity of rival theories one can only finally invoke simplicity at a much later stage when the theories have been compared for explanatory power, accuracy and fruitfulness as well. In other words I agree with the aim of the objection, but am pointing out that it is premature to raise it at this point as it does not bear on what Swinburne actually says.

Swinburne's method is to invoke simplicity at a very early point in the argument and to use it as the main determinant of prior probability. Against this a series of points need to be made in the proper order. The major point is that we should not evaluate simplicity independently of other criteria. I am indeed going on to argue that a loss of simplicity may be offset by a gain in explanatory power (or a gain on some other criterion). That to my mind is the correct form of the objection. (I think in fact that Swinburne probably could accommodate the point within his Bayesian calculations though, if I have read him correctly, he does not do so). He does not do so because he argues that theism is the simplest theory tout court when compared with D (dualism) and S (materialism). My objection against this is that this argument rests too much weight on the criterion of simplicity. It invokes the criterion too much at too early a point, and it would be better to consider the evaluation of theories in terms of their simplicity relative to their explantory power. But in order to illustrate the force of this methodological objection I need to argue in stages first that Swinburne's excessive early reliance on simplicity is misplaced, and then that a loss of absolute simplicity could be offset by a gain in explanatory power.

In order to reach my conclusion about criteria and methods I need to spend some time showing that Swinburne's argument does indeed involve an overestimate of simplicity. Let us return to Swinburne's contest between P, S, and D.

His terminology is sometimes slightly different from mine but I think that no serious injury will result. Let us identify P as Swinburne's modified traditional personal theism. S is what he entitles materialism or the like. I propose to treat S as a group of related theories in a research programme. The version of S which I shall sometimes consider is S'. This variant of S is less crudely materialistic than Swinburne's S. S' is the view that the behaviour of human persons is not crudely reducible to talk of electrons and neurons but will ultimately be included within an enlarged programme of unified science comprising the natural and the human sciences. To my mind S' is preferable to Swinburne's S, but as I shall be arguing mainly about the role of simplicity in relation to the contract between P, D and either S or S' I think my distinction between S as crude materialism and S' as a more subtle theory does not do any injustice to Swinburne. Let us entitle S' as the theory of a unified science as opposed to S which is materialist reductionism. D remains as before the dualistic theory described earlier. It is the theory that p (the explanation of the behaviour of persons) and s (the explanation of other events according to modified Hempelian scientific laws) are independent of one another. (Swinburne argues as we noted above that on this view neither of these elements is reducible to the other).

The crucial question is this. Swinburne claims that P and S are simpler than D and that P is simpler than S.[127] Does this example help to clarify our question about the role of simplicity as a criterion? I think it does.

Let us consider first the claim that P and S are each simpler than D. Swinburne argues:

> 'Now clearly both the theist's view and the materialist's view result in a significantly simpler world-view than the dualist world-view.'

He continues:

> 'A world-view in which all personal explanations have a complete explanation in scientific terms, or all scientific explanations have a complete explanation in personal terms would be a simpler world-view than others, and as such more likely to be true.'

and further:

> 'dualism gives a very messy, unsimple, picture of the world...'[128]

This is all very brief. The appeal to simplicity here is almost assumed as self evident. Can we try to specify what kind of simplicity is involved? Swinburne correctly notes that part of what he says reflects the appeal made by 'materialists' (we might add 'and upholders of a unified science') to simplicity. In this case it is the type of simplicity achieved by identifying one set of entities with another or by showing one set of entities to be an extension of another class of entities. We have already noted a difference between this kind of simplicity and simplicity achieved by elimination of entities.[129] But dualists usually object that the identification in question, or the thesis of compatibilism in question, cannot be carried through. In other words simplicity alone is held not to decide the matter. We shall return to this point shortly.

But when Swinburne comes to contrasting theism and materialism, his equivalent of our contrast between P and S or S', he makes a different move. Here he puts up two arguments. The first claims that theism offers 'equal simplicity' to the rival more scientific view.[130] The second then draws attention to difficulties in the materialist account. From those he concludes as we noted above

> 'a detailed materialist theory could not be a simple enough theory for us to have reasonable confidence in its truth.'[131]

So in effect Swinburne is claiming that P and S promise equal simplicity but that S fails to deliver it. His objection to the rival view S is that it fails to carry through a programme of reducing personal explanation to laws of a Hempelian kind. We cannot pursue here [132] the question of the persuasiveness of Swinburne's objection. If he were right in arguing that S fails at this point then this would be either an instance of a failure of accuracy or an instance of a rival theory needing to be made more complex in order to deal with such a failure. Swinburne seems to imply that S suffers the latter fate.

Even if it were true that S is more complex in some such manner that does not resolve our interest in the applicability of the criterion of simplicity. The complexity in S (or S') would be exemplified by the need for a large number of imprecise and untested hypotheses about the correlation between brain states and mental events. This complexity then, it is argued detracts from the simplicity

of S. But we need to distinguish between a complexity of this kind and a more fundamental type of simplicity. Let us examine the claim by Swinburne that:

> 'Theism is simple in postulating that in this way explanation is all of one kind'.[133]

This is a much more fundamental issue as the argument here is that two whole systems of hypotheses (s and p) can be coordinated in one unifying theory P. Both S and P aim at this more basic type of simplicity. So let us try to set up the character of the appeal to simplicity and then test it. It is claimed that both S and P aim at a basic simplicity but that P is preferable to S because S contains incidental complexities in its component hypotheses. This seems to resolve one of our problems by showing us how we can rank different types of appeal to simplicity. S and P are preferable to D, as S and P aim at a more basic type of simplicity. But S it is claimed is less preferable than P as S contains an incidental loss of simplicity. This notion of ranking is clearly very useful.

Unfortunately we have solved one problem about different types of simplicity but are on the way to uncovering another. The notion of incidental complexity works just as much to the disadvantage of P. It can be shown that P also contains an incidental loss of simplicity. Swinburne says very little about this. P involves a different type of incidental complexity. It requires the assumption of an additional entity namely that in addition to human persons and the objects investigated by the natural sciences there is also

a personal God. A theory which assumes the existence of more entities than its rival theories is in that respect less simple because less parsimonious, or less economical in its ontology than its rivals.

The issue can be stated thus. We can rank some appeals to simplicity. Both P and S are preferable to D in that they aim at a fundamental unity of explanation. But S is judged inferior to P by some on the grounds that it includes a clutter of complex hypotheses, while P is judged inferior to S (or S') by others on the ground that it lacks ontological economy. It is hard to resolve this latter clash by ranking the different types of simplicity. We are faced with a choice of additional hypotheses or an additional entity.

We need to specify a little more precisely what this last loss of simplicity might be. Swinburne's version of theism involves assuming the existence of an additional personal being who is nevertheless very different from the other persons with whom we are already familiar. In this respect it seems a more complex theory, because it requires more assumptions and includes reference to more beings. This loss of simplicity in the form of loss of economy might be acceptable if it were offset by a gain on some other criteria, such as a gain in explanatory power.

Before proceeding to that issue, however, one further argument needs to be considered. This is that there is a qualitative simplicity which needs to be taken into account. It runs:

'Theism postulates God as a being with intentions, beliefs,

> and capacities, but ones of a very simple kind, so simple that it postulates the simplest kind of person that there could be.'[133]

This is a rather strange argument which appeals to a very special sense of the term 'simple'. It depends on the view that a finite quantity demands further explanation while an infinite one does not and that the latter is therefore in this sense 'simpler'. Even if we were to concede this for the sake of argument[134] it does not meet our objection noted above. A theory which includes this additional qualitatively 'simple' being is still less simple because less economical than one which does not.

So let us consider the procedure of offsetting a loss in simplicity with a gain elsewhere, for example, with a gain in explanatory power. This might help if our aim is to show that the effect of a metaphysical argument for theism is to increase the probability of the conclusion. If simplicity is thought to measure prior probability, and explanatory power then to give us the posterior probability, a loss on the former and a gain in the latter gives us a rise in probability. But this Bayesian analysis is one we have already questioned. More seriously the crucial question is whether the argument is one to which the conclusion is more probable than not. For this we need as high a score on each of the criteria as possible. All the same losses in one place can be offset by gains in another provided that our favoured theory does better than all its rivals.

The problem is that as with simplicity so with the

procedure of offsetting gains and losses, different people will evaluate the application of the criteria differently. Thus a supporter of D could claim that though dualism is a less unified theory it scores highly on accuracy in that it doesn't make problematical claims about reducing p to s or s to P'. Meanwhile a supporter of S or S' could claim that though P gains on explanatory power, S and S' gain a predictive success every time a mental event is correlated with a brain event. Thus S and S' can appeal to fruitfulness in making predictions. Our larger exploration must continue with a look at these criteria. Meanwhile we can draw some interim conclusions about simplicity.

1) It is possible to compare metaphysical arguments on grounds of simplicity. 2) Different types of simplicity need to be distinguished. 3) When we can rank these different types of simplicity we can resolve a clash, but not all of them can be ranked. 4) Simplicity needs to be considered in relation to other criteria and not used too much too soon in isolation from them. Simplicity is of value in making a preliminary sifting of rival candidates for rational belief. But in the last resort it has to be used in conjunction with other criteria.

(3) Explanatory Power

The criterion of explanatory power has already been mentioned and here it must be considered more specifically. We must look at its role as a criterion of rational metaphysical belief. Let us begin by considering the contention that a theory is preferable and so more rationally

believable if it explains a greater variety of facts than its rivals do.[135] To this proposal we would immediately need to append a series of qualifications about the rivals being comparable in accuracy, fruitfulness, simplicity and the like. We would also need to specify the domain within which a greater variety of facts is explained by one theory than by its rivals. But we must proceed in stages one step at a time.

Generality is in some instances a virtue. We prefer the view that copper conducts electricity to the view that this piece of copper does so this afternoon. But we also prefer precision to vagueness. We prefer the view water boils at 100°C to the view that water boils when heated enough. But we also prefer the addition of restrictions demanded by accuracy. Thus we find that water boils at 100°C at sea level. A theory which explains the boiling of water at given temperatures and pressures is more preferable still. It is more preferable because it explains anomalies which upset its predecessor, and also enables us to make new tests. It is general and yet specifies precise qualities, and it accounts for more data than the competing theories mentioned above.

In the case of the explanatory power of beliefs about historical events we have already noted that it is more often the case that an explanation renders an event probable than that it renders it deductively certain. In an earlier chapter the example was cited of troops defecting.[136] The historical statement discussed there implied a theory to the effect that troops are more likely to defect if unpaid. But a further factor was also included. If the troops in question

differ in religious allegiance from the rest of the army the likelihood of their defection may well be increased. Let us call the first explanatory theory h_1 and the second h_2. We would well argue that h_1 plus h_2 has greater explanatory power than either h_1 or $h_2$alone or several rival theories. The more complex but also more precise theory which includes h_1 and h_2 would explain more cases of desertion than h_1 alone.[137] Thus h_1 alone might help to explain the defection of the Ghassanid prince discussed in an earlier chapter, but the addition of h_2 to the theory would not only explain that event better, but would also explain events such as the defection of Druze soldiers from the Israeli army during the occupation of Lebanon in more recent times. The difference of religious allegiance was there a crucial factor. So a theory has greater explanatory power the more instances and the more varied instances it can explain.

This provisional account of explanatory power will need to be refined further and its bearing on the comparison of metaphysical beliefs then explored. I propose to do this by continuing to draw on the points made in an earlier chapter about historical explanation. These will help to elucidate criterion of explanatory power which is under discussion here. I argued earlier that historical judgements imply belief in theories which explain historical events. These theories can be formulated as laws which declare certain events to be probable given specified antecedent conditions. The current issue is the claim that the more events such theories explain, and the more varied those events, the greater

the explanatory power of the theory. Can we further clarify the notion of comparative explanatory power?

One could argue that a theory only really explains a set of events if the statement that those events will occur is deducible from rather than made probable by the theory. This line of argument would suggest that a theory which declares the occurrence of e_1 to e_n certain rather than probable has greater explanatory power than a theory which declares e_1 to e_n merely probable. One could support this with the argument that unless a theory said why an event did occur, rather than why it was likely to occur, we have not fully explained the event. But while deductive explanations might be preferable in general they may not be available or may be offered but rejected for various reasons. For instance I am told that Quantum Mechanics (QM) plus a theory of hidden variables (HV) would render some explanations in physics deductively certain rather than statistically probable. But though the theory QM + HV might seem preferable because in this sense stronger in explanatory power it is so far treated only as a speculative possibility. The reason for that is that attempts to test plausible hidden variable hypotheses have so far ended in failure.

The attractiveness of a deterministic theory in this context is that it would have greater explanatory power in the sense that it would make the effects certain rather than probable. But, as we have seen, in some cases the preference for a theory with greater explanatory power in this sense is resisted. A similar resistance can be seen in the case

of theories of historical determinism. If there were laws of history which made historical events inevitable rather than probable the theories incorporating such laws would have a similar advantage in explanatory power over more probabilistic kinds of historical explanation. Yet here too such an advantage in explanatory power is treated with suspicion at least in some quarters.[138] Our current concern is with this criterion of explanatory power and we cannot digress into detailed arguments over historical inevitability. It is possible however to see that a gain in explanatory power does give a belief in hidden variables or a belief in historical determinism a certain initial gain in plausibility. What then is the constraining factor which prevents such an advantage being clear and decisive? In the one case it is the failure of predictions which would be expected if the hidden variable theory were true. In the other case there are notoriously many objections. These would include conflict with other theories about human responsibility, and the failure to specify precise mechanisms for historical determinism. Our concern is with criteria, and here I propose only to maintain the following conclusion. A gain in explanatory power of the kind specified above renders a theory preferable (a belief more rational) unless it is achieved at the expense of a setback in relation to one of the other criteria. Such a setback could be characterized by a failure in expected prediction, or a clash with another well supported theory, or a lack of accuracy and precision.

We could also consider a gain in explanatory power of a

less extreme kind. A non-deterministic theory might render the event in question e more probable than it was on other theories. In this case the preferred theory remains a probabilistic one but one which makes the event more likely than the rival theory does. (If we describe this as a qualitative gain, then we could describe the more deterministic version of the previous paragraph as a maximally qualitative gain in explanatory power.). As an instance of a theory which renders the event e more probable than it was before, we could again draw on an example from the chapter on historical study above (Chapter 4). There we considered the example of an historian pointing out that conditions were favourable to a revolution as the government had no military forces to hand and there was acute unemployment. The implied generalization is the following. Revolution is likely if unemployment is acute and the government has no military forces at hand. This theory has more explanatory power than a theory which specifies either of the two conditions alone. High unemployment alone does not so often lead to revolution, nor do governments without strong forces regularly invite revolution. But the two factors combined make the event e much more probable. The gain in explanatory power is qualitative in that the preferred belief renders the event e more probable, and the preferred belief is therefore to that extent more rational.

There are other ways in which a gain in explanatory power can be registered. One of these is that a theory A is rationally preferable to theory B if A explains more facts than B. In this case we seem to have quantitative rather

than a qualitative advantage in explanatory power for A over B. I propose to consider two cases where such an advantage might be claimed. In the first case (within an explanatory schema) we can infer from theory B evidence e_1 to e_n. (The qualification is important as we need to distinguish cases of inference within an explanatory schema from other cases of inference. A rogue theory such as P & Q & R & S.... therefore Q & R & S.... would allow us to infer any number of facts but would have no explanatory power.). From the explanatory theory B we can infer evidence e_1 to e_n. But from the rival theory A (which is also explanatory) we can infer evidence e_1 to e_{n+i}. In this case I am stipulating that evidence e_1 to e_n consists of evidence which is already to hand, but e_n to e_{n+i} also contains novel data which are predicted by theory A and are successfully observed after the prediction is made. I wish to distinguish this type of gain in explanatory power from a more basic kind. In this case I propose to argue that what we have is really an instance of fruitfulness in making predictions. This is a criterion which overlaps the criterion of explanatory power but which I propose to consider at a later point as a separate issue.

Having distinguished the case of the prediction of novel data I can now turn to the more basic example with which I am chiefly here concerned. This is where a pair of rival theories C and D differ in explanatory power as follows. From D we can infer e_2 to e_n but from C we can infer e_1 to e_n. C therefore explains all that D explains and at least one other datum additionally. I propose to call this a quantitative gain or

advantage in explanatory power. The question at issue now concerns the preferability of the theory with the quantitative advantage in explanatory power.

The comparison of C and D in the previous paragraph attempts to set out in slightly more formal terms part of what is involved in the earlier discussion. The case of the defecting soldiers was the example there. In history, as in science, we prefer a theory with greater explanatory power of this kind. A theory which explains the defection of the Ghassanid prince from Heraclius and of Druze soldiers from the Israeli army[139] is preferable to one which explains only the one event or the one type of event.

I therefore propose the following account of the notion of a gain in explanatory power. Such a gain is qualitative if it renders the data in question more probable than they would otherwise have been. Such a gain is maximally qualitative if it renders the data in question certain rather than probable. Such a gain is quantitative[140] if the new theory explains more data and especially more varied data than its competitors explained. We have looked at examples of such gains or possible advantages in the case of rational beliefs about history and the like. Can an analogous case be made for greater explanatory power as a virtue of metaphysical beliefs?

As in earlier sections of this chapter I propose to discuss this issue with reference to an example selected from one of Swinburne's arguments. I have specified various ways in which an advantage in explanatory power might be estimated. Swinburne is, of course, working on Bayesian assumptions about

epistemic probability. So he discusses explanatory power in these terms. He sees explanatory power as expressible in terms of that part of Bayes' theorem[141] which assesses P(e/h.k)/P(e/k). In informal language explanatory power is determined by the probability of the evidence given the hypothesis (or theory) in question and our background knowledge, divided by the probability of the evidence on background knowledge alone. Applied to this particular example what is being assessed is whether the universe we experience is more likely to have come about given the truth of theism than given the truth of some rival theory. Despite my other reservations about the use of Bayesian probability theory, I do think that this part of his account is acceptable as long as one recognizes the importance of other criteria such as coherence, accuracy, scope and fruitfulness. Swinburne is long on coherence and simplicity and rather short on the others.[142] But the account of explanatory power is acceptable given this qualification.

In order to look more closely at Swinburne's use of the criterion of explanatory power I propose to select quotations from one of his arguments and discuss these. Again I do not intend to take issue with every point on which I might disagree with Swinburne, but rather to use his work as an argument which makes an appeal to explanatory power. He maintains:

> '... the occurrence of certain phenomena will confirm, i.e. raise the probability of the existence of God, if and only if it is more probable that those phenomena

will occur if there is a God than if there is not.'

He continues by saying that one assesses the explanatory power of theism with regard to those phenomena by asking:

> '... how much more likely does the existence of God make the occurrence of those phenomena than it would be if we do not assume the existence of God.'[143]

It is clear at the outset that Swinburne has decided to focus on one of the options outlined above rather than the other. He is not discussing an argument which claims that theism explains more data than its rivals. He is propounding an argument which claims that theism makes 'the phenomena' more likely than rival theories do. This supports my interpretation of what he is doing which I gave earlier. Whether or not this is how he ought to proceed, this is how he does proceed. I agree that it might be possible to restrict the rival theories to those scientific theories which remain strictly within the limits of physics and argue that science explains data within the universe whereas theism offers a quantitative advantage in explanatory power in providing an explanation of why there is this universe as well as the data within it. But this is <u>not</u> the example under discussion and <u>not</u> what Swinburne is considering. He is arguing for what is (on my classification) a qualitative rather than a quantitative advantage in explanatory power. He probably has his reasons for doing so.[144]

Having established which kind of argument this example provides we can now go on to consider it in a little more detail. Is Swinburne arguing for a maximally qualitative advantage?

Is he in other words claiming that given the truth of (his version of) theism the universe as we know it is a deductively certain consequence? Again no. This is not the argument he puts forward. He considers and rejects such an argument:

> 'On that view God has an obligation, or at any rate an overriding reason, to create the best of all possible worlds. This answer... has the consequence that the only states of affairs which we can expect to exist, if there is a God, will be ones belonging to the best of all possible worlds. The probability, if there is a God, that they will exist is 1; the probability that any other state will exist is 0.'

He goes on

> 'This answer seems to me to be mistaken. A God will not necessarily bring about the best of all possible worlds. For there is every reason to suppose that there is no unique best of all possible worlds.'[145]

Our concern is not with whether he is right or wrong to reject the notion of a best of all possible worlds. It is to note that he does not argue for the rationality of that view which offers maximal qualitative explanatory power. In fact he rejects such a view. Though in the main Swinburne claims to follow Leibniz here, he drops this element of Leibniz's argument.

What Swinburne does do is to reformulate a version of one of the eighteenth century arguments as one of his own 'C-inductive arguments'. He claims that the universe is 'very unlikely to come about but for God's agency' and so 'the existence of a complex physical universe ... is a good C-inductive argument'

for theism.[146] One of his reasons for choosing the version of the argument given by Leibniz rather than that of Clarke is the full treatment given to the latter by W.L.Rowe in 1975. Swinburne is however somewhat over-confident about the similarity between different versions of the argument.

> 'In so far as I consider one detailed example of a cosmological argument, I shall consider Leibniz's version, but most of my remarks will apply to most versions of the argument.'[147]

This cheerful assumption was printed a year earlier than the careful monograph on the history of the argument by Craig[148] which argues impressively for a classification of different types of such argument. Craig pays careful attention to the Arabic kalām argument as well as to Aquinas and Leibniz. He argues strongly for the difference between these versions and against the assimilation of the arguments by Aquinas to those of Leibniz.[149] I mention this as J.L.Mackie, whom I shall also cite, had the advantage of writing after Craig's work was published. This can be seen in his criticism of Swinburne.

> 'Although his starting-point is like Leibniz's, his conclusion is more like that of the <u>kalam</u> argument, in taking creation by a person as the one satisfactory beginning of things.'[150]

This is not just a historical niggle. The arguments are different in character and use different premises. If Craig is right, there is a crucial difference between the principles used as, or assumed by, the different premises.[151] But our concern here is with the criterion of explanatory power. Here we can see that Swinburne and Mackie are both using such a criterion, but

differing over whether it has been employed successfully. This is for us the important issue, not whether different versions of the cosmological argument have been inadequately distinguished. Both writers see greater explanatory power as an advantage.

Mackie's remarks clearly suggest that if the unexplained element were reduced this would be an advantage. His tactic is therefore to argue that there is no gain in explanatory power. He says of Swinburne:

> 'But without introducing the concept of something that contains its own sufficient reason, or whose essence includes existence - unsatisfactory though, in the end, these notions are - he has nothing to support the claim that by adding a god to the world we reduce the unexplained element.'[156]
> (The italics are his.)

This does not fairly describe Swinburne's position in fact, but we must let that pass. The point at issue which concerns us is the claim which Mackie rejects but which Swinburne asserts. This is the claim that theism 'reduces the unexplained element'. What Swinburne claims is that the universe is 'unlikely to have come about but for God's agency'. For him therefore theism gains in explanatory power if it can account for something which is otherwise unlikely. The gain in explanatory power which is claimed by Swinburne is what I have classified as a qualitative gain. His argument is that the universe is more likely to exist, as it does, given theism, than otherwise.

In fact J.L.Mackie is not wholly correct in saying that Swinburne's argument is like the Kalām argument. Swinburne

does briefly consider a version of the argument which claims the universe to be of finite duration, and explained by a person who caused its first state. But he gives much more weight to those forms of the argument which allow for the universe being 'infinitely old'.[152] But much more important for our purpose is the character of the gain in explanatory power which is claimed by Swinburne. This is a very complex issue but I hope that the main point at issue for our purposes will become apparent as the argument proceeds.

Swinburne is not in reality arguing that the existence of God makes the universe as it is more likely (than it would otherwise have been) in the same way as a law L and preceding conditions C make a historical event E more likely than it would otherwise have been. In each case we are confronted with a belief which is commended to us on the grounds that it explains what we experience. But the type of explanatory power in each case is different. In the one case L and C explain E because L and C cause E in the way that other events are caused. But in the case of Swinburne's Leibnizian argument the type of explanation is different. He points to the

> 'claim that everything not metaphysically necessary has an explanation in something metaphysically necessary.'[153]

He is very cautious in his treatment of this element in Leibniz's argument. What he does say is

> 'Leibniz claims that the universe is not metaphysically necessary, and so that its existence needs explanation. He may be right, but I cannot see how you can argue for this claim except in terms of the relatively greater

simplicity and explanatory power of a potential explanans.'[154]

If I understand him correctly, Swinburne is saying that such a claim is defensible, but by his mode of arguing by appeal to explanatory power.

It is clear enough that Mackie and Swinburne are evaluating metaphysical beliefs by considering their explanatory power. Even if the one claims, and the other denies, that the belief in question reduces the unexplained element, this is how they argue. But has the criterion here been pressed beyond its normal limits? I suppose we should not be surprised that a metaphysical argument[155] should invoke explanation in metaphysical terms. It seems clear enough that the type of explanatory power is similar in one respect to what we considered earlier and different from it in another. It is similar in that the claim is that the belief in question renders the evidence more probable than it would otherwise have been. The claimed gain in explanatory power is qualitative. But it is different in at least one crucial respect. The kalām version of the argument seems to use a notion of explanatory power closer to historical and scientific appeals to explanatory power. But Swinburne's Leibnizian argument appeals to explanation over and above that provided by reference to laws and preceding conditions. If the more Leibnizian version is regarded as the version of the argument at issue, then it raises difficulties for assessment in terms of the criterion of explanatory power used elsewhere.

Swinburne discusses both types of argument. In the case of the more Leibnizian version he envisages a situation such

as the following. For each of the (in this case infinite) sequence of states of the universe (S) God (G) brings it about that the appropriate law (L) operates and so brings about the next state. He writes

> 'We suppose that such a person G brings it about at each instant of time, that L operates, and so brings it about that for each S_{n+1} that S_{n+1} brings about S_n.'[156]

It is clear from the quotation from Leibniz which follows, that he envisages this as providing a gain in explanatory power in the following way. The reason for what is not metaphysically necessary (the world) is something which is metaphysically necessary. J.L.Mackie rejects the original form of this Leibnizian argument on the grounds that the principle on which it relies is not demonstrable. But Swinburne makes the more qualified claim that the principle scores in having greater explanatory power. Against this J.L.Mackie only replies that 'the concept of something that contains its own sufficient reason' is 'unsatisfactory', and so the unexplained element is 'not reduced'. This does not fully meet Swinburne's point. Swinburne's point is that there is a gain in explanatory power. Unless some more effective argument is deployed to exclude the type of gain which he is pointing to, he can justifiably claim that his argument is one which raises the probability of its conclusion.

There is clearly a case for arguing that theistic arguments can be scrutinized in terms of the criterion of explanatory power. There is controversy over how successfully they meet that criterion. But we can contend that if it is

rational to assess historical and other arguments for their explanatory power, then it is at least as rational to assess these arguments by a similar criterion. Perhaps in some cases the criterion of explanatory power is being used in a way which goes beyond the historical and other examples we selected for comparison. If so that may be ground for caution, but not for rejecting such an extended use of the criterion. We would need a more effective counter-argument before our caution turns to rejection.

Perhaps some such counter-argument will oblige us to add a further qualification and to exclude certain types of gain in explanatory power. But that is not to hand. In the meantime I propose the following principle.

EP1 'A belief which at least equals its competitors on the other criteria and exceeds them in explanatory power is to be preferred to its competitors.'

If the more sceptical protest that this principle is too liberal, I would offer the following reply. Of course we do not accept just any gain in explanatory power, but those gains which we reject, we do so precisely because they are achieved at the expense of losses on other criteria. Unless some <u>further</u> ground for rejecting gains in explanatory power is provided I stand by EP1. This concludes my consideration of explanatory power. We must move on to the discussion of the remaining criteria.

(4) <u>Fruitfulness</u>

A further criterion which is much debated in other contexts is that of fruitfulness in making predictions. We could formulate this criterion provisionally as follows. 'A theory which

successfully predicts novel evidence is preferable to a rival theory which is less successful in so doing, other things being equal'. We need to discuss the role of such a criterion, its requirements, and above all its suitability as a criterion for assessing metaphysical beliefs.

At this point it is possible to draw in some features of the earlier discussion. The issue of fruitfulness in making predictions was touched on at two earlier points in this work. In Chapter 5 I discussed it in relation to Swinburne's use of Bayesian conditionalization. It was raised there as that issue is a fundamental one which affects the whole of Swinburne's argument. It is central to his use of Bayes' theorem. The way he selects the evidence on which he assesses the probability of theism is a crucial matter. In assessing the probability of h on e it is essential to be clear just what e represents. Swinburne faithfully follows a standard Bayesian procedure in adjusting e whenever further evidence is fed into the argument. But I pointed out that this procedure was designed for the successive inclusion of fresh evidence. Swinburne, however, uses it almost exclusively for feeding in additional evidence from a set of evidence which is already to hand at the outset. This latter element is not in itself a ground for rejecting his argument. It is quite in order to consider existing evidence as forming a cumulative case. That I am not contesting. Indeed I positively endorse that part of his procedure. But the lack of consideration of fresh evidence is a different matter. This departs from the methods and criteria used in other disciplines. The users of Bayesian conditionalization,

and the followers of Lakatos, and others, are agreed on the importance of the prediction of fresh evidence in the natural sciences.[157] Fruitfulness in making predictions is there a central criterion. We must now take up again the question of whether it can be given a much reduced or even negligible role in the assessment of metaphysical beliefs as Swinburne contends.

At this point another element in our earlier discussion comes into play. In Chapter 4 the role of historical argumentation was discussed. In that earlier discussion I considered the place in historical study of the criterion of fruitfulness in predicting fresh evidence. The availability of fresh evidence to historians is relatively rare. New documents do become available, previously unknown records appear, sometimes whole cities are rediscovered by a chance event which precipitates archaeological excavation. But the testing of historical theories by such novel discoveries is relatively rare. It does not happen with the frequency that theorists such as Lakatos claim that it does in the natural sciences. There is therefore something of a case for arguing that at least in historical study this criterion occupies a lesser place. One might then argue that an analogy between historical and metaphysical beliefs would warrant us in giving predictive success a much lower place in the latter domain also. But considerable caution is needed in any such use of an argument for similarity between historical and metaphysical beliefs.

I argued earlier that prediction plays a very small

overt role in historical study, but that it has an important implicit role. Historians usually reject with vigour any suggestion that their study of the past has lessons for the future. But that is not the sense in which prediction is being discussed here. I argued earlier that the role of implicit prediction in historical study is central to the notion of historical reconstruction of the past. The account of the past that we construct is a provisional one. Implicit in our conclusions about the historical past is a condition. The condition is that if fresh evidence becomes available, our account can be reassessed. Of course even the fresh evidence is itself interpreted evidence. That I do not deny. But if we make a new discovery the statement or statements describing that discovery can be used to test the earlier account. The consistency between the two sets of statements is tested. If the two sets are inconsistent at least one must be revised or rejected.

In the examples I discussed earlier, the discoveries of Coptic texts were seen to have raised the probability of the theory of an independent early Gnosticism. Fresh discoveries also led to the revision of judgements about the date of an ancient Jewish synagogue. To these examples one could add a more recent case. Under the Treaty of Versailles in 1919 responsibility for starting the war in Europe in 1914 was fixed by the allies on Germany. Liberal historians later tried to discredit this theory, and sought to prove that all the major powers were responsible. However the work of Fritz Fischer in 1961 vindicated the theory in the eyes of many modern

German historians. Fischer used documents others had not tracked down, or considered relevant. Here we see a theory which began as a highly political accusation and which was later contested. However it contained an implicit prediction which subsequent study of previously unused documents strikingly vindicated.[158] What had been discredited as political propaganda, much later acquired considerable new support from an unexpected quarter.

Fruitfulness in making predictions is an important criterion for testing theories. I have recapitulated points made earlier in this work in order to underscore the relevance of those earlier discussions to this issue. The earlier discussions draw attention to the role of the criterion in the work of philosophers of science, and also to the implied use of the criterion in historical argumentation. But what are we to make of this criterion in relation to the assessment of metaphysical beliefs? We must approach that issue step by step. I have suggested that we consider a formula such as the following:

F1 'Other things being equal a theory which successfully predicts novel evidence is preferable to a rival theory which is less successful in so doing'.

The initial clause is intended to remind us that other criteria are also being used. The formula is suggested as a provisional account of the criterion of fruitfulness. Various amendments are no doubt needed of which one might be

F2 'Where statements describing novel evidence are in conflict with theory B but are predicted by theory A

we should prefer theory A (unless there is good reason to doubt those statements).'

Here the clause stating the exception reminds us that statements describing evidence are themselves theory-laden and themselves subject to scrutiny. (This is ground for caution, but not ground for thorough scepticism about the assessment of theories. I have already at an earlier point argued against the view that theory-ladenness leads to radical incommensurability.)

One of the issues raised by F1 and F2 is the provision of a suitable account of what constitutes 'novel' evidence. This has been much discussed. One account defines novel evidence as evidence that was previously unknown. But this excludes too many instances of theories which have been strikingly confirmed by evidence which was known to others but not considered by the person who constructed the theory.[159] Another suggestion attempts to capture this distinction in two clauses which mention

a) cases where the theory entails facts previously unknown to the scientific community

or b) cases where it was unknown to the scientific community that the theory explains these facts.[160]

I propose a variation on the latter which runs as follows:

N1 'If e is a statement describing evidence, then e is novel if the evidence which e describes was previously not available to the academic community or if e was not seen by the academic community to be predicted by the theory in question.'

This would allow for cases where scientific or historical

evidence has long been available, but where it is suddenly realized that it confirms a theory which had been formulated without reference to that evidence.

The criterion of fruitfulness is closely related to two similar criteria for testing theories. One of these is the criterion of refutability or criticizability. The other is the requirement that a theory be open to testing by means of a 'crucial experiment'. The latter demand is more severe if it is envisaged as demanding that we must be actually able to devise an experiment which would decide between rival theories. Theories would not be distinguishable if no such experiment were imaginable, but being able to imagine a 'crucial experiment', and being able to devise one, are very different matters. My formulation of F2 above is aimed at capturing that element in the demand for a 'crucial experiment' which lies between these two cases. It is neither so vague as to allow the novel situation to be purely notional, nor so strong as to assume that a 'crucial experiment' can always actually be devised. With regard to criticizability the situation is a little different. A much earlier debate focussed on falsifiability as a criterion of meaningfulness.[161] The issue under discussion here is different. It is concerned with rational preference for one belief as against its competitors. 'Falsifiability' or criticizability is a factor here also. It could be described as the capacity for a theory or a belief to live dangerously, by exposing itself to refutation, while not being refuted. It is some kind of index of successful brinkmanship (especially as Popper describes it). How then is it related

to my F1 and F2? I would argue the following. A theory, if it is to be at all rationally believable, must at least expose itself to the risk of 'falsification'; it must be criticizable. But it is the more rationally believable the more it risks predictions which are successful, and the less its rivals do this. I do not claim that F1 and F2 provide a definitive account of the criterion we are considering. But I formulate them as provisional accounts, intended to sketch out some of the issues involved. The need to subject such formulations to further improvement is clearly endemic in the whole discussion of such criteria.

What then are we to make of the applicability of some such criterion to metaphysical beliefs and especially to our chosen example? Swinburne does mention the criterion but argues against its necessity as follows:

> 'It will be useful... to make another important point... It is sometimes said that we are only justified in accepting a hypothesis if we have tested it by finding that it predicts certain events and then waited to see whether or not those events happen... although we often test hypotheses in this way, we do not have to do so if they are to be rendered probable by our evidence...'[162]

The last point is no doubt correct, as is his statement that successful prediction is not implied by Bayes theorem. But Bayesian conditionalization does envisage just such a test, and Swinburne does not mention the point, though he uses one of the main elements of Bayesian conditionalization, namely the successive adjustment of e to accommodate more and more evidence.

Also even if the criterion of prediction were in conflict with Bayes' theorem, that might be ground either for suspecting the total adequacy of Bayesian confirmation theory, or perhaps more justifiably, the reliability of a particular way of using Bayes' theorem.

Swinburne continues his argument by saying that Newton's theory of motion was judged highly probable on the evidence then available, though making no new immediately testable predictions. But that point can be accommodated by my formulation of the criterion of fruitfulness. My F1 and F2 do not demand fruitfulness of every theory, but merely say that a fruitful theory (or progression research programme) is preferable to a less fruitful one (or to a degenerative research programme). If a theory scores highly on other criteria and no more successful rival threatens it, then that, I agree, is sufficient to make it rationally believable.

Another objection by Swinburne runs as follows, and turns on a clause which has already given us trouble:

> 'More generally, whether e renders h probable surely cannot depend crucially on whether we had thought of h before we saw e. Probability would become a highly subjective matter... if that were so.'

This does point to a real difficulty, but it is not so great a difficulty if we phrase the requirement relating to fresh evidence carefully. The crucial factor is the comparative success of rival theories in the face of novel evidence. If we previously considered e unlikely in any event, or unlikely on our previously favoured theory, then the occurrence of e, or the

realization that e is likely on that theory, will oblige us to reassess the probability of the theory. In any case we cannot make of Bayes a shibboleth. Bayesians have recently had to defend their view in terms of its adaptability to requirements seen to be appropriate on other grounds. For instance they have come to argue that they can cope with the notion that successive positive instances raise the confirmation of a theory by decreasing amounts.[163] The controversy between Bayesians and others is an exceedingly complex matter and cannot be given short shrift. This is why I object to particular elements in Swinburne's arguments over Bayesianism, but reserve judgement about the outcome of that larger issue.

Despite the fact that Swinburne minimizes the role of the criterion of fruitfulness, he does himself reject a rival theological view on the ground that it is ad hoc. He discusses Plantinga's use of the free will defence to account for natural evil as caused by free agents such as fallen angels. This Swinburne rejects as follows:

> 'For if the hypothesis that these angels exist and have power over nature is added to the hypothesis of theism to save it from falsification, then it has the status of an ad hoc hypothesis'.[164]

He urges against this the objection that an ad hoc hypothesis complicates a theory to which it is added and so lowers its probability. But that is not the only factor. (Some might argue that Plantinga's defence is not ad hoc, but part of traditional theism, but we are discussing criteria, and must let issues of this other kind alone.) What constitutes an

ad hoc element in a theory has been extensively debated by Lakatos and his followers and opponents. In this context a hypothesis is judged ad hoc in various ways. One distinction is between an addition which is ad hoc$_1$ (it doesn't predict) and one which predicts but is ad hoc$_2$ because unsuccessful. On either of these views the description of a supplementary hypothesis as ad hoc is crucially linked with the criterion of predictive fruitfulness. No doubt Plantinga could, if so minded, adopt a defence similar to that of Hick. He could argue that his theism, plus a free will defence citing fallen angels, is not ad hoc. He could invoke eschatological verification, and argue as follows. Not only does theism predict that the faithful will find themselves in the divine presence, but also that the unrepentant and the unfaithful will find themselves, like Faust, in the clutches of those fallen angels which his version of theism mentions. The argument between Swinburne and Plantinga does have interesting points at which the criterion of predictive fruitfulness does become relevant.

So far I have considered the criterion in question as a positive one. It favours theories which score, or promise, predictive success. But what of theories which undergo disconfirmation? These can be said to make explicit or implicit predictions which fail. Surely we must interpret the criterion in such a way as to capture this point also. Let us construct an example which attempts to capture a relatively unsophisticated view of the disconfirmation of certain forms of theistic argument. Some such positions might be thought to have contained overt, or implicit, predictions that the

earth was the centre of the universe, that human beings did not descend from other species by natural selection, and that the Bible does not contain historical error or self-contradictions on historical or other topics. Of course such matters could be classed as peripheral hypotheses, rather than part of the central theory. But the disconfirmation of such hypotheses, though peripheral, does have some implication for the rational credibility of the central core of a theory.

It might be argued that there are other versions of theistic belief, which do not contain the peripheral items in question. So those who now hold such beliefs could claim that their position is unaffected by predictive failures of this kind. This is fine as long as one works with a static view of the testing of theories. But if one takes seriously the concept of long term research programmes being progressive or degenerating, the situation looks very different. According to this view, theories and beliefs are not independent items which each stand or fall on their own merits. The method of evaluating research programmes looks at sequences of theories. It asks how they stand up to successive tests, and how they have been revised over time. It is the particular genius of Lakatos that he identified, and articulated, this aspect of the study of method and criteria.

From this perspective, a series of theories which undergoes successive disconfirmations is non-progressive. If, after a failure, a theory is restated so as to make fresh predictions which succeed, it is reinstated as part of a progressive research programme.[165] But if a tradition staggers from one

disconfirmation to another, and survives by dropping peripheral hypotheses it is making use of a content-reducing strategem.

This issue is put sharply, but Lakatos was never one to pull his punches. Can such a forthright insistence on the rigorous use of this criterion be resisted? It captures and goes beyond Popper's thesis of the asymmetry[166] between confirmation and disconfirmation. For Popper one additional 'corroborating' instance only slightly raises the degree of corroboration whereas one 'falsifying' instance damages a theory heavily. This is a point often overlooked by conservative defenders of biblical narratives. In pointing to cases where they claim that archaeology proves the bible true, they overlook the asymmetrical impact of even one disconfirmation. But Lakatos goes beyond Popper. He is concerned not only with isolated predictive successes and failures, but with the effect of a series of successes or a series of reverses, the latter forming what he calls a 'degenerating problem-shift'.

Can the crude example cited above be countered by sophisticated defence in terms of the criteria and methods to be used? One line of defence would be to argue that as long as the reverses only affect peripheral hypotheses, the reverses are not serious. This defence could appeal to Glymour's maxim[167] that we should prefer theories whose central rather than whose peripheral hypotheses are tested. Provided that some clear distinction between central and peripheral items is on offer, and provided that the central core is tested, this defence seems promising.

Another supporting line of defence would be the following.

It can be argued that long established theories or beliefs are less in need of testing against novel evidence. If a theory has stood the test of long experience, we would not expect it either to produce or to require predictions of novel data. We might however wish to stipulate that it should be free of disconfirmation, especially (as argued above) in relation to its central core. A long standing belief can also, it would seem reasonable, afford to shed a few incidental elements. Indeed we would expect it to do so. Very few scientific or historical beliefs survive totally unchanged over a long period of time. But on the other hand many of our beliefs do remain substantially intact even when Kuhn's 'paradigm shifts' cause metaphorical earthquakes elsewhere. Though I have from the outset agreed with some fallibilists that any one of our beliefs may be false, I would still maintain that some of our beliefs about history and science have survived relatively unchanged. In such cases to demand fruitfulness in making new predictions is inappropriate. This line of defence is preferable to some other arguments. For instance the contention that history uses the criterion less does not offer nearly so promising a line of argument, as I have indicated above. The criterion is important there, when implicit predictions can be tested.

Another factor which deserves mention is the notion of tenacity in persevering with a theory. Both in history and in other disciplines a theory is not abandoned as soon as it suffers some disconfirmation. It can be considered rational to persevere with a theory. The theory can be modified, or the disconfirmatory evidence challenged. Or it can be argued

that future evidence will shift the balance of probability in favour of a theory which has suffered a temporary reverse. In such circumstances it may be rational to go on maintaining the belief in question, especially if non-evidential considerations come into play. I discussed earlier, in chapter two, the issue of maintaining a belief on non-evidential grounds. Here the issue is the virtue of tenacity as a rational factor. The success of one theory over its rivals can, we are told by the theorists of theory, only be decided in the long run. But how long is the long run?[168] This principle of tenacity can be considered as a technical equivalent of part of what is involved in regarding faith as involving greater commitment than mere belief. But our concern is with criteria for rational belief, rather than with those aspects of faith which go beyond the bounds of rational belief. In the case of beliefs about matters of history and natural science, tenacity is recommended in the hope that a research programme which is at present in decline will become progressive again.[169] In other words it implicitly predicts future success in terms of the criterion of fruitfulness which we are considering.

What then of that criterion? When I formulated my version of F1 and F2 I did so in a way which took account of some of the points about fruitfulness we have just considered. This version of the criterion does not make fruitfulness a sine qua non. It is rather a matter of evaluating competing beliefs to determine which it is most rational to hold. A belief may be a rational belief if it is consistent, and accurate, and has good explanatory power, and is not unduly complex, without

predicting novel facts, provided that certain other conditions are met. No rival belief should exceed it in accuracy, simplicity and explanatory power. The preferred theory should not undergo predictive failure at least with regard to its central core. Also there must not be a competing theory which is at least equal to it on the other criteria, and also superior to it in fruitfulness.

This formulates the criterion of fruitfulness in comparative rather than in absolute terms. It attempts to capture the notion that we may rationally believe something despite its unfruitfulness in making successful predictions, provided that an otherwise equally credible belief does not exceed it in fruitfulness. This last consideration raises an issue which will continue to give trouble and remain unresolved. There are likely to be cases of clashes between the criteria. One belief or set of beliefs may prevail on some of the criteria, and a competitor do better on other criteria. But before confessing that this issue remains unresolved we must turn to the criterion of accuracy.

(5) Accuracy

The last of the criteria which I propose to examine is that of accuracy. Though discussed last I would rank it earlier. It has certain obvious connections with earlier criteria. Also, if as I suggested earlier, the criterion of simplicity should be given a lower ranking then that could be ranked fifth and accuracy placed second. But any ranking that can be given will only be a weak one, as I shall argue later.

It is appropriate that any beliefs which we hold should

actually do better justice to the evidence than any of its rivals. In this sense it must be accurate. But what is it to 'do justice to the evidence' or 'give an account of the evidence' in the sense here intended? Clearly it must be something different from fruitfulness in predictive success. The requirement of accuracy is that a theory account for existing evidence. How then does it differ from explanatory power? An accurate belief correctly describes the evidence in question. A belief with good explanatory power makes that evidence more likely or more evidence likely (or more evidence more likely) than its competitors do. Accuracy is a matter of correct description rather than explanation, and deals with existing rather than fresh evidence. Accuracy is also different from the question of consistency with other beliefs. We overrule the conclusions of existing beliefs in the interests of accuracy. All the same there is some connection here. We may decide that a belief more accurately describes the data because it reduces anomalies. The estimation of time by use of a pendulum rather than by a water clock is deemed more accurate as it reduces anomalies elsewhere.[170]

This criterion is close to a requirement specified by Newton-Smith in slightly different terms. He requires that a good theory at least preserve the observational success of its predecessors.[171] In the case of historical examples a description is more accurate if more precise and if it takes account of more data. So we prefer 'Caesar was assassinated on the Ides of March 44B.C.' to 'Caesar died in 44B.C.' or 'Caesar was killed on the Ides of March 44B.C.'. The statement that

Caesar was assassinated uses a term with smaller extension and so more precise than the statement that Caesar was killed.[172] In some contests between rival beliefs only a certain degree of accuracy or precision may be of consequence. In the case of history, and certainly in the case of religious belief, accuracy might also be invoked in a slightly different sense. Attention to symbolic, poetic and metaphorical features of the language in question can be of great importance. This is a different process from examining accuracy by strict measurement. It is, however, not wholly dissimilar. Attention to literary nuances can be as painstaking a matter as that of measuring minute particles or getting dates right. But it is not an issue with which I propose to tangle here.

An issue which is of more direct concern here is the problem of accounts of observation or accounts of data being theory-laden. Indeed one could go farther and argue that without prior conceptualization we could not have any experiences at all, or make any observations. It is, however, one thing to argue that our accounts of the data are theory-laden, and another to claim that this renders them incorrigible. Here an adaptation of my earlier argument against radical incommensurability is relevant. One astronomer may experience a star where another experiences a planet. Or one may experience an object as a star, the other may experience it as a planet. But in this case at least we can adopt a tactical device to enable first comparison, and then preference to be made. The strategem in question could be called 'theoretical descent' by analogy with 'semantic ascent'. The two astronomers could

agree on the observation of a 'bright object' at a certain place and time. The latter description is still laden with theory, but with a theory of lower level. For the object to be designated a star certain other conditions would be needed, and different ones for it to be described as a planet. In the case in question the latter did indeed turn out to be the correct description. I do not necessarily claim that this argument resolves all cases of conflicting descriptions of data, but it points to one way of solving such difficulties. For the moment I wish to stay with the more straightforward examples of a preference on grounds of accuracy for a particular theory about events in the historical past.

An example of a gain in accuracy in historical archaeology can be found in the case of dating by radio-carbon. This method was developed in 1948 by Libby. By measuring the extent of the decay of carbon 14 in samples, the date of the samples can be calculated. Thus the absolute chronology of a whole range of archaeological finds can be determined by the use of radio-carbon dating. Given the reliability of the radio-carbon method any historical theory about ancient artefacts which agrees with the results of radio-carbon dating is to be preferred as more accurate than any historical theory which conflicts with such dating. One must add the proviso that such conflict exceed the normal margin of error. Even so this is sufficient to favour many theories against a host of rivals. But this is not the whole story. Theory A about an ancient city is preferable to theory B if judged more accurate, because more in accord with the results of carbon 14 tests. This inference

depends on theory A not suffering some major disadvantage on another criterion. It also relies of course on the assumption that radiocarbon dating is more reliable than say analysis of site strata. But to contest the comparative accuracy of radio-carbon dating would require a greater disturbance of our other beliefs than to accept it in this case.

There has, however, been one successful instance of a challenge to the accuracy of radio-carbon dating. The method was itself compared with the results of counting tree-rings on wood up to 8,000 years old. The two systems diverged slightly. As a result the estimates, reached by radio-carbon methods, of dates prior to 1,000B.C. have been recalibrated.[173] A crude radio-carbon date of 2,450B.C. is now converted to a revised date of 3,000B.C.. The crucial assumption of course is that which asserts that the quantity of radio-carbon varies more than the formation of tree rings. But other evidence does support this assumption, levels of carbon 14 have fluctuated in this century.

We prefer more accurate to less accurate historical theories. Our judgement of what is an appropriate standard of accuracy may itself depend on anterior reasoning. For instance, to reject the greater accuracy of revised radio-carbon dating would oblige us to discard more of our cherished theories, and to accept a theory with less predictive and explanatory power. No doubt similar considerations favoured the shift in science from calculation of time by the water wheel to the use of a pendulum, and then to quartz vibration. But we must let that pass.

Is a preference for more accurate theories an appropriate criterion in the domain of metaphysics? In the case of Swinburne's metaphysical theism one might argue that even his C-inductive approach is bound to rely largely on factors which cannot be measured empirically. So in some cases we might have to allow that accuracy could be tested, if at all, only for the peripheral hypotheses of some metaphysical beliefs. In that case we would have to formulate our criterion rather carefully.

A1 Prefer a theory which is more accurate than its rivals, or prefer it if more of its component hypotheses are accurate than is the case with its rivals, other things being equal.

Again our last clause in A1 is designed to refer to the role of the other criteria. But is there an area where a procedure such as Swinburne's does allow for an estimate of the accuracy of a theory?

One place where Swinburne does come close to the issue with which I am concerned is in his discussion of arguments from religious experience. I wish to say immediately and very clearly that I have considerable reservations about the nature of Swinburne's arguments in that chapter. On the one hand I hold that his principle of credulity is too liberal, and allows him to make larger claims for the evidential value of some types of religious experience than is warranted. On the other hand I would argue that his examples of religious experience are strange and strangely assessed. Here I think he has in fact underestimated the impressive character of

certain other types of religious experience and their evidential value. It may seem strange to criticize him from two different directions but my objection is that he has offered too trite an account of religious experience, and that his principle of credulity allows him to claim too much from an inadequately grounded account of the experiences. But I say this only to make it clear that I in no way subscribe to the details of Swinburne's account. The issue of concern here is, however, whether a metaphysical belief is preferable to its rivals on grounds of accuracy. We must stay with this issue of method, and object to points of substance in Swinburne's account only when strictly necessary.

The crucial factor in Swinburne's argument is his 'principle of credulity'. He argues that such a principle is needed to provide a proper account of other experiences and that it cannot reasonably be excluded as not providing a good account of religious experiences.[174] He claims that

> 'a religious experience apparently of God ought to be taken as veridical unless it can be shown on other grounds significantly more probable than not that God does not exist.'[175]

In fact he does include some restrictions on this principle. It is, however, a bold contention even when qualified somewhat. I take it that he is claiming in effect that a theistic account of religious experience is preferable to any other account on the grounds that it is more accurate or more empirically adequate than its rivals. Even if Swinburne himself does not use quite that terminology, other writers do.

For instance Long regards H.D.Lewis as claiming that religious experience is only adequately understood if reference is made to a reality other than the person or persons to whom the experience comes.[176] Katz is not so directly concerned with the transcendental reference of religious experience as with questioning the thesis that 'mystical experience is always the same or similar in essence'. He argues that such a claim would have to be demonstrated by

> 'recourse to, and accurate handling of, the evidence, convincing logical argument, and coherent epistemological procedures.'[177]

In fact Katz argues vigorously that equation of nirvāṇa and devekuth in Buddhist and Jewish mysticism rests on inaccurate descriptions of the two traditions of mysticism. These writers do seem to agree that accuracy of description is one factor in assessing beliefs which rest in one way or another on evidence of religious experience.

In the case of J.L.Mackie's critique of the more far reaching claims that religious experience supports theism, he does not specifically mention accuracy of description. He does, hardly surprisingly, focus on the question of whether natural histories of religion can provide a 'better explanation' of the data.[178] But though he does not specifically mention the point, there is nothing in his argument to exclude the principle that other things being equal we should favour an account of religious experience which is more accurate in its description of the experiences. The more serious point is, however, not so much whether other writers do consider accuracy of description as

rendering a belief more rational, as whether we can formulate such a principle satisfactorily. There are several real difficulties about the notion of an accurate description of an experience.

One of the main difficulties which will need to be considered is whether the device of 'theoretical descent' can be readily used to enable accounts of religious experience to be compared. If the kind of religious experience people have is to some extent bound up with the religious language and symbols and expectations that each person has, then it will be very difficult to assess the accuracy of the description in relation to the experiences. Even so some appeal to accuracy may be made. In effect Katz does this. Though he points to the difficulties of distinguishing between interpretation and experience, he does use an argument which appeals to accuracy. He rejects the thesis that different types of mystical experience can be divided into a small class of 'types' which cut across cultural boundaries. He does so because he argues the description of devekuth and the description of nirvāṇa, for instance, have been inaccurately compared. What is at issue here is not so much whether Jewish mystics accurately describe their own experiences, as whether those who compare different traditions of mysticism have accurately attended to the details of those descriptions. We could call this a second level application of a criterion of accuracy.

Some of Swinburne's examples do envisage something more like our notion of theoretical descent. In this respect his discussion is valuable, but his examples are sometimes very

strange. For instance it is hard to take seriously discussions based on examples such as

'I saw Poseidon standing by the window' or

' I am alone and seem to see and talk to a figure dressed in white which I take to be an angel.'[179]

In each case the context in the appropriate literature (whether Homer or the Bible) has not been taken into account. We would need to take note of stories in which Odysseus says he has been shipwrecked by the malevolent fury of Poseidon, for instance. This latter example would at least pay better attention to literary context.

But Swinburne is on surer ground when he insists on two basic distinctions. He argues for internal rather than external description. By this he means that we should use language which does not prejudge the issue at stake. He distinguishes descriptions like 'I heard a bus' from those like 'I heard a noise that seemed to come from a bus'. The one does, the other does not prejudge the issue of whether a bus was there. Then a further distinction is needed between things which merely seem to be F, and those which seem to be F in an epistemic sense. In the latter case we are inclined to think that they are F. Here I adapt and abbreviate his argument, but I think it makes a useful distinction.

Swinburne's examples fall into five groups. He cites experiences of God as follows. 1) Those mediated by common phenomena (seeing the world as God's handiwork). 2) Those mediated by public but unusual events (visions of Mary at Fatima). 3) Private experiences which can be described by normal sensory vocabulary (a dream vision of an angel).

4) Private experiences which are hard to describe (some mystical experiences). 5) Experiences which do not come via sensations (a conviction that one has a vocation from God which does not depend on any auditory sensation). Where Swinburne's account is weak, is in his preference for rather uncritical accounts of visions. But his classificatory scheme is superior to the examples which he cites to illustrate it. Perhaps we could briefly digress for a moment to include some additional examples, though our main purpose is not to discuss the problems of arguments from religious experience, but to claim that in such arguments appeal to a criterion of accuracy is appropriate.

My additional examples would include one from a 17th century writer and two from Long's discussion of an argument by H.D.Lewis. Long cites Lewis as pointing to an awareness of an irreducible mystery and an enlivened sense of some supreme and transcendent reality as involved in the being of anything at all.[130] Robert Barclay, a 17th century writer described his experience of early Quaker meetings as follows:

> 'For when I came into the silent assemblies of God's people, I felt a secret power among them, which touched my heart; and as I gave way unto it I found the evil weakening in me and the good raised up.'[181]

One could add to this example that of a sense of unmerited grace. A further important element is also found in the arguments of Lewis as described by Long. This is the character of prophetic ethical demand descibed as follows:

> 'we are directed by the prophet beyond the moral insight

itself to the apprehension of the transcendent which is understood to be the source and ground of these moral obligations.'[182]

What we must consider is whether an argument which appeals to such data can be assessed in terms of its accuracy.

Swinburne's way of using such an argument is by appeal to his principle of credulity. He argues

'If it seems (epistemically) to S that x is present, that is a good reason for S to believe that it is so, in the absence of special considerations - whatever x may be.'[183]

His special considerations restrict the principle. These ask if the subject is under an influence which distorts perceptual judgement, or lacks the required capacities in other contexts, or has insufficient experience in such matters, or is working with an inadequate description, or if it is very likely on other grounds that x was not present. Rowe is highly critical of applying a principle of credulity to religious experience[184] on the grounds that the argument fails if we have no means of distinguishing between delusory and veridical experiences. Rowe's riposte seems as dismissive as Swinburne's principle is optimistic. Attempts to distinguish between genuine and mistaken accounts of such experiences are not lacking. But our concern is not with the success or failure of such attempts as with the principle that a criterion of accuracy is a relevant test.

There is clearly grave difficulty in transferring principles (such as that of credulity) from cases where we are dealing

with what is publicly observable to cases chiefly involving private sensations. However we have already noted at least one more successful type of appeal to accuracy. We can at least criticize as inaccurate those theories which rest on inaccurate accounts of people's descriptions of their experiences. Thus it can be argued that Swinburne fails to note the special characteristics of the literary use of dream visions in Matthew. Also Katz rightly criticizes the thesis that mystical experiences are similar, on the grounds that the descriptions have not been accurately studied. But can we do more? I would suggest two possible lines of approach here. The first relates to Katz's claims that his pluralistic account of mysticism is preferable because more accurate in that it accommodates all the evidence and does justice to the specificity of the evidence. His appeal to secondary accuracy suggests that an appeal to primary accuracy cannot be made. But if different accounts of experiences of 'God', 'Brahman' and 'nirvāṇa' imply incompatible beliefs, we cannot rest content with pluralism. The primary accuracy of at least some of the descriptions is called in question, even if we have, at present, no very definite way of resolving the dilemma then facing us. Further attention to the question of primary accuracy is also demanded by the phenomenon of change of belief. Suppose someone experiences e_1 which is closely bound up with interpretative schema T_1 and subsequently experiences e_2 which is closely bound up with interpretative schema T_2. Further let us suppose that this person concludes that T_2 and e_2 require a re-evaluation of T_1 and e_1. In such a case either T_1 and e_1 or T_2 and e_2 is called in question.

Our concern is with criteria. I have drawn attention to some more substantive problems in this section simply in order to show that I am aware that application of a criterion of accuracy in certain areas is highly problematical. I have, however, at the same time drawn attention to the relevance and indeed necessity of citing the criterion even if we do not always have the means of applying it in every case. The principle at issue will therefore need to be stated in comparative rather than in absolute terms. I am not arguing that only those beliefs are rational which, amongst other things, are fully tested for accuracy. I am arguing that between rival beliefs we should prefer as rational that belief which most satisfies tests of secondary and primary accuracy where such tests can be applied. Once again this preference is subject to the comparisons between rival beliefs which we make on the other criteria.

This concludes the discussion of criteria for rational belief in this area. Despite the sequence of the sections I have indicated above a slight preference for a different order in terms of rank. This would place consistency first and accuracy second, explanatory power and fruitfulness would then follow, but simplicity be moved down into fifth place. But the question of ranking will reappear shortly.

Conclusions

My main conclusion can be stated briefly. It is that reference to the criteria specified above are at the heart of assessing the rationality of at least some metaphysical beliefs as well as being central to the assessment of historical and other beliefs. These criteria are consistency, accuracy, explanatory power, fruitfulness and simplicity.

Can we provide an independent justification of the rationality of rationality? That I doubt. Such a task is comparable to the notoriously problematical tasks of deducing deduction, or justifying induction. One might provide a more limited defence. One can argue that any belief which is preferred on these criteria is defended by a procedure as rational as that used in preferring other beliefs on these or similar criteria. So if metaphysical and historical and scientific beliefs are judged rational by reference to these or similar criteria, then the method of declaring one set of these beliefs rational is as rational as that of defending the others. That is not an insignificant claim.

There might be further ways one could consider the defence of the criteria for declaring beliefs to be rational. One could consider the counter arguments of sceptical reasoners, and ask whether they in fact make moves which depend upon an appeal to consistency, accuracy, explanatory power, fruitfulness and simplicity. For instance those who use historical examples to cast doubt on scientific methods would rightly be criticized for assuming the reliability of their account of the historical past, in this case of the history of science, in order to

discredit some instances of scientific method. By such means one can defend method against those who are against it. But I only lightly sketch such a defence. It would need a longer account in some other work.

Does the theory of rational belief which I have outlined apply to itself? There is no inherent objection to suggesting that the theory of rational belief itself be assessed with reference to criteria such as consistency, accuracy, explanatory power and simplicity. It was one of the merits of advances made by Popper that his theory of the logic of scientific discovery was fruitfully applied in other fields. So the application of such theories to themselves may not be as problematical as the application of the verification theory was to itself. I do not claim novelty in citing the criteria, but only in considering further implications of appealing to criteria which are already in use.

The chief problem with the whole method of appealing to criteria as I have, is that which arises when one theory scores well on some criteria and another theory on other criteria. The problem of a clash between the criteria does raise difficulties at least in some cases. Kuhn noted this problem in the case of scientific controversy.[185] It also arises in the areas with which this work is concerned. It is like disputes in textual criticism. One variant in a text may be preferable on one of the rules of the discipline, another on another. I regard this as a more serious difficulty than incommensurability on the grounds of allegedly radical meaning variance. What is to be done in the face of a clash of criteria? One suggestion

would be some kind of ranking. This solution cannot be pressed too hard as notorious exceptions may upset it. But should we weakly acquiesce in the view that there is no decision procedure at all? I think not. We do not have a decision procedure which will cover every case. But that is not the same as being without grounds for making preferences in many cases.

I have already dropped hints about factors which would favour a weak ranking of the criteria. I have, for instance, criticized Swinburne for giving simplicity too strong a role. I have also agreed with a widespread assent to the view that internal consistency is normally a sine qua non. So internal consistency is normally the first criterion. Consistency with other beliefs and accuracy follow closely. Satisfaction of these criteria should only be overruled when we can argue that the 'other' beliefs are less well supported than the one we are assessing, or when the massive success of a rival belief obliges us to reassess the accuracy of our previously favoured belief. Explanatory power and fruitfulness in making predictions follow on closely together. A theory which scored well here, but which lost some simplicity in doing so, is a theory we would prefer. But we would have reservations about a theory which explained a lot, or predicted well, but was inconsistent or inaccurate. So perhaps some kind of weak ranking might help reduce the problems raised by clashes between criteria. But these clashes cannot be eliminated altogether. Also a massive advantage on one criterion might well upset our weak preferences in terms of ranking. If it really is the case that quantum mechanics

is accepted for its many successful predictions despite inconsistency with other well grounded beliefs or perhaps even internal inconsistency, then such a ranking would be upset. (Unfortunately the extent of the clash in this case is highly controversial.) In the case of metaphysics some might argue that theism scores so well in explanatory power that any incidental inconsistencies whould be resolved by adjustment, or by toleration of anomalies, rather than by subordination of the gain to the losses. It is hard to disallow all such cases of an upset to our weak ranking.

The existence of clashes between criteria must be taken seriously, but it should not be exaggerated. It does not licence thorough scepticism or anarchism. In many established disciplines we have longstanding beliefs which satisfy the criteria. Without striking agreement on many beliefs, the human community and the academic community could not function as it does. This is perhaps a kind of doxastic equivalent to Wittgenstein's dictum quoted in the opening pages that a doubt which doubts everything is not a doubt. Yet though there is much agreement, there are clashes of criteria, and there are disagreements. Whether such disagreements occur largely in areas where there are clashes in criteria would be a whole field of study in itself. A positive correlation would raise the explanatory power of the view taken here, but other causes of such disagreement undoubtedly exist. Interests differ, and differences of interest contribute to differences of belief. But our concern is with rational belief and the fascinating variety of other factors affecting diversity of belief must be left alone here. My aim has been to focus on

what renders a set of beliefs rational and to emphasize this. I do not deny all voluntary elements in relation to belief, nor that our interests affect our decisions in this area.

Someone might argue that we should treat rational and irrational beliefs symmetrically when asking why people believe what they do. Why, it might be asked, should sociologists refrain from exploring those interests which encourage people to hold beliefs we deem rational? Surely they should not regard irrational beliefs as explicable in terms of interests, and rational beliefs as different in kind? I have no objection in principle to the argument that people may arrive at beliefs we deem rational as a result of factors such as interests. Perhaps it might be shown that in the past some of the factors which led past scientists to a heliocentric view of the solar system were connected with an interest in the occult. But that does not diminish the rational and scientific grounds for the heliocentric view of our corner of the universe. Beliefs may be overdetermined. There may be irrational factors conducive to the holding of a belief which is on other grounds deemed rational. This part of the symmetry thesis is not incompatible with my position. But I would argue that there is also an element of asymmetry in that irrational beliefs would be explicable in sociological terms but would lack rational defences of equal cogency to those available for rational belief. What I reject is a full blooded relativism which would reduce the distinction between rational beliefs and others. The theory of rational belief advocated here is one which appeals to rational criteria as much as possible, but which admits that

other factors do operate, especially where there is a clash of criteria.

I have argued that at least some metaphysical beliefs can be assessed by appeal to criteria. They are, in other words, candidates for being rational beliefs. Detailed application of the tests to particular beliefs goes beyond the scope of this work. Here however I have argued for a criterion of demarcation which focusses on the division between rational and non-rational beliefs. There is also no doubt a need for a criterion of demarcation between beliefs in different disciplines. But I consider the distinction between rational and non rational beliefs more important than that between beliefs which belong to different disciplines. We can assess metaphysical views for their probability on rational criteria. This means that we should not just dismiss them out of hand on the grounds that they are not part of science. Nor should we limit ourselves to asking whether metaphysical views are possible or impossible. Of course that is an important question. But the comparative probability is even more important. That we can attempt to assess by assessing competing beliefs for their comparative success in satisfying the criteria.

This work has been concerned with rational belief. I have argued that in several disciplines what we are dealing with are revisable beliefs. These beliefs are revisable, but we can argue for their rationality. At the outset we considered the relation between knowledge and rational belief. Then I defended the importance of the notion of belief with its implicit reference to a believing subject. In discussing belief we need

to consider both dispositions and conscious mental states. In the case of rational belief we need to assess evidential support. Even if evidential support is not the sole ground on which we should rest belief, it is the main one, and this is especially so for beliefs we claim to be rational. As beliefs interact and interlock so we must consider larger complexes of belief. In the case of historical beliefs we can detect instances of a cumulative case based on an appeal to criteria. Mitchell argues that there is an analogous way of arguing for the rationality of metaphysical beliefs. Closer attention to historical beliefs reveals that though these raise some special issues, the criteria used there are largely comparable to those under discussion in the philosophy of science. Criteria of explanatory power and predictive success are relevant to historical study even if they are used in slightly different ways there. In the case of metaphysical beliefs Swinburne argues for a set of such beliefs by appealing to criteria such as simplicity and explanatory power. His view is to be contrasted with those who defend theses of incommensurability or who depend heavily on a notion of commitment. Swinburne emphasizes rational scrutiny and on this general point I am in agreement with him. Yet his use of Bayesian methods is somewhat problematical. I argue that some metaphysical beliefs can be assessed by testing them according to criteria, without necessarily subscribing to a particular Bayesian method, or agreeing with Swinburne's priorities about the criteria, or other of his more controversial points. Consistency, accuracy, explanatory power, fruitfulness and

simplicity are appropriate criteria here as elsewhere. I would rank the criteria somewhat differently from Swinburne, especially in giving less emphasis to simplicity. There are cases where serious clashes between the criteria prevent decisions from being clear cut. But this should not overshadow those cases where decisions are more straightforward. In these cases reference to the criteria outlined above can enable some beliefs, and some sets of beliefs, to be preferred to others on rational grounds. There are limits to the rational assessment of belief. The point at issue is not whether there are such limits. It is the claim that within those limits we have criteria for preferring some beliefs as being more rational than others.

Notes to Chapter One

1. L.Wittgenstein On Certainty (Oxford: Blackwell, 1969) paras 337, 354, 450, 458.

2. N.Malcolm 'Knowledge and Belief' Mind 61 (1952) 178 - 189.

3. See the controversy between R.Taylor and N.Malcolm noted in N.Malcolm, Knowledge and Certainty (Englewood Cliff: Prentice Hall, 1963) 71n.11.

4. An argument that fallibilism and necessity are not incompatible is presented by S.Haack 'Fallibilism and Necessity' Synthese 41 (1979) 37 - 63.

4a. In the sense that there is something that I believe.

5. E.L.Gettier, 'Is Justified True Belief Knowledge?' Analysis 23 (1963) 121 - 3. (Smith has strong evidence that (d) 'Jones will get the job and Jones has ten coins'. So Smith is justified in believing (e) 'The man who will get the job has ten coins'. But Smith is unaware that he has ten coins and that he (Smith) will get the job. So (e) is a justified true belief but Smith does not know (e).).

6. K.Lehrer, Knowledge (Oxford: Clarendon Press, 1974) 224. See also n22 below, and D.Scaduto-Horn, 'A Problem for Defeasibility Theories' J.Ph. 79 (1982) 40-45.

7. Cf. P.L.Mott 'Haack on Fallibilism' Analysis 40 (1980) 177 - 183.

8. J.Kekes, A Justification of Rationality (Albany: State Univ. of New York Press, 1976) 76 - 91.

8a. A wider range of normal usages is given in R.L.Franklin, 'Knowledge, Belief and Understanding' Ph.Q. 31 (1981) 193 - 208.

9. T.S.Kuhn 'Reflections on my Critics' in Criticism and the Growth of Knowledge ed. I.Lakatos and A.Musgrave (Cambridge U.P. 1970) 265 - 6. There has also been much debate over the notion of verisimilitude. Popper has now withdrawn his definition of verisimilitude, but not the use of the term cf. K.R.Popper, Realism and the Aim of Science (London: Hutchinson, 1983) xxxv .

On realism and the notion that there is something in virtue of which the assertion that p is true or false, and that one reason for not knowing that p would be that p is false, see the debate over anti-realism in, for example, Truth and Meaning ed. G.Evans and J.McDowell (Oxford: Clarendon, 1976).

Notes to Chapter Two

10. J.Hintikka 'Semantics for Propositional Attitudes' in Intentionality, Mind and Language ed. A.Marras (Chicago: Univ. of Illinois Press, 1972) 429 - 457 esp. 454.

11. J.W.Cornman 'Intentionality and Intensionality' in Marras (1972) 52 - 65 (See n.10 above), but also A.Marras 'Intentionality and Cognitive Sentences', ibid.,66 - 74.

12. Cf. R.Chisholm, Person and Object (London: Allen and Unwin, 1976) 159 - 175.
Cf. also N.Chomsky Language and Mind (New York: Harcourt 1968).

13. W.V.Quine 'Reply to Føllesdal' in Words and Objections ed. D.Davidson and J.Hintikka (Dordrecht: Reidel, 1969) 336.

14. W.V.Quine 'Quantifiers and Propositional Attitudes' Journal of Philosophy 53 (1956) 177 - 187 cf. 180.

15. Cf. W.V.Quine 'On Frege's Way Out' Mind 64 (1955) 145 - 159.

16. R.M.Chisholm 'Notes on the Logic of Believing' Philosophy & Phenomenological Research 24 (1963-4) 195 - 201.
Cf. also Chisholm, Person and Object, 174 - 5.

17. S.Haack 'Epistemology with a Knowing Subject' Review of Metaphysics 33 (1979 - 80) 308 - 335.

18. On belief as occurrence and belief as disposition see H.H.Price, Belief (London: Allen & Unwin, 1969) 189 - 289. (For an account which gives a large place to the role of images in belief see T.L.S.Sprigge Facts, Words and Beliefs (London: RKP, 1970).)

19. J.R.Lee, 'Belief as a Dispositional Property', Philosophical

Studies 30 (1979) 207 - 8. The criticism is directed against Quine and Ullian, The Web of Belief (Random House, 1970) 3 - 4, (cf. 2nd ed. 1978 p.10).

20. The difficulty was noted by R.B.Braithwaite 'Belief and Action' PASS 20 (1946) 1 - 19, though in a slightly different form.

21. K.Lehrer et al., 'Reason and Evidence: An Unsolved Problem' Ratio 9 (1967) 34 - 48.

22. K.Lehrer 'The Gettier Problem and the Analysis of Knowledge' in Justification and Knowledge ed. G.S.Pappas (Dordrecht: Reidel 1979) 65 - 78.

22a. On this see H.H.Price, 'Belief and Will', PASS 28 (1954) 1 - 26 and H.Fingarette, Self Deception (London: RKP 1969) Ch. 2.

23. R.Firth 'Chisholm and the Ethics of Belief' Ph.R. 68 (1959) 493 - 506.

24. R.G.Swinburne Faith and Reason (Oxford: Clarendon, 1981) 25.

25. D.Hume, A Treatise of Human Nature ed. L.A.Selby-Bigge (Oxford: Clarendon, 1978) 624.

26. So J.W.Meiland 'What Ought We to Believe?' Am.Ph.Q. 17 (1980) 15 - 24.

27. One might argue that the variation is due to differences in inductive standards, as Swinburne does. See Swinburne, Faith and Reason, 45 - 54. Also people differ in their view of what the evidence is, and sometimes an individual will accept an argument he previously rejected. For further factors see Ch. 6 below.

28. A.Palmer 'Belief' Ph. 56 (1981) 33 - 45.

29. The example is adapted from one discussed in the article cited at note 26 above.

30. See recently M.Kaplan, 'A Bayesian Theory of Rational Acceptance' J.Ph. 78 (1981) 305 - 330.

31. See for example Applications of Inductive Logic ed. L.J.Cohen and M.Hesse (Oxford: Clarendon, 1980) esp. pp156 - 201 and the further works cited there.

32. As $0.9^7 = 0.4782969 < .5$.

Notes to Chapter 3

33. For a version of the former view see R.Chisholm Theory of Knowledge (Englewood Cliffs: Prentice Hall^2 1977) 63 for a version of the latter see K. Lehrer Knowledge Ch. 7-8.

34. B.Mitchell, The Justification of Religious Belief (London: Macmillan, 1973) 61. Mitchell is largely concerned with traditional Christian theism, as is Swinburne also. I may occasionally make reference to other examples of what he calls 'large-scale metaphysical systems'.

35. Mitchell, Justification, 39, 156.

36. Mitchell, Justification, 39. His contention is discussed in a thesis by R.A.R.Sykes 'The Ethics of Religious Belief' (Oxford: unpublished D.Phil. Thesis 1978).

37. Cf. Applications of Inductive Logic ed. L.J.Cohen & M.Hesse.

38. Mitchell, Justification, 44, 57, 74, 95 uses the word analogy. The parable of the explorers is given on pp43-4 and the example from Roman history on pp51-53.

39. See Mitchell Justification 61 for the phrase 'large-scale metaphysical systems'.

40. Support from historians of widely differing viewpoint can be found in the following books: G. Bornkamm, Jesus of Nazareth (London: Harper, 1960), G.Vermes, Jesus the Jew (London: Collins, 1973) M.Grant, Jesus (London: Weidenfeld & Nicholson, 1977).

41. Mitchell, Justification, Ch.4&5.

42. J.L.Mackie, The Miracle of Theism, (Oxford: Clarendon, 1982).

(42.) It might have been possible to select for consideration instead of Mitchell and Swinburne an earlier work by V.A.Harvey The Historian and the Believer (London: SCM, 1967). This work has much excellent discussion of historians and philosophers such as Troeltsch, Bradley and Collingwood and of theologians and biblical scholars such as Bultmann, Barth, Richardson, and Niebuhr. One of its strengths is its consideration of interpretation and of perspectivism and its discussion of examples from biblical study. It gives much less attention to Dray, Gardiner, and Walsh and none to Goldstein and the recent debates he has provoked. Also Harvey's work is too early to give attention as Mitchell does to the problem of rational belief in the light of controversy over the work of Popper, Kuhn, Lakatos and their followers and opponents (of these only Popper gets one incidental mention). (Nor for the same reason is there discussion of Price, Quine, Lehrer and Chisholm on the problems of belief). Harvey's work has many excellent features and makes many telling points in relation to the topics which it discusses. But it is chiefly concerned with the use of critical historical method in relation to biblical study and with theologians such as Barth and Tillich. The aim and purpose of the present work is rather different.

Notes to Chapter 4.

43. L.Goldstein, Historical Knowing (Austin, Texas 1976).

44. W.H.Walsh 'Truth and Fact in History Reconsidered' History and Theory 16.4 Beiheft 16 (1977) (The Constitution of the Historical Past) 53-71 esp.56.

45. W.H.Walsh, An Introduction to Philosophy of History (Harvester,[3] 1970) 84.

46. P.Gardiner The Nature of Historical Explanation (Oxford: Clarendon 1961) 52-54. See also A.Donagan, The Later Philosophy of R.G.Collingwood (Oxford: Clarendon, 1962) 233-235. (One would also need to qualify the word 'always'.)

47. See T.S.Kuhn The Structure of Scientific Revolutions (Chicago: Univ. of Chicago, 2nd ed. 1970) 199 'accuracy, simplicity, fruitfulness, and the like'. See also T.S.Kuhn 'Reflections on my Critics' in Criticism and the Growth of Knowledge ed. I.Lakatos and A.Musgrave (Cambridge: C.U.P., 1970) 231-278 esp. 262 'Simplicity, scope, fruitfulness and even accuracy can be judged quite differently (which is not to say they may be judged arbitrarily) by different people'. These sections contain important clarifications of his position by Kuhn. See also T.S.Kuhn, The Essential Tension (Chicago: Univ. of Chicago, 1977) 322 and 330. Amongst earlier discussions of criteria see C.G.Hempel Aspects of Scientific Explanation (London: MacMillan, 1965) 117.

48. W.V.Quine and J.S.Ullian, The Web of Belief (New York: Random House[2] 1978) 71. These authors speak of 'virtues' rather than 'criteria'.

49. See C.Hay 'Historical Theory and Historical Confirmation' History and Theory 19 (1980) 39-57 esp.50 - 52. She is discussing the views of M.G.Murphey, Our Knowledge of the Historical Past (New York: Bobbs Merrill, 1973).

50. By this phrase I mean what I argued above, that historical events are not directly observable but are inferred from the present evidence which is directly observable, and that the past events cannot be reiterated in the way in which a chemist can repeat an experiment, past events can only be reinspected insofar as fresh evidence allows fresh inferences.

51. See the comments by W.Dray 'The Historical Explanation of Actions Reconsidered' in The Philosophy of History ed. P.Gardiner (London: OUP, 1974) 66 - 89 esp.68.

52. L.J.Cohen The Probable and the Provable (Oxford: Clarendon, 1977) 301 following W.Salmon.

53. C.Hay 'Historical Theory and Historical Confirmation' 39-57. Also see Harvey, The Historian and the Believer, 45-48.

54. C.Seignobos, A History of the French People (London: Cape, 1933) 285. The example is selected for discussion in P.Gardiner, The Nature of Historical Explanation (Oxford: OUP, 1952 & 1980) 66.

55. S.Runciman, A History of the Crusades, Vol. 1 (London: Penguin 1965) 16, cf. Cohen, Probable and Provable, 284.

56. Cohen, Probable and Provable, 251.

57. L.J.Goldstein, Historical Knowing (Austin: Univ. of Texas Press, 1976) 84.

58. Goldstein, Historical Knowing, 15.

59. Goldstein, Historical Knowing, 210.

60. L.J.Goldstein 'History and the Primacy of Knowing' in

The Constitution of the Historical Past 29-52 (=History and Theory Beiheft 16) esp. p.33.

61. Ibid. 46, my next citation is from p.47. For Pompa's view see n68.

62. See D.Childs Britain since 1945 (London: Benn 1979) 88.

63. See Ancient Synagogues Revealed ed. L.I.Levine (Jerusalem: I.E.S., 1981) 52 - 62 (a discussion between Loffreda, Foerster and Avi-Yonah). The objectors also argue that later coins could have fallen between the stone slabs of the floor, or have been dropped when the synagogue floor was repaired.

64. I.Lakatos The Methodology of Scientific Research Programmes (Cambridge: CUP, 1978) 32.

65. Ibid., 31 - 34.

66. Walsh, 'Truth and Fact', 68 (See n.44 above).

67. Ibid., 70.

68. Of course if it is in fact the case that the choice between rival theories is undecidable, whether or not we know that, it does not mean that the theories are indistinguishable. To avoid that conclusion we need only to be able to specify what would enable us to decide between them were such evidence to exist (even if in the actual world it does not). There is also criticism of Goldstein's anti-realism by L. Pompa in 'Truth and Fact in History' an article in Substance and Form in History ed. L. Pompa and W.H.Dray (University of Edinburgh, 1981) 171-186. Pompa argues that the notion of a historical past presupposes the notion of a real past.

68a See n.38 above and the text thereto.

Notes to Chapter Five

69. R.Swinburne, The Existence of God (Oxford: Clarendon Press, 1979) 291.

70. Review in The Philosophical Quarterly 31 (1981)85-88.

71. In The Journal of Philosophy 77 (1980)502.

72. N.Tennant, 'Entailment and Proofs' PAS 79 (1979)167-189 see also N.Tennant, 'A Proof-Theoretic Approach to Entailment' Journal of Philosophical Logic 9 (1980)185-209.

73. So D.Z.Phillips using biblical allusion in his review of Swinburne's Faith and Reason in T.L.S. May 28, 1982,588.

74. Ibid. 588

75. Mitchell, Justification, 64-74 but note his caution about conceptual relativism on p67, also cf.p.95.

76. The example is aimed at what Kuhn says in Structure of Scientific Revolutions, 114-116 (Kuhn has in more recent works expressed himself more cautiously, Thus T.S.Kuhn, The Essential Tension (University of Chicago Press, 1977) xxii and 338-9).

77. D.Z.Phillips, Faith and Philosophical Enquiry (London: R.K.P., 1970)132.

78. D.Z.Phillips, Religion Without Explanation (Oxford: Blackwell, 1976) 174 cf.180-181.

79. So J.L.Mackie The Miracle of Theism (Oxford: Clarendon, 1982)212.

80. As yet unpublished. There is also an interesting but very different argument from moral order in R.M.Adams 'Moral Arguments for Theistic Belief' in Rationality and Religious Belief ed. C.F.Delaney (University of Notre Dame Press, 1979)116-140.

81. Such as obedience to the golden rule of treating others as one would wish to be treated oneself, or its more philosophical ethical equivalent the principle of universalizability of ethical maxims. The argument in the text is not intended in any way to disparage such a view, or to claim that the Pauline tradition does so, but rather to draw attention to the fact that Christianity (like Judaism) places unmerited divine generosity prior to moral demand.

82. J.Hick, Philosophy of Religion (Englewood Cliffs:Prentice Hall, 1963)70-71 (=^{2} 59-61) see also Hick, The Centre of Christianity (London: S.C.M., 1977)47-9.

83. J.Hick, Evil and the God of Love (London: Fontana, 1968) 317-323 sets out his idea of 'epistemic distance' in some detail. On weighting or ranking criteria see below (Ch.6).

84. James 2.19 Τὰ δαιμόνια πιστεύουσιν καὶ φρίσσουσιν.

85. Hick, Evil, 368.

86. R.G.Swinburne, An Introduction to Confirmation Theory (London: Methuen, 1973).

87. T.Seidenfeld, Philosophical Problems of Statistical Inference (Dordrecht: Reidel, 1979) 12.

88. There are many discussions of the contention that if one does not use the probability calculus one is open to a betting situation in which one must lose. For one recent discussion see C.Glymour, Theory and Evidence (Princeton University Press, 1980) 72-4. (He rejects the contention).

89. Swinburne, Existence of God 51-56 & 64 for scientific examples, p.66 for example from detective fiction.

90. Cohen, Probable and Provable 49-120 esp. 93-115 for this paradox. (Results vary as choices of k vary.)

91. Swinburne, Existence of God, 52.

92. Ibid. 53.

93. Ibid. 53

94. Ibid. 68.

95. See n.87 above.

96. See n.90 above.

97. Swinburne, Existence of God, 56.

98. I.Lakatos, Methodology, (Philosophical Papers Vol.I) 48-52 & 90-93 his view is presented with a wealth of scientific examples.

99. So Swinburne, Existence of God, 202.

100. Swinburne does argue that while it is reasonable to believe each item in a complex creed (set of beliefs) it may also be wise to recognize that there is at least one false belief in onds set of beliefs. This is a concession in the face of what is sometimes called the lottery paradox, or the paradox of the preface, or the problem of conjunction, where the conjunction of a set of individually probable beliefs may either be improbable or even impossible. This would permit Swinburne to jettison one or two hypotheses without great concern. But the repeated loss of peripheral hypotheses will at some point cast doubt on the central theory.

101. On simplicity see M. Hesse, The Structure of Scientific Inference (London: Macmillan, 1974)223-257. but also Glymour, Theory and Evidence 77-82, E.Sober, Simplicity

(Cambridge Univ. Press, 1975), E.Fales 'Theoretical Simplicity & Defeasibility' Ph.Sci. 45 (1978) 273-8.

102. J.A.Adler 'Criteria for a Good Inductive Logic' in Applications of Inductive Logic (1980) ed. Cohen and Hesse, 379-405 esp.387.

103. Swinburne, Existence of God, 284 (cf.286 and 90-106).

104. See for example the papers of Hesse and Niinuluoto in Applications of Inductive Logic, ed. Cohen and Hesse, i.e. M. Hesse, 'What is the Best Way to Assess Evidential Support for Scientific Theories ?' (202-217), and I. Niinuluoto, 'Analogy, Transitivity, and the Confirmation of Theories' (218-234).

Notes to Chapter 6.

105. F.Suppe (ed.) The Structure of Scientific Theories (Chicago: Univ. of Illinois Press, 1974) 170 cf. pp. 175-6, 230-2, 388. A.Grünbaum 'Is Falsifiability the Touchstone of Scientific Rationality' in Essays in Memory of Imre Lakatos (Boston Studies in the Philosophy of Science Vol. 39) ed. R.S.Cohen, P.K.Feyerabend & M.W.Wartofsky (Dordrecht: Reidel 1976) 213-252 esp. 235-6.

106. See Ch. 1 & 2 above.

107. See Ch. 3 & 4 above.

108. The Coherence of Theism (Oxford: Clarendon, 1977) esp. 233 & 281.

109. See the severe criticisms in I.Lakatos The Methodology of Scientific Research Programmes 59-68.

110. On maximally consistent subsets see N.Rescher The Coherence Theory of Truth (Oxford: Clarendon, 1973). Rescher is concerned as I am with coherence as a criterion, though he is concerned with the theory of truth, and more especially with coherence as a criterion of truth. My concern is more with criteria for rational belief and rational theory choice. On 'content reducing strategems' see I.Lakatos The Methodology of Scientific Research Programmes; note p41 the requirement of content increasing changes. Also see Hesse M., The Structure of Scientific Inference, 238.

111. See D.Z.Phillips 'Faith, Scepticism and Religious Understanding' in Religion and Understanding ed. D.Z. Phillips (Oxford: Blackwell, 1967) 63-79 esp. 76. This

seems to be the position he is maintaining, though on difficulties of deciding just what he is really saying see the earlier discussion in Ch.5 above.

112. A similar situation would arise if we focussed on A.Kenny's claim in his The God of the Philosophers (Oxford: Clarendon, 1979) that there is an internal inconsistency between divine omniscience and the view that God is not responsible for evil actions performed by human agents. We would need criteria other than consistency for preferring one way rather than another out of the dilemma that Kenny presents to us.

113. K. Popper, The Logic of Scientific Discovery (London: Hutchison, 1959 & 1968) Ch. 7.

114. See C.Glymour, Theory and Evidence 46-47.

115. Relevant literature includes E.Sober Simplicity (Oxford University Press, 1975), Hesse M. The Structure of Scientific Inference (London: Macmillan, 1974) Ch.10 Fales, 'Theoretical Simplicity and Defeasability' Philosophy of Science 45 (1978) 273-8, E.Sober, 'The Principle of Parsimony', British Journal for the Philosophy of Science 32 (1981) 145-156.

116. The discussion of the 'grue' paradox goes back to the debate instigated by Nelson Goodman. The particular line of argument here stems from D.Edgington's criticisms of E.Sober in Mind 87 (1978) 623-6.

117. Several critics have pointed out that on Sober's 1975 theory each of a pair of theories can be shown to be simpler than the other depending on which of a pair of

questions one asks. Let us compare $-\exists x(Fx)$ with $\exists x(Fx)$. The first is more informative and so Sober -simple if the question is whether a is F. But in relation to the question 'Is there an F?' they rank equally. In relation to the question 'Is there an F or a G?' the second is Sober -simpler because more informative. This is unsatisfactory. Cf. S.Haack in Philosophical Quarterly 26 (1976) 373-4.

118. (Supplementary note on the problem of defining simplicity.) Limited comfort can be derived from the more obvious characterizations of simplicity. P is simpler than P & Q. If B entails A and A does not entail B, then A is simpler than B. But most serious examples of rival choices do not take this latter form. The elimination of superfluous assumptions is an obvious form of the appeal to simplicity. We prefer 'Jean is 40 and so will probably live to 50' to 'Jean is 40 and has blue eyes and so will probably live to 50'. The extra clause has no genuine explanatory power. But in the case of 'Jean is 40 and in good health and so will probably live to 50' we prefer the extra clause. A gain in accuracy and explanatory power offsets a loss of simplicity. But appeal to simplicity (or modesty, or economy, or parsimony) leads us to resist the addition of clauses which are irrelevant. To say that however is to state the issue rather than to define it.
Another distinction which is of some value, but again inadequately defined, is that between the reduction effected by denying that something exists and the reduction

effected by identifying one object with another. Denial of the existence of phlogiston or of aether belongs to the first category. Identification of larger subatomic particles with combinations of quarks would (if correct) be an example of the second type of simplification. On this distinction see Sober 'The Principle of Parsimony' cited n.115 above.

119. R.Swinburne, The Existence of God, 103-4.

120. Ibid. 104-105.

121. Ibid. 106.

122. Ibid. 172. (The emphasis here is his).

123. See C.Glymour, Theory and Evidence, passim, Quine & Ullian, The Web of Belief, 20 cf. also J.J.C.Smart 'Quine's Philosophy of Science' in Words and Objections ed. D.Davidson and J.Hintikka (Dordrecht: Reidel, 1969) 3-13, also see n.105 above.

124. I have argued against radical incommensurability theses above. We are here dealing with a lesser issue.

125. Hesse, Structure of Scientific Inference, 228-9.

126. P' containing all the sentences of P except that which says that s and p are reducible to P'. I propose the device merely to avoid reflexivity.

127. See the quotations cited above and especially that cited in the text to n.122 above.

128. The three quotations are from Swinburne, Existence of God, 104-5.

129. See n.118 above and the article by Sober cited there and in n.115 above.

130. Swinburne, Existence of God, 105.

131. Ibid. 172.

132. Ibid. 40 cites a somewhat curious argument by Richard Taylor as the main ground for the objection.

133. Ibid. 93-94.

134. It has been pointed out that other aspects of this claim that theism postulates a very simple kind of person are highly problematical. It is possible to use simple familiar language to describe an entity which is in fact very complex. The complexity appears once the surface simplicity of the language is probed in an attempt at definition or description. If correct this objection would find a further difficulty in Swinburne's case. But we have already presented a fundamental objection.

135. On this see P.R.Thagard, 'The Best Explanation: Criteria for Theory Choice' Journal of Philosophy 75(1978) 76-92 with references to earlier literature by Hempel and others.

136. See n.55 above and the text thereto in Ch.4 above.

137. In each case we must pass over the need to include further clauses. For example we could include the provisos that the troops in question do not stand to gain a large amount of plunder, or hate the enemies of their overlord even more than they dislike their overlord. These complications were discussed earlier.

138. Isaiah Berlin Historical Inevitability (London: O.U.P.,1954)

cf. also E.Nagel 'Determinism in History' in Philosophical Analysis and History ed. W.H.Dray (New York: Harper, 1966) 347 - 382.

139. During the Israeli occupation of Lebanon Druze soldiers from Israel who had been prepared to serve against other Arabs were not so willing to serve against their fellow Druze in Lebanon.

140. The element of variety is important. Of course a theory which satisfactorily explains a variety of instances of troops defecting is preferable to one which accounts for large numbers of instances of unpaid troops deserting. The reasoning is comparable to a preference for Newton's Laws over those of his predecessors on the grounds that they explain a wider variety of phenomena (motion of planets, behaviour of satellites of planets, tides and (eventually) comets).

141. Swinburne, Existence, 107 - 8.

142. He offers a brief discussion of scope on pp 52 - 3.

143. Ibid, 108.

144. The alternative policy would require an arbitrary restriction on the consideration of rival theories. It would involve comparing theories from different disciplines against different sets of data. What he does is to consider the explanatory power of rival theories of a similar metaphysical character to explain the same set of data. Even if the alternative policy were adopted the argument would still turn on whether some kind of gain in explanatory power is involved. It would however, if established, be a

gain of the other type.

145. Swinburne, Existence, 113.

146. Ibid., 131.

147. Ibid., 119.

148. W.L.Craig, The Cosmological Argument from Plato to Leibniz (London: Macmillan 1980).

149. Ibid., 282 - 4.

150. Mackie, Miracle of Theism, 101.

151. Craig, Cosmological Argument, 284 - 5 criticizes Rowe and others for 'reading the principle of sufficient reason back into these pre-Leibnizian thinkers'.

152. Swinburne, Existence, 121.

153. Ibid., 127.

154. Ibid., 128.

155. Other recent discussions of the cosmological argument include D.D.Crawford, 'The Cosmological Argument, Sufficient Reason, and Why-Questions' Int. J. for Phil. Rel. 11 (1980) 111 - 122, H.Meynell The Intelligible Universe (London: Macmillan, 1982), T.W.Bartel 'Cosmological arguments and the uniqueness of God' Int. J. Phil. Rel. 13 (1982) 23 - 31, K.Ward Rational Theology and the Creativity of God (Oxford: Blackwell 1982) 29-32, C.Dore 'Rowe on the cosmological argument' Int. J. Phil. Rel. 14 (1983) 25 - 31.

156. Ibid., 126. (He provides a diagram.) On the whole issue of explanatory considerations see also K.Ward, Rational Theology, 29 - 32. He argues for the idea of total intelligibility as a regulative principle. He says that the axiom of intelligibility can't be given a proof though conjectures

based on it can be tested for coherence, adequacy and simplicity, though not by decisive experiment. He then continues 'given an assumption of intelligibility, there is a self-explanatory being...' (p32). This is a different argument from that of Swinburne, but one which also appeals to criteria such as coherence, adequacy and simplicity as well as to intelligibility.

157. See above for the work of Lakatos who makes considerable use of this criterion. For an argument showing that a particular version of the criterion will satisfy both the demands of Lakatos and those of some Bayesians see I.Niinuluoto 'Novel Facts and Bayesianism' Brit. J. Phil. Sci. 34 (1983) 375 - 379. See also E.McMullin 'The Fertility of Theory and the Unit for Appraisal in Science' in Essays in Memory of Imre Lakatos ed. R.S.Cohen et al., esp. pp.400 - 410.

158. For this example I am indebted to two colleagues in the History Dept. at the University of Edinburgh. This was communicated to me in a private letter from Dr.P.Addison dated 25th August 1983, citing F.Fischer, Griff nach der Weltmacht (Düsseldorf: Droste 1961).

159. See M.R.Gardner 'Predicting Novel Facts' Brit J. Phil. Sci. 33 (1982) 1 - 15.

160. Niinuluoto, 'Novel Facts', 375 - 6.

161. With reference to discussion of this criterion in relation to theistic belief see especially J.Hick 'Eschatological Verification Reconsidered' Rel. Stud. 13 (1977) 189 - 202 and the numerous earlier works there cited.

162. Swinburne, Existence, 66. The next quotation from him is from p. 67.

163. A.Grünbaum, 'Is Falsifiability the Touchstone of Scientific Rationality? Karl Popper versus Inductivism' in Essays in Memory of Imre Lakatos ed. R.S.Cohen et al., 213 - 252 esp. 241 - 2.

164. Swinburne, Existence, 202.

165. Lakatos, Methodology, 30 - 52, passim.

166. K.Popper, The Logic of Scientific Discovery (London: Hutchinson, 1959) 265 - 9.

167. Glymour, Theory and Evidence, 154.

168. On this see P.K.Feyerabend, 'Consolations for the Specialist' in Criticism and the Growth of Knowledge ed. I.Lakatos and A.Musgrave, 197 - 230 esp. 215. Feyerabend is rightly critical of the vagueness which appears at this point. It can be said in defence of the notion of tenacity that some theories have recovered after surprisingly long periods of decline.

169. One could argue that one of the central tenets of theism could have predictive success in the following way. Central to Christian theism is the belief that undeserved grace and undeserved love transform the individual and the community. Certain modern psychoanalytic theories have propounded a partially analagous belief. Against the view that all such theories are unfalsifiable, and so unreasonable, someone could argue for their testability. If it could be shown that people so treated gain in psychic health and maturity when measured against a group of other untreated people

one would have a positive result. Clearly this is only an outline sketch of a possible test. But it has been suggested in support of the view that such theories may not be as untestable as is sometimes imagined.

170. I owe the example to E.Nagel, The Structure of Science (London: RKP, 1961) 179 - 83.

171. He calls this observational nesting (which sounds rather ornithological). See W.H.Newton-Smith The Rationality of Science (London: RKP, 1981) 226.

172. For further discussion see J.L.Gorman, 'Precision in History' in Substance and Form ed. Pompa & Dray, 120-132. He says that the crucial factor is the conveying of more information. He goes on to discuss relevance of information, and whether the conceptual scheme of classification is acceptable.

173. J.W.Michels, Dating Methods in Archaeology (New York: Seminar Press, 1973) 153. He provides a table showing the range of variation over several thousand years, and a list of recalibrated dates.

174. So Swinburne, Existence of God, 254n.

175. Ibid., 270.

176. E.T.Long, 'Lewis on Experience, Reason, and Religious Belief', Review of Metaphysics 35 (1981) 87 - 110 esp. 98 - 99.

177. S.T.Katz, 'Language, Epistemology, and Mysticism' in Mysticism and Philosophical Analysis ed. S.T.Katz (London: Sheldon, 1978) 22 - 74 esp. 65.

178. Mackie, Miracle of Theism, 183.

179. Swinburne, Existence of God, 245 & 252.

180. Long 'Lewis on Experience' 94 - 6.

181. H.Van Etten, George Fox and the Quakers (New York: 1959)32.

182. Long 'Lewis on Experience' 102.

183. Swinburne, Existence of God, 260.

184. W.L.Rowe, 'Religious Experience and the Principle of Credulity' International Journal for the Philosophy of Religion 13 (1982) 85 - 92 esp. p.91. On the problem of deceptive experiences see G.Mavrodes 'Real v. Deceptive Mystical Experiences' in Mysticism & Philosophical Analysis ed. Katz, 235 - 258.

185. Kuhn, Structure, 199 - 200.

186. Other works which have been consulted but not directly cited earlier in the notes include Knowledge and Belief ed. A.P.Griffiths (Oxford: OUP, 1967), J. Hintikka, Knowledge and Belief (Cornell University Press, 1962), R. Needham, Belief Language and Experience (Oxford: Blackwell, 1972), W.V.O.Quine, Word and Object (Cambridge Mass: MLT., 1960).

Bibliographical Index

References are indexed under names of authors or editors and refer to the numbers of the end notes. References which are underlined contain the main entry of the title of a new article or book. Bibliographical details will be found in the note so underlined. Subsequent entries normally use short titles.

Abbreviations

Am. Ph. Q. American Philosophical Quarterly

Brit. J. Phil. Sci. British Journal for the Philosophy of Science

Ibid. Ibidem

Iff If and only if

Int. J. Phil. Rel. International Journal for the Philosophy of Religion

J. Ph. Journal of Philosophy

PAS Proceedings of the Aristotelian Society

PASS Proceedings of the Aristotelian Society Supplementary Volume

Ph. Philosophy

Ph. Q. Philosophical Quarterly

Ph. R. Philosophical Review

Ph. Sci. Philosophy of Science

Rel. Stud. Religious Studies

T.L.S. Times Literary Supplement